Social Thought and Social Change: Methodological Dilemmas at the Intersection of

Science and Ethics

by

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Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor of Philosophy in the Department of
Political Science in the Graduate School
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<u>ABSTRACT</u>

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Abstract

I argue that ethical convictions are crucial to the maintenance and transformation of social institutions. Moreover, since ethical convictions are sometimes corrigible and open to persuasive transformation, ethical persuasion can be a powerful source of social change. However, I observe that the dominant analytic techniques of the social sciences are ill equipped to understand the nature and import of ethical convictions, and even less well equipped to inform ethical persuasion. I argue this, in part, explains why social science research has often proved of little value in trying to address prominent social concerns.

This diagnosis raises a puzzle and a challenge. The puzzle is why some social scientists would wholly commit themselves to methods that cannot adequately deal with important dimensions of social structure. I show this is due to a misguided conception of science, one which seeks an "absolute perspective" that requires reducing or explaining away ethical convictions.

The challenge, once this vision of science is rejected in favor of a more pragmatic one, is 1) to understand the systematic limits of different methodological approaches and 2) to see how an account of ethics, rightly understood, can complement social scientific knowledge in service of better social outcomes.

I evaluate three dominant methodological approaches in the social sciences, namely, statistical modeling, formal modeling, and biological-behavioral research.

Although all are useful within certain domains, I show that each has systematic limits relating to the dynamism of ethical convictions. I demonstrate how these methods can fail on their own terms and can blind researchers to important resources for social change, such as possibilities for persuasion.

Finally, I develop an account of the relationship between ethics, rationality, and persuasion drawing on the work of Hans-Georg Gadamer, Alasdair MacIntyre, and Charles Taylor. This account rejects prominent "scientific" attempts to explain ethical allegiances as biologically hardwired or structurally determined, and it further challenges accounts of ethical naturalism and pluralistic neutrality.

I conclude by illustrating the constructive role that ethical persuasion has played in a number of development projects, which help demonstrate my thesis that debates about visions of "the good" matter profoundly for human flourishing.

Dedication

To my teachers.

First, my parents.

Then, the many others, living and dead.

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Acknowledgements

There are many people to whom I am grateful for their help in this project and my education at large. I have dedicated this work to my teachers, particularly my parents, without whose guidance at various levels this project would have been inconceivable. My committee members – Michael Munger, Michael Gillespie, Evan Charney, Thomas Spragens, and Stanley Hauerwas – all deserve special recognition both for the enormous amounts I have learned from them (although some may think it was not enough) and for their patience and encouragement as I pursued what was admittedly an eclectic and usual dissertation. Mike Munger and Michael Gillespie's guidance over the years has been particularly valuable. When we met at the first Visions of Freedom FOCUS dinner in 1999 neither they nor I ever thought it might lead to this. I should also note that Evan Charney has been a tremendous resource in recent years as we both delved into emerging controversies in behavioral genomics.

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Preface

This study addresses a methodological weakness in the social sciences, one that limits our ability to engage successfully in certain kinds of social change, or so I will argue. The inquiry takes shape from the following basic puzzle: the "ethical convictions" that people hold evidently influence their behavior in important ways. Such convictions help constitute the goals that people pursue and also circumscribe the means that people are willing to employ in service of those goals. Ethical convictions thus provide unique behavioral structure to the social phenomena that social scientists study. Moreover, we witness an extraordinary variation in these convictions over time, across cultures, and within our own social milieu. Although ethical convictions shape human action and human identity at a deep level they have also proved corrigible and open to persuasive transformation in certain instances. Unfortunately, the dominant analytic techniques in the social sciences are ill-equipped to understand the nature and import of ethical convictions. These methods are even less well equipped to help us engage in persuasion aimed at rationally transforming the substance of these convictions. Thus, the social sciences are generally at a loss when it comes to understanding and influencing ethical convictions, which are undoubtedly important for the way people act. What are the reasons for this? Does it matter? Can we do better?

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The reasons, in fact, run deep. Inquiring into the sources of our ethical convictions and their openness to persuasion raises profound questions about the nature of human rationality and the historical development of human consciousnesses. At certain points in their genesis, the social sciences were conceived as a way to bypass these difficult and contentious issues. It was often argued that the social sciences could concern themselves with facts, while others could explore vexing questions about the nature and justification of value. However, the behavioral import of value judgments is something that the social sciences could often not afford to ignore. When unable to bypass issues of "value preference" or simply accept these preferences as given, many approaches in the social sciences turned towards attempts to reduce value judgments to non-conceptual determinants, be they structural features of one's social surrounding or aspects of one's biological constitution. The imperative to remain appropriately "scientific" prevented considering the conceptual development of ethical convictions in ways that should have linked to valuable resources in philosophy, political theory, and the humanities.

This is a caricature of a very complex story that I will develop at length later in this study, but the basic claim it foreshadows is that it is no accident that the social sciences have shied away from questions of ethics and persuasion. These are not just neglected areas of research, which, once recognized, we can expand our studies to include. Rather, deeply ingrained in the methodological commitments of many approaches in social science - approaches that have served us well for addressing certain

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kinds of problems - are ways of thinking about human rationality, ethics, and persuasion that blind us to important aspects of social structure and possibilities for change.

Does it matter? Indeed, the implication of this diagnosis is that such questions do matter for many concrete problems of widespread concern. We pay significant costs in terms of our practical capacities when we exclude important phenomena from view because they are not tractable within particular methodological paradigms. Moreover, in affirming that these questions matter for concrete problems I intend to distance my argument from a tradition of criticism that faults the social sciences for their inability to provide a theory of everything. This, at least, appears to be the implicit objection of a wide range of critics who argue that, because the social sciences cannot reconstruct the complex intentions, perspectives, and self-understandings that every individual agent brings to each social interaction, these disciplines fall short of satisfactory explanation. True as this may be, such objections misconstrue the promise of social science. Increasingly that promise has been understood and articulated in terms of pragmatic, piecemeal improvements. In recent decades many social scientists have relinquished aspirations to provide anything like a "theory of everything," instead focusing on problems-driven research in the hope of improving the status quo. And at the level of practice, theoretical insights from the social sciences have proved extremely useful in this regard.

In a later argument about the nature of social science I will briefly review the history of the debates that gave rise to the contemporary, pragmatic account of social

science disciplines. At this stage, I want to emphasize that my claims about rationality, ethics, and persuasion are made with an eye towards their practical implications – that, in fact, the social sciences are often handicapped with regard to their own pragmatic goals because of the methodological commitments that obscure questions of ethics and possibilities for persuasion. Recognizing the importance of ethical convictions and conditions under which rational persuasion has the best chance of taking place has wide ranging consequences, both for our understanding of Western liberal democracies and for challenges encountered in the political and economic development in the third world.

Can we do better? Ultimately, I maintain that ethical discourses cannot be reduced in their entirety to non-conceptual determinants, be they biological, structural, geographical, etc. The human mind, through its historical and creative dimensions, generates concepts, and reasons conceptually, in ways that escape such reduction. However, it is the case that practices of ethical argumentation, formation, and judgment depend, like all human activities, on material conditions that provide necessary but not sufficient support. Moreover, ethical concepts can be both expressed and promoted by social institutions. Indeed, there is a sense in which institutions function like extended arguments, presenting and taking for granted certain ideas and norms. However, although the simple institutionalization of any process may help, it does not guarantee the persuasive success of its implicit arguments. These can cease to convince, and there are many historical examples of the revolutionary upheaval of long established institutions by those who lived in or under them. What the social sciences can hope to contribute in

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cooperation with our best social theory is knowledge useful for constructing social conditions in which discourses and practices of ethical formation, argumentation, and dialog have the best chances of taking place. The social sciences may also lead us to recognize, through their own methodological limits, problems for which what is required is some form of ethical persuasion rather than social scientific knowledge.

"Doing better" thus entails recognizing the distinct importance of ethical persuasion for social life, as well as the reality that such persuasion develops according to its own semi-autonomous logic at a conceptual level. The task of using insights from social science to construct social spaces and institutions in which ethical practices and discourses can flourish on their own terms will, of course, be a recursive process. How we decide to carve out these spaces and institutions will reflect reigning judgments about the nature of ethics and rationality, and such judgments are generally contested. This is a basic predicament of ethical judgments.

Related to this is the distinctly political problem of how to negotiate the fact that many traditions of ethical thought are in conflict with one another on various issues and their adherents do not find rival claims persuasive at the present moment. There is a need to find sufficient common ground to allow remaining disputes to proceed in a tolerable manner (and it may be that certain ethical perspectives are judged fundamentally intolerable by those with power to enforce their exclusion).

There is a fundamental indeterminacy to the kind of support the social sciences can lend to these predicaments. Although the social sciences can help us create

conditions, institutional and otherwise, for ethical discourses/practices to develop and contest one another, these discourses will develop on their own terms, beyond the control of social scientific knowledge.

Ultimately, although the social sciences may provide knowledge that can be used in service of ethical formation and persuasion, the more basic challenge is persuading others to adopt those evaluative standards we believe are right, true, justified, and so on… not to mention socially useful.

In order to prevent unnecessary confusion I need, at the outset, to spell out in more detail what I mean by ethics. "Ethics" is sometimes thought to refer to something possessed by a saintly minority, a sort of rare, heroic commitment, involving the abnegation of one's own interests for the sake of another's or some higher law. Construed as such, "ethics" is something that Mother Teresa, Gandhi, or perhaps Oscar Schindler had, but by definition not something that could ever be found in the general population. Rather, so-called realists like Machiavelli have argued that the average person has to be understood as distinctly unethical: "…one can say this generally of men: that they are ungrateful, fickle, pretenders and dissemblers, evaders of danger, eager for gain."[1]

On this view, shared by more than a few modern theorists of politics, it is a liability to grant too large a place to ethics in one's account of the functioning of society.

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[1] N. Machiavelli, *The Prince* Trans. Harvey C. Mansfield, Jr (Chicago: University of Chicago Press, 1985), 61.

As Machiavelli so eloquently remarked, "…it is so far from how one lives to how one should live, that he who lets go of what is done for what should be done learns his ruin rather than his preservation."[2] Sure, it might be useful to *appear* "ethical" on certain occasions, but genuine ethics are rare because they hold people back from their true interests. "If men were angels," observed Madison in *Federalist 51*, we would not have the problems of government that we do. So, on one read of the realist tradition (although Machiavelli alone could certainly be read in other ways[3]), since ethics is (at best) the province of a saintly few, it can never play a systematically important role in society.

Another common view is that "ethics" deals with a small set of particularly difficult choices about right and wrong that most people will never be in a position to have to deal with. On this account, genuine ethical dilemmas are very rarely encountered. We can formulate them in the abstract, with questions like "should you push an innocent fat man in front of a train if that will prevent three innocent skinny people from getting run over further down the track?"[4] But plainly these are improbable as actual scenarios.

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[2] Ibid.

[3] Ruth Grant has called attention to the inexorable place of hypocrisy in political life and suggested a slightly different concern raised by Machiavelli, namely that a committed "moralist" can be ruinous to good politics because of his or her inability to make pragmatic concessions that involved abandoning high moral ideals. Ultimately this a sort of "ethical" concern with the ruinous consequences of unyielding ethical convictions, which mirrors the converse concern about "immoral" ethical convictions. See Ruth Grant, *Hypocrisy and Integrity: Machiavelli, Rousseau, and the Ethics of Politics* (Chicago: University Of Chicago Press, 1997).

[4] Such "trolley car" dilemmas pervade recent scholarship in analytic and experimental moral philosophy and are intended to provide a way to examine utilitarian and deontological intuitions.

Moreover a general feature of such thoughtfully concocted dilemmas is that they really do seem hard to resolve, since decent reasons can support all sides. So, ethical controversies can characteristically appear *both* irresolvable *and* not particularly important. There may be certain lines of work in which such dilemmas do genuinely arise on a recurrent basis, such as in medical experimentation on humans. But it makes sense to delegate debates in these fields to experts who understand the complex facts and can bring sophisticated philosophical resources to bear. However, trying to concern everyone with "ethics" makes about as much sense as teaching tropical islanders how to evade polar bears. The training isn't easy to begin with, and can be reserved for those special few who will ever go on arctic expeditions. From a less glib perspective, Rousseau raised something of a similar objection to those in his day who wanted to interject particular intellectual disputes into public discourse. "There is something inhumane," he remarked, "about troubling peaceful souls, and distressing men to no purpose, when what one is trying to teach them is neither certain nor useful."[5] Real ethical dilemmas are just not that relevant to everyday life, and trying to concern people with them is a waste of resources, possibly leading to great confusion.

Related to both these views is a third, which conceives of ethical notions as distinctively theoretical and of little consequence to the way people feel and act. We may

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[5] Rousseau, "Letter to Voltaire" in *Discourses and other early political writings*, Ed. Victor Gourevitch, (Cambridge: Cambridge University Press, 1997), 244. It is important to note that Rousseau is, of course, absolutely not someone who thinks ethical convictions are irrelevant to social life.

have ethical opinions "in our head" as to whether to push the fat man in front of the train, but who knows what we would actually do if confronted with a similar dilemma in real life? In its extreme formulation, this separation between the theoretical opinions one endorses and the way one actually behaves has little to do with what was traditionally called "weakness of the will" – namely, the difficulty people might sometimes encounter in living up to higher ideals. Rather, the extreme formulation posits a psychological separation between our "beliefs" and our "actions." Ethical opinions are one matter, ethical behavior another.[6] The fact that being a professor of ethics is not necessarily correlated with being a good person is sometimes offered as an empirical verification of this account.

However, over the last century philosophers of human action have offered us the strongest reasons for rejecting the extreme formulation of this view as incoherent, and have also provided many resources for thinking about the complex relationships between beliefs and actions. Much controversy remains, of course, particularly in light of investigations taking place today at the intersection of "mind, brain, and behavior." However, the view of ethical opinions as detached from feeling and action is antithetical to the way the term "ethics" is used in this project. Likewise, accounts that construe ethics as the distinct province of elite minorities, or an empirical claim about the prevalence of "selfless" behavior, or generally irrelevant attitudes toward unrealistic

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[6] Brennan and Lomasky have for example argued in *Democracy and Decision* (Cambridge, Cambridge University Press: 1994) that there can often be total split between opinions and behavior.

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questions are all deeply at odds with the understanding of ethics employed here. How, then, is ethics conceived in laying out this study?

Ethics, I would like to claim, concerns our evaluative judgments about first order goods. This account is closely tied to an understanding of human action and human rationality that sees people as characteristically acting to achieve certain ends, or goods. Some ends of action may be pursued only as means to further ends, as when one buys a subway token, otherwise worthless in itself, as a way to secure transportation home. We are familiar with such instrumental judgments, which concern how to best achieve the ends we have in mind. But what ends ought we to pursue in the first place? This is a question about what are here termed "first order goods." These are the goods that provide ultimate reasons for action, for the sake of which instrumental reason is employed.

It is important to note that first order goods need not be "higher goods" in any normative sense. Pleasure is a perfectly coherent candidate for a first order good. Also, there are clearly *many* candidates for first order goods, from ideals to sensations, which could in turn be interrelated in various ways. And on the level of practical reason, one "action" could at the same time both realize a first order good and be instrumental to another, as when I eat a sandwich both because it is a source of pleasure in itself and because it instrumentally promotes my health.[7]

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[7] And since these two goods are separable, we could analyze tradeoffs between these goods by asking whether I think the pleasure of certain foods is worth their damage to my health, or whether I would be willing to consume various nutritional supplements that are unpleasant and distasteful but promote my health.

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This account does posit a strong connection between ideas and behavior and takes for granted Alasdair MacIntyre's claim that "Every action is a bearer and expression of more or less theory-laden beliefs and concepts."[8] Thus it makes sense to ask of any human actions what implicit beliefs about goods lie behind them.[9] However, as MacIntyre has emphasized, identifying and evaluating the goods for which we act is first and foremost a practical, not a theoretical, ability.[10] Someone may lack the fluency or skills necessary to provide an articulate, conceptual account of his or her reasons for action, but still be able to discriminate between goods in practice and to act accordingly. What is important to see is that human actions implicitly endorse certain judgments about what is better and worse. In this obvious sense, the valuations people have of different goods are indispensible for an account of their behavior. *Judgments about goods are expressed in actions.*[11]

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[8] Alasdair MacIntyre, *After Virtue* (Indiana: University of Notre Dame Press, 1984), 61.

[9] Of course there are a range of human responses that require little or no conceptual mediation, particularly autonomic reflexes, like those which cause us to yank our hand off a hot stove. As such these don't qualify as genuine human action as it is being discussed here. The crucial criterion is that human actions are those over which we can exert reflective control. As a test case, breathing generally does not qualify, although it can when it becomes a matter of intentional focus and conscious manipulation or a particular expression of unconscious desires/fears.

[10] Alasdair MacIntyre, *The Unconscious: A Conceptual Analysis, revised edition*. (New York: Routledge, 2004),15.

[11] This is similar to the concept of "revealed preference" in economic theory (see Samuelson, "A Note on the Pure Theory of Consumer's Behaviour" *Economica* (1938): 61-71.) - although, MacIntyre is making a claim about the nature of practical rationality, rather than trying to supply a method for imputing a particular utility function (which is how many economists hope to use the concept of revealed preference).

Of course, acting to achieve certain goods and reasoning as to how best to achieve them is common not only to humans, but to various animals as well. What seems to be distinctively human, however, is the ability to entertain and evaluate various different ends. So, MacIntyre argues that for human children the most important transition in their development towards rational agency occurs "when a child becomes able to consider the suggestion that the good to the achievement of which it is presently directed by its animal nature is inferior to some other alternative good and that this latter good therefore provides a better reason for action than does the good at which the child has been aiming."[12] On this account, it is a feature of rational maturity to be able to recognize when one has been wrong, not only about the facts of some instrumental calculation, but also about the ultimate desirability of certain goods. Bracketing for the moment questions about what could constitute rational warrants for such transformations in judgment, it seems undeniable that they occur all the time as a matter of fact.

This account also claims a close interrelation between judgments, perceptions, feelings, and actions. These need not work together in harmonious unity, but judgments held with enough conviction color not only the way we see the world, but also the desires, feelings, and passions that form the motivational fabric of action. In light of the large variety of ends towards which human desires can be transmuted, ethics indicates, in part, our attempts to distinguish merely apparent goods from genuine goods, base desires

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[12] Alasdair MacIntyre, *Dependent Rational Animals*. (Chicago: Open Court,1999), 56.

from enlightened desires. And, on the stronger versions of this account, coming to be persuaded in matters of judgment may also effect a transformation of desire. The details of such a relationship are highly controversial, but substantiating them is beyond the scope of the present study. The most important feature of ethics as it is construed here is simply the claim that there are a large variety of apparent goods that may serve as ends of human action and that we can, under certain circumstances, subject these goods to critical reflection in an attempt to establish whether or not they are in fact genuine goods.[13] Moreover, being persuaded of something's genuine goodness influences the motivational fabric of our lives and moves us towards action. Thus, judgments about what we consider "good" play an indispensible role in shaping human behavior. However, much remains to be said about what constitute "genuine goods" and how we could hope to know them as such.

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[13] The term "genuine goods" masks an important ambiguity here, which could be exploited in very different directions. Most simply, it could be taken in a subjectivist (is this authentically my own good, what I "really" want?) or an objectivist (is this in fact what I should want, what is good for me given the kind of being I am?) way – in either, the ethical valence remains. The details are explored at greater length later in the study.

1. Aims, Scope, and Implications

1.1 Introduction

This study is concerned with the nature of ethical convictions and ethical persuasion and the relevance of both to human society. It aims to show not only *that* ethical convictions are crucial to the development and maintenance of social institutions, but also *how* they are important. The investigation addresses two particular aspects of this "how."

First, it engages the question as to how an understanding of the importance of ethical convictions complements the best knowledge that our social sciences provide. Or, put another way, how is this insight into the social importance of ethical convictions useful? Part of the argument here involves illustrating the methodological limits of various analytic approaches in social sciences that bracket off the distinct behavioral import of ethical convictions and questions about their development. We might describe this first set of arguments as being about the *contribution* of ethical commitments to social structure and the investigative methods appropriate to recognizing this contribution.

Second, this study explores what makes the development of ethical convictions a unique enterprise in and of itself. This section concerns the *constitution* of ethical convictions. Such convictions, it argues, characteristically take shape conceptually from "first person" perspectives and concern what we can broadly call the desirability of first

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order goods. Ethical convictions are difficult to reduce to non-cognitive factors, although such factors clearly influence them at the margin. The constitution of ethical convictions raises deep questions about the nature of human rationality, the epistemological foundations of ethics, and the constitution of human nature itself. This study cannot hope to definitively resolve these questions; but in light of considerations about the relationship between human rationality and human nature, a strong case is made that the constitution of ethical convictions will always take place within conceptual frameworks and that changes in ethical convictions will generally involve engaging in various forms of persuasive arguments concerning conceptualizations of goods.

A corollary follows from these two sets of inquiries. If ethical convictions make important contributions to social structure, and ethical convictions are themselves constituted within evaluative conceptual frameworks, then the maintenance and development of social institutions will depend in some crucial respects on our abilities to persuasively sustain and spread certain evaluative perspectives. That is to say that preserving many of the institutional features of society that we most appreciate, as well as resolving social problems that have so far proved intractable, may depend on our ability to engage at a conceptual level in persuasive dialogue about the desirability and interrelation of certain fundamental goods. And, so, many social ills may be impossible to resolve without such engagements. In such cases there would be no "positive" or "causal" technological expertise that could engineer a desired solution. Some social

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"improvements" may only be obtained by persuasively transforming the ethical convictions that people hold.[1]

At the outset, it is important to qualify a number of these theses. They will be unpacked at great length in the following chapters, but lend themselves to certain ambiguities that are helpful to clarify in advance. First, this project does not defend an exclusive priority for so-called "hermeneutic methods" over social scientific methods for the purposes of studying and changing human society. Many critics of social science in the last century of debates over the respective merits of the so called "*naturewissenschaften*" and "*geisteswissenschaften*" have seen methodological approaches as an either/or question. Either the human world is a linguistically, culturally, and therefore contingently structured phenomena for which interpretive methods are the *only* path to understanding and change, or the human being is a creature of the natural world for which scientific techniques hold the *only* promise of definitive knowledge and control. Both these alternatives are likely mistaken for reasons explored in the second and sixth chapters of this study.

It is true, as a number of people including John Searle have pointed out, that all "institutional facts" are only facts because they are interpreted as such.[2] They don't exist as possible objects of empirical study outside of the interpretive framework of human language. Money, for example, is only money, if people interpret it as money. But this

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[1] Of course, there may be technological knowledge regarding how to best create and structure conditions for productive engagements of this sort. This will be the subject of much of chapter seven.

[2] John R. Searle, *The Construction of Social Reality* (Free Press, 1995).

insight does not preclude the usefulness of various sorts of analytic approaches that treat money as an "objective" or "natural" variable. Precisely because human language is always something essentially shared, such that meanings are not open to contingent, private, wholesale revision, facts that are only "interpreted" as facts are facts nonetheless. So, the truth that the human world is always a conceptually interpreted world is often not an impediment to the useful application of scientific techniques for understanding the systematic relationship between social "variables" of interest.

So, this inquiry admits quite happily that investigations in the social sciences can provide much useful knowledge, and indeed have done so, without any need to approach their objects of study through first person perspectives of meaning and value. The question, as some have construed it, about the epistemological "possibility" of social science is moot. The social sciences have demonstrated themselves to be too useful for that sort of question to make sense. The real question concerns the reasons for the limits of social sciences – the sorts of problems they find most difficult to address – and whether ethical persuasion, if it is indeed possible, can enhance and complement our attempts to deal with such problems.

Thus, this project is about understanding ways in which to fruitfully integrate knowledge gained from the social sciences with tasks that can only be undertaken from perspectives and with conceptual tools that lie outside their scope. It is about the ways in which our social thought and our attempts at social change, dominated as they are by social science approaches, can be enriched by understanding the unique role that ethical

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convictions play in social life and the persuasive possibilities for rationally engaging and transforming such convictions.

The preface laid out a working definition of ethics, at the heart of which lie evaluative judgments about goods, judgments that are characteristically formed within first person perspectives that find expression in socially shared conceptual frameworks. The first two chapters explore a number of unresolved problems in social thought, including challenges that social sciences have been particularly ineffective in dealing with, as well as persistent conundrums in political theory. In the middle chapters of this project I illustrate how three related methodological attempts within the social sciences to address these problems come up short and how these shortcomings relate to dilemmas concerning the nature of rationality and ethics. In the final chapters, I defend and elaborate an account of ethics that is able to address these dilemmas. I then proceed to suggest the implications of this account for certain problems that social scientists have found intractable. I aim to show how this richer understanding of ethics, which is not itself amenable to scientific reduction, can profitably complement our best social science – both by identifying certain problems for which ethical persuasion is a necessary component of an adequate resolution and by suggesting how institutions can be structured to support conditions for rationally productive ethical formation and dialogue.

Very briefly, the analytic approaches I examine are statistical modeling, formal modeling, and biological-behavioral research. Each of these methods is useful for addressing certain sorts of questions under certain sorts of conditions. However, these

methods also have inherent limits. Statistical models work well for uncovering regularities in stable contexts, but cannot predict the type and scope of novel, innovative behavior. Formal models take an agent-based approach that is useful for predicting strategic behavior when goals and rules are well defined. However, such models trade exclusively on instrumental rationality, and their applicability is severely restricted by the fluidity of "preferences" and "rules" in the real world and the complicated nature of human psychology. Increasingly, however, biological research hopes to make the human mind scientifically tractable and to uncover biological determinants of human behavior. Research on this front is fascinating and has lead to many useful discoveries. However, whatever the ultimate relationship between the mind and brain, a detailed review of this literature suggests we have the strongest reasons to expect that the human mind will remain constituted by concepts, ideas, and beliefs– and that these form the substance of the ethical convictions people hold.

My account of the limits of these methods naturally leads to some deep questions about the nature of human rationality, the conceptual constitution of beliefs, and the relationship between theory and practice. Drawing on the work of Alasdair MacIntyre, Charles Taylor, and Hans-Georg Gadamer I defend an account of ethics which understands ethical convictions as socially formed, conceptually mediated, and possibly open to rational change through persuasion. According to this account, at the heart of ethics lie judgments concerning the nature and hierarchy of various goods, judgments that find expression and defense in traditions of reasoning that we both inherit and innovate.

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In an extended discussion of naturalism and ethics, I acknowledge ways in which our biology is undoubtedly a starting point for reflection about such goods, but also contend that biological knowledge is unable to resolve our numerous ethical disputes. Moreover, I argue that the extensive focus on instrumental rationality in the social sciences clouds our understanding of the importance of debates about "first order goods." Also, the frequent conflation of rationality with science compounds the difficulty we have in conceiving how ethical disputes, or indeed any questions concerning "value," can aspire to be a rational enterprise if they are not "scientific." These and other considerations lead me to reject increasingly influential accounts of ethics that view ethical convictions as biologically hardwired (for which any diversity in convictions is attributed to underlying biological variation or to "ideological diseases" that humans are disposed to contract a-rationally from surrounding social structures).

In contrast, MacIntyre, Taylor, and Gadamer provide compelling resources for thinking about the character of rational persuasion in ethics and the conditions that support rationally defensible inquiry. The account of persuasion that emerges from their work and which I wish to defend is similar to that mapped out by Bryan Garsten at length in his recent book, *Saving Persuasion*. As Garsten explains, "Persuasion in the strict sense identifies a way of influencing that is neither manipulation nor pandering…To truly persuade people is to induce them to change their own beliefs and desires in light of what has been said."[3]

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[3] Bryan Garsten, *Saving Persuasion* (Cambridge: Harvard University Press, 2006), 7.

Having 1) established the importance of ethical convictions for questions of human "behavior" and 2) suggested that ethical convictions are conceptually constituted in ways that are open to rational engagement and persuasion, I turn in the final section of the dissertation to some concrete illustrations of the relevance of these conclusions for particular problems in social science.

In particular, my conclusions have implications for two broad areas of social concern, namely, the negotiation of political differences within Western liberal democracies and the political-economic development of the third world. It is useful to sketch out in advance the way in which I believe my explorations of ethics and persuasion can enhance our social thought in both of these areas.

1.2 Ethics and Persuasion in Liberal Democracies

One of the central problems of modern politics has been the problem of how to negotiate the peaceful and productive coexistence of people who have very different conceptualizations of the good. The problem with diverse conceptualizations of the good is that they tend to place their adherents in conflict and deprive conflicting parties of common standards of evaluation to which they might appeal to settle such conflicts. Throughout the modern period a number of strategies have been proposed and explored to deal with this problem, the potential dangers of which were dramatically displayed in Europe's "wars of religion." At the heart of many of these strategies were different conceptions of human reason.

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The great hope of many thinkers associated with the so-called "enlightenment" was that abstract reason could provide a demonstrative basis for social organization and political morality. Since reason is universal, everyone could legitimately be held accountable to the morality of reason. Moreover, the universality of reason ensured equality in the access people had to moral truth. Because it was thought that right reason necessarily persuaded, or more accurately compelled, our assent, being subject to the dictates of reason could also be consistent with freedom. This is a loose sketch of ideas articulated by figures as such as Kant and Condorcet, and these ideas have animated more contemporary accounts of "high liberalism," such as one finds in the early Rawls.

However, the great challenge to enlightenment moral reason is the historical fact that accounts of what right reason require have been as diverse as conceptions of the good. Proponents of high liberalism are thus often confronted with the uncomfortable reality that their reasons are not persuasive to many of their fellow citizens, who presumably must be thus judged fundamentally irrational. Also, it is the case that more recent philosophical currents, including what is often described as the "historical turn," have undermined at an epistemological level many of the hopes and aspirations of enlightenment reason. On the other hand, there is no doubt that appeals to natural rights, human rights, justice-as-fairness and other ethical ideals indebted to higher views of reason have proved to be powerful sources of ethical motivation for many in the horizon of modern politics.

Another strategy for dealing with ethical pluralism, notably indebted to Hobbes and Hume, despaired of the ability of reason to bring us to shared conceptualizations of

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the good. Reason on this account is merely instrumental, "the spy and scout of the passions." Although such reason cannot help us adjudicate between different values, it could still be useful for politics. What instrumental reason can grasp is that in order to achieve whatever goods we may have in mind we generally need the cooperation of others. Thus self-interest of various kinds can underwrite a minimal political morality, if incentives are properly aligned and understood. Hobbes saw that, compared to the brutal anarchy that results when everyone indiscriminately pursues their own goods, ceding power to a sovereign authority that provides social stability through force can make long term sense for the satisfaction of individual desires. However, in this scheme those desires remain unchanged. Society functions not because of common evaluative judgments that motivate harmonious relations but because of the threat of violence, which realigns instrumental strategy.

It is difficult to imagine a society in which force does not play any role. However, the essential question for political theory is what justifies force, and such justifications are characteristically linked to goods that deserve to be defended. For Hobbes, however, there are no goods beyond the subjective desires of individuals and none of these desires deserve to be prioritized over others. There may be a political calculus that prioritizes some desires over others in order to secure an order in which a greater satisfaction of preferences can take place. However, in so far as any "good" provides a rationale for such a society it is a something like the utilitarian good of a greater quantity of satisfied preferences – a good people are persuaded of in so far as it advances their own existing desires.

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The Hobbesian solution, it is often noted, has many equilibria, including forms of tyranny that are only marginally better than violent death. In the cost-benefit calculations of instrumental reason it is clear why violence is generally so effective for motivating behavioral compliance. However, the promise of structuring society exclusively through instrumental reason, abandoning efforts at "preference change," is limited. I will discuss these limits at greater length in my examination of development economics, but, in brief, violence is not only ethically suspect, but is also an extremely costly means of social control.

Other social contract theorists have built upon on instrumental accounts of reason, while objecting to Hobbes's assumption that diverse conceptualizations of the good necessarily lead to conflict. A rosier picture of our natural ethical dispositions and the harmony of human interests developed out of the Scottish Enlightenment. Figures like Mandeville and Smith sought to show how, in the first instance, private vices could lead to public virtue and how universal human sentiments naturally disposed people to peaceful and mutually advantageous market exchange. Of course, such assurances would ring hollow to those in embroiled in total war.

There is a deep insight, however, in the Smithian perspective. If indeed the most fundamental "moral sentiments" in a society were sufficiently social then other differences could be the subject of bargaining and perhaps even mutual gain. The Hobbesian fear of a "war of all against all" is most warranted in situations where fundamental conceptions of the good are radically divergent. The lesson of much political theory in the modern period seems to be this: the degree to which a society must ruled by

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force is proportional to the level of fundamental disagreement about conceptualizations of first order goods amongst its citizens.

As a matter of historical record, the stability of Europe in the wake of the wars of religion was made possible neither by instrumental force nor universal reason alone. To these was added a strategy of political separation and containment, which sought to minimize latent conflict by dividing peoples according to their dominant conceptions of one of the chief goods, in this case, religion. The "Westphalian system" by no means ended conflicts, but it reconfigured the political geography to separate peoples with deep ethical convictions that led to conflict. It is not surprising then, that modern political theory has often been suspicious of deep ethical convictions, seeing them less as a source of social virtues and more as threats to peace.

The concept of toleration, which has become so central to modern liberal theory, represents a sustained attempt to work out both what kind of ethical difference is acceptable in Western societies and what sort of ethical commitments are legitimate for modern states to require. For Locke, toleration meant relaxing the demand of religious uniformity in England, a policy for which he provided both theological and practical arguments. Chief amongst the latter was the claim that enforcing religious conformity was itself a cause of civil unrest. The state does not have the right to ask of its citizens that they all profess a certain religion, and when it does it puts those who hold other religious convictions in unnecessary enmity towards the state. Crucial to Locke's argument was the claim that many of the demands of religious uniformity in fact bore no relationship to being a good citizen. Protestants of various stripes could be good citizens,

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and tolerating them would diffuse social conflict. Catholics and atheists were another matter, since their respective allegiance to a foreign power and inability to take oaths were subversive of the political order.

Toleration thus conceived was not an open door to all manners of difference, but a calculation about what sort of ethical convictions were essential to a political order and about which convictions, although perhaps contentious and misguided from various perspectives, could nonetheless be "tolerated," however reluctantly. Moreover, many religious faiths came to see tolerance as theologically important, since "faith" that was coerced might not be genuine. Toleration was also, as noted, a way of exploring the limits of the state's competence to pronounce on matters of fundamental controversy such as religion. Debates about the relationship between church and state persist today with great passion, but terms of these debate have changed dramatically from Locke's time, to which very few would like to return. However, analogous questions surfaced in the context of campaigns of ideological indoctrination in the 20th century, which saw the Soviet Union and Maoist China pioneer official state philosophies and re-education programs while persecuting dissenters. It is not difficult to understand the perceived utility of ideological conformity from the perspective of a totalitarian leader. Indeed, there is a latent temptation in all politics for those in charge of social order to want either to control the dominant ethical convictions or suppress their ungoverned sources in civil society.

Western liberal democracies have defined themselves in opposition to the totalitarianisms of the 20th century, and this has entailed a commitment to toleration

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conceived both as a limitation on government and an openness to ethical pluralism. *Democratic* institutions have been understood as a way to give expression to diverse ethical commitments in the political arena in the process of arriving at determinate political judgments. At the same time, overarching *liberal* frameworks supposedly provide non-negotiable ethical foundations for the political order, under the guise of human rights and other basic freedoms. Perhaps the most central dialectic in contemporary political theory concerns whether liberalism or democracy should have priority.

In the context of these debates it is remarkable, at first glance, how widely accepted basic liberal and democratic principles are in the West. They are endorsed in various ways and indeed interwoven into many different ethical frameworks endorsed by Western citizens. However, whether to prioritize liberalism or democracy is a question typically posed in reference to more extensive liberal or democratic conceptions than those that are already expressed in the political status quo.

The paradigmatic weakness of more extensive versions of liberalism is the inability of liberal reason to persuade those who are not already committed liberals. In the absence of such persuasion, liberal politics will appear an illegitimate imposition. The standard critique of Rawls' early work was that his ethical convictions were built into the concept of reason he invoked. Rawls' turn towards "political liberalism" was a turn toward a liberalism that was more concerned that it actually be persuasive to others, rather than rationally justified in its own mind. Similarly, Jurgen Habermas's development of "discourse ethics" and Amy Gutmann's account of "deliberative

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democracy" were attempts to secure more extensive liberal ethical commitments through processes of persuasion. These projects continue to be criticized for writing liberal assumptions into the terms of the deliberative debate.[4] However, the fact that liberal theorists increasingly think of the justifications of liberalism in deliberative-persuasive terms is a significant shift. Still, the focus has been on procedure, justification, and legitimacy.

Liberal theorists have been responding in part to challenges from democratic theorists who view liberal reason as hegemonic and insufficiently sensitive to the realities of ethical pluralism. Radical democrats such as William Connolly and Sheldon Wolin have articulated and endorsed a vision of "agonistic" politics, rooted in local, particular interests and ethical commitments. They would like to see more possibilities for collective action than permitted by entrenched liberal frameworks that limit the scope of majoritarian decisions and local activism.

Indebted in various ways to Rousseau, radical democrats wish for a politics that would be dangerously chaotic and unprincipled in the eyes of many liberals. Moreover, liberals are particularly wary of the outcome of democratic procedures in countries without liberal frameworks or traditions, such as many in the present Middle East. Closer to home, liberals worry about the effects of mass immigration from non-liberal societies. If democratic action is endowed with too much power, it is feared that anti-liberal communities might subvert liberalism from within.

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[4] By, for example, Michael Walzer and Seyla Benhabib.

These are the poles of debate in much contemporary political theory. The problem of how to order a society in which there are diverse ethical convictions and the question of what sort of convictions must be excluded are both very old issues, which have been addressed through variations on the strategies outlined above. The present configuration of liberal and democratic theory tends to consider these issues in light of questions of justification – what structures of deliberation would legitimate a more extensive liberalism, what democratic procedures would give appropriate voice to diverse concerns?

However, my interests in ethical convictions and persuasion do not concern justification as much as motivation, although the two are related. One of the major criticisms of modern liberal democracies is that they rely on ethical foundations that the liberal state itself cannot supply and may even undermine. One finds some suggestions of this thesis in Tocqueville, but it has become most firmly established through contemporary political science literature on "social capital" as articulated by Robert Putnam and others. Recognizing that our social order depends in crucial respects on ethical convictions that derive from sources that are beyond politics, many have been concerned to better understand these sources and revivify them through both private efforts and public support.

The sociological literature on this topic confirms many claims with which the theoretical perspectives of MacIntyre, Taylor, and Gadamer would be sympathetic – that ethical convictions develop in the contexts of practices and ideas communicated by families, churches, schools, private associations, artistic productions, political rhetoric,

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TV and movies, news media, and so on. This has led to proposals to grant these institutions greater autonomy, or even to subsidize them, in the hope of generating more "social capital." However, such proposals have been met with two related concerns. First some question whether it is proper for the state to support, directly or indirectly, the development of thicker ethical attachments amongst its citizens. This question is often animated by an aspiration in liberal theory that would have the state be formally neutral towards thick conceptions of the good. Second, there is a concern with the fundamental indeterminacy of such support. Granting greater autonomy or resources may empower these various institutions in civil society but cannot hope to direct the development of their ethical concepts. Subsidies to educational institutions may support both the Montessori school and the Madrasah; use of public facilities by private associations could benefit both the NAACP and the Ku Klux Klan. Thus the state will lack control of what it has put in motion.

In a very interesting way, the first concern mirrors one aspect of the toleration debate by inquiring into the state's competence to promote ethical allegiances. However, I will argue at length that such concerns need to be qualified by the recognition that it is impossible for the state not to endorse and promote some ethical conceptions over others. The question is not whether it will do so – governance and law cannot but do so – the question is how it will do so and which conceptions will receive the greatest support or censure. The second concern mirrors the larger problems and dangers of democracy explored above. The diversity and autonomy of different ethical conceptions holds both promises and peril for society. The promise is that granting liberty and support to

institutions of ethical formation empowers them to develop and sustain more extensive ethical convictions amongst their adherents. While this may have certain motivational and behavioral benefits for society (or may not), it also entails the danger of increasing conflict rooted in different conceptions of the good. Of course, as noted earlier, how one evaluates these promises and perils will depend on one's own ethical commitments, so these debates will be recursive in an obvious way that we should not lose sight of.

The extreme social control sought by totalitarian states, which severely limited the freedom of private associations and other institutions of civil society, is not an option seriously considered by liberal-democratic societies today. Our view of these matters tends toward positions mapped out in different ways by Mill and Tocqueville, which view ethical pluralism as healthy to society but also want to see its dangers contained. Mill's thought includes a double strategy for such containment. First the classically liberal "harm principle" provides a minimal social morality. Second, Mill argued that public conflict between ethical convictions was epistemically productive – that having to defend one's convictions and hear challenges from others would in fact tend towards the extinction of the worst ideas and the development of the better ones.

Mill's view of reason contains much enlightenment optimism, but in weaker forms it is an essential hope of liberal democracy, namely that rationally productive dialogue and development can take place across different ethical convictions. Indeed, I would like to defend a weak version of this thesis from a philosophical perspective later, drawing on Taylor, MacIntyre, and Gadamer.

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The hope of *democratic* society is a hope about the possibilities of rational persuasion. The point of *liberal* society is that, given the reality of deep ethical differences, there need to be ground rules that channel conflict towards attempts to persuade rather than attempts to coerce others through state power. The political arena is the preeminent space in which rival ethical frameworks come in conflict. The balancing act, which is always itself an ethical predicament, concerns how to support the flourishing of diverse traditions of ethical formation on their own terms, while maintaining a basic, liberal order that constrains serious political conflicts that emerge between rival ethical conceptions.

Again, we must understand that this very ideal of a liberal-democratic society is itself a powerful ethical conception. I will explore at greater length how the particular account of rationality and ethics that I want to defend can help us better negotiate the balancing act between liberalism and democracy. At this stage, however, I want to highlight an ethical feature of liberal democratic societies that often goes unnoticed, namely the near universal support found amongst Western citizens of all ethical stripes for the constitutional forms of liberal-democratic regimes. One of the most persuasive ethical ideals in the modern era has been the idea of liberal-democratic society itself, conceived as a system of popular representation that protects various rights and liberties. Although there many debates about how to conduct the balancing act, there are firm boundary conditions placed on politics and expressed in legal, constitution, and ideological terms. These "stick" precisely because they are so widely accepted and endorsed as good.

Thus, I want only to highlight the rather basic claim that liberal-democratic societies can function as they do because of such widespread ethical commitments amongst their citizens. Moreover, this affirms the importance of our ongoing ability to articulate compelling reasons for diverse ethical traditions to endorse a common ethical judgment about the desirability of a basic, liberal political framework – one which rejects political violence, protects certain liberties, and places limits on what can be sought through politics. I will call this the "liberal-constitutional ethic." It is often framed in the language of an overlapping consensus, but in fact I suspect the reason that citizens from so many diverse ethical perspectives endorse it has more to do with Judith Shklar's arguments for a "liberalism of fear." Recognizing the depths of our ethical disagreements with fellow citizens and cognizant of the dangers of political power, nearly all citizens have found persuasive reasons to endorse a minimal liberal framework. Whatever reasons ultimately convince, continuing to persuade citizens to endorse a minimal liberal framework is an important project for maintaining the stability and order of liberal democratic societies. That particular ethic ensures that diverse and rival traditions of ethical formation can be supported and flourish without the fear of civil war. Moreover, requiring the liberal-constitutional ethic of those who seek citizenship is a natural conclusion of the ethic itself, as are attempts to persuade non liberal societies of its desirability.

These thoughts foreshadow, at least, how I envision the arguments advanced in this study contributing to and drawing from debates in contemporary political theory. I would also like to draw brief attention to another set of claims about the nature of liberal

democracies concerning the contributions that social sciences can make to them. Some social scientists may have little interest in the problems of political theory mapped out above. Perhaps ethical convictions matter for esoteric debates about the justifications of liberal democracy, but the problems of behavior, conflict, and social order are something that can be adequately and independently managed by social scientific knowledge.

In opposition to this assertion of the self-sufficiency of social scientific knowledge, I will argue that much of the social structure and behavioral predictability that the social sciences profitably discover is ultimately derivative of certain overarching frameworks of value that are individually held but socially expressed (configurations of incentives and power being endogenous to such frameworks). Many of the most socially significant evaluative frameworks within liberal-democratic regimes are firmly rooted, and social scientists can do useful research without having to worry about radical changes in this underlying structure. However, realizing that much social science research is contingent on deeper frameworks of value that are culturally and historically specific is important for understanding why the findings of some research may not travel intact to other times and places. Also, this recognition illuminates an important dimension of human agency.

The fact that so many aspects of our lives are predictable and open to certain kinds of manipulation may lead some to conclude that human agency is devoid of any reality or significance. However, this would be to confuse freedom with indeterminacy. If, in fact, human agency is intentional, then human activities will have structure. This structure will derive from the characteristic wants, motives, and strategies that people

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pursue. Moreover, knowledge of peoples' objectives, goals, and habits will prove useful for incentivizing or dissuading certain actions.

Although human agency is exercised in a host of discreet choices, and some of these choices demand complex judgments, agency is also exercised in the development and revision of our evaluative convictions. Persuading one another of such convictions is an important aspect of human agency. Thus, a fundamental dimension of social change comes from the development and revision of socially shared ethical convictions (although there are certainly other dimensions to change driven by technological innovation, institutional reconfiguration, natural disasters, etc.). The importance of attending to the ethical dimension of change is well illustrated by challenges encountered in efforts to develop the third world.

1.3 Ethics and Persuasion in the Developing World

"New Institutionalist" research in development economics provides an example of how recognition of the nature and import of ethical convictions can profitably complement our best social science. In order to function as they do many institutions depend on widespread ethical convictions; and, of course, institutions can themselves be important vehicles for teaching, practicing, and challenging such convictions. Case studies examining problems of violence, exploitation, and constitutional governance in the developing world illustrate instances where ethical persuasion can be enormously helpful and, in so doing, point towards the substantial ethical judgments at stake. Consider, by way of introduction, an instructive example of how ethical convictions are

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relevant to the creation and maintenance of social institutions: the problem of violence in developing countries.

This problem is particularly important for understanding difficulties encountered in developing political-economic institutions in the third world. Despite concerted efforts by Western countries and transnational organizations over the last 40 years, including the targeted investment of some 2.3 trillion dollars[5], much of the developing world remains mired in poverty, despotism, and violence. Why have powerful interventions, backed by the best analysis social scientists have to offer, not been more effective in changing this tragic landscape?

As the story is now often told, for a long time development projects adopted a macro-economic approach focused on so-called growth variables and investments in infrastructure.[6] It was widely believed that poverty was the root of all evils and that the development of markets and economic growth would alleviate the political and social problems of despotism and violence. However, the macro-economic approach met with limited success, and in a number of contexts ironically lent support to violent and corrupt regimes. The failures of many development programs have been widely documented in such books as Deepak Lal's *The Poverty of Development Economics*, Mark Calderisi's

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[5] William Easterly, *The White Man's Burden* (New York, NY: Penguin, 2006), 4.

[6] Or at least this is how things really got moving in the post WWII economic approach to development. There had been an earlier interest "cultural endowments" and "institutional development," but these research agendas were abandoned as economics promised to be better able to formalize policy analysis and planning. See Vernon Ruttan, *Social Science Knowledge and Economic Development* (Ann Arbor: University of Michigan Press, 2003).

The Trouble with Africa, and William Easterly's *The Elusive Quest for Growth* and *The White Man's Burden*.

There are several reasons that these projects were not as successful as had been hoped. Part of the story has to do with the methodological limits of macro-economic analysis when dealing with wide variations in local contexts and individual level incentives. But a more fundamental problem was that the purely economic approach had things backwards. Rather than simply being a symptom of underdevelopment, despotism and violence prevent the formation of flourishing markets in the first place. So, increasingly, experts came to understand that political, economic, and social problems go hand in hand. Among the most influential and insightful articulations of this view came from the Nobel Laureate Douglass North and his students in the "New Institutionalist" school of political-economic analysis, including Avner Greif and Barry Weingast. They all agree that markets are crucial for economic growth, but they show that the real puzzle and challenge concerns how to establish markets in the first place.

For markets to function there have to be institutions like property rights, norms of fairness, a capable police force, and effective courts of law. Such institutions are also key for constraining violent crime. This is not a coincidence. As Avner Greif has argued in a seminal paper, the rise of Western economies depended fundamentally on the development of two kinds of institutions, namely what he calls "contract enforcement"

institutions and "coercion constraining" institutions[7]. And, in fact, Greif goes on to suggest that "coercion constraining" institutions are often the most important and historically prior to all others.

His argument is straightforward. We are familiar with the importance of contract enforcement mechanisms. They ensure that when an entrepreneur enters into a deal other parties will make good on their end of the bargain. Courts of law provide this service in most Western industrialized countries, but other informal institutions like trade guilds or reputational networks can accomplish the task as well. However, according to Greif, utilizing any of these institutions presents a problem, because they make public revelations about one's wealth. This information can then be used by those with coercive power to identify and expropriate that wealth. In short, violence pays, and the information provided by many contract enforcement mechanisms makes violence even more lucrative. Thus Greif argues that the fundamental problem of development economics concerns figuring out how to constrain violence in the first place.

The extensive work of the development economist Paul Collier, outlined in his best seller, *The Bottom Billion*, confirms this analysis with historical case studies and empirical research[8]. Collier identifies what he calls "the conflict trap" as a foundational cause and effect of third world poverty. This "trap" refers to persistent patterns of violent, internal challenges to governments, as well as state sanctioned violence used to preserve

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[7] Avner Greif, "Commitment, Coercion, and Markets: The Nature and Dynamics of Institutions Supporting Exchange" in *Handbook of New Institutional Economics*, ed. Menard and M.Shirley (New York, NY: Springer, 2005).

[8] Paul Collier, *The Bottom Billion* (Oxford University Press, 2007).

power, and also to organized crime used for economic gain within a regime. Collier notes that "seventy-three percent of people in the society of the bottom billion have recently been through a civil war or are still in one," and civil war is most likely to break out in low-income countries.[9] The sad truth is that in the least developed countries, violence is often one of the easiest ways to make a buck. Collier reports the comments of the rebel leader Laurent Kabila, who explained to a journalist that in Zaire all one needs to orchestrate a rebellion is $10,000 and a satellite phone.[10] In a poor society $10,000 can purchase a small army, and via a working phone rebels can make hundreds of millions of dollars of deals for resource extraction. Collier finds strong evidence that the causal arrow runs both ways—war begets poverty and poverty begets war—and conservatively estimates the cost of a civil war in a developing country at $64 billion.[11] Like Greif, Collier argues that escaping from the cycle of violence is the first and necessary step towards economic development.

Both researchers make related but different proposals for how to escape the conflict trap, searching for strategies to make violence a less profitable enterprise. Collier focuses on "exogenous" forces that can be brought to bear by the international community in the form of military intervention, legal charters, and stabilizing aid. At the centerpiece of this approach is an attempt to have first world countries credibly commit to not doing business with violent factions, and in extreme cases be ready to use military

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[9] Ibid., 17, 19.

[10] Ibid., 21.

[11] Ibid., 32.

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force to defeat them. His prime examples of such responses are the actions taken by the diamond cartel DeBeers to exclude conflict diamonds from the world market and the British invasion of Sierra Leone (in which, after a decade of civil war and extended reign of terror against civilians, rebel groups were defeated in a matter of days by a few hundred British soldiers). However, Collier readily admits that the regions of conflict are too numerous and the challenges of military intervention too perilous for this to be a widespread approach. Still, he argues that more soft power can be employed by the developed world to address the economic foundations of conflict and to support building blocks of a free society like independent media outlets.

Greif focuses more on "endogenous" factors that have enabled salutary balances of power to emerge in various past contexts. He analyzes a number of historical examples that showcase the development of effective coercion constraining institutions: the rise of the *Podesteria* system in 12th century Genoa in which a foreign enforcer would serve as a temporary ally against aggressor clans within the city, the 11th century *Doge* magistrate in Venice, supervised by an elected council but authorized to adjudicate problems of social order, the States General in 15th century Flanders, the independent Italian financiers that provided a check on the Spanish Hapsburgs in the 16th century, the long history of standoffs between nobles and the crown in England, and other examples. Greif scrutinizes the ways in which financial incentives and violence operated in each of these circumstances, and at the heart of his expansive analysis is the claim that "protection is

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afforded…only to those who can retaliate."[12] The coercion constraining institutions he identifies "rely on balancing one's coercive power with either the coercive power of others or their ability to inflict economic sanctions on those who abuse rights. CCIs [coercion constraining institutions] deter abuses of rights by creating the shared belief that attempting to do so will lead to a costly retaliation."[13] In this analysis he follows a long line of political thought since Thomas Hobbes that looks to incentives— in the guise of both threats and benefits— for the foundational account of political life. Hobbes famous dictum, "covenants without swords are mere words," seems to hold true in Greif's analysis.

Both Greif and Collier agree with a longstanding insight articulated by John Locke and others, namely that the "feasible extent of markets depends on protection from coercive power."[14] Both search for ways to guarantee that violence is used constructively, in support of contract enforcing mechanisms and as a preemptive threat against those who would use violence for economic gain. Greif summarizes his position as follows: "Effective CCIs make violence economically productive as it is used to protect property rights from abuses such as expropriation by the state, the ravages of a civil war, and large scale military raids. They secure property rights by discouraging those who can acquire coercive power to abuse rights from doing so, and by motivating those who have coercive

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[12] Avner Greif, "Commitment, Coercion, and Markets: The Nature and Dynamics of Institutions Supporting Exchange" in *Handbook of New Institutional Economics*, ed. Menard and M.Shirley. (New York: Springer 2005), 748.

[13] Ibid., 747.

[14] Ibid., 747.

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power – rulers, the elite, states – to protect rights."[15] For him, the central political problem is about getting incentives right and thus creating balances of power in which those who can inflict violence find it in their interests to use violence in support of a flourishing society rather than to its detriment.[16]

There is surely much wisdom, both theoretical and historical, in this analysis, but it also has its limits. The limits derive from the very nature and meaning of incentives, and the difficulty of specifying and arranging them in good order. Political scientists have tended to think of incentives in straight-forward monetary terms, and clearly they are right that money provides a powerful source of motivation. However, humans possess a psychological constitution that complicates any simplistic account of economic motivation, particularly in reference to questions of violence. As the political theorist Harvey Mansfield has argued, "Politics is about what makes you angry, not so much about what you want. Your wants do matter, but mainly because you feel you are entitled to have them satisfied and get angry when they are not." [17]

Most political scientists today think politics is about power and the conflict of self-interest. This is misleading. What is more important is what people take their self-interest to be and the ways in which they think about power in the first place.[18]

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[15] Ibid.

[16] This is a perspective that can complement very well the claims made by Hernando DeSoto in *The Mystery of Capital* (New York, NY: Basic Books, 2000) that I discuss later.

[17] Mansfield, Harvey. "How to Understand Politics" delivered as the Thomas Jefferson lecture, republished in *First Things* (August/September 2007), 42.

[18] Also, the very way in which people understand, imagine, and think about "the self" may vary widely. See, Charles Taylor, *Sources of the Self* (Cambridge, 1992).

Understanding and changing that, however, is difficult. That is not to deny that analysis of incentives is useful, but what constitute "incentives" in the first place have to be considered in light of larger questions about the first order goods and ultimate ends through which people envision and direct their lives. Few societies are held together simply by the mutual satisfaction of private interests. Rather, most societies are built upon a widespread set of beliefs that link social practices with things that people think are of ultimate significance. Only within this matrix do many particular incentives make sense as incentives.[19] So, what count as incentives may vary a great deal across difference societies and cultures, and will be rooted in larger conceptual frameworks. Changing these frameworks-- that is, changing what is considered desirable in the first place-- may be as important an enterprise as reworking existing incentive networks.

Again, this is not to deny that humans have characteristic, natural desires. Clearly our preferences for food, shelter, and companionship are precisely that. Moreover, Hobbes was certainly right to believe that fear of violent death can motivate masses of people to action. At the margins, threats of violence and inducements of food, medicine, and shelter are clearly powerful tools for reshaping societies on the brink of chaos. However, they are insufficient for developing the vast array of social relations that characterize flourishing societies. Aligning general incentives such that personal interests

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[19] Brennan and Pettit make a somewhat related argument about the way in which psychological benefits/incentives can be essentially social and systematically influential in their recent book, *The Economy of Esteem* (Oxford University Press, 2004).

are not inconsistent with the common good is necessary but insufficient for constraining violence. Two other issues are of paramount importance.

First, people's ultimate goals must not include violent ends. Those who analyze incentives tend to think of violence as simply a means to other ends. However, violence can often be intrinsically intertwined in the substantive goals and activities people pursue for their own sake, as when honor through dominance is highly valued in itself.[20] Moreover, across the developing world one encounters societies in which activities like selling subordinates into slavery, the abuse of women, and sex with children are considered acceptable if not desirable. So, on the level of "preference formation," it is important that various violent practices cease to appear as incentives or ends in themselves.

Second, we must acknowledge that explicit incentive networks can never be exhaustive, that it is impossible to ensure in every instance that it will pay to be good. A system of law backed by a strong executive can help a great deal, but also presents problems of its own. Hobbes famously argued that a powerful and thus unrivaled authority could lay down the law by threatening to punish those that violate it. However, this solution encounters the question of how to ensure that this sovereign power is used for good and not for ill. Any organization powerful enough to maintain law is also

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[20] I mean here to indicate acts of violence, not just the "power" or "influence" that might accrue to those who make threats of violence, whether consummated or not. It is tempting to think that no one really enjoys acts of violence, that they are always means to something else. But this need not be the case; people can go looking for fights for no other reason than they enjoy fighting.

powerful enough to violate it. With this admission looms the possibility of an infinite theoretical regress, as we repeatedly ask, "and who shall guard the guardians?"

One avenue for mitigating this problem was developed in the American Constitution, which attempted to create multiple loci of power that could provide countervailing forces that checked "ambition with ambition." However, the opportunities for such founding moments are rare and the intended equilibrium is precariously vulnerable to historical contingencies. So, what maintains the salutary social structure of Western democracies if not a perfect network of material incentives? Or, to put it another way, why not lie, cheat, steal, rape, pillage, and murder to achieve your ends if you can get away with it? This is a question that applies to both individuals and governments.

The answer, I want to claim, has to do with ethics on two levels. At the most basic level, a vast majority of citizens of Western democracies believe there are certain things one should not do to others – rape, torture, kidnap, murder – even if one stands to benefit from such behavior and could get away with it. For a mixture of cultural/ historical/ educational/ moral reasons people simply take for granted that some things are beyond question, and for that reason these options seldom present themselves as serious temptations.

At a more general level, ethical visions about what is good, true, and admirable also inspire the formation of "personal preferences." So, aspects of ethics – doctrines of right, criteria of good, ideal visions, and commitments to values – provide important determinates of human behavior in ways not reducible to ostensive material incentives.

This is crucial for understanding how political institutions that provide for property, contract enforcement, and security are maintained.

By making the most destructive and violent crimes socially and psychologically unacceptable to a majority of the population and by inspiring the formation of first order preferences that support social virtues, ethical visions help solve the political problem that Hobbes and countless political scientists since him have described. Moreover, the social import of ethical convictions is only magnified when they concern leaders, elites, and others in positions of power.

Ethical visions contribute a unique support to political and social institutions that in turn make market exchange and a host of other social goods possible. That is not to suggest, however, that Western institutions do not have their own ethical deficits and characteristic social pathologies. Indeed they do, and the ethical poverty of the developed world will be the subject of later chapters.

If people always told the truth, fulfilled their promises, and refrained from violating "rights" there would be no need for coercive institutions of justice. Obviously, this is not the way the world works. We cannot reliably expect that everyone will act according to disinterested moral scruples, or that they will even agree about what those scruples ought to be. Rather, we need guarantees that powerful organizations will enforce our valid claims when others do us wrong. These are the guarantees that enable markets

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and civil society to take shape.[21] However, these guarantees cannot themselves be guaranteed by an infinite regress of explicit incentives. Nor can they ever be perfectly effective in adjudicating all claims. They require the something like the "ideological" allegiance of a populace on various fronts.

In Western democracies, ethical claims about what is right and wrong for government to do motivate powerful political movements that can effectively check and rectify egregious abuses. But it is important to see that such motivation seldom derives from immediate economic incentives (or even derivative economic incentives, as rational choice literature on problems of collective action has shown). On another level, ethical allegiances provide a social lubricant that alleviates the burdens placed on legal institutions. In so far as people come to endorse standards of trust, merit, shame, responsibility, and so on communicated through ethical visions, social virtues that are otherwise rare can becomes second nature. Transparent standards of right backed by credible threats of enforcement lessen the need to actually invoke legal adjudication. Moreover, a functioning legal system means one doesn't have to personally avenge vendettas. The role of ethics presented here does not presuppose that humans are either angels or devils. Rather, it is because we are most often something in between that ethics plays such a crucial role.

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[21] See North, "A Transaction Cost Theory of Politics" *Journal of Theoretical Politics* Vol. 2, No. 4 (1990), 355-367.

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So, part of the genius of Western democracies is that they don't actually have to rely on exhaustive incentives and pervasive enforcement to constrain violence. Rather their citizens have generally and for the most part come to internalize beliefs that make recourse to violence unfathomable. There are general, society-wide norms that can be taken for granted as well as various, particular traditions of ethical reflection that inspire more extensive ethical commitments. Certainly there is a large minority of exceptions, but these are still small enough that they can be fairly well managed through a justice system, selective incentives, and individual attention.

Broken societies mired in violent anarchy clearly can be helped, at least in the short run, by economic and military interventions from the West, and the logic of incentives may be straight-forward. However, to actually build the long term foundations for a stable, peaceful, and prosperous society, institutional design of incentives must go hand in hand with the development of ethical convictions that enable citizens to distinguish strict self interest from what Tocqueville called "self interest rightly understood."

Here we encounter a very difficult set of questions, namely how to engage and change the ethical convictions that are so crucial to the behavioral fabric of a society, particularly as pertains to habits of violence. Part of the difficulty in addressing this question is due to the fact that such questions are highly politicized within the Western academy. Many see attempts to influence ethical convictions as cultural imperialism at worst and groundless at best (an anthropologist at NYU recently persuaded the UN to suspend funding for a program that had been successful at eradicating female genital

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mutilation in north African villages because, the anthropologist argued, we have no right to interfere with their customs.) Moreover, some have argued that the modern liberal state ought to be formally neutral towards all competing conceptions of "the good" (except, so it is claimed, those minimal conceptions required for a tolerant liberal state), and thus projects aimed at moral/civic/ethical education are seen as inherently suspect.

Beyond these challenges, however, lies a deeper set of epistemological difficulties concerning the methods appropriate for engaging people's deeply held convictions. Where do such convictions come from in the first place? Can they be rationally grounded? How can rival convictions be brought into productive dialogue? Such questions raise distinctively philosophical problems, and are often addressed in practice through various artistic, literary, and educational enterprises that assume first person, interpretive perspectives. That is to say that it is not clear how to construct a "science" of the formation of ethical convictions. Still, we engage in arguments and persuasion about such convictions all the time. What is desperately needed is an account of how to understand these practices and how to integrate them with insights gained from both the natural and social sciences.

As things stand right now, there is a strong priority given to various "scientific" methodologies for understanding and addressing causes of violence. There are, no doubt, important biological factors that mediate violent behavior; and there are also plenty of insights into the social conditions underlying violence that we can discover through statistical analysis and modeling of incentives. However, restricting ourselves to such approaches excludes an entire area of inquiry that clearly has profound behavioral

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implications, namely that which concerns the conceptualizations of ultimate goods that provide individuals with their aspirations, scruples, and senses of obligation.

This point of course applies to many behavioral questions beyond violence as well. It is a mistake to avoid confronting these conceptual issues because they present difficult questions, or to try to reduce ethical convictions to hardwired, non-cognitive, or structural factors. Moreover, recognizing the importance of ethical convictions and engaging them need not be divorced from serious considerations of economic incentives and biological dispositions. However, in so far as ethical convictions are an essential constituent of a peaceful social order, understanding, engaging, and transforming them *at a conceptual level* must be part of the agenda for anyone who cares about salutary social change.

Thus, this study defends the importance of such enterprises. It seeks to identify those areas of social thought and social change where what is called for is not a technical knowledge, but rather persuasive ethical arguments about the desirability of certain goods.[22] In addition, this study identifies various educational projects, cultural practices, and institutional arrangements that are important for communicating overarching values and virtues that we cherish, which are crucial to the foundations of our present social order. It also suggests certain institutional arrangements that can help to transmit and to

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[22] Here I intend "argument" in the most expansive sense of the term, which could involve exemplary displays, artistic reflections, participatory habituation, as well as more conventional "intellectual" arguments.

sustain rationally productive conflicts between rival ethical traditions. Finally, it argues that it is perilous to neglect these enterprises, whether done out of an epistemological concern for scientific tractability or a normative concern for "value neutrality."

We cannot avoid questions of ethics. However, we can address them more or less adequately; and ignoring them is surely the least adequate response. A better account of ethical convictions and ethical persuasion is the key to making the social sciences more useful for many of our most pressing social concerns.

2. How We Got Here: a short history of the social sciences and their relationship to ethics

"There are certain operations, establishments, and institutions, beneficial to the community at large, which it is the office of the community to introduce, direct, and superintend, and which are calculated to supply the defects of personal inclination, and to parry the struggle of opposite interests, whether for the improvement of agriculture, industry, and commerce, or to prevent or diminish the evils entailed on our nature, or those which accident is continually accumulating upon us.

Till the commencement of the epoch we are now considering, and even for some time after, these objects had been abandoned to chance, to the rapacity of governments, to the artifices of pretenders, or to the prejudices and partial interests of the powerful classes of society; but a disciple of Descartes… perceived how necessary it was that political economy, like every other science, should be governed by the principles of philosophy and subjected to the rules of a rigid calculation."

- Condorcet, *Sketch for a Historical Picture of the Progress of the Human Mind*. [1]

"The age of chivalry is gone. That of sophisters, economists, and calculators, has succeeded; and the glory of Europe is extinguished forever."

- Burke, *Reflections on the Revolution in France*[2]

2.1 Overview and Claim

I do not intend to rehearse the long and complex history of the social sciences.

However, certain features of that history are important for understanding the current

methodological commitments of social scientists, as well as the standards by which their

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[1] M. de Condorcet, Translated from the original French. *Sketch for a Historical Picture of the Progress of the Human Mind* (Philadelphia: Online Library of Liberty, 1976) Epoch 9.

[2] Edmund Burke, *Reflections on the Revolution in France* in *Select Works of Edmund Burke.* A New Imprint of the Payne Edition. Foreword and Biographical Note by Francis Canavan (Indianapolis: Liberty Fund, 1999). Vol. 2. paragraph 269, [89].

research is evaluated. It is helpful to trace out these features in order to appreciate how my claims about the importance of ethical persuasion enter into the contemporary landscape.

My ultimate concern in this chapter is to explain why and how ethics was excluded or ignored in the development of the social sciences and to suggest why it would be useful to reconsider the situation we have inherited. In brief, when social science research is driven by an ideal of methodological purity this tends to exclude considerations of ethics and persuasion. If, however, the goal is to be effective in achieving social change, there are reasons to take a wider view and think seriously about the nature and import of ethical persuasion. Fortunately, social scientists have increasingly endorsed an ideal of research that sees its purpose as being pragmatic and effective. This position provides fruitful terms for the arguments I wish to make, namely that certain methodological commitments have blinded social scientists to the value of many useful resources for social change. If indeed driven by pragmatic concerns, then social scientists should naturally be willing to reexamine the practical limits of their methods. The burden on my position is to show how ethical persuasion can help address social problems that have remained intractable when approached through conventional methodological analysis.

The argument I want to have with social scientists is thus a practical one about how best to achieve the results we agree are desirable. When judged by this metric of successful results, we see how the ideal of methodological purity and the consequent exclusion of ethics distorts and handicaps projects that social science research is meant to

support. However, this is not at all to suggest that the methodological tools of social science are useless. They are extremely powerful for investigating certain sorts of questions under certain sorts of conditions, and part of my aim is to explain their genuine utility for aspects of social inquiry. However, integrating the insights provided by these methodological tools with an understanding of the nature and importance of ethical persuasion is no easy task, in part because the impulse to exclude or reduce questions of ethics is deeply ingrained in the way most people think about the nature of scientific research.

Many have argued that the methods of science require placing questions of ethics to the side. Facts are one thing, values another. Since value is the subjective creation of the human mind, and not an "objective" or" natural" feature of the world, certain scientific perspectives see questions of value as something that science cannot investigate or understand. There is, I believe, some truth to this way of thinking, although it relies on a narrow conception of what science is.[3] More troubling is the conclusion that, since "values" cannot be explained scientifically, they must be fundamentally irrational. You can't argue with taste, so the saying goes, and many see values as little more. They are capricious and unaccountable to reasoned consideration. Others believe values can be explained and understood scientifically, but doing so requires us to view the subjective and conceptual dimensions of value judgments as epiphenomenal. People's ethical

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[3] See Charles Taylor on desirability characterizations (Taylor, "Understanding in Human Science" *The Review of Metaphysics*, Vol. 34, No. 1 (September1980), 25-38.)

convictions are on this account ultimately the unconscious products of some objective superstructures - class interest, birth order, geography, etc. - or indicative of chemical imbalances in the brain and hardwired biological dispositions. [4]

The account of ethical persuasion I would like to defend entails two claims that do not sit easily with these common perspectives on science and value. On the one hand I will argue that reason can be brought to bear on ethical reflection, reasoning that ultimately concerns ends, not simply means. Thus we can hope to persuade people regarding questions of ethical conviction in ways that are more or less rationally defensible.[5] On the other hand, I would like to claim that reasoning about ends involves concepts and judgments that are not amenable to scientific explanation and reduction. That is to say that there is more to reason than can be captured by scientific understanding, and the domain of our rational capacities extends beyond what science can illuminate. So, ethical persuasion is possible and important but is conducted on terms that transcend full scientific mastery.

Such claims are by no means original, but I hope to reconsider their implications for our understanding of the nature and possibilities of social science. They do raise deep questions about human reason, which I consider at length in the final chapters of this study. More immediately, though, it is important to see that these claims only pose a

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[4] There is a wide range of thinkers associated with such positions from various sorts of Marxists and Structuralists, to Jared Diamond (Diamond, *Guns, Germs, and Steel* (London: Norton, 1997)) sociobiology in the tradition of E.O. Wilson.

[5] As I will explain, there is no simply litmus test for rationality, it will always be a question of more or less. Rather than describe ethical convictions as rational or non rational, I would like to suggest they can be more or less rationally defensible.

threat to certain accounts of social science, while leaving others intact (and perhaps enriching them). The central issue raised by these claims is the relationship between truth and method. Or, put another way, it is the question of how we understand the epistemological limits of methodological approaches within the social sciences.

In the modern era there have been two general frameworks for thinking about the nature of science. One focuses on theoretical unity and methodological consistency (which, following Bernard Williams, I will call the ideal of "absolute science"); the other focuses on problem solving and practical results (the ideal of "pragmatic science"). Although we might expect these perspectives to complement one another - for example, consistent theories should be more useful than inconsistent ones - they entail very different priorities, approaches to research, and standards of success. I will explore the nature and influence of these two frameworks in detail below, but the basic claim I want to foreshadow at the outset is that there has been a shift over the last half century, beginning in the 1950's, in the way most social scientists defend their enterprise. Whereas, for much of their history the protagonists of the social sciences adopted the standards and aspirations of "absolute science," in recent decades these aspirations have been ostensibly abandoned in favor of "pragmatic science."

As we will see, there were many good reasons for this evolution.[6] It was influenced, in part, by developments in the philosophy of science that undermined some of the higher epistemic ideals of "absolute science".[7] The transition was also a way of addressing a longstanding humanistic critique, which argued that the methods of science could never fully account for the meanings, motivations, and freedom underlying human agency. This sort of critique raises powerful objections to social science understood as an "absolute science." However, this critique leaves the project of "pragmatic science" largely unscathed.

Unfortunately, many critics of social science have not recognized this development and continue to argue within the terms of the old debate. They try to demonstrate that the social sciences can never live up to the ideal of absolute science. But, in fact, that ideal has been largely abandoned in favor of pragmatic science. And from the perspective of pragmatic science, it simply doesn't matter if one's assumptions about human nature are "unrealistic," or one's research ignores the historically contingent "meanings" behind action. The only question is whether research provides insights that help us manage problems better than we might have otherwise. This is the perspective advanced, for example, by Milton Friedman in a famous essay that has proved widely influential in the social sciences (which I will examine at length later).

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[6] It remains a "dialectical" evolution. There has not been a complete transition, although consensus now leans towards the pragmatic account – certainly in rhetoric if not in practice.

[7] Within both the natural and social sciences. See Ron Giere *Science without Laws* (Chicago: University of Chicago Press, 1999).

The pragmatic account of social science is a powerful rejoinder to many of the epistemological critiques of the methodological foundations of social science, including those that invoke a conception of ethics as sketched above. However, the pragmatic approach raises questions of its own regarding why any particular methodology proves useful and how ethical persuasion might be useful.

All methods aim to provide resources for reliably discovering knowledge over a class of relevant cases. However, the ideals of absolute science and pragmatic science differ in their expectations about the universality and consistency of methodological approaches. The pragmatic view remains agnostic about questions of ultimate causality and readily recognizes limits in the useful application of different methods. Moreover, this view is willing to admit the reality and import of phenomena that lie beyond the scope of its methods and is therefore willing to think seriously about how to integrate insights from "non-scientific" investigations. The phenomenon of ethical persuasion is not a threat to the scientific coherence of the pragmatic conception, but rather something that can potentially complement our pragmatic aims.

When one examines the configuration of social science research today in more detail one finds a curious agglomeration of these two scientific perspectives. Although most social scientists now adopt a pragmatic stance when pushed to give an epistemological account of their enterprises, many still tacitly maintain many of the old aspirations of absolute science when working within their own particular methodological paradigms. Their approaches to research can thus appear somewhat schizophrenic, using

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the ideal of pragmatism to head off epistemological scrutiny but still harboring a belief in the universal scope of their methods. So, many of the traditional critical perspectives on social science are not entirely mistaken to perceive the continued influence that the ideal of absolute science exerts on contemporary researchers.

For a number of reasons I will argue that social scientists should indeed follow through on abandoning the ideals of absolute science, as has been proposed by many practitioners in recent years. To do so, however, will require a more radical transformation of habits of thought than many social scientists realize. One purpose of this chapter is to highlight the residual influence the ideal of absolute science exerts on contemporary social scientists. Researchers need to give up on the fantasy of ultimate reduction and complete causal realism, and better appreciate how limited and contingent their findings are. Indeed, they will find that recognition of such limits will actually make their findings more useful.

From another direction, the traditional, "hermeneutic" critics of social science need to understand that the terms of the debate concerning the nature and promise of social science are fundamentally changed when the social sciences proceed as pragmatic enterprises. Many past debates about social science are limited by the particular view of science in question. The point that we cannot completely understand the manifest complexity of human thoughts and actions through scientific techniques *does* mean that we can never have a truly absolute science of human society. However, there are plenty of useful insights to be gained regarding social phenomena that can only be revealed

through the detailed analytic investigations provided by the social sciences. Although these methods will always trade on some basic reconstruction of human purposes and depend on various judgments that science cannot provide, the important questions concern not whether such assumptions are realistic or universalizable, but whether the insights gained by investigations utilizing these methods are helpful for addressing a problem better than we might have otherwise.

So, the methodological debates worth having are ones that ask why an approach has or has not been useful.[8] The standard of success for any methodological approach is its ability to provide knowledge that can help resolve identified social problems.[9] Those who want to critique and enrich the abilities of social scientists have to focus debate on questions about why certain methods have been useful and the conditions under which they promise to continue to be useful. More relevant for my purposes here is the question formulated in reverse- has reliance on particular methodological approaches failed to be useful for addressing important social problems and, if so, why? The short answer: because these methods systematically excluded questions of ethics and possibilities for persuasion from view, and did so out of a concern for scientific reduction indebted to the ideals of "absolute science."

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[8] There can, of course, be disagreements about what constitutes useful insights, but the perspective of a particular problem provides a much more determinate framework for evaluation than does an open ended search for scientific insights into the nature of society.

[9] Either in the short or the long term. The fact that some research programs take a while to establish genuinely useful insights can easily be accommodated by this standard.

To summarize, then, the layout of these arguments: Social scientists have rightly abandoned many traditional pretensions of "absolute science." And indeed social science was indefensible on such terms, something which hermeneutic critics of the social sciences had long argued. Increasingly, social scientists have articulated a powerful account of their disciplines on pragmatic grounds, which I believe successfully addresses many of the objections of critics. However, certain aspects of the ideal of absolute science have persisted within the contemporary landscape, particularly a drive towards reductionism, attempts to universally extend methods that have limited scope, and an unwillingness to integrate insights that are not born of a "scientific framework" - especially with regard to questions of ethics and persuasion.

By briefly tracing out the origins and development of the ideal of absolute science I hope to diagnose its continuing and detrimental influence on contemporary researchers. As the debate rightly turns towards the question of how the social sciences can be most successful in helping us address social problems, I explore two sets of questions in later chapters. First, are there intrinsic limits to the useful application of our standard methodological approaches in the social sciences? Indeed there are, and they can be well characterized and understood. Retrospectively we can also see how the mistaken application of these methods has worsened, rather than improved, many social problems researchers had hoped to ameliorate. Second, I show how, in light of these limits, effectively addressing many large scale social problems will often depend on augmenting what we learn from the social sciences with efforts to persuade others to change their

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ethical commitments. Moreover, there are particular lessons we can learn, drawing from various disciplines, about how to structure institutions in such a way that rationally defensible ethical persuasion has a better chance of taking place. Ultimately, since ethical convictions play an essential role in enabling institutions and outcomes we value, ethical persuasion is a challenge and task that we avoid at our peril, and one we must undertake again and again.

If the social sciences are ultimately justified on pragmatic grounds, we need to understand how and why they have failed to be useful for addressing particular social problems. We will find that methodological commitments driven by a particular ideal of science have often proved a hindrance rather than a resource for social thought and social change. Moreover, the limits of these methods relate directly to the unique ability humans have to consider and revise the ethical convictions that form the motivational fabric of their lives. Understanding these limits illustrates why ethical persuasion is often a necessary component for successfully achieving desired social goals.

2.2 Visions of Science

"Scientific" methods for studying human society have advanced tremendously in the last century. Powerful statistical techniques and computer processing now enable us to sift through enormous amounts of information in order to discover (under certain circumstances) useful relationships between variables like inflation and unemployment, population density and riots, central bank independence and interest rates, and so on. Concepts developed in game theory and advances in computational modeling enable us to

predict likely outcomes in a variety of strategic contexts, useful for understanding phenomena as diverse as evolutionary selection and civil war to legal settlements and Ebay auctions. And advances in the biomedical sciences are increasingly unlocking the secrets of the human organism, revealing not only the causes of various diseases, but also the relationships between host of behavioral traits and their biological foundations in neurological structures, genomics, and evolutionary psychology. Moreover, it is often claimed that these various, sophisticated methods of analysis and the knowledge they produce have played an important role in improving society. No doubt there is some truth in this claim, and with regards to specific issues like economic development in the first world it is clearly sustainable.

   Against the background of these theoretically powerful and practically useful methodological advances in the social sciences, however, it is worth examining what they haven't done, and perhaps can't do. Thus the first question that we need to address: what sort of social problems have remained intractable in the face of these advances, and why?

   The world remains a very complex, frequently tragic, and often dangerous place in which large populations are continually plagued by violence, starvation, and preventable diseases, not to mention a host of lesser evils. Things could be much better, indeed radically so in many circumstances. Although there has been much "progress" across the globe as measured by many basic indicators of health, wealth, and well-being, with some 400 million people having risen out of subsistence poverty in the last decade,[10]

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[10] Zakaria. Fareed. *The Post American World* New York, NY: Norton, 2008) 3.

significant populations remain dominated by the most abject conditions of violence, malnutrition, and disease. Closer to home there are the ongoing challenges of maintaining security amidst threats by hostile states and terrorist groups, sustaining the political, economic, and social conditions for prosperity and the administration of justice, and negotiating countless social challenges that continuously emerge from the changing fortunes of history. In the case of the third world we know that things could be better, given the comparative perspective that other societies provide, and in the first world we have many reasons to hope things might be better.

Thus, there is no question that many social challenges have remained unresolved by the hopeful interventions of social scientific knowledge. However, much rests on the answer to "why?" After all, a similar question could be posed to the natural sciences at various stages of their development as to why they were unable to understand features of the physical world that would prove extremely valuable to master. Eighteenth century theories of caloric heat couldn't account for properties of energy conservation later to be explicated as kinetic motion in modern thermodynamics. Nineteenth century physicists could not reconcile contradictory findings supporting both corpuscular and wave theories of light. And bloodletting was standard medical practice until at least 1850. However, looking back at the limits of science past necessarily illustrates the progress made since, and it can often seem that all that was needed for the rational development of these disciplines was simply more time, effort, and resources devoted to the scientific project.

It is true that, as historians of science have pointed out, the conceptual innovations that animate scientific change cannot be simply reduced to the cumulative application of

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"scientific methods." Nonetheless, the natural sciences appear an enviable and initially plausible analogue for the social sciences. So, perhaps the social sciences simply need more time, effort, and resources in order to transcend their present limits. All sciences have puzzles at their horizons and thus, necessarily, limits to their knowledge. The guiding assumption of ongoing scientific research is that these limits can continually be transcended by scientific progress. Perhaps, then, it is only this universal feature of science that accounts for the inability of social scientific knowledge to effectively address many of our social ills today.

However, there is an alternative possibility, namely that crucial features of human society lie permanently beyond the scope of the *methods* we currently employ, such that no amount of time, effort, and resources can alter what are the limits of these methods in principle. The attempt to do so would be like straining to perceive radio waves with the naked human eye. Our eyes just aren't made for detecting those wavelengths; we can only see the visible light spectrum.[11] How plausible is this analogy with social science methods? Are they systematically blind to important human phenomena that lie beyond their inherent scope? Social scientists themselves have been divided on this question. Few claim that they can give a global "account of everything," and the fact of methodological pluralism within the social sciences –between both statisticians and formal modelers, but also varieties of "qualitative" approaches – implies some recognition of the limits and complementarities of different approaches.

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[11] Or perhaps this example should be reversed. We can see many things about the nature of society with our naked eyes that social science methods remain blind to.

Much rides on the details of the why our current methods haven't been able to tackle certain problems, but answering this depends on the criteria for success. Thus a second key question: What would it mean to say that a methodological approach is inadequate or has failed? This is already difficult to establish in the natural sciences, where there are a number of examples of methods having been abandoned because of their inadequacy in light of new knowledge. Augury, phrenology, astrology are all paradigm cases of the wholesale rejection of certain methods. For various reasons our current understanding of the universe simply rules them out as candidates for any fruitful application. More difficult are cases in which a methodological approach is useful for certain domains and applications, but also limited in its ultimate scope, such that it does not apply universally. In such cases, boundary conditions and qualifications need to be incorporated into our use of the method itself.

For example, in contemporary physics, the applicability of traditional Newtonian mechanics breaks down at the micro level of quantum dynamics and the macro level of general relativity. Of course, Newtonian mechanics works perfectly well for dealing with many of the engineering problems we encounter, from shooting a basketbal to putting a man on the moon. As long as we are aware of the limits of Newtonian mechanics and do not, say, use its principles to construct a nuclear reactor, these limits do not present practical problems. However, for many concerned with the philosophy of science these limits do present interesting epistemological problems regarding how to think about the theoretical unity of physics. Indeed one of the great aspirations in theoretical physics is to provide an overarching framework that encompasses and reconciles all physical relations

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from the quantum to the cosmological level - and approaches such as string theory, spin networks, and quantum cosmology are attempts to supply such a framework.

I mention these rather distant examples in the physical sciences only to illustrate that questions about the methodological limits of scientific approaches and quandaries concerning the underlying unity of scientific knowledge are quite alive in the "hard" sciences. Thus, one should not suppose it to be a *prima facie* affront to the scientific aspirations of social science to raise similar questions about the limits and unity of their methodological approaches. The question as to whether the natural sciences are even an appropriate analog for the social sciences is, of course, a matter of longstanding controversy, but I would like initially to admit the comparison as a way to delve deeper into an examination of how we judge the adequacy of a methodological approach.

This question of the proper criteria for judging the adequacy of a methodology was central to the philosophical debates of the seventeenth century, which initiated modern science as we know it. It is worth considering these briefly, because two very different conceptualizations of science emerged, one which we can describe as loosely Cartesian and another Baconian. Both of these visions of science provided powerful frameworks that transformed the nature and purpose of human inquiry, and both shared the goal of increasing human knowledge, power, and control over nature. However, they mapped out very different epistemological aspirations, and conflating these aspirations has led to unnecessary confusion in debates about the nature and purpose of social science.

The central line of demarcation I would like to draw between these visions concerns what we can refer to as the difference between "absolute science" and "pragmatic science." The absolute conception of science is a term Bernard Williams develops in his study of Descartes to describe the high epistemological ideal of Cartesian knowledge and the central role that method plays in providing access to truth. Absolute science aims at "certain truth" rather than mere probable knowledge, and following the proper method is what guarantees truth. Thus conceived, absolute science places a high premium on systematic consistency and unifying coherence across all domains of knowledge. It seeks to render the world as it truly is, without any partiality or limits. From this perspective, methods cannot truly be scientific if they do not draw on consistent underlying theory and reliably provide dispositive resolution of questions of truth. Modern Physics comes the closest of any science to recognizing the epistemological ideals of absolute science, with its drive towards a unified theory of everything and mathematically dispositive descriptions of the world.

In contrast, the "pragmatic" account of science is driven less by ideals of pure epistemology than by a concern for progress in solving particular technological problems. It is often quite happy to remain agnostic about grand unifying theories and methodological inconsistencies. The important criterion for evaluating any method is whether investigations employing it have hit upon new, useful insights. Inquiries of this pragmatic sort have been revolutionized by development of statistics, and their "success" may consist not of the dispositive discovery of universal laws but, more often, statistical

generalizations that enable people to make better "bets" than they might have done otherwise.

Of course, these two accounts of science did not spring fully formed from the heads of Francis Bacon (1561-1626) and Renes Descartes (1596-1650). Moreover, the popular sketch of Descartes as a proponent of rationalist-deductive science and Bacon as a proponent of empirical-inductive science is misleading. Descartes wrote conflicting things regarding the import of empirical surveys and demonstrations, and Bacon didn't articulate anything like what we would consider a logic of experimental control or statistical inference.[12] However, what is remarkable is how clearly differentiated the thoughts of these two philosophers were concerning the epistemological aspirations of science. It is worth briefly examining these aspirations as they were originally articulated, as well as their ongoing legacy within our contemporary scientific imagination.

2.3 Bacon and Descartes

At first glance, the similarities between the thought of Bacon and Descartes seem to overshadow their differences. Both agreed that the intellectual currents of their times were mistaken and believed there needed to be a wholesale reconsideration of the paths to knowledge. Both were also motivated on some level by a practical concern for improving society. As one scholar notes, Descartes' reflections that were to provide the starting

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[12] See Bernard Williams, *Descartes: The Project of Pure Enquiry* (London; New York: Routledge, 2005) 243-246.

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point for his *Discourse on Method* "began not with abstract metaphysical thoughts but with the criteria for distinguishing good technicians, lawgivers, and scientists set against the backdrop of a world that had failed to find a principle of order that could restrain religious passions and establish political peace."[13] Likewise, Bacon aspired to improve the estate of mankind and believed "the real and legitimate goal of the sciences is the endowment of human life with new inventions and riches."[14] Most significantly, both Descartes and Bacon saw "method" as the key issue for achieving knowledge that could genuinely improve human life.

However, Bacon and Descartes articulate very different accounts of what scientific method entails. The ability of scientific investigation to improve our power over nature is fundamental for Bacon's account in a way it is not for Descartes. Conversely, the certainty of method and primacy of indubitable knowledge ground Descartes' account in a way that is fundamentally at odds with Bacon's.

In his *Novum Organum or True Suggestions for the Interpretation of Nature* (1620), Francis Bacon begins with the assertion "Knowledge and human power are synonymous."[15] Genuine knowledge, according to Bacon, originates in our experience of particulars and gradually builds up from these experiences to more systematic conclusions ("axioms") about how the world works. Such conclusions should be

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[13] Michael Gillespie, *The Theological Origins of Modernity* (Chicago, 2008) 263 (Manuscript copy).

[14] Francis Bacon, *Novum Organum*, LXXXI.

[15] Ibid., 1.iii.

provisional- that is, open to revision based on new experience- and also understood as limited in their scope - that is, limited in the power that they confer. When this process is in good order Bacon suggests we will continually build up and modify these "intermediate axioms" based on experience and the intelligent attention to detailed, controlled experiences that he calls experimentation.

Bacon admits that our collections of useful, intermediate axioms will eventually suggest the truth of some "general axioms," but this achievement is incidental and "unattempted." By contrast, Bacon thinks the great intellectual error of his time springs from a desire to move directly to the most general axioms in an attempt to establish first principles and "their supposedly indisputable truths." He cautions against those who build "systems" to understand the whole world, which tend to create "idols" that are "dictatorial" and constrain our ability to entertain new insights from experience.[16] He also criticizes the contrary but related excess of "induced skepticism and vague unbounded inquiry."

Rather than an all-or-nothing approach to knowledge, which tries to establish first principles and universal axioms or, failing that, sinks into general skepticism and refuses investigations that are not certain, Bacon argues for the utility of partial and probable knowledge. "The highest and most general [axioms]" sought by the intellectual tradition that Bacon criticizes "are notional, abstract, and of no real weight." Rather, it is what

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[16] Ibid., LXVII.

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Bacon describes as partial and probable "intermediate axioms" that are "true, solid, full of life, and upon them depend the business and fortune of mankind."[17]

The right way forward for science depends in Bacon's account upon a new understanding of the possibilities of experimentation and the process of induction. Induction from experiments is a project through which one separates and analyzes nature "by proper rejections and exclusions, and then conclude for the affirmative after collecting a sufficient number of negatives."[18] This project lends itself to a division of labor and also promises to be cumulative as the conclusions of experimentation continually identify new areas for investigation. According to Bacon, "An experiment is produced two ways; viz., by repetition and extension, the experiment being either repeated or urged to a more subtile thing."[19] True science is thus conceived as a cumulative and yet probabilistic path of trial and error: "Our road is not a long plain, but rises and falls, ascending to axioms, and descending to effects."[20]

From the "particulars" of experimentation, which can extend to what Bacon calls "literate experience," we build up more generalized "intermediate axioms" that demonstrate their truth through the effects they enable us to produce in the world. The limits of these effects draw our attention to new particulars, and in examining them we better understand the limits of our knowledge or discover fruitful extensions of it:

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[17] Ibid., CIV.

[18] Ibid., CV.

[19] Francis Bacon, *Advancement of Learning*, Ch 2, 220.

[20] Francis Bacon, *Novum Organum*, CIII.

In forming our axioms from induction, we must examine and try whether the axiom we derive be only fitted and calculated for the particular instances from which it is deduced, or whether it be more extensive and general. If it be the latter, we must observe, whether it confirm its own extent and generality by giving surety, as it were, in pointing out new particulars, so that we may neither stop at actual discoveries, nor with a careless grasp catch at shadows and abstract forms, instead of substances of a determinate nature.[21]

The thing to avoid is a methodological orientation that, in its search for certain and universal knowledge, prevents one from attending to new insights not already presumed by the general principles of a theoretical framework at the outset.

Bacon realized that some may object to his method because the knowledge it produces always remains provisional and uncertain. But he argues that the alternative is simply naive and false overconfidence in what we know. There is a real danger that we will become blinded by premature theoretical abstractions, which are not sensitive to their contingency and genuine scope. Bacon's method proposes to provide not certainty as such, but rather "fitting certainty":

Another objection will be made against us, that we prohibit decisions and the laying down of certain principles, till we arrive regularly at generalities by the intermediate steps, and thus keep the judgment in suspense and lead to uncertainty. But our object is not uncertainty but fitting certainty, for we derogate not from the senses but assist them, and despise not the understanding but direct it. It is better to know what is necessary, and not to imagine we are fully in possession of it, than to imagine that we are fully in possession of it, and yet in reality to know nothing which we ought. [22]

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[21] Ibid., CVI.

[22] Ibid., CXXVI.

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Bacon criticized the philosophical methods of his and preceding eras because he believed their ideal of knowledge prevented genuine progress in the discovery of useful insights that would enable mankind to shape the world to its benefit. If the aspiration to "abstract" and "certain" truth is placed on too high a pedestal, the drive to attain it can lead to interminable debates and theory induced blind spots that do not support practical progress. Thus, rather than associate truth with the high epistemic ideals of certainty, abstraction, and universality, Bacon conceives truth in pragmatic terms: "Truth, therefore, and utility, are here perfectly identical, and the effects are of more value as pledges of truth than from the benefit they confer on men (XXIV)." Bacon is not only explicit in his hope that his methodological vision will provide practical benefits, but he also makes the additional claim that pragmatic truths provide the path to truth itself.[23]

Like Bacon, Descartes hoped to reduce erroneous knowledge and improve the world through a new kind of method, but the means and aspirations of his path diverged radically from Bacon's. Debates about the continuities and shifts in Descartes thought run deep, due in part to the extraordinary scope of his enterprise and its intersection with fundamental questions in metaphysics, theology, and philosophy of mind. Early in his life

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[23] See Ibid., XXIV, where Bacon makes an interesting, and perhaps rhetorical move, in which he affirms the primacy of the contemplation of divine wisdom, but argues that in fact our high theories are but idols, and it is only by understanding the inner workings of nature through his method that we come to a true understanding of the ideas of the divine mind that guide creation: "The former are mere arbitrary abstractions; the latter the true marks of the Creator on his creatures, as they are imprinted on, and defined in matter, by true and exquisite touches."

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he conceived of the "marvelous" idea of a universal science.[24] His strategy for grounding such a science underwent subtle changes over the course of his philosophical career, and these have been the subject of much interest and scholarly debate. However, throughout his work, from his unpublished *Rules for Directing the Mind* (~1628), to the *Discourse on the Method of Rightly Conducting one's Reason and Seeking the Truth in the Sciences* (1637), and the more esoteric *Meditations on First Philosophy* (1641) Descartes articulated an ideal of science that aimed at nothing less than certain truth.

It was a requirement of reason, as Descartes understood it, that certain truth is the only proper object of knowledge: "Reason persuades me already that I should withhold assent no less carefully from things which are not clearly certain and indubitable, as from things which are evidently false; so if I find some reason for doubt in each of them, this will be enough for me to reject them all…if the foundations are undermined, anything built on top of them falls down by itself… ."[25] The same principle is found at the beginning of the *Rules*, the second of which is "We should attend only to those objects of which our minds seem capable of having certain and indubitable cognition." The claim that "All knowledge is certain and evident cognition" leads Descartes to "reject all merely probable knowledge, and only to trust what is perfectly known and cannot be doubted."[26]

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[24] (Michael A. Gillespie, *Nihilism before Nietzsche* (Chicago: University of Chicago Press, 1995) 30)

[25] Descartes, *Meditations* First Meditation, VII 18 HR 145.

[26] Descartes *Reg*. ii: X 362, HR1 3.

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Why was Descartes not interested in merely probable knowledge? Commentators have suggested two sorts of explanations. One is to look for a psychological account of his peculiar fascination with certitude, perhaps to be found in theological anxieties of his time that considered certitude of highest value, particularly with regard to salvation. Others have suggested that, in fact, the search for certitude is a genuine and necessary component of the philosophical search for truth more generally. Descartes, on this view, was probing fundamental questions about the ultimate possibility of true knowledge and the universal methods of science. Bernard Williams, who is sympathetic to this second reading, further suggests that the primacy of certitude indicates a point of departure in Descartes' project from the basic aspiration to provide useful knowledge for improving man's estate. Of course, "certain knowledge" will be useful; but probable knowledge is useful too. By rejecting such knowledge as a goal of inquiry, Descartes prioritizes "truth" over utility, unlike Bacon for whom the two are equated. Ultimately, if Descartes' project of pure inquiry succeeds and his method has a universal scope, the resulting certain knowledge would promise the greatest utility of all. This is, however, a big "if" - and without such success, Bacon's path of mere probable knowledge would prove the more useful.

The central question for Descartes, once certainty is identified as the goal, is thus: what can I not doubt, and how can I build upon and extend whatever is indubitable to arrive at greater knowledge of the truth of things? In the *Rules* Descartes suggests that experience and intuition provide immediate and certain knowledge, but by the *Discourses* he comes to see that even sense experience cannot be completely trusted, since we can be

mistaken in our perceptions, as in the case of illusions or, more radically, in hallucinations and dreams. Thus Descartes' famous development of the principle "*cogito ergo sum*"- I think therefore I am. He argues that his own existence is the one thing he cannot doubt, for although he can possibly be deceived about everything else, to be deceived at all he must exist. Consciousness can never doubt its own existence. More controversial are Descartes subsequent moves in the *Discourse* and *Meditations* through which he posits the necessary existence of a perfect being, God, whose goodness guarantees the certain truth of "clear and distinct ideas" as well as the consistency and universality of mathematics.

In the *Rules*, Descartes argues that "those who are looking for the right road to truth should not concern themselves with any object about which they cannot have a certainty equal to that of the demonstrations of arithmetic and geometry."[27] Indeed, mathematics provides the paradigmatic case of rendering ideas clear and distinct, and Descartes maintains that mathematical operations, including deduction, preserve and extend certain knowledge. Related to this, the fourth *Rule* states: "A method is necessary for investigating the truth of things." As Williams explains, Descartes' attempt to identify a method of acquiring true beliefs totally free of error requires "a method that is error-proof" and "no method can be error proof which allows a state of affairs in which the method has been correctly applied but has produced a belief that is nonetheless false."[28] By a method, Descartes means "certain and easy rules - rules such that, if one has

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[27] Descartes *Reg.* ii X 366, HR 15.

[28] Bernard Williams, The Project of Pure Inquiry (London; New York: Routledge, 2005) 34.

followed them exactly, then one will never suppose anything false to be true."[29]

Mathematics thus must form the foundation of his scientific method, for only math enables the organization of clear and distinct ideas and assures certain conclusions from its operations. Moreover, to understand the world mathematically we must break up its material components into enumerable parts, and accompanying this process is a principle of reduction that aims to explain things in the simplest constitutive terms: "If we are to understand a problem perfectly, we must free it from any superfluous conceptions, reduce it to the simplest terms, and by a process of enumeration, split it up into the smallest possible parts."[30]

At the end of the *Meditations*, having established the existence of God, mathematics, and his thinking soul - all incorporeal entities- , Descartes turns to the question of whether corporeal things exist and how they are known. They are of course objects of sense perception, but as we have noted the senses can be mistaken about reality. Descartes concludes, "We must allow that corporeal things exist. However, they are perhaps not exactly what we perceive by the senses, since this comprehension by the senses is in many instances very obscure and confused; but we must at least admit that all things which I conceive in them clearly and distinctly, that is to say, all things which, speaking generally, are comprehended in the object of pure mathematics, are truly to be recognised as external objects."[31]

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[29] Descartes, *Reg.* 4,1 85.

[30] Descartes, *Rule* XIII.

[31] Descartes, *Meditations,* Meditation VI ~73.

The first implication of this stance is that method and math provide the only reliable access to reality. What they discover is more true than what we perceive by the conventional lights of human perception. Indeed only conclusions that have passed through the filter of mathematical method can have the status of genuine knowledge. Things that cannot be established by such an approach are evidentially unreal and insubstantial. Descartes' conclusions may appear something of a *non sequitur* without a corollary claim that he develops earlier in the *Meditations*, namely that things can only exist if they can be described and understood in mathematical terms. God in his goodness only composes the universe of things that are clear and distinct and thus amenable to complete mathematical understanding. Since all material things in the world should be amenable to clear and distinct reasoning, there is ultimately no distinction between pure mathematics and physics. We need only to analyze the world in terms of the simplest units that present themselves as clear and distinct, and then extend our understanding of their interrelations and effects through mathematical reasoning. With this method we can come to understand the world as it truly is, beyond the biases and mis-perceptions of vague, everyday human perception.

We are now in a position to understand why Bernard Williams characterizes the most significant aspect of Descartes project as the development of an "absolute conception" of science, in which "Descartes ultimately strives to establish an Archimedean point, a true representation of reality as it is in itself, in which all partial

knowledge can be reconciled into a complete, universal account."[32] As John Cottingham

explains in the forward to Williams study, "Descartes' project of pure inquiry is supposed

to give us the kind of knowledge that is free from the relativity arising from the

preconceptions of the local cultural context in which we operate, and even free from the

particular perspective of our human standpoint."[33] An absolute conception is one

achieves an objectivity that completely transcends the subjectivity that characterizes

human life.

In light of this aspiration for an absolute conception of science, Williams shows

that Descartes insistence on certainty becomes more understandable:

> If we are to make an attempt to ground the absolute conception of reality
> which knowledge seems to call for, then the project of undercutting every
> conceivable source of error takes on a new importance. It is a matter not
> just of overcoming limitations on enquiry and hence occasional error, as
> understood within the framework of our outlook, but of overcoming any
> systematic distortion or bias or partiality in our outlook as a whole, in our
> representation of the world: overcoming it, that is to say, in the sense of
> gaining a standpoint (the absolute standpoint) from which it can be
> understood in relationship to reality, and comprehensibly related to other
> conceivable representations.[34]

Although this aspiration to objectivity is a recurrent theme in the history of

philosophy, Descartes vision provided a new and influential way of conceiving how it

could be achieved, in which method and mathematics played the most fundamental role.

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[32] Bernard Williams, The Project of Pure Inquiry (London; New York: Routledge, 2005).

[33] John Cottingham in the foreword (xi) to Bernard Williams, *Descartes: The Project of Pure Enquiry* (London; New York: Routledge, 2005).

[34] Ibid., 51.

For Bacon, there are no clear and certain ways to truth, there are only better and worse ways, admixed with error, which we can improve over time in useful ways if we are sufficiently attentive to experience. Method can help us better attend to experience through the process of detailed and cumulative experimentation, and any particular method can be evaluated by its ability to produce useful results. For Descartes, by contrast, scientific method comes first and serves as a filter for all genuine knowledge. Only knowledge which is gained through the application of method is truly worth knowing, and anything that cannot be investigated by scientific method has an air of unreality- it must not exist, or it is too confused to make a real object of knowledge. For those inspired by Descartes' vision of absolute science and his understanding of the role of method, the great promise of scientific investigations is that their methodological starting points ensure the validity of their conclusions. Everything then depends on beginning from the most sound and universal methods. Those methods, properly applied, grant access to things as they really are. This is the aspiration of "absolute science."

It is striking, when we look at the details of Descartes' work, how little direction he actually provided to scientists beyond his general endorsement of a mathematical physical system. His contributions to analytic geometry notwithstanding, Descartes is not known for his scientific achievements and he added little of technical use to scientific practitioners. What he did contribute that was of major and lasting importance was the ideal of absolute science and how it could be exclusively approached through mathematical method. That is, he articulated an ideal of truth which involved an intrinsic

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link to method. It was this ideal, rather than any technical contributions, that was to influence the shape and aspirations many scientific enterprises since.

Of course I do not want to claim that any number of scientists actually thought of themselves and their enterprises as Cartesian, or ever looked to the writings of Descartes to direct their own work. Rather, I am suggesting that Descartes articulated a distinctive vision of the nature of scientific method that became influential in the way science was conceived by many in modernity. This included the belief that science could provide access to an "absolute conception" of reality as well as the association of truth with method. I also do not want to claim that Descartes' account was *sui generis*; he certainly drew on sympathetic intellectual currents that preceded him. But his account of science was radically distinct from Bacon's and indicated an influential, alternative path.

If any modern science provides reason to endorse Descartes' vision it is physics. Galileo ("mathematics is the language in which God has written the universe") was a contemporary of Descartes and provided him with a prime example of how math could be used to understand the universe. A generation later, the development of calculus by Leibniz and Newton as a way to mathematically study change and the latter's formulation of universal gravitation and three laws of motion seemed to confirm the unity of math and physics that Descartes believed could provide the foundation of an absolute science. Indeed, progress in the natural sciences more generally convinced many that the aspirations of absolute science could actually be realized.

The ideal of an absolute conception also lent additional weight to understanding the unity of the sciences. In order for science to be absolute, the findings of one branch

had to be consistent with the findings of another. Moreover, the greatest science would be that which provides the means of organizing and accounting for all derivative sciences. Thus, as was sometimes suggested, biology could be understood in terms of chemistry, chemistry in terms of physics, and physics in terms of math.

However, this account raises an obvious and serious problem: what to make of an absolute conception of reality in which humans are part of the picture? Are humans entirely a component of the natural world and thus amenable to scientific investigation? Can their thoughts and actions be understood in a scientific framework, and, as the unity of science might suggest, ultimately reduced to physics and math? If so, what would it mean to talk about human freedom and responsibility? These are deep and important questions that are still with us today. However, they did not pose an immediate problem for Descartes because his philosophical system included some peculiar features that helped him avoid the apparent dilemmas they raise. In particular, Descartes dualism, which separated the thinking substance of the human soul from material reality, as well as his theology, carved out a unique space for human freedom and, in fact, separated man from nature.

According to Descartes, we resemble God in our freedom and thus are not the mere products of material causality on his notoriously controversial view:

> I likewise cannot complain that God has not given me a free choice or a will which is sufficient, ample and perfect, since as a matter of fact I am conscious of a will so extended as to be subject to no limits... It is free-will alone or liberty of choice which I find to be so great in me that I can conceive no other idea to be more great; it is indeed the case that it is for

the most part this will that causes me to know that in some manner I bear the image and similitude of God.[35]

This perspective is likely no longer credible to many contemporary readers, but it meant Descartes had no problem maintaining a conception of ethics and human agency on the one hand and an absolute conception of science on the other.

What about later thinkers who reject Descartes theology and dualism? Once these have collapsed, humans are presumably left as part of the material world, and like all material, reducible to scientific understanding. This, at least, is one sort of conclusion that gathers important adherents in the history of the social sciences (as well as today). However, these issues have proved an enormously complex and controversial.

The question of the "human sciences" indeed drives a wedge into the otherwise promising project of absolute science, which our manifest successes in the natural sciences seem to support. And, as Williams points out, "There is no obvious impossibility in the idea that the natural sciences should be able to give absolute explanations of a determinate and realistically conceived world, while the social sciences could not do this and should not be expected to."[36] But then, ought the social sciences be understood as a distinct project, with methods and objects so different as to be separate? In particular, should the place and nature of method in these enterprises be the same as that which grounds the natural sciences, or must a different vision of science be carved out for them?

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[35] Descartes, *Meditations*.

[36] Bernard Williams *Descartes : The Project of Pure Enquiry*(London; New York: Routledge, 2005), 287.

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Williams recognizes this as one of the most pressing philosophical questions that confronts a world that has given up on the "positivist fantasy," which aspired to create an absolute science that includes human life. Indeed, in Williams judgment, the challenge of understanding "the human" ultimately makes the project of absolute science, strictly conceived, untenable, and he concludes, "In the face of such considerations, the most ambitious ideas that have been entertained of the absolute conception must fail: the ideal, for instance, of a cumulative, convergent, self-vindicating unified science of man and nature (286)."

For many generations of social thinkers it was not obvious that the project of absolute science must fail with reference to man. And in the present day there are some approaches and schools of thought that consider the possibility still alive. Although I think they are mistaken, it is important to see why the notion of absolute science is not easily discarded. Bound up with this notion are deep questions about the ontological structure of the universe, the nature of human life, and the possibility of knowledge. Many versions of philosophical realism would seem to be committed to the absolute conception in principle. And, indeed, for those who would endorse a Baconian account of scientific method, many important questions are left unanswered regarding the unity of the sciences and the reasons that pragmatic approaches actually work. So, the absolute conception would seem to offer more promising resources for the philosophically inclined who desire a truly comprehensive account of reality. At some level, however, debates about the existence of such an account must give way to our actual abilities to articulate and use it. In this sense it remains an open question; but if (as I believe) the

proof is ultimately in the pudding, it will be difficult to distinguish from the Baconian approach in practice.

In light of these debates, Williams remarks: "With regard to Descartes, at any rate, it can be said that the commitment to realism, and to an absolute conception of the world which includes a conception of matter given by a realist physical science, is fundamental to him. It can even be said, I think, that any view which loses touch with realism in these matters is more directly opposed to the Cartesian outlook than any which retains the realist connection - even if the latter abandons, as it must, characteristic Cartesian beliefs in God, in dualism, and in the search for certainty."[37] It is of course the case that many who aspired to build an absolute conception of science since Descartes did indeed abandon his theology and dualism, and even relaxed the demand for certainty with the development of statistics and the suggestion of fundamentally probabilistic events in physics. However, they maintained his belief in the intrinsic link between truth and scientific method. Without Descartes' theology and dualism humanity becomes entirely an element of nature to be scientifically understood. The question is whether that is possible within the methodological aspirations laid out by Descartes' project.

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[37] Ibid., 233.

2.4 Legacies of the Absolute Conception of Science on Social Inquiry and Ethics

As I have hinted, I do think the Baconian vision of science stands as a viable alternative to the absolute conception articulated by Descartes.[38] Moreover, in recent decades practicing social scientists have, by and large, endorsed a pragmatic account of their enterprises close to that of Bacon's. However, it is important to see how the ideal of absolute science shaped the development of the social sciences, as well as to understand the reasons it was abandoned. With that history in view, we can then further see how certain aspects of the ideal of absolute science continue to exercise a pernicious influence in contemporary social science research, prioritizing methodological tractability over pragmatic capacities.

The ideal of absolute science provided the animating force behind many intellectual projects in the modern era, influencing both the social sciences and philosophy. When, in his Preface to the *Metaphysical Foundations of Natural Science*, Kant states, "I maintain that in any particular doctrine of nature only so much genuine science can be found as there is mathematics to be found in it,"[39] he is expressing a claim characteristic of the absolute conception. And when Heidegger writes of Descartes' *Regulae* "Only one who has really thought through this relentlessly sober volume long enough, down to its remotest and coldest corner, fulfills the prerequisite for getting an

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[38] For an extremely illuminating exploration of the political and social dimensions of the early epistemological debates between Baconian and Cartesian science see Shapin and Schaffer's *Leviathan and the Air Pump* by (Princeton: Princeton University Press, 1989).

[39] Immanuel Kant, Preface, *Metaphysical Foundations of Natural Science.* (Cambridge: 2004).

inkling of what is going on in modern science...out of these [rules] the basic character of modern thought leaps before our eyes,"[40] he articulates a view about the genealogy of modern thought that is widely recognized as important (although perhaps not sufficient).

Space does not allow a full examination of the history of this ideal within the social sciences, but a few examples are illustrative of its character. Not only did the ideal of absolute social science demand an "objective" account of the operations of society, but this ideal also tended to crowd out traditional considerations of ethics and politics.

The influence of the ideal was particularly strong in France (the country of Descartes) after the Revolution. Burke was among the first to draw attention to the political dangers of that Revolution and the intellectual errors of its engineers, who presumed to have developed a "political metaphysics."[41] The revolutionaries were driven, in Burke's view, by a false, scientific conception of what society could be, one which intrinsically excluded what Burke considered the real substance of politics and morals and masked the revolutionaries' own blind ambition:

> They have much, but bad, metaphysics; much, but bad, geometry; much, but false, proportionate arithmetic; but if it were all as exact as metaphysics, geometry, and arithmetic ought to be, and if their schemes were perfectly consistent in all their parts, it would make only a more fair and sightly vision. It is remarkable, that in a great arrangement of mankind, not one reference whatsoever is to be found to any thing moral

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[40] Martin Heidegger, "Modern Science, Metaphysics, and Mathematics" in *What Is a Thing?* Trans Barton and Deutsch (Chicago: Regnery, 1967), 276.

[41] Edmund Burke, *Reflections on the Revolution in France* in *Select Works of Edmund Burke.* A New Imprint of the Payne Edition. Foreword and Biographical Note by Francis Canavan (Indianapolis: Liberty Fund, 1999). Vol. 2. paragraph 239.

or any thing politic; nothing that relates to the concerns, the actions, the passions, the interests of men.[42]

Burke suggested that a mistaken confidence in the scientific principles of the Revolution fueled its brutal reign of terror and he further argued that scientific frameworks could not recognize or create the true foundations of political society.

The development of scientific social thought in France later formed the core exemplars in Friedrich Hayek's "abuse of reason" project, in which he criticized the intellectual ideal of "scientism." Many of the same French thinkers who elicited Hayek's condemnation were singled out more recently in Yuval Levin's cautionary rehearsal of the totalitarian uses of science in his book, *The Tyranny of Reason*[43] and in Michael Oakeshott's *Rationalism in Politics*.[44] The well known characters in these accounts exemplify how the absolute conception of science shaped influential approaches to social inquiry.

The Marquis de Condorcet's (1743-1794) *Sketch for a Historical Picture of the Progress of the Human Mind* stands out as an extraordinarily prescient document in the history of social thought, envisioning many developments such as social security schemes that would be successfully realized centuries later. His studies also provided many useful and lasting insights relevant to the design of voting systems. Condorcet was convinced that social progress would be the work of science and that social science could become an absolute science. Thus he noted, "The only foundation of belief in the natural sciences is

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[42] Ibid., part XII paragraph 449.

[43] Yuval Levin, *The Tyranny of Reason* (Maryland: University press of America, 2000).

[44] Michael Oakeshott, *Rationalism in Politics* (Liberty Fund, 2010).

the principle, that the general laws, known or unknown, which regulate the phenomena of the universe, are regular and constant," and asked "why should this principle, applicable to the other operations of nature, be less true when applied to the development of the intellectual and moral faculties of man?"[45] Condorcet further believed in the reductionist program characteristic of the absolute conception of science, as well as in the conciliatory promise of scientific knowledge, suggesting "All the errors in politics and in morals are founded upon philosophical mistakes, which, themselves, are connected with physical errors." Although Condorcet was among the earliest social theorist to recognize the extraordinary utility of probability theory for scientific inquiry and the management of social risks, probable knowledge did not strike him as threat to the project of discovering regular, constant laws. Rather than an inferior form of knowledge, he thought probability supplied but one path to the genuine knowledge of laws. Ultimately, Condorcet conceived of no limits to the scientific enterprise extended to society.

Henri de Saint-Simon (1760-1825), a figure of marginal importance in his own time but influential for wide range of theorists and social movements in the following generation called Condorcet's *Sketch* "one of the most beautiful productions of the human mind."[46] It inspired Saint-Simon's own project, which aspired to unify all knowledge,

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[45] M. de Condorcet, Translated from the original French. *Sketch for a Historical Picture of the Progress of the Human Mind* (Philadelphia: Online Library of Liberty, 1976) Epoch 10.

[46] Henri de Saint Simon, quoted in Mary Pickering, *Auguste Comte: An Intellectual Biography* Cambridge; New York: Cambridge University Press, 1993) 71.

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examining and coordinating it "from the point of view of physicism."[47] In particular, he argued that the scientific direction of society would be possible "by regarding our social relationships as physiological phenomena." For this to succeed, however, "It is necessary that the physiologists chase from their company the philosophers, moralists, and metaphysicians, just as the astronomers have chased out the astrologers and the chemists have chased out the alchemists."[48] Saint-Simon envisioned a world governed by his "Council of Newton" composed of mathematicians, scientists, and artists tasked with discovering "a new law of gravitation applicable to social bodies."[49] Indeed, according to Saint-Simone it was a great failure of Newton not to have extended his theory of gravity into an all encompassing philosophical system for society.

This seemingly fanciful vision inspired Saint-Simon's most influential student, Auguste Comte, to seek the advent of the "age of positive science," which would involve "the passage from the idea of many particular laws regulating the phenomena of the diverse branches of physics to the idea of a single and unique law regulating them all."[50] Comte's new science of "social physics," later renamed "sociology," aimed to provide universal understanding of social phenomena through the application of scientific methods. Although Comte denied that this project required representation through

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[47] Henri de Saint Simon, quoted in F.A. Hayek, *The Counter-Revolution of Science: Studies on the Abuse of Reason* 1980) 224.

[48] Henri de Saint Simon, quoted in F.A. Hayek, *The Counter-Revolution of Science: Studies on the Abuse of Reason* 1980) 220.

[49] Robert P. Crease, *The Great Equations: Breakthroughs in Science from Pythagoras to Heisenberg* (London: Norton) 86.

[50] Quoted in F.A. Hayek, *The Counter-Revolution of Science: Studies on the Abuse of Reason* 1980) 228.

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mathematics,[51] his contemporary and sometimes rival, Adolphe Quetelet, who pioneered the use of social statistics, took a view similar to Descartes that equated the advance and universality of science with mathematical formulation: "The more advanced the sciences have become, the more they have tended to enter the domain of mathematics, which is a sort of centre towards which they converge. We can judge of the perfection to which a science has come by the facility, more or less great, with which it may be approached by calculation."[52] Both of these men embraced methods driven programs of social research that hoped to attain the perspective of absolute science as such.

The thought of Saint-Simon would of course (along with Hegel) profoundly influence Marx, whose "scientific socialism" was in Marx's view "a question of laws ..tendencies working with iron necessity towards inevitable results."[53] The sad history of Marx's "science" in the 20th century is well known and a cautionary tale about the totalitarian tendencies latent in "absolute" social science when declares its certainty prematurely - a danger Burke had diagnosed so early.[54]

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[51] Auguste Comte *System of Positive Polity: Theory of the future of man, with an appendix* (Paris: Carilian Ghoeury and Vor Dalmont) 580.

[52] Quoted in F.A. Hayek, *The Counter-Revolution of Science: Studies on the Abuse of Reason* 1980), 357.

[53] R.C. Tucker, *The Marx-Engels Reader* (New York, NY: Norton, 1978) 296.

[54] Admittedly, the varieties of Marxism are vast and their legacies mixed. My quick gloss on the history of scientific marxism is, I think, substantially uncontroversial. See, for example, "The Rise and Fall of 'Scientific Socialism'" by Arthur Mendel (*Foreign Affairs*, October 1966). However, for a richer appreciation of complexities of Marx's own thought and its historical appropriation see Leszek Kolakowski's *Main Currents of Marxism: The Founders, The Golden Age, The Breakdown* (London: Norton, 2005).

The ideal of absolute science guided other schools of social thought in the 20th century, beyond varieties of Marxism and scientific socialism. (Although it is interesting to note that between the communities of "Popular Front" in France, the "Visible College" in England, and "Red Vienna," a substantial number of intellectuals and leading scientists in the first half of the century were strongly devoted to scientific socialism.)

The ideal was alive in Karl Pearson's (1857-1936) influential treatise *The Grammar of Science* (1892), which argued for the methodological unity of the sciences including the social. In a statement wonderfully illustrative of the absolute conception he wrote, "the field of science is unlimited; its material endless; every group of natural phenomena, every phase of social life, every stage of past or present development is material for science. The unity of all science consists alone in its method, not in its material."[55]

"The goal of science is clear," wrote Pearson, "it is nothing short of the complete interpretation of the universe."[56] Moreover, he argued science and its methods extend to every facet of existence - "the material of science is coextensive with the whole life, physical and mental, of the universe" - and the scientific interpretation of the universe is, on this view, the only true interpretation. Pearson asks whether there are certain fields of philosophy such as metaphysics in which the rules of scientific methodology do not apply. He concludes, "these fields, if indeed such exist, must lie outside any intelligible

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[55] Pearson *The Grammar of Science* (London: Adam and Charles Bark, 1900).

[56] Ibid., 14.

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definition which can be given the word knowledge."[57] Only if there are "facts and

sequences to be observed among them" can scientific investigations proceed, otherwise

"the possibility of all knowledge disappears."[58] He thus endorses the radical Cartesian

equation of truth with scientific method. Those who argue that human life cannot be

entirely understood and directed through scientific method are mistaken in Pearson's

view. Rather, "Mental and social facts are...not beyond the range of scientific treatment,

but their classification has not been so complete, nor for obvious reasons so unprejudiced,

as those of physical or biological phenomena."[59] Ultimately, Pearson maintains there is

"no way to gain a knowledge of the universe except through the gateway of scientific

method."[60] Metaphysicians who think otherwise are but poets, trading in imagination

rather than reason, but unaware of this fact.

     Pearson, generally considered the father of mathematical statistics, was someone

whose life work contributed immediately to the conduct of science. A pioneer of research

in biometrics, genetics, and population statistics, Pearson was also a socialist and ardent

proponent of eugenics. Social Darwinism was in many respects a mainstream perspective

in Pearson's time, but his own work in biology help solidify his "scientific view of a

nation," in which peoples were kept efficient "chiefly by way of war with inferior

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[57] Ibid., 15.

[58] Ibid.

[59] Ibid., 16.

[60] Ibid., 17.

races."[61] The laws of social evolution are, according to Pearson, ultimately rooted in determined, human biology. He is aware that human agency seems to pose a challenge for the scientific control of society, but scientifically understood, "the anti-social will itself is seen as a heritage from bad stock, or as a arising from the conditions of past life and training. Society begins more and more to regard incorrigible criminals as insane, and slight offenders as uneducated children."[62]

In his more methodological contributions to social inquiry Pearson helped replace the concept of "causation" -which raised unnecessary philosophical problems in his view - with the statistically shaped concept of "intensity of association" (alternatively "contingency" or "correlation") which capture the forces at work on an object better than attempts to enumerate a host of discrete, individuated causes. Thus, while he departed from the simplistic mechanistic determinism characteristic of Newtonian physics, his philosophy of science was still "realist" and committed to the ideals of absolute science, namely: a universal method, able to comprehend all facets of the universe including human society through mathematical analysis.

The aspiration to make social science an absolute science did not necessarily entail excluding culture as an object of study, as might be suggested by the more ambitious claims concerning the reduction of human behavior to physics. Bronisław Malinowski (1884-1942), perhaps the most influential anthropologist of the 20th century,

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[61] Karl Pearson *National Life from the Standpoint of Science* (London: Adam and Charles Black, 1905), 44.
[62] Ibid., 125.

articulated a sophisticated account of the cultural foundations of human life. However, his ultimate thesis aligned him firmly within the ideal of absolute science, as evidenced by the claims of his most famous article, "Culture as a Determinant of Behavior." In it he concludes, "Culture is a determinant of human behavior, and culture as a dynamic reality is also subject to determinism. There exist scientific laws of culture (440)."

According to Malinowski, the "faith" behind his work was that humanity could only escape the dire straits in which it found itself though "the establishment of a rational and empirical, that is, scientific, control of human affairs. "[63] Unsurprisingly, he believed his own project of scientific cultural anthropology "can and must provide the foundations of the social sciences."[64] Moreover, he was envious of the advances of the natural sciences and saw those advances as dangerous in themselves, unless the social sciences lived up to their scientific possibilities and provided the tools to engineer a new social order: "The greatest need of to-day is to establish a balance between the stupendous power of natural science and its applications and the self-inflicted backwardness of social science and the consequent impotence of social engineering."[65] Few cultural anthropologists today continue to share this view of the scientific nature and promise of the discipline, but the vision that Malinowski articulated is precisely what would be required if the discipline were to live up to the ideal of absolute science.

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[63] Bronislaw Malinowski "Culture as a Determinant of Behavior" in *Sex, Culture and Myth* (New York: Harcourt, Brace & World, 1962), 167.

[64] Ibid.

[65] Bronislaw Malinowski "Culture as a Determinant of Behavior" *The Scientific Monthly*, Volume 43, Issue 5, 440-449

Perhaps the most notable and influential expression of how social science could become an absolute science came mid-century from Carl Hempel (1905-1997). Known for his "deductive-nomological" account of scientific understanding, Hempel argued that all genuine explanation in both the natural and social sciences depends on referring to general laws: "In history as anywhere else in the empirical sciences, the explanation of a phenomenon consists in subsuming it under general empirical laws."[66] For Hempel, "the methodological unity of empirical science" - a classic ideal of absolute science to which he was strongly committed - required a unified logic of explanation across all domains of knowledge, including the social.[67] Because "only the establishment of concrete laws can fill the general thesis with scientific content, make it amenable to empirical tests, and confer upon it an explanatory function"[68] social science must, in his account, proceed by formulating and empirically testing hypotheses that provide causal laws from which phenomena of interest could be explained and thus predicted.

He considered three objections to extending this deductive nomological account to social phenomena, all of which he rejected. The first objection was that the events of human history possess a "peculiar uniqueness and unrepeatability" that makes them inaccessible to causal explanation by laws, which presume a class of repeatable events. However, Hempel claimed that events in physical sciences are unique in the same sense

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[66] C.G. Hempel, "The Function of General Laws in History" *The Journal of Philosophy* (1942), 45.

[67] Ibid.

[68] Ibid.

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and all that is required for explanation by law is the repeatability of antecedent characteristics. The point is similar to reasons Pearson invoked for being uncomfortable with the term causality, the problem being that the amount of discrete causes operating on any particular object are vast and in some sense unique in their spatio-temporal combination. Hempel's notion of general law is meant to indicate laws formulated with reference to a particular set of antecedent conditions, which serve as the application criteria for the law. He thought this sort of generality is obtainable for both natural and social phenomena, although he will admit the utility of statistical laws, which serve a halfway house on the road to universal laws.

The second argument Hempel considers is related, namely that since each human person is influenced by his or her own unique history, his or her actions cannot be subsumed under a general law. Again, Hempel argues this is not unique to humans, that history also enters into physical systems but is not an impediment to physical laws (this is perhaps his most implausible dismissal). Finally, Hempel considers the common objection "that the explanation of any phenomenon involving purposive behavior calls for reference to motivations and thus for teleological rather than causal analysis." He admits that purposive behavior is unlike the causality found in physical sciences because it entails taking into account perceptions about the future. However, this simply indicates for him a different type of cause (perceptions of the future acting as a cause), one equally open to investigation by the deductive-nomological method. If anticipation/expectation can be a matter for scientific investigation, then the influence of these perceptions on

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human action can be scientifically understood too. Thus Hempel maintains, "there is no formal difference on this account between motivational and causal explanation."[69]

Hempel was particularly dismissive of historians who try to use an "empathetic perspective" to reconstruct the actions of historical figures from those agent's points of view. Such approaches, Hempel argued, substitute "vague analogies and intuitive 'plausibility' for deduction from testable statements and are therefore unacceptable as scientific explanations."[70] Like Pearson, Hempel was sensitive to worries repeatedly expressed by critics about the implications of his view for human agency. Hempel claimed his positions did "not in any way imply a mechanistic view of man, of society, and of historical process; nor of course, do they deny the importance of ideas and ideals for human decision and action,"[71] although a number of his supporters and detractors thought his account did just that.

At the root of Hempel's vision of science lay a fundamental commitment to the methodological unity of science. This is what he affirmed immediately following his denial that he was committed to a mechanistic view of society: "What the preceding considerations do suggest is, rather, that the nature of understanding, in the sense in which explanation is meant to give us an understanding of empirical phenomena is

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[69] C.G. Hempel and P. Oppenheim. "Studies in the Logic of Explanation." *Philosophy of Science* 15, no. 2 (1948), Section 3.

[70] C.G. Hempel, "The Function of General Laws in History" *The Journal of Philosophy* (1942), 45.

[71] C.G. Hempel, "Explanation in Science and in History," in *The Philosophy of Carl G. Hempel: Studies in Science, Explanation, and Rationality.* Edited by James Fetzer (Oxford:2001) 295.

basically the same in all areas of scientific inquiry."[72] The unity of science - so uniquely characteristic of the absolute conception - is what drove Hempel to believe all human phenomena had to be explained as products of general laws, even as the details of what that might mean became less and less clear over the course of Hempel's scholarly exchanges.

Hempel had a long and productive philosophical career and he would later qualify or reject a number of his early claims about the nature of science published during the 1940's and 1950's (which form the core of the examples above). Thomas Kuhn, Hempel's colleague at Princeton starting in 1964, is reported to have profoundly influenced many of his philosophical shifts. In any case, we see in the early Hempel how the ideal of absolute science some three hundred years after Descartes continued to inspire a particular understanding of social science, viewing its methods as identical to those of the natural sciences, and aspiring to provide a singular, objective account of reality - to which only scientific method provides access. This vision of social science demands the consistent unity of all methods, which is secured through mathematical representation.

This vision of social inquiry was certainly not the only one in modernity. One need only think of the vast range of perspectives represented by the canonical texts of political theory: Machiavelli's understanding of virtu', fortuna, and the role of historical exemplars; Rousseau's account of nature, history, and social psychology; Burke's politics

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[72] Ibid.

of prudence, the German romantics, etc. Indeed there were also alternative approaches to social science more narrowly construed that did not fit the mould of absolute science. We might think of the analysis developed in the *Federalist Papers,* Adam Smith's *Wealth of Nations, or* Alexis de Tocqueville's *Democracy in America* - all projects tightly wedded to understanding and addressing social questions of their times through whatever intelligent resources were available. It is doubtful that any of these figures thought of their enterprises as Baconian, but they did follow a more pragmatic and less methods driven path of social inquiry than those inspired by the ideal of absolute science.[73]

The influence of the absolute conception on social inquiry was mirrored by a similar influence on philosophy and ethics. This history is perhaps even more complex than the story of the social sciences, but the end result was that questions of ethics were increasingly located beyond the domain of reasoned discourse. Initially such questions were understood to be part of disciplines other than science, such as philosophy and theology. However, as these disciplines themselves sought to become "scientific," - a requirement for genuine knowledge within the dispensation of the absolute conception- it

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[73] I have avoiding a discussing "behaviorism," a research program that served as a lightning rod for many social science debates in the mid twentieth century. In fact, behaviorism was a term applied to a number of research programs. Some of them could arguably be categorized as subspecies of an absolute conception while others constituted a transition point between the absolute conception and more pragmatic social science. In any case, it is not essential for my argument that I offer an opinion regarding where different behaviorisms would fall in these categories. Indeed trying to address such a complex issue in passing would detract from the larger argument. ("It is possible to gain knowledge of the actions of men on the evidence supplied by their overt behavior just as it is possible to discover and know the atomic constitution of water on the evidence supplied by the physical and chemical behavior of that substance (Schutz "Concept and Theory Formation in the Social Sciences." *The Journal of Philosophy* 51, no. 9 (1954): 257-73) I would like to admit that it may be possible to gain useful knowledge about human activities that way, but the knowledge gained will be of a very different sort than that gained by the same sort of study of molecules.)

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became less and less clear how to understand the material of ethics as an object of reason if ethics had to be considered scientifically.

The separation between science and the interrelated concerns of "morals, ethics, and politics" began early. The Royal Society of London for the Improvement of Natural Knowledge was founded in 1660 with the explicit charge, "to improve the knowledge of natural things, and all useful arts, manufactures, mechanical practices, engines, and inventions, by experiments (not meddling with Divinity, metaphysics, morals, politics, grammar, rhetoric, or logic)."[74] The exclusion of morals and politics was partly a condition for receiving its Royal charter from Charles II. Associations were still considered politically dangerous entities in 17th century England, with the civil wars in recent view.

The Royal Society initially dedicated itself to the development and discussion of experiments, very much in line with the vision of Francis Bacon, one of its most prominent founding members. However, as Mason reports, Bacon's influence "declined during the 1670's and...was supplanted by a 'Galileian' trend as manifest above all in the work of Newton who became a fellow of the Society in 1671."[75] The French Academy of Sciences was founded in 1666 and, as in the English case, served as a locus for scientific discussions, periodically advising the crown on questions of public interest. However, as

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[74] Quoted in Proceedings of the Royal Society of Edinburgh, Volume 9, 485.

[75] Mason, *History of the Sciences* p 260. Quoted in Simon Schwartzman "The Focus on Scientific Activity" in Burton Clark, ed., *Perspectives on Higher Education: eight disciplinary and comparative views.* University of California Press: Berkley, 1984), 202.

Simon Schwartzman notes, in addition to an internal focus on scientific inquiry and technical questions, the members of both these organizations "launched a protracted assault on the traditional culture and philosophy, whose strongholds were traditional universities."[76] These societies were a model of a new sort of learning and sought to influence and displace other modes of inquiry. Newton's thought in particular raised the philosophical profile of the absolute conception that Descartes mapped out, and successes in the natural sciences more generally helped assert the philosophical seriousness of absolute science and its implications for other disciplines. These implications generally involved challenging and abandoning more traditional modes of intellectual inquiry.

The story of how the ideal of absolute science impacted philosophical thought in the modern era is but an aspect of a larger drama concerning the fate of reason in modernity. A central hope for both Bacon and Descartes, as well as later generations of so-called Enlightenment thinkers, was that reason (understood in a variety of ways - e.g. scientific method, *a priori* reflection, inquiry detached from the church/theology) could provide a new and more secure foundation for morality and political life. However, the march of reason also engendered new forms of skepticism that critiqued not only "outdated" forms of philosophy but the capacities of reason itself.

David Hume famously attacked the notion that morals could be grounded in reason and did so in a way that was indebted to a conception of science reflected in his "empiricism." He famously ended his *Enquiry Concerning Human Understanding* with

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[76] Ibid., 202.

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the admonition, "If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion." For related reasons, he concluded "Morals and criticism are not so properly objects of the understanding as of taste and sentiment."[77] That is, values are not facts but rather expressions of agreeableness; and on Hume's view morality simply names conventions that have proved generally useful over time.[78]

Hume's skepticism about morals provided a prominent counterpoint to the enlightenment hope that ethics could be founded upon reason, and sparked a number of renewed attempts to articulate such a foundation, pre-eminently by Kant. Meanwhile, as scientific reason advanced in modern Europe it helped erode previous conceptions of ethics rooted in theology and normative cosmology, at least in the mind of many educated elites. This was, in part, a byproduct of having disenchanting the natural universe and presented a new model of knowledge.

Hume of course thought that as superstition waned, our naturally social sentiments could be better expressed, understood, and channeled. However, others feared that skepticism would in fact lead to a dangerous moral void, and this lent increased

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[77] David Hume, ed. L. A. Selby-Bigge, M.A. 2nd ed. *An Enquiry Concerning Human Understanding* Sect. XII. (Oxford: Clarendon Press, 1902) 366.

[78] Hume was also worried about the deleterious social influences of both superstition (Catholics) and enthusiasm (Protestants). Deflating both of these was an important project alongside Hume's general moral skepticism.

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urgency to the project of grounding morality. Kant tried to do this with a new account of reason that could relate the "noumenal" and "phenomenal." Others, sensing the void, sought to re-enchant the universe through resources beyond cold and calculating reason, and he we can think of various romantic movements in both philosophy and the arts. Out of this complex scene in the 18th and 19th centuries we witness the advent of what Charles Taylor has described as the "many ways" - the development of ethical visions that tend to be more diverse and subjective. There of course remain strong theological communities and philosophical schools committed to articulate ethical systems. But there is also the less articulate development of ethical ideals that are wedded to new forms of social order, as well as a proliferation of ethical convictions that are shaped by individual experiences not closely indexed to philosophical or religious systems.

By the end of 19th century many intellectuals were alarmed by the apparent distance between science and various schools of philosophy. The diversity of philosophical opinions suggested to some that philosophy was not a promising path to knowledge at all. The most radical 19th century critic of reason, Friedrich Nietzsche, perceived not only the "death of god" but also the death of a concept of reason dating back to Plato that hoped to justify humanistic values. Nietzsche saw that with the demise of this concept all values were open to revaluation. He thought this development would signal the advent of an age of nihilism and result in cataclysmic warfare. Some bold individuals might be able to face up to the abyss - the fact there is no truth or ground for morality - but most would crushed by this revelation. Nietzsche expressed one direction in which the collapse of reason/philosophy might lead.

A very different sort of response to the diversity of philosophical opinion and the lack of foundations was to try to make philosophy itself scientific. According to Alan Richardson this was the underlying aspiration behind the philosophical approach of "logical positivism," which dominated so much of 20th century philosophy.[79] As we will see, although adherents of this school were completely at odds with Nietzsche's perspectivalist and deflationary account of truth, they shared with both him and Hume the view that ethics could not be a subject of reason.

Richardson traces the origins of logical positivism to a malaise and dissatisfaction with interminable philosophical debates on both sides of the Atlantic at the beginning of the 20th century, as well as the declining public import of philosophy when compared to the sciences. Richardson contends that at this time "pragmatism," as represented in America by C.S. Pierce, William James, and Josiah Royce, presented a formidable approach to philosophy that engaged with science and yet did not aspire to become an absolute science in itself. However, when Dewey inherited the philosophical spotlight as a spokesman for pragmatism around 1914, Richardson argues that Dewey became a transitional figure moving American pragmatism closer to an ideal of philosophy explicitly modeled on science.

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[79] This account forms the basis of Richardson's forthcoming book, my knowledge of which comes through his public lectures at the 2009 Vienna Institute Summer University on "The Culture of Science and Its Philosophy."

In any case, the main arena in which logical positivism developed was Vienna, amongst a remarkable group of intellectuals most of whom were associated with the Vienna Circle. The central claim for the approach of logical positivism pioneered by this community was that philosophy itself could be scientific- that is, continuous with the methodological project of absolute science. Of course it is easy to see why this would have to be the ultimate fate of philosophy if one took the ideal of absolute science seriously since, according to this conception, all knowledge can ultimately be accessed only through scientific method.

The central task for logical positivists was therefore to convert philosophical questions into scientific ones, which involved, as the moniker suggests, a combination of empiricism and the mathematical formalism of logic. Philosophy thus understood could be part of the larger enterprise of science, the aim of which, according Otto Neurath, a central figure of the Vienna Circle, "is to reach the goal, unified science, by applying logical analysis to the empirical material."[80] The philosopher Max Black later explained the idea behind philosophy's turn towards science in terms clearly indebted to the absolute conception: "By adopting the scientific method philosophers are to learn from scientists and mathematicians how to agree; and steady calculation, guaranteed to produce an acceptable answer, is to replace philosophical disputation."[81]   In an attempt

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[80] Otto Neurath, "The Scientific World Conception" in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 33.

[81] M. Black *Philosophical Analysis: A Collection of Essays* (Books for Libraries, 1971).

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to secure the scientific status of philosophy and end disputation, however, ethics was explicitly expelled as an object of rational inquiry.

The philosophical vision that animated most members of the Vienna Circle and the larger approach of logical positivism was outlined with exquisite clarity in Otto Neurath's famous essay "The Scientific World Conception." The primary goal ahead, Neurath states, is "unified science." This can only be achieved in the sciences and philosophy by "the search for a neutral system of formulae, for a symbolism freed from the slag of historical languages, and also the search for a total system of concepts. Neatness and clarity are striven for, and the dark distances and unfathomable depths rejected."[82] By becoming scientific, philosophy could end its interminable debates, because, "The scientific world conception knows no unsolvable riddle."[83]

The process of making philosophy scientific was a twofold enterprise. First, philosophical problems had to be formalized and linked to empirical questions. Only in this way could the meaning of any statement be truly understood. Thus Moritz Schlick's famous formulation of the verifiability principle, which was to guide a new sense of philosophical meaning: "The meaning of a proposition is the method of its verification."[84] However, this process had the secondary purpose of alerting philosophers to problems that could not be empirically formalized, thereby suggesting such problems were mere

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[82]Otto Neurath, "The Scientific World Conception" in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 31.

[83] Ibid., 31.

[84] Moritz Schlick "Meaning and verification" *Philosophical Review* Vol. 45, No. 4 (July 1936), 339-369.

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illusions and had no meaning. This was the dual agenda for how logical positivism sought to transform old philosophical debates: "Clarification of the traditional philosophical problems leads us partly to unmask them as pseudo-problems, and partly to transform them into empirical problems and thereby subject them to the judgement of experimental science."[85]

The pseudo problems on this view turn out to be the problems of ethics, metaphysics, and theology: "The metaphysician and theologian believe, thereby misunderstanding themselves, that their statements say something, or that they denote a state of affairs. Analysis, however, shows that these statements say nothing but merely express a certain mood and spirit. To express such feelings for life can be a significant task. But the proper medium for doing so is art, for instance lyrical poetry or music."[86] Thus the position many of the logical positivists arrive at is similar to Hume's. Ethical claims are have no empirical meaning and are thus not matter for reasoned consideration in themselves. They can be analyzed according to their effects, but they are essentially just expressions of emotional states.

Neurath hoped that, if carried through, the project of logical positivism would order all concepts into "a reductive system, a 'constitutive system'."[87] He further explained that at its bottom such a system would contain "concepts of the experience and

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[85] Otto Neurath, "The Scientific World Conception" in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 31.

[86] Ibid.,32.

[87] Otto Neurath, "The Scientific World Conception" in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 33.

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qualities of the individual pysche" and then "in the layer above are physical objects; from these are constituted other minds and lastly the objects of social science."[88] The merger of philosophy into science and the unity of all the sciences would thus include a foundation for the social sciences, but exclude ethics as a branch of philosophy.

Logical positivism hit a number of hurdles in the middle of the 20th century within its own terms.[89] Wittgenstein, whose short *Tractatus* had been an inspiration to Vienna Circle, forged a very different path in his *Philosophical Investigations*, which illustrated amongst other things the difficulty of understanding "meaning" in positivist terms.[90] The details of the verifiability principle inspired a set of debates often identified with Popper about the nature of induction and the implications of probabilistic reasoning. Rudolph Carnap argued for the impossibility of "absolute verification" and sought to amend aspects of Schlick's account by introducing a probabilistic notion of theory testing. However, most of these figures, particularly Carnap, continued to maintain the underlying view that logical positivism showed that there were no fields of philosophical inquiry beyond those that could be conducted within the terms of science. Quine, who in his own way challenged central tenets of logical positivism through his attack on the

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[88] Ibid.

[89] For an account of the ethical challenges that logical positivists encountered in the context of the ideological battles of the Cold War see George Reisch, *How the Cold War Transformed Philosophy of Science : To the Icy Slopes of Logic* (Cambridge, UK; New York: Cambridge University Press, 2005). Reisch argues that the search for values in the wake of Nazism and the ideological struggle of the Cold War convinced many to turn away from the ethical quietism of logical positivism.

[90] See Brian McGuinness "Wittgenstein and the Vienna Circle" *Synthese* Volume 64, Number 3, 351-358.

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analytic-synthetic distinction, nonetheless endorsed the more fundamental project merging science and philosophy, famously asserting that "philosophy of science is philosophy enough."[91]

Logical positivism was, of course, not the only philosophical game in town during the early and mid twentieth century, although its influence was certainly widespread. Its guiding principles flowed, as I have tried to show, from the ideal of an absolute conception of science that we can trace back to the Cartesian vision. Logical positivism was perhaps the most serious and ambitious attempt to see the ideal of absolute science extended beyond the domain of the natural sciences. As we saw, a number of approaches to social inquiry/social science were likewise animated by this ideal. It is understandable why social scientists in the first half of the twentieth century found additional encouragement from and resources in the project of logical positivism. The reader may not be surprised to learn that Hempel was not only a close reader of Neurath's but also a student of Carnap's, and later serve as Carnap's assistant at Chicago.

Attempts to conform social science and philosophy to the ideals of absolute science have been distinguished by the emphasis they place on the primacy of method, the necessity of mathematical representation, ultimate reduction, and a belief in the universal scope of their approaches. More generally, the absolute conception of science within modernity helped forge two convictions that proved widely influential: 1) the idea

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[91] W. V. Quine Mind, *New Series*, Vol. 62, No. 248 (October 1953), 446.

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that scientific methods provide the only lens through which we gain genuine knowledge of the social world (and this knowledge will characteristically be objective, reductive, and mathematical), 2) knowledge, thus construed, requires excluding ethics, because ethics is not the provenance of reason. In practice, however, attempts to realize the aspirations of absolute science have continually run into difficulties reducing, predicting, and controlling human life. Beyond their internal disputes, these attempts have attracted persistent external criticism from those who believed the ideal of absolute science was fundamentally a mistake, at least in reference to mankind and its history.

As I have suggested, I count myself among those who think the ideal of absolute science is fundamentally mistaken. Space does not allow for a full critique of this ideal along with a consideration of the extraordinarily complex epistemological and ontological issues at stake. Fortunately, however, many insightful critiques have been developed at length elsewhere. Even more fortunately, in the second half of the 20th century mainstream intellectual perspectives increasingly rejected the ideal of absolute science, particularly with reference to human life. Thus, I take my task not to be one of tearing down the absolute conception (since that work has been largely accomplished) but of showing the promising and yet unrealized implications of having abandoned this ideal.

2.5 The Transition from Absolute to Pragmatic Science

It is useful to briefly consider some of the more prominent reasons the absolute conception fell out of favor, along with the alternative visions of social science and ethics

that were articulated in response (although I will delay an extensive discussion of alternative accounts of ethics until a later chapter). The shift away from absolute social science was influence by three factors worth highlighting:

1)   Absolute social science failed to deliver the goods it promised. The confidence that French positivists or scientific socialist had in their methods stood in stark contrast to their abilities to shape social realities on the ground. This was certainly not for lack of trying. Many extraordinary minds and formidable political and economic resources were devoted to achieving the scientific mastery of society. Although western societies did undergo radical changes over the past few centuries, including the "rationalization" of many public institutions, history seldom developed along the lines sought by social engineers. The failures of scientific socialism were particularly pronounced, because the control that social planners sought to exercise proved inimical to the operation of markets. More generally, modern conceptions of freedom and liberty, which many hoped could be fully realized through the scientific control of society, instead showed themselves to be in fundamental tension with strategies of scientific social management. Ultimately the aspiration to achieve an absolute science of society came to appear more utopian than scientific.

2)   Intellectual currents in the second half of the twentieth century were profoundly influenced by a "historical turn" in philosophy and science studies, which deflated the notion that science was an unbiased, progressive, objective, and purely

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logical enterprise. Kuhn's *The Structure of Scientific Revolutions* drew attention ways in which scientific inquiry was socially embedded and thus reflected apparently contingent influences. Kuhn's new way of thinking about the history of science implicitly questioned the guarantees of certainty supposedly built in to the methodological foundations of science. This touched off deep debates about whether science was simply "socially constructed," or could still claim to be a rational, realist enterprise, with rival positions staked out, for example, by Imre Lakatos and Paul Feyerabend.

A generation of scholarship within science studies illustrated deeper and deeper social influences at work in the development of scientific theories, adding further fuel to the social constructionist debates.[92] The so called "science wars" within academia were not always the most civil and lucid exchanges, but a scholar like Ian Hacking provides a good overview of the claims and issues at stake.[93] These debates elicited various responses from philosophers - including, on one hand, a range of positions supportive of the constructionist view from Rorty's *Philosophy the Mirror of Nature* to Derrida's deconstruction and, on the other, a range of positions arguing for the rationality of science in terms other than those of the absolute conception.

The manifest technological achievements of the natural sciences make it extremely difficult to deny that science has provided unique and genuine knowledge of reality, and philosophers such as Ronald Giere and Nancy Cartwright provided some of

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[92] See, for example *Leviathan and the Air Pump* by Shapin and Schaffer (Princeton: Princeton University Press, 1989).

[93] See Ian Hacking *The Social Construction of What* (Cambridge: Harvard University Press, 2000).

the more compelling accounts of how the natural sciences could hope to be a realist enterprise without adopting the claims of the absolute conception. Both of their approaches embraced pragmatic standards of inquiry and abandoned the idea of natural "laws" in favor of a more limited and instrumental view of "models." This sort of alternative vision of science will receive greater attention below.

3)   The ideal of absolute social science had always attracted critics who argued that scientific methods could never grasp the inter-subjective meanings that motivate human action and thus could not in principle capture all of social reality. By the turn of the century these objections had coalesced into a standard "interpretivist" or hermeneutic critique of the social sciences. There were different paths, however, for this sort of critique to take.

Wilhelm Dilthey (1833-1911) famously demarcated the *naturwissenschaften* (natural sciences) from the *geisteswissenschaften* (human sciences), and argued that the later required their own unique methods based in hermeneutics. However, as I will show in more detail later, Dilthey's account of hermeneutics aspired to its own sort of positivism reminiscent of an absolute conception of science. As a method, Dilthey claimed hermeneutic understanding enabled "individually structured consciousness [to]

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reconstruct - and thereby know objectively - the distinct individuality of another."[94] He believed hermeneutics could provide clear and universal access to the character of human meaning, and thus the human sciences could be true/complete sciences, only approached through this different kind of method, which had its own guarantee of truth. This positivist account of hermeneutics was abandon by most later thinkers associated with the term.

Mid-century, Peter Winch staked out a related but ultimately very different account of the implications of the interpretive problem for the social sciences. Building on insights from Wittgenstein's later work, Winch illuminated how humans generally act on the basis of reasons that can only be understood as internal to the particular ways of life that give acts meaning. Thus, according to Winch, "understanding" human action entails "grasping the point or meaning of what is being done or said"[95] and this, he notes, "is a notion far removed from statistics and causal laws."[96] These sorts of claims were continuous with classic hermeneutic arguments. More controversial was Winch's conclusion that "the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanations offered in the natural sciences."[97]

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[94] Dilthey, "The rise of Hermeneutics" in *Philosophies of Social Science: The Classic and Contemporary Readings*. edited by Delanty and Strydom. (Open University Press, 2003)*,* 98.

[95] Peter Winch. *The Idea of a Social Science and Its Relation to Philosophy*. 2nd edition (Routledge: 1990) 115.

[96] Ibid., 115.

[97] Ibid., 72.

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In calling these two types of explanations logically incompatible, Winch meant to attack more than just the reductionist dream of understanding everything in terms of material causes. That ideal of absolute science is automatically rejected by those who accept the basic insight of hermeneutics. Rather, Winch also meant to challenge the notion that methods employed in the natural sciences, which sought to discover causes through the analysis of empirical regularities, could have any valid applications in social inquiry. Winch's conclusion forms the core of his argument with Weber, who, despite affirming the distinctive importance of "interpretive understanding" for the social sciences, "thinks the kind of law that the sociologist may formulate to account for the behavior of human beings is logically no different from a 'law' in natural science."[98]

Winch's argument about the *logical* incompatibility of human life with the sort of explanations sought by scientific methods was complex, and he tried to spell it out from different angles. The "sociological laws" that Winch rejected in principle referred not (only) to universal, deterministic laws but rather "statistical regularities." Statistics, Winch held, cannot aid our understanding human action, only "better interpretation" can do that.[99] The crucial claim for Winch is that "a context of humanly followed rules ...cannot be combined with a context of causal laws."[100] Thus he thought that the interpretive nature of human understanding and action demanded a complete break with methods of inquiry drawn from the natural sciences. Genuine social science, on this view,

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[98] Ibid., 117.

[99] Ibid., 115.

[100] Ibid., 117.

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learns nothing from the methods of natural science, but can only proceed through the work of interpretation, explaining human activities through the particular, subjective concepts of meaning structures and language games. So Winch endorsed the complete separation of the natural and social sciences in a manner similar to Dilthey, although Winch did not think the interpretative problem could be objectively solved through hermeneutic methods. Our interpretations, on his view, were never perfect, only better or worse, more or less insightful and persuasive. But "scientific methods" could never form a proper framework for social inquiry.

Like Dilthey's, Winch's account has been largely rejected by mainstream perspectives in the philosophy of social science. Alasdair MacIntyre articulated a set of objections to Winch's position that, to my mind, proved persuasive on their own merits and characteristic of the larger problems in Winch's thought. According to this critique, Winch was not attentive to the way in which reasons act as "causes" and thus he mistakenly rejected the idea that systematic causes might operate within certain social phenomena. An even greater error, in MacIntyre's view, was Winch's claim that human actions could only be understood in terms of concepts that the agent in question had access to. "There is a connection," MacIntyre suggests, "between Winch's view that social science is not appropriately concerned with causal generalizations and his view that only the concepts possessed by the members of a given society (or concepts logically tied to those concepts in some way) are to be used in the study of that society."[101]

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[101] Alasdair MacIntyre, "The Idea of a Social Science" in *Against the Self-Images of the Age: Essays on*

MacIntyre argues that this second principle would rule out a wide range of explanations of behavior, such as ideology and false consciousness, where we rightly have reasons to believe people are influenced by causes that they themselves did not grasp conceptually. Moreover, MacIntyre points out, "social scientists are concerned with the causes and effects *of being unemployed, having kin relations of a particular kind, rates of population change*, and a myriad of conditions of individuals and societies, the descriptions of which have a logical character other than that of action descriptions."[102] So, plainly, not all social phenomena can be considered artifacts of intentional human action.[103] MacIntyre's final criticism of Winch's account was that it entailed a kind of epistemological relativism because it implied we could never be in a position to critique the actions of agents from perspectives that are not their own.

Although few still adhere to Winch's particular account of social science, there are many critics of social science who think the truth of hermeneutics entails that social investigations that draw on methods pioneered in the natural sciences are fundamentally mistaken. They would like to draw a firm line that excludes the application of "scientific methods" in investigations of social phenomena. Often these critics are quite right to attack the idea that the social sciences could live up to the absolute conception of science, which the insights of hermeneutics show to be implausible. However, these critics seem

Ideology and Philosophy (Indiana: University of Notre Dame Press,1978) 266.

[102] Ibid., 222.

[103] By "intentional" here I mean mediated by intentionality not the more colloquial "foreseen."

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even less aware than Winch of the how the social sciences might coherently proceed by drawing on "scientific methods" without any of the misplaced epistemological pretensions of achieving an absolute perspective. Even if the truth of hermeneutics does illustrate the impossibility of "absolute" social science, this truth does not preclude the value of analytic social inquiry. A more Baconian approach to social science could find both the insights of hermeneutics and the analytic methods of science useful for understanding and addressing social problems.

2.6 The Possibilities of Pragmatic Science: Weber

The classic attempt to integrate a recognition of the hermeneutic dimensions of human life with an agenda of social scientific research drawing on "scientific methods" was forged by Max Weber at the beginning of the 20th century. Weber's studies were widely influential, although his methodological statements, particularly his *Methodology of the Social Sciences*, which I will examine below, were generally neglected by mainstream social scientists of the day. Weber is often credited with pioneering "methodological antipositivism," and this is true in the sense that he opposed what we can now recognize as accounts of "absolute" social science. In this vein he asserted, "there is no absolutely 'objective' science of culture."[104] Yet, Weber was deeply

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[104] Max Weber 'Objectivity' in social science" in " in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 111. [Excerpted from Weber's *The Methodology of the Social Sciences* (Free Press, 1949)]

committed to "an empirical science of concrete reality."[105] How did these two commitments fit together?

Weber's account of the importance of "verstehen" (interpretive understanding) for social inquiry greatly expanded the fundamental insights of Dilthey without embracing Dilthey's positivism. Weber built upon the basic claim that we have to understand social phenomena as arising from meaningful categories of human experience. Unlike the absolute perspective sought by the natural sciences, Weber maintained, "All knowledge of cultural reality..is always knowledge from particular points of view."[106] It is only from such points of view that we can begin to recognize and categorize many human activities. Weber argued we cannot discover what is meaningful to us from a "'presuppositionless' investigation of empirical data...Rather perception of its meaningfulness to us is the presupposition of its becoming an object of investigation."[107] So, social science is always indebted for its very parameters and problems to interpretive perspectives, which can never claim some trans-human objectivity. Moreover, since human behavior is shaped by meanings that are culturally contingent, understanding people's actions will often involve reconstructing the internal logic of their interpretive frameworks, and this is unlike anything studied by the natural sciences.

Thus, Weber concludes, "In the social sciences we are concerned with psychological and intellectual phenomena the empathetic understanding of which is

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[105] Ibid.

[106] Ibid., 118.

[107] Ibid., 114.

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naturally a problem of a specifically different type from those which the schemes of the exact natural sciences in general can or seek to solve."[108] This raise a distinctive methodological problem, which he describes as follows: "We have designated as 'cultural sciences' those disciplines which analyze the phenomenon of life in terms of their cultural significance. The significance of a configuration of cultural phenomena and the basis of this significance cannot however be derived and rendered intelligible by a system of analytic laws, however perfect it may be, since the significance of cultural events presupposes a value orientation towards these events."[109]

Unlike Dilthey and Winch, however, Weber did not conclude that social inquiry is unable to draw on approaches from the natural sciences. To the contrary, he held that we could subject social phenomena to detailed, analytic investigations and these could provide causal insights that are not otherwise obvious. Moreover, Weber was not opposed to trying to analyze social realities in terms of laws, regularities, and general concepts. Indeed he thought one might discover law like regularities and these could coincide in certain instances with the interpretive project of understanding the nature and effects of culturally contingent meanings; but he cautioned "it would be disastrous if their occasional coincidence caused us to think that they were not distinct in principle."[110] Society was not predetermined by some universal system of laws, although Weber recognized that systematic causes could indeed be operative in society in ways that lend

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[108] Ibid.,113.

[109] Ibid., 114.

[110] Ibid.,115.

themselves to "scientific investigation." However, he pointed out that these regularities are generally derivative of widely held interpretive frameworks, and thus lacked the universal and static character of physical laws.

Weber also had a lawyerly concern for establishing causality with regard to specific incidents and problems. He argued that our capacity to interpret the behavior of others often granted us genuine causal insights, useful for both explaining past events and predicting future ones. Thus Weber was ultimately a realist and believed we could attain a great deal of causal knowledge with regard to the social world. He defined sociology as "the science whose object is to interpret the meaning of social action and thereby give a causal explanation of the way in which the action proceeds and the effects which it produces."[111] Although our knowledge of cultural realities depends on our interpretive skill, Weber maintained that this knowledge "it is entirely   causal knowledge exactly in the same sense as the knowledge of significant concrete natural events."[112] A cause is a cause for Weber. Social events are no less caused than physical events and detailed investigation can uncover causality in both domains. Weber realized that the social sciences could never attain the same level of universality and certainty as the natural sciences, but the purpose of both - discovering causality - was the same.

As Stephen Turner, a Weber scholar who writes on the philosophy of social science, explains, Weber's methodological commitments were both nuanced and

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[111] Max Weber "The Nature of Social Action" in *Max Weber: selections in translation*. Edited by Runciman. (Cambridge:1978) 7.

[112] Max Weber 'Objectivity' in social science" in " in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 118.

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pragmatic. For Weber, "The determination of causality or responsibility did not require scientific laws, but required a judgment that, in a class of similar cases, subtracting a given condition would have lowered the probability of the outcome."[113] Moreover, "the model also allowed explanations of ordinary intentional actions as simultaneously intentional and causal."[114] So, Weber thought the truth of hermeneutics did not preclude a search for causality in social phenomena in ways that drew upon methodological insights from the natural sciences.

However - and this is extremely important - Weber's pragmatic understanding of the social sciences drove a wedge between him and those who hoped that social science could be absolute. Weber maintained that "an 'objective' analysis of cultural events, which proceeds according to the thesis that the ideal of science is the reduction of empirical reality to 'laws,' is meaningless."[115] Thus, although Weber admits we may discover useful "social laws," these do not grant knowledge of social reality in itself, but are simply what he describes as "one of the various aids used by our mind" to understand that reality.[116]

Weber's commitment to causal realism also has to be qualified by his claim that we can never arrive at a complete causal understanding of a social event. There is never a single, determinate law at work, but always the confluence of a multitude of factors, only

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[113] Stephen P. Turner "Cause and the Persistence of Teleology" in *The Blackwell Guide to the Philosophy of the Social Sciences* (Malden, MA: Blackwell Pub., 2003) 33.

[114] Ibid.

[115] Ibid.,116.

[116] Ibid.,117.

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some of which are knowable or amenable to intervention. Weber explains why ultimate causality is not a proper aim of social science in a compact passage:

> An exhaustive causal investigation of any concrete phenomena in its full reality is not only practically impossible- it is simply nonsense... where the individuality of a phenomenon is concerned, the question of causality is not a question of laws but of concrete causal relationships; it is not a question of the assumption of the event under some general rubric as a representative case, but of its imputation as a consequence of some constellation.[117]

What Weber describes as a "constellation" we might refer to as a set of important factors. Although there may be, technically speaking, a nearly infinite set of discrete causes that influence any particular outcome, we should only be interested in the distinctive contribution of the few that we can hope to understand and control.

Ultimately, for Weber, noting social regularities is never the fundamental aim of the social sciences. The aim is simply to understand what is going on well enough to predict and thus direct future developments. Thus he states "the knowledge of causal laws is not the end of an investigation but only a means,"[118] and he later expands this point: "the establishment of regularities is not the end but rather a means of knowledge. It is entirely a question of expediency, to be settled separately for each individual case, whether a regularly recurrent causal relationship of everyday experience should be formulated into a 'law'."[119]

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[117] Ibid., 116.

[118] Ibid.

[119] Ibid., 117.

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Weber emphatically rejected the ideals of absolute social science. He did not believe we could obtain a completely objective stance free of the interpretive perspectives that characterize human life, which are always essentially contestable. Moreover, he did not think social phenomena were governed by trans-historical, deterministic laws. Thus he attacked, "the meaninglessness of the idea which prevails occasionally even among historians, namely, that the goal of the cultural sciences, however far it may be from realization, is to construct a closed system of concepts, in which reality is synthesized in some sort of permanently and universally valid classification and from which it can again be deduced."[120]

Weber did hold that careful investigation could reveal regularities in society that could be useful for understanding causal influences and addressing social problems. However, the pragmatic orientation of this account meant that generalizations were not the goal, and Weber argued, "In the cultural sciences, the knowledge of the universal or general is never valuable in itself."[121] One commentator aptly sums up Weber's methodological position as follows:

> Weber rejects the notion that historical science can be adequately comprised under the nomothetic model. Instead he stressed the importance for history of what can be 'narrative causality': one 'imputes' causality by referring a particular, unique, historical 'constellation' to a foregoing constellation without reference to a general law - although law like generalizations have a vital role in telling us what is likely and unlikely in history.[122]

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[120] Ibid., 119.

[121] Ibid., 117.

[122] John Milbank *Theology and Social Theory: Beyond Secular Reason* (Cambridge, Mass., USA: B. Blackwell, 1991) 83.

Weber insisted that the goal of the social sciences was not to produce the kind of universal knowledge esteemed by "absolute science." Rather, social science, which necessarily depended on interpretive skill, could at best produce qualified, causal insights useful for our purposes, be they historical understanding or policy advice.

Although there is plenty that one might dispute in Weber's sociological studies, he was in many ways a methodological visionary, having articulated a pragmatic account of the social sciences over and against what we can identify as an absolute conception. There are many details of his account that are underdeveloped by the standards of contemporary social science research, many particular methodological tools he did not discuss or foresee, and certain loose ends that require more thorough going argumentation. However, as mainstream social scientists began to consider these issues in more depth around the middle of the century and developed an explicitly pragmatic account of their methods, they built on Weber's fundamental break with the project of absolute social science, whether or not his influence was acknowledged. I will consider these subsequent developments supporting a full blown pragmatic account of the social sciences shortly, but need finally to note an area in which Weber's vision was both lacking and influential.

Weber preserved one characteristic feature of the absolute conception of science that I believe was both mistaken on its own terms and proved detrimental to his

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pragmatic aims, namely an understanding of ethical convictions as fundamentally beyond

reason. Weber accepted the fact-value distinction mapped out by Hume and held that "an

empirical science cannot tell anyone what he should do - but rather what he can do - and

under certain circumstances what he wishes to do."[123] For this reason Weber considered

the social sciences to be "value neutral." Weber further maintained that "general views of

life and the universe," among which he includes ethics, "can never be the products of

increasing empirical knowledge." Strictly speaking, this position might only commit him

to rejecting the view that ethical convictions can be rationally indebted to empirical

realities, while perhaps preserving the possibility that non-empirical reasons could

underwrite ethics, such as Kant, for example, held.[124] Although Weber might have

limited himself to some narrow claim about the inability of empirical science to ground

value judgments, he took the additional step of concluding that values are fundamentally

a-rational, such that reason can never be brought to bear on them.

Weber outlined a long list of insights that the social sciences can offer with regard

to ethics: they can illustrate the effects of ethical convictions, highlight practical or

logical inconsistencies in the convictions themselves, even "judge" such convictions

critically in a dialectical manner - pointing out their historical effects and their success or

failures in obtaining the ends in question. However, Weber finally holds, "As to whether

the person expressing these value judgments should adhere to the ultimate standards is

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[123] Max Weber 'Objectivity' in social science" in " in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 109.

[124] Many would undoubtedly find this notion controversial in its own right.

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his personal affair; it involves will and conscience, not empirical knowledge."[125] Or, again, "to judge the validity of such values is a matter of faith."[126]

Weber's belief that reason could not bear on values was of one piece with the philosophical orientation of logical positivism, which took shape from the ideals of absolute science and began to develop around the same time, and clearly reminiscent of both Hume and Nietzsche. Facts are one thing, values another. Some have suggested that Weber's assertion of the value neutrality of social science was the one methodological claim of his that was widely endorsed by social scientists of the day, in no small part because it seemed to bolster the ideal of scientific objectivity (despite Weber's qualifications of such objectivity). Value neutrality is an attractive ideal, both because it seems to confirm that one's research is simply "about the facts" and to help fend ethical questions about one's work. In addition to the claim of value neutrality, many have seen Weber's position on the a-rationality of ethics as the dominant view in 20th century social thought more generally.

A strong critic of this view of ethics, Alasdair MacIntyre lucidly characterized Weber's stance in a passage worth quoting at length:

> [For Weber,] Questions of ends are questions of values, and on values reason is silent; conflict between rival values cannot be rationally settled. Instead one must simply choose- between parties, classes, nations, causes, ideals....'Values' says Raymond Aron in his exposition of Weber's view 'are created by human decisions...' and again he ascribes to Weber the view that 'each man's conscience is irrefutable' and that values rest on 'a

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[125] Max Weber 'Objectivity' in social science" in " in *Philosophies of Social Science: The Classic and Contemporary Readings.* edited by Delanty and Strydom. (Open University Press, 2003), 109.
[126] Ibid., 110.

choice whose justification is purely subjective.' It is not surprising that Weber's understanding of values was indebted chiefly to Nietzsche and that Donald G. Macrae in his book on Weber (1974) calls him an existentialist; for while he holds that an agent may be more or less rational in acting consistently with his values, the choice of one particular evaluative stance or commitment can be no more rational than any other. All faiths and all evaluations are equally non-rational; all are subjective directions given to sentiment and feeling.[127]

I agree with MacIntyre and others that Weber's account of ethics was typical of a dominant view of reason in late modernity that could not conceive of how reason could relate to questions of ethics. This view became increasingly entrenched as reason was understood to be synonymous and coextensive with science. In this respect, Weber remained influence by a particular ideal associated with the absolute conception, despite being so alert to the dangers of trying to fashion the social sciences according this absolute ideal.

Weber's position on ethics is striking in part because Weber was so exquisitely attuned to the influence that ethical convictions have on social life. His famous study of *The Protestant Ethic and the Spirit of Capitalism* investigated the powerful social effects the ethical ideals of Protestantism exerted on the economic development of modern Europe. Moreover, his seminal essay on *Politics as a Vocation* demonstrated a brilliant understanding of how ethical convictions move people to act in ways that have significant implications for politics. In that essay Weber suggests that the "ethic of

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[127] Alasdair MacIntyre, *After Virtue : A Study in Moral Theory* (Notre Dame, Ind.: University of Notre Dame Press, 2007) 26.

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ultimate ends" and the "ethic of responsibility" can both have effects that their adherents may find unpalatable. The ethic of ultimate ends, which would rather see the world perish than commit one injustice, will be a ruinous stance for a politician, who has to face the inherently tragic trade-offs of a complex world. The ethic of responsibility, which tries to manage violence for good effects, easily devolves into unprincipled opportunism that knows no boundaries. Weber almost seems to make an ethical argument about the limits of each stance; but in the end he says he's merely pointing out their effects and asserts "One cannot prescribe to anyone whether he should follow an ethic of absolute ends or an ethic of responsibility, or when the one and when the other."[128]

Given the powerful influence that ethical convictions have on social life, which Weber illustrates so convincingly, it would clearly be useful if we could shape ethical convictions. That is to say, ethical change could be a powerful component of pragmatic social science. However, if ethical convictions are, as Weber suggests, a matter of arbitrary choice, then it is unclear how they might be changed. Moreover, if ethical convictions cannot be shaped by reason, then attempts to influence people's convictions must, it seems, trade on force, deception, or emotional manipulation.

There is an alternative account of the relationship between reason and ethics that I will defend and map out in the penultimate chapter of this study, drawing on the work of

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[128] M. Weber, "Science as a Vocation." *Daedalus* 87, no. 1 (1958): 111-34.

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Alasdair MacIntyre, Charles Taylor, and Hans-Georg Gadamer. Although this account does not share the enlightenment hope of decisively grounding ethics in reason, it shows how ethical convictions are corrigible and open to evaluations that can claim a certain kind of rationality over time and under certain circumstances. This account holds that it is possible for ethical persuasion to be a rational enterprise, and thus not inherently the work of violence or manipulation. Moreover, this account suggests promising ways to pursue ethical persuasion, which in turn can help further many goals of pragmatic social science.

My more immediate task, however, is to show why the possibility of rational ethical persuasion is so important to pragmatic social science. If the goal of social science is to help achieve particular outcomes, rather than to construct an absolute science, then epistemological reasons for avoiding questions of ethics or projects aimed at ethical persuasion drop away. Moreover, as I have suggested and will later argue through a number of case studies, many social change projects supported by social science research have been handicapped by methodological commitments that render these projects blind to the influence of ethical convictions and possibilities for transforming such convictions. These methodological commitments are legacies of an absolute conception of social science and they need to be qualified in order to better serve the aims of pragmatic social science.

2.7 Pragmatic Social Science Ascendant

The middle of the 20th century witnessed an extraordinary rich set of discussions concerning the nature of the social sciences. As the absolute ideal showed signs of strain for reasons listed above, and, at the same time, the social sciences were tasked with increasingly large roles in government research and policy making, a wide range of social scientists and philosophers engaged in protracted debates about the scope and methods of social science. These exchanges helped dethrone the absolute conception and form a new consensus supporting pragmatic science.[129]

It would be misleading to say that the pragmatic stance became the dominant stance, although it is now undoubtedly mainstream. It is, I believe, the stance that most social scientists now invoke when pressed to give an epistemological justification of their enterprises. However, in practice, many social scientists apparently harbor the belief that their own research programs approach something like an absolute standpoint. So, the pragmatic conception of social science does not dominate in practice in the same way that the absolute conception once did. However, in summarizing the mature accounts of the pragmatic vision of social science I hope both to illustrate the widespread influence of this conception and to illuminate my own reasons for endorsing the pragmatic stance.

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[129] See, for example, the essays collected in May Brodbeck's *Readings in the Philosophy of the Social Sciences* (Macmillan Pub Co; 2nd printing edition, 1968) or many essays in the five volume series *Philosophy Politics and Society* (Yale University Press, 1979). Or for a more analytic treatment of the promise and perils of behavior see Taylor's *The Explanation of Behavior* (Charles Taylor, *The Explanation of Behaviour* Routledge, 1964).

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Perhaps the most classic and influential account of the methods of pragmatic, instrumental social science comes from Milton Friedman's 1953 paper "The Methodology of Positive Economics."[130] Described by Uskali Mäki and the editors of Cambridge Press as "The most cited, influential, and controversial piece of methodological writing in twentieth-century economics," and again as "the most important methodological statement in twentieth-century economics," and called "the most influential work on economic methodology of this century" by Dan Hausman,[131] it is an understatement to say the piece was widely read and endorsed.[132] In this paper Friedman sought to clarify what he saw as widespread misconceptions about the aims of economic research and the standards by which its models and methods should be evaluated.

Friedman shared Weber's believe that social science, particularly economics, can be value neutral and discover useful social regularities. Like Weber he also eschewed the search for ultimate causality and saw the purpose of social science as limited and instrumental. For Friedman, the proper aim of social science was to provide "sufficiently accurate predictions," and he describes economics as "a body of tentatively accepted generalizations about economic phenomena that can be used to predict the consequences

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[130] Milton Friedman "The Methodology of Positive Economics" In *Essays In Positive Economics* (Chicago: Univ. of Chicago Press, 1966), 3-16, 30-43. Originally published in 1953 AER.

[131] Hausman, D. *The Philosophy of Economics: An Anthology* (Cambridge: Cambridge University Press, 1985), 210.

[132] Uskali Mäki, *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* Erasmus Universiteit Rotterdam (Cambridge University Press, 2009) http://www.cambridge.org/uk/catalogue/catalogue.asp?isbn=9780521686860

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of changes in circumstances."[133] Thus Friedman argued that economics has to be evaluated by its ability to make predictions that are useful to us and "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience."[134]

Criticizing some fellow economists, Friedman insists that logical completeness and consistency are not a primary goal of social science, but simply an aid for keeping track of the details of theories and their implications. Moreover, he attacks those who "retreat into purely formal or tautological analysis" and maintains that useful social science has to be able to predict rather than retrospectively describe social phenomena. Otherwise, he cautions, economics tends to drift off into "disguised mathematics."[135]

Successfully predicting outcomes of interest is no easy task. The question is: how are we likely to achieve predictive success? One approach, which resonates with the absolute conception of social science, would be to search for a completely "realistic" account that takes into consideration all possible causal influences and yields a comprehensive analysis that could deal with any change in circumstance. Friedman parodies such an approach, writing:

> A completely 'realistic' theory of the wheat market would have to include not only the conditions directly underlying the supply and demand for wheat but also the kind of coins or credit instruments used to make exchanges; the personal characteristics of wheat-traders such as the color of each trader's hair and eyes, his antecedents and education, the number of members of his family, their characteristics, antecedents, and education, etc.; the kind of soil on which the wheat was grown, its physical and

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[133] Ibid., 39.

[134] Ibid., 8-9.

[135] Ibid., 12.

chemical characteristics, the weather prevailing during the growing season; the personal characteristics of the farmers growing the wheat and of the consumers who will ultimately use it; and so on indefinitely.[136]

The knowledge that would be required to achieve an absolute, objective grasp of this situation is beyond us. Indeed, Friedman remarks, "Any attempt to move very far in achieving this kind of 'realism' is certain to render a theory utterly useless."[137]

This is the sense in which Friedman maintains social science should not be committed to "realism." A complete grasp of reality - such as the absolute conception hoped to obtain - is not possible. Of course Friedman thinks "positive economics" does help us understand reality, but only narrow parts of it and for specific purposes. This pragmatic perspective, which remains agnostic about ultimate causality, profoundly shapes Friedman's methodological vision for the social sciences.

How do we formulate theories on Friedman's view? He notes that there are, strictly speaking, an infinite number of hypotheses that can be consistent with any particular set of empirical data. We have to make choices, picking out and focusing in on a few hypotheses for actual investigation. These choices reflect our purposes and our constraints. We will often pick out some theories because they are simple, tractable, and only require information we have available. Considerations of fruitful future extensions may also play a role. In any case, these theories will always involve gross oversimplifications of reality. However, since the purpose of these theories is not to describe reality in its fullness, but merely to help us achieve particular outcomes better

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[136] Ibid., 32.

[137] Ibid., 32.

than we might have otherwise done, simplified assumptions should not be judged by their verisimilitude. Rather, we need to evaluate assumptions, hypothesis, and models by the quality of predictions they supply for our particular projects. Thus Friedman asserts, "the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic,' for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. The two supposedly independent tests thus reduce to one test."[138]

Friedman's summary remarks in the essay affirm the most important features of his pragmatic vision of social science, namely that the goal is not realism or universality but rather analysis that is sufficiently insightful to expand our abilities to deal with the question at hand:

> It is frequently convenient to present such a hypothesis by stating that the phenomena it is desired to predict behave in the world of observation as if they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important. In general, there is more than one way to formulate such a description - more than one set of "assumptions" in terms of which the theory can be presented. The choice among such alternative assumptions is made on the grounds of the resulting economy, clarity, and precision in presenting the hypothesis; their capacity to bring indirect evidence to bear on the validity of the hypothesis by suggesting some of its implications that can be readily checked with observation or by bringing out its connection with other hypotheses dealing with related phenomena; and similar considerations. Such a theory cannot be tested by comparing its "assumptions" directly with "reality." Indeed, there is no meaningful way in which this can be done. Complete "realism" is clearly unattainable, and the question whether a theory is realistic "enough" can be settled only by seeing whether it

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[138] Ibid., 15.

yields predictions that are good enough for the purpose in hand or that are better than predictions from alternative theories.[139]

Note that this instrumental account of social science entails no *a priori* judgment about which particular methods are likely to be most useful for any given purpose. The focus on predictive success does imply that any analytic approach should issue some empirical implications or predictions. However, a wide range of statistical models, rational choice models, or biological models could plausibly contribute useful insights to a project. The proof of any approach's utility will be in the pudding, as it were. Also there are strategies -such as "out-of-sample testing" - that can provide additional reasons to believe the insights of a model will hold up to real world applications. Friedman's essay does suggest, however, why attempts to craft comprehensive, universal social scientific frameworks are not likely to be useful. The only truly accurate model of the world is the world itself. Social science will always be a limited and partial enterprise, but one that has demonstrated utility when properly focused.

At the end of his essay Friedman argues that "undue emphasis on the descriptive realism of 'assumptions' has contributed to neglect of the critical problem of determining the limits of validity of the various hypotheses that together constitute the existing economic theory... ."[140] What Friedman here identifies as a "critical problem" is indeed one of the most important tasks for social scientists once their methods are understood within a pragmatic framework. Since "method" itself does not contain an intrinsic

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[139] Ibid., 41-42.
[140] Ibid,. 43.

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guarantee of certain knowledge, as the absolute conception had hoped, social scientists cannot go around confidently applying their methods to everything. Methods do not invariably track the truth about the world, but are simply analytic abstractions that prove useful in certain circumstances.

Methods have to be evaluated based on the utility of their insights for the project at hand. Thus one of the most important tasks for social scientists is to understand the systematic limits of their methods for various practical purposes. Understanding the limits of different methodological approaches helps ensure methods are employed in the most useful ways, and guards against their overconfident and sometimes disastrous misapplication. According to the pragmatic account of social science it is imperative that we be driven by particular problems rather than a blind attachment to certain methodological approaches. This is a theme often encountered in contemporary social science debates, in which Ian Shapiro has been one of the more vocal critics calling for "problems driven" rather than "methods driven" research.[141]

Friedman won the Nobel Prize in Economics in 1976 (in large part for his work on monetary policy). His methodological statement outlining an instrumentalist, pragmatic account of social science was widely read and influenced many social scientists both within economics and beyond. For example, his account forms the basic epistemological framework for Scott Demarchi's recent and insightful book,

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[141] See his essay "What's Wrong with Political Science" in *The Flight from Reality* in the Social Sciences. (Princeton: Princeton University Press 2005) 181.

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Computational and Mathematical Modeling in the Social Sciences (Cambridge 2005).

Friedman's vision has also influence the National Science Foundation's Empirical

Implications of Theoretical Models (EITM) Program in the social sciences.

Unsurprisingly, some of the details of Friedman's account have attracted criticism.

Daniel Hausman claimed that Friedman was unduly dismissive of the need for "micro-

foundations" in social science, although Demarchi persuasively argues that Hausman

both misinterprets Friedman and under-appreciates the power of the pragmatic

account.[142] Some have also taken Friedman's claim that useful models generally require

unrealistic assumptions as something that commits him to a radically anti-realist stance.

However, Kevin Hoover has shown that Friedman is not a radical in that sense and his

work was open to considerations of micro-foundations and arguably realist in a more

narrow sense.[143]

It is notable that the one of the most recent Nobel Prizes in Economics to be

awarded specifically for methodological innovations went to Clive Granger, whose 1999

book, *Empirical Modeling in Economics*, recapitulates a pragmatic account of social

science.[144] Surveying the field, Granger cautions against two extremes: over extended

models that try to fit all of reality into a particular specification and retrospective models

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[142] Daniel M. Hausman *The Philosophy of Economics: An Anthology* Second edition (Cambridge: Cambridge University Press, 1984) 17-21. (See also Scott De Marchi, *Computational and Mathematical Modeling in the Social Sciences* (Cambridge: Cambridge University Press, 2005), 18-24.

[143] See Kevin Hoover "Milton Friedman's Stance: the methodology of causal realism." UC Davis Working Paper http://www.econ.ucdavis.edu/working_papers/06-6.pdf

[144] Clive Granger, *Empirical Modeling in Economics: Specification and Evaluation* (Cambridge; New York: Cambridge University Press), 1999.

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that consist only of a-theoretical data mining. Both overestimate our ability to accurately capture incredibly complex processes in the real world. Granger shares Friedman's view that models should be built and evaluated with specific pragmatic purposes in mind. "It is rare," Granger notes, "for one model to be superior for all possible purposes: forecasting, policy making, conditional forecasts, testing hypotheses, or investigating the effects of a previous policy change... ."[145] Social science models cannot hope to understand everything, and Granger suggests economists too often overreach towards this goal or become obsessed with elegantly satisfying the internal standards of a particular methodological framework.

Granger, himself a pioneer of econometric methods, writes "I would argue that most economic research should not want to be considered to be like pure mathematics but should be associated with a clear-cut and precisely stated objective."[146] He reports he is "amazed that some methodologists appear to be content with economics providing an 'explanation' for what has occurred in the past," because in his view, "generally, there is not a unique explanation for what has occurred in the past."[147] Social science may generally provide insights into the past events, but fully explaining them is not the proper objective. Rather, Granger concludes "a theory or model should be evaluated in terms of the quality of the decisions that are made based on the theory or model."[148] Like

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[145] Ibid., 18.

[146] Ibid., 40.

[147] Ibid., 41.

[148] Ibid., 57.

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Friedman, Granger argues that the social sciences have a limited and instrumental purpose, and need to be evaluated based on their ability to help us address particular problems. He maintains the methods of social science do not grant unqualified access to reality and cautions that researchers can become distracted from the pragmatic promise of social science by being too enamored with methodological sophistication and standards of evaluation internal to particular methodological approaches.

The fact that Granger wrote the book in the tone he did suggests that many social scientists do not entirely or consistently embrace the pragmatic account as Granger envisions it. This is likely true. However, we should also note that there are a range of particular methodological commitments than can be found underneath the umbrella of pragmatic social science. Social scientists committed to this broad account may nonetheless find much to argue about with regard to details of their studies and approaches.

Granger and Friedman were both mainstream economists, whose work had significant, real world impact - Friedman with regard to monetary and exchange rate policy and Granger with regard to financial econometrics. Others have sympathetically explored deep epistemological questions raised by the pragmatic account more as critics of social science than practitioners, and I think here of Nancy Cartwright - *Hunting Causes and Using Them: Approaches in Philosophy and Economics* (Cambridge 2007)[149] - and Deirdre McCloskey - *The Cult of Statistical Significance: How the Standard Error*

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[149] Nancy Cartwright. *Hunting Causes and Using Them: Approaches in Philosophy and Economics* (Cambridge, 2007).

Cost us Jobs, Justice, and Lives (with Stephen Ziliak, Michigan 2008)[150]. The range of particular positions staked out by those who embrace the pragmatic vision is thus large. What unites these perspectives is a basic rejection of the absolute conception of social science. However, among those who explicitly promote the pragmatic conception of social science there is a recurrent genre of writing that aims to remind social scientists of how to be useful by not falling into the trap of method induced blindness.

Methods driven research is, in large part, a legacy of the absolute conception, but it can also be simply a matter of habit, easy publications, and a way of avoiding creative work. Many social scientists would undoubtedly affirm that they believe their research can help make the world a better place, however small they may admit that contribution is likely to be. Reinforcing this idea, grant applications from the National Science Foundation require researchers to justify their projects with a statement about the broader social impact of their research and its promise to improve society. However, the pragmatic account of social science involves more than a desire to do useful research - it involves recognizing the limited, instrumental value of methods and evaluating research by its pragmatic insights rather than methodological fidelity. These are the issues that mainstream social scientists continue to wrestle with in practice, even though lip service is universally paid to the pragmatic rationale for social science research.

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[150] Deirdre McCloskey and Stephen Ziliak. *The Cult of Statistical Significance: How the Standard Error Cost us Jobs, Justice, and Lives* (Michigan, 2008).

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2.8 Implications of the Pragmatic Conception for Social Science and Ethics

2.8.1 Implications for Social Scientists

This historical sketch helps clarify and focus my main claims. As I suggested in the first chapter, ethical convictions profoundly influence the way people act by providing basic sources of motivation. Thus, ethical persuasion – the ability to shape peoples' ethical convictions – should be a powerful resource for social change. However, I have also suggested, and will further demonstrate in the next three chapters, the dominant methodological tools of social science are not well equipped to recognize the contributions that ethical convictions make to social structure or to provide resources for engaging in ethical persuasion. To some extent this is simply a feature of scientific analysis, which aims to describe the current and past arrangement of things in objective, observer-independent terms. The account of pragmatic science shows why this can often be a useful enterprise, even if it neglects deeper realities at play. However, the pragmatic account of social science also rejects the idea that we have to be wedded only to "positive" methodological analysis. Rather, if success is measured by our ability to achieve desired outcomes, then methods have to be judged by the quality of their insights, and those insights will often need to be augmented by "non-scientific" resources – including resources that can help us diagnose existing ethical impediments and support attempts at ethical persuasion.

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Unfortunately many policy makers and program officers who rely on social science research to guide their projects (as well as many social scientists themselves) are narrowly wedded to considering only what positive methods reveal. In many cases this has handicapped the otherwise well meaning and laudable efforts of reformers. This is also a claim I will further substantiate in the next three chapters with concrete examples.

What is the reason that "methods induced blindness" persists? Moreover, if ethical persuasion is valuable, why has it been so neglected? There are, as I have suggested, two main reasons for this state of affairs. The first is that the absolute conception of social science has exerted a powerful influence on the way people understand society and possibilities for change. I have tried to illustrate the origins, nature, and legacy of this conception in the foregoing historical survey. A second reason for neglecting ethics springs, ironically, from a kind of ethical concern for personal choice, which thinks it is illegitimate to try to shape people's ethical convictions (or as the cliché has it - "impose one's values" on others), whether through persuasion or any other means. I will explore this objection in a later chapter.

My general answers, then, to the initial questions – Why haven't we been more successful in addressing serious social ills? How should we evaluate our methods? Why has ethics been neglected? – should now be clear. Although efforts to address social ills are often enhanced by the methodological tools of social science, these methods can also prove detrimental. If one focuses only on what methods reveal, important dimensions of social problems may go unrecognized, particularly with respect to ethics.

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I have tried to show how the primacy of method, which grew out of an absolute conception of science, went hand in hand with the exclusion of ethics from social theory and philosophy. This resulted in what Eric Voegelin and others have described as a "new science of politics" within modernity, one which held "the study of reality could qualify as scientific only if it used the methods of the natural sciences, that problems couched in other terms were illusory problems - questions that did not admit of answers by the methods of science should not be asked… indeed could not be asked with any important meaning."[151] However, I have also shown how support for the absolute conception broke down and have outlined the terms of an alternative, pragmatic account of the social sciences – one which I endorse along with many distinguished social scientists of recent years. The epistemological mistakes of absolute science were implicitly recognized by the turn to pragmatic science, although the full implications of the pragmatic stance have not yet been realized. Some social scientists still believe in the infallibility of their methods, and even amongst those who embrace the pragmatic account, many, such as Weber and Friedman, maintain an artificial divide between "positive" and "normative" theory.

A central challenge for those who accept the pragmatic account of social science is to understand the systematic limits of different methodological approaches. Rather than presume that "scientific methods" necessarily provide access to any social reality to which they are applied, those working within the pragmatic conception accept that

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[151] Eric Voegelin. *The New Science of Politics: An Introduction* (Chicago, 1987) 4.

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methods are partial, limited, and instrumental. Not only does the pragmatic stance recommend problems driven research over methods drive research, but it also draws attention to the dangers and distortions of methods driven research. Understanding the nature of the limits inherent in different methodological approaches is thus an important project, one that promises to enhance the possibilities of pragmatic social science at large.[152]

Within the social sciences the limits of "scientific methods" are often directly related to the capacity humans have to acquire and change their ethical convictions. This capacity reflects much of what makes humans exceptional – our ability to reason about ends as well as means, our culturally situated starting points, and our consciousness of history. So, in the process of examining the limits of our methods we will also come to better understand the unique contribution that ethical persuasion stands to make to our understanding of society and our capacities to shape it. We will better understand that accomplishing many of the social outcomes we desire[153] will, as a "positive" matter of fact, depend on our ability to persuade people to adopt ethical convictions that support such outcomes.

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[152] Alex Rosenberg argues for radically different kind of conclusion to these debates. With regard to economics, he suggests economists is more or less incapable of becoming a useful empirical science as is perhaps better off abandoning pragmatic aims all together and retreating into formal theoretical analysis – like Euclidian geometry, but with fewer real world applications. I discuss some of these claims from his *Economics – Mathematical Politics or Science of Diminishing Returns?* (Chicago, 1992) in my latter chapter concerning rational choice formal modeling.

[153] And we can, of course, debate whether they are in fact desirable.

With this history in view, we can see that the pragmatic account of social science has important implications both for hermeneutic critics of social science and for practicing social science researchers.

2.8.2 Implications for Hermeneutic Critics

The implications for hermeneutic critics are twofold:

1)   First, the standard hermeneutic critique of social science, which argues that the social sciences can never achieve an absolute purchase on human life because of the contingent, intentional basis of human agency, has largely been exhausted. Indeed this critique has been exhausted by its very success in aiding the transition towards pragmatic social science. It is increasingly recognized that the social sciences cannot achieve an absolute perspective, in large part because of the intentional, conceptual, and historical foundations of human life. With the adoption of the pragmatic perspective, however, this basic insight of hermeneutics loses most of its critical punch. Much of the intellectual artillery traditionally developed by hermeneutic critics was calibrated for a target – absolute science – that is no longer the dominant view. Unfortunately, this development has been missed by some younger hermeneutic scholars who continue to attack the very idea of social science as if absolute science were the only possible model. These scholars appear oblivious to the pragmatic alternative – an alternative which suggests why analytic investigations of society can be useful even though they can never achieve an absolute perspective.

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Consider, as an example of this outdated critique, a recent article by Mark Bevir and Asaf Kedar in the September 2008 issue of *Perspectives on Politics* entitled "Concept Formation in Political Science: An Anti-Naturalist Critique of Qualitative Methodology." In this article, the authors lament that even qualitative researchers in Political Science are implicitly wedded to "naturalist assumptions." "Naturalism" according to the authors, "arises from the belief that similarities between the natural and social worlds are such that they should be studied in the same ways…Hence, we can define naturalism as the idea that the human sciences should strive to develop predictive and causal explanations akin to those found in the natural sciences."[154] Bevir and Kedar claim that naturalism leads social scientists to explain everything in terms of determinate, general laws. The authors conclude that the assumptions of naturalism "are inappropriate to the human sciences given the meaningful and contingent character of human action and the situations of the social scientist," and go on to assert that *any* analytical approach that does not reconstruct contingent beliefs and meanings is inappropriate for political analysis.

Like Winch decades earlier, Bevir and Kedar see only two possibilities for social inquiry – either it takes the character of absolute science, trying to reduce all social life to objective, determinate laws modeled on the natural sciences, or it must proceed entirely as a hermeneutic enterprise, which tries to reconstruct the specific and contingent meanings that motivate particular actions on a case by case basis. The argument of their

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[154] Mark Bevir and Asaf Kedar ,"Concept Formation in Political Science: An Anti-Naturalist Critique of Qualitative Methodology" in *Perspectives on Politics* Vol.6 Issue 3 (2008) 504.

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article takes the form of an impossibility proof: Since analytic approaches pioneered in the natural sciences clearly cannot account for all of human life, these approaches are "philosophically inappropriate" tools for social inquiry. Thus the alternative, an exclusive focus on interpretive reconstruction, must be correct.

In a single sentence in the opening paragraph of the article, the authors brush aside the idea that methods might be judged on pragmatic terms, asserting that, philosophically speaking, interpretive and scientific perspectives are ultimately inconsistent with one another. With this assertion, the authors ironically appear beholden to the high epistemic ideals that animate the absolute conception of science. They completely neglect any discussion of the utility of analytic approaches for addressing social problems and retreat entirely to a philosophical preoccupation with the purity of method – only, in their case, they want to disqualify all non-interpretive approaches as philosophically inappropriate.

The pragmatic conception of social science, rightly understood, rejects the false dichotomy set up by Bevir and Kedar and other hermeneutic critics. The question of philosophical appropriateness of different methodological approaches often cannot be settled without reference to the particular goals any inquiry is intended to serve. In light of the ascendance of the pragmatic conception, rather than attack the idea of absolute social science, those with hermeneutic skills should focus on how they can contribute to attempts to address social problems. Often they will find that they do have distinctive contributions to make, which augment analytic methods or illustrate the limits of such

method for particular problems. However, an entirely critical stance that rejects the possibility of social science is neither helpful nor persuasive.

2) A second implication of the pragmatic conception is that those with hermeneutic skills ought to be open to using these skills for more than mere description. Many classical proponents of hermeneutics showed why interpretive inquiry is essential for a fuller *understanding* of social phenomena. The project of developing deeper understanding of this sort is undoubtedly a valuable scholarly enterprise in and of itself. But the aim of pragmatic science is not *understanding* as such. Rather the purpose is explicitly dynamic – either to produce social change or to guard against it. Granted, a sufficient amount of understanding is a necessary prerequisite for effectively addressing social problems, and hermeneutic enterprises (the writing of history, psychoanalysis, ethnography, political theory etc.) will clearly provide useful background knowledge for pragmatic projects. However, many skilled hermeneuticists are reluctant to "intervene" in the social phenomenon that they describe and understand so well. They see themselves as observers, not participants, in the cultural life of those they study. However, the richness of their interpretations likely grants hermeneuticists insights into how a culture might be vulnerable to certain challenges or open to certain changes. If interpretivists want to contribute to the project of pragmatic science they should be more open to using their skills in service of persuasion rather than mere description. About this, at least, Marx's

was right – "the philosophers have only interpreted the world, in various ways: the point, however, is to change it." [155]

There is, of course, considerable controversy within disciplines such as cultural anthropology regarding whether and how it is legitimate for an "outsider" aim to change the "native" culture, with the majority of the discipline generally opposed to this idea.[156] Much of this opposition stems from a stance of cultural relativism which holds it is illegitimate to change a culture because no one culture is in principle better than another. Related to this is the view that there is an inherent value to the diversity of cultures. Granted, the legacy of imperialism is grim enough to caution against confident judgments of cultural superiority; and there is arguably an epistemic value to the existence of alien cultures, which unsettle assumptions about the naturalness of one's own way of life. However, debates surrounding these issues tend to advance quickly to extremes. One side can identify practices that appear manifestly intolerable and worthy of extinction. The other can point to historical examples of misplaced moral confidence and disastrous cultural hubris. I will examine the deeper questions raised by these concerns and a possible way through them in a later chapter. However, for now I want simply to note that if anyone is qualified to help us negotiate the complex issues at play and perhaps inform persuasive dialogue with rival or alien cultures it will be anthropologists,

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[155] Karl Marx. "Theses on Feuerbach" in *The German Ideology, including Theses on Feuerbach*. (Prometheus: 1998) 11th thesis, page 574. Here Marx suggests a kind of agency that recurrently surfaced in his work despite its contradiction to his other theses.

[156] See, for example, the writings of Richard Schweder: *Culture Theory*; *Thinking Through Cultures*; *Metatheory in Social Science*; and *Why Do Men Barbecue?*

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historians, political theorists and others who have developed interpretive skills and are familiar with the variety of ways in which people might live.

Clifford Geertz, one of the most influential exponents of "the interpretative approach," describes anthropology in terms that suggest its cultural insights cannot be used as forces for change: "the aim of anthropology is the enlargement of the universe of human discourse... Culture is not a power, something to which social events and behaviors institutions or processes can be causally attributed; it is a context, something with in which they can be intelligently -- that is, thickly -- described."[157] Anthropology is a descriptive enterprise, and culture, on this view, is a medium too thick to analyze in terms of its effects. Despite this stance, so characteristic of the discipline as a whole, Geertz recounts in one revealing essay, "Culture and Social Change: The Indonesian Case," his own ambiguous relationship to the goal of cultural change.

Geertz recalls that, as the economic development of poor countries emerged as an explicit goal of Western organizations in the 1950's, there was considerable interest in the influence of culture. Many believed traditional cultures were an impediment to "modernization" and thus had to be transformed; others looked to leverage distinctive cultural traits – the Islamic family structure, cooperative work practices, etc – in ways that could contribute to economic growth. Economic "experts" pursued a wide range of *ad hoc* collaborations with various sorts of anthropologists. Although Geertz reports that a genuine meeting of minds never took place, he also suggests there was something

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[157] Clifford Geertz. *The Interpretation of Cultures*. (Basic Books: 1977) 14.

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laudable about the attempts of the early generation of development economists to at least consider culture.

It is clear from his essay that Geertz remained extremely suspicious of these economists and their naïve attempts to use culture as a tool for development. Moreover, he was uneasy with the role in which he and other anthropologists were cast in being asked to contribute insights that supported an agenda of modernization and economic growth. Geertz describes the anthropologists of the time as uncomfortable with the "growth ethos," which many viewed as "ethnocentric at best, imperialist at worst." However, despite all of these anxieties and reservations Geertz reports that many anthropologists were "brought to see the necessity of change by their encounter with mass poverty," and so they wanted to contribute on some level to the project of alleviating the immiseration of those they studied.[158] Geertz's first book *Agricultural involution: the processes of ecological change in Indonesia* was in part a contribution to these debates, although Geertz maintains it rejected both the culture-as-obstacle and culture-as-stimulus views.

Looking back on this period of *ad hoc* interdisciplinary exchange in which different parties seldom saw eye to eye, Geertz tacitly admits that these efforts were commendable. At least the economists of this period saw that culture was an important consideration in light of their objectives. They grasped for answers, often arriving at partial or implausible ones, but they perceived the reality of a fundamental issue that

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[158] Clifford Geertz "Culture and Social Change: The Indonesian Case" in *Man*, New Series, Volume 19, No. 4 (December 1984), 512.

could not be avoided. Geertz also suggests that anthropologists were, despite all misgivings and misunderstandings, right to want to contribute to the project of alleviating systematic sources of abject poverty. However, for various reasons a more productive collaboration was never realized. By Geertz's telling, disciplinary disagreements between anthropologists and economists drove them further apart, and development economics came to be dominated by scientifically tractable, reductive economic theories from which cultural considerations were expunged. The result was a highly unsatisfactory parting of ways. In the decades that followed, economic development was approached through reductive theories – what Geertz calls "omega point models" (sharing what I have described at the aspirations of absolute science). Geertz offers an insightful critique of the framework of "economism," suggesting that its reduction and exclusion of culture in fact limited its ability to understand and direct social change. Geertz's analysis is worth quoting at length:

> It is not economic analysis itself that is the problem, any more than it is quantification. It is economism: the notion (to which, in fact, anthropologists, at least in Indonesia, seem rather more susceptible these days than do economists) that a determinate picture of social change can be obtained in the absence of an understanding of the passions and imaginings that provoke and inform it. Such understanding is inevitably limited. (Who knows what the javanese are really like?) And the determinateness it brings is inevitably partial. (Who can assess what a permutational sense of time means for capital formation?) But without it there is nothing but polemic, schematicism and endless measurements of amorphous magnitudes: history without temper, sociology without tone.

> If the debates that have arisen around 'the involution thesis' are ever to be properly adjudicated and, at least, some reasonable determination made as to whether the present crisis in the Indonesian rural economy is one of incremental immiseration (as the returns from agriculture are distributed ever more thinly across the swelling rural population) or whether it is one

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of a classic, have and have-not confrontation (monopolisation of the means of production, dispossession of the working class), we shall have to know a great deal more about the concrete particulars of social life than we are likely to get from global categories, divergent data and, if I may say so, the processed sentiments of evangelical social theories. Nor is it only the particulars of peasant life, in the narrow sense, that need to be uncovered, but those of commerce and artisanry, of state-society relationships, of religious differentiation and aesthetic transformation, and much else as well.

This is not a counsel of perfection. It is not necessary to know everything to know anything. Nor is it a counsel of despair. There are other forms of dynamism than those Marxists and Liberals have already thought of, as well as other forms of disaster. It is merely a plea for us to begin again to look for answers to our questions where the answers might conceivably be. The shamelessly ad hoc grappling with the whole grand conglomeration of social practices, the willingness to take factual or analytical instruction from whatever direction it might come, and above all the determination to situate processes of change within local ways of going at life that marked the first phases of 'developmental theorizing' in Indonesia may have lacked a certain rigour and certainly lacked a sufficient precision. But, at least, they did not confine us to searching for lost coins only where the light was, and they did not imagine that it was advantage that made the world go round.

The case is particular, but the point is general. Whatever one may think of omega point models of social change, in which everyone ends up a class warrior or a utility maximiser (and I, obviously, think very little of them), there is no chance of analysing change effectively if one pushes aside as so much incidental music what it is that in fact is changing: the moral substance of a sort of existence. The Renaissance, the Reformation, the Enlightenment and the Romantic Reaction made the modern world as much as trade, science, bureaucracy and the Industrial Revolution; and, indeed, vast changes of social mind, they made it together. Whatever happens in Asia, Africa, and Latin America-Rough Beasts or New Forms of Architecture-it will, you can count on it, involve comparable passages, comparably vast.[159]

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[159] Ibid., 524.

I am highly sympathetic to this general diagnosis. However, when we survey the field of development economics today, we find that many of its brightest minds are increasingly taking culture seriously, and I think here of the work of Wallis, North, and Weingast summarized in *Violence and Social Orders*, Paul Romer's recent articles on the role of ideas, institutions, and human capital in growth theory, and Elinor Ostrom's work on the way in which informal cultural norms help manage common resource pools, for which she was recently awarded the Nobel Prize in Economics. It is the anthropologists who are reluctant to move beyond their inherited disciplinary boundaries. Many continue to see their work as entirely descriptive, and demur at the idea of actively working to change a culture in service of ideals of "development." This is unfortunate, if, as I maintain, many social outcomes we hope to achieve do necessarily depend on the transformation of ethical features of particular cultures. Of course, anthropologists may alert us to the unnoticed social costs of such transformations, and we can argue about whether outcomes like less starvation and less disease are worth those costs. However, having recognized the importance of ethical change for making certain outcomes more attainable, the utility of anthropological knowledge – and indeed "interpretive skill" more generally – becomes clear. The point of pragmatic social science is to change the world rather than describe it, and those with hermeneutic skills are best equipped to help social scientists engage in the ethical persuasion needed to support such change.

In a recent book the distinguished development economists Vernon Ruttan remarked "My greatest disappointment with anthropology – 'the science of man' – has

been its failure to make the knowledge that it has acquired accessible for the development of the societies that have been the object of attention. The failure of anthropologists to engage more directly in issues of development has been a continuing puzzle (Redfield and Warner 1940, Rhoades 1984, Hackenberg and Hackenberg 1999)."[160] The puzzle is no doubt complex, and the situation has been aggravated by parties on both sides of the traditional hermeneutic divide between disciplines. However, the project of pragmatic social science, which rejects the reductive aspirations of the absolute conception, is capable of appreciating the importance of hermeneutic skills. If only those who possess such skills were more willing to employ them in service of dialogue and persuasion, rather than mere description. Ruttan concludes his chapter on cultural endowments and economic development with the claim, "What traditional cultures, peasant societies, and ethnic enclaves need from anthropology is the knowledge that will enable them to engage the broader national and international worlds in which they exist. This is also what would be most useful to development economists and to national and international development assistance agencies."[161]

There are of course many worthwhile scholarly enterprises that are never meant to be contributions to pragmatic social science. I do not mean to denigrate these or imply that the only intellectual endeavors worth pursuing are those that have pragmatic

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[160] Vernon Ruttan, *Social Science Knowledge and Economic Development: An Institutional Design Perspective* (Ann Arbor: University of Michigan Press, 2003), 66.

[161] Ibid, 67.

implications. However, the pragmatic conception of social science does shed new light on the utility of hermeneutic skills. Success in achieving many desired social outcomes will depend on deploying hermeneutic skills in service not only of understanding, but also dialogue, debate, and persuasion.

These then are the two major implications of the pragmatic account of social science for those associated with hermeneutics: the need to move beyond traditional critiques of the possibility of social science and recognition of the unique contribution that hermeneutic skills can make to pragmatic projects through not only understanding but active persuasion. As for mainstream socials scientists, the immediate lesson of the pragmatic account is that researchers need to understand the useful limits of different methodological approaches.

2.9 Conclusion: Moving Beyond Absolute Science

As I have suggested, contemporary social scientists often act as if they are of two minds with regard to the pragmatic rationale for their research. Although many ostensibly endorse the pragmatic stance in principle, in practice they often pursue methods driven research and display a remarkable overconfidence in the universality of their methods. In light of the pragmatic conception of social science, researchers need to scrutinize the continuing influence that the ideal of absolute science exerts on their work and abandon methodological frameworks when they prove an impediment to problem solving. Again,

this is simply to echo Ian Shapiro's call for more "problems-driven" rather than "methods-driven" research.[162]

Upon scrutiny, it is clear that the dominant methodological approaches within the social sciences do have systematic limits. These limits can be well characterized and understood, and understanding these limits in turn suggests more useful tactics for research. Moreover, these limits often relate to the capacity people have to acquire and revise ethical convictions. Thus, understanding the limits of different methods also alerts us to the unique importance of ethical persuasion and the ways in which we need to augment our technological knowledge with such persuasion if we hope to achieve certain social outcomes.

Despite the ascendance of the pragmatic perspective, many prominent contemporary methodologists continue to present their own methodological approaches as if these approaches could achieve a universal/absolute perspective. These researchers admit of no limits to their methods in principle and think all that is needed for social progress is additional research extending these methods and more rigor in applying them. This methodological ideal of the absolute conception is still alive and well in certain wings of contemporary social science. Numerous researchers still aspire to make social science an absolute science through methodological frameworks that are supposed to provide an Archimedean point for universal social science.

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[162] Shapiro, Ian. *The Flight from Reality in the Human Sciences* (Princeton, N.J.: Princeton University Press, 2005) 180.

This aspiration is alive, for example, in the universal logic of statistical scientific inference advanced by King, Keohane, and Verba in their book *Designing Social Inquiry*, which serves as the introductory research text for most graduate students in Political Science:

> …the best scientific way to organize facts is as observable implications of some theory or hypothesis. Scientific simplification involves the productive choice of a theory (or hypothesis) to evaluate; the theory then guides us to the selection of those facts that are implications of theory."…"In principle and in practice, the same problems of inference exist in quantitative and qualitative research. Research designed to help us understand social reality can only succeed if it follows the logic of scientific inference. This dictum applies to qualitative, quantitative, large-n, small-n, experimental, observational, historical, ethnographic, participant observation, and all other social scientific research.[163]

It is alive in Gary Becker's account of the economic approach to human behavior outlined by rational choice theory, which Becker believes can provide a universal framework for the social sciences:

> The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach…I am saying that the economic approach provides a valuable unified framework for understanding *all* human behavior…The heart of my argument is that human behavior is not compartmentalized, sometimes based on maximizing, sometimes not, sometimes motivated by stable preferences, sometimes by volatile ones, sometimes resulting in an optimal accumulation of information, sometimes not. Rather, all human behavior can be viewed as involving participants who maximize their utility from a stable set of preferences and accumulate an optimal amount of information and other inputs in a variety of markets. If this argument is correct, the economic approach

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[163] King, Keohane, and Verba, *Designing Social Inquiry* (Princeton NJ: Princeton University Press, 1994) 46, 229.

provides a unified framework for understanding human behavior that has long been sought by and eluded Bentham, Comte, Marx, and others.[164]

It is alive in the biological speculations about genetic determinism that are thought to explain ethical convictions as hardwired and to shore up strategic models with evolutionary foundations, as advocated by Ken Binmore:

> The moral rules that really govern our behavior consist of a mix of instincts, customs, and conventions that are simultaneously more mundane and more complex than traditional scholarship is willing to credit. They are shaped largely by evolutionary forces – social as well as biological. If one wishes to study such rules, it doesn't help to ask how they advance the Good or preserve the Right. One must ask instead how they evolved and why they survive. That is to say, we need to treat morality as a science.[165]

> [W]hat we count as fair depends on both our culture and on our genes. Since cultures vary, any universal principles of justice – its deep structure – must presumably be written into the genes that we all share as members of the same species. If I am right in guessing at the existence of such a deep structure, the next question asks itself. What shape does the deep structure of fairness take? …The thesis that I defend in this book is that the common deep structure of human fairness norms is captured in a stylized form by an idea that John Rawls called the device of the original position in his celebrated Theory of Justice.[166]

Of course, statistical techniques, rational choice models, and biological-behavioral research are all clearly useful for addressing wide range of social questions. The problem is with those who would like to make these approaches absolute – those who think these methods provide the only reliable or "scientific" access to social reality.

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[164] Gary Becker. *The Economic Approach to Human Behavior* (Chicago: 1976) 14.

[165] Binmore, K. G. *Natural Justice* (Oxford University Press, 2005) 1.

[166] Ibid., 15.

In the following three chapters I take up a crucial task for pragmatic social science, examining the nature and limits of our dominant methodological approaches: statistical methods, formal models, and biological-behavioral research, respectively. My aim is to provide a detailed evaluation of these methods (a "critique" in Kantian sense) that illustrates both their utility for a certain range of problems and conditions, as well as their systematic limits.

There is also an interesting story to be told about how the recognition of the intrinsic limits of some methods has fueled innovations in other methods. Thus problems of strategic behavior that are not tractable with backwards looking statistical models provided an impetus for the development of rational choice models; and behavioral deviations from rational choice predictions provided an impetus for exploring biological foundations of human (ir)rationality.

To many methodologists the fact that methodological approaches are limited will be old, indeed obvious, news. However, in addition to mapping out these limits and showing how some methodological approaches try to address shortcomings of other approaches, I will focus on how the limits of each of these approaches are distinctively related to ethics. Amongst those who still believe in the possibility of absolute social science, there is a strong drive both to deny the limits of scientific methods and also to reduce and thus explain away ethics. My defense of the conceptual natural of ethical convictions and my identification of the limits of different methods thus go hand in hand.

Finally, in keeping with the pragmatic premises of my argument I use examples and case studies in the following three chapters that illustrate how an exclusive reliance

on scientific methods has often led policy makers and program officers astray. The examples and case studies I use largely concern the political-economic development of the third world. I focus on issues of third world development because it is with regard to these that the implications of my arguments are most conspicuous and because the stakes are so high. By examining the practical failures of projects supported by some of our best social science research methods, I show how a strict focus on what scientific methods reveal has proved an impediment rather than an aid to the pragmatic goals of much research.

My basic points – 1) that our scientific methods have limits 2) that these limits often relate to the capacity of people to acquire and revise ethical convictions, 3) that relying exclusively on these methods often hinders our ability to achieve the very outcomes social science research is meant to support, and 4) arriving at more desirable outcomes will often depend on augmenting insights provided by scientific methods with attempts at ethical persuasion that cannot be scientifically mastered – are all points that I believe apply across the board with regard to contemporary social thought. Prominent failures in efforts to develop of the third world provide convenient terms in which to substantiate my pragmatic claims; and prominent successes, which I examine in the final chapter, also help illustrate how ethical persuasion can inform social change projects and help us attain desired outcomes.

The search for certainty through scientific analysis to support social change projects is understandable. However, as Aristotle remarked of politics we should be wary of trying to extract more precision than the subject matter allows. Not only have

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"scientific" studies have often yielded mistaken advice, but they have also led decision makers to be overconfident in what they think they know. The complexity of human society and reality of human agency should caution us towards humility and prudence in trying to change society for the better. The difficult questions we face do admit of better and worse answers, but are seldom matters for which we can possess the certainty idealized by the absolute conception.

The pragmatic account of science is in principle open to considering the limits of analytic methods rather than taking them as infallible scientific starting points – and indeed a better understanding of those limits provides a basis for more useful research. Pragmatic social science thus shares in John Maynard Keynes' simple judgment that "I would rather be vaguely right, than precisely wrong."[167] Ultimately, being vaguely right is in fact more useful than being precisely wrong. In our present circumstance, getting things right requires moving beyond the methodological absolutism and exclusion of ethics characteristic of absolute social science.

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[167] Bradford and Harcourt. "Units and Definitions" in Keynes, Harcourt, and Riach, *A "second edition" of The General Theory, Vol.1* (Routledge: 1997) 108.

3. Statistics

"For the Enlightenment, anything which cannot be resolved into numbers, and ultimately into identity, is illusion; modern positivism consigns it to poetry. Unity remains the watchword from Parmenides to Russell.

- Horkheimer and Adorno, *Dialectic of Enlightenment*[1]

"Upon those that step into the same rivers different and different waters flow...They scatter and…gather…come together and flow away…approach and depart"
[you never step into the same river twice]

- Heraclitus of Ephesus[2]

3.1 Introduction to Statistics and Its Problems

At the most basic level, statistics is the study of patterns. More generally, statistics draws on probability theory to make generalizations and inferences from patterns of data. It is hard to overstate the importance of statistical analysis for modern civilization. Entire branches of physics and chemistry would be unthinkable without statistical concepts (fluid dynamics, statistical mechanics, nuclear/quantum physics, etc.). The logic of statistical inference informs nearly all our biomedical discoveries (e.g. double blind, randomized, placebo controlled trials), and the "scientific method" itself is now often thought of in statistical terms.

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[1] Adorno, T. W. and M. Horkheimer. *Dialectic of Enlightenment* (Verso Books, 1997) 4-5.

[2] Kirk, G. S. *The Presocratic Philosophers : A Critical History with a Selection of Texts*. Edited by Malcolm Schofield (Cambridge [Cambridgeshire]; New York: Cambridge University Press, 1983).

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The patterns that form an object of study for statistics can have various sources. They may be the result of determinate causal processes in nature, like the statistics of gas laws and thermodynamics, or they may be artifacts of contingent historical events or human practices, like evolutionary statistics or baseball. Our use of statistics is generally divided in to two categories, namely descriptive statistics and inferential statistics. Descriptive statistics involves summarizing features of data in ways that provide a useful overview of the data's properties. We may want to know what the average temperature has been in Durham, North Carolina for January over the last ten years, or the variability of rainfall in August. These sort of statistical questions merely require collecting and characterizing data. Descriptive statistics can be of substantial interest in and of themselves, but oftentimes we would like to go further and use statistical data to draw conclusions- either about the nature of events in question or to predict the likely occurrence of future events. Inferential statistics is the umbrella term used to describe the wide field of using statistics to draw conclusions about how the world works and to forecast into the future.

Central to statistics is the concept of a probability distribution – a set of possible outcomes to which we can attach a probability for each of the outcomes of obtaining. For a coin toss there are two outcomes – heads or tails – and, if the coin is fair, each of the outcomes has a 50% chance of happening. For temperatures in Durham, NC in August there will be a large range of possible outcomes, with the highest probabilities concentrated around 80 degrees Fahrenheit. Probability theory enables one to make predictions about the likelihood of future events if we know the probability distribution

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for the variable in question. However, a probability distribution is an idealized concept, something that we never have pure epistemic access to in reality. Thus, statistical forecasting involves two conceptual steps: estimating probability distributions from past data, and using these estimates as a basis for future predictions. For statistical forecasting to work, it is crucial that the underlying distributions be stable or, if they are changing, that they change in a way we can track and characterize. Otherwise, one confronts the chaos of complete randomness, in which case anyone's guess is as good as someone else's.

True uncertainty is something that statistic cannot handle, and occurs when probability distributions are unknown or unstable. The economist Frank Knight famously developed the distinction between "true uncertainty" and "risk," the latter of which can managed through probability theory because it implies the underlying distribution is fairly well known. However there are many instances in which we face true uncertainty in society. This may be because we lack quality data that would enable us to understand the probabilistic parameters of some phenomena. But it could also be because the phenomena we are interested are not the product of some stable, underlying process. The existence of true uncertainty is a fundamental challenge to the useful application of statistics.

Also, there is a longstanding controversy in statistics regarding how to understand the processes that generate the data in question. "Objective" and "subjective" interpretations of statistics, which break down along the lines of "Frequentist" and "Bayesian" approaches, differ in how they conceive of randomness or error entering into

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the data we observe. In the case of the coin flip, the outcome is technically not random. Rather, it is determined by a number of factors – coin geometry and weight, launch angle and force, air resistance, distance to the ground, elasticity of surface materials, etc. If we had full knowledge of these factors, we could perfectly predict the outcome of every coin toss. Indeed, a team of engineers at Harvard has designed a mechanical coin flipper that reliably flips heads by imparting the same initial force conditions.[3] The apparent randomness of a regular coin flip is thus only randomness from our subjective perspective. Statistics, in this case, is a way of dealing with our ignorance of the true causal process, and sufficient symmetrical randomness in the uncontrollable influences of human coin tossing produces a statistically "fair" coin. Ultimately, from this perspective, probability theory is a way of dealing with our ignorance of the details of the underlying causal process. However, developments in Physics over the last century have raised the question as to whether genuine indeterminacy and randomness can be part of physical phenomena themselves. Radioactive decay is the paradigmatic example of a stochastic process at the atomic level. The average decay rate of a radioactive element is completely predictable, but it is currently impossible to predict when any particular atom will decay. In this case it appears that randomness is part of the physical phenomenon itself.

Related to these ontological questions concerning the nature and origins of indeterminacy is the issue of whether we should understand probability in terms of degrees of certainty (credence) as a Bayesian would recommend or in terms of the

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[3] http://www.associatepublisher.com/e/c/co/coin_flipping.htm

likelihood of a certain hypothesis being wrong as a Frequentist would recommend.[4] There are also approaches such as maximum likelihood estimation that combine aspects of both of these perspectives, but likewise involve assumptions that can be controversial. Sometimes our particular purposes suggest that one perspective would be more useful than another. We may want to ask what the probability of an outcome (10 heads out of 11 flips) is given that we have a fair coin, or we may want to know the probability that we have a fair coin given that outcome.

Radical Bayesians such as Leonard Savage helped develop the theory of expected utility in the mid twentieth century and made gambling the paradigmatic perspective for assessing probability.[5] Since Bayesians take probability to be an entirely subjective judgment of personal perspective, rather than an objective property that adheres in things, gambling provided an ideal way of getting at the substance of probability, by eliciting someone's belief about the likelihood of an event in a context in which misrepresenting that belief was costly. Probability for radical Bayesians is simply a matter of how much an individual will pay for certain types of bets or gambles.

There are interesting philosophical issues at stake in the way we conceive of probability, and different perspectives sometimes recommend different statistical approaches in practice. However, I flag these issues only to table them for the moment (I will return to some later). Although statistical debates generally focus on these issues of

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[4] Stephen Senn, "Bayesian, Likelihood, and Frequentist Approaches to Statistics" in http://appliedclinicaltrialsonline.findpharma.com/appliedclinicaltrials/article/articleDetail.jsp?id=84710&pageID=1&sk=&date=

[5] Leonard Savage, *The Foundations of Statistics* (New York: Dover Publishing, 1972).

subjectivity, variation, error, and randomness I want to call attention to the converse problem of structure.

3.1.1 The Importance of Structure for Statistical Inference

The coherence and utility of statistical analysis requires that there be some underlying structure generating the data of interest. The great advantage of statistical inference is that we can remain largely ignorant about the details of such structure. An event may have inherently random components at its very foundation, or we may introduce randomness through our ignorance of uncontrollable factors or from errors in measurement. However, at some level there is underlying structure in the process that produces an event of interest.

In certain circumstances statistics can be useful for uncovering this underlying causal structure. This is often the aim of research in the hard sciences, and methods for doing so are a subject of intense theoretical interest and debates.[6] However, the real glory of statistics is that it enables us to make predictions without fully understanding the underlying structure. When one comes to phenomena as incredibly complex as weather patterns, philosophical question about the sources of variability and randomness appear moot. Variation simply exists and the challenge is to model it in ways that prove productively accurate. There is undoubtedly structure there – Durham summers will be

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[6] See for example Judea Pearl's work, particularly *Causality* (Cambridge: 2009).

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hot, winters will be cold – although we cannot hope to grasp the immensely complex physical processes that determine the weather from day to day with perfection.

Nature presents us with processes of varying levels of complexity. Many processes appear determined by static, physical "laws" – molecular bonding, gravitational pull, etc. – while processes of higher complexity like weather or organic life involve causal structures that are not only complex, but can also undergo change. Consider, for example, our current concern with the possibility of climate change; that is, the prospect that weather patterns themselves may be fundamentally altered in ways that signal a definitive break with past patterns.

3.1.2 Origins of Social Statistics

Human society also has patterns that we can subject to statistical study. The Englishman John Graunt (1620-1674) was one of the first people to do so. He won admission to the Royal Society in for his work noting regularities in mortality rates recorded by parishes in and around London. In his *Natural and Political Observations Made upon the Bills of Mortality* Graunt reported a number of insights that emerged from aggregated mortality records, for example: "some Disease, and Casualties keep a constant proportion, whereas some other are very irregular (18)," diseases in the city peak in the autumn season (41), and a higher proportion of people die in the city than in the countryside (69).[7] There was considerable interest amongst Graunt's contemporaries in

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[7]John Graunt, "Bills of Morality" at http://www.edstephan.org/Graunt/00.html

using these insights to measure and manage outbreaks of bubonic plague. The utility of this sort of knowledge for public health was recognized, even if it was unrealized in Graunt's time.[8]

In the eighteenth and nineteenth centuries economic and demographic data became increasingly important to the management of the state, and the study of social patterns emerged as a new field of inquiry.[9] Indeed, the term "statistics" derives from the Latin *status* and *statisticum* suggesting 'knowledge of the condition of things' or 'pertaining to the state'.[10] As governments increasingly kept records for the purposes of their own administrative affairs, these records provided new insights into the nature of society when aggregated an analyzed.

Prior to the nineteenth century, probability theory had developed somewhat autonomously, as a pure mathematical enterprise. Important figures whose names are still recognized in contemporary statistics, such as Pascal, Bernouli, Bayes, and Laplace, developed the mathematical foundations of probability theory with little attention to practical uses, beyond games of chance. However, in the nineteenth century the relevance of probability theory for the management of populations became increasingly clear. Gauss (1777-1855) was perhaps the most important transitional figure in applying

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[8] He was able to show that contrary to popular opinion, the plague did not necessarily occur when a new monarch came into power, and he also established that London's population growth was fueled by immigration, not increased urban birth rates. (http://www.bookrags.com/biography/john-graunt-soc/)

[9] See Foucault, Michel, 1926-1984. *Security, Territory, Population : Lectures at the Collège De France, 1977-78*. Sécurité, Territoire, Population. English. Edited by Michel Senellart Basingstoke ; New York : Palgrave Macmillan : République Française, 2007,

[10] Royal Statistical Society, *Journal of the Royal Statistical Society*, Volume 46 (Great Britain) 657-658.

probability theory to empirical investigations, and Quetelet (1796-1874) developed statistics as the centerpiece of his "social physics." By the end of the century, methods of statistical analysis had become a central tool for natural scientists as well, particularly physicists (Boltzman, Maxwell, Einstein) and biologists (Brown, Galton, Pearson). Indeed it was biologists who pioneered many of the techniques of "regression analysis" for the study of populations that later came to dominate the social sciences.

For "hard" scientists, the idea of causal structures operating in nature was nothing new. Rather the great challenge in adopting statistical analysis for the natural sciences came in trying to make the sense of the concept of a statistical law. Laws by definition were supposed to imply determinacy. Initially it was not clear what it could mean for physical structures to be understood in probabilistic terms. In the study of human society, however, the converse question arose. Did regularities in social phenomena ultimately imply determinism and the lack of free will? Where did social structure and empirical regularities come from?

The statistical study of human society began, for a number of its intellectual pioneers, with the study of regularities in human physiology. Quetelet was astonished to find that the measurements of some 5,738 Scottish soldiers recorded in the *Edinburgh Medical Journal* displayed a symmetrical distribution. The largest group of measurements all fell right at the average of the entire population (40 inches in chest diameter) and the frequency of other measurements diminished in proportion to their

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distance from this mean.[11] Quetelet devised a clever analogy for why this sort of distribution would be generated by nature. The variety of body types reported was not unlike the reports one would get if only one body were measured over and over by different people. In measuring this one person, some would mistakenly overshoot (holding the measuring tape too loosely or being sloppy in their attention) others would undershoot. Most would get it fairly accurate, and extreme errors in measurement would be rare; moderate errors would be less rare.

Quetelet proposed that nature itself generates individuals as if measuring off from some idea type. Most people approximate this norm very closely, but there will be deviations or errors from the ideal, decreasing in number the further one goes out in any direction. Quetelet believe subsequent studies confirmed this theory of his. He examined the measurements of some 100,000 conscripts in the French army, and "found that they proceed in the most regular order and range themselves symmetrically on the two sides of the mean; in other words, the numbers group themselves with the same order as if they had been measured on the same individual 100,000 times in succession, with a probable error of 2 inches."[12] This theory informed Quetelet's claim that "each race of men has its particular type" as well as his larger social doctrine predicated upon the "average man."

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[11] Incidentally, it was later shown that Quetelet made a number of slight errors in his calculation.

[12] Quetelet, Translation Olinthus Downes "Letter XXI" in *Letters on the Theory of Probabilities as Applied in the Moral and Political Sciences*, (London: Charles and Edwin Layton) 95.

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Quetelet's worked proved a decisive contribution to the concept of a "normal" distribution and its application to social phenomena. In concluding that "The difference which nature makes in the heights of men is not greater than that which inexperience would produce in the measurements taken of one individual man (95, letter XXI)" he connected the idea of error to an underlying ideal structure. In this particular case the structure was physiological/biological; and it is easy to see how some social phenomena could flow directly out of biological variation. However, when Quetelet went looking, he found that stable distributions characterized a wide variety of social phenomenon – marriages, crimes, types of death. This raised profound questions about the structural sources of these regularities and the existence of human agency. In one of the most famous passages in his *Essay of Social Physics*, Quetelet explored the sources and implications of these patterns:

> The constancy with which the same crimes repeat themselves every year with the same frequency and provoke the same punishment in the same ratios, is one of the most curious facts we learn from the statistics of the courts; I have stressed it in several papers; I have repeated every year: There is an account paid with a terrifying regularity; that of the prisons, the galleys, and the scaffolds. This one must be reduced. And every year the numbers have confirmed my prevision in a way that I can even say: there is a tribute man pays more regularly than those owed to nature or to the Treasury; the tribute paid to crime! Sad condition of human race! We can tell beforehand how many will stain their hands with the blood of their fellow creatures, how many will be forgers, how many poisoners, almost as one can foretell the number of births and deaths.

> Society contains the germs of all the crimes that will be committed, as well as the conditions under which they can develop. It is society that, in a sense, prepares the ground for them, and the criminal is the instrument ...

> This observation, which seems discouraging at first sight, is comforting at closer view, since it shows the possibility of improving people by

modifying their institutions, their habits, their education, and all that influences their behaviour. This is in principle nothing but an extension of the law well-known to philosophers: as long as the causes are unchanged, one has to expect the same effects.[13]

Statistical analysis of society illuminated unrecognized regularities but in so doing also suggested ways in which society might be changed. The all important question, however, had to do with the origins and nature of the structures that gave rise to social patterns. Some of these structures might be hardwired into physiology (indeed the social Darwinists of the late nineteenth century thought that most social structures were either derivative of biological realities or best understood in reproductive/evolutionary terms). But other structures are more obviously "artificial" and open to change. Statistics enable researchers to perceive a host of social regularities for the first time. Where there seemed to be chaos and contingency before, statistical investigations revealed underlying order - not the order of a completely determinate and predictable mechanical system, but a probabilistic order rooted in certain structural patterns and regularities. As Ian Hacking notes, in the 19th century the process of recognizing and "taming" chance through statistical analysis signaled a significant intellectual revolution. It helped uncover the existence of underlying, probabilistic order in human life, and "statistical laws" came to be understood as "fundamental processes of nature and society."[14]

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[13] A. Quetelet, *Sur l'Homme Et Le Développement De Ses Facultés Ou Essai De Physique Sociale*Bachelier, (1835).

[14] Ian Hacking, *The Taming of Chance* (Cambridge [England]; New York : Cambridge University Press, 1990).

Although statistics helps identify the existence of underlying structure, the details of such structure often remain opaque. However, for those who want to employ statistical knowledge to help change society, questions about the nature of the underlying structure are extremely important. Quetelet and others were made aware of this in one way by the difficulty they encountered demarcating the boundaries of "populations." The average measurements of Scottish soldiers differed from those of French soldiers, and new conscripts differed from soldiers of a different generation. Quetelet settled on a concept of "race" which provided each group of people with an ideal type. Later, Darwinists would suggest how the types of races could themselves change. For other phenomena, the population boundaries and source of structure were more puzzling. Suicide rates tend to be very stable within countries, but differ widely between countries (Nordic peoples top the list). Crime rates also show a fair amount of stability within particular societies, but can differ dramatically between them. Accounting for the social and individual sources of these regularities became a central interest for a school of "structuralist" sociologist like Durkheim, whose study of *Suicide* was one of the most ambitious attempts to understand the genesis of social regularities.

At issue was the fact that social structures are generally not universal; rather they are human artifacts open to change. Statistics can help identify their existence and describe their features, and this knowledge can provide resources for changing the underlying structure. However, the possibility of structural change also poses a problem for the logic of statistical inference itself. This is because the utility of statistical inference breaks down in the face of radical structural change. Statistics requires stability

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in patterns in order to apply probability theory and draw valid inferences. If the structure of those patterns is changing, statistics quickly loses its predictive power. That is not to say that statistical analysis cannot deal with change. It can, but doing so requires that there be some underlying structure to the change itself.

The great challenge then for the useful application of statistics to social thought and social change lies in the nature and malleability of the "data generating" structure. Statistics, recall, rarely reveals the details of this structure directly. Rather, we infer the general structure from previous data. But how ought we to deal with new data that starts to deviate from past patterns? Is such data a rare aberration, the result of a large error in measurement or a hiccup in the data generating process? Or does this data indicate the process itself is changing, that a new pattern is emerging?

3.1.3 The Challenge of Change

Anyone who surveys the history of human civilization for the last 3,000 years will be struck by incredible variation in human societies and the revolutionary transformations that have continually refashioned the way people live. The sources of these transformations have been diverse. In recent history the most conspicuous driver of change is technology. But we have also witnessed revolutions in economic production, political institutions, habits of thought, ways of relating to others, standards of art, and norms of culture, to name but a few fundamental dimensions of change. Even the height of the "average man" has changed around the globe as populations have gained access to

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better nutrition. An honest appraisal of the history of human civilization suggests that one of the few constants is change itself.

Change, as I have noted, poses a problem for statistical analysis. At the most basic level there is the problem of identity. Conceptually, probability theory requires that events be similar enough to qualify them as instances of the same kind of thing, and thus generated from the same underlying structure. We have to compare apples to apples and oranges to oranges. In the face of radical change, or in situations of radical social difference, it is often difficult to satisfy this requirement of identity. Is *hari-kari* in Japan the same thing as suicide in the Netherlands? Would it make sense to pool them together in a statistical search for the correlates of self inflicted death? Are the Corcyrean revolution described by Thucydides, the American revolution, the French revolution, and the Bolshevik revolution all instances of something called a "revolution," which can be analyzed as if they were generated by the same underlying process? Is the basket of goods and services used to determine the consumer price index today really comparable to the basket used 40 years ago? Statisticians often try to rely on simple, formal rules to categorize events of interest according to objective standards. Rather than argue about the definition of a civil war, one can simply define civil wars as domestic altercations that claim 1000 war related causalities per year of conflict.[15] But definitions will only truly be useful for statistical analysis to the extent that they group things together that indeed arise

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[15] This is the standard definition in the quantitative conflict IR lit.

from sufficiently similar data generating processes. In a world of radical change, the statistical requirements of identity can be hard to satisfy.

Related to this basic challenge is the problem of changes in the underlying "processes" that generate events. For example, one of the best current predictors of quarterly economic growth in the US is the Ceridian-UCLA Pulse of Commerce Index (PCI). Ceridian manages gas payment cards that are used by most major trucking companies. Truckers use these as credit cards to fill up at gas stations. By tracking the quantity and location of diesel purchased on these cards Ceridian can create an index that reflects the quantity of goods being moved around the country by trucks. It turns out that this "real time" index is highly correlated with quarterly adjusted GDP growth – a statistic that takes many additional months to measure, aggregate, and report.[16] The PCI accurately predicted the recession of 2008 at a time when most economists still denied an economic downturn was on the horizon. Thus the PCI appears to be a wonderful statistical tool that provides useful forecasts of real GDP growth, simply by tracking the gas consumption of truckers.

Of course, a hundred years ago this particular statistic would not have worked as a good indicator of GDP growth. Motor vehicles were just being brought to market and most commerce was conducted by railcar, buggy, and boat. Diesel sales work as a good indicator today because of the structure of contemporary commerce and dominance of diesel trucking. However, as some commentators have suggested, it is likely that the PCI

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[16] Daniel Gross "The 18 Wheeler Recovery" in http://www.slate.com/id/2253823/

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may cease to be useful forecaster of GDP in coming years due to a variety of technological changes. Hybrid and electric trucks are entering the industry and will gradually push down diesel fuel consumption per mile travelled. Thus, although this will lead the PCI to register comparatively lower levels of diesel consumption this will not in fact reflect a real decrease in the quantity of good shipped. The structure that traditionally held these variables together is changing. Economists would be mistaken if they took the PCI-GDP correlation to be a permanent one.

Statisticians have developed many ways to try to address the fundamental problem of structural change. If we know the structure of the change itself, then we can control for it. In the PCI example, if Ceridian could get an accurate measure of the increasing fuel efficiency of trucks, then its analysts could offset their estimation of diminishing fuel consumption by the factor of efficiency in order to preserve a good measurement of the underlying relationship between the movement of goods and GDP. Of course there could be other sources of change that are less easily noticed or measured. For example, the delivery of digital content over the internet has drastically reduced the quantity of CD's and DVD shipped around the country; but this does not indicated a real decline in the commerce of data. Ultimately, unless structural change itself is determined by a stable process that we can study, change poses a serious challenge to the utility of statistical analysis.

Again, there methods statisticians can use to try to control for or to model change. "Matching" techniques can help address the problem of comparing apples to oranges. Instrumental variables can be used to provide a more robust examination of causal

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structure. Granger causality can be used to study dynamic processes that are correlated with other dynamic processes. We can argue about whether emerging trends are linear or non linear, and the exotic functional forms the latter could take. I will discuss the details of some of these strategies later, but what is important to note is that each of these depends on rather strong theoretical assumptions and special conditions for them to serve as adequate remedies for the problem of change. None of them is a magic bullet. In fact, the possibility of unpredictable structural change remains an insurmountable challenge to the useful application of statistical analysis for many social problems.

This is a theoretical point that is recognized and begrudgingly admitted by leading social scientist, although much empirical research proceeds as if unpredictable structural change in "data generating processes" were unlikely. A wide range of literatures consider the theoretical issue, but the extent of the challenge it poses to useful statistical modeling is, I believe, under-appreciated. Economists talk about "exogenous" shocks to their models as a way to refer to the unexpected influence of parameters that were not included as part of the initial logic of a model. However, if exogenous shocks happen often enough, the utility of the initial model is easily compromised.

There is also a great deal of debate about whether many social, particularly economic, processes are stationary or non-stationary. A "stationary" process is one that has a constant underlying trend to which the process will return despite intermittent shocks. A "non-stationary" process, however, is permanently affected by shocks and will not return to the initial trend. In fact, a process that is non-stationary in its "first moment" (i.e. has a unit root, see footnote) has a variance that diverges to infinity as time increases,

which is simply to say that we can say nothing about its long term outcome.[17] Truth be told, in the long run nearly all social processes are undoubtedly non-stationary. The question is whether they are sufficiently stationary (trend stationary) for the time horizon that concerns us.

In recent years, more attention has also been paid to the so-called "black swan" problem, about which Nassim Nicholas Taleb has written so elegantly. Taleb forcefully argues that history is disproportionately affected by hard to predict, rare events that are impossible to model statistically. September 11, the rise of the computer, the internet, World War I are all events that profoundly shaped the course of human history, but were entirely unpredictable a few years before their occurrence or invention. Taleb uses the term "black swans" to describe these events, in allusion to a common example from logic texts of medieval Europe, which held that "all swans must be white" because only white swans had ever been seen before. The discovery of a species of black swan in Australia upset this classic exemplar of probable inference. The simple fact that we haven't seen something happen before is no guarantee that it might not happen in the future, and this is a great weakness of statistical inference, which looks backwards to make predictions about the future.

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[17] There is a large technical literature examining whether a process has a "unit root"- a technical term indicating that 1 is a root of the characteristic equation (the first moment) of a given stochastic process modeled as a time series (I will explain this is more detail later). If a process has a unit root then it is non stationary. It is also possible for a process to be non stationary in its higher moments, with non constant variance, skewness, kurtosis, etc., which likewise presents problems for forecasting.

This underlying problem is also captured in the classic distinction drawn by the economist Frank Knight between risk and uncertainty. Stochastic processes that are stable and measurable can be the subject of risk analysis. Such analysis forms the foundation of the insurance industry and has wide ranging applications for any business. For example, actuarial tables reveal a fairly stable distribution of life expectancy, and on the basis of these mortality statistics, companies can issue life insurance. As long as the probabilities for death rates are fairly stable, insurers can make money by offering policies whose expected value in payoffs is less than the aggregate premiums.

More generally, managing risk is simply a matter of actuarial calculations about expected value. Business and financial institutions are able to plan for and hedge against possible futures through the intelligent analysis of risk. It was for this reason that Knight observed "measurable uncertainties do not introduce into business any uncertainty whatsoever." However, Knight was keenly aware that risk is only a special kind of uncertainty, one for which we understand the basic parameters of the underlying probability distribution. He noted that there was a "higher form of uncertainty not susceptible to measurement and hence to elimination."[18] This is what Knight called "true uncertainty."

True uncertainty is a fundamental feature of the social world and there is little that statistics can do to combat it. Knight noted that it is precisely the reality of uncertainty that gives economic competition its form and accounts for the importance of the

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[18] F.H. Knight, *Risk, Uncertainty and Profit,* (Washington: Beard Books Inc., 2002).

entreprenuer. Entrepreneurs are constantly trying new things, innovating in ways that have no precedent and no guarantee of success. The uncertainty of the future is what provides them with opportunity, and their activities generate new forms of social structure that mark a break with the past. The process of "creative destruction," which Schumpeter so brilliantly described, ensures that many social structures will not remain static.

Indeed, this is the central insight of information theory in the tradition of Austrian economics. According to this perspective, non-stationarity and subjective re-evaluations of probabilities are immanent to almost all aspects of social life. The parameters of any model that one writes down are liable to be changing constantly. Thus, Austrian economists argue that the central economic problem concerns how to deal with constant changes in the world and the impact of people acquiring and responding to new information.[19]

When things are going well people are apt to overestimate the constancy and structural stability of their ways of life. Economists spoke quite confidently in recent memory about the end of recessions. Consider Ben Bernake's now infamous speech delivered in 2004 to the Board of Governors of the Federal Reserve System on the topic of what he called "the great moderation." Bernake began by noting, "One of the most striking features of the economic landscape over the past twenty years or so has been a

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[19] See for example, F.A. Hayek, "Use of Knowledge in Society" *NYUJL & Liberty* 1 (2005).

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substantial decline in macroeconomic volatility."[20] He suggested that this decrease in volatility was here to stay. Stability in economic growth was, on his account, secured by advances in "structural features of the economy" that improved the economy's ability to absorb shocks and by increasingly intelligent macroeconomic models guiding monetary policy. Bernake also entertained the idea that this stability could simply be an artifact of good luck –there having been fewer and less severe shocks over the last two decades. Ultimately, however, he gave the greatest credit to advances in macroeconomic modeling and monetary policy.

The financial crisis begun in 2008 made a mockery of Bernake's conclusion. Although macroeconomic modeling and monetary policy have undoubtedly improved in recent decades, Federal Reserve models did not adequately incorporate recent structural changes in the world's economy, such as massive infusions of Chinese savings into US debt. These savings helped suppressed long term interest rates in the US in a way that decoupled them from many traditional statistical indicators of asset inflation. Although the financial crisis had a number of root causes, many economist now argue in retrospect that one of the contributing causes was the Fed's decision to keep interest rates too low for too long. Alan Greenspan has argued against this conclusion, suggesting that the incremental tightening of the short term federal funds rate would have had an inadequate impact on the long term rate with regard to deflating the housing bubble, and stronger anti-inflation measures would have posed serious threats to growth – which is something

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[20] http://www.bis.org/review/r040301f.pdf

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that almost no one would have accepted given the uncertainty of the alternative. However, Greenspan does admit, "We had been lulled into a state of complacency by the only modestly negative economic aftermaths of the stock market crash of 1987 and the dotcom boom. Given history, we believed that any declines in home prices would be gradual. Destabilizing debt problems were not perceived to arise under those conditions."[21] That is simply to confirm that history is not always a perfect guide in a world that is changing in fundamental ways.

3.1.4 Is Change Really a Problem for Statistics?

Many statistically oriented social scientists will surely object that the problem of structural change is not as pervasive and crippling a challenge as I have suggested. Yes, they may admit, it is a theoretically valid concern and may be true of certain classes of social questions, but statistics has nonetheless proven a powerful and useful tool for social analysis. It would be ridiculous to reject statistical analysis outright. Indeed, I do not deny that statistical analysis can be extraordinarily useful, and I certainly do not mean to reject statistical methods *tout court*. In dealing with complex and large amounts of data, which are increasingly ubiquitous in our information age, statistical methods are necessary for even beginning to grasp what is going on. And statistical models often provide valuable information that we could not obtain otherwise. Moreover, the social

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[21]http://www.brookings.edu/~/media/Files/Programs/ES/BPEA/2010_spr.ing_bpea_papers/spring2010_greenspan.pdf

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world is not one of complete Hericlitean flux. It does have significant structural stability in many respects; and in contexts of stability statistical techniques illuminate much that escapes other forms of investigation.

As Gary King and Eleanor Powell have noted, statistical analysis has proven useful for a wide range of social questions, and the predictions of statistical models often outperform expert opinion:

> For example, in a head-to-head contest two political scientists with a crude six-variable statistical model predicted the outcome of U.S. Supreme Court cases (without reading them) more accurately than a set of 83 law professors and other legal experts reasoning qualitatively and with access to enormously more information and decades of jurisprudential experience (Martin et al., 2004). For another example, political scientists have long been more successful at forecasting presidential elections than pundits, pollsters, and others (Campbell, 2005; Gelman and King, 1993). Tetlock (2005, p.64) has shown that most of his 284 articulate, highly educated, and experienced experts forecast many aspects of the political future with "less skill than simple extrapolation algorithms." Similarly, two political scientists with no medical training built a statistical model that out-performs physicians (assessing individual causes of death) in determining cause-specific mortality rates (King and Lu, 2008). These are but four of hundreds of such examples in many scholarly areas. Indeed, at least since Meehl (1954), numerous similar contests and comparisons have taken place across various fields of study and practice. The result is not always the same, but the same very strong tendency favoring the quantitative estimates is ubiquitous (Grove, 2005).[22]

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[22] King, G. "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science." *American Journal of Political Science* 30, no. 3 (1986): 666-87.

Likewise, as Clark Glymour has observed, "The evidence is nearly overwhelming that, on average, 'mindless' regressions do as well or better than experts at predicting complex behavioral outcomes, whether degree completion or recidivism."[23]

The question I want to raise is not whether statistics *can* be useful, but about the boundary conditions that set limits on the utility of statistics. Statistical models rely on a number of assumptions in order to make valid inferences, and we can scrutinize how well these assumptions are likely to obtain for any problem. The simplest parametric techniques for statistical modeling in the social sciences, such as Ordinary Least Squares regressions, rely for their coherence on extensive assumptions about the characteristics of the data and data generating process. Deviations from many of these assumptions can be corrected for in the rare case that we know the nature of the deviation. However, the problem of structural change in the data generating process itself is something that is almost impossible to diagnosis from looking at the data or to correct for accurately (and this can also be said of the related problem of omitted variable bias). The problem of structural change affects non-parametric statistical techniques as well.

My argument, then, is with those who think that statistics provides the best way to understand *any* social problem (for which we have a modicum of "data"), or who think more generally that statistics is the foundation of all valid knowledge. Some prominent methodologists do believe statistics can provide an absolute foundation for a unified science of nature and society. In a provocative article entitled "How Not to Lie Without

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[23] C. Glymour "Social Statistics and Genuine Inquiry: Reflections on the Bell Curve." *Intelligence, Genes, and Success: Scientists Respond to the Bell Curve* (1997), 263.

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Statistics," Gary King and Eleanor Powell continue an argument famously articulated by King, Keohane, and Verba in *Designing Social Inquiry*. The basic claim of that book was that 1) causal inference, statistically defined, is the fundamental objective in all science and 2) in the social sciences an overarching logic of statistical inference is appropriate for both "large N" quantitative research, *as well as* so-called "qualitative" research that deals with smaller and more unique case studies. King and Powell recapitulate an aspect of this claim in their paper:

> When sufficient information about a problem can be quantified (a crucial qualification!), a high quality statistical analysis is far superior to qualitative judgment. Mathematics and statistics enable human beings to reason properly even when informal human reasoning fails. Human reasoning, in turn, fails in highly predictable ways that qualitative experts have not been able to overcome even when the field of statistics has. Qualitative judgments by subject matter experts are routinely out-distanced, out-performed, out-reasoned, and out-predicted by brute force statistical approaches. This is true even when the data analysts know little about the substantive problem at hand and the quantified information seems shockingly incomplete to subject matter experts.[24]

Although they are rightly impressed by how many statistical models have outperformed experts, the authors err in asserting the superiority of statistical analysis for all social problems and in suggesting the irrelevance of detailed, expert knowledge about the particulars of individual circumstances. To the contrary, I would like to claim that detailed knowledge of particulars provides important insights into the limits of statistical analysis for certain problems. This sort of knowledge is particularly relevant for

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[24] King, G. "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science." *American Journal of Political Science* 30, no. 3 (1986): 666-87.

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identifying and dealing with situations in which a data generating process is liable to be changing in its structure.

King and Powell ultimately claim "Every inferential statement, empirical pattern, and notion of uncertainty can be represented sufficiently well, for the purposes of social science analysis, by the statistical theory of inference." This is surely mistaken, not the least because of the problem of structural change. It is also the case that a statistical theory of inference illuminates very little about, say, the process of a human conversation and conclusions drawn from it, much to the chagrin of those who work on artificial intelligence and have yet to produce a computer capable of passing the Turing test.[25] King and Powell aim too high in their aspirations for statistics, and indeed I would identify them with a tradition of absolute science that has been making similarly grand claims for scientific methods for the last few centuries.

To their credit, both King and Powell have conducted truly excellent and insightful social inquiries of their own drawing on statistical methods. And they admit there is much bad statistical work done in the social sciences. Indeed, many have lamented that the statistical training of the average social scientist is not very sophisticated, and studies have suggested that empirical work in the social sciences is riddled with mathematical errors and egregious but basic mistakes in research design.[26] So, statistical studies are often quite obviously inadequate, but according to King and

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[25] The ability of a computer to engage in an intelligent conversation with a human interlocutor.

[26] See for example, B. D. McCullough and Ross McKitrick. "Check the Numbers: The Case for Due Diligence in Policy Formation" *Studies in Risk & Regulation* (Fraser Institute: February, 2009).

Powell this is the fault of incompetent researchers not an indication of the methodological limits of statistics. King and Powell ask, "Should the fact that many statistical analyses are done badly cause us to conclude that quantitative approaches have no practical value? This would be the case without a fairly unified approach to the theory of inference."[27] It is precisely this logic of inference, mapped out in *Designing Social Inquiry*, that King and Powell elaborate and defend in their article as the final solution to social science research. I will examine their claims and layout my own critique in detail below, but at this point want simply to highlight the unbounded faith that some social scientist continue to have in the ability of statistics to constitute a complete methodological foundation for the social sciences.

If, however, the utility of statistical analysis breaks down in the face of radical structural change, the challenge for social scientists is to characterize and identify circumstances in which such change is occurring and likely to distort the conclusions of statistical models. I would like to suggest that there are at least two prominent sources of radical structural change in human society, and both can bear an important relationship to ethical convictions.

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[27] King, G. "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science." *American Journal of Political Science* 30, no. 3 (1986): 666-87.

3.1.5 Sources of Social Change

Unlike molecules, which behave according to stable physical properties that in turn give rise to aggregate phenomena captured, for example, by gas laws, the ways in which people behave are not written in stone. Many of the regularities we observe in society are derivative of widespread cultural frameworks that dictate how we behave towards other and how we can expect them to behave towards us. Some of these conventions appear arbitrary and trivial, like whether we greet others with a handshake or a bow, but cultural conventions often encode rather thick ethical expectations and can impose significant costs. It is notable that individuals generally abide by these norms despite the fact they could be abandoned without formal sanction. The practice of tipping in American restaurants is often cited as an interesting case of the widespread acceptance of a "costly" norm that has no mechanism of "enforcement." It is simply accepted that providing a 15% tip is the right thing to do when one has been served well by wait staff. Moreover, if we look closely, costly cultural norms that are self-enforced because of their ethical valence are less exotic and more widespread than one might expect from reading standard social science literature. People routinely make sacrifices for friends and neighbors, complex manners constrain the way we interact with strangers, and informal norms of fairness and legitimacy influence our evaluation and censure of public institutions. There are many stable and generally advantageous patterns in society that are held together by nothing more than common convictions and judgments about what is good and appropriate – judgments that may not be shared by societies in different times and places.

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The social implications of widespread ethical convictions can be immense. Think only of the way in which "attitudes" towards race have changed in the US – from the founding, to the Civil War, and Reconstruction through the Civil Rights era, and over the last two decades. Convictions about the meaning of race and the illegitimacy of racial discrimination have changed dramatically, and these changes have had a profound impact in the way that our social institutions work and on the social patterns we witness – in schools, in marriages, in private associations, and in business practices, not to mention explicit issues of public policy.

Thus, one source of social change comes from changes in the broad range of ethical commitments and larger cultural norms that structure the way people interact with each other and expect others to behave. These norms can be complex in their genesis and maintenance and are sometimes fragile. When a lightning strike caused a 24 hour blackout in New York City in the summer of 1977, social order broke down with astonishing speed. Widespread looting spread throughout the night, in the course of which 35 blocks of Broadway were destroyed, 1,616 stores were looted, some 4,500 looters were arrested, and 550 police officers were injured. The resulting damages were estimated in excess of $300 million dollars.[28] The reaction was undoubtedly aggravated by the economic recession of the time and existing anxieties concerning crime, but it stood in stark contrast to a similar blackout in New York in 1965, in which the city

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[28]David Frum, *How We Got Here: The '70s* (New York, New York: Basic Books, 2000) 14,15.

witnessed the lowest amount of crime ever recorded for a single night.[29] The chaos of the '77 blackout was also remarkably different from what we saw more recently in the Northeast Blackout of 2003 (the second largest blackout recorded in human history), which left most of New York without power for a similar length of time. Coming not long after the city has experienced the trauma of the 9-11 terrorist attacks on the World Trade Center, and a resulting surge in solidarity, there was no widespread looting or social unrest.

Although the social dynamics operating in each of these blackout were undoubtedly complex, the outcomes were influenced on some level by the expectations people had about what was right to do and what they could expect of others. Generally such beliefs and expectations are systematically rooted in what Charles Taylor has called "social imaginaries," an umbrella term meant to capture "the ways people imagine their social existence, how they fit together with others, how things go on between them and their fellows, the expectations that are normally met, and the deeper normative notions and images that underlie these expectations."[30] I will have a lot to say about this important concept of a social imaginary later. It is, I believe, a more capacious account of what North and Denzau have indicated by the term "shared mental models" in a series of paper highlighting the importance of shared beliefs and expectations for institutional

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[29] Ibid., 14.

[30] Charles Taylor, *Modern Social Imaginaries* (Durham: Duke University Press, 2004), 23.

performance (I will draw on many of their insights as well).[31] Although social imaginaries are typically shaped over a long time horizon by arguments, debates, institutional structures, educational regimes, and so on, these imaginaries can change very quickly in the face of new historical circumstances ("revolutions" provide exemplars of such change). Ultimately, the project of maintaining or changing the moral and cultural sources of social patterns can be more important than statistically studying their aggregate effects.[32] Changes in "social imaginaries," including changes in underlying ethical convictions, are one source of the kind of structural change that can undermine the utility of backward looking statistical models.

There is also a second, more common source of structural social change, which is rooted in the strategic considerations of practical rationality. People interact strategically with one another and the larger social world. Even if their "preferences" remain stable, people change their behavior based on new knowledge when this knowledge indicates how they can better accomplish their ends. This could include technological innovations, new configurations of incentives, and/or changes in the behavior of others. This general

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[31] See, Denzau, Arthur and North, Douglass. "Shared Mental Models: Ideologies and Institutions" *Kyklos* 47(1), 3-31, and Denzau, North, and Roy "Shared Mental Models: A Postscript" in *Neoliberalism: National and Regional Experiments with Global Ideas* (New York: Routledge, 2007). In these and other articles, Denzau and North have made extraordinarily perceptive observations about the importance of "belief systems" to institutional performance. Their basic account of what "shared mental models" consist of is useful, but limited in certain respect by their attempts to operationalize it for social science research. I consider their account to be a king of a subspecies of what Taylor indicates by "social imaginaries" and believe Taylor's conceptualization is richer and more insightful in many respect. Ultimately, however, these two accounts are very similar.

[32] It is one thing to know that Catholics in Chicago voted Democrat in the 1950's but the more interesting and powerful question is why that changed.

phenomenon is related to Frank Knight's observation about the role that uncertainty and innovation play in economic competition, and it indicates a problem that financial economists are very familiar with. To put it simply, significant rewards accrue to those who can innovate better ways to do things. We continually modify out world through our attempts to better ourselves and to avoid exploitation by others. This process of creative destruction that lies at the heart of a flourishing economy ensures that change is constant. This is one thing that both Capitalists and Marxists could always agree on: markets involve "constant revolutionising of production, uninterrupted disturbance of all social conditions, everlasting uncertainty and agitation."[33]

Marx and Engles did, it seems, overestimate the degree of political chaos produced by markets, but it is true that capitalist societies are undoubtedly dynamic societies. Perhaps the most interesting development in political economy over the last 150 years has been the emergence of what Wallis, North, and Weingast have called "open access" societies, in which both economic and political competition serve to dissipate rents and produce social outcomes that serve a large majority of citizens. It is the extreme dynamism of these societies that helps guard against the despotism of narrow, entrenched interests. [34](It should also be noted that open access societies rely on constitutional frameworks with broad ethical/ideological support in order to keep competition bounded

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[33] Marx and Engels, Translation, G.S. Jones *The Communist Manifesto* Penguin Classics, 2002.

[34] North, D. C., J. J. Wallis, and B. R. Weingast. *Violence and Social Orders: A Conceptual Framework for Interpreting Recorded Human History* (New York: Cambridge University Press, 2009.

by certain rules of justice and equality, about which I will have much to say later). In contrast, "closed access societies" characteristic of most "natural states" try to limit competition, dynamism, and change. Closed access, natural states are governed by elites who try to maintain power and wealth through a monopoly on force and the controlled creation of rents. Social dynamism is dangerous to these elites because it threatens to upset this established order. However, social dynamism is a fact of human existence. Natural states try, often in vain, to limit change, while open access states have found ways to accelerate and productively channel change. In any case, change is a fundamental feature of human society and characteristically driven by our ability to innovate and to interact strategically with others in an attempt to better accomplish our ends.

Statistical models are notoriously bad at dealing with problems involving strategic considerations. This is because strategic problems involve trying to outmaneuver an intelligent rival, and doing so generally requires breaking from past patterns and innovating new approaches that the other party does not anticipate. Military leaders confront this sort of strategic problem on the battlefield, and history is rife with examples of generals who lost because they came prepared to fight the previous war. Business executives face similar problems of strategy. They must constantly innovate and improve or risk being outmaneuvered by a competitor. The inability of statistical models to deal with forward looking strategic considerations was one reason for the relatively recent interest in building models up from "micro-foundations," which capture some of the strategic considerations of individuals, as well as the larger interest in rational choice modeling (which I will discuss at length in the next chapter). Indeed, it was no accident

that rational choice theory was pioneered by cold war strategists thinking about the unprecedented strategic challenges of nuclear deterrence.

Perhaps the most ironic feature of statistical analysis in reference to the strategic problem is the fact that statistical models can be undermined by their very success in providing useful insights. This is because the very process of exploiting statistical insights can destroy the initial patterns that gave rise to them.[35] A wide range of examples demonstrate this problem.

A study at Disney theme parks once discovered that when rides had multiple lines, the line on the left tended to move faster than the line on the right. Various theories were put forward as to why this might be. Some suggested this was because people favor the side of their dominant hand, or because Americans veered to the right due to their road system, or because people had a psychological disposition to favor the "right" side, subconsciously driven by the linguistic association between "right" and "good." Whatever the reason, it simply was the case that the left line moved faster on average than the right during the period studied. Had those who conducted this study kept its conclusions to themselves, they could have successfully decreased their average waiting time for rides at Disney. The problem was that the results of the study were widely reported, and within a few months the results no longer held. Convinced that studies had shown that the left line was superior, people started choosing the left with greater

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[35] It is also the case that patterns held in place by strategic considerations can complicate statistical investigations, as is often the case when there are endogeneity problems in a model. Models not sensitive to the endogeneity problem might conclude that fire engines cause fires or that the amount of money a candidate spends doesn't influence his or her chance of winning.

frequency, even when it looked slightly longer than the right. Widespread knowledge of the previous statistical patterns of line formation altered the way people behaved in choosing lines and thus did away with the pattern itself.[36] This trivial case is an instructive exemplar of the problem of statistical insights being undone by their success. Notice the problem is not that statistical analysis is not useful. To the contrary it is very useful for the first people to arrive at this knowledge. However, putting this knowledge to use can itself alter the structure that gave rise to the pattern in the first place.

In no field is this phenomenon more recognized than financial econometrics. The financial world provides some of the largest and most detailed data sets a social scientist could hope for. Yet, financial forecasting is extremely hard to do in the medium and long term. The problem is that genuine insights into current economic trends create opportunities for financial gain. The very process of exploiting these opportunities tends to eliminate them, and thus to engender a search for new opportunities. Statisticians can look at patterns and trends in prices, but because the future constantly changes (not the least because people strategically respond to emerging trends), past performance is never a guarantee of future results. In the near term statistical analysis may be able to identify useful trends and opportunities, but in the long term these are arbitraged away and subject

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[36] There is a surprisingly large interest by Disney goers in maximizing ride time. See:
http://www.ehow.com/how_5310999_out-disneyland-trip-great-tips.html
http://www.ehow.com/how_4787370_wait-shorter-lines-disney.html
http://travelingmamas.com/on-the-move-approved-walt-disney-world-with-kids-2009-and-giveaway/
http://www.ridemax.com/

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to unanticipated shocks. Burton Makiel's book, *A Random Walk Down Wall Street*, is a classic exploration of this phenomenon.[37]

The rise of quantitative trading has illustrated the promises and perils of statistical analysis in a context where the underling structure is subject to constant change through innovation and strategic action. Quantitative traders mine financial data for patterns, which they use to make bets without any knowledge of underlying economic fundamentals. Many quantitative trading firms have made extraordinary amounts of money in the last two decades using this approach. However, these gains are almost always on short term, high frequency trades, and modelers are in a constant arms race with other modelers to discover and exploit emerging patterns before others do.

Despite their high returns in periods of economic stability, quantitative traders did not fare much better than average investors during the recent financial crisis. *Market Watch* reported that quantitative hedge funds, whose "hedging" strategy was supposed to generate returns in any economic climate, "ended the year [2008] down almost 20%, a record, according to industry performance tracker Hedge Fund Research." [38] Moreover, some have argued that a mistaken confidence in the stability of past quantitative finance trends helped fuel the recent economic meltdown. The "Gaussian Copula Function" pioneered by the financial statistician David Li was used extensively by investors leading up to the crisis as a way to model default correlations using "credit default swaps" data

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[37] Malkiel, B. G. *A Random Walk Down Wall Street* (New York: W. W. Norton & Company, 2007).

[38] Allstair Barr, "Soros among firms that made money in 2008, 2009" http://www.marketwatch.com/story/soros-among-firms-that-made-money-in-08-and-09-2010-01-13

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rather than historical default data. Default correlations were crucially important for assembling and pricing "collateralized debt obligations" (CDO's), and at the start of the crisis mortgage backed securities comprised about 40% of all revenue streams in CDO's.[39] As Darrell Duffie, a Stanford finance professor and advisor to the bond ratings agency Moody's, noted "The corporate CDO world relied almost exclusively on this [Li's] copula based correlation model."[40] An article in Wired Magazine reported the unfortunate result:

> Li's copula function was used to price hundreds of billions of dollars' worth of CDOs filled with mortgages. And because the copula function used CDS prices to calculate [default] correlation, it was forced to confine itself to looking at the period of time when those credit default swaps had been in existence: less than a decade, a period when house prices soared. Naturally, default correlations were very low in those years. But when the mortgage boom ended abruptly and home values started falling across the country, correlations soared.
>
> Bankers securitizing mortgages knew that their models were highly sensitive to house-price appreciation. If it ever turned negative on a national scale, a lot of bonds that had been rated triple-A, or risk-free, by copula-powered computer models would blow up. But no one was willing to stop the creation of CDOs, and the big investment banks happily kept on building more, drawing their correlation data from a period when real estate only went up.[41]

The warning printed on every piece of investment literature – past performance is no guarantee of future results – was realized with devastating clarity in the financial

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[39] http://www.hudson.org/files/documents/Mason_Rosner%20for%20posting.pdf

[40] Felix Salmon, "The Recipe for Disaster" in http://www.wired.com/techbiz/it/magazine/17-03/wp_quant?currentPage=all

[41] Ibid.

crisis. Those with a broader historical view and more attention to economic fundamentals were better equipped to diagnose the dangers than those in quantitative finance who simply followed past statistical correlations.

There is another way in which the statistical techniques of quantitative finance have proved liable to strategic destabilization, one which has been highlighted by Andrew Lo, a finance professor at the Massachusetts Institute of Technology. When quantitative traders look at past financial data one thing that is excluded from their analysis is the response that other quantitative traders will have to the same data. Each will be vying in the current financial markets to make a profit off of past trends, and neither will be able to perfectly anticipate the other's actions (if they could, they would each try to preempt the other anyway). When their strategies get coded into a trading algorithm there can be unanticipated interactions with the strategies of other quants, who are likewise intervening in new ways in the markets.

Wired Magazine reported on the unintended consequences that can result: "In two dramatic episodes during the second week of August 2007, several prominent and successful US hedge funds suddenly suffered enormous losses in a few hours. The collapse - known as the 'quant meltdown' -- has been traced by Lo and colleague Amir Khandani to a deadly feedback loop between hedge funds following very similar strategies."[42] Quantitative traders were effectively done in by other quantitative traders, neither of which had realized how the other could fundamentally upset their expectations

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[42] Mark Buchanan "The Business of High Frequency Trading" http://www.wired.co.uk/wired-magazine/archive/2010/04/start/investigation-the-business-of-high-frequency-trading?page=all

about the structure of financial markets. In a world where people innovate and change their behavior based on new knowledge, statistical regularities are never certain to hold, particularly if they provide insights that people can strategically exploit for gain. Overestimating the structural stability of social patterns can lead to very costly overconfidence in our ability to predict the future. This true of a wide array of social problems, not just those closely related to economics.

Many of these concerns about the inability of statistical models to address problems of structural change were outlined in Robert Lucas' famous critique of the "econometric" theory of economic policy.[43] Lucas argued that econometric techniques, although often useful for short term forecasting, are not adequate for evaluating policy regimes. Lucas's point was that even if econometric techniques perfectly estimated the "true structure" of an economic process prior to a policy change, the policy change could itself fundamentally alter this structure and thus produce results discontinuous with predictions draw from the old structure. The "Lucas Critique" was immediately responding to debates about the correlates of inflation in the late 70's, but its implications were widespread. Lucas's ultimate conclusion suggested why statistical models dealing with aggregate economic data and lacking micro-foundations that capture the decision making logic of individuals will generally not reveal much about the effects of policy

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[43] Robert Lucas. "Econometric Policy Evaluation: A Critique", in Brunner and Meltzer, *The Phillips Curve and Labor Markets*, Carnegie-Rochester Conference Series on Public Policy, **1**, New York: American Elsevier (1976), pp. 19–46

interventions: "given that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any change in policy will systematically alter the structure of econometric models."[44]

Lucas's critique highlighted the role that expectations play in people's behavior, and he showed why changing expectations could fundamentally alter the structure of economic processes. He convincingly argued for the importance of incorporating "rational expectations" into statistical models via explicit micro-foundations for a large range of economic research questions. I will explore some details and implications of the Lucas Critique later in this chapter, and the critique provides compelling reasons for the turn towards rational choice theory that I examine in the next chapter. However, I want to note that while the critique is a devastating indictment of econometric models that ignore the dynamism of underlying structure, the remedy of micro-foundations that Lucas calls for is easier said than done. Indeed, in many cases it will likely be impossible to provide sufficient micro-foundations, and this could be for various reasons – because the ways people practically reason are themselves fundamentally changing, because strategic situations admit of multiple equilibria, or because we have no idea of the true range of considerations that enter into individual decisions. However, the Lucas Critique reaffirms the importance of structure for statistical inference and draws our attention to the importance of understanding the sources of structural change.

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[44] Ibid., 41.

3.1.6 Ethics and Change

As I noted earlier, structural change in society can be driven by broad shifts in the dominant social imaginaries, which are often indebted to changes in ethical convictions (such as beliefs about racial equality). That is one way in which ethics and ethical persuasion can be relevant to statistical patterns. There is a second way in which ethics can be relevant, relating to the problem of the strategic exploitation of statistical patterns, but this requires careful spelling out. Like all knowledge, statistical insights can be used for good or ill. Criminals could conceivably study the best times and places to rob someone with reference to the likelihood of a quick police response. Although that is one sort of ethical concern, it is not exactly what I have in mind in speaking of the strategic exploitation of statistical patterns. Rather, the problem is that in using statistics to address social ills well-intentioned reformers may misunderstand the underlying causal structure of a problem and end up incentivizing rather than alleviating it. Statistically justified policy interventions have been a poster child of sorts for the so-called "law of unintended consequences."

Those in power often think they can technologically manipulate the covariates of some social phenomenon in order to engineer a desired outcome. However, if people on the ground behave strategically it may be impossible to break out of bad social patterns simply by intervening in parameters historically correlated with a problem. Breaking out of bad social pattern may require a mix of careful attention to incentives at the individual level as well as forms of ethical forbearance that keep individuals from strategically

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undermining transitions to better social outcomes. Understanding and structuring incentives is a subject of intense research in the social sciences, but the challenge of ethically persuading people to forgo immediate opportunities in deference to their own and others' longer term benefits is less well recognized and understood. As will become clear in the next chapter, there are many situations in which strategically maximizing benefits to oneself can destroy social institutions that provide valuable goods to oneself and others. If one crafts policy only on the basis of statistical models one will be oblivious to the way in which ethical persuasion may be needed in order to transition out of pernicious social patterns.[45] As I have noted, statistical models are generally bad at dealing with problems of strategic action and the dynamism that strategic responses introduce into social structure. Ethical persuasion can help shape and constrain strategic action, which is an important possibility, despite being invisible to backwards looking statistical models.

As Lucas and countless others have pointed out, people respond to incentives, and by changing incentives we can upset good social patterns or entrench bad ones in ways not foreseen by statistical models. It was an often made observation that the welfare system in the US from the late 60's to 1996 tried to address poverty in many ways that were counterproductive. Having studied the correlates of poverty, those in charge of welfare policy directed financial resources to individuals in economically precarious circumstances with the expectation this would help them escape those circumstances.

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[45]Although, I suppose one could make the influence of attempts at ethical persuasion an explicit object of statistical inquiry.

Unfortunately, as many studies later demonstrated, these policies often had the perverse effect of drawing people into undesirable conditions. When welfare policy was set up so that "a pregnant, low income single woman is better off going on welfare than marrying a man with a typical low income job" such women were understandably reluctant to enter into marriage; and while this might have made them more independent and better off the same could not be said of children growing up in single parent homes.[46] The welfare reforms of 1996 wisely did away with guaranteed entitlements and reduced the penalties for marriage, and since then the rate of unwed childbearing, which had increased sevenfold since 1965, has leveled off (and actually decreased amongst blacks).[47] In addition to a change in incentives, the welfare debates of the mid nineties were accompanied by a sustained discourse on the benefits of two parent households for children (the legislation also provided funds for "pro-marriage" educational initiatives and counseling programs for spousal abuse). Although difficult to know, it is not inconceivable that the popular promotion of an ethic of responsible parenting help contribute to the positive movements in marriage rates alongside the change in financial incentives.

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[46] See, for example Thomas Sowell *Knowledge and Decisions* (New York: Basic Books, 1980) and Charles Murray *Losing Ground: American Social Policy, 1950-1980* (New York: Basic Books, 1994).

[47] Robert Rector and Patrick Fagin, "The Continuing Good News About Welfare Reform" in http://www.heritage.org/Research/Reports/2003/02/The-Continuing-Good-News; Christine Kim and Robert Rector, "Welfare Reform Turns Ten: Evidence Shows Reduced Dependence Poverty" in http://www.heritage.org/Research/Reports/2006/08/Welfare-Reform-Turns-Ten-Evidence-Shows-Reduced-Dependence-Poverty

A problem similar to the welfare case has been documented in the literature on aid directed to third world countries for "development." If aid is given to countries based on how poorly they perform on a variety of social indicators, this may ironically provide elites in such countries with incentives not to address these problems.[48] Aid may reward failure rather than mitigate it. Poorly designed aid programs based on naïve statistical models of economic growth have, according to prominent African development experts such as Dambisa Moyo "fostered dependency, encouraged corruption and ultimately perpetuated poor governance and poverty."[49] As Jagdish Bhagwati noted in reviewing Moyo's book, *Dead Aid*, "Many aid recipients were smart enough to realize that once wealthy nations had made a commitment to support them, shortfalls in their domestic efforts would be compensated by increased, not diminished, aid flows."[50] Growth models such as the famous Harrod-Domar model, which advised using aid to boost shortcomings in the savings rate of poor countries in order to maximize growth, ended up creating a species of what economists call moral hazard. Third world elites became further insulated from their bad policies rather than more responsive to the intentions of aid organization and the needs of their peoples.

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[48] Dambisa Moyo Moyo, D. "Dead Aid: Why Aid is Not Working and how there is a Better Way for Africa." (2008); Todd Moss, Gunilla Pettersson, and Nicolas van de Walle "An Aid-Institutions Paradox? A Review Essay on Aid Dependency and State Building in Sub-Saharan Africa" www.cgdev.org/files/5646_file_WP_74.pdf; Paul Collier and Anke Hoeffler "Unintended Consequences: Does Aid Promote Arms Races?" in *Oxford Bulletin of Economics and Statistics*, 69, 1 (2007) 0305-9049 (http://www.ssrnetwork.net/uploaded_files/4048.pdf); Karen L. Remmer "Does Foreign Aid Promote the Expansion of Government?" *American Journal of Political Science* Volume 48 Issue 1, 77 – 92

[49] http://www.ft.com/cms/s/0/d70b7cba-4732-11de-923e-00144feabdc0.html

[50] Jagdish Bhagwati, "Banned Aid" in http://www.foreignaffairs.com/articles/65905/jagdish-bhagwati/banned-aid?page=2

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The lesson to be learned is not that is impossible to help the poor through the provision of resources, but rather that attempts to do so through backwards looking statistical models are liable to be open to strategic manipulation and other incentives problems (even if models have correctly identified meaningful constituents of growth). Although aid organizations are increasingly trying to address this issue by intelligently structuring conditional aid programs, incorporating performance benchmarks, and extracting credible commitments from stakeholders, the basic problem of corruption has proved an intransigent obstacle in many countries. Problems of corruption in the third world are extremely well documented, and I will examine them in more detail at the end of this chapter. Corruption is a good example of the way in which strategic exploitation can undermine otherwise promising statistical insights. Although the structuring of incentives has an important role to play in overcoming problems of corruption in the third world, this is also a problem for which ethical persuasion can play an invaluable role. Undoing a culture of corruption is easier said than done, but there are examples of it being done that are instructive of the way in which ethical persuasion can help constrain strategic exploitation.

3.1.7 Summary of Claims

To recapitulate my overarching claims thus far: the utility of statistical analysis is severely compromised in the face of underlying structural change in social phenomena. Such change can be driven by large scale shifts in preferences and expectations, which

comprise what Taylor has called "social imaginaries," or change can be driven, very quickly, by innovation and strategic reasoning. I do not mean to suggest that these two forms of change are exclusive or exhaustive, but they indicate two powerful and systematic sources of social change. Also, although such change need not be indebted to transformations in ethical convictions, it can be – and I have suggested above ways in which ethics can motor or constrain change in important respects.

I am now in a position to ask and answer the crucial question for those who wish to employ statistical analysis for the greatest benefit in social science research: under what conditions are statistical models likely to be solid and effective guides for addressing social problems? The problem of structural change in social processes leads me to the following general conclusion: Statistical methods in the social sciences work best under conditions of relative social stability and similarity (otherwise their results will be incoherent or un-portable); and the insights of statistical models generally last only when they are not of strategic value or, if they are of strategic value, when their insights are kept sufficiently secret (otherwise they will be exploited and arbitraged away).

When we survey the social world we find that there are many such circumstances and problems for which statistical insights are likely to be of little value, if not misleading. If there is one area of major social concern in which statistical modeling has proved most perilous it is development economics. The situation of what we commonly call the "third world" is one in which there is a great deal of social instability within developing countries and vast cultural/historical/geographical difference between them. Moreover, it is an area in which recommendations of statistical models have been not

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only ineffective, but often strategically exploited by corrupt officials representing the supposed beneficiaries of aid.

In the final section of this chapter I will examine the perilous history of statistical models in development economics, highlighting their limits in practice. First, however, I need to explain the logic of statistical inference and its technical challenges in more detail.

3.2 The Logic of Statistical Inference

Returning to the rudiments of statistical theory, the logic of statistical inference endorsed by King, Keohane, Verba and others is straight forward. It builds upon the basic logic of scientific inference through which researchers aim to isolate the causal influence of some discreet treatment or parameter. In the classic formulation of scientific inference one crafts experiments that introduce a discreet treatment and then observe whether the treatment produces effects different than those observed without the treatment. Since it is, strictly speaking, impossible to observe the effects of a treatment and non treatment on the same object, scientific investigation make use of a "control" – and object that is for all purposes identical to the object that was treated for the purpose of counterfactual comparison. By comparing the difference between the control and the treatment the causal effects of the treatment should be made manifest.

There are a number of challenges to successfully implementing this method in practice. It is essential that the objects used for the control and treatment are sufficiently identical and that the actual treatment is the only way in which they differ in relation to

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the effect. Otherwise the observed differences might be due to factors other than the treatment. Also, researchers confront problems of accurate measurement. Human's can make perceptual mistakes, and the instruments they use to measure phenomena can also be a source of inaccuracy. Statistical theory provides powerful tools to deal with both of these problems – the problem of ruling out the possibility that random, uncontrollable factors account for differences between a treatment and control and the problem of measurement error. Statistics addresses these problems through the use of populations.

3.2.1 An Example

Suppose a scientist wants to know whether smoke inhalation reduces the blood oxygen levels of mice. She might take two mice of the same species, keep one in a smoke free environment and expose the other one to a smoky environment for months. She would try hard to keep every other aspect of the lives of these two mice exactly the same – same food, same exercise, same sunlight, etc. At the end she could take blood samples from both of the mice and test whether the smoky mouse indeed had a lower blood oxygen level than the mouse with clean air.

However, consider all the ways in which her conclusions might be mistaken. Suppose the clean mouse had a rare genetic condition that made its blood oxygen levels naturally lower than the average mouse, or that the smoky mouse had a rare condition that naturally made its levels higher. The two mice would not be sufficiently identical for the purpose of comparison and the conclusion of the study would be skewed because of the influence of an unaccounted for difference between the two mice. Or consider all the

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things that could go wrong with measuring blood oxidation levels. If the lab technician accidentally exposed the blood to air in the course of the analysis that would throw off the proper measurement of the oxygen level, or if a phone call slowed the technician down in taking the sample to the measuring machine that could skew the reading too. However, all of these problems could be mitigated by performing the experiment on a large population of mice. Some of the mice in both the clean and smoky groups might have genetic conditions that made their blood oxygen levels naturally higher or lower, but these will average out in a large population to yield a good indication of the profile of a standard mouse. Also, any random errors made by the lab technician in measuring the oxygen level will be averaged out, with the mean approaching a highly accurate measurement.

Studying large populations helps eliminate the problem of random influences on the underlying process or random measurement errors leading to invalid inferences. Large populations are useful because of statistical properties described by the central limit theorem and the law of large numbers. The central limit theorem states that independent and identically distributed random variables with a finite mean and variance will approximate a normal distribution. The law of large numbers shows that as observations of such a process increase, the average results converge towards the true mean.[51] The intuition behind these theorems is straight forward – if errors and deviations are randomly distributed their effects will wash out in a large population, meaning that

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[51] As a side note, the implications of the central limit theorem and law of large numbers are frequently exaggerated.

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the larger our population the more confident we can be that we are capturing its true properties (with regard to its mean and variance). Using large populations and probability theory scientists can increase their confidence that their experimental results track genuine features of the world rather than being random artifacts of chance, unrelated to the intended experimental treatment.

The stipulation of independent and identical distributions required by the central limit theorem is important. If a mouse included in the clean air population had a contagious virus that lowered blood oxygen levels and spread through a large part of the clean air population, this would alter the character of the population, and the blood oxygen levels of these mice would cease to be independent of each other. They would be systematically skewed by this extraneous factor. Likewise if the scientists introduced rats into the population in order to increase the N of the study, this would not be appropriate since rats have a different physiological baseline for oxygen levels and different physiological responses to smoke. That would be like introducing apples into a study of oranges – the underlying natural distributions/responses would not be identical. For an experiment to be meaningful, it is essential to begin with things that are appropriately similar. It is also important that researchers select populations of mice that are representative of mice in general, if the researchers would like to make conclusions about mice in general. These are all obvious considerations whose importance is confirmed by probability theory. The biggest advantage of employing populations and examining results statistically is that researchers can guard against random errors and perturbations

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and thus have greater confidence that the patterns they witness are indicative of the underlying reality.

The quality of information statistically derived from large populations can be truly remarkable in circumstances where the basic conditions of the central limit theorem and law of large numbers are met. A well known episode in the history of statistics, Francis Galton attended a country fair in 1906 and observed a weight judging contest for a fat ox. Some eight hundred people entered their best guesses as to how much the ox would weigh after it was gutted, and the closest guesses were awarded prizes. No one was lucky enough to have exactly guessed the correct weight, which was 1,198 pounds. Afterwards, Galton asked to borrow the tickets so he could examine them statistically. Supposedly, Galton was interested in demonstrating how poor common opinions were, and later remarked in a journal article that "many non experts competed… like those clerks and others who have no expert knowledge of horses, but who bet on races, guided by newspapers, friends, and their own fancies…the average competitor was probably as well fitted for making a just estimate of the dressed weight of the ox, as an average voter is of judging the merits of most political issues on which he votes."[52] When Galton tabulated the crowd's responses and calculated the mean, he was astonished to find that it came to exactly 1,197 – only one pound off from the final true weight.

Much has been made of the so-called "wisdom of crowds" suggested by this famous example. James Surowiecki recently wrote a very popular book by this same title,

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[52] See Surowiecki, "The Wisdom of Crowds." (Random House, 2004) xii.

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in which he argued, "under the right circumstances, groups are remarkably intelligent, and are often smarter than the smartest people in them."[53] However, Surowiecki and other promoters of the wisdom of crows often neglect to note how unique the circumstances of the ox judging contest were. What enabled this to be a nice example of the central limit theorem and law of large numbers at work was the fact that there was a well defined problem with a well defined solution along a single dimension of possibilities for which there already existed a wealth of common knowledge and which involved a skill – estimating weight from sight – that people had some experience with in practice, however rudimentary. This meant that the way in which people generated their guesses of weight had some basic, common structure. People knew what would be outlandishly high or low guesses and they were able to do a decent job approximating within a limited range in which skill and error was more or less symmetrically distributed. Had the task been one with which people were less familiar and which did not draw on common perceptual skills – such as guessing the number of grains of sand in a large barrel – the average guess would not have fared well. Likewise, Condorcet's famous jury theorem, by which he sought to demonstrate that groups arriving at decisions through a majority vote would yield decisions superior to individuals if the probability of every individual being correct was greater than .5, depended on some very particular and unlikely assumptions. Above all the assumption of independence is one that is unlikely to be satisfied in contexts of human deliberation and judgment, and the notion of an

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[53] Ibid., xiii.

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underlying probability of correct judgment is difficult to give much meaning to in many circumstances.

These sorts of caveats apply to attempts to use the central limit theorem and the law of large numbers to argue for the wisdom of crowds. However, the wisdom of large data sets is easy to establish if the data generating process is "hardwired," with stable characteristics of independence and identical distributions – rather than being artifacts of human judgment. Large samples of the same things give a much better indication of the true properties of a population than do small samples.

Returning to our fictional study of mice we can see why a well controlled experiment utilizing a large population can make researchers very confident they have properly identify the causal effect of the treatment by comparing the average outcomes of the two populations. In practice, however, this sort of idealized experimental set up is impossible to construct for most questions of interest to social scientists. The idea of finding a proper control group is difficult to begin with. If you wanted to perfectly know the effect of foreign direct investment on Japanese economic growth following the Second World War what country or countries could serve as controls? What countries look exactly like Japan in 1946 on all variables conceivably relevant to economic growth from cultural history, to population, to geography, to having been attacked with an atomic bomb? The answer is, of course, none. Moreover, it is almost always infeasible, unethical, or simply inconceivable that social scientist could subject populations to "treatments" in order to study the social effects of policies, disasters, innovations or other phenomena of interest.

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Periodically, social scientists stumble on to so-called "natural experiments" – situations in which there are two very similar populations, one of which experiences a particular event or set of events of interest (Putnam's study of social capital in northern and southern Italy was one prominent case). Such situations can serve as an ideal "laboratory" for careful investigations of the influence of particular events. However, these situations are relatively rare with regard to most issues of serious concern, and social scientists are not in a position to, say, subject half of a society to a civil war in order to study its comparative effects. Although social scientists have increasingly tried to conduct "experimental research" on a small scale, such as with economic or psychological experiments amongst small groups of people, questions about the scalability and external validity of these studies suggests they may not be particularly relevant to understanding complex social problems in the real world (although they may provide powerful ways to challenge theories of social scientists - more on this in the next chapter).

Social scientists generally do not have access to carefully controlled environments with large, identical populations randomized along all likely confounding influences, as recommended by the classic logic of scientific inference. Instead, social scientists have to use "observational" data to try to tease out comparative differences that can be attributed with some likelihood to changes in particular variable(s) of interest. Statistical theory suggests conditions under which this sort of search is likely to be fruitful. The crucial question is whether a large, randomly sampled population will be effectively randomized

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with respect to all influences on the outcome, unrelated to the treatment variables in question.

Perhaps the best way to explain this challenge is by examining the analogous implications for our mouse study. If we couldn't put mice in a controlled lab experiment we would have to observe them in the real world. Scientist would go out and collect data on mice that live in smoky environments and ones that live in clean air environments. However, these mice would differ in all sorts of ways other than the quality of their surrounding air. They would have different diets, live at different altitudes, be of different ages, have different life histories, etc. – all of which could also significantly influence their blood oxygen levels. If the mice the scientists sampled for the smoky group tended to be over representative of one these additional factors affecting blood oxygen levels, this would skew the resulting inference - differences in outcomes would be attributed to smoke which in fact were due to other factors. Statistical theory suggests a possible solution. If the sample is sufficiently large and sufficiently random then the effects of extraneous factors will be averaged out and thus neutralized. As long as the extraneous influences are not themselves correlated with quality of air, a sufficiently large and truly random sample will ensure that the average influences are equal across both groups and thus will not distort one's measure of the average effect of smoke indicated by the difference between the two groups.

Large samples and randomization are the key to making valid inferences from observational data. However, sufficient randomization can be difficult to achieve. Moreover, complexities in the underlying structure of the data generation process can

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complicate attempts to measure the effects of the variable in question (in our case, smoke exposure).

The logic of inference requires that there be conditional independence between the explanatory and explained variables, which is to say that the value of the explanatory variable is independent of the values of the variable being explained. We want to know if smoke affects blood oxygen levels, but what if blood oxygen levels in turn affected whether a mouse was exposed to smoke? In our case, if having a low blood oxygen level somehow made mice instinctually seek out fresh air, this would pose problem for our analysis. It would mean there is a recursive relationship between the cause and the effect, which would make it difficult to tease out which was which. Statisticians refer to violations of conditional independence as problems of "endogeneity." There are complex ways in which a statistician can try to correct for endogeneity if it is an unavoidable feature of the data (via a control function, for example), but doing so requires substantive assumptions about the structure of the recursive relationship.

Another consideration of underlying structure concerns the problem of omitted variable bias. There are many factors that might influence blood oxygen levels, such as diet or altitude. However, as long as these factors have no intrinsic relationship to smoke exposure we would not need to explicitly consider them in our analysis of the effects of such exposure. Their independent influences on our mice will be averaged out and thus neutralized through randomization. However, if some factor that affects blood oxygen levels were systematically related to smoke exposure this would bias our estimates of the impact of smoke exposure. It is not implausible to imagine this being the case in our

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study. High altitudes tend to decrease blood oxygen levels in mammals, but high altitudes also tend to be colder, which could lead mice at altitude to seek out fires for warmth and thus be exposed to smoke. If true, then high altitude would affect blood oxygen levels on its own and also induce mice to greater smoke exposure, which in turn would further affect blood oxygen levels. In order to properly measure the effects of smoke on blood oxygen levels we would have to control for altitude. (Similarly, one could imagine smoke exposure affecting the appetite of mice and thus their diet. If the number of calories in a mouse's diet in turn influenced blood oxygen levels, we would have to control for diet as well.)

Omitted variable bias can severely affect statistical conclusions. Apparently strong correlations can turn out to be completely spurious in extreme cases of omitted variables. Unfortunately there is no statistical "test" to determine if omitted variable bias exists. It is something that researchers have to argue about at the level of theory and intuitive plausibility. Moreover, there is no way to statistically "correct" for omitted variables short of including them as controls. However, simply adding more control variables in the hopes that one will not omit any of importance is absolutely not a solution. First of all, adding more variables can create dimensionality problems, requiring much larger data sets in order to draw statistically sound conclusions (I will discuss this problem in more detail later). Most importantly, though, as Kevin Clark has pointed out, "The inclusion of additional control variables may increase or decrease the bias, and we

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cannot know for sure which is the case in any particular situation."[54] It really is a problem that can only be addressed based on the best prior theoretical arguments we can muster about the likely structure of the phenomenon.

Given all these caveats about judgments researchers will be required to make about the structure of the underlying process, we can begin to formalize the problem of mathematically estimating the average causal effect of smoke exposure on the blood oxygen levels of mice with observational data. (Here I follow an analogous example and formalization by King, Keohane, and Verba).

The mathematical expressions that statisticians use have built into them certain idealizations of probability theory. Thus, statisticians conceive of observed outcomes as instances of a random variable drawn from an underlying distribution with stable properties. For our purposes we can let Y_i Y_i Y_i indicate the random variable for which y_i

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[54] Kevin A. Clarke, "The Phantom Menace: Omitted Variable Bias in Econometric Research" *Conflict Management and Peace Science* 22, no. 4 (2005), 1. Clark also provides a quick formal confirmation of the problem of omitted variable bias which I reproduce here:

Suppose that the correct specification of a regression model is $Y_i = \beta_1 + \beta_2 X_{i2} + \beta_3 X_{i3} + \beta_4 X_{i4} + \varepsilon_i$, $\varepsilon_i \sim N(0, \sigma_2)$, but we estimate the misspecified model $Y_i = \beta_1 + \beta_2 X_{i2} + \beta_3 X_{i3} + \varepsilon_i^*$, where $\varepsilon_i^* = \beta_4 X_{i4} + \varepsilon_i$, and β_2 is the coefficient of interest. Under the assumption that the expected value of ε_i $\widehat{\beta_2}$ is zero, the expected value for $\widehat{\beta_2}$ is given by Hanushek and Jackson (1977) as $E[\widehat{\beta_2}] = \beta_2 + \beta_4 b_{42}$, where

$$b_{42} = \frac{(r_{42} - r_{32}r_{43})}{1 - r_{32}^2}\sqrt{V_4/V_2}$$. b_{42} is the regression coefficient on X_2 in the "auxiliary" regression of the excluded variable, X_4, on the included variables, X_2 and X_3. Thus, the effect of omitting X_4 depends on the magnitude of the excluded coefficient, β_4, the correlations between the included variables and the excluded variable, r_{42} and r_{43}, the correlation of the included variables, r_{32}, and the variances of X_2 and X_4 (denoted V_2 and V_4).

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y_i is the measured blood oxygen level in the *i*-th mouse and X_i X_i be the random variable

for which x_i x_i indicates whether the *i*-th mouse was found in a smoky environment, with

X taking the value of 1 if the environment was smoky and 0 if not. By convention, we'll

use μ μ to indicate the average of the random variable Y_i Y_i. We want to estimate

difference between average blood oxygen levels in these two environments, which will

give a measure of the effect of smoke on oxygen levels. Let us use β to indicate the

magnitude of this effect. β can be expressed as:

$$\beta = E(Y_i|X_i = 1) - E(Y_i|X_i = 0) = \mu^S - \mu^C$$

Which is simply to say that β is equal to the expected value of the blood oxygen

level of a mouse when it is in a smoky environment minus the expected value of the

blood oxygen level of a mouse when it is in a clean environment –and this in turn is equal

to the difference between the average values observed in the smoky and clean

environments.

The value of blood oxygen levels as a function of the average effect of

smoky environments can thus be expressed in the following simple model:

$$E(Y_i) = \mu^C + X_i(\mu^S - \mu^C)$$

(or) $$E(Y_i) = \mu^C + \beta X_i \tag{1}$$

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As most readers will recognize, this resembles a rudimentary form of regression. Regression analysis is a way of estimating parameters such that they enable a particular function to best approximate some given set of data. Regression analysis relies on all of the statistical considerations outlined above to produce parameter estimates (and to describe the statistical properties of those estimates). It is important to note that regression analysis is not the only way to approach questions of statistical inference. However, regressions provide a powerful and convenient way to study phenomena that we believe can be described as functions of particular variables, and is by far the most utilized statistical technique in the social sciences.

In order to deal with the problem of omitted variable bias suggested above, when we have reason to believe altitude needs to be included, we could specify the following linear regression model:

$$Y = \alpha + \beta_1 X + \beta_2 A + \varepsilon \qquad\qquad (2)$$

Where X refers to smoke exposure and A to altitude level; and the coefficients β_1, β_2 indicate the respective effect of each on blood oxygen levels, conditional on the other variable. ε refers to the "errors" - the unaccounted for influences on blood oxygen levels. However, in a sufficiently large and randomized population the expected value of these errors should be zero $[E(\varepsilon_i) = 0]$.

In trying to estimate values of β_1, β_2 that make this function best fit the data, if we assume that errors are uncorrelated and have equal variance, then the gauss-markov theorem shows that the "best, linear, unbiased, estimators" are those that minimizes the

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squared errors of the residuals. In plain language, that is simply to say that we want to calculate the coefficients that minimize the distance between the predictions of the function and the actual data. Doing so is a straightforward (albeit laborious) calculation, and software packages now do this in a matter of seconds. All researchers have to do is upload their data and specify the regression function they want to estimate. This so called "Ordinary Least Squares" regression is most common form of regression utilized by social scientists. As noted, the gauss-markov theorem at its heart does rely on the assumption of uncorrelated errors (no autocorrelation) and equal/constant variance (homoskedasticity). There are ways to test if this is the case, and there is a huge technical literature regarding how to correct for violations of these assumptions. Correction is possible but again requires further assumption and claims about the structure of the underlying process.

Other considerations of structure can also complicate attempts to model phenomena with simple OLS regressions. So far in this example we have talked as if all we are interested in is whether a mouse was found in a smoky environment when it was sampled. That is, our measure of smoke exposure is an all or nothing binary variable – a mouse is either exposed or not. However, for theoretical reasons suggested by biology we may want to measure duration of smoke exposure, giving us a continuous rather than dichotomous variable. We could express this as a percentage – the percentage of a mouse's life during which it is exposed to smoke. This would require more work in the field (we might tag mice and check up on them periodically) but it could be done. Biological theory may also suggest, however, that the relationship between smoke

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exposure and blood oxygen levels is not linear. Going from never being exposed to smoke to being exposed 10% of the time may have a bigger effect than going from being exposed to smoke 45% of the time to 55% of the time. That is to say that the relationship might have exponential properties. However, the effects of altitude might still be linear. If the relationship of percentage smoke exposure to blood oxygen is indeed non-linear then the functional form of our model would be seriously misspecified. We would need to specify a more appropriate model incorporating an exponential functional form. For example:

$$Y = \alpha + \beta_1^X + \beta_2 A + \varepsilon \qquad (3)$$

Estimating values for β_1 , β_2 that make this function best fit the data would be more complex than in the OLS case. With non-linear equations there may not be a closed form expression for calculating non-linear least squares (the "best fitting" parameters), but we can use iterative numerical approximation techniques for these estimations (also done by statistical software). In any case, this nonlinear functional form will generally reveal a different relationship between our variables than we would have arrived at with our linear form. The expectations we have about underlying structure, which are written into the functional form of the model, will radically affect the conclusions we draw from the model.

Recall that our original aim in this hypothetical study was to establish the influence of smoke exposure on the blood oxygen levels of mice. We were interested in drawing a conclusion about whether and how much smoke affects blood oxygen. The

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problem would be slightly more complicated if we wanted to come up with a predictive model that would allow us to predict blood oxygen levels in mice more generally. For this model to work well in the real world we would want to include all the variables that have significant and independent influences on blood oxygen levels, which might include age, diet, and amount of exercise in addition to smoke exposure and altitude. A plausible predictive model might look something like:

$$Y = \alpha + \beta_1^X + \beta_2 A + \beta_3 Age + \beta_4 Diet + \beta_5 Exercise + \varepsilon \qquad (4)$$

Although, upon further theoretical consideration (or perhaps because we didn't get the results we liked with this model) we might have reason to believe that some of these factors interact with each other in ways that powerfully affect blood oxygen levels. Age and exercise may both be important predictors of blood oxygen levels on their own, but perhaps in combination they have a disproportionate effect – exercising when old may really boost oxygen levels more than exercising when young. In this case we could introduce interaction effects into our model between exercise and age:

$$Y = \alpha + \beta_1^X + \beta_2 A + \beta_3 Age + \beta_4 Diet + \beta_5 Exercise + \beta_6 (Exercise)(Age) + \varepsilon \qquad (5)$$

We can compare this and the previous model to see if one fits the data "better" than the other (better could mean a few things here: goodness of fit, smaller p values, etc- more on that momentarily). Also, we should note that as the number of variables in the model increase, probability theory demonstrates that we need a lot more data in order to

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draw conclusions with the same level of statistical confidence we had in the smaller models.

The big question at this point is: how ought we to evaluate these models and choose between them? As I have noted, with regard to many structural questions, such as the problem of omitted variables, there is no clear way to judge whether one model is "more correct" than another. We make assumptions in crafting any model, and most of these assumptions cannot themselves be established by statistics. With regard to specifying functional forms, prior theory also plays the primary role, although there are metrics, such as goodness of fit measures, that one can use to argue for the appropriateness of one specification over another. With regard to the gauss-markov conditions (heteroskedasticity, autocorrelation), there are various ways of statistically testing for violations of these assumptions and avenues for mitigating the biases they introduce if we understand them well enough. With all these standard caveats in the background, most researchers end up evaluating models based on the statistical properties of the model and its estimated parameters.

The most straightforward measures of how well a model fits the data is (at least for linear models) the so-called "R-squared" value, which consists of the ratio of the explained variance of the model's predictions to the total variance of the data. Being calculated in reference to variance of the data, this measure can be difficult to grasp intuitively. The dependent variable in any sample has a particular mean and variance. A statistical model helps to predict deviations from that mean. R-squared indicates the fraction of those deviations that are accounted for by the model's predictions. In this

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sense it is a measure of how well the model explains the data's variation, and the closer to 1 the more variance it explains.

This can, however, be a deceptive metric for a number of reasons. One could perform various mathematical transformations of the data (say a logarithmic transformation), which would change the variance of the data but preserve its underlying systematic relationships (and thus still yield valid estimators when analyzed). The way it is calculated, R-squared conflates the causal strength of the estimators with the goodness of fit relative to variance. Moreover, as a technical matter one can always specify a model of sufficient complexity to perfectly fit past data. Using higher order polynomials is an easy way to accomplish this with most data sets. However, "over fitting" a model in this manner almost always ensures that it will not do well on new data (higher order polynomials, for example, will blow up or down towards infinity at their tails, and their oscillations in the middle are unlikely to track new data if they were conveniently generated to fit all the bumps of past data). Because adding additional variables introduces additional "degrees of freedom" to a model, which generally enhance its goodness of fit, statisticians developed an "adjusted R-squared" metric, which corrects for the number of explanatory terms in a model (adjusting down if the additional variables do not increase fit more than would be expected by chance). Also, statisticians have developed a generalized R-squared metric for use with non-linear models.[55]

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[55]See N. Nagelkerke, "A Note on a General Definition of the Coefficient of Determination," *Biometrika*, vol. 78, no. 3, (1991) 691–692.

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R-squared metrics give some sense of the how well a model fits the data, but there are other arguably less ambiguous measures. The standard errors of the regression estimates are useful ways of judging how likely errors of different sizes will be when employing the model (although correctly estimating the standard error depends on the true mean of ε being 0). Mean squared errors (MSE) are perhaps the most intuitive and insightful metric of model fit. As the name suggests, they indicate the average of the data's squared deviation from the model predictions.[56] The MSE thus shows the size of the average deviations from the model's predictions. Ultimately an F-test stands as an important statistical evaluation of the model as a whole. It enables us to test the likelihood that all coefficients in a model are zero. The larger the F-statistic, the more likely it is that the model coefficients are not zero, suggesting the model is of some explanatory value.

Using these metrics and tests we might want to investigate whether model (4) had lower mean square errors or a larger F-statistic than model (5), and this could provide reasons to believe one is better for forecasting blood oxygen levels than another. However, social scientists are typically less interested in overall model fit and more interested in knowing whether a particular variable is important to an outcome. In our case, the initial question as to whether smoke exposure affects oxygen levels is of this sorts. The coefficients estimated by the model suggest the magnitude and direction of the average effect of smoke on oxygen levels. However, the data contains a lot of variation,

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[56] I am indebted to Chris Gelpi for having alerted me to the mathematical strengths of different goodness of fit estimators which were not immediately obvious to me.

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randomness, noise, etc. – what are the chances that our coefficient estimates are way off? A "t-statistic," which consists of a coefficient divided by its standard error, enables us to address this question. By locating the t-statistic of a particular variable on a student's t distribution (which describes the probabilities associated with t statistics taking into account degrees of freedom) we can identify the "p-value" of a coefficient. As one statistics resource succinctly explains, "the P value is the probability of seeing a result as extreme as the one you are getting (a t value as large as yours) in a collection of random data in which the variable had no effect." [57] That is to say, p-values indicate how likely it would be for a model to arrive at this particular coefficient estimate if the true value of the coefficient were zero (the "null hypothesis").

It is on the basis of the p-value that researchers declare that a coefficient is "statistically significant." Generally, this is said when the p-value is less than .05, although there is nothing magical about this number and there is no reason a researchers should neglect to report the exact p-value. It is important to note that identifying a coefficient as statistically significant in itself tells us nothing about the magnitude of the coefficient's effect. This is something that has to be judge by the estimate of the coefficient and its role in the model (along with the model's general adequacy) – a point forcefully highlighted in McCloskey and Ziliak's recent book The *Cult of Statistical*

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[57] Princeton University, "Interpreting Regression Output"
http://dss.princeton.edu/online_help/analysis/interpreting_regression.htm

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Significance: How the Standard Error Costs Us Jobs, Justice, and Lives.[58] Moreover, these authors point out that, more generally, "Statistical significance is neither necessary nor sufficient for substantive scientific significance."[59] What statistical significance does suggest is how unlikely it would be that an estimated coefficient is simply an artifact of chance given the variability of the data.

At this point we could perform a t-test on β_1 in models (2), (3), (4), and (5). Although this wouldn't tell us anything about whether one model was better than another, it would help confirm whether smoke exposure affects blood oxygen levels, if we have the model right. Again, drawing valid statistical conclusions regarding the magnitude and significance of the effects of smoke exposure depend on all the structural assumptions of the model being correct – omitted variables, functional form – as well as all the important data assumptions holding or being corrected for– sufficient randomization, sufficient identity/independence/homogeneity, no uncorrected heteroskedasticity or autocorrelation, etc.

3.2.2 Structural Assumptions are Important!

I use this example drawing on mouse biology because it is a case in which all relevant structure is presumably derived from some fixed, underlying physiological reality. In the course of outlining the logic of statistical inference and the rudimentary assumptions of regression analysis and evaluation we have observed the many, serious

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[58] Ziliak, S. T. and D. N. McCloskey. *The Cult of Statistical Significance: How the Standard Error Costs Us Jobs, Justice, and Lives* (Ann Arbor: University of Michigan Press, 2008).

[59] http://www.press.umich.edu/pdf/9780472070077-response.pdf

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difficulties that arise in trying to properly deal with structural considerations – and this in a context where the deep structure is ultimately static. Consider how much more complex and perilous statistical analysis is when the underlying structure is liable to be changing! As I have suggested, there are good reasons to believe that many social processes indeed are open to radical structural change.

The idea underlying the use of regression analysis for social investigations is clever. We treat social phenomena as if they arose from stable processes that are functions of a limited number of stochastic variables. Social phenomena are considered as generic instances of some ideal type, each generated from the same underlying process but manifested in slightly different forms because of random perturbations. We move from Quetelet's concept of the average man to a social scientist's concept of the average civil war. Both, we suppose, have an underlying ideal structure generating the phenomena with a certain amount of natural variance. Even if the whole scheme strikes us as too idealized, social scientists hope that the conceptual analogy is close enough to prove reasonable basis for the useful application of statistical methods.

With the mouse example, I tried to illustrate how difficult it is to get regression analysis right, and how these difficulties compound the less certain we are about the characteristics and stability of the underlying structure. In a succinct passage, Clark Glymour provides a wonderful summary the various challenges of employing regressions for social analysis:

> Regression is a wonderful method for extracting causal information from data, provided very strong assumptions are warranted, for example, that none of the regressors are effects of the outcome variable, and that there

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are no unrecorded or neglected factors that influence both the regressors and the outcome variable, and that various distribution assumptions are met.

There is an enormous literature on ways of detecting erroneous distribution assumptions in regression models – non-normality, nonlinearity, heteroscadicity, autocorrelation, etc – and heuristics for fixing some of these flaws. Despite this attention, statistics textbooks routinely preach against using regressions as a method for inferring either the existence or strength of causes from nonexperimental data. The reasons have to do with the sensitivity of regression conclusions to causal assumptions that cannot be checked by the usual regression diagnostics. The most frequent worry of this sorts is, in statistical jargon, correlated error – the error term in the regression model may be correlated with one or more of the regressors. Sampling variation aside, neither I nor most people believe correlations come from nothing, and I understand correlated error to mean that the regression model omits variables that influence both the outcome variable and one or more of the regressors, so that the association between the regressors and the outcome may be due, in whole or in part, to omitted influences. Mosteller and Tukey, for example, devote an entire chapter to examples of fallacious causal inferences from regressions, and when unpacked each of their cases involves an omitted common cause of regressor and outcome variables. Another, less commonly noted but equally serious, concern is that in observational samples the values of the outcome variable may have influenced which units appear in the sample, resulting in a bias in regression estimates of linear dependencies.

Correlated error and sample selection biased by the outcome variable are only particular issues within a more general body of concerns; the estimates obtained using a regression model depend on intricate ways, rarely discussed in the statistical literature, on whether the causal claims of the model are a correct account of how the data were generated. A great deal of social science…is in no position to make a case for the causal assumptions necessary to use regression reliably in causal inference. [60]

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[60] C. Glymour, "Social Statistics and Genuine Inquiry: Reflections on the Bell Curve." *Intelligence, Genes, and Success: Scientists Respond to the Bell Curve* (1997), 257. A terminological note- Glymour uses the term "regressors" to refer to what are commonly called the "independent variables" and the term "outcome variable" to refer to what is commonly called the "dependent variable."

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In order for regression analysis to work as social scientists hope, researchers have to carefully consider the nature of the data generating structure they believe is responsible for creating the patters they want to analyze. As noted earlier, statistical models have proved powerful insights into social processes. However their insights can be quickly undone if the structure of the pattern is changing, and I have suggested at least two systematic reasons that social structures often do change, namely because of broad shifts in the way people relate to one another, which I have described as social imaginaries, as well as ubiquitous change introduced by strategic behavior and innovation. In situations of social stability and similarity, statistical models promise to yield lasting insights if specified correctly; but in situations where systematic change or difference is likely, statistical modeling will have lower and lower returns.

Gary King and Eleanor Powell admit that change can be a problem but they suggests that the statistical literature considering the problem of "nonstationary" is equipped to handle it. Arguing against those who think "path dependence" poses a problem for statistics, King and Powell write,

> To see this point, note that any historical or time series process can be decomposed into stationary and nonstationary components. The stationary components, no matter how complicated, are those which follow the same probabilistic patterns whenever they occur, so that for example the effects of events or shocks to the time series do not grow without limit over time. The nonstationary components are those parts with increasing or decreasing returns, or any feature dependent on a particular historical time.[61]

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[61] King, G. "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science." *American Journal of Political Science* 30, no. 3 (1986), 13.

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The first thing to note is King and Powell's faith that social processes do have significant stationary components, which is to say that at the level of deep structure they follow the same probabilistic patterns whenever they occur. This is already to neglect the possibly of radical structural change –the thought that something we are interested in might not follow the same underlying probabilistic pattern in all times and places. It is absolutely not clear that there is a long term stationary component to many social phenomena of interest. However, even if we accept that there is some stable deep structure generating the kind of thing were investigating, sufficient shocks from non stationary components can destroy the utility of our stationary knowledge. Unless the non stationary components can also be modeled with sufficient accuracy, the "shocks" of non stationary components can overwhelm the stationary process. The statistical literature on nonstationarity is impressive in its technical sophistication, but at the end of the day it provides no guaranteed solution to the problem of structural change.

Although I have focused on the difficulties of regression analysis, which is the dominate form of statistical analysis in the social sciences, let me reiterate that there are other forms of statistical analysis, some of which avoid the shortcoming of regressions for particular problems. King and Powell are right to note, "A strange notion exists within the qualitative method literature, and in some areas of quantitative research, that quantitative estimates of causal inferences require some form of regression analysis. This notion is false. Not only does the quantitative literature include numerous types of

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inferential methods for estimating causal effects, but some methods are very close to those used in the qualitative methods literature."[62]

King and Powell go on to promote "matching" techniques – ways of pairing things that are similar with respect to most relevant variables but different with respect to a treatment variable of interest. Making statistical inferences from groups of matched pairs helps minimize effects from confounding variables and is a way of making sure that researchers have taken the problem of similarity/homogeneity seriously. Other non-regression based statistical techniques, such as cluster analysis, factor analysis, analysis of variance, neural networks, and various kinds of non-parametric models can also serve as useful tools for social inquiry; but none of them can circumvent basic problems of structure that I have raised.

As King and Powell note in qualifying their enthusiasm for matching:

> Of course, matching, like regression and all other quantitative and qualitative methods used to estimate causal effects from nonexperimental data, requires that the investigator identify all the potentially confounding variables to control for, not merely one convenient variable as in our example. As always, the potential confounders include all variables which meet three conditions: they are causally prior to the treatment, related to the treatment, and affect the outcome variable after controlling for the treatment; other variables can be ignored. Avoiding omitted variable bias is a difficult problem in observational data, but it is of course well-known throughout the discipline and so we at least have ways of thinking about and attacking it. Matching and other approaches do not enable researchers to sidestep omitted variable bias, only to avoid making all but the most minimal additional assumptions after the omitted variables are identified and measured.[63]

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[62] King, G. "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science." *American Journal of Political Science* 30, no. 3 (1986), 20.

[63] Ibid.

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All statistical methods depend on assumptions about the identity and variability of the underlying structure that gives rise to the phenomenon of interest. Omitted variable bias names but one aspect of the importance of getting structural assumptions right. In a diverse and changing social world there are many circumstances in which our ignorance of important features of structure, particularly the dynamism of the structure itself, severely limits the utility of statistical analysis.

3.2.3 Forget About Structure?

There is a plausible rejoinder to all of this concern with structure. The logic of statistical inference outlined by Gary King and colleagues is intended to provide a coherent account of the conditions under which statistical correlations will actually pick up on genuine causation. But suppose we do away with concerns about causation all together. In many cases we may not have the slightest clue about the underlying mechanisms or possible confounding variables, but raw correlations can still turn out to be useful. Ancient peoples had no idea why a celestial object like the moon should be able to influence the sea level, but that did not prevent them from making useful forecasts of the tide based on the moon's cycle. An extreme version of the instrumentalism we saw endorsed by Milton Friedman might recommend throwing out theoretical statistical concerns derived from a comprehensive account of causal inference. Instead we could search for all manners of correlation, regardless of theory, and see if these correlations work for predicting things of interest. A-theoretical data mining evaluated by its success

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in making predictions might sound like a plausible instrumentalist response to the theoretical concern with structure.

The problem with such a stance is that the logic of statistical inference demonstrates why a-theoretical data mining will rarely achieve the predictive success it supposedly aims at. If, in fact, a-theoretical correlations were judged by their "out of sample" predictive success, this would serve as a reasonable form of quality control, but most correlations would undoubtedly fail. The stipulation of out of sample predictive testing is a way of reintroducing causal considerations from the back door, as it were. Data mining will only "work" reliably if it is picking up on some genuine structure. And all the considerations outlined above in the logic of statistical inference suggest why it will be difficult for statistical techniques to pick up on genuine structure in many situations.

So, there is no way to escape the basic lessons we learn from statistical theory. Granted, it may be useful to experiment with implausible statistical models to see if they pick up on some new insights that prove useful when tested in the real world. However, the great danger in encouraging a-theoretical data mining is that social scientists can lose sight of the all important requirement of out of sample predictive success. If, instead, researchers evaluate models based only on the statistical properties of the models themselves in relation to existing data, there is absolutely no reason to believe the results will tell us anything. This is because a model can always be crafted to fit past data with exquisite accuracy, but we generally observe a tradeoff between model "over fitting" and predictive success.

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As I suggested earlier, a functional form of sufficient complexity and/or the addition of extra control variables can always enable one to craft a model that fits a given data set very closely. This is a well documented fact that is often commented upon. High order polynomials can be introduced into a linear regression to fit past noise very well, but there are generally good theoretical reasons to doubt that that this functional form is capturing something fundamental about the data generating process. Indeed such over-fitted models almost never perform well on new data, and they have mathematical properties that make them radically diverge at the tails of the data set. However, with regard to their initial dataset, over-fit models will have wonderful statistical properties – a high R-squared, low mean squared errors, and likely lower p-values than other plausible models. It can be a real challenge to know if one has over-fitted a data set by adding additional variables or employing exotic functional forms, and there is no way to test for over-fitting in the context of a single data set.

More generally, those who would turn to non-parametric statistical models are confronted with the so called "curse of dimensionality" – the fact that as one increases a model's parameter space linearly, the quantity of data needed to draw conclusions of the same statistical quality increases exponentially. Put more intuitively, as the number of variables increases the easier it becomes for a model to wrap itself around the existing data. In order to preserve the same density of data one had in an initial model, when adding additional variables one must increase the amount of data by an exponential factor. For example, if one began with ten data points in two dimensions from which one estimated a regression line, one would need a hundred data points in three dimensions in

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order to estimate a plane with the same statistical resolution. So, while increasing

parameters will make models "fit" the data much better, we'll have much less confidence

in the statistical quality of such models if the amount of data stays the same.[64]

All of these concerns lead Scott Demarchi to make the following observation

about the quality of statistical research in the social sciences:

> Unbeknown to anyone save the original researcher, choices are made in empirical work. Lots of choices. Given the obvious problem of false correlation, it does not seem too much of a stretch to imagine that any empirical modeler, given time, can produce almost any result that is desired. Journals and monographs, by their nature, only report 'positive' results and only the 'final' model. How much pain or guesswork or outright cheating at the margins that goes into an empirical paper is never seen in print.[65]

This is a serious problem for social science research. If researchers are not

attentive to theoretical considerations of structure or disciplined by out of sample testing

on new data, there is little reason to believe statistical models of social phenomena are

likely to be useful for addressing any concrete problems. All of these concerns simply

serve to strength my initial claim that statistical analysis is likely not to be useful in many

contexts. Theory further suggests that statistical analysis has the best chance of be useful

in contexts of relative similarity and stability, in which the insights of such analysis do

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[64] See Scott Demarchi's extensive discussion of dimensionality problems 46-50, includes formal illustration of these problems. (Scott DeMarchi, *Computational and Mathematical Modeling in the Social Sciences* (Cambridge: Cambridge University Press, 2005).

[65] Scott DeMarchi, *Computational and Mathematical Modeling in the Social Sciences* (Cambridge: Cambridge University Press, 2005), 13.

not provide knowledge of strategic use to those involved in the phenomena under investigation.

3.3 An Illustration of Where Statistics Fail to Be of Much Use – The "Developing" World

There are many areas of social concern that do not meet these ideal conditions for the fruitful application of statistical methods. Perhaps the most conspicuous set of social problems that fall under this category are those pertaining to the political and economic development of the third world. Countries in the third world share some common features. They are poor, lack infrastructure, have histories of political instability, are typically autocratic, have meager, corrupt, or discriminatory legal institutions, and are often times plagued by social unrest and political violence. However, they also differ in many important ways. They have different geographical features, different resource endowments, different population characteristics, different neighbors, and, above all, different cultures, political histories, and (often) notions of the good. Citizens of more prosperous nations have been understandably interested in helping peoples of third world escape poverty, disease, famine, and oppression. Indeed extraordinary resources have been devoted to ameliorating the problems of the third world over the past 50 years for both humanitarian and strategic reasons, and some 2.3 trillion dollars have been spent by Western, government-sponsored development organizations alone.

Five decades of concentrated aid and development efforts have, however, achieved far less than anyone had hoped. Although there have been notable

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improvements among a number of countries, the larger picture for the so-called "bottom billion" is one of stagnation and occasional decline, particularly in Africa. The historical relationship observed between aid and growth is, on the face of it, particularly abysmal (as indicated in the following graph[66]).

Figure 1: Inverse Relationship between Aid and Growth in Africa

The largest aid programs run by organizations such as the World Bank, USAID, and the IMF have understandably sought to employ the best research that the social sciences can provide in service of development. For statistically oriented social scientists this problem might seem straight forward. Simply investigate what factors/variables are

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[66] Taken from William Easterly, "Can Foreign Aid Buy Growth?" *Journal of Economic Perspectives* 17, no. 3 (2003), 35. Online version available:
http://www.nyu.edu/fas/institute/dri/Easterly/File/EasterlyJEP03.pdf

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correlated with economic growth and government improvement, and then have aid organization promote those factors. It is tempting to think "development" could be technologically engineered in this manner through statistical knowledge of the correlates of good outcomes. This sort of approach, however, has proved perilous for a number of reasons.

First, there are significant problems with the quality and quantity of data that exists. It is often hard to collect information of interest, and data going into the past can be of very poor quality.

Second, there is an extraordinary amount of instability in the third world. Quiet coups, civil wars, bloody revolutions, natural disasters, massive migrations, drought, famine, earthquakes, tsunamis, and other calamitous event occur frequently in these vulnerable societies and subject them to structural changes at a faster rate and on a wide scale than is commonly witnessed in developed countries. Moreover, the very purpose of western aid is to *change* these societies, and outside "interventions" can alter previous structures in fundamental ways. Thus, there are many likely points of discontinuity in the social structures of the third world, not the least of which are created (or hope to be) by those who attempt to develop these societies.

Third, problems of strategic exploitation pervade the interaction between aid organizations and the elites of these societies. With huge amounts of money at stake, it should be no surprise that aid efforts can create incentives problems, as elites try to maximize their own cut of the aid rather than the good outcomes sought by aid organizations. The problem of what economists call moral hazard is particularly severe

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when decisions to award aid are driven by statistical indicators of bad performance. One can end up entrenching bad outcomes and promoting dependence by providing resources on this basis. A more general problem is that aid resources can quickly be dissipated through various forms of corruption and lack of accountability. Attempts to invest in "growth factors" can, even if such factors are properly identified, be undone by corruption and other forms of strategic exploitation.

Finally, statistical investigations of third world development run up against a fundamental problem with regard to statistical theory, namely the problem of identity – comparing apples with apples. This problem is related to the general problem of structural change, but is even more basic. In order to have enough data to hope to make valid inferences, statistical studies of developing countries generally rely on pooling together information from different countries. But what reason is there to believe that the processes driving economic growth in Indonesia are the same as those that drove growth in Ethiopia or El Salvador? It is an extraordinary leap to assume that the data generating processes underlying outcomes of interest are identical across different societies. This problem is compounded by the frequently vague conceptualizations social scientists invoke to characterize the outcomes they study. Social scientists talk about "democratization" as if it were some universal process that had a clear meaning. Although researchers have tried hard to formalize this concept, with indexes like "Polity IV" scores or Freedom House rankings, such formalization begs the question as to whether what they call democratization are instances of the same generic thing, brought about by the same underlying processes. By assumption, statistical models of third world

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development typically rule out the possibility that social phenomena in these societies are genuinely different and possibly unique.

As David Collier and James Mahon noted in their classic APSR article on the problem of "conceptual stretching," "Stable concepts and a shared understanding of categories are routinely viewed as a foundation of any research community. Yet ambiguity, confusion, and disputes about categories are common in the social sciences. A major source of this difficulty is the perpetual quest for generalization."[67] In trying to generalize across countries, researchers routinely neglect the particularities of local contexts and dismiss the possibility that there are deep differences that make it inappropriate to identify events in different societies as instances of the same generic process, be it "democratization," "liberalization," "industrialization," "modernization," "authoritarianism," "corporatism," or any number of other reified social concepts. Social scientists often "stretch" their research concepts in an attempt to establish some general theory that applies across different societies, cultures, times, and places. But is the same thing – the same data generating process – really occurring in these different circumstances? If not, the logic of statistical inference loses its coherence.

The problem of false generalization and conceptual stretching is cleverly illustrated in Alasdair MacIntyre's parody of the "theory of holes":

> There was once a man who aspired to be the author of a general theory of holes. When asked "What kind of hole – holes dug by children in the sand

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[67]Collier, D. and J. E. Mahon Jr. "Conceptual" Stretching" Revisited: Adapting Categories in Comparative Analysis." *American Political Science Review* 87, no. 4 (1993): 845-55. http://www.la.wayne.edu/polisci/kdk/seminar/sources/collier2.pdf

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for amusement, holes dug by gardeners to plant lettuce seedlings, tank traps, holes made by road makers?" he would reply indignantly that he wished for a *general* theory that would explain all of these. He rejected *ab initio* the – as he saw it – pathetically common-sense view that of the digging of different kinds of holes there are quite different kinds of explanations to be given; why then he would ask do we have the concept of a hole? Lacking the explanations to which he originally aspired, he then fell to discovering statistically significant correlations; he found for example that there is a correlation between the aggregate hole-digging achievement of a society as measured, or at least one day to be measured, by econometric techniques, and its degree of technological development. The United States surpasses both Paraguay and Upper Volta in hole-digging. He also found that war accelerates whole digging; there are many more holes in Vietnam than there were…had he concerned himself not with holes but with modernization, urbanization, or violence, I find it difficult to believe that he might not have achieved high office in the APSA.[68]

MacIntyre draws our attention to the way in which generalized categories can underwrite implausible or absurd research programs. Studies of development are particularly vulnerable to these sorts of overambitious generalizations.[69]

Even King and Powell – apologists for the universal application of statistics – admit that cross country work often runs into serious difficulties on this front. In the context of discussing the problem of conceptual stretching – the problem of overextending general concepts to categorize diverse social phenomena – these authors confess that it does poses a serious challenge to statistical analysis:

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[68] A. MacIntyre, "Is a Science of Comparative Politics Possible?" in *Against the Self Images of the Age* (Indiana: University of Notre Dame Press, 1978).

[69] This is but an aspect of a deeper problem in social thought concerning how one negotiates particularity and universality. Aristotle offered one of the most penetrating explorations of the problem of how particulars can be instances of a universal in his *Metaphysics*, but he ultimately leaves the problem unresolved. The problem may indeed be insoluble at a metaphysical level, and for pragmatic purposes we always have to ask whether things are "sufficiently similar" to be grouped together for useful analysis.

…the term conceptual stretching refers to "the distortion that occurs when a concept does not fit the new cases" (Collier and Mahon, 1993, p.845). To avoid conceptual stretching, qualitative scholars attempt to select cases that fit their categories and carefully adjust their categories or concepts to fit their cases. Conceptual stretching is not only important in designing research but is also at center stage in a large intradiscplinary dispute generated by severe criticism qualitative scholars have levied at cross-national statistical studies over the last half-century ("no branch of political science has been in more extreme ferment than comparative politics during the last fifteen years"; see LaPalombara 1968, p.52 and Girosi and King 2008, Section 1.5). Since these early days, qualitative scholars have branched out from area studies to be more comparative in more careful ways, and quantitative scholars have developed methods to avoid observations that do not belong in the same data. The connections between these two approaches are not well known in the two literature but deserve to be.

For example, King and Zeng (2006, 2007) proved mathematically that when a statistical quantity of interest is far from the data, inferences are more model-dependent, which means that small, indefensible changes in statistical assumptions can lead to unacceptably large differences in empirical conclusions. This proof was designed for quantitative work, but it also applies directly to the problem of conceptual stretching in qualitative work. It gives some precision to the qualitative notion that the farther you stretch a concept by applying it to new cases from distant (conceptual) lands, the more untenable are the assumptions that would need to be defended and justified in order to shore up a claim that one's inferences are still valid.[70]

Moreover, Bill Gibson's survey contribution to the *International Handbook of*

Development Economics supports all of my claims:

Econometric models, as applied to developing countries, suffer from more extreme violations of the underlying assumptions of the classical linear regression model than in the more stable environment of advanced countries. Strictly speaking, time series econometric models would only apply to a self-replicating stationary state in which nothing of fundamental

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[70] G. King, "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science." *American Journal of Political Science* 30, no. 3 (1986): 25.

importance changed over the estimation period. In particular, the assumption of repeated samples drawn from independent and identical conditional probability distributions (i.i.d.) for each value of the independent variable is severely compromised. This is well known, of course, and tests and corrections for heteroskedasticity are widely available and widely applied. In time series models, the i.i.d. assumption implies structural stability and is violated as a matter of course in developing economies, since structural change, rather than stability, is the explicit objective of most development policies. Beyond the violation of the most fundamental assumption of structural stability and heteroskedasticity, econometric models suffer from simultaneity bias, omitted variables and other model misspecifications, selectivity bias, as well as measurement and censored and cluster error. Econometric models applied to developing economies often ignore, for example, structural rigidities such as foreign exchange and skilled labor shortages, and the presence of a large informal sector (Behrman and Hanson, 1979). Policy and coordination problems are sometimes also overlooked, as are various endogeneities peculiar to developing economies, such as credit flows, human capital formation and even monetary and fiscal policy when authorities lack independence.[71]

Despite these various problems, warning, caveats, and concerns, social scientists have not shied away from placing a great deal of faith in the ability of statistical research to guide development projects. In fact, statistical methods have become increasingly tailored to accommodate cross country studies. "Pooled cross section time series" (PCSTS) regressions now dominate empirical research in comparative politics and development economics. Although PCSTS regressions are formulated to extract the most information from cross-country time series data, this approach to modeling does not escape any of the concerns raised above. Indeed, the shortcomings of PCSTS research are well known and often discussed by methodologists. Nonetheless, PCSTS models remain

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[71] Bill Gibson "Data Problems and Empirical Modeling in Developing Economies" in *International Handbook of Development Economics*, (Ed. Dutt, Krishna, Ros) Volume 1. (Northampton, MA, USA: Edward Elgar Publishing, Incorporated, 2008) 84.

the most widely used methodological tools in political economy and it is from theses that many aid organization draw conclusions that guide their development efforts.

The economists Frederico Podesta offers a concise overview of the attraction of PCSTS models as well as their characteristic shortcomings. I quote his overview at length, because it provides a very efficient summary of the standard view of the technical strengths and weaknesses of these models:

> Pooled analysis combines time series for several cross-sections. Pooled data are characterized by having repeated observations (most frequently years) on fixed units (most frequently states and nations). This means that pooled arrays of data are one that combines cross-sectional data on N spatial units and T time periods to produce a data set of $N \times T$ observations. Here, the typical range of units of analyzed would be about 20 (if we examine developed countries), with each unit observed over a relatively long time period, like 20-50 years. However, when the cross-section units are more numerous than temporal units (N>T), the pool is often conceptualized as a "cross-sectional dominant," conversely, when the temporal units are more numerous than spatial units (T>N), the pool is called "temporal dominant" (Stimson 1985). Given this preamble, we can write the generic pooled linear regression model estimable by Ordinary Least Squares (OLS) procedure
>
> $$y_{it} = \beta_1 + \sum_{k=2}^{k} \beta_k x_{kit} + e_{it}$$
>
> Where i = 1,2,….; N; refers to a cross-sectional unit; t = 1,2,….; T; refers to a time period and k = 1,2,….; K; refers to a specific explanatory variable. Thus, y_{it} and x_{it} refer respectively to dependent and independent variables for unit i and time t; and e_{it} is a random error and β_1 and β_k refer, respectively, to the intercept and the slope parameters. Moreover we can denote the $NT \times NT$ variance-covariance matrix of the errors with typical element E($e_{it} e_{js}$) by Ω. Estimating this kind of model and some of its variants (see below), solves many problems of traditional methods of the comparative research (i.e. time series analysis and cross-sectional analysis). Several reasons support this.
>
> The first reason concerns the "small N" problem suffered by both time series and cross-sectional analysis. The limited number of spatial units and

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the limited number of available data over time led data sets of these two techniques to violate basic assumption of standard statistical analysis. Most specifically, the small sample of conventional comparisons shows an imbalance between too many explanatory variables and too few cases. Consequently, within the contest of the small sample the total number of the potential explanatory variables exceeds the degree of freedom required to model the relationship between the dependent and independent variables. In contrast, thanks to pooled TSCS designs, we can greatly relax this restriction. This is because, within the pooled TSCS research, the cases are "country-year" (NT observations) starting from the country i in year t, then country i in year t+1 through country z in the last year of the period under investigation This allow us to test the impact of a large number of predictors of the level and change in the dependent variable within the framework of a multivariate analysis (Schmidt 1997, 156).

Second, pooled models have gained popularity because they permit to inquiry into "variables" that elude study in simple cross-sectional or time-series. This is because their variability is negligible, or not existent, across either time or space. In practice, many characteristics of national systems (or institutions) tend to be temporally invariant. Therefore, regression analysis of pooled data combining space and time may rely upon higher variability of data in respect to a simple time series or cross-section design research (Hicks 1994, 170-71).

A third reason to support pooled TSCS analysis concerns the possibility to capture not only the variation of what emerges through time or space, but the variation of these two dimensions simultaneously. This is because, instead of testing a cross-section model for all countries at one point in time or testing a time series model for one country using time series data, a pooled model is tested for all countries through time (Pennings, Keman e Kleinnijenhuis 1999, 172).

Given these advantages, in the last decade pooled analysis has became central in quantitative studies of comparative political economy. Several authors have utilized pooled models to answer to classical questions of this discipline. An accumulating body of research has used this statistical technique to test the main hypothesis concerning the political and institutional determinants of macroeconomic policies and performances (Alvarez, Garrett, Lange 1991; Hicks 1991; Swank 1992). Most specifically, regarding the study of public policy, we can cite empirical works on political and socio-economic causes of the welfare state development (Pampel and Williamson 1989; Huber Ragin and Stephen 1993; Schmidt 1997). Regarding research on both economic policies and

performances, researchers have tried to verify and characterize a macro-economic partisan strategy. In particular, they have shown that, once in office, different parties attempt to manage the economic cycle using the standard fiscal and monetary instruments. However, these same studies have discovered that the ability of parties to pursue their most preferred macroeconomic strategies depends on institutional structures of the domestic labor market (Comptson 1997; Oatley 1998), and increasingly internationalized markets (Garrett 1998; Garrett and Mitchell 1999). Finally, several authors have utilized TSCS analysis to examine the impact of political and economic variables on the financial openness of domestic markets (Alesina et al. 1994; Quinn and Inclan 1997). Therefore, pooled TSCS analysis is an inalienable instrument for the development of the comparative political economy. However, the popularity of this statistical technique does not depend only on its application in substantive research, but also recent papers discussing methodological issues that it implies (Stimson 1985; Hicks 1994; Beck and Katz 1995; 1996). In particular, this latter literature is more numerous now because pooled TSCS designs often violate the standard OLS assumptions about the error process.3 In fact, the OLS regression estimates, used by social scientists commonly to link potential causes and effects, are likely to be biased, inefficient and/or inconsistent when they are applied to pooled data.4 This is because the errors for regression equations estimated from pooled data using OLS procedure and pooled data tend to generate five complications (Hicks 1994, 171-72).

First, errors tend to be no independent from a period to the next. In other terms, they might be serially correlated, such that errors in country i at time t are correlated with errors in country i at time t+1. This is because observations and traits that characterize them tend to be interdependent across time. For example, temporally successive values of many national traits (i.e., population size) tend not to be independent over time.

Second, the errors tend to be correlated across nations. They might be contemporaneously correlated, such that errors in country i at time t are correlated with errors in country j at time t. As Hicks (1994, 174) notes, we could not expect errors in the statistical model for Sweden to lack some resemblance to those for the Norway or errors for Canada and the United States to be altogether independent. Instead, we would expect disturbances for such nations to be cross-sectionally correlated. In this way, errors in Scandinavian economies may be linked together but remain independent with errors of North American countries.

Third, errors tend to be heteroschesdastic, such that they may have differing variances across ranges or sub sets of nations. In other words, nations with higher values on variables tend to have less restricted and, hence, higher variances on them. For example, the United Stated tends to have more volatile as well as higher unemployment rates than the Switzerland. This means that the variance in employment rates will tend to be greater for bigger nations with large heterogeneous labor forces than for small, homogeneous nations (Hicks 1994, 172). Moreover, errors of a TSCS analysis may show heteroschesdasticity because the scale of the dependent variable, such as the level of government spending, may differ between countries (Beck and Katz 1995, 636).

Fourth, errors may contain both temporal and cross-sectional components reflecting cross-sectional effects and temporal effects. Errors tend to conceal unit and period effects. In other words, even if we start with data that were homoschedastic and not auto-correlated, we risk producing a regression with observed heteroschestastic and auto-correlated errors. This is because heteroschedasticy and auto-correlation we observe is a function also of model misspecification. The misspecification, that is peculiar of pooled data, is the assumption of homogeneity of level of dependent variable across units and time periods. In particular, if we assume that units and time periods are homogeneous in the level (as OLS estimation requires) and they are not, then least squares estimators will be a compromise, unlikely to be a good predictor of the time periods and the cross-sectional units, and the apparent level of heteroschedasticity and auto-correlation will be substantially inflated (Stimson 1985, 919).

Fifth, errors might be nonrandom across spatial and/or temporal units because parameters are heterogeneous across subsets of units. In other words, since processes linking dependent and independent variables tend to vary across subsets of nations or/and period, errors tend to reflect some causal heterogeneity across space, time, or both (Hicks 1994, 172). Therefore, this complication, like the previous one, could be interpreted as a function of misspecification. If we estimate constant-coefficients models, we cannot capture the causal heterogeneity across time and space.[72]

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[72] F. Podestà, "Recent Developments in Quantitative Comparative Methodology: The Case of Pooled Time Series Cross-Section Analysis." *DSS Papers SOC* (2002).

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Podesta goes on to discuss additional modeling techniques through which one can mitigate these five problems related to the distribution of error, particularly suggestions made by Beck and Katz in a series of influential papers. Indeed, in so far as researchers are concerned about the adequacy of their PCSTS models, they tend to focus only on identifying and correcting problems of error modeling. However, the inadequacy of PCSTS models for studying many problems in political economy has to do less with error modeling and more to do with underlying assumptions about the identity and stability of the data generating structure. Researchers unfortunately focus on trying to correct for things statistically that are really problems of structure and theory, which cannot be mechanically solved through corrections within a given model.

On this point, one of the most eminent theorists of PCSTS models, Nathaniel Beck, agrees. In a recent issue of Political Analysis devoted to PCSTS modeling, Beck led off with a provocative introduction, entitled "From Statistical Nuisances to Serious Modeling: Changing How We Think About the Analysis of Time-Series–Cross-Section Data." His basic claim was that social scientists need to think more carefully about fundamental issues of structure, rather than tangential issues of error correction.

Beck begins by noting that the use of pooled cross section time series models is widespread and increasing: "The use of TSCS data in political science, and particularly comparative politics and international relations, has become increasingly common.

Adolph, Butler, and Wilson (2005) estimate that about 5% of all political science articles (on JSTOR) published during the last decade used TSCS or related designs."[73]

His account of the reasons that researchers became interested in this approach and of its characteristic limits is similar to Podesta's:

> TSCS data became popular, particularly in political economy, because the initial complicated regressions on 15 or 20 observations were bound to be uninformative. These regressions were very sensitive to inclusion or exclusion of one particular country, or other seemingly arbitrary choices. Political economy scholars naturally gravitated toward TSCS designs that seemed to make it possible to move from only 15 or 20 data points to 20 or 30 times more than that. Fortunately, at the same time, the seminal piece by Stimson (1985) made clear the complications of such data, and the need to take those complications seriously.[74]

However, Beck suggests that many researchers mistakenly took marginal lessons about possible complications for the main lesson. Such researchers focus on "statistical nuisances" of error modeling rather than deeper questions about whether models are adequate for investigating data with complicated structural origins:

> Unfortunately most users of TSCS data worried mostly about violations of the Gauss-Markov assumptions (nuisances), rather than interesting features of the data that cried out for modeling. When Katz and I (Beck and Katz 1995, 1996) first published our work, we were reacting to some fixes for violations of the Gauss-Markov assumptions that had quite poor properties for the types of data being analyzed. We attempted to provide a simple methodology that would allow for some technical issues to be easily handled. One reason we wanted a simple method was so that political scientists could pay attention to the important issues of model specification that required the insights of political scientists and not statisticians or econometricians. This is not to say that the technical issues dealt with by statisticians and econometricians

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[73] Beck, Nathaniel. "From Statistical Nuisances to Serious Modeling: Changing How We Think About the Analysis of Time-Series–Cross-Section Data." *Political Analysis* 15:2 (2002) 97-100.

[74] Ibid.

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are unimportant, but they should not be dealt with at the expense of ignoring more substantive issues raised by TSCS designs.[75]

The more substantive issues are precisely those issues of underlying structure I raised above.

Perhaps the most typical kinds of development studies pursued with PCSTS models are those that try to identify correlates of good outcomes, like economic growth or democratization. Researchers look for "growth factors" – such as economic sector investments, government policies, trade relations, etc – that show a significant statistical correlation with economic growth. Development organizations then try to promote these factors. Such studies and their findings have played a particularly important role in negotiations with poor countries regarding the policies they are required to implement, either as a condition or object of aid.

The problems with this approach – compounded by all the concerns expressed thus far about the limits of statistical analysis in contexts of diversity, change, and strategic action – are explained with exquisite clarity in a recent working paper by Harvard Economists Dani Rodrik. The conclusion of the article is summarized well by its title, "Why We Learn Nothing from Regressing Economic Growth on Policies." Again, this is a passage worth quoting at length because it shows how deeply problems of structure, strategy, and model endogeneity plague statistical investigations meant to advise policies in and towards the third world:

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[75] Ibid.

Government use policy to achieve certain outcomes. Sometimes the desired ends are worthwhile, as is the case when policy is targeted on removing market failures. At other times, they are pernicious, as in the case when policies aim to create and distribute rents. Cross-country regressions have been the tool of choice to date in assessing the effectiveness of policies and the empirical relevance of these two diametrically opposite views of government behavior. This paper argues that such regressions are uninformative about the questions that motivate the analysis. The standard growth regression in which economic growth (or any other performance indicator) is regressed on policy tells us nothing about the effectiveness of policy and whether government motives are good or bad.

There is a voluminous empirical literature which attempts to estimate the effects of economic policy on growth. The typical cross-country growth regression takes the form

$$g_i = \alpha \ln y_{i0} + Z'_i \beta + \gamma s_i + \epsilon_i$$

where s_i is a policy variable for country i, y_{i0} is initial income and Z_i is a vector of other covariates. Such growth regressions are sometimes specified in panel form, with growth and all left-hand side variables averaged over 5- or 10-year sub periods. The object of the exercise is to obtain an estimate of, the impact of policy intervention on growth. Regressions of this type are ubiquitous in academic research, as well as in policy work carried out by development agencies, where they are used to predict the effect of policy reforms.

The list of economic policies that have been included in cross-national regressions includes:
- fiscal policy (Easterly and Rebelo 1993)
- government consumption (Barro 1991)
- inflation (Fischer 1993)
- black market premia on foreign exchange (Sachs and Warner 1995)
- overvaluation of the exchange rate (Dollar 1992)
- financial liberalization (Eichengreen 2002)
- trade policy (Lee 1993)
- state ownership in industry or banking (La Porta, Lopez-de-Silanes, and Shleifer 2002)
- industrial policy (Ades and di Tella 1997)

While economic growth is the most frequently used measure of economic performance, sometimes other performance indicators such as productivity and investment are used as the dependent variable. Djankov et al. (2002)

regress a variety of public goods (ranging from health outcomes to product quality standards) on regulations that restrict firm entry. Similar regressions are run also across industries or states/regions, regressing a performance variable on policies that apply at the relevant level. Besley and Burgess (2002), for example, analyze the impact of labor regulations on differential growth rates across Indian states.

As the empirical growth literature has grown, so has the critical evaluation of it. There is by now a wide-ranging discussion of the shortcomings of growth regressions, which focuses on problems relating to:
- parameter heterogeneity
- outliers
- omitted variables
- model uncertainty
- measurement error
- endogeneity

Temple (1999), Durlauf, Jonhson, and Temple (2004), and Easterly (2004) provide very useful recent critical surveys of the empirical growth literature. A dominant concern has been the lack of robustness. Levine and Renelt (1992) documented a while back that growth regressions are generally quite non-robust to variations in the set of conditioning variables. Sala-i-Martin, Doppelhofer, and Miller (2004) have tried to deal with this problem by Bayesian averaging of OLS estimates, to see which of the standard regressors are robustly correlated with growth. Easterly (2004) emphasizes that the large policy effects uncovered in growth regressions are typically driven by outliers, which represent instances of extremely "bad" policies.

The question analyzed here is how to interpret the estimated coefficients from such regressions when policies are not random but are used systematically by governments to achieve certain ends, whether good or bad. So the focus is on the endogeneity of the policy variables inserted on the right-hand side of the regression. Endogeneity problems are of course nothing new in growth regressions. But what is special here is that policy endogeneity is not just an econometric nuisance, but typically an integral part of the null hypothesis that is being tested. The supposition that governments are trying to achieve some economic or political objective is at the core of the theoretical framework that is subjected to empirical tests. In such a setting, treating policy as if it were exogenous or random is problematic not just from an econometric standpoint, but also conceptually.

My point is best made in the context of a specific application. Consider as an illustrative example an article by La Porta, Lopez-de-Silanes, and Shleifer (2002) in which the authors analyze the consequences of government ownership of banks around the world. The authors begin the article by distinguishing two perspectives on the role of government banks. The first perspective is a "developmental" one, which they attribute to Alexander Gerschenkron. In this view, latecomers resort to state-ownership of the financial system to overcome market imperfections, mobilize resources, and catch up with advanced countries. The second perspective is a "political" one, in which government ownership allows politicians to transfer incomes to favored groups in return for their support. To discriminate between the two stories, La Porta et al. regress per-capita GDP and productivity growth on their measure of government ownership of banks (along with other standard regressors). This exercise reveals a robust negative relationship between government ownership and economic performance. The authors interpret this result as supportive of the political view, and inconsistent with the developmental view.

But there is a problem here. The cross-national variation we observe in government ownership is unlikely to be random by the very logic of the theories that are tested. Under the developmental perspective, this variation will be driven by the magnitude of the financial market failures that need to be addressed and the governments capacity to do so effectively. Under the political motive, the variation will be generated by the degree of "honesty" or "corruption" of political leaders. I show in this paper that the cross-national association between performance and policy will have a very different interpretation depending on which of these fundamental drivers dominate. Unfortunately, none of these drivers is likely to be observable to the analyst. In such a setting the estimated coefficient on state ownership is not informative about either the positive or the normative questions at stake. It cannot help us distinguish between the developmental and political views, because the estimated coefficient on government ownership will be negative in both cases. The intuition is straightforward: a government that cares about social welfare (and nothing else) will increase its policy intervention in response to larger market failures, but not so much as to completely insulate economic performance from their adverse consequence. A negative correlation between government ownership and growth might as well be taken as confirmation that governments are acting socially optimally! And under no circumstances can it tell us whether societies would be better or worse off

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if government ownership were legislated away (or, for that matter, made mandatory).[76]

It should come as no surprise that many of the best social scientists working on questions of economic development expend a large amount of their time re-examining and criticizing questionable, but influential, empirical studies. William Easterly's article "Can Foreign Aid Buy Growth?" is a good example of this dynamic at work. Easterly begins by noting the extraordinary impact that a paper by Burnside and Dollar (2000) had on policymakers in aid organizations. Entitled "Aid, Policies, and Growth" and published in the *American Economic Review* the paper purported to show that aid promotes growth if a country has good policies.[77] Easterly summarized the nature and impact of the paper's claims as follows:

> The authors set out to investigate the relationship between foreign aid, economic policy and growth of per capita GDP using a new database on foreign aid that had just been developed by the World Bank. They run a number of regressions in which the dependent variable of growth rates in developing countries depends on initial per capita national income, an index that measures institutional and policy distortions, foreign aid and then aid interacted with policies. To avoid the problems that aid and growth may be correlated over periods of a few years, but not on a year-to-year basis, they divide their sample into six four-year time periods running from 1970–1973 to 1990–1993. In certain specifications, they also include variables for ethnic fractionalization, whether assassinations occurred, dummy variables for certain regions and even a measure of arms imports. In many of their specifications, they found the interaction term between foreign aid and good policy to be significantly positive, and they summarized (p. 847): 'We find that aid has a positive impact on growth in

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[76] See Dani Rodrik. The conclusion of the article is summarized well by its title, "Why we Learn Nothing from Regressing Economic Growth on Policies." *Harvard University* (2005).

[77] C. Burnside and D. Dollar, "Aid, Policies, and Growth." *American Economic Review* 90, no. 4 (2000), 847-68. I am indebted to Kevin Grier for bringing this paper to my attention.

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developing countries with good fiscal, monetary, and trade policies but has little effect in the presence of poor policies.'

I believe the Burnside and Dollar (2000) paper meets high academic standards and is intuitively plausible. Their conclusions are appropriately hedged, and the paper has become a healthy stimulus to further research. However, their paper also was the basis of a policy recommendation to increase foreign aid, if only other policies were good, without further testing of whether this result holds when expanding the dataset or using alternative definitions of "aid," "policies" and "growth." Their general finding was passed on from one media report to another and was cited by international agencies advocating an increase in foreign aid.[78]

Easterly documents the widespread impact the paper had in policy debates and decisions by development organizations. The paper was cited by the British Department for International Development, the Canadian International Development Agency, the World Bank, the Economist, the New Yorker, the Financial Times, and the White House, often as a direct justification for new aid/investment initiatives.

However, Easterly proceeds to argue that the results of Burnside and Dollar do not hold up to closer examination. He conducts a series of what economists call robustness checks – examinations of small departures from the initial model and data – that strongly suggest that the Burnside and Dollar results are not robust. Slight (but very plausible) changes in the way one defines "aid," "policies," and "growth" make the results go away. Furthermore, Easterly and colleagues were able to retest the original model on additional/higher quality data. The outcome was likewise negative:

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[78] Easterly, William. "Can Foreign Aid Buy Growth" *Journal of Economic Perspectives,* Volume 17, Number 3. (Summer 2003) 23–48.

Easterly, Levine and Roodman (2003) use the exact same specification as Burnside and Dollar (2000), but simply added more data that had become available since their study was performed, as well as hunting for more data in their original sample period of 1970–1993. (We were able to find more data even over their sample period by going to the original sources—for example, on institutional quality—rather than secondary sources.) Using a sample covering 1970–1997, we carried out their same regression with four-year averages with the same control variables including terms for aid/GDP, their policy index (a weighted average of budget deficits/GDP, inflation and an index of openness to trade) and the interaction between aid/GDP and the policy index. We found that the coefficient on the crucial interaction term between aid and policy was insignificant in the expanded sample including new data, indicating no support for the conclusion that 'aid works in a good policy environment'.[79]

As Easterly notes, aid can do a great deal of good in certain circumstances:

…in some cases foreign aid has been strikingly successful. For example, the World Bank's $70 million loan to the Ceara state government in the Brazilian northeast concluded in June 2001. The loan facilitated innovative government-led initiatives in land reform, rural electrification and water supply and a fall in infant mortality. There are countrywide success stories like Uganda, with heavy involvement by the World Bank and other aid agencies. Earlier success stories associated with aid included South Korea and Taiwan. There are also sectoral success stories, like the elimination of smallpox, the near elimination of river blindness, family planning and the general rise in life expectancy and fall in infant mortality, in which foreign assistance played some role.[80]

However, generalized statistical models often add little by the way of special insights to the challenge of identifying these unique opportunities for good. More basic, theoretical considerations of the nature of the problem and the interests of those involved, as well as concrete measures of accountability and real time evaluation can, in Easterly's view, go a lot further in crafting sensible development assistance. Of course, collecting

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[79] Ibid., 27.

[80] Ibid., 36.

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and analyzing statistics can always be useful, post-hoc, in evaluating past programs. At the end of the day, however, statistical models are unlikely to reveal some special technique for "buying growth" or other desirable outcomes. Researchers interested in development will have to deal with the complex set of beliefs, interests, and constraints found amongst peoples in the third world; and understanding these in their specificity and diversity may require knowledge that comes from interacting with these people, rather than snapshots drawn from aggregate statistical data.

The prominent criticisms of people like Rodrik and Easterly help to illustrate and to confirm my claims about the perils of employing statistical analysis to engineer better outcomes in the developing world. The inability of statistical knowledge to provide a technology of social change in the third world in turn helps to prove my larger point about the systematic limits of statistical inference for social analysis. *Pace* King, Keohane, Verba, and Powell, not all social phenomena are well suited to statistical understanding and mastery. The severe challenges that confront statistical modeling in the developing world are but instances of the general challenges that structural change, strategic action, social diversity and particularity can pose to statistic analysis in developed contexts as well.

3.4 Do Red Lights Make Cars Stop?

It is helpful to conclude with a hypothetical problem that, at some level of analogy, serves as a useful illustration of the challenges of using statistics and further

suggests how we might go about addressing those things that statistics can't. Suppose that an alien were to come to earth disguised as a human and take up the job of a traffic engineer in a large American city, where he is tasked with reducing accidents and improving traffic flows. The alien, although naïve, has a basic knowledge of human civilization and realizes he could accomplish this goal if there were a way to systematically stop cars at certain places for given intervals. The alien-engineer begins a search for such a technology. Remember, he is naïve, but he notices that there are lights at particular intersections that seem to influence traffic. Upon closer examination he hypothesizes that red lights make cars stop. He then conducts a large statistical study and finds that indeed red lights are highly correlated with quickly stopped cars. On the basis of this knowledge he could successful engineer better traffic outcomes in areas of the city plagued by collisions and gridlock. The conclusions of the statistical study would be of great pragmatic value.

Granted, the engineer would confront some puzzling cases of this technology failing. Further investigation would reveal that red lights seem to lose their power over some cars late at night when no one else is on the road, and they also consistently fail to stop cars carrying pregnant women in labor to the hospital and cars carrying bank robbers fleeing the police. Experimentation with red light cameras, which document whether cars stop and send the information to police, suggests that that cameras and lights together increase the stop rate late at night. However, even in combination they still have little effect on pregnant women and fleeing criminals. But generally and for the most part the

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red light technology would work and would be a useful tool for addressing most traffic problems.

Suppose that the successful traffic engineer then joins an international development organization and is tasked with solving the serious traffic problems of two cities in the third world. First he goes to Guatemala and, after learning which intersections in Guatemala City have the highest collision rates, he sets up traffic lights, just like in the US. The outcome, however, is disappointing. Collisions continue at a modest rate. Further studies reveal that red lights make cars stop less than half the time in Guatemala. The results are even worse in Rwanda. Although there are fewer cars, there is a high collision rate, and the red lights have almost no effect on cars at all. Puzzled, the engineer returns to the US to commission another study of the correlates of car stopping, this time with more control variables.

This hypothetical story, although fanciful, illustrates an important point. *Social phenomena are, at their root, constituted by human actions, and human actions are characteristically intentional.* By the term "intentional" I simply mean to indicate that humans act on the basis of reasons, motives, perceptions, beliefs, desires, and so on. Many of the psychological motors of human action are fundamentally conceptual and thus historically and culturally contingent. Therefore, the reasons/ desires/motives/ calculations that inform human actions can vary quite a bit. Stability in social patterns depends on stability in these concepts, judgments, constraints, and calculations that guide human actions. Conversely, by transforming people's concepts, beliefs, desires, constraints, or ways of reasoning one can affect significant changes in social patterns.

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The key to the lesson of the story above is the obvious fact that red lights do not make cars stop. At least, they don't make cars stop through material causality in the same way that sand in a gas tank makes a car stop. Red lights only make cars stop because cars are controlled by humans and red lights have a particular meaning that is grasped as a sufficient reason for action. In order for red lights to make cars stop, the lights first have to be grasped as something meaningful. Someone who has never seen or heard of a traffic light before would not know what, if anything, a red light signals – they are not objects to which we have "innate" reactions. Responding to red lights is something that has to be learned. Once it is learned it can become second nature, a matter of habit.

However, in order for the perception of a red light and its meaning to result in action it must provide sufficient reason for action. This is particularly true at "the beginning." Eventually, as these reactions become matters of unconscious, second nature this reasoning may be implicit rather than explicit. But this is an action that is ultimately responsive to reasons. The reasoning that informs one's reactions to red lights will involve considerations of how one's ends are served by so reacting. Once the purpose and social meaning of traffic lights are grasped, there are various considerations that might influence one's reasoning about the right response. At the most basic level, people will likely be driven by considerations of self preservation to obey traffic lights because they help avoid costly and deadly collisions. In periods of high traffic this rationale will generally be compelling (although for this technology of red lights to work well, other drivers must understand their meaning and logic as well). In period of low traffic, when collisions are less likely, this rationale for not running lights is less compelling on its

own. However, if a law is passed that imposes penalties on light running and a creates a system of surveillance and enforcement, a self-interested evaluation of the costs and benefits of running lights will tip back in favor of obeying them for most people. Here the "costs" are an artifact of explicit human design, but ones that can similarly motivate behavior. There are then additional reasons that may motivate a person to obey traffic signals beyond what we commonly identify as considerations of self interest.

The fact that red lights have the good social purpose they do and have been promulgated as law by a legitimate political authority might lead people to conclude that obeying red lights is the morally right and proper thing to do. One ought to stop for a red light at an empty intersection in the middle of the night, even if there is no chance of being caught. An ethical valence, broadly conceived or defined, can lead people to follow the rule – it simply *is* the right thing to do. Even this judgment, however, might admit of qualifications. Perhaps stopping is the right thing to do, except in emergencies. Ambulances are rightly allowed to run red lights and those with pregnant women in labor on the way to the hospital should be permitted to run them too, if the intersections are clear enough.

The aggregate patterns of red light behavior in a society will be driven by the understanding people have of red lights and people's judgments about good reasons for obeying them. These understandings and judgments can quickly become entrenched as unconscious habits and result in stable patterns, but it is important to see that such patterns are open change under certain conditions. Also, unless the same knowledge and

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judgments are shared by other societies, there is no reason to believe the same patterns would hold in those societies.

If we were interested in increasing compliance with red lights in a society there are a number of things we might do. We could try to educate people about the usefulness of red lights and the risks of grave bodily harm involved in running them. We could raise the costs associated with running them by increasing fines and surveillance. We could try to persuade people about the rightness and goodness of obeying traffic laws – and this could include attempts to circumscribe and closely define legitimate exceptions. Finally, there might be legitimately useful social science research to be done that could provide insights into why people sometimes don't notice and respond to traffic signals.[81]

Although this red light example is conveniently simplistic, I would like to suggest that it can serve as a useful analogy for a wide range of social phenomena. The patterns of such phenomena are constituted and held in place by common understandings of the meaning of things, beliefs about costs and benefits, judgments about what is good, right, and desirable, and raw habit. This mixture of norms, habits, and various judgments of self interest is what ultimately provides social patterns with their structure. It is to these, then, we must turn to understand and deal with social problems not handled well by statistical analysis.

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[81] See, for example, John Staddon's article "Distracting Miss Daisy: why stop signs and speed limits endanger Americans" in *The Atlantic* (July/August 2008), in which he suggests that too many traffic directives at a single intersection can overwhelm our perceptual/cognitive capacities, making it more likely some people will miss the most important cues.

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3.5 Methodological Individualism and the Turn Towards Microfoundations and Rational Choice

This strategy of understanding and explaining aggregate social phenomena in terms of the beliefs and judgments of the individual agents involved is often described as "methodological individualism" – a term first coined by Joseph Schumpeter. Methodological individualism does not entail a complete rejection of statistical analysis, but this perspective advises researchers to support their models with explicit "microfoundations," – that is, accounts of the logic underlying the individual decisions that produce aggregate phenomena. The turn towards microfoundations and theories of individual behavior as a basis for more promising statistical work lies at the heart of a recent movement in Political Science focusing on the "empirical implications of theoretical models" (EITM), which has been heavily supported by the National Science Foundation.

A major proponent of this movement, Chris Achen, provides a wonderful summary statement of the problems with the current state of statistical modeling in the social sciences, in which he also illustrates why meaningful research generally depends on a careful examination of microfoundations:

> Empirical work, the way too many political scientists do it, is indeed relatively easy. Gather the data, run the regression/MLE with the usual linear list of control variables, report the significance tests, and announce that one's pet variable "passed." This dreary hypothesis-testing framework is sometimes seized upon by beginners. Being purely mechanical, it saves a great deal of thinking and anxiety, and cannot help being popular. But obviously, it has to go. Our best empirical generalizations do not derive from that kind of work. How to stop it? The key point is that no one can

know whether regressions and MLEs actually fit the data when there are more than two or three independent variables. These high-dimensional explanatory spaces will wrap themselves around any dataset, typically by distorting what is going on. They find the crudest of correlations, of course: Education increases support for liberal abortion laws, for example. In the behavioral tradition, that counts as a reliable finding. But no one knows why education is associated with that moral position (higher intellect discovering the truth? Mindless adoption of elite tribal norms? Coincidence due to correlation with something else entirely?), and that leaves open the possibility that abortion attitudes do not work the way our simple linear statistical models assume that they do.

Are educated Protestant evangelicals more enthusiastic about relaxed abortion laws than less-educated members of their denominations, for example? In the political science literature, at least, almost no one knows; we have not published the relevant cross-tabulations, and so we know very little about interactions of that kind. Instead, we proceed as we have been trained, looking at the coefficients in large statistical models. Hence, we know only that when linear probit models have mushed their way helplessly through national samples with jumbles of Baptists, Quakers, agnostics, Mormons, Christian Scientists, Jews, Catholics, and Presbyterians—some black, some white, some Asian, and some Hispanic—then education acquires a positive coefficient in predicting liberalism concerning abortion. Whether these different groups of people have unique histories, respond to their own special circumstances, and obey distinctive causal patterns, we do not know because we do not check. In consequence, no real knowledge about the influence of education on abortion attitudes follows from the positive coefficient. Getting rid of this cheap sense of "empirical findings" is probably the central task that quantitative political science faces.[82]

For reasons explored above, Achen points out there is no basis for trusting findings generated by large, complicated statistical models unless those models have convincing microfoundations. In the absence of microfoundations, Achen argues that we are only likely to be able to substantiate the assumptions of very simple statistical

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[82] Achen, C. H. "Toward Theories of Data: The State of Political Methodology." *Political Science: The State of the Discipline* (1983) 443. An online version available at: http://qssi.psu.edu/files/Achen_ART.pdf

models, which leads him to make a radical proposal for a "simple rule, to be applied when no formal theory structures the investigation and we must rely on the art of data analysis." This is Achen's so called "Rule of Three" (ART): "A statistical specification with more than three explanatory variables is meaningless." As he explains, "ART may sound draconian, but in fact, it is no more than sound science. With more than three independent variables, no one can do the careful data analysis to ensure that the model specification is accurate and that the assumptions fit as well as the researcher claims."[83]

Achen's endorsement of parsimony for statistical models that lack underlying formal theory has a clear epistemological justification, but we should note that it is by no means a guarantee of good research. Without plausible microfoundations even the tightest of correlations may be hard to countenance, regardless of their simplicity. The warning that correlation does not necessarily imply causation is the most basic caveat of all statistical research. In many cases, simple statistical models with high correlations are of dubious value if one cannot identify a plausible underlying mechanism responsible for the causal structure of the correlation. In a complex world, correlations can be mere artifact of chance.

Consider the case of the Washington Redskins and US presidential elections. For 64 years and 17 presidential elections whether the Redskins won their last home football game correctly predicted the winner of the US presidential race.[84] If the Redskins won

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[83] Ibid., 446.

[84] For a nice overview see: Paula Duffy "Will the Redskins regain status as predictor of the presidential election outcome?" http://www.examiner.com/x-426-Sports-Examiner~y2008m10d9-Will-Redskins-

their final home season game the incumbent party retained control of the White House. If they lost, the incumbent party also lost. This trend goes all the way back to the origins of the Redskins franchise in 1936, when their win over the Chicago Cardinals predicted the reelection of Franklin Delano Roosevelt. The prediction held perfect throughout the rest of the century, even correctly predicting the 2000 election, in which Gore received the most popular votes but Bush won on electoral votes. The Redskins lost their last home game to the Tennessee Titans that year, predicting that the Democrats would lose the White House. As far as statistical associations go, this perfect correlation over 64 years was extraordinary, and certainly much stronger than most statistical results in social science. But was there any plausible reason to believe it was due to anything other than chance? Ah, but the statisticians might point out, the probability of this result occurring by mere chance is less than .000763% ![85] But this, of course, depends on how one looks at it. The odds of a particular team's record predicting the electoral outcome is indeed low, however the odds that some sports team in America would have a record that predicts the outcome is modest. Indeed, if one considers the thousands of possible teams, from football, basketball, baseball, soccer, hockey, etc. at the college and professional

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regain-status-as-predictor-of-presidential-election-outcome and
http://www.snopes.com/politics/ballot/redskins.asp

[85] If coincidence of the Redskin's outcome and the electoral outcome were complete chance there is a 50% probability we would witness it in any given election. However, the probability that it would obtain in 17 consecutive elections just by chance is .5^17 – very small indeed.

level that one might search for such a correlation, the likelihood of finding some association is actually high.

Alas, the trend fell apart in 2004 with the triumph of Bush over Kerry. The Redskins had lost to the Green Bay Packers that year predicting a Republican loss. However, the prediction held true again in 2010 when Obama beat McCain after the Redskin's final home defeat at the hands of the Pittsburg Steelers. Although the aberration in 2004 destroyed the perfect correlation, a logit or probit model of this relationship would still show an extraordinarily significant relationship, much more than the average social science result that qualifies for publication in leading journals. Ultimately, however, the inability to suggest any plausible underlying mechanism to explain this relationship disqualifies it as a reliable indicator of presidential outcomes. More generally, without plausible microfoundations it is hard to have much faith in any statistical result.

What counts as plausible microfoundations or stable results will still be open to dispute. Consider the abstract of a recent paper published in the *Journal of Finance*:

> This paper investigates the stock market reaction to sudden changes in investor mood. Motivated by psychological evidence of a strong link between soccer outcomes and mood, we use international soccer results as our primary mood variable. We find a significant market decline after soccer losses. For example, a loss in the World Cup elimination stage leads to a next-day abnormal stock return of −49 basis points. This loss effect is stronger in small stocks and in more important games, and is robust to methodological changes. We also document a loss effect after international cricket, rugby, and basketball games.[86]

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[86] http://www.afajof.org/afa/forthcoming/3096.pdf

I leave it to the wise reader to judge whether this mechanism sound plausible and these findings reliable.[87]

The conclusion we arrive at, that either explicit or plausible microfoundations are important for believing the results of most statistical models, is but a subspecies of my larger claims about the importance of structure. The structure of most social phenomena ultimately derives from human actions, which are characteristically shaped by intentional/conceptual features of the human mind – and these are in turn historically and culturally contingent in many respects. The possible variations in human beliefs, concepts, goals, and perceptions indicates a fundamental source of structural social change; and such change, as we have seen, limits the utility of statistical analysis.

Specifying microfoundations for statistical models is a way of keeping track of structural changes liable to influence outcomes of interest. This is the basic proposal that results from the Lucas Critique. However, specifying microfoundations is easier said than done, and it can indicate a number of different things in practice. Researchers might outline a theory of individual behavior that results in a number of empirical implications that can be subject to statistical collaboration. If the empirical implications of a model can be sufficiently confirmed through statistics, this can provide reason to believe the model is on to something. Microfoundations can also refer to specific variables introduced into a regression because of their theoretical importance at the individual level. Most commonly, microfoundations refer to formal models of individual choice,

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[87] If so, you have a new tool for beating the market.

which may include individual utility functions (representations of what people value) and stylized mathematical representations of the tradeoffs between choice options. Building models of this sort lies at the heart of rational choice theory. But can the process of human reasoning really be captured by mathematical formalization?

This turns out to be a real challenge, and the problem of change plagues rational choice models too. If the ways that people reason are themselves fundamentally changing this will render most formal models of choice useless. Indeed the phenomenon of "preference change" is perhaps the greatest challenge to the utility of rational choice models. If all human activities could ultimately be attributed to a single, deep, underlying motive, then rational choice models might hope to achieve universal scope. In many models, the desire for money serves as the basic motive attributed to agents, and there are, of course, a lot of situations in which this motive is undoubtedly dominant. However, motives are ultimately diverse and they can change, which proves a problem for the reliability and portability of rational choice models. Also, the constraints people face and their perceptions of those constraints can change as well, further destabilizing rational choice models. Even strategic situations that are well defined in terms of the goals and constraints of those involved may admit of multiple equilibria, which diminish the predictive power of formal models. Finally, with regard to many social phenomena we may simply have no idea of the true range of considerations that enter into individual decisions, making it impossible to represent the mathematically. These are all challenges I examine in more depth in the next chapter, which deals with the promises and perils of rational choice modeling.

Rational choice models need not be statistical, but such models (or even plausible suggestions for such models) are often a prerequisite for good statistical work in the social sciences. People are not like the molecules of gas laws, inevitably moved to predictable patterns by nature of their unchanging properties and the influences of outside forces. Rather, people act "intentionally," which is to say on the basis of their desires, goals, beliefs, values, and perceptions – all of which are open to change in radical ways. In situations where we believe these are changing, be it through grand shifts in social imaginaries or the more ubiquitous impact of innovation and strategic exploitation, statistical models are likely to be of little use.

The methodological shortcomings of statistics alert us to the importance of considering the sources of social structure; that is, the importance of people's intentions, believes, goals, desires, strategies, etc. Clearly, these are matters that can concern "ethical convictions" and things that ethical persuasion can have a unique role in shaping. The turn towards rational choice formal modeling indicates a recognition of the importance of the intentional character of human action and, by extension, people's particular concepts, beliefs, aims, and judgments. Rational choice theory is an attempt to systematically understand these intentional features of human agency and their implications for social outcomes. Although rational choice theorists are right to draw our attention to importance of intentions, beliefs, goals, desires, perceptions, and so on, it is not clear that these can be scientifically understood and mastered as many rational choice modelers would like. The utility of rational choice formal modeling as well as its shortcomings and limits is the subject of the next chapter.

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4. Rational Choice Formal Modeling

"To get a good grip on what people are likely to do requires first approximating what they believe about the situation and what they want to get out of it. By estimating carefully people's wants and beliefs, anyone can make a reliable forecast of what each and every one of them will do. And if you can predict what will happen, then you can also predict what will happen if you alter what people believe about a situation. This is, in short, how we can use the same logic for both prediction and for engineering the future."

- Bruce Bueno de Mesquita, *The Predictioneer's Game*[1]

"The iterated Prisoner's Dilemma has become the E. coli of social psychology. Just as important as its use as an experimental test bed is the use of the Prisoner's Dilemma as the conceptual foundation for models of important social processes."

- Robert Axelrod, *The Evolution of Cooperation*[2]

" 'All people are insane,' he said. 'They will do anything at any time, and God help anybody who looks for reasons.' "

- Kurt Vonnegut, *Mother Night*[3]

4.1 Introduction to Rational Choice

The basic premise of rational choice (formal) modeling is simple and

uncontroversial. Human behavior is characteristically purposeful or intentional, which is

to say people act to achieve particular ends, fulfill particular desires, achieve particular

goods, and so on. Thus, we can generally understand, and even predict, what people will

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[1] Bruce Bueno De Mesquita, *The Predictioneer's Game: Using the Logic of Brazen Self Interest to See and Shape the Future* (New York: Random House, 2009) 3.

[2] Robert Axelrod, *The Evolution of Cooperation* (New York, NY: Basic Books, 1985) 28.

[3] Kurt Vonnegut, *Mother Night* (New York, NY: Random House, 1966) 115.

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do based on their goals and their beliefs about how to best achieve those goals. This basic means-ends framework of instrumental rationality is fundamental to our understanding of human agency. Indeed in order to make most human actions intelligible we must understand them as intentional attempts to achieve particular ends, given particular beliefs about how to best do so. Human actions that are not intelligible within such a framework are liable to appear as mysterious, or simply "irrational."

By explicitly identifying people's goals and beliefs and formalizing these in a mathematical framework rational choice models hope to explain and to predict behavior in ways that may not be immediately obvious. Mathematically, drawing conclusions from formal models is simply a matter of deduction; however, the deductive conclusions of formal models can often reveal unusual or unforeseen outcomes. Also, by illustrating how certain outcomes derive from a particular set of initial parameters (a given configuration of ends, beliefs, and constraints) formal models can identify ways of engineering different outcomes, by imposing different constraints or changing particular beliefs (and perhaps goals).

As James Johnson helpfully explains "'Rational Choice Theory' actually consists of a family of theories, usually but not always mathematical, that investigate the ways that actions taken by rational individual decision makers can interact in often surprising ways to generate stable aggregate outcomes."[4] Rational choice models are an exemplar of

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[4] From one of Jim's many course syllabi on the topic:
http://www.icpsr.umich.edu/icpsrweb/sumprog/syllabi/68308;jsessionid=42AA109E8E780B508296B1507927B7B9

"methodological individualism," focusing on the beliefs and goals of individual agents. However, in certain cases groups can also be included in rational choice analysis if they act with sufficient cohesion with regard to purposes, such as firms, states, or gangs sometime do.

In rational choice theory, the ends, goals, desires, and so forth that serve as motivating reasons for action are subsumed under the general term "preference." Whether and how one can represent preference mathematically has been a subject of considerable debate within the rational choice tradition. At the simplest level, a theory of preference presumes that an individual is able to make comparative judgments between two goods or states of affairs. Debreu and, later, Rader formalized this notion in terms of a binary relation R (with the informal meaning of "as least as good as") that a consumer can apply to every pair of goods (or states).[5] Given the two alternatives A and B, either A R B or B R A or both. If A R B but not B R A, then A is preferred strictly to B. If B R A but not A R B then B is preferred strictly to A. If both A R B and B R A, then the agent is indifferent between A and B. This "comparative" theory of preference arguably exhausts all possible attitudes that an agent can have towards two alternatives set before them.

However, there are additional conditions one could assume, which would make a comparative preference ordering serviceable for a global, mathematically tractable account of preference. First, is the assumption of transitivity. If an agent prefers A to B

[5] See Rader, Trout "The Existence of a Utility Function to Represent Preferences", 1963, *Rev Econ Stud.;* Debreu, "Representation of a Preference Ordering by a Numerical Function", 1954, in Thrall et al., editors, *Decision Processes*. and Debreu *: An axiomatic analysis of economic equilibrium*. (Yale University Press, 1959).

and B to C, then the principle of transitivity implies the agent prefers A to C. An important caveat is generally appended to the formulation of this principle, namely that the preference ordering should be complete. That is, an agent is able to rank all possible outcomes in a preference ordering (indifference between particular states is allowed). This is one way of ensuring that all possible considerations enter into the preference ordering and intransitivity does not artificially appear as an artifact of initially unaccounted for alternatives.

In certain cases there will exist a mathematical function (U) that can assign an index number to each possible outcome such that if A R B then $U(A) \geq U(B)$ (with corresponding other properties, e.g. if A R B and B R A then $U(A) = U(B)$). The technical conditions for the existence of this sort of function are specified by Rader[6]. This is a so called "utility function," which provides a way of mathematical formalizing and systematically representing preferences. Utility functions characterize the details of someone's preferences over a domain of possibilities. Such functions can be as simple as a discreet, ordered ranking of preferred outcomes (B preferred to A preferred to C), although such rankings could in principle extend to a near infinite set of possibilities. When preferences can be reduced to a singular metric and continuously characterized in reference to a domain of variables, utility functions can take on more elaborate

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[6] Rader, Trout "The Existence of a Utility Function to Represent Preferences", 1963, *Rev Econ Stud*

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specifications, such as Banks and Duggan's[7] rendering of voter preferences in their

"Dynamic Model of Democratic Elections in Multidimensional Policy Spaces:"

A policy strategy for type t individuals is now a Borel probability measure $\bar{\pi}_t$ on $\bar{X} = X \cup \{s\}$.[12] Given a profile $\bar{\pi} = (\bar{\pi}_1, \ldots, \bar{\pi}_{|T|})$, and assuming that all future representatives who do not shirk are re-elected, the continuation value of electing a challenger for a type t voter can be expressed as a function of $\bar{\pi}$ only:

$$v_t(\bar{\pi}) = \sum_{t' \in T} \gamma_{t'} \left[\bar{\pi}_{t'}(\{s\})[(1 - \delta)u_t(x_{t'}) + \delta v_t(\bar{\pi})] + \int_X u_t(x)\bar{\pi}_{t'}(dx) \right],$$

implying

$$v_t(\bar{\pi}) = \frac{\sum_{t' \in T} \gamma_{t'} \left[\bar{\pi}_{t'}(\{s\})(1 - \delta)u_t(x_{t'}) + \int_X u_t(x)\bar{\pi}_{t'}(dx) \right]}{1 - \delta \sum_{t' \in T} \gamma_{t'}\bar{\pi}_{t'}(\{s\})}.$$

(I will consider below whether such elaborate specifications of preference can indeed ground useful models.)

Utility functions provide a way of tracking an agent's evaluation of the comparative desirability of different scenarios and the logic underlying his or her decision making. By construction, an agent hopes to achieve that outcome that is ranked highest by their utility function (and rankings, as I've indicated, can be cardinal or ordinal). Having formalized preferences in functional form, it is simply a matter of calculus to find arrangements that maximize utility over a given domain of possibilities (for continuous functions; for discreet/ ordinal functions, the search will be comparative). In some cases, preferences may be as simple as a preference for maximizing a particular

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[7] Jeffrey S. Banks and John Duggan (2008) "A Dynamic Model of Democratic Elections in Multidimensional Policy Spaces", *Quarterly Journal of Political Science*: Vol. 3:No 3, pp 269-299.

variable, say wealth, in which case the preference ranking is given by the very magnitude of the monetary reward attached to an outcome.

Note that, if we cannot assume transitivity and completeness, a set of binary preferences may not admit of representation in a "functional" form, in the sense each element of the domain being matched with only one element in the co-domain. However, preferences can still be "represented" as the set of all binary preference relations.

There is, of course, considerable controversy concerning the nature of preferences and our ability to adequately represent them mathematically. Indeed, as I will show, the limits of rational choice analysis are intimately related to limits in its understanding of preferences.

Preferences provide the foundation of formal models. However, modeling also involves specifying the particular "constraints" that agents confront in trying to satisfy their preferences. Since constrains have to be subjectively perceived in order to exert their cognitive influence on choice, constraints are often subsumed under the general term "beliefs." Such constrains can include the institutions, or "rules of the game," that structure people's interactions with others (or with material features of the world), as well as the actions of other strategic agents and one's expectations about their likely actions.

Modeling the constraints imposed by static features of the world is generally straight-forward: Someone is going to hike the Appalachian trail, she would like to bring a sleeping bag, tent, water purification system, jerky, trail mix, snickers bars, and many other things. However, she can only carry so much weight, so she has to make choices about what to give up. The question of how she can best satisfy her preferences given the

weight constraint will involve a consideration of the tradeoffs in the way she values these particular items. If her valuations are captured with sufficient resolution by a utility function we can predict what she'll decide to pack given different weight constraints.

Modeling becomes more complicated when an agent faces constraints generated by another agent, who in turn is responding to constraints generated by the first agent. Situations of strategic interaction, where my best response depends on your response, which in turn depends on my response, are the subject of a subfield of formal modeling typically indicated by the umbrella term of "game theory." As Thomas Schelling explains:

> There are two definitions of game theory. There is a soft one and a hard one. According to the soft one, game theory is the study of how two or more entities – people, governments, organizations – make choices among actions in situations where the outcomes depend on the choices both or all of them make, where each has his or her or its own preferences among the possible outcomes – how they should (might) rationally make their interdependent choices. Each individual needs to anticipate the decisions the others are making. But that means that each needs to anticipate what the others are *anticipating*. And that means anticipating what the others anticipate oneself to be anticipating! This may sound like an infinite regress, but essentially it only means finding a set of expectations that are consistent with each other. Somehow a common expectation of the 'expectable' outcome must be recognized and acted on.
>
> There is another definition, the 'hard' one, that probably reflects, or until recently reflected, the interests of most game theorists, according to which '*Game theory can be defined as the study of mathematical models of conflict and cooperation between intelligent rational decision-makers*' (Myerson, 1991). (My 1975 American Heritage Dictionary, New College Edition, defines game theory as 'the mathematical analysis of abstract models of strategic competition . . .') The difference is two-fold: the emphasis on 'mathematics' or 'mathematical models', even the exclusivity

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of mathematics and mathematical models, and the emphasis on 'rational' decision.[8]

Many formal models dealing with social phenomena include some game theoretic components, for the simple reason that interactions between intelligent agents generally involve strategic considerations.

There is a great deal of controversy regarding the meaning of "rationality" in rational choice modeling, which I will explore at length later. For some, rationality simply implies consistency and completeness in preferences (that they be reflexive, transitive, and complete). This guards against the indeterminacy of preference cycles (which are likewise implausible, given that they make people vulnerable to easy exploitation). Others have a more substantial interpretation of rationality as something that requires particular types of preferences or entails that people make decisions according to a particular logic (say, Bayesian updating of expectations, or minimization of maximal expected loss). On the former view, concerning only the consistency of preference, rationality indicates the minimal assumptions necessary for generally making sense of and describing purposeful choice. On the latter view, rationality is a normative concept that shows how people ought to choose in order to be fully rational. The divide between the descriptive/explanatory view of rationality and the normative view can sometimes be ambiguous. However, most social scientists tend to embrace the

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[8] Thomas C. Schelling, "Game Theory: A Practitioner's Approach" *Economics and Philosophy*, 26 (2010) 27–46.

descriptive/explanatory account. They take their models to be an indication of how instrumental reason leads people to make particular choices, which in turn suggests why particular aggregate social outcomes result.

Rational choice models can be as simple as the 2x2 payoff matrix of a prisoner's dilemma or as complex as the voting model mentioned above (and possibly more complex). Although drawing conclusions from formal models is ultimately a matter of mathematical deduction, models can incorporate complex, probabilistic decision logics, and can suggest multiple, stable outcomes (multiple equilibria). Formal models can also provide the microfoundations for statistical models, which attempt to confirm the empirical implications suggested by the formal model.

Rational choice models have proved useful in various ways. These include:

1) Illustrating Feasibility Constraints – models can show how particular properties or outcomes we might desire are logically inconsistent with one another. This is the subject of much work done on voting theorems.

2) Illustrating Reasons for Paradoxical or Undesirable Outcomes – models can explain how the decisions of individuals can lead to outcomes that none of them ultimately prefer. Game theoretic models such as the prisoner's dilemma and public goods games illustrate the logic behind suboptimal decision "traps" and the difficulties inherent in various forms of collective action.

3) Identifying Effective Strategic Interventions – when models have adequately captured preferences and constraints, models suggest ways of intervening to change the expected outcome.

4) Mechanism Design – an adequate understanding of the decision logic(s) and incentives that guide agents can enable us to design "institutions" that have desirable properties, such as the satisfaction (or frustration) of particular individual preferences and the production of more desirable aggregate outcomes. Mechanism design pays particular attention to problems of incentives, credible commitment, and enforcement illuminated by rational choice theory. There are a number of examples of institutions whose success can be attributed to insights drawn rational choice models.

Examination of the practical successes of rational choice models, however, suggests that the utility of rational choice analysis is limited in various ways. Rational choice models tend to be useful in situations where 1) the motives of agents are clear and unlikely to change, 2) the satisfaction of preference is easily quantified, 3) the preferences in question are sufficiently strong/dominant, and 4) the rules/constraints confronting agents are well known and stable. In short, rational choice models are best suited for economic contexts. There are other social contexts that may share these features and thus likewise be fruitful ground for the application of ration choice models. However, there are many social phenomena that do not, and attempts to capture their dynamics though rational choice models can quickly become misleading or absurd.

The over-extension of rational choice models into contexts where preference and rules are dynamic and complex has led to a sustained set of controversies concerning the

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"pathologies" of rational choice analysis.[9] In reviewing some of these controversies, I want to highlight the problem of preference formation as the fundamental methodological hurdle to the universal extension of rational choice models. The sheer variety of possible "preferences," and the fact that an agent's preferences can radically change, poses a permanent problem for rational choice models, which are necessarily anchored upon a static notion of preference.

Of course, the dynamism of preferences might itself be something one seeks to model. In very limited cases this is perhaps be possible, but to do so with respect to all preferences would demand nothing less than a scientific understanding of the origins of all "values." In this sense, it would require explaining ethical convictions, ethical persuasion, and indeed all of human reasoning in absolute, scientific terms. The problem of preference formation accounts, in part, for the significant, current interest in biological research that hopes to reduce value judgments to biological determinants. However, in so far as the human intellect escapes explanation in terms of mechanistic biology and the best account we can give of the origins of preferences remains one rooted in ideas and concepts, the utility of rational choice models will be limited to contexts in which there are sufficiently stable preferences and constraints. As is the case with statistics, rational choice models are not equipped to deal with many forms of genuine change.

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[9] E.g. Donald Green and Ian Shapiro, *Pathologies in Rational Choice Theory: A Critique of Applications in Political Science* (New Haven: Yale University Press, 1994).

4.2 The Glories of Rational Choice

In particular contexts, for particular problems, rational choice models have demonstrated their usefulness. We should consider these success stories before turning to a deeper examination of the limits of rational choice models.

4.2.1 Feasibility

At the simplest level, formal modeling is nothing more than an attempt to express features of the world in mathematical form and, using the power gained by mathematical representation, to deduce logical implications in light of assumptions about the initial parameters. Mathematical formalization also makes it easier to detect logical inconsistencies in these assumptions. And, when particular constraints are specified, models can outline the range of possible (feasible) outcomes within those constraints. Questions of consistency and feasibility have been a central concern for researchers who examine the properties of different voting regimes.

One of the central purposes of voting is to ensure that individual preferences are reflected in collective decisions. There are many intuitive properties that we might think a voting regime should have. For example, if there is an outcome that all individuals prefer to another outcome we would hope that when people vote the former outcome will triumph over the latter (a version of what is sometimes called pareto efficiency). We might also think that people's preferences shouldn't change in response to the voting options (but rather preferences should be complete, transitive, and consistent). Finally, we might think it inappropriate if one person's preferences always determined the

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outcome (dictatorship). However, by formally representing these properties one can examine their implications in more detail. Arrow's impossibility theorem – one of the most famous results in formal models of voting – shows that under a particular interpretation of these properties they are inconsistent with one another.[10]

Although there are ways we might reasonably modify our understanding of these properties to circumvent the paradox that Arrow identifies, the social choice literature spawned by his results has raised a number of concerns about the consistency and feasible outcomes of different voting regimes. Long ago Condorcet showed that, under certain conditions, even if voters have individual preferences that are completely transitive (some prefer A>B>C, others B>C>A, and others C>A>B), pair-wise majoritarian voting can result in intransitive preferences (A>B>C>A), making this system of voting inconclusive. Following Arrow, Gibbard and Satterthwaite demonstrated that nearly all voting systems are susceptible to various form of strategic manipulation through the intentional misrepresentation of preferences or the ordering of alternatives.[11] That is to say, minorities can often achieve outcomes that a majority of voters dislike through strategic voting and/or agenda control.

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[10] For the most accessible proofs see W. Vickery, Utility, strategy, and social decision rules. *Quart. J. Econ.* 74 (1960), pp. 507–535; or Amartya K. Sen. Quasi-transitivity, rational choice, and collective decisions. *Rev. Econ. Stud.* 36 (1969), pp. 381–393.

[11] Mark A. Satterthwaite, "Strategy-proofness and Arrow's Conditions: Existence and Correspondence Theorems for Voting Procedures and Social Welfare Functions", *Journal of Economic Theory* 10 (April 1975): 187–217; Allan Gibbard "Manipulation of voting schemes: a general result," *Econometrica*, Vol. 41, No. 4 (1973): 587–601.

Although formal modeling does not prescribe any "ideal" voting system (indeed it suggests there are no perfect systems), modeling alerts us to the liabilities and virtues of different voting procedures. This knowledge can be helpful in a number of ways, from aiding in the design of voting regimes, to identifying effective strategies for change within a regime, to illustrating how one can guard against particular forms of instability and manipulation. These insights all flow from the power of mathematical formalization, which enables models to trace out the logical implications of preferences and constraints, which may otherwise go unrecognized and misunderstood.

4.2.2 The Logic of Undesirable Outcomes

Closely related to the demonstration of inconsistencies or feasible outcomes are insights that formal models grant into the underlying logic of undesirable outcomes. Rational choice models show how individuals, strategically acting on what are ostensibly their "best interests," can arrive at social outcomes that none of them prefer. The classic exemplar of this phenomenon is the so-called "prisoner's dilemma."

In the stylized telling, two partners in crime are arrested, separated by the police who do not have enough evidence for a serious conviction but do have enough evidence for a less serious conviction, and given the following options. If both prisoners confess to the serious crime they will both be given a modest prison sentence of, say, five years. If both prisoners refuse to confess, they will only receive a short prison sentence of one year for the lesser crime. However, if one prisoner refuses to confess and the other one confesses, the one who confesses will be granted complete amnesty for his cooperation

and allowed to go free, while the one who did no confess will be given a harsh, exemplary prison sentence of ten years.

The prisoners face a strategic dilemma, which can be visually represented in the following "payoff matrix":

Prisoner 2 -------->		don't confess	confess
Prisoner 1 -->	don't confess	1,1	0,10
	confess	10,0	5,5

The "best response" of each prisoner, conditional on the other prisoner's response is to confess. That is to say, whatever prisoner 2 does, prisoner 1 is better off confessing. Notice that if prisoner 2 *does* confess, prisoner 1 is better off confessing (he receives one rather than ten years of jail). And if prisoner 2 *does not* confess, prisoner 1 is better off confessing (he receives zero rather than five years of jail). The same logic applies for prisoner 2's decisions. He is always better off confessing as well, whatever prisoner 1 does.

However, the ultimate result is perplexing. Both prisoners end up with 5 year sentences even though there is another outcome *that they both prefer*, namely the scenario in which they both receive one year sentences. The problem is this requires that they both refuse to confess; but there are strategic reasons that neither can expect the other will choose this option, given that they are always better off confessing (if one person were to refuse to confess, the other could get off free by confessing). So, the way

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the incentives are set up, in the process of pursuing their own "interests," each person ends up in a situation that is inferior to another possible outcome.

Although the prisoner's dilemma is a highly stylized scenario, many social scientists have claimed that structurally similar problems – in which incentive configurations lead to "suboptimal outcomes" – pervade the real world. Perhaps a more realistic model of a situation that people actually face is the so-called public goods problem. In the stylized "game" version each participant begins with an initial sum of money (say, $20) that she can either keep or invest in a communal resource. Money invested in the communal resource is multiplied by some factor (say, doubled) and divided out equally to the participants, which is meant to indicate the genuine, common benefits that accrue to having public goods commonly available. If everyone invests all their money, the result is that everyone's money is doubled – (each participant would receive $40) a nice outcome all around. However, a person could receive even more money if she kept her initial money ($20) to herself and just "free-loaded" off the public contributions of others. If there were four participants, three of whom invested everything ($20x3=$60), each player would still receive an equal portion of the doubled communal pot ($60x2=$120 / 4=$30) – not a bad gain.

However, in this scenario, the free rider would also still have her initial money ($20), making her total take higher than everyone else's ($20+$30= $50, which is greater than the $30 that the other three participants receive). This logic of free riding, however, is evident to the other participants as well. Each of them would always personally receive more money by free riding and not contributing, regardless of what the other players do.

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And if one participant contributed money while everyone else free loaded, that contributor would end up losing half her investment ($20x2=$40 / 4= $10). Followed to its conclusion, this means that if participants are narrowly interested in maximizing their wealth no one will have reason to contribute to the communal resource, and no one's wealth will grow. Even though everyone could double their wealth if they all invested fully, the comparative incentive to free ride leads "rational, strategic, maximizing" participants to contribute nothing and thus end up in a situation far inferior to what they could have achieved with full investment.

Unlike the fanciful prisoner's dilemma, the public goods game likely has a wide range of meaningful analogues in the real world. Indeed, as the name suggests, social scientists have argued a similar logic explains the lack of investment in actual resources that are described as public goods. This logic can also be extended to help understand how problems of "free-riding" and strategic exploitation can plague attempts to preserve and manage common resources, such as fisheries, where it is difficult to assign property rights that would give people incentives to use resources in sustainable ways.[12]

The logic of undesirable outcomes illuminated by these and various other sorts of rational choice models shows how people's strategic responses to incentives can lead to outcomes they would like to avoid and, in fact, could avoid if they and others could be trusted (or incentivized) to act differently.

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[12] See Garrett Hardin, "The Tragedy of the Commons" *Science* (December, 1938).

4.2.3 Identifying Effective Strategic Interventions

If a rational choice model indeed reveals the logic of decision making operative in some context, it may also suggest useful avenues for intervention, which could enable interested parties to achieve different outcomes. Rational choice models are inherently equipped to engage in counterfactual predictions. In the prisoner's dilemma and public goods game just discussed it is clear why the structure of incentives leads to suboptimal outcomes. If there were a way of changing these incentives, different outcomes could be achieved. In fact, in these scenarios the participants themselves have reasons to seek out interventions, given that there are superior alternatives within sight. If participants had a way of securing credible commitments to mutually advantageous (although not maximally advantageous) behavior they could overcome these decision traps.

In an idealized public goods scenario, participants might agree, *ex ante*, to place some of their money in escrow with a third party who only returns that money to them if they fully invest their remaining funds in the community resource. With regard to real instances of the prisoner's dilemma, the mafia long ago devised a system that reconfigures incentives to prevent jailed members from confessing mafia secrets in exchange for plea bargains. The term "*omerta*" designates a standing promise that the mafia will do their best to kill the loved ones of anyone who rats to the police. In light of this threat, the prospect of jail time compares favorably with amnesty.

Such interventions seek to change outcomes by changing incentives. However, interventions can also work by change the rules of the game. In a recent book, Bruce Bueno de Mesquita describes helping a retiring CEO ensure that he would not be

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replaced by a candidate he believed to be particularly unqualified.[13] The board of directors in charge of electing the successor usually did so by voting for all of the candidates simultaneously. The person with the most votes would be declared the winner. However, it was clear to the CEO that the "worst" of five candidates was poised to win through this process. The CEO was happy to have any of the other four candidates elected. After examining the fairly well known opinions of board members concerning the relative merits of different candidates Bueno de Mesquita saw that various alternative voting rules would result in defeat of the "worst" candidate. Some of these rules were too complicated or conspicuous to propose. However, a runoff system – in which the board's two favorite candidates faced off in a pair wise election followed by additional pair-wise elections for the winner against remaining candidates – seemed fair to the board, which agreed to adopt this rule. Little did they realize that, although the initially favored ("worst") candidate would win in the first run off, he would lose in one of the subsequent runoffs to a candidate who no one felt was a contender but was comparatively preferred once other candidates had been ruled out. The CEO got the outcome he wanted and the board members never complained about the process, believing it was a perfectly "fair" election, although some admitted being surprised by the result.[14]

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[13] Bruce Bueno De Mesquita, *The Predictioneer's Game: Using the Logic of Brazen Self Interest to See and Shape the Future* (New York: Random House, 2009) 39-43.

[14] Bruce Bueno de Mesquita also assembles an impressive list of rational choice predictive successes in political bargaining (both in domestic and international contexts) in his "The Methodical Study of Politics" in *Problems and Methods in the Study of Politics*, ed. Shapiro, Smith and Masoud (Cambridge: Cambridge University Press: 2004).

4.2.4 Mechanism Design

Insights pertaining to strategic interventions naturally lead to a more general question about the possibility of systematically structuring social institutions to achieve more desirable outcomes. Mechanism design is the name given to attempts to engineer better institutions through rational choice analysis.[15] One of the central aims of mechanism design is to identify institutional rules that are "incentive compatible," meaning that all agents involved are given incentives to adhere to rules that produce good aggregate outcomes. This requires giving special attention to issues regarding enforcement, commitment, and expectations.

Mechanism design has been particularly fruitful in economic contexts, and one of the most useful applications has been with regard to devising auctions. Auctions are a way of allocating resources through various forms of bidding. Different auction rules can produce very different outcomes, benefiting buyers more or less than sellers, or being more or less efficient at awarding a resource to those who value it most. The use of auctions is widespread, from internet sites like Ebay to international stock exchanges. Auction environments share many of the features of fruitful rational choice analysis that I outlined above. Those who participate in them generally have well defined, stable goals – they want to obtain a good for the best price, and for no more than it is worth to them. The rules of the auction are also well defined, set exogenously, and not subject to change.

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[15] See Roger B. Myerson "Mechanism design"
(http://home.uchicago.edu/~rmyerson/research/mechdes.pdf)

The utility of rational choice analysis for mechanism design is well illustrated by the famous case of the FCC spectrum auctions. In 1993 the U.S. Congress authorized the Federal Communication Commission to auction off portions of the electromagnetic spectrum to communications companies who would use the new spectrum bands for personal communications devices. Previously portions of the spectrum had been given away to companies through an "administrative hearing process" that was tasked with awarding the spectrum to the most deserving users or, later, through a lottery system. The former system was criticized for being opaque and arbitrary, the latter for encouraging non-serious companies to enter the lottery simply in order to resell any spectrum rights they happened to win. Neither process raised any money, but rather gave portions of the spectrum away for free.

When it decided to use auctions to allocate new portions of the spectrum, the US government had a clear vision of the outcomes it wanted the auctions to attain:

> The auction was intended to achieve an efficient allocation (making sure that the spectrum rights went to those companies that most valued them and could make best use of them), to prevent monopolies, and to promote small businesses, rural telephone companies, minority-owned and women-owned firms (as prescribed by the Government and the FCC policy). Moreover, it was understood that the volume of revenue raised by the auctioneer (the FCC) was an important factor to be taken into account.[16]

This provided the auction designers with particular objectives. Meeting these objectives, even the simple goals of efficiency and revenue generation, was not a trivial

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[16] Francesco Guala, "Building Economic Machines: The FCC Auctions" *Studies in History and Philosophy of Science*, Vol. 32, No. 3 (2001): 455.

task. The electromagnetic spectrum and telecommunications industry had some peculiar properties that could affect the way that auctions performed (for example, spectrum licenses can change in value depending on which other spectrum licenses one controls, and this bundling problem makes pricing these licenses particularly complex[17]). Moreover, prominent past attempts to use auctions to allocate resources in unique markets with few bidders had met with significant failures. New Zealand used a "second price" auction (in which highest bidder wins but only has to pay the amount of the second highest bid) to allocate its radio spectrum. Although in large markets this auction is typically efficient and revenue maximizing, with a small number of bidders the second highest bid price can be significantly below the winning bid. Indeed, in the New Zealand auction this dynamic, along with the failure to specify a reserve price, led in some cases

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[17]The basic problem of spectrum auction design is formalized very clearly in Bkowsky, Cull, and Ledyard's "Mutually Destructive Auction Design: The FCC Auction Design Problem"(*Journal of Regulatory Economics*, Volume 17, No. 3 (May 2000).): "Let there be a set X of K licenses $K=\{1,\ldots k\}$ to be allocated to a set of potential bidders, $I=\{1,\ldots,n\}$. A feasible allocation assigns a subset Xi

$(X$ to each i so that the collection of sets $Xo,Xi,\ldots Xn$ is a partition of X. Bidders possess valuations, defined by $Ui(Xi)-yi$, for each subset of x where yi is what the ith bidder will pay. An efficient feasible allocation $a=(Xi,\ldots,Xn)$ is one such that there is no other feasible allocation $a\sim=(i n X X \sim \sim ,\ldots)$ such that $Ui(i X\sim)>Ui(Xi)$ for all i. Given this, if monetary transfers between bidders are possible, then an efficient allocation solves:

$$\max_{X_i-X_n} \sum_i U^i (X_i)$$
$$\text{subject to } \bigcup_{i=1}^m X_i \subseteq X$$
$$\text{and } X_i \cap X_j = 0 \ \forall ij \in I$$

If the true $Ui(Xi)$ for all bidders were known, this would be a standard non-linear maximization problem solvable with the right algorithm. However, the true $Ui(Xi)$ may not be known to the mechanism designer. Simply asking bidders to specify their $Ui(Xi)$ may not be particularly useful. Given the profits obtained from owning a license, individual welfare maximizing behavior may lead bidders, when asked, to overstate $Ui(Xi)$. The use of a standard algorithm to solve this problem will misallocate licenses because incentive compatibility constraints lead to biased information. The mechanism design problem involves determining what must be known about the respective bidders' $Ui(Xi)$ in order to solve the above problem." (4).

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to licenses with a winning bid $100,000 NZ being given away for $6 NZ.[18] In Australia, the use of a sealed bid auction (in which bid are submitted privately, and the highest declared winner) to distribute the satellite television spectrum met with similarly sub-optimal results from the perspective of the government. The auction rules did not impose any penalty for withdrawing a bid once it was declared the winner. In many cases, high bidders withdrew so as to revert to lower bids they had simultaneously submitted, resulting in revenues half as high as the initial winning bids.[19] The experiences of Australia and New Zealand provided cautionary tales for the FCC as it began to consider the best auctions for its purposes.

The FCC solicited proposals for auction design, and received some 160 comments mostly from members of the communications industry, although many firms had hired teams of academic economists to advise them. The FCC's proposal was continuously refined through the theoretical suggestions and objections of these economists. Early on, the FCC proposed using a two stage combinatorial auction, in which licenses were initially auctioned bundled together via sealed bid and then on an individual basis, with the ultimate allocation decided by whichever method generates the most revenue. Economists Paul Milgrom and Robert Wilson (working for Pacific Bell) and Preston McAfee (working for AirTouch Communications) objected to this design showing that it was vulnerable to free riding problems, and suggested an alternative "simultaneous

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[18] John McMillan, "Selling Spectrum Rights" *Journal of Economic Perspectives* (American Economic Association) Volume 8 Number 3 (Summer 1994): 145-162.

[19] Ibid.

ascending bid" design able to deal with bundled goods without free riding problems.[20] Over the course of months these discussions continually improved features of the auction design.

With regard to many issues, although rational choice analysis suggested possible liabilities of different auctions, important parameters were either unknown or too complex to draw definitive judgments about how these systems would work in practice. A group of economists at Caltech led by Charlie Plott was hired to create "an experimental testbed" to examine how these auctions would perform with real participants. Their experiments helped confirm the superiority of simultaneous ascending bid auction to the combinatorial one, and also provided additional insights into how to optimize the former design (for example, their experiments showed how to head off bidding cycles by controlling the rate of new rounds and the information revealed to bidders between rounds). [21]

The auctions were implemented in July of 1994. The Office of Management and Budget had estimated the spectrum licenses to be worth $10.6 billion.[22] However, going into the auctions, industry leaders, such as MCI chairman Bert Roberts dismissed this estimate as ridiculous.[23] The auctions in fact raised over $17 billion.

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[20] Francesco Guala, "Building Economic Machines: The FCC Auctions" *Studies in History and Philosophy of Science*, Vol. 32, No. 3 (2001) 453–477.

[21] Ibid., 462; 467-470.

[22] John McMillan, "Selling Spectrum Rights" *Journal of Economic Perspectives* (American Economic Association) Volume 8 Number 3 (Summer 1994): 145.

[23] R. Preston McAfee and John McMillan"Analyzing the Airwave Auctions" in *Readings in Games and Information* ed. Eric Rasmusen (Malden: Blackwell Publishing, 2001) 326.

Fortune Magazine described the auctions as, "The most dramatic example of game theory's new power," and asserted the outcome "was a triumph, not only for the FCC and the taxpayers, but also for game theory (and game theorists)."[24] Summing up this triumph, John McMillian, one of the economists closely involved with the refining the auction's rules, declared:

> The FCC's spectrum auction is unprecedented in its use of economic theory in the design of the auction. The theorists' contribution showed in the choice of an auction with multiple rounds of bids; in the preference for a simultaneous auction when licenses are interdependent and have high value; in the form of the stopping rule and the use of an activity rule for the simultaneous auction; and in the nature of the bid-withdrawal penalties. The FCC's adoption of a simultaneous multiple-round auction ahead of a sequential or a single-round-sealed-bid auction-which are more conventional but arguably less effective for selling spectrum licenses-was a triumph for game theory.[25]

Although the knowledge that informed the design of the FCC's auctions consisted of more than just game theory, the success of the auctions was a vindication of the utility of rational choice analysis and mechanism design.[26] Again, the conditions for this success

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[24] *Fortune*, February 6, 1995.

[25] John McMillan, "Selling Spectrum Rights" *Journal of Economic Perspectives* (American Economic Association) Volume 8 Number 3 (Summer 1994): 160.

[26] Francesco Guala argues that it is an overstatement to claim this as a victory for rational choice analysis because no complete rational choice models of the auctions could be specified and many of useful insights came from the experimental testbeds (an some consider experimental research discontinuous from rational choice analysis). However, he admits that in a looser sense described by McMillian, this was indeed a vindication of the utility of rational choice analysis: "The FCC auctions provide a case study in the use of economic theory in public policy. They have been billed as the biggest-ever practical application of game theory. Is this valid? A purist view says it is not. There is no theorem that proves the simultaneous ascending auction to be optimal. The setting for the FCC auctions is far more complicated than any model yet, or ever likely to be, written down. Theory does not validate the auction form the FCC chose to implement. The purist view, however, imposes too high a standard. The auction form was designed by theorists. The distinction between common-value and independent-value auction settings helped clarify thinking. The intuition developed by modeling best responses in innumerable simple games was crucial in

are worth noting. This was a context in which the rules of the game could be unambiguously promulgated and enforced by the government, and the motives of the participants were stable and well understood. However, attempts to pursue mechanism design in non-economic contexts, particularly to address issues of "constitutional design," have been notoriously mixed in their results.

4.3 The Pathologies of Rational Choice – of little help for big questions?

Rational choice analysis has attracted a large number of critics. Many are willing to admit the utility of rational choice with regard to a narrow range of economic contexts but argue that social scientists have over extended rational choice analysis, mistaking it as an adequate framework for understanding any social phenomena. Rather than illuminating unique insights into purposeful behavior, rational choice, the critics charge, ends up an imperialistic quest that invents "just so" stories about the rationale behind all social phenomena. Some rational choice models do little more than offer an elaborate demonstration of the obvious, adding little new to our understanding. The more typical charge, however, is that rational choice analysis actually distorts what is really going on – by offering an over simplified logic, an implausibly complex logic, or various partial and

helping the auction designers anticipate how bidders might try to outfox the mechanism (McMillan et al., 1997, p. 429)." Quoted in Francesco Guala. "Building Economic Machines: The FCC Auctions" *Studies in History and Philosophy of Science*, Vol. 32, No. 3 (2001): 460.

contingent logics that prove of little actual use in dealing with concrete concerns. Rational choice analysis, the critics allege, tends towards increasingly complex mathematical models that attempt to save rational choice theory through the elaboration of universal logics of instrumental choice. The result is a kind of academic game, in which the goal is to invent "toy" rational choice models that can serve as a sort of analogy with real social phenomena, with little attention to the practical value of these often tenuous analogies. Rational choice research thus ends up methods driven rather than problem driven.

Donald Green and Ian Shapiro, two of the most prominent critics of rational choice, summarize many of these objections at the outset of their book, *The Pathologies of Rational Choice*:

> We contend that much of the fanfare with which the rational choice approach has been heralded in political science must be seen as premature once the question is asked: What has this literature contributed to our understanding of politics? We do not dispute that theoretical models of immense and increasing sophistication have been produced by practitioners of rational choice theory, but in our view the case has yet to be made that these models have advanced our understanding of how politics works in the real world. To date, a large proportion of the theoretical conjectures of rational choice theorists have not been tested empirically. Those tests that have been undertaken have either failed on their own terns or garnered theoretical support for propositions that, on reflection, can only be characterized as banal: they do little more than restate existing knowledge in rational choice terminology.

> The discrepancy between the faith that practitioners place in rational choice theory and its failure to deliver empirically warrants closer inspection of rational choice theorizing as a scientific enterprise. In our view, the weaknesses of rational choice scholarship are rooted in the characteristic aspiration of rational choice theorists to come up with universal theories of politics. This aspiration leads many rational choice theorists to pursue ever more subtle forms of theory elaboration, with little

attention to how these theories might be operationalized and tested—even in principle. When systematic empirical work is attempted by rational choice theorists, it is typically marred by a series of characteristic lapses that are traceable to the universalist ambitions that rational choice theorists mistakenly regard as the hallmark of good scientific practice. These pathologies manifest themselves at each stage of theory elaboration and empirical testing. Hypotheses are formulated in empirically intractable ways; evidence is selected and tested in a biased fashion; conclusions are drawn without serious attention to competing explanations; empirical anomalies and discordant facts are often either ignored or circumvented by way of post hoc alterations to deductive arguments. Collectively, the methodological defects of rational choice theorizing that we discuss in this books generate and reinforce a debilitating syndrome in which theories are elaborated and modified in order to save their universal character, rather than by reference to the requirements of viable empirical testing. When this syndrome is at work, data no longer test theories; instead, theories continually impeach and elude data. In short, empirical research becomes theory driven rather than problem driven, designed more to save or vindicate some variant of rational choice theory than to account for any specific set of political phenomena.[27]

At the heart of Green and Schapiro's diagnosis is the claim that most rational choice models in fact prove of little pragmatic use.[28] These models ignore the details and complexity of empirical reality in a quest for a universal method. Moreover, many models issue few "testable" empirical implications that could help us evaluate how well the model maps to reality. Green and Shapiro see contemporary rational choice research

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[27] Donald Green and Ian Shapiro, *Pathologies in Rational Choice Theory: A Critique of Applications in Political Science* (New Haven: Yale University Press, 1994) 6-7.

[28] Green and Shapiro wrote this book in the mid 1990's when most rational choice research was concentrated in American politics, particularly theories of voting behavior, legislative bargaining, and bargaining between branches of government. I will explore some of shortcomings of the voting and bargaining literature later, but it is helpful to note that Green and Shapiro's particular criticisms were mostly crafted in reference to research in American politics.

as dominated by the aspiration to achieve something like an "absolute science," rather

than driven by pragmatic aims of better addressing some concrete problems.

Indeed, Peter Ordeshook, one of the most accomplished rational choice theorists

in Political Science, agrees in this respect:

> Green and Shapiro's critique, though sometimes incomplete and
> inaccurate, nevertheless seems to be largely correct: the substantive
> relevance of much formal rational choice analysis is tenuous, and its
> empirical content lacks coherence. Even the treatment of such basic
> matters as voting, committee agendas, and spatial conceptualizations of
> preferences are confounded by dubious assumptions and often wholly
> irrelevant analyses.[29]

And, Ordeshook, goes on to add, rational choice analysis often shares with much other

political science research a lack of pragmatic implications. Rational choice, like so much

social science research, is not useful.

> …the remedies Green and Shapiro offer are no more likely to move use
> toward a useful understanding of politics than is the vast majority of
> research found in the current political science literature, regardless of the
> paradigm to which that literature corresponds.
>
> The core of my argument is this: Green and Shapiro, rational choice
> analysts, and most other political scientists fail to distinguish between
> science and engineering—that is, between the discovery of first principles
> and the identification of the empirical generalities to which they pertain on
> the one hand; and, on the other, the resolution of practical issues. Too
> many rational choice researchers try to do science when engineering better
> describes their goal. The failure to make this distinction leads to research
> that does not identify first principles, isolate empirical phenomena that
> warrant empirical generalization, delineate phenomena that are
> manifestations of complex interdependent processes, develop expertise

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[29] In *The Rational Choice Controversy: Economic Models of Politics Reconsidered* ed. Jeffrey Friedman
(New Haven: Yale University Press, 1996).

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that has practical relevance, or refine our ability to predict something other than gross or trivial events.[30]

Ordeshook is less convinced than Shapiro and Green that social science research ought to be immediately pragmatic. As his engineering analogy suggests, he endorses a common view that sees some social science research as building basic theory, just as pure physics does. On this analogy, we should not ask of pure research that it be useful, although subsequent generations of engineers may discover useful applications.[31] In any case, Ordeshook admits that rational choice analysis in political science has not yielded many "engineering" insights that would vindicate its pragmatic worth.

4.4 Demonstrating the Usefulness of Rational Choice for "Big" Questions of Political Development?

In the course of debates about the pathologies of rational choice, both practitioners and critics highlighted this concern that much rational choice analysis outside of economic contexts did not yield insights that were practically useful.

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[30] Ibid.

[31] This analogy is hard to countenance if one accepts the claims I developed in the second chapter regarding the nature of social science. The analogy between rational choice and physics presumes that there is indeed some absolute perspective to be grasped by rational choice, for which engineering simply involves the continuous extension of its insights into practical applications. However, as I have argued, there is in fact no absolute perspective as such with regard to social phenomena, so the only method we have of evaluating social science research is a pragmatic one, based on its ability to help us deal with problems better than we might have otherwise. Of course, a social scientist might possess some genuine insight, whose practical import remains to be worked out for various circumstances. However, the common perspective that apologizes for the uselessness of social science research by affirming that it is still at the "pure/theoretical" rather than "applied" stage relies on a ridiculous and untenable analogy with the natural sciences.

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Responding in part to this charge, a number of social scientists sought to demonstrate the utility of rational choice analysis for pressing social concerns, particularly pertaining to the political-economic development of the third world. A collection of articles published in 1998 under the title of *Analytic Narratives* explored rational choice applications for "comparative and historical questions of regime transition, social conflict, democratic stability, economic development, and international governance," and this collection was conceived in part as a rebuttal to the criticisms of Green and Shapiro.[32] This was an ambitious attempt to illustrate the power that rational choice insights could bring to large social questions. Although these studies contained numerous rich suggestions, they also failed to convince many social scientists of their genuine utility, particularly Jon Elster whose criticisms I will examine below. Other applications of rational choice to large questions of politics and development, such as one finds in the voluminous writings of Persson and Tabellini, Avner Grief's *Institutions and the Path to the Modern Economy*, and Acemoglu and Robinson's *Economic Origins of Dictatorship and Democracy* have been likewise ambitious and controversial. Debates about the applicability of rational choice to problems of political development have in turn highlighted the intrinsic limits of rational choice analysis for social thought more generally.

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[32] Bates, Greif, Levi, Rosenthal, and Weingast, "Analytic Narratives" *American Political Science Review*, Volume 93 No. 3 (September 2000) 685.

In the introduction of this study I explored some of the difficulties that arise in trying to understand the structure of incentives in the developing world. I also suggested that, although we can often *describe* what better institutions would look like, it is not at all clear how to develop these institutions in the first place. We can give a good account of why defending "human rights," protecting property, and providing for education would help peoples of the third world escape poverty, disease, oppression, and violence but we don't know how to "institute" these institutions, how to get them adopted, endorsed, and made to "stick."

Although it is possible to diagnose much about nature of the problems of the third world, finding a way out of them is not easy within the purely instrumental perspective of mechanism design. Using Avner Greif's work on the problem of violence in the third world, I suggested that, although existing incentives networks may be perverse and in need of reconfiguration, a large part of the problem has to do with what are taken to be incentives in the first place. Questions about the nature of incentives (and motivation more generally) pervade rational choice analysis of problems of development. It is useful to examine how these questions surface in two of the studies that comprised the Analytical Narratives (AN) volume, which was intended to vindicate the utility of rational choice, alongside Jon Elster's criticisms of these studies.

4.4.1 Analytic Narratives and Jon Elster's Critique

Avner Greif contributed the first chapter to the AN project in which he sought to explain how the *podesta* system was responsible for bringing peace and prosperity to

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Genoa in the period between 1100 and 1350. According to Greif, up until 1194 Genoa was plagued by an economically suboptimal arrangement in which the major clans of the city engaged in arms races to keep each other at bay. At times this resulted in an uneasy peace with major resources devoted to armaments, and at other times, often linked to changes in external military threats, semi-permanent clan warfare broke out further devastating the city. However, Genoa's economy grew tremendously starting around the end of the 12th century, which Grief attributes to the *podesta* system – an arrangement in which an external administrator was financially supported by all the clans and tasked with adjudicating problems of city politics that arose between them. Grief developed a formal model of the "podestaria game" – a sort of elaborated prisoner's dilemma collusion game – in which he showed that within certain parameterizations of clan/podesta strength and the relative interests of each there could be a credible and stable equilibrium of interests.

Jon Elster finds Greif's proposal unconvincing on many fronts. Elster takes issue with the model itself, pointing out that it fails to take into consideration military investments that might be required to face external enemies, which could easily destabilize the military investment equilibrium of the clan on clan model. Elster also raises an empirical concern about the actual ability of the podesta (who commanded a private force of no more than twenty soldiers) to enforce particularly controversial decisions against much more powerful clans. Concerns about the logic of the model as well its empirical verisimilitude lead Elster to doubt that clans would substitute economic for military investment as suggested in the final (forth) stage of the model, which Grief

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takes to explain the economic growth in Genoa in the 13th century. Although Greif's analysis provides a profoundly interesting conjecture, it remains highly speculative, improbably simplistic, and perhaps ultimately impossible to substantiate in detail.

The second chapter of AN consisted of a study by Jean-Laurent Rosenthal who sought to explain "the divergent courses of French and English absolutism in the seventeenth and eighteenth centuries" in part through a formal model relating rates of taxation to gains and losses through wars financed by taxation. In essence, the model assumes monarchs want to maximize returns from wars but are constrained in waging wars by the amount of taxes they can raise to finance warfare. Monarchs with absolute authority are further assumed to have greater tax discretion than monarchs who have to share fiscal authority with nobles. Finally, the model stipulates that ruling elites "share the returns from winning and losing according to the extent of their fiscal control of the domestic economy (103)" and if the war ends in a "draw" (which happens in almost half the cases of one specification) the elite bears all the costs of conducting the war.[33] The result is that countries with Monarchs who share fiscal power with nobles do not tax as much as would be optimal for realizing maximal gains from war. This, Rosenthal claims, explains the differences in tax rates and the political development of France and England in the seventeenth and eighteenth centuries.

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[33] Jon Elster. "Rational Choice History: A Case of Excessive Ambition" *American Political Science Review*, Vol. 94, No. 3 (Sep. 2000) 687, (quote from *Analytic Narratives*, p. 103).

Elster raises a number of objections to this model, which in his final judgment "does not go any way at all towards explaining the observed differences."[34] First, Elster points out that the model's assumptions are not only simplistic but very far from the empirical realities. For example, "the idea of spoils divided in proportion to fiscal control is unsubstantiated, empirically and theoretically." Indeed, the problem is not just that the model's assumptions are "unrealistic," but they are theoretically inconsistent. As Elster points out, there is a specification of the model in which the elite raises no taxes despite controlling 10% of possible tax revenue. The model predicts that the king will give them 10% of the war spoils. However, this violates the underlying assumption that the king is wealth maximizing. It does not work, within the logic of this model, to explain the king's generosity as a way to secure the allegiance of nobles. Sharing wealth is an *ex post* decision, not an *ex ante* promise. Elster suggests that the idea that the elite bear all the costs in case of a "draw" in a war is likewise both empirically implausible and theoretically suspect. Finally, Elster calls attention to the fact that the model's empirical predictions concerning the tax differentials between England and France in this period are actually quite poor. Although the model suggests that England's rates will be (slightly) higher than France's (6.3% vs 5% in one specification) the magnitude of the observed difference is much larger. Moreover, Elster points out that the results of the model are entirely dependent on the functional form Rosenthal posits to express the relationship between quantity of taxes raised, the expenses of raising them, and the probability of

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[34] Ibid., 687.

success in war in relation to tax expenditure. He provides absolutely no justification of the details of these functions (it is hard to know how one could justify them), and they in turn drive the major conclusions of the model.

The greatest challenge Elster raises to Rosenthal's analysis has to do with the motivations of monarchs in the first place. Elster writes, "It would be as absurd to deny that French absolutist Kings were preoccupied with revenue at to assert this was their only concern. They were, in a word, reward-sensitive. That is not to say, however, they were reward-maximizing (692)."[35] As Elster points out, anyone familiar with the period knows that French elites were obsessed with glory and honor, and that the satisfaction of, say, humiliating their rivals, might be every bit as motivating as the prospect of revenues from war. Indeed, it is easy to conceive of the latter being passed up for the former. This leads Elster to conclude that "nonrational concerns could often distort their [the king's/nobles'] thinking or shorten their time horizon so as to undermine the instrumental efficiency of their behavior," although he does not deny that when the material interests of kings were serious threatened they could turn into wealth maximizing agents.[36]

After offering additional critiques of the other studies in the AN volume, Elster turns in his review to some summary considerations of the shortcomings of rational choice theory. First, he is concerned that many rational choice studies engage in post-hoc

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[35] Ibid., 692.

[36] Ibid., 692.

justifications that shed little light on the reasoning that agents actually employ to arrive at

their decisions:

> Much of applied rational choice theory is a combination of just-so stories and functionalist explanation. One constructs a model in which the observed behavior of the agents maximizes their interests as suitably defined, and one assumes that the fit between behavior and interest explains the behavior. Suppose that higher education tends to make people pay more attention to the future, and paying more attention to the future tends to make people better off. It is then a tempting step to conclude that people choose higher education in order to reduce their rate of time discounting (Becker and Mulligan 1998), or (in a more general version) that this particular benefit of education explains why it is chosen. It is, however, a temptation that should be firmly resisted, in either version (Elster 2000, 26-9). Unless one can demonstrate an intention (first version) or a causal feedback loop from the consequences of the behavior to the behavior (general version), the coincidence of behavior and interest may be only that-a coincidence.[37]

Elster also raises a technical concern with the ability of rational choice models to

incorporate and deal with varieties of uncertainty that pervade large questions of social

change:

> Except for a minor feature of Weingast's chapter, all the models in AN assume full information. In the real world, of course, high-stake politics is permeated by uncertainty. No model of political behavior that ignores this fact can be successful in predicting out-comes. There are at least five kinds of uncertainty. The first is brute factual uncertainty (will there be a major earthquake in greater Los Angeles over the next decade?). The second is higher-order uncertainty about the cost of resolving first-order uncertainty (do I have time to ascertain the enemy's position before going into battle?). The third is strategic uncertainty due to multiple equilibria (do cartel members play tit-for-tat or sudden death?). The fourth is uncertainty due to asymmetric information (is my opponent irrational or only faking?). The fifth is uncertainty due to incomplete causal understanding (will tyrannical measures imposed by a dictator make the

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[37] Ibid., 693.

subjects more compliant or less?). The compound effect of these (and perhaps other) forms of uncertainty will, in most complex situations, tend to be overwhelming. At the same time, existing models of decision making under uncertainty, of equilibrium selection, and of games with asymmetric information tend to be very artificial. One might say, perhaps, that it is to the credit of the AN authors that they stay away from these models. Yet, I think the proper conclusion would have been to eschew formal modeling altogether.[38]

4.4.1.1 Technical Concerns, Model Testing

There are additional technical concerns with rational choice models that Elster does not highlight, which are nonetheless worth considering. Formal models are generally "brittle," which is to say they are sensitive to starting assumptions, little changes in which can drastically alter equilibrium results. The "tipping points" of models are often what makes them interesting, but if a model is not specified with exquisite accuracy its predictions about the all-important transition points will be off. Also, models often result in "all or nothing" predictions (there is no account of "error" in perfectly rational behavior), while in the real world we witness a range of outcomes. Even models that incorporate probabilistic strategies and admit of multiple equlibria still posit a narrow set of discreet outcomes, and it is not at all clear how one should evaluate the performance of such models in the real world when a much wider range of outcomes are observed. This has sparked extended controversies concerning model evaluation.

It is hard to make sense of the idea of "statistically testing" a model directly if the model does not issue a distribution of outcomes. Although some modelers have tried to

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[38] Ibid., 693.

introduce statistical distributions into model predictions – via the concept of "quantile response equilibrium" developed by McKelvy and Palfrey or the introduction of "trembling hand" errors (suggested by Selten) – this can upset the logic of a model, and in any case the statistical interpretation of goodness of fit remains unclear.[39]

4.4.1.2 Technical Concerns, Model Analogies

The way rational choice models figure into social analysis is often not for the purpose of making a particular prediction, but by way of suggesting an insightful analogy into the structure of a problem. However, saying that models provide researchers with insightful analogies begs the question of the nature and import of the analogy. There may be a sense in which "Congress is like an n-person, two stage bargaining game with single peaked preferences over a multidimensional policy space" just as there is a sense in which "life is like a box of chocolates." In the latter case it is true in the sense that you never know what you're going to get, and in the former case in the sense that in both Congress and the bargaining game there will be vote trading among the bargainers. Because the possible analogies to be drawn between any formal model and any social phenomenon are likely endless, we always need to inquire into the quality and usefulness of the analogy. Ultimately, the "right" answer to the question of how accurate a model should be in terms of its predictions is likely that it depend on the purposes for which the

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[39] See Richard D. McKelvey, and Thomas R. Palfrey. "Quantal Response Equilibria for Normal Form Games." *Games and Economic Behavior.* 10, (1995) 6-38. and Selten, R. "A reexamination of the perfectness concept for equilibrium points in extensive games." *International Journal of Game Theory* 4:25-55 (1975).

model is being used – an implicitly pragmatic criterion that is not addressed in any of the Analytical Narrative studies.

4.4. Varieties of Motivation in Social Change?

After considering his various technical concerns, Elster finally raises the general problem of motivation, which he believes proves an insurmountable obstacle to the extension of rational choice modeling to many social problems. Not only can motives be complex, but they can be shaped by forces that escape specification in a rational choice framework.

> The main obstacle to analytic narratives, understood as rational choice history, arises at the level of motivations. As suggested by my earlier discussion, I want to make two claims. First, nonrational motivations are important and pervasive. Wars have been lost because soldiers were taught that it was dishonorable to take defensive measures (Dixon 1976, 54-5). Analyses of why some individuals harbored Jews in Germany or German-occupied countries during World War II whereas others did not suggest that a major factor was that the former were asked by someone to do so (Varese and Yaish 1999). On a rational choice account, this would be a matter of information: To harbor someone, you first have to know about their existence. On an alternative and perhaps more plausible account, it is a matter of the emotional difficulty of refusing a face-to-face request. In war trials after World War II, individuals accused immediately after liberation were sentenced much more severely than those tried for identical crimes two years later (Elster 1998a), which most plausibly can be explained by appeal to the dynamics of anger and hatred. These are scattered examples, which could be multiplied indefinitely. Yet, if we embrace the most abstract characterization of analytic narrative as deductive history, rather than rational choice history, such facts are not necessarily fatal to the project. To the extent that emotions – their triggering and their dynamics – can be modeled in a way that yields definite predictions, they can be incorporated into an analytic narrative. My second claim, however, is that we do not know how to construct such models. We do not know how to predict the behavior that will occur when an individual is entirely in the grip of an emotion. Fear, for instance, can

lead to fight, flight, or freezing, and we do not know which will be triggered in a given situation. We may not even be able to predict which emotion will be triggered. If A favors B at the expense of C, will C feel envy toward B or anger toward A? Also, we do not have good models of the trade-offs at work when emotion and rational pursuit of a goal coexist as motivations. (On all these points, see Elster 1999).[40]

In the AN studies, as well as most other attempt to model political-economic processes in the developing world, agents are assumed to be primarily motivated by economic considerations. But as Elster points out this is often not the case, and it is extremely difficult to accurately incorporate other preferences/motivations that may influence behavior.

This focus on economic motivation in development studies holds true if we look at Acemoglu and Robinson's attempt to explicate a universal logic of democratization in their widely admired book *The Economic Origins of Dictatorship and Democracy*. They summarize the main thesis of the study as follows:

> This book develops a framework for analyzing the creation and consolidation of democracy. Different social groups prefer different social institutions because of the way they allocate political power and resources. Thus, democracy is preferred by the majority of citizens but opposed by elites. Dictatorship, nevertheless, is not stable when citizens can threaten social disorder and revolution. In response, when the costs of repression are sufficiently high and promises of concessions are not credible, elites may be forced to create democracy. By democratizing, elites credibly transfer political power to the citizens, ensuring social stability. Democracy consolidates when elites do not have a strong incentive to overthrow it. These processes depend on (1) the strength of civil society, (2) the structure of political institutions, (3) the nature of political and

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[40] Ibid., 694.

economic crisis, (4) the level of economic inequality, (5) the structure of the economy, and (6) the form and extent of globalization.[41]

The authors explain at the outset that their approach is "'economic based' in the sense that we stress individual economic incentives as determining political attitudes, and we assume people behave strategically in terms of game theory."[42] The book includes nearly 400 pages of sophisticated formal models of revolution, democratic consolidation, voting, and so forth, meant to provide a universal account of how democracies emerge and persist. However, this analysis is all premised on the claim that, "the only reason that agents care about political institutions is because of their different economic consequences." The authors admit "If ideological preferences are primary much of our analysis is not relevant."[43] The authors do attempt to incorporate soft ideological preferences that do not overwhelm economic concerns into some of their later models, but the underlying economic logic remains. [44]

Even if one did want to specify models that take "ideological" preferences as primary, modeling ideological concerns is easier said than done. This is particularly the case in development contexts, which are rife with values and beliefs that are inconsistent with purely economic concerns – including witchcraft, ritual mutilations, honor killings,

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[41] Daron Acemoglu and James A. Robinson *The Economic Origins of Dictatorship and Democracy* (Cambridge: Cambridge University Press, 2006.)

[42] Ibid., xii.

[43] Ibid., 211.

[44] For an excellent exploration of the importance of considering and modeling ideological preferences see Munger and Hinich, *Ideology and the Theory of Political Choice* (Ann Arbor: University of Michigan Press, 1994) and Brennan and Lomasky, *Democracy and Decision: A Pure Theory of Electoral Preference* (Cambridge: Cambridge University Press, 1993).

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tribal rivalries, and cycles of revenge to name but a few. These things not divorced from

standard economic "incentives," but form their own incentives in ways that are not

reducible to economic interests while interacting with such interests in ways that will be

hard to characterize *ex ante*.

Ultimately, Elster does not deny that rational choice has its uses in social thought,

but he argues these uses are limited. Summarizing his perspective, he writes:

> All this is not to say that rational choice theory cannot illuminate historical
> analysis, as long as its claims are suitably modest. Collective action theory
> has changed forever the way social scientists and historians think (or
> ought to think) about rebellion, revolution, and related phenomena.
> Hobbes, Tocqueville, and Marx may use language that reminds us of
> modern discussions of the free-rider problem, but formal analysis is
> needed to bring out its relation to the subtly different phenomena modeled
> in the game of Chicken or the Assurance Game. Montaigne and Descartes
> may have understood at a qualitative level that iterated interactions differ
> importantly from one-shot interactions, but they did not and could not
> anticipate game-theoretic results about the conditions under which
> behavioral differences are likely to arise. Modern analyses of credibility
> and precommitment have revolutionized our understanding of strategic
> behavior. The idea of burning one's bridges has always been known, but
> only after Schelling (1960) has the motivation for such behavior been fully
> understood. Again, examples could be multiplied indefinitely. The need
> for modesty appears in two ways. First, as I have been at some pain to
> emphasize, one should avoid the postulate of hyperrationality. Collective
> action, iterated games, and credibility are simple ideas that can be and
> have been refined to yield rococo (or baroque?) constructions that no
> longer bear any relation to observable behavior. To be useful, they have to
> be constrained by what we know about the limitations of the human mind.
> Second, because formal analysis has nothing to say about the motivation
> of the agents, it cannot by itself yield robust predictions. Although it is
> extremely useful to know that the structure of material interests in a given
> case is that of a one-shot Prisoner's Dilemma, that fact does not by itself
> imply anything about what the agents will do. If they have nonmaterial or
> even nonrational motivations, they might behave very differently from the
> noncooperative behavior we would expect if they were exclusively

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swayed by material interests. If they are in fact observed to cooperate, then we will have to search for nonmaterial or nonrational motivations. Rational choice theory tells us what to look for, not what we will find.[45]

Elster's criticisms provide a useful corrective to the universal ambitions of rational choice theorists. In order to better understand the systematic limits of rational choice models, however, we need to examine this all-important problem of motivations more closely.

4.5 The Problem of Motivation

Rational choice theory illustrates very well the importance of "preferences" for behavior. However, the diversity and dynamism of preferences poses a serious problem for the utility of rational choice models. There may be dominant and stable motives in certain social circumstances – preeminently in economic contexts, but perhaps in some political ones as well. However, for rational choice theory to become a truly universal method of explanation it would have to be able to provide an account of the sources and nature of motivation; and for it to be universally useful for addressing social problems it would need to provide resources for preference change.

We have the very best reasons to believe that neither of these can be achieved within a "scientific" framework. This is because, as Charles Taylor has forcefully argued:

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[45] Jon Elster. "Rational Choice History: A Case of Excessive Ambition" *American Political Science Review*, Vol. 94, No. 3 (Sep., 2000): 695.

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> The only general rule in history is that there is no general rule identifying one order of motivation as always the driving force. 'Ideas' always come in history wrapped up in certain practices, even if these are only discursive practices. But the motivations which drive towards the adoption of and spread of these packages may be varied; and, indeed, it is not even clear that we have a typology of such motivations ('economic' v. 'political' v. 'ideal', etc) which is valid through human history.[46]
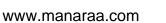
Motivations, particularly ethical convictions, are not hardwired. They change and do so in ways that are not reducible to an a-historical scientific account. This, at least, is the position I will defend in more detail in the next two chapters.

4.5.1 What do People Want? Narrow and Broad Interpretations of Utility

Relating to this problem of motivation, there has been considerable debate within rational choice theory about what goes into a utility function. It is often said that rational choice theory assumes that individuals act according to their "self-interest." If all that is meant by this claim is that individuals do what they want to do – which can be anything, including promoting the well being of others – this ends up a mere tautology. However, some have interpreted this dictum to entail a stronger claim about the nature of human preferences, that they are (or should be) primarily "egoistic" – narrowly focused on the aggrandizement of personal wealth, power, or honor.

Gary Becker and George Stigler have advanced what is perhaps the most serious attempt to understand all human behavior in terms of economic self-interest. In their 1977 article arguing against the common thesis that people's aims can be both diverse and

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[46] Charles Taylor, *A Secular Age* (Cambridge, Massachusetts: Belknap Press, 2007) 213.

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subject to change, Becker and Stigler claim that "tastes neither change capriciously nor differ importantly between people."[47] They defend the hypothesis "that widespread and/or persistent human behavior can be explained by a generalized calculus of utility-maximizing behavior," which they believe is fundamentally economic for all people. To substantiate this claim Becker and Stigler examine phenomena commonly associated with preference change – addictions, fashions, habituation – all of which, they argue, can be understood in terms of stable underlying preferences that are only manifested in different ways because of changes in the environmental context, particularly shifts in relative prices.

Their paper was an elaboration of ideas that appeared in Becker's book *The Economic Approach to Behavior*, where, as I indicated earlier, Becker maintains, "human behavior is not compartmentalized, sometimes based on maximizing, sometimes not, sometimes motivated by stable preferences, sometimes by volatile ones, sometimes resulting in an optimal accumulation of information, sometimes not. Rather, all human behavior can be viewed as involving participants who maximize their utility from a stable set of preferences and accumulate an optimal amount of information and other inputs in a variety of markets."[48] Becker's approach represents one end of the rational choice debate

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[47] George J. Stigler and Gary S. Becker, "De Gustibus Non Est Disputandum" *The American Economic Review*, Vol. 67, No. 2 (Mar., 1977): 76.

[48] Gary S. Becker *The Economic Approach to Behavior* (Chicago: University of Chicago Press, 1976) 14.

about preferences, and although unpersuasive to many, it has been particularly attractive to economists.[49]

Amartya Sen has defended the opposite perspective, arguing in his 1976 article "Rational Fools: A Critique of the Behavioral Foundations of Economic Theory" that it is a profound mistake to think that preferences are (or should be) fundamentally economic, egoistic, and unchanging. Sen traces out the history of this idea in economic thought and shows how it has been permuted in complex discussions of the nature of commitment, sympathy, and "ethical" preferences. Ultimately, Sen maintains that narrow view of economic egoism that pervades contemporary social science is an unfortunate by product of simplified assumptions that proved useful shortcuts for other aspects of economic theory. What is needed, in his view, is a "richer structure" than most rational choice accounts of preference assume. However, he thinks it is indeed possible for a more capacious notion of preference to inform rational choice models.[50] Technically speaking,

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[49] One illustration of the limits of economic explanations for political behavior is highlighted by Green and Shapiro, who point to the voluminous literature on the logic of voting as a prime example of rational choice analysis drifting off into irrelevant and unpersuasive analysis. According to the most basic rational choice reconstruction of voting, we should expect that no one will vote because the economic costs of voting are non-trivial (in terms of time and effort) while the expected impact and value of one's vote is close to zero in an electoral system with millions of other voters. From a cost benefit perspective voting simply doesn't make sense. And yet people vote. One interpretation is that people are simply irrational, which is what a strong commitment to the universality of economic motivation would suggest. However, models of voting quickly turned to incorporating an additional variable (ideology, or "civic-mindedness") into the model, meant to indicate the utility that people receive from the very act of voting. By introducing this additional form of utility, modelers could save the model, but at the expense of it becoming trivial. The reason that people vote then ends up being because they value voting. The initial economic logic of the model is appropriately abandoned, but one ends up with a vacuous tautology – an elaborate model that says nothing more than that people vote because they like it.

[50] Geoff Brennan's recent book with Phillip Pettit, *The Economy of Esteem* (Oxford: Oxford University Press, 2004), provides an admirable example of the kind of modeling Sen seems to suggest. It considers how rewards of "esteem" can motivate agents and examines the comparative statics of it effects. However,

this is easy, but it begs the question of how we can come to know with sufficient accuracy the wider variety of motivations that can influence behavior.

Sen also mentions but does not explore the (at that time emerging) recognition that economic self interest does a very bad job of describing the way people actually behave in a variety of economic contexts. Indeed, the most sustained critique of narrow economic self-interest has come from experimental/behavioral economics research of the last two decades. At first, economists acted surprised to find that the average person does not conform at all to classical wealth maximizing rational choice predictions in a variety of strategic games. People cooperate in prisoner's dilemmas, invest in public goods, return money in "trust" games, and are generous in ultimatum games. Experimental researchers such as Kahneman and Tversky pioneered behavioral economics studies that cast doubt not only upon narrow assumptions of economic motivation, but also on the supposedly axiomatic assumptions of "rational" choice, such as preference consistency and transitivity.[51]

One response to the supposedly sub-optimal behavior of individuals in these games was to suggest revised accounts of rationality, such as bounded rationality (which rationalizes certain kinds of ignorance based on the costs of information) or mini-maxing

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as important as esteem undoubtedly is, one cannot help but think Brennan and Pettit only touch the tip of the iceberg in terms of the variety of non-economic motivations that move people on a daily basis.

[51] See Kahneman, D., Slovic, P., & Tversky, A. *Judgment Under Uncertainty: Heuristics and Biases* . New York: Cambridge University Press (1982); and Kahneman, D., "Maps of Bounded Rationality" [Nobel Prize Lecture] *The Nobel Prizes 2002*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 2003

(minimization of maximal expected loss), which would explain the behavior as conforming to a deeper notion of "rationality." On one level this seems a necessary and plausible way to address a longstanding concern that rational choice models propose decision logics that are far too complex for most people to actually use as a basis for everyday behavior. However, coming up with a new account of rationality to fit behavioral patterns is also liable to the charge of being an ad-hoc, arbitrary enterprise. Along these lines, James Murphy suggests:

> Attempts to modify equilibrium analysis in rational choice theories have been largely comic: if we find that people are ignorant, then ignorance is optimally rational, given the costs of information; if we find that people are impulsive and passionate, then passion and impulse are optimally rational, given the costs of deliberation; if we find that people act out of habit, then habits are optimal decision strategies given the costs of thought; and so on. The auxiliary theorems modify rational choice theory in the sense that a cat is modified by the mouse it eats.[52]

4.5.2 The Perils and Promises of Non-economic Preferences

Inquiries into the nature of rationality and the sources of irrationality now constitute a major research program in the social sciences (and biology, as we will see in the next chapter), which it is hoped will help augment and refine rational choice modeling. Related, and equally important, are studies aimed at understanding the "non-economic" preferences that people have. However, to phrase the question in that way is already misleading. If it is the case that preferences are dynamic, there may be no

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[52] James B Murphy "Rational Choice Theory as Social Physics" *Critical Review*, Volume 9, Issue 1 & 2 Winter 1995, 155-174, 172. I am indebted to Robert Nau for alerting me to Murphy's work.

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"characteristic" preferences that people have over certain domains. If so, the study of preference would consist of little more than taking frequent surveys and polls; and, as survey research on political preferences demonstrates, people's preferences can change quite dramatically over weeks and months. This means that using "ideological" or other preferences as the foundation for formal models runs up again the difficult task of measuring and specifying such preferences. Again, preferences matter. For rational choice models to be most successful we have to know what existing preferences are. Moreover, models show why it would be particularly useful if we were capable of shaping preferences.

Examining behavioral economics research in more detail reveals that there are particular kinds of preferences that experimentalists find most perplexing, namely preferences that trump economic considerations in interpersonal contexts. Some researchers have referred to these as ethical preferences. Perhaps the most central research problem in contemporary behavioral economics and rational choice theory is the attempt to understand why people leave "money on the table." That is, why do a vast majority of people forgo opportunities to maximize their wealth, letting some money go to other people with whom they are interacting. Not only is it the case that people routinely cooperate when there are opportunities for exploitation, but individuals often give money away in ways that can be interpreted in terms of generosity and concepts of fairness. An enormous research program has emerged to investigate this "perplexing"

behavior, which behavioral economics have characterized using the concepts of "trust, reciprocity, and altruism."

One of the central theoretical questions for behavioral economics has been to distinguish instrumental motives from motives of fundamental preference. Initially, researchers thought that "trust, reciprocity, and altruism" might be explicable entirely in an instrumental economic framework, as a strategy for reaping greater rewards over repeat interactions. However, closer investigations complicated this aspiration, demonstrating that people continue to demonstrate "trust, reciprocity, and altruism" in context where there is complete anonymity and no prospect of interacting with another person again. So, the literature increasingly came to accept that "other regarding preferences" had to be understood as primary.

This admission created both problems and hopes for rational choice theorists. The fact that a variety of "ethical preferences" systematically distort people's valuation of economic rewards makes it more difficult to be confident that one can draw predictions from knowledge of economic incentives. On the other hand, "ethical preferences" provide both a theoretical explandum for paradoxes of institutional analysis and a new possibility for structuring behavior beyond the manipulation of economic incentives. Regarding the former, ethical preferences provide a way of solving the problem of an infinite regress of incentives in institutional thought – a problem I outlined in the introduction of this study. If there are fundamental preferences for the properties of an institutional arrangement itself, these can trump incentives to undermine the institution through exploitative opportunities. "Ethical preferences" explain why institutions can

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persist despite individual economic incentives to undermine them. The great mystery of liberal democratic regimes from the perspective of new institutional thought is precisely why ruling elites don't maximize their extraction of rents – something that would enrich them but destroy desirable features of the institutions over time. The primacy of "ethical preferences" provides a theoretical answer.

This perspective also raised a new possibility for those who use rational choice to think about opportunities for improving social outcomes. Instead of reshaping economic incentives to form a perfect network of behavioral compliance, the project of shaping ethical preferences could also be a way to direct behavior. However, this theoretical possibility runs into the problem that rational choice and behavioral economics are not well equipped to understand the origins of ethical preference. Rational choice theory, in its classical formulation, takes preferences as given. However, rational choice ends up demonstrating with exquisite clarity the practical importance of being able to shape and change preferences. What people value matters for their behavior – and examining the instrumental strategies people pursue to achieve given ends is possibly less important than understanding and shaping their ends in the first place.

4.5.3 Dealing with Ethical Preferences in a Scientific Framework?

"Scientific" attempts to pursue this question of value formation and to deal with the existing limits of rational choice modeling have developed in two distinct but related directions. The first tries to make the instrumental "rationality" of rational choice a basis for normativity, moving rational choice in a normative direction. The second tries to make rational choice models more descriptively realistic by loosening the guiding

concept of rationality to include systematic irrationalities. Both approaches suggest ways to scientifically explain or deal with the nature of ethical preferences. Despite their illuminative moments, neither perspective, I argue, provides a convincing account of the origins and nature of ethical convictions. Their failure to do so highlights the importance of non scientific accounts of the development of ethical concepts.

4.5.3.1 Grounding Ethics in Instrumental Reason

One response to the problem of ethics has been to try to provide a normative account of ethics drawing on rational choice theory. On the extreme formulation, this approach hopes to given an account of how particular ethical convictions are actually require by rationality (understood as instrumental reason). This approach marks a break with the more limited, descriptive/explanatory account of rationality employed by most formal modelers in the social sciences. Rather, it endorse a normative vision of rationality, as suggested by Harsayni: "Our theory is a normative (prescriptive) theory rather than a positive (descriptive) theory. At least formally and explicitly it deals with the question of how each player should act in order to promote his own interests most effectively in the game and not with the question of how he (or persons like him) will actually act in a game of this particular type."[53] Rational choice models, on this reading, issue in prescriptive conclusions, indicating what people should do in order to maximize their utility. People who fail to follow through on these conclusions are taken to not be

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[53] John Harsanyi, *Rational Behavior and Bargaining Equilibrium in Games and Social Situations* (Cambridge: Cambridge University Press, 1977) 16.

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fully rational. This line of thinking has been extended in ways that connect with a long tradition of contractarian accounts of ethics.

It is often said that rational choice is concerned exclusively with means rather than ends. It asks how people can best achieve their ends, and ends themselves lie beyond reason. Ethics, on this view, can be nothing more than a system of hypothetical imperatives – maxims that explain why "if you want x, you need to do y."[54] However, Contractarian and, later, evolutionary social theorists have tried to show that, upon further scrutiny, instrumental rationality demands that an agent be committed to certain ethical norms. Starting with only an instrumental notion of rationality (as well as an assumption about people's characteristic preferences), these theorists argue that ethical commitments become necessary for the long term satisfaction of preference. This is a way of trying to ground ethics in reason, and show that those who deviate from various forms of trust, reciprocity, and altruism are irrational – that is, they fail to maximize utility.

These accounts have met with a number of problems. Although it is easy to show why particular ethical norms are good for the group and often good for an individual, a purely instrumental account of rationality provides individuals no reason abide by norms when they could profitably deviate from them. Thus, these accounts do not provide a compelling answer the question of why not lie, cheat, steal, rape, murder when you can

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[54] For an explication of this perspective, Phillipa Foot "Morality as a System of Hypothetical Imperatives" *The Philosophical Review*, Volume 81, No. 3 (July 1972) .

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get away with it and stand to benefit. This problem surfaces in various forms when rational choice theorists model the details of this theory of ethics.

The political scientist David Axelrod argued in his widely praised book, *The Evolution of Cooperation*, that the cooperative strategy he called "tit-for-tat" was a superior to all others in a repeated series of prisoner's dilemma games. The strategy involved cooperating on the first move, and, on every subsequent move, repaying the other player with whatever move he or she selected in the previous round. The interpretation given to this strategy and result was that "cooperative" behavior (which stood ready to punished deviators) was the best over time and what rational people would choose. According to Axelrod, "What accounts for TIT-FOR-TAT's robust success is its combination of being nice, retaliatory, forgiving and clear. Its niceness prevents it from getting into unnecessary trouble. Its retaliation discourages the other side from persisting whenever defection is tried. Its forgiveness helps restore mutual co-operation. And its clarity makes it intelligible to the other player, thereby eliciting long-term co-operation."[55] On Axelrod's telling, tit-for-tat sounds like the game theoretic analogue of a good citizen. But is being a good citizen always the best strategy?

Axelrod implies it is. Indeed, he paints an extraordinarily rosy picture of the universal virtues of cooperation:

> It is encouraging to see that cooperation can get started, can thrive in a variegated environment, and can protect itself once established. But what is most interesting is how little had to be assumed about the individuals or the social setting to establish these results. The individuals do not have to

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[55] Robert Axelrod, *The Evolution of Cooperation* (New York, NY: Basic Books, 1985).

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be rational: the evolutionary process allows the successful strategies to thrive, even if the players do not know how or why. Nor do the players have to exchange messages or commitments: they do not need words, because their deeds speak for them. Likewise, there is no need to assume trust between the players: the use of reciprocity can be enough to make defection unproductive. Altruism is not needed: successful strategies can elicit cooperation even from an egoist. Finally, no central authority is needed: co-operation on the basis of reciprocity can be self-policing.[56]

Axelrod's conclusions are unfortunately unwarranted. Although it is true that two people playing the tit-for-tat strategy constitutes a Nash equilibrium in an infinitely repeated prisoner's dilemma, there are in fact many feasible Nash equilibria in this game, few of which have the nice cooperative implications of tit-for-tat. For example, a strategy of infinite punishment following the first defection can also constitute a Nash equilibrium (and the Folk theorem shows that in repeated games any outcome can be a feasible solution if it satisfies the minimax condition for each player, which indicates that there can be a game theoretic rationale for nearly any outcome in a repeated game). Axelrod also ran an actual tournament simulating this game in which people were asked to submit strategies that were then played against one another. The results appeared to vindicate tit-for-tat, but, as the game theorist Ken Binmore later pointed out, Axelrod's tournament results were in some sense rigged, and in any case not robust to a range alternative strategies.[57] Moreover, Binmore cites a number of simulations that show that the entry of "mean" players with exploitative strategies can destabilize the equilibrium of cooperative players and profit by so doing. So, both game theory and actual simulations cast doubt

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[56] Ibid., 174.

[57] See Binmore's review of Axelrod's The Complexity of Cooperation: Agent-Based Models of Competition and Collaboration (*Journal of Artificial Societies and Social Simulation* (1998).)

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upon Axelrod's rosy conclusion that cooperative behavior is instrumentally superior in the long run.

The philosopher David Gauthier has advanced a somewhat different argument about the way in which rational choice theory provides a justification for particular moral norms. Gauthier's basic claim in his 1986 book *Morals by Agreement* is that "rational choice enables us to state, with new clarity and precision, why rational persons would agree *ex ante* to constraining principles, what general characteristics these principles must have as objects of rational agreement, and why rational persons would comply *ex post* with the agreed constraints (10)."[58] However, the weakest part of Gauthier position is the argument he provides for the third part, the problem of why rational utility maximizers would comply with constraints. Gauthier introduces the notion of a "disposition to choose" which stands above and directs individual decisions. He claims that those who adopt the disposition of "constrained maximizers" (i.e. those who do not defect from bargains when they can benefit from doing so) can be expected to have more opportunities for cooperation, and thus preference satisfaction, than those who adopt the position of "straight forward maximizers." Ergo, more utility accrues to those who act in a constrained way, and it is rational to accept such moral constraints.

Gauthier's argument is unpersuasive for at least two reasons. The first is that his concept of a "disposition" obscures the fundamental question of why someone shouldn't exploit in the special case in which their deeds could go completely undetected, or the

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[58] David Gauthier, *Morals by Agreement*. (Oxford: Oxford University Press, 1986) 10.

special case in which the gains to be had far outweigh any future benefits of cooperation. Both of these are extremely plausible scenarios in the real world. If someone indeed could exploit without ruining future opportunities for cooperation, it seems clear that Gauthier's theory of rationality would advise him or her to do so. The same conclusion follows for situations in which there are enormous benefits to exploitation that overshadow future cooperation. Gauthier's treatment of this issue also takes for granted what is fundamentally an empirical question, namely whether people indeed segregate into these two dispositional types, with the "constrained maximizers" actually having more opportunity to profit over time. Even if this were true on average, it would not provide a rational warrant in the special cases.

A second weakness of Gauthier's account is that it presumes that there is some clear conception of what exploitation is. At the extremes, the distinction may be clear, but there are a host of situations where the distinction is essentially contestable. [59] Gauthier's idealized account of the long term average superiority of the disposition to cooperate ignores the fact that the moral standards that distinguish the "constrained maximizers" and the "straight forward maximizers" may not be clear.[60]

The role that dispositions are asked to play in Gautheir's account in fact motion towards the last common strategy for explaining the rationality of ethical convictions on

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[59] For an excellent summary and criticism of Gauthier's views see chapter 4 of B. Michael Byron Jr.'s dissertation *Rationality and the Paradoxes of Decision Theory: A Critique of Rational Choice Views*. I am indebted to him for the second point about the ambiguity of the notion of fairness.

[60] Consider the difference between paying tribute to the mafia for protection of one's business and paying taxes to the state for national defense. Would Gauthier's account consider the mafia graft exploitative and the government's not?

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the basis of instrumental reason, namely the turn towards evolutionary theories of moral sentiments. Ken Binmore has been one of the most lucid and provocative exponents of uniting rational choice and evolutionary biology to explain morality. On Binmore's account, ethical convictions ultimately need to be a matter of preference and not mere strategy, but evolutionary theory explains why the kind of ethical preferences that support long term cooperation become inscribed into human psychology at the level of preference. According to Binmore, "*Fairness* is the social tool *washed up* on the human beach by the tide of evolution for solving coordination problems."[61] The "fairness norms" that Binmore believes characterizes the ethical preferences to be found amongst all but the most abnormal human beings, written into our genome through evolutionary processes, are, on his telling, essentially captured by Rawls' intuitions regarding the original position. Binmore argues that rational choice, evolutionary theory, and biological research provide the key to making our study of morality a true science, and that science reveals that most people are, deep down, committed to a conception of justice as fairness on the Ralwsian model. I will explore, in the next chapter, challenges that have been raised to accounts like Binmore's. The marriage of rational choice and evolutionary theory has resulted in a number of rich conjectures. Progress in the biological sciences, however, suggests that the reality is more complex than envisioned by Binmore's speculations.

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[61] K.G. Binmore, *Natural Justice* (Oxford UK: Oxford University Press, 2005), 200.

In any case, Axelrod, Gauthier, and Binmore exemplify attempts to leverage instrumental reason in service of a scientific account of ethical convictions. Each, in their own way, argues that instrumental rationality requires certain normative convictions. And, we can see how difficulties in this project of describing and justifying ethical behavior through instrumental reason naturally leads towards evolutionary, biological accounts, which make instrumental reason the fulcrum for inscribing instrumentally useful ethical preferences into human nature. Even if ethical convictions cannot be defended as a straight forward artifact of instrumental strategy, evolutionary theory can suggest a quasi instrumental account of the origins of certain types of "ethical" preferences. However, in so doing, biological accounts of ethical preference further remove ethics from the domain of reason (understood non-instrumentally). I will argue in the sixth chapter that it is a mistake to view ethical convictions as a-rational, non-cognitive, hardwired preferences or as artifacts of purely instrumental reason. In contrast, I maintain that ethical convictions rely on conceptual judgments that are open to reasoned evaluation. This account provides an alternative to the scientific attempts to explain ethical convictions as artifacts of purely instrumental strategy or biological hardwiring. But more on that later.

4.5.3.1 Experimental Investigations of Ethics and Other "Irrationalities"

Leveraging instrumental rationality to account for ethical preferences has been one way that rational choice theorist have tried to transcend current limits and address the issue of ethics. The emphasis in these approaches is on what (instrumental) rationality

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requires. There is another direction research has taken, which de-emphasizes the importance of rationality. Rather than beginning with a concept of what rationality requires, this perspective follows in part Oskar Morgenstern's early characterization of game theory, namely: "In analyzing games, the theory does not assume rational behavior; rather, it attempts to determine what "rational" can mean when an individual is confronted with the problem of optimal behavior in games and equivalent situations."[62] On this account, rational choice theory should be concerned not with defending idealized accounts of what rationality requires, but on figuring out why certain strategies in fact prove favorable and widely practiced in light of various considerations. On one reading, this entails a more empirically oriented approach to rational choice theory. The aim of research in this tradition is to make formal models descriptive of how people actually choose. This approach is naturally more open to investigating how a range of possible preferences can serve as ends of action and it places a greater importance on empirically investigating the preferences people have. This approach is also interested in empirical investigations of "irrationalities."

The point of modeling in this tradition is to describe how people do behave, rather than to suggest how they should behave. If, upon, investigation, people in fact behave in ways that appear "irrational" then understanding and modeling irrationality becomes an important part of rational choice. As we saw, however, Elster claimed in his review that the social sciences are not well equipped to do this:

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[62] Oscar Morgenstern. "Game Theory: Theoretical Aspects." *In International Encyclopedia of the Social Sciences* vol.6. New York: Macmillan (1968:62).

The social sciences today, however, cannot offer a formal model of the interaction between rational and nonrational concerns that would allow us to deduce specific implications for behavior. As mentioned earlier, the idea of modeling emotions as psychic costs and benefits is jejune and superficial. The fact that emotion can cloud thinking to the detriment of an agent's interests is enough to refute this idea.[63]

But Schelling argues against this view, maintaining that:

Irrationality, or I should say 'irrationalities', plural, can be manageable in game theory as long as the nature of the particular 'irrationality' can be identified. For example, does one participant not understand the other's language; is one deaf; does one suffer from claustrophobia or some other debilitating phobia; is one a small child, or an elderly person suffering dementia; is one known to be susceptible to overwhelming rage; is one known to be subject to a particular superstition; does one suffer a form of amnesia; is one addicted to a substance; is one innocent of any statistical sophistication, incapable of thinking probabilistically; is one for the time being inebriated or under the influence of a sedative or other drug? Or, of course, both of them. And are either the 'irrational' individual, or the other party, or both, aware of the particular 'irrationality' and how it affects decisions? Camerer (2003) explores many ways that idiosyncratic behaviour can be accommodated in game theory.[64]

Similar to Sen, who envisions incorporating a richer range of preferences into utility functions, Schelling and Camerer envision expanding rational choice models to incorporate various non-rational influences on behavior. Both Schelling and Camerer would undoubtedly admit that whether this can be done is an empirical question, although they clearly believe it can be done in some respects. At first glance, the same reservations that apply to Sen's vision would seem to plague Schelling and Camerer's – the variety of

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[63] Jon Elster, "Rational Choice History: A Case of Excessive Ambition" *The American Political Science Review*, Vol. 94, No. 3 (Sep., 2000), 692.

[64] Thomas C. Schelling, "Game Theory: A Practitioner's Approach" *Economics and Philosophy*, 26 (2010): 45.

possible preferences and of possible irrational influences is so great that trying to understand and model them with sufficient resolution appears hopelessly complex. However, if irrationalities are rooted in universal features of human psychology it is plausible that irrationalities could be systematically understood.

Indeed behavioral economics research has identified a number of common psychological quirks that lead people to deviate systematically from traditional assumptions of rational choice theory. Researcher have documented loss aversion (people are much more sensitive to economic losses than gains), endowment effects (people will pay much more to keep an object they possess than they will to buy that object from someone else), winner's curse (people regret winning auctions because they think they have overbid), benchmark effects (people will respond differently to whole number prices- $10 vs $9.95- and often commit themselves to action based on symbolic price points- Dow at 10,000) and so on. [65] And if one wants to understand why, for example, housing markets behave so strangely in recessions the endowment effect and loss aversion provide important insights.

Researchers have looked to both cognitive and evolutionary psychology for an explanation of why such apparent "irrationalities" pervade human decision making. A common explanation is that the heuristics (or rules of thumb) that people employ are efficient ways to address the vast majority of decisions people face, even though they prove sub-optimal in rare circumstances. It would be too cognitively taxing to live up to

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[65] Richard H. Thaler, *The Winner's Curse: Paradoxes and Anomalies of Economic Life* (Princeton, NJ: Princeton University Press: 1994).

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pure ideals of rational choice all the time. Both our physiology and our laziness make us "predictably irrational," to use Daniel Ariely's term. Recognizing psychological quirks can help improve the predictive quality of models and aid in structuring social institutions such that they are cognitively manageable.[66] Such research can also attune us to the ways in which others might prey on our psychological weaknesses (as casinos arguably do). Indeed, it is useful for us to be alerted to the ways in which we are not the maximizing, rational actors idealized by early forms of rational choice theory.

There likely are cognitive and biological bases for systematic irrationalities. The way researchers investigate such irrationalities is through the use of experiments meant to tease out and characterize these quirks. I will examine in greater length in the next chapter the promise and perils of such research. However, I want to flag the particular problem of investigating "trust, reciprocity, and altruism" and other "ethical preferences" within an experimental framework, which now constitutes a major wing of research in behavioral economics. This research assumes there is a constant meaning and structure to ethical preferences, which is almost certainly untrue. Experimental research is unlikely to be able to understand the variety, complexity, and dynamism that characterize ethical preferences. This is because ethical preferences take shape in reference concepts and judgments shaped by language, culture, education, and such, which are not amenable to experimental control.

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[66] Again, John Staddon's article "Distracting Miss Daisy" (*The Atlantic* (July/August 2008)) provides a good example of the importance of taking cognitive limits into consideration in designing social institutions such as traffic signs.

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A few features of experimental research into "trust, reciprocity, and altruism" are worth nothing. This research typically conceives of ethical preferences as hardwired character traits, cemented either through biological dispositions or structural factors (such as class or age). I have documented in my previous work how, for example, dispositions to "trust" are considered to be inflexible character traits both by experimentalists and by many working on problems of trust in comparative politics.[67] In this way of looking at things, ethical preferences appear as fundamentally a-rational, and it should not surprise us that experimentalists investigate such preferences in the same way they investigate "irrationalities." Since ethical preferences are not explicable in terms of instrumental reason, they tend to be understood as forms of irrationality – or, at any rate, something beyond reason. Like tastes, they can only be taken as given. But this perspective depends on having accepted that instrumental rationality exhausts reason. It is precisely this notion of reason that I will challenge in the sixth chapter.

The descriptive, empirical turn in rational choice theory ends up in the same place as the normative turn, conceiving as ethical preferences as ultimately explicable as a kind of biological hardwiring. There are, however, good reasons to resist thinking of ethical preferences in this way – a way that suggests they are not corrigible or open to reasoned persuasion. Indeed it is very odd to think of them in this way. It seems to contradict our phenomenological experience of having seen persuasion in practice moving ourselves and others to different convictions. It likewise makes our historical understanding of the

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[67] See my "Demystifying Trust: Experimental Evidence from Thailand and Cambodia"

range of ethical convictions expressed in different cultures and times almost unintelligible. What, then, makes this reductionist program plausible? The key, I believe, lies in its scientific appeal. If preferences were able to be exhaustively understood, in biological or other terms, this would hold the promise of finally providing a foundation for the social sciences. That is, a reductive account of human preference and values would be an important cornerstone of an absolute conception of science. The onus of the argument, however, is upon those who believe biological reduction is possible. In the next chapter I examine the current state of this research and suggest reasons why the reductionist hope is likely to go unfulfilled.

4.6 Conclusion

Rational choice theory has proved extremely useful in a narrow range of circumstances, namely those in which preferences and constraints/beliefs are well known. Unfortunately for rational choice theorists, these conditions are rarely satisfied with regard to many important social phenomena. Although rational choice models can occasionally offer useful analogies and insights into such phenomena, the rational choice approach is intrinsically limited by the complexity and dynamism of human motivations. Rational choice illustrates the importance of motivations while being incapable of saying much about them. This presents a particular problem in contexts where motivations of agents are diverse or foreign, as is often the case in trying to model the behavior of peoples in the developing world. Rational choice theory shows why ethical persuasion could be important for shaping behavior and building institutions, although the theory's

purely instrumental account of rationality provides inadequate resources for understanding and engaging in such persuasion.

Although behavioral research has increasingly shed light on the systematic sources of "irrationality" in human psychology, the central question of the origins and nature of ethical convictions will likely not be illuminated by such research. Attempts to understand "preferences" within a purely scientific framework are misguided. The questions raised by rational choice theory in reference to complex problem of social structure require nothing less than a full blown account of practical rationality. Rational choice indicates a kind of shortcut, a way of dispensing with deeper accounts of practical rationality, in the special cases where motivations, beliefs, and constraints are well known. However, the kind of account of practical rationality needed to understand human action more generally is one that takes note of the conceptual development of motivations, and which thus depends on skills of interpretation and historical understanding. Freudian psycho-analysis is perhaps the closest that any self-consciously "scientific" approach has come to a larger conception of practical rationality, but philosophical account of practical rationality go back at least to Aristotle.

The idea of intentional action that lies at the heart of rational choice theory is ultimately not that different from Aristotle's. People act to achieve what they perceive as good. The all important question, though, is where do perceptions of good come from? Although human nature, which is to say human biology, is a necessary starting point for answering this question, the turn towards biological research to provide a complete, scientific account of human motivations is a project inspire by the absolute conception of

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science, which I ultimately expect is unrealizable. In the next chapter I offer a critical evaluation of the methods and current status of biological-behavioral research. In the chapter following that, I sketch out an alternative, non-reductive, non-scientific account of the nature of ethical convictions – one that suggests both why such convictions will never be fully captured by scientific methods and how it is that ethical persuasion is possible.

5. Biological-Behavioral Research

"He who understands the Baboon would do more towards metaphysics than John Locke."

- Charles Darwin *Notebook M* [1]

"Given an hour of a man's life and an anthropometric seraph could calculate all that he has ever been and all that he will ever be"

- Mortimer Collins, *Marquis and Merchant*[2]

"All we need, something which can be given us only now, with the various sciences at their present level of achievement, is a *chemistry* of moral, religious, aesthetic ideas and feelings, a chemistry of all those impulses that we ourselves experience in the great and small interactions of culture and society, indeed even in solitude. What if this chemistry might end with the conclusion that, even here, the most glorious colors are extracted from base, even despised substances? Are there many who will want to pursue such investigations?"

- Nietzsche, *Human, All Too Human*[3]

"The astonishment which I felt on first seeing a party of Feugians on a wild and broken shore will never be forgotten by me, for the reflection at once rushed into my mind—such were our ancestors. These men were absolutely naked and bedaubed with paint, their long hair was tangled, their mouths frothed with excitement, and their expression was wild, startled, and distrustful. They possessed hardly any arts, and like wild animals lived on what they could catch; they had no government, and were merciless to every one not of their own small tribe. He who has seen a savage in his native land will not feel much shame, if forced to acknowledge that the blood of some more humble creature flows in

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[1] Charles Darwin,: *Notebook M* 1838 (http://darwin-online.org.uk/EditorialIntroductions/vanWyhe_notebooks.html)

[2] Mortimer Collins *Marquis and Merchant* London 1871 vol.3 141 (quoted in Ian Hacking *Taming of Chance* 180).

[3] Friedrich Nietzsche. *Human, All Too Human: A Book for Free Spirits* in *The Nietzsche reader,* Volume 10 edited and translated by Keith Ansell-Pearson, Duncan Large. (Wiley-Blackwell, 2006) 161.

his veins. For my own part I would as soon be descended from that heroic little monkey who braved his dreaded enemy in order to save the life of his keeper, or from that old baboon, who descending from the mountains, carried away in triumph his young comrade from a crowd of astonished dogs- as from a savage who delights to torture his enemies, offers up bloody sacrifices, practises infanticide without remorse, treats his wives like slaves, knows no decency, and is haunted by the grossest superstitions."

-   Darwin, *Descent of Man* [4]

5.1 Introduction to Biological-Behavioral Research

In the previous chapter I showed why research into the biological foundations of human behavior constitutes the final frontier in the search to make the social sciences truly scientific from an "absolute" perspective. Since we are ultimately biological creatures, many hope that a full understanding of biology will reveal not only the sources of psychological disease and various forms of irrationality, but the nature of rationality itself. This will necessarily include a reductive biological account of human values, which can explain where perceptions concerning desirability of goods come from.

Of course, some human behaviors can be explained biologically in terms of stimulus-responses systems. Someone's hand touches a hot stove and she recoils immediately. But actions mediated by consciousness have thus far not been explicable on such a model. Even though biological factors can clearly influence or diminish consciousness, such as certain drugs do, consciousness itself and the conceptual judgments expressed and mediated by language have not admitted of mechanistic

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[4] Charles Darwin. *Descent of Man, and Selection in Relation to Sex* (Princeton University Press, 1981) 404-405.

reduction. However, many believe such reduction is possible in principle and something which biological research increasingly promises to provide. If indeed values are ultimately determined by mechanistic biology, understanding this process would finally provide a scientific purchase on intentional human behavior. This, in turn, could enable the social sciences to achieve a genuinely absolute perspective.

The grand hope is that human consciousness could ultimately be understood in biological terms, although there are serious philosophical questions about what this could mean. In the meantime, the more proximate hope is that a wide range of human behaviors, and perhaps even ethical convictions, could be understood or predicted through biological data. Whatever the merits of the philosophical positions for and against the possibility of ultimate reduction, a detailed evaluation of the state and methods of current biological-behavioral research programs suggests they are far from providing anything like a biological account of ethical convictions.

That is not to suggest, however, that biological-behavioral research cannot be or has not been useful. Biological research increasingly reveals insights into the nature of our physiological liabilities, particularly with regard to various pathologies that affect the brain and can lead to psychological deficits. Research has also shown how biological factors influence conscious judgments at the margin, and has identified biological correlates of behavior than can be useful for behavioral predictions in certain circumstances. However, despite these genuine and useful insights, the methods and current discoveries of biological research give little reason to believe that we will be able to master the workings of the human mind through some physiological account. People's

convictions about the good must, for the most part, be understood in terms of concepts and judgments that are intelligible only within the interpretive framework of language, culture, and history. Although the human brain is a biological system it somehow gives rise to the human mind, which transcends any current account we have of mechanistic causality. Understanding the nature of ethical convictions and their openness to persuasion requires approaching them through linguistically mediated mental categories such as intentions, beliefs, and concepts. Although our biological nature provides a starting point for reflections on value, such reflections move beyond innate or instinctual desires, and indeed may shape desire and perception at a deep level. That at least is part of the argument I advance in the next chapter.

This chapter proceeds as follows: I first offer an overview of the research methods at the forefront of current revolutions in biological knowledge, namely gene sequencing and brain imagining technologies. These methods have opened up entirely new areas of inquiry and have fueled hopes in the possibility of understanding human behavior in purely biological terms. Second, I offer a critical examination of recent research on the biology of decision making. I focus in particular on questions regarding the nature of rationality, the sources of human aggression/conflict, elite decision making, and ethics. Third, I examine in detail attempts to use brain imaging technologies to detect lies. This provides an instructive example of how some have tried to make biological research practically useful in service of problems related to ethics. Finally, I offer a summary appraisal of the methodological limits and current state of biological-behavioral research. While admitting the extraordinary insights such research is likely to provide about the

sources of irrationality and physiological deficits (diseases, etc.), I argue that we have little reason to believe biological research will provide a way of bypassing the conceptual constitution of ethical convictions and the need for ethical persuasion.

5.2 New Frontiers in Biological Research

The hope that biological research can complete the social sciences is spurred in part by current revolutions in biological knowledge and technological developments in biological research methods. Gene sequencing and brain imaging technologies have been the driving motor of the current revolution in biological knowledge. Each has opened up new areas of biological inquiry that were unthinkable only a generation ago and dramatically contributed to advances in molecular biology.

5.2.1 Genetics

The human genome contains some three billion nucleotide base pairs comprising over 20,000 genes, which together form the blueprints of human life. The genetic architecture we inherit from our parents is ground zero of human nature. Although our genetic endowment interacts with and is modified by the "environment" in countless ways throughout our lifespan, it is an obvious place to start looking for explanations of disease and behavior. Indeed there are a many diseases that are well characterized by their genetic causes and inherited in classic Mendelian fashion - sickle cell, Huntington's disease, cystic fibrosis- and others that appear to be strongly influenced by particular gene mutations - breast cancer, colon cancer, Alzheimer's. Also, various genetic conditions are known to cause or contribute to certain psychological and behavioral

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deficits, such as Down syndrome, fragile X, and Lesch Nyhan syndrome. When it first became clear that advances in gene sequencing techniques would enable researchers to map the entire human genome there was considerable excitement in the hope that we would discover simple genetic explanations for a wide range of diseases and that the explanatory power of genetics could extend to patterns of human behavior, psychological dispositions, and character traits.

Unfortunately, progress in disease genomics has been slower going than many envisioned. Although our genetic architecture must necessarily be part of the story of how diseases arise in conjunction with environmental interactions, the degrees of complexity involved are increasingly understood to be far greater than previously imagined. The classic model of Mendelian inheritance, in which a single trait (or phenotype) is caused by a single gene (or gene variant, called an allele), is the exception rather than the rule when it comes to understanding the origins of most diseases. These may depend on multiple gene interactions combined with extensive environmental influences, not to mention complicated dynamics at the level of epigenomics and ongoing mutations. To take but one example, schizophrenia is highly heritable and thus should presumably be an ideal candidate for genetic analysis. However, studies have repeatedly failed to identify any particular genetic variations significantly associated with the disease.[5] More recently, researchers have suggested that there may be many small, random mutations that occur on dozens of various genes involved in brain function, any

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[5] Riley, B. and Kendler, K. "Molecular Genetics of Schizophrenia" in *Neurobiology of Mental Illness* (Charney and Nestle eds.) (Oxford: 2005) 247-262.

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of which can produce symptoms classified as schizophrenia.[6] If true, this tremendously complicates attempts to pharmacologically ameliorate the genetic sources of "schizophrenia," as there may be hundreds of different, particular mutations that lead to the disease - or, to be more accurate, hundreds of discrete diseases.

If understanding the genetic sources of disease is complicated, investigating the genetic sources of human behavior promise to be even more complex- although there have been notable discoveries (discussed below). To statistically enable the kind of studies that could hope to tease out higher orders of complexity requires massive amounts of data. If gene sequencing had remained as costly as it was at the beginning of the human genome project, which by its completion in 2003 had cost some $3.3 billion, it would be prohibitively expensive to pursue large genetic studies. However, one of the most important developments for genetic research has been the exponentially decreasing costs of gene sequencing. In 2007 two individuals had their entire genomes mapped at a cost of a million dollars each. By 2008 this cost had decreased to $60,000 each for a number of research participants whose genomes were mapped in under a month. One company currently offers the service for $5,000, and many believe a $1,000 genome is around the corner.[7] In the meantime it is possible to sequence targeted areas of interest in the genome (examining particular "single nucleotide polymorphisms"-i.e. genetic

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[6] Duke Medicine News and Communications, "Schizophrenia Genetics: Evidence Fingers Emerging Class of Culprit" (http://www.dukehealth.org/health_library/news/schizophrenia_genetics_evidence_fingers_emerging_class_of_culprits)

[7] Aldhous, Peter. "Genome sequencing falls to $5,000" in *New Scientist.* 6 February, 2009.

variations) for a small fraction of that cost. Already there is a flood of genetic data coming down the research pipeline, and we will increasingly have access to an expanding universe of genetic information matched to varieties of survey and behavioral data. However, only time will tell if more complex and informative genetic relationships can be discovered through statistical innovations utilizing larger and more refined data sets.

A few immediate consequence of the genomic revolution are worth noting. First, it has refocused debates about human nature on the phenomenon of variation. There have always been significant controversies in the anthropology, sociology, psychology, and philosophy concerning whether human nature should be understood as something fundamentally universal (and, if so, the basis for that universality) or segregated into meaningfully different types (ranging from caste hierarchies to neutral, complementary diversities). Positions in these debates fall along a spectrum, but our increasing recognition of the amount of genetic variation present (or possible) in the human species has led many to re-conceive of human nature as itself fundamentally diverse. This has in turn led to a renewed interest in identifying "character types" with an eye towards establishing their roots in human genetic variations. Also, this perspective has led many to suspect that explanations of abnormal/deviant behavior are to be found at the genetic level (or in physiological states derivative of particular gene-environment interactions). So, on this view, ethical convictions can appear either as something universal inscribed in human biology at large, or as something peculiar linked to particular genetic variations or environmental influences.

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Second, although studies that associate genetic variations with diseases and behavior tend to garner the most publicity, perhaps the most important consequence of genomic research has been its contributions to our knowledge of fundamental biology, particularly at the molecular level. We still have only the faintest idea of how numerous processes in a cell hold together, but the ability to examine and manipulate genes has been a tremendous engine for fundamental biological research in this and other areas.

Finally, increasing recognition of the complexity involved in our genetic architecture has also illuminated the limits of "purely" genetic explanations of the human organism. Genes are always part of the story, but they are seldom the full story. Our genetic architecture puts in motion biological systems that recursively interact with and are modified by their environments and which reflect many stochastic influences as well. This is particularly the case with the most complex and plastic of genetically engineered organs, the human brain.[8]

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[8] In this chapter I mostly ignore the subfield of "evolutionary psychology" which is concerned with suggesting evolutionary hypotheses that explain why people think and act like they do. Although there is a voluminous literature on the subject, I find most of it essentially speculative (indeed bordering on the genre of fiction) and in any case of little practical value for any debates in the social sciences. Whether or not our biology is as it is because of particular evolutionary pressures on the savannah 30,000 years ago is moot. Our biology is what it is and we have various methods to directly investigate its current configuration. Although it may be interesting to speculate about what forces lead to the evolutionary emergence of the human brain, I see such speculations contributing very little of practical value to problems that the social sciences deal with. However, for an interesting claim about the "social" influences on human brain development see Drew Bailey & David Geary "Hominid Brain Evolution: Testing Climatic, Ecological, and Social Competition Models" *Human Nature,* March 2009, Pages 67-79.

5.2.2 Brain Imaging

Research at the intersection of "Mind, Brain, and Behavior" has also been subject to radical advances in recent years driven by technological developments. Various imaging and measurement techniques such as electroencephalograms (EEG), positron emission tomography (PET), and functional magnetic resonance imagining (fMRI -which will be examined in more detail below), have granted a window of sorts into the brain. They promise to shed new light, not only on various brain pathologies, but also on the very nature of human perception, decision making, and consciousness. These technologies bring us much closer to understanding the biological dynamics immediately underlying human behavior and, like genomic technologies, they have spurred an enormous quantity of new research. According to one estimate, in 2007 about eight peer-reviewed articles employing fMRI were published per day.[9] Neurological research has also greatly expanded our understanding of chemical pathways and systems in the brain, illuminating the influence of neurotransmitters (e.g. dopamine, norepinephrine, and serotonin), hormones (e.g. cortisol, testosterone, oxytocin) and other chemicals on brain function and human behavior.[10]

Understanding the human brain is the holy grail of biological research. Going forward, deep debates about the relationship between the mind and the brain will

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[9] Jonah Lehrer, "Picture Our Thoughts: We're Looking for Too Much in Brain Scans," The Boston Globe (August 17, 2008). Cited by Selim Berker, "The Normative Insignificance of Neuroscience" *Philosophy and Public Affairs* 37 (2009) 293-329.

[10] For a good overview see Blaustein, J. and Lajtha, A. *Handbook of Neurochemistry and Molecular Neurobiology: Behavioral Neurochemistry and Neuroendocrinology* 3rd Edition, (Springer: 2006).

undoubtedly persist and become richer, but many of the findings in neuroscience will prove useful regardless of how these deeper debates develop.

5.3 Implications of Biological Research for Understanding Human Rationality and Ethics

Perhaps no concept in the social sciences has been more heavily influenced by biological research in recent decades than "rationality." The history of this concept is complex, but to a large extent the social sciences in the 20th century treated rationality as a normative ideal. Rationality could mean consistency in preferences (reflexivity, transitivity, and completeness), the use of probability theory to deal with risk, Bayesian updating from past experiences, or even scientific induction more generally. Rationality, thus construed, had to do with whether people chose the best means to instrumentally achieve their goals and the logical consistency of those goals. Irrationality was something that should not persist because it is suboptimal from an agent's perspective and enables exploitation by others. Thus, "rational behavior," subjectively defined, could be expected from most people, and on the basis of such expectations social scientists could make useful predictions.

However, behavioral research of the last few decades increasingly demonstrated that people often don't adhere to normative ideals of rationality. Rather, people exhibit inconsistent preferences, perceptual biases, inefficient strategies, and so on. This led to the development of more sophisticated accounts of rationality that attempted to explain apparent irrationality by deeper rational considerations. These included accounts such as

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rational ignorance, bounded rationality, satisficing (accepting suboptimal outcomes in light of the disproportionate costs of achieving optimal ones), and minimaxing (minimizing the maximal potential for loss). Although there does appear to be some method to the apparent madness of many human behaviors – which is to say we are often "predictably irrational" to use Daniel Ariely's term – biological research has shown why it is likely a mistake to think there is a universal, single, underlying unity to human decision making.[11] Although there may be some conceptual unity to an agent's beliefs, goals, and strategies, our physiology also makes us liable to various deviations from intellectual coherence. The traditional catalog of vices - lust, gluttony, sloth, anger, envy, greed, pride, etc - is not a bad place to start looking for manifestations of our biological liabilities. Moreover, many of these liabilities have a good biological rationale. We need biases, heuristics, and dispositions in order to cognitively manage our world. The question for social scientists is how exactly these liabilities systematically influence social phenomena, and in situations where this is the case how to predict and mange these influences.

5.3.1 Decision Making and Theories of the Brain

The range of research investigating the neurobiology of decision making and brain genomics is vast. Many neuroscientists have presented evidence for modular theories of the brain, according to which certain functions are localized in different brain regions. Antonio Damasio has popularized much of his own work that highlights the

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[11] Daniel Ariely. *Predictably Irrational: The Hidden Forces that Shape Our Decisions*. (Harper Collins, 2008).

ways in which emotional centers of the brain are engaged in different cognitive tasks.[12]

He argues that "rationality" often depends on significant emotional contributions, which provides a key to understanding many behavioral abnormalities and paradoxes of reason.

Others, such as William Uttal, have cautioned against the temptation to reduce brain processes to specific regions and modules, arguing that cognition is always a process that involves the whole brain.[13] His defense of distributed processing places a greater emphasis on the complexity and plasticity of the brain. However, many of the most interesting and robust findings in neuro-chemistry hold regardless of one's theory of brain organization. These include associations between sex hormones (testosterone, estrogen) and aggression[14], neurotransmitters (dopamine, serotonin) and depression[15], oxytocin and trust[16], cortisol and stress[17] -to name some of the more prominent discoveries.

5.3.2 The Biology of Decision Making and Problems of Politics – Aggression, Conflict, and Violence

The biology of decision making has a special relevance to the study of aggression,

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[12] Damasio, A. *Descartes' Error: Emotion, Reason, and the Human Brain*. (Penguin: 2005).

[13] Uttal, W. *The New Phrenology: The Limits of Localizing Cognitive Processes in the Brain*. MIT:2003

[14] McDermott, R. et al. "Testosterone and Aggression in a simulated Crisis Game." In *The ANNALS of the American Academy of Political and Social Science* 2007; 614

[15] Willner, P. "Dopamine and Depression" in Dopamine in the CNS Vol 2 (DiChiara ed.) pp.387-416 Springer: 2002.

[16] Zak, P. et al. "Oxytocin is associated with human trustworthiness" in *Hormones and Behavior*, 48(5), 2005. pp. 522-527.

[17] Vedhara, K. "An Investigation into the relationship salivary cortisol, stress, anxiety, and depression" in *Biological Psychology* 62(2), 2003. pp. 89-96.

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conflict, and violence, particularly with regard to problems in Comparative Politics, Development Economics, and Security Studies. Decisions to "fight," whether in the context of interstate wars or local riots, have provided social scientists with the greatest paradoxes of rationality. According to a prominent line of thinking in the tradition of rational choice analysis, fighting doesn't make sense. Since fighting imposes tremendous costs, and one side generally loses, both parties (but particularly the weaker) should be able to negotiate a better outcome ex ante. James Fearon's classic article, "Rationalist Explanations for War," forcefully explored this paradox, suggesting that few mechanisms can account for war on strictly rationalist terms and those that can depend in various ways on incomplete information.[18] While there may be rational reasons that complete information is difficult to obtain, there are also plenty of "irrational" factors that might obstruct information as well. Thus, for both rationalist and non-rationalist theories of conflict, one way of explaining why war happens is to point to information asymmetries and misperceptions. In retrospect, it is often quite clear that at least one party made a miscalculation about the enemy's strengths or interests. History also suggests that personalities, tempers, and shifting sentiments of honor and vengeance can fuel misperceptions and short-sighted decisions in strategic contexts.

Robert Jervis famously investigated the sources and nature of misperception in international politics while demonstrating their profound impact on the course of world

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[18] Fearon, J. "Rationalist Explanations for War" *International Organization*. 49(3) Summer, 1995. pp. 379-414

events in his 1976 book, *Perception and Misperception in International Politics*.[19] Many believe that biological research will further illuminate the systematic cognitive and perceptual biases that Jervis documents. The genetic and neurological foundations of such biases should become clearer, and with this understanding many hope we can better guard against their detrimental influences. However, an examination of experimental research meant to explore the kinds of questions Jervis raises shows that there are a number of methodological hurdles to realizing a genuinely useful biological account of these problems.

In the introduction to *Perception and Misperception* Jervis noted that psychologists were already working on similar issues surrounding the nature of perception. However, he identified five major faults in the psychological literature that made its findings of questionable use for problems of elite decision making and diplomacy. I believe at least four of these apply with almost equal force to much of the current research in neuro-decision theory and behavioral genomics.

The first of Jervis's complaints was that more attention is paid to "emotional rather than cognitive factors" in explaining human behavior. Research such as Damasio's complicates this accusation - if emotion and cognition are two sides of the same coin, then it would be hard to neglect the study of the latter in favor of the former. However, I believe the majority of biological-behavioral research today focuses on what we might generally describe as "semi-cognitive" factors - factors that may influence decisions and

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[19] Jervis, R. *Perception and Misperception in International Politics*. Princeton: 1976.

reasoning but do not have any appreciable connection with larger belief systems or conceptual frameworks. This is only a slight concern, as I expect there is much to learn from the nature of sub-cognitive influences, but one of the challenges for bio-behavioral research going forward will be its ability to say something meaningful about the "ideational" sources of human action.

Jervis's second charge is that data are derived from laboratory experiments that are extremely remote from processes of interest in the real world. Indeed, experimental protocols typically deal with very small stakes and use highly artificial simulations that bear only the slightest analogy to the phenomena researchers would ultimately like to explain (consider the research on the "dictator gene" discussed below). The external validity of laboratory findings is a concern across the board in social science experiments, but the problem is particularly pronounced when dealing with the unique circumstances of diplomatic crises.

Jervis's fourth concern (setting aside his third concern with policy bias), is that research often disregards the institutional settings and particular dangers/opportunities of political circumstances, and thus threatens to "over psychologize" (we might say over-biologize) people's decisions when there are much simpler explanations at hand in terms of beliefs, interests, and political realities. This concern expands upon the previous one questioning the portability of findings, but also explains why seemingly robust findings may not be portable. There are many instructive illustrations of this problem in behavioral finance.

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Systematic biases in economic behavior found in the general population (overbidding in certain types of auctions) are often not found in seasoned traders, who are subject to strong arbitrage pressures to rationalize their decisions and thus have learned not to pursue low probability bets.[20] Although behavioral economics has shed a great deal of light on the nature of animal spirits in financial markets, it is less likely to be useful for predicting the unique, high stakes economic decisions of business leaders. Likewise, when an eminent primate biologist recently suggested in a talk that Bush's invasion of Iraq was nothing more than an "in-group out-group" reaction of an alpha male, his perhaps tongue in cheek comment exemplified Jervis's concern that overly psychologized (biologized) explanations could exclude important dimensions of reality.

Jervis's fifth and most serious challenge to the psychology literature of the day was that most theories of behavior "did not account for the ways that highly intelligent people think about problems that are crucial to them." In some sense this criticism does not transfer well to biological research. Biological influences on thought and behavior may not be apparent to individual agents or reflected in prior beliefs about how they reason. Biological research is likely to help clarify the role of dispositions in ways we could not have explored without recent advances in bio-metrics and statistical analysis. Moreover, decisions concerning conflict likely do engage deep emotions such as anger,

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[20] See Burns, P. "Experience in Decision Making: A comparison of students and businessmen in a simulated progressive auction" in Research in Experimental Economics (Smith ed.) JAI: 1985- as well as Haigh, M. and List, J. "Do Professional Traders Exhibit Myopic Loss Aversion? An Experimental Analysis" in *The Journal of Finance*. 60(1), 2005. pp. 523-534. Interestingly, the habitual risk aversion of traders often leads them to make less in laboratory settings than naïve participants.

fear, and vengeance. Thus, even highly intelligent people may exhibit sensibilities that present themselves as matters for further biological inquiry.

Yet, Jervis's challenge does raise most directly the lurking question of how insights into biological influences and dispositions relate to intelligent thought. It is one thing to know that certain hormones make people more aggressive, but quite another to assert that such hormones are the reason that Hitler sought territorial expansion or Chamberlain sought appeasement. Of course, findings about the biological bases of aggression are highly suggestive of such links, but connecting those dots straight away is likely overly reductive.

Although it is reasonable to conjecture that, lacking a certain physiological/genetic makeup, Hitler would never have sought territorial expansion in the first place, this does not necessarily tell us why Hitler sought territorial expansion. Was his ostensible "reasoning" irrelevant to his decision? Somehow our understanding of biological influences and dispositions must also include space for the influence of more complex conceptual judgments when such judgments clearly play a supporting role as well.

5.3.2.1 Experimental Research – Trust

Take, for example, the extensive research done on the hormone oxytocin. Known to be involved in "pair bonding" in mammals, a number of research teams have found that in economic trust experiments, people who exhibited more trusting behavior also had

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higher levels of oxytocin circulating in their blood.[21] Moreover, in separate trials researchers were able to induce higher levels of trusting behavior in subjects by administering them oxytocin beforehand. The differences were not enormous, but they were significant and reproduced in a number of studies. Many have interpreted these findings as proof that trust is to be explained in terms of a-rational biological forces. Interestingly, many of the behavioral effects of oxytocin (increased risk taking, gregariousness, sociability) are similar to those of alcohol (with the exception of intoxication), and recognizing such influences can obviously have strategic advantages- it is no mystery why casinos serve free drinks. Yet it would be premature to conclude that the behavioral manifestations of elevated levels of oxytocin or ethanol equate to what we commonly mean by "trust."

First, with regard to hypothetical utility of pharmacological manipulation, I would venture to guess that neither of these chemicals would, if administered to rival factions, be successful in securing trust in cases where differences in interest are profound and well known, such as the Israeli-Palestinian conflict or hostilities between Al-Qaeda and the United States (although using oxytocin in the interrogation of Al Qaeda detainees would be a very interesting, if ethically questionable, experiment). At a deeper level, there is the basic question of how brain chemicals themselves not only drive but also

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[21] Kosfeld, M. et al. "Oxytocin increases trust in humans" in *Nature*, 2 June, 2005. pp. 673-676.
 Zak, P. et al. "Oxytocin is associated with human trustworthiness" in *Hormones and Behavior*, 48(5), 2005. pp. 522-527. Zak, P. et al. "Oxytocin increases generosity in humans.: *PLoS One*, 2(11) 2007.

respond to cognitive judgments. We are apt to think of oxytocin in terms of its causal influence on trust rather than as a mediating variable between cognitive judgment and biological affect. However, research has also shown that the *experience of being trusted* apparently raises oxytocin levels on its own. Thus, cognitive judgments appear to retain some influence over components of what otherwise seems a reductionist account of the nature of trust. Much current research is focused on identifying relationships between biology and behavior at a very general level, but for these findings to be of use we will have to explore the details of these relationships with much more nuance and detail. Disentangling the interplay between "mechanistic" biological influences and higher capacities of the human intellect promises to be a very complex enterprise.

I do not wish to downplay the depths to which biological factors influence our attitudes and behaviors. Because we are biological beings, it is of course a truism that our biology matters, and in colloquial terms there are reasons to believe that "strong" biological factors are involved in decisions and behaviors relating to violence and conflict. The simple fact that a high proportion of crime in developed countries (~50% by the estimation of some studies[22]) is committed under the influence of drugs or alcohol is highly suggestive of this, as is the fact that the vast majority of crime worldwide is committed by males between the ages of 15-35.[23] Moreover, attitudes and behaviors that appear unusual or unintelligible seem ripe for biological explanations. However,

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[22] For example, see this report by the Australian government
(http://www.health.gov.au/internet/drugstrategy/publishing.nsf/Content/mono64-l~mono64-l-ch5)

[23] The UK's statistics are representative of many countries'
(http://www.statistics.gov.uk/CCI/nugget.asp?ID=1661)

conceptual judgments also shape human behavior and these are extremely difficult to account for in reductive biological terms.

5.3.2.2 Experimental Research – Personality Types and Aggression

The ability to conduct genetic association studies, along with an increasing recognition of the degree of human genetic diversity, has resurrected old paradigms in psychology that sought to classify people into various "personality types." Perhaps the most classic construct of this sort, the "authoritarian personality," was criticized for its conceptual imprecision, which in retrospect seemed a way to pathologize the attitudes of certain ideological positions. Contemporary research in behavioral genomics has access to larger and more precise data sets, although conceptual precision in defining "phenotypes" continues to be a challenge.

It is, *prima facie*, reasonable to ask whether people are biologically disposed to be aggressive or passive, violent or peaceful, risk loving or risk-averse, realist or idealist, sadists or saints. However, it is important to recognize how much conceptual baggage comes with trying to define and measure these "types." Upon scrutiny it is hard to imagine how some of these terms could be meaningfully understood biologically. What counts as aggression or realism will reflect judgments about which there may be substantial disagreements. Also, there is an inherent trade-off between generality and particularity in studying these concepts. From an evolutionary perspective there are reasons we might expect some people to be quicker to anger than others. But is undoubtedly a mistake to treat anger as if it were a simple property that will be manifested in the same or similar ways in all possible contexts. Precisely how such a

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disposition manifests itself will likely be highly sensitive to social mores, childhood upbringing, legal institutions, and cathartic outlets - not to mention intermediate biological influences from diet or drugs. For such reasons, it is *prima facie* unlikely that we'll find special biological dispositions underlying ever more discrete categories of attitudes/behavior - the anger of Americans after 9-11, the anger of peace activists over the Iraq invasion, the anger of North Carolinians over the decision to relocate Guantanamo detainees to Illinois. Attempting to find a particular biological disposition behind every human behavior resembles the old scholastic mistake of multiply universals, inventing a new generalization for every special case we encounter.

Consider, as illustrative of many of the concerns expressed above, the case of the "ruthlessness gene" reported by *Nature* in April of 2008.[24] Science writer Michael Hopkin authored this news release, entitled "'Ruthlessness gene' discovered: dictatorial behavior may be partly genetic, study suggests," which was accompanied by photos of Adolf Hitler, Robert Mugabe, Saddam Hussein, and Benito Mussolini. It reported the main findings of a then forthcoming article in the journal *Genes, Brains, and Behavior*.[25] According to Hopkin, "The study might help to explain the money-grabbing tendencies of those with a Machiavellian streak — from national dictators down to 'little Hitlers' found in workplaces the world over." Hopkin interprets the experimental protocol of the

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[24] Hopkin, M. "'Ruthlessness gene' discovered: Dictatorial behaviour may be partly genetic, study suggests" *Nature News*. 4 April, 2008.

[25] Knafo, A. et al. "Individual differences in allocation of funds in the dictator game associated with length of the argininevasopressin 1a receptor RS3 promoter region and correlation between RS3 length and hippocampal mRNA" in *Gene, Brains, and Behavior* (7) 2008. pp. 266-275.

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study- the "dictator game" - as giving participants two basic options: "behave selflessly, or like money-grabbing dictators such as former Zaire President Mobutu, who plundered the mineral wealth of his country to become one of the world's richest men while its citizens suffered in poverty." Indeed the study did employ the "dictator game," but the relationship of this game to dictatorship consists of nothing more than a linguistic coincidence. Hopkins interpretation was profoundly unwarranted. What exactly did the study find?

Genetic samples were taken from some two hundred student volunteers at the Hebrew University who played a simple economic game. Students were randomly divided into two groups and those in the first group were given 50 shekels (~$14). Each of these students could then decide whether to give away some of this money to a student from the second group with whom they were randomly and anonymously paired through a computer screen. In this exercise, which could more accurately be called the "generosity game," researchers found that those who transferred the most money were more likely to have longer versions of the gene AVPR1a. This gene is related to the hormone vasopressin, which is known to influence mammalian sociability. Students also filled out questionnaires measuring attitudes of "altruism" and "benevolence," which likewise showed some association with the AVPR1a allele. To their credit, the study's authors were much more careful and reserved in the language of their paper, phrasing the differences they found in terms of "altruism" and "benevolence." However, the unwarranted extrapolations drawn by the *Nature News* editor - drawing a line from the less generous participants in this game to mass murders -were profoundly misleading.

From the perspective of fundamental biological research this study is, on its merits, genuinely interesting for a number of reasons, but those searching for an explanation of Stalin or Hitler would be well advised to look elsewhere.

5.3.2.3 Experimental Research – Genetics of Aggression

We should also note in passing the large amount of current research examining the relationship between "aggression" and the gene encoding monoamine oxidase A (MAOA). MAOA is an enzyme that helps metabolize neurotransmitters, and different versions of the gene are found in the population at large. At least one experimental study has suggested that variations of this gene influence levels of retaliation in response to discrete provocations.[26] A number of longitudinal studies have found associations between shorter MAOA alleles and various measures of "addiction" and "anti-social behavior;" and the prevalence of this allele in the Maori population, where many of these studies first took place, led to its being characterized as the "warrior gene."[27] However, examining the details of these studies suggests that this moniker exaggerates the biological reality. Many studies have failed to find any significant, direct relationship between MAOA and various measures of criminal behavior, violence, and aggression.[28] Rather, this gene has been the poster child for GxE (gene environment interaction)

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[26] McDermott, R. et al. "Monoamine oxidase A gene (MAOA) predicts behavioral aggression following provocation." in *Proceedings of the National Academy of Sciences* 106(7) 17 February, 2009.

[27] Lea, R., and Chambers, G. "Monoamine oxidase, addiction, and the 'warrior gene' hypothesis" in *The New Zealand Medical Journal*. 120(1250) 2 March, 2007.

[28] Buckholtz, J., and Meyer-Lindenberg, A. "MAOA and the neurogenetic architecture of human aggression" in *Trends in Neuroscience* 31(3) 6 February 2008.

studies. Avshalom Caspi and Terrie Moffitt pioneered this field with a study that found that low MAOA was associated with "anti-social behavior" in people who were abused as children, but showed no effect on those without a history of abuse.[29] Replication studies of this GxE interaction have had mixed results, but some have suggested that low MAOA is associated with *lower* aggression in those who have not been abused (compared with the general population).[30] This may explain why so much variation persists in this allele, if it can grant benefits or liabilities depending on certain environmental triggers (protective in one case, but detrimental in the other). In any case, the title of "warrior gene" is a significant over-dramatization of existing results.

5.3.3 Distinguishing Extreme Irrationality and Brain Disease from the Marginal and Mundane Liabilities of a "Healthy Brain"

To return to the question of elite decision making and diplomacy, one of the greatest problems with "strong" biological urges or genuine brain illnesses is that they can make individuals impervious to reason, conventionally construed. Much of the Comparative Politics literature is premised on a (minimal) rational actor model that understands people as having goals they reason instrumentally to achieve. Indeed, our understanding of human action generally depends on its being intelligible on some level in terms of ends and means, purposes and strategy. It is for this reason that "mad men" at

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[29] Caspi, A., Moffitt, T. et al. Role of genotype in the cycle of violence in maltreated children. *Science*. (297) 2002. pp.851–4.

[30] Kim-Cohen, J. "*MAOA, Maltreatment, and Gene-Environment Interaction Predicting Children's Mental Health: New Evidence and a Meta-Analysis*" *Molecular Psychiatry*. (11) 2006 pp. 903-913.

the helm of power are so dangerous in principle, as they can be unpredictable or impervious to negotiation even when it would suit their ostensible interests. Much of the stability of the international world order depends on the use of incentives to structure and align interests. If one's ability to understand and respond to incentives and compelling arguments is compromised by biological urges or an enfeebled mind, this poses a challenge for diplomacy.

Consider Thomas Schelling's rather colorful, but nonetheless tragic, account of Anglo-American dealings with Iran mid-century:

> Recall the trouble we had persuading Mossadegh in the early 1950s that he might do his country irreparable damage if he did not become more reasonable with respect to his country and the Anglo-Iranian Oil Company. Threats did not get through to him very well. He wore pajamas, and, according to reports, he wept. And when British or American diplomats tried to explain what would happen to his country if he continued to be obstinate, and why the West would not bail him out of his difficulties, it was apparently uncertain whether he even comprehended what was being said to him. It must have been a little like trying to persuade a new puppy that you will beat him to death if he wets on the floor. If he cannot hear you, or cannot understand you, or cannot control himself, the threat cannot work and you very likely will not even make it.[31]

This concern is a valid one, even if the example is extreme. The debacles of that era of US foreign policy continue to have ramifications into the present day. A better appreciation of the "irrationality" of Mossadegh might have led diplomats to deal very different with the situation.

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[31] Schelling, T. *Arms and Influence* Yale: 2008. p.38 I am grateful to my colleague Eric Lorber for drawing my attention to this passage.

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Reflecting on the broad nature of biological influences that can operate on judgment at any given moment may lead to a kind of fatalism regarding our ability to understand or predict the behavior of others. Pascal averred that world history might be different but for the length of Cleopatra's nose. Would Mark Antony have fallen for her and out of favor with Rome otherwise? Hitler forbade anyone to smoke in his presence. Did that make Chamberlain, a smoker, nervous and impatient at Munich? Many historically momentous incidents are ripe for speculation of this sort in hindsight. However, can an understanding of biology meaningfully inform strategy ex ante?

Here I would like to argue we need to distinguish between different kinds of biological influences. Although it is true that all thinking is dependent on our biology at some level, it is still meaningful to distinguish between: 1) a generally "healthy brain" that can support capacities of consideration and judgment characteristically associated with human rationality and 2) a brain with serious biological deficits, which diminish or destroy capacities crucial for rationality. This idealized distinction can admit of a spectrum of intermediate types and tough cases. However, at the extreme end, biological deficits - and I think here of various brain diseases - have clear, and often predictable, debilitating effects. They call for medical interventions, not arguments. Moreover, advances in neuroscience and genomics are likely to shed a great deal of light on the many ways in which the brain can break down, as well as enable new therapeutic interventions.

At the other end (of "normal" rational capacities), it will still be the case that people are affected by biological dispositions and influences. However, part of becoming

a mature human being is learning to be aware of and to deal with such drives and urges. Considerations and judgments of reason must be possible within the gamut of biological forces that weigh on a "healthy" brain, if reason is to be possible at all. And with regard to those ways in which the average person is often "predictably irrational" - loss aversion, wishful thinking, perceptual biases, etc - these are likely to come under increasing scrutiny as the stakes of any decision become higher. Finally, we should recognize that one of the important functions of social institutions is to help order and control biological passions. The story of Ulysses strapped to the mast while sailing past the Sirens is instructive. By understanding our biological weaknesses we can often mitigate their effects when they threaten our deeper or more long-term interests.

Of course there will be many tough cases in the middle, instructive for thinking about what we mean by rationality: the idiot savant capable of mathematical genius but unable to recognize faces, the psychopath with cunning analytic abilities but devoid of empathy. Still, psychopaths and savants are rare, as are those with brain damage and mental illness. Psychopaths can obviously be dangerous, and if your opponent is one that is important to know. Moreover, one of the most important tasks for political institutions is to keep "mad men" (and women) out of power. We don't want to give a paranoid schizophrenic keys to a nuke. With regard to those who are not plagued by serious forms of brain disease, however, the import of biological research on the nature of decision making appears rather modest.

5.3.4 Biology and Ethics

Although it makes sense to look for explanations of particularly crazy or unintelligible behavior at the biological level, it would be a mistake to think that conflict and violence are caused only by biological deviants. In his book *Moral Minds: How Nature Designed Our Universal Sense of Right and Wrong*, the Harvard biologists Marc Hauser, help popularize the notion that normal humans come hardwired with innate moral convictions, which according to Hauser roughly correspond to the ideals of Rawlsian justice.[32] Anyone familiar with history is likely to be incredulous of such a claim, and indeed it has come under sustained critique (e.g. see Jesse Prinz[33]). However, this thesis reflects a certain optimism about human nature, eager to believe that aggressiveness/violence is something rare, which requires explanation via some biological anomaly. Living in the relative tranquility of a rich, western, liberal democracy, it is understandable why someone might mistake the habits of fellow citizens for universal features of human nature. However, those in developing countries that lack extensive institutions able constrain to violence are likely to see things differently.

To take one minor example, consider an incident from 2001 in the Nigerian town of Jos. A Christian woman walking home through a commercial district on a Friday afternoon became involved in an argument with a security guard stationed outside of a prominent mosque. The details of the dispute are unclear, but apparently the woman was

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[32] Hauser, M. *Moral Minds: How Nature Designed Our Universal Sense of Right and Wrong*. Abacus: 2008.

[33] Prinz criticizes Hauser's work in three related papers (http://www.unc.edu/~prinz/research.html)

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commanded to take a different route home out of sight of the crowded mosque. The argument turned to shouting and the rumor quickly spread that a Christian woman had been slapped by a Muslim man. Within minutes a street fight erupted. Soon, the town, which had never experienced a riot, was engulfed in bloodshed. In the three days following, over 1000 people were killed in close quarter combat, and dozens of churches and mosques were burned, as previous peaceful neighbors turned against each other in a desperate escalation of violence.[34] The well documented phenomenon of mass violence should caution against the assumption that a minority of biological deviants make up the quarrelsome and contentious.

The biological foundations of violence likely run wide and deep in human nature. They may be more pronounced in certain populations - it is no accident that most of the rioters in Jos were men. However, our capacities for violence clearly engage, and are mediated by, conceptual judgments that are not similarly instinctive - judgments about what constitutes an offense or interest in the first place. Had the woman in Jos been slapped by a mere thief in a botched robbery, the reactions would likely have been very different.

Those who work on the biological foundations of behavior are right to call our attention to the place of emotions in human life. Many political theorists have argued this point as well. For example, Harvey Mansfield suggests, "Politics is about what makes you angry, not so much about what you want. Your wants do matter, but mainly because

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[34] This example is taken from the dissertation research of Alexandra Scacco. (http://www.columbia.edu/~als2110/files/Scacco_Who_Riots.pdf)

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you feel you are entitled to have them satisfied and get angry when they are not."[35] We are, in Mansfield's view, fundamentally "thumotic" creatures, and our political theories, particularly our accounts of human rationality, need to recognize this. However, it is likely a mistake to think our emotions come hardwired. Our biological dispositions towards anger and violence combine to make us thumotic by nature, but their specific exercise is often mediated by concepts that are historically developed and learned, such as the nature of honor.

Theories such as Hauser's that posit universal liberal moral convictions hardwired into human nature are in fact absurd if one considers the vast historical range of human cruelty, most conspicuously displayed in recent times by the holocaust in Nazi Germany to the Gulags of Soviet Russian. Indeed, a contemporary prison psychologist observes that atrocities of the same nature if not the same scope persist today:

> In Central America, I witnessed civil war fought between guerrilla groups intent on imposing totalitarian tyranny on their societies, opposed by armies that didn't scruple to resort to massacre. In Equatorial Guinea, the current dictator was the nephew and henchman of the last dictator, who had killed or driven into exile a third of the population, executing every last person who wore glasses or possessed a page of printed matter for being a disaffected or potentially disaffected intellectual. In Liberia, I visited a church in which more than 600 people had taken refuge and been slaughtered, possibly by the president himself (soon to be videotaped being tortured to death). The outlines of the bodies were still visible on the dried blood on the floor, and the long mound of the mass grave began only a few yards from the entrance. In North Korea I saw the acme of tyranny, millions of people in terrorized, abject obeisance to a personality cult

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[35] Mansfield, H. "How to Understand Politics: What the Humanities Can Say to Science" NEH Jefferson Lecture, 2007.

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whose object, the Great Leader Kim Il Sung, made the Sun King look like the personification of modesty.[36]

The claim that everyone shares liberal ethical convictions deep down in their biological nature rings hollow and untrue. We cannot dismiss the problem this way, nor is violence something that we are likely to be able to explain without recourse to concepts, beliefs, and self understanding of those who perpetrate it.

In surveying the emerging literature in neuroscience and genomics related to "mind, brain, and behavior" my own suspicion is that the most remarkable discoveries will likely concern ways in which the brain breaks down. Such knowledge promises to enable dramatic new therapeutic options for those afflicted with these conditions. By extension, this knowledge may help us diagnose and understand the deviant behavior of certain elements of society, and perhaps even certain elites. However, for the vast majority - those with "healthy brains" - conventional analysis of ideology, interests, and beliefs is likely to hold the key to the most important decisions and behavior.[37] Granted, even "healthy brains" will be beset with various different biological dispositions, including perceptual biases, emotional liabilities, systematic "irrationalities," but these

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[36] Theodore Dalrymple "The Frivolity of Evil" *City Journal*. Autumn, 2004.

[37] Some studies have claimed that ideological beliefs are themselves genetically hardwired, but these suffer from many of Jervis's concerns, as well as other methodological shortcomings. For example, Alford, Funk, and Hibbing have claimed that ideological convictions are genetically inherited ("Are Political Orientations Genetically Transmitted?," (American Political Science Review 99, 2 (May): 153-167) and Fowler and Dawes have claimed voting is determined by two genes that code for neurotransmitters ("Two Genes Predict Voter Turnout" Journal of Politics 70(3):579-594 July 2008). Neither of these studies withstands methodological scrutiny, see Evan Charney's "Genes and Ideologies" Perspectives on Politics 6.2 (June, 2008): 299-319.

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generally tell only part of the story, which must also be informed by concepts, ideas, purposes. Perhaps research on healthy brains will require us to develop a more subtle language to identify and differentiate the influence of biological forces. It is unlikely, though, that this research will require us to abandon traditional considerations of interests and ideology, power and institutions, etc.

In fact, there are additional reasons to believe that elites will be under particular pressures that guard against dramatic biological irrationalities. The filtering mechanisms through which elites come into power, the high stakes of their decisions, and strong demands of rationalization they face all make it less likely that their decisions will reflect "knee-jerk" reactions. More interesting questions can be raised about the rationality and decisions of democratic masses. Animal spirits may play a greater role here, and a number of interesting theses have been advanced on this front. For example, the diets and lifestyles of wealthy nations tend to lower testosterone (a hormone linked to aggression) across the population, which perhaps plays some role in the (comparative) reluctance of these nations to go to war - a kind of biological boost to the democratic peace thesis. On another front, some have looked to evolutionary psychology for an explanation of why territorial disputes are much more likely to lead to violent conflict than other disputes.[38]

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[38]That they are more likely to lead to conflict has been persuasively argued in the literature, see: Vasquez, J., and Henehan, M. "Territorial Disputes and the Probability of War 1816-1992." *Journal of Peace Research*. 38 (2) March 2001. pp. 123-138. I am indebted to my colleague Sean Zeigler for many stimulating discussions regarding evolutionary psychology and its links to theoretical perspectives in security studies.

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5.3.5 The Limits of Biological Research

Although biological discoveries will continue to shed light on the nature of human decision making across the board, for reasons explored above I expect the most powerful discoveries will mainly apply to issues at the margins of elite decision making and political behavior. This assessment relies on a deeper judgment that the reductionist hope of understanding the human mind in terms of its biological components is likely to elude us - or is, at least, a very long way off. It is no secret that biological reduction is appealing to some social scientists who hope to make the study of human behavior completely "scientific," but this aspiration is fraught with well known objections - and its possibility will have to be demonstrated rather than assumed. In the meantime we will continue to have to negotiate the conceptual, and thus historical, dimensions of human beliefs and behavior as we have done in the past, albeit with a greater understanding of distinctly biological influences.

5.4 Concerns with the Practical Applications of Biological Research: The Example of Brain Imaging "Lie Detectors"

The current interest in using biometric technologies to detect dishonesty provides a cautionary tale regarding the practical deployment of biological research. The idea of using "biological profiling" to identify abnormal biological states that could be cause for suspicion is not new. It has long been noted that the body reveals secrets that one might otherwise wish to conceal. As one commentator has noted, "It may be true that hiding

emotions is actually harder than hiding a bomb."[39] Humans are adept at reading body language, particularly facial micro-gestures, although our intuitions are often hard to explain or defend. However, there are also objectively measurable bio-correlates of fear, anxiety, stress, etc. Some of the telltale signs are familiar ones - heart rate, eye motions, temperature, breathing - but others involve more sophisticated physiological correlates. Moreover, a remarkable number of biological measurements can be obtained non-invasively by passive biometric sensors.

In their simplest configurations, these technologies are an extension of methods long used by polygraph tests to detect deception. These rely on the premise that conscious deception elicits physiological reactions that stand out when compared to "normal" baseline states of straight-forward honesty. However, this simple detection paradigm is famously open to countermeasures. There are various was to manipulate one's vital signs - through mental exercises, muscle contractions, or even biting one's tongue - that can reliably confound polygraph results. Thus, the use of these tests in courts or for employee screening has been controversial. A National Academy of Science report on "The Polygraph and Lie Detection" issued in 2003 concluded that, when used on naive populations, polygraphs could detect lies significantly better than chance, but they are not accurate enough to justify their use for large scale security screening.[40]

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[39]North, A. "Step to the Right and Open your Brain: Will Mind Reading Improve Airport Security?" in *Jezebel*. 8 January, 2010.

[40] "The Polygraph and Lie Detection" National Academies Press: 2003 (http://www.nap.edu/openbook.php?record_id=10420&page=6)

Nevertheless, "better than chance" may be useful odds for targeted screening applications. The United States military has been interested in using polygraph technology in the field to enable quick screening of suspicious persons, for example in the vicinity of a fresh IED. Two years ago a handful of troops in Afghanistan were supplied with polygraph handsets - officially called the "Preliminary Credibility Assessment Screening System"- which employ three biometric sensors that attach to a suspect's hand.[41]

A number of companies are currently in the process of bringing related technologies to market. Suspects Detection Systems Ltd. has developed modules that collect and analyze "psycho-physiological" data from persons of interests, meant to identify those who merit further investigation. The company claims its results are highly accurate with a false positive rate of less than 4%.[42] Although these technologies are ostensibly employed for profiling purposes prior to actual investigations of guilt, false positive are a major concern. There are many innocent reasons that someone could display abnormal biological signs. Reliable methods of biological profiling will depend on using multi-factorial assessments that go beyond the simple and common bio-markers of stress.

Another company, WeCU Technologies Ltd., claims its methods can overcome many of the traditional limits of biological profiling: "The system is based on a unique probing method which uses knowledge from the behavioural sciences in combination

[41] (http://www.newlaunches.com/archives/us_troops_to_get_portable_lie_detector.php)

[42] (http://www.suspectdetection.com/solution.html#)

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with advanced biometric sensors. The system is effective for the detection of individuals who are manipulative, calm, do not have guilty knowledge, and are not being deceptive at the time of the detection. At the same time, it eliminates false results..."[43] One of the innovations of their approach is the use of small cues that elicit different autonomic responses from individuals familiar with them. For example, by flashing an obscure terrorist code word on a screen and using optical temperature and heart rate sensors to monitor people walking past, this technology is apparently successful in detecting those with special knowledge of the code word. As our understanding of such autonomic responses increases, along with the sensitivity of biometric scanners, biological profiling will perhaps prove useful a useful tool, making it more difficult for terrorists to blend in.

5.4.1 Mind Reading and Interrogation

The concept of biological profiling raises deeper questions about how the body can reveal things that the conscious mind would like to conceal. Those tasked with interrogation have an obvious interest in leveraging biological knowledge to elicit truthful information from those suspected of criminal activity. In the past, biological knowledge played and small and generally sinister role in interrogation- most conspicuously in service of torture. In more recent decades scientists have explored using various chemicals agents as truth serums. Rather than employing pain to make someone talk against their will, these chemicals aim to break down biological mechanism of

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[43] (http://www.epicos.com/epicos/extended/israel/wecu/wecu_home.html)

inhibition and self-control. Indeed, drugs such as sodium amytal, sodium pentathol, and scopolamine do make people more talkative, although the truthfulness and usefulness of what they said is highly disputed.[44] Although chemically enhanced interrogation is perhaps ethically preferable to traditional torture it suffers from similar sorts of objections. There is also a spectrum of less dramatic ways to break down resistance, highlighted in recent debates about "enhanced interrogation techniques," such as sleep deprivation, stress positions, and light control.

The acrimony of these debates helps to explain the considerable interest in "mind reading" technologies, which are fervently being explored by a number of research teams. The great hope is to employ brain imaging technologies to detect lies and guilty knowledge. When US Intelligence Director Dennis Blair recently announced that the government's newly created High-Value Detainee Interrogation Group will conduct "'scientific research' to find better ways of questioning top terrorist suspects" he would not discuss the nature of these research projects.[45] However, the search for a brain imaging lie detector is undoubtedly near the top of the list. A technology able to bypass the intransigence of terrorists and obtain high quality data about what they know would solve many legal and strategic needs.

There are currently at least five different approaches to "mind reading" being explored. One uses electroencephalography to detect signals that are supposedly emitted

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[44] Marks, J. "Interrogational Neuroimaging in Counterterrorism: A 'No-Brainer' or Human Rights Hazzard?" in the *American Journal of Law and Medicine* 33(2/3) April, 2007.
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(http://www.breitbart.com/article.php?id=CNG.64f93e3e5ec86eaffb1af7e7d2f5a565.2d1&show_article=1)

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shortly after a brain recognizes something "familiar" to it, providing what some have called a "brain fingerprint." Another uses laser spectroscopy to look for surface patterns of the brain correlated with deception. Yet another analyzes facial micro-gestures, and there are also experiments examining whether periorbital thermography (measurement of temperatures around the eye) can detect deception.[46] Perhaps the most promising research thus far has come from the use of functional magnetic resonance imaging (fMRI). This technology is able to monitor real-time changes in blood oxygenation levels within the brain based on the differences in their magnetic properties. Because active neurons elicit more oxygen from blood than inactive ones, it is believed that changes in blood oxygenation correspond to localized neuronal activity. Many believe fMRI holds the best chance of providing data detailed enough to discriminate different brain states associated with lying or guilty knowledge.

In 2005, the journal *Nature* ran a news article with the title "Brain imaging ready to detect terrorists, say neuroscientists."[47] It reported the latest round of tests with an fMRI lie detector by a research team at the University of Pennsylvania. The team, lead by psychiatrist Daniel Langleben and neuropsychiatrist Ruben Gur, reported a 99% accuracy rate in its ability to distinguish whether participants in an experiment lied about which cards they drew at the outset of the study. According to Gur "A lie is always more complicated than the truth"..."You think a bit more and fMRI picks that up." Although

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[46] Greely, M. "Neuroscience Based Lie Detection: The Need for Regulation" in *Using Imaging to Detect Deception: Scientific and Ethical Questions*. American Academy of Arts and Sciences: 2009.

[47] Wild, J. "Brain imaging read to detect terrorists, say neuroscientists" in *Nature*. 437 (457) 22 September, 2005.

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fMRI studies have not reliably found any "lying centers" in the brain, Langleben and others have claimed that the distribution of brain activity looks different when subjects are engaged in deception. A company named "No Lie MRI" now uses algorithms derived from the Penn team's research to offer commercial lie detecting services to a wide range of customers.[48] Another company, Cephos, offers similar MRI lie detecting services and lists on its website over 30 scientific articles that support its technology, many published in top, peer-reviewed journals.[49]

The details of many laboratory studies of fMRI lie detection have, however, drawn extensive criticism. First, it is unclear whether providing false information under instruction in a laboratory even counts as lying. More generally, there is a concern that the low stakes and trivial tasks of most research protocols are not good models for investigating the nature of high stakes lies in the real world. Also, although most scientist are acutely aware that their findings are sensitive to specific parameters of a lying task, this caveat is often overlooked by those enthusiastic about fielding this technology. Discriminating a lie from a truth in a forced choice task is very different than detection of deception in more general statements; and guilty knowledge tests, which try to measure some memory/familiarity response to cues, are another challenge altogether. Some critics have also suggested that the most dramatic experimental successes in fMRI lie detection are driven by artifacts that have nothing to do with lying.

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[48] (http://noliemri.com/pressNPubs/Publications.htm)

[49] (http://www.cephoscorp.com/lie-detection/index.php#working)

For example, in the card experiment used by the Penn research team subjects were presented an envelope with a seven of spades and five of clubs.[50] They were to tell the truth about having one of these cards but lie about the other, and if successful in their lie would receive $20. In the scanner subjects were shown a number of cards in succession (including many control cards from the rest of the deck) and asked to identify only the card(s) they had received. Since subjects would only positively identify that one card which they had chosen to be the truth card, they spend most of their time in the scanner answering "no" to move through the deck, looking for that special card they had chosen as their truth card. Nancy Kanwisher, a prominent neuroscientist at MIT, has suggested that the "neural signature of the supposed 'truth' response is really just the neural signature of a target detection event," which would mean this experiment is not tracking lying at all.[51] Moreover, she argues that the statistical algorithms employed by Langleben and colleagues to boost their detection rates are highly sensitive to the peculiarities of the experimental protocol.

Debates about the merits of particular experiments will continue and likely lead to more robust and interesting investigations. However, a serious challenge awaits those who would like to successfully apply any fMRI lie detection technology to terrorists,

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[50]Langleben, D. et al. 2005. Telling truth from lie in individual subjects with fast event-related fMRI. *Human Brain Mapping* (26) 2005. pp. 262–272.

Davatzikos, C. et al. Classifying spatial patterns of brain activity with machine learning methods: Application to lie detection. *Neuroimage* (28) 2005. pp. 663–668.

[51] Kanwisher, N. "The Use of fMRI in Lie Detection: What Has Been Shown and What Has Not" in *Using Imaging to Detect Deception: Scientific and Ethical Questions*. American Academy of Arts and Sciences: 2009.

namely the existence of simple and effective countermeasures. These are summarized by

Kanwisher:

> Functional MRI data are useless if the subject is moving more than a few millimeters. Even when we have cooperative subjects trying their best to help us and give us good data, we still throw out one of every five, maybe ten, subjects because they move too much. If they're not motivated to hold still, it will be much worse. This is not just a matter of moving your head— you can completely mess up the imaging data just by moving your tongue in your mouth, or by closing your eyes and not being able to read the questions. Of course, these things will be detectable, so the experimenter would know that the subject was using countermeasures. But there are also countermeasures subjects could use that would not be detectable, like performing mental arithmetic. You can probably activate all of those putative lie regions just by subtracting seven iteratively in your head."[52]

The vulnerability of fMRI such interventions along with extensive concerns about

the generalizability of laboratory findings inform her ultimately dim view of the

usefulness of this technology: "Because the published results are based on paradigms that

share none of the properties of real-world lie detection, those data offer no compelling

evidence that fMRI will work for lie detection in the real world. No published evidence

shows lie detection with fMRI under anything even remotely resembling a real-world

situation. Furthermore, it is not obvious how the use of MRI in lie detection could even

be tested under anything resembling a real-world situation."[53]

Others, however, see a great deal of promise in the results thus far and expect

future innovations can overcome current hurdles to confidently employing mind reading

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[52] Ibid. p. 12.

[53] Ibid.

technologies in the investigation of terrorists. I am told a number of studies boasting lower error rates, better protocols, and cross subject regularities await publication. Time will tell if these can overcome the powerful objections of critics, with whom my own sympathies currently rest.[54]

The quest for an fMRI mind reader is exemplary of the ambitious hopes placed on emerging biotechnologies that some believe can revolutionize the provision of security and justice. On the other hand, this quest has fueled the fears of those who worry both about the dangers posed to privacy if the technology works and the dangers of false convictions if it doesn't. In any case, it is likely that debates about the feasibility of these technologies will overshadow debates about their desirability. While the search for effective lie detectors continues, a number of the biological profiling methods discussed are perhaps useful for screening purposes. However, it is important that those who employ them not confuse the probabilistic "red flags" of biological profiling with dispositive evidence of guilt. Ultimately, problems of dishonesty and truthfulness are unlikely to be solved through a disciplinary society of surveillance and brain imaging.

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[54] For further discussions of the promise and limits of using imaging technologies for deception see: Langleben, D. "Detection of Deception with fMRI: Are we there yet?" in *Legal and Criminal Psychology*. (13) 2008. pp. 1-9.; Rosenfeld, P. "'Brain Fingerprinting': A Critical Analysis" in *The Scientific Review of Mental Health Practice*. 4(1) Spring/Summer 2005.; Wolpe, P. et al. "Emerging Neurotechnologies for Lie-Detection: Promises and Perils" in *American Journal of Bioethics*. 5(2) 2005. pp. 39-49

5.5 Summary Position

In the preceding sections I have identified a number of inherent limits in research methods of contemporary biological-behavioral research. I have also shown that current findings, while profoundly interesting, are rather modest and stand in stark contrast to the tremendous hope put in this research. There are undoubtedly deep philosophical questions lurking in the background. For example, how is it that consciousness and all its characteristic features emerge from our bio-chemical "machinery?" Are the characteristic features of "intentional agency" – our perceptions of choice, capacities for self control, ability to make considered judgments, development of purposes – nothing but post hoc psychological fictions or do they indicate a real capacity for action and thought that transcends our common understanding of necessity of physical mechanism?

These questions are not new; indeed they pervade the history of philosophy in different forms. In some sense, since the advent of modern science in the 17th century we have been on a collision course with a contemporary formulation of these problems. In his "third antinomy" Kant famously considered the incompatibility of two fundamental assumptions of modern thought, parsimoniously summarized by one Kant scholar as the assumptions: "that the motion of all natural beings is causally determined, and that human beings are free and self moving. The first is necessary to modern science and the second to modern morality."[55] Kant's own resolution of this antinomy is less than persuasive. However, these issues could safely remain at the level of esoteric

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[55] Michael A. Gillespie. "Philosophy and Rhetoric in Kant's Third Antinomy" in *The Political Science Reviewer* (ISI, Vol XXX 2001) 7.

philosophical debates since the locus of their concern – the material constitution of human life – remained an impenetrable mystery. But our technological advances are changing that. While much of the immediate interest in neuroscience and genomics stems from their possible therapeutic benefits, the philosophical interest in them is just as great. The million dollar question is whether their findings require us to fundamentally revise traditional notions of rational agency, and if so, how? Perhaps they will provide some definitive resolution of metaphysical dilemmas concerning the relationship between mind and brain, or at least lay a new foundation for a true science of human behavior and, by extension, politics.

Having so much at stake has, in some sense, poisoned the philosophical discourse surrounding discoveries in these fields. The framing of much research, as well as our reception of it, is tainted by aspirations to be relevant to big questions – as if particular experiments were able or meant to settle these issues. It is also the case that many researchers have sensationalized their results for the purpose of greater publicity. In making unwarranted extrapolations, however, they often appear ignorant of the strong and improbable philosophical implications of their hype. Thus, for a variety of reasons, we often look at the results of recent research through a framework of strong dichotomies. Either they lend credence to a view of human action driven by a-rational biological mechanisms or, in failing to say much about such mechanisms give support to our default understanding of "folk psychology." That is to say, many are eager to pigeonhole experimental results as either supporting or failing to support biological determinism of various sorts.

However, these neat alternatives of "mechanistic determinism" and "uncaused mental freedom" are likely misleading. As Charles Taylor has observed, dividing our options between these two alternatives begs rather large philosophical questions. It unwittingly suppresses other possibilities that might involve more complex relationships between mind and matter.[56] Descartes has become something of a scapegoat for naturalist and neuroscientists who want to disabuse us of any notion of immaterial mind (see for example Damasio's wide selling *Descartes Error*). However, the exclusive division of the world between immaterial mind and mechanistic matter is itself a provenance of Cartesian philosophy. Many hard core "materialists" thus remain more Cartesian than they realize in the way they envision the underlying possibilities that basic research can confirm or disconfirm.

The truth about our biological constitution may be more complex than the partisans of mechanistic determinism or uncaused mental freedom have imagined. In fact it is striking that so many canonical thinkers, both ancient and modern, developed psychological accounts that, although committed to human "exceptionalism," do not fit easily into the Cartesian dichotomy: Plato's famous allegory of the charioteer (reason) struggling to direct the two unruly horses of concupiscent appetite and irascible *thumos*; Aristotle's characterization of the *zoon logon echon* (language bearing animal) as a creature of habit, discipline, passion (rationally appropriate or overweening), and contemplative wonder; Cicero's disdain for Epicureanism and Stoicism, recognition of

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[56] Taylor, Charles. "Descombes' Critique of Cognitivism", in *Inquiry*, vol 47, 2004, pp. 203-218.

the emotional force of rhetoric, and vision of friendship grounded in shared apprehension of reason; Augustine's elaborate neo-Platonic psychology, in which desires of the flesh play a central role; Aquinas's insistence that all knowledge is of the senses, "I am not my soul," and advice to take a warm bath to clear the mind; Machiavelli's claim that it is necessary to treat others as both "man and beast" to rule them successfully; Montaigne's extended ruminations on the frailties of the human body and their effects on our thoughts, occasioned by his own long bought with kidney stones; Rousseau's revisionist reconstruction of human nature and the educational implications mapped out in the *Emile*; the "theory of moral sentiments" developed by Adam Smith and endorsed in various ways throughout the Scottish Enlightenment; Tocqueville's reflections on "soft despotism," which enervates and bends the will through slight pleasures and the assured satisfaction of material necessities; not to mention the rich distinction and nuances embedded in our "ordinary" folk psychology, all suggest both that intentional human agency is genuine and that our agency is nonetheless fundamentally circumscribed and shaped by biological endowments and influences. Indeed, as Charles Talyor argues, "All explanations which are teleological-intentional take account of boundary conditions which are material (212)." Thus, "to say that we are intent on some goal may be to say that we will do anything in our power to encompass this goal, but what is in our power is purely a matter of material conditions (213)." Our ordinary ways of speaking, and indeed many of our most influential theorizations of agency, entail a complex relationship between biological influences, proclivities, and limits alongside genuine capacities of conscious, rational deliberation.

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Thus, although I agree with Charles Taylor's negative judgment regarding the possibility of the complete biological reduction of the mental, mapped out in his essay "How is Mechanism Conceivable?"[57], this by no means necessitates ignoring or rejecting ways in which human physiology is essential to understanding our mental capacities. We already have many rich traditions of thinking about biological influence on human thoughts and actions. And emerging research may help refine these traditions of thought. But the onus is on those who want to move us towards more "biologized" theories of action, to actual demonstrate through sound research and pragmatically useful applications the truth of their biological theories. So far, attempts to provide a full account of human cognition in a mechanistic model have failed. However, the failure of the more simplistic models have lead to a number of influential theories that provide terms for admitting genuine human agency, in contrast to the deterministic picture advanced by partisans of biological hardwiring.

Michael Tomasello, a leading developmental psychologists who works with both young children and non human primates, has been a pioneer of a usage-based theory of language acquisition that draws on a wide range of biological research to argue that language and cognition are essentially mediated by socio-cultural environments. Tomasello's position, outlined in *Constructing a Language* and *The Cultural Origins of*

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[57] Charles Taylor "How is Mechanism Conceivable" in *Philosophical Papers* Vol.1 (Cambridge University Press, 1985).

Human Cognition,[58] is part of a large perspective in cognitive linguistics that maintains:1) language is not an autonomous cognitive faculty, 2) grammar is conceptualization, and 3) knowledge of language emerges from language use.[59] Tomasello and colleagues marshal extensive biological support for the thesis that human language endows us with creative capacities that transcend innate biological dispositions.

Even Steven Pinker, the leading exponent of the older Chomskian view of an innate/universal symbolic grammar underlying language use, conceives of human intelligence as an "open-ended combinatorial system." In Pinker's view, "Even if we're equipped with a fixed set of grammatical rules and a fixed vocabulary, we can spin out a mind-boggling array of sentences that have never been uttered before. Each one of those sentences corresponds to a distinct thought. The open-ended creativity of language is just a way of externalizing the open-ended creativity of thought. People can come up with new ways of resolving conflicts or attaining social goals in the same way that they can cook up new technological solutions to problems. You don't need an unconstrained ghost in order to account for human ingenuity."[60]

In any case, there are strong theoretical positions and much biological research that runs against the aspiration to provide a biologically reductive account of human value, sought by those who would like to give the social sciences an absolute foundation.

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[58] Tomasello. *Constructing a Language* (Harvard University Press:2003) and *The Cultural Origins of Human Cognition*(Harvard University Press:1999).

[59] William Croft and D. Alan Cruse. *Cognitive Linguistics* (Cambridge, 2004) 1.

[60] Pinker quoted in "Biology vs. the Blank Slate" in *Reason Magazine* (interview conducted by Ronald Bailey and Nick Gillespie) October, 2002.

It is worth noting two final sets of considerations that counsel against biologically determined accounts of ethical convictions.

First, such accounts do place ethical convictions entirely beyond reasoned consideration. If the explanation for why someone believes justice requires a welfare state or thinks that abortion is illegitimate is simply that their genetic makeup disposes them to such judgments, the idea of trying to persuade them of the merits of different positions makes no sense. It would be like trying to persuade someone that a lemon tastes sweet. If ethical convictions are not mediated by conceptual judgments, there is no way to engage them through reasoned persuasion. The ends of human action really would not be open to conscious reflection and deliberation. Not only does this present a problem for notions of morality that hold it is reasonable for particular ends to be recognized as better or worse, but this evacuates any meaning from the very idea of ethical reflection. All it leaves us with is instrumental rationality servicing whatever ends are biologically program or elicited through pathways essential detached from ideas, concepts, and judgments.

"Mimetic" theories of cultural transmission, which try to understand the spread of ideas in evolutionary terms (both Darwinian and Lamarkian), although plausible on some level, also encounter serious problems in the way they construe rationality. It is easy to see how the human mind could provide a foundation for a kind of Lamarkian evolution, acquiring new ideas that prove useful for some problem and passing those ideas on to children and friends. Richard Dawkins has proposed the term "meme" to describe ideas

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that propagate themselves according to this model.[61] Better ideas (that confer survival or help people flourish in various ways) spread, while inferior ideas become extinct. However, the analogies to be drawn between the way ideas spread and benefit people and the reproduction and survival of genetic material are tenuous. Although humans are undoubtedly mimetic, they do not simply adopt whatever ideas are propagated with the greatest frequency or volume. Human civilization is certainly enriched by our ability to learn and pass down ideas, but our capacities of reason make this process more complex than anything suggested by the theoretical mechanisms of Darwinian or Lamarkian evolution. The more we can reason about our adoption of memes the less a theory of memes is able to supply a neat evolutionary account of cultural development. Perhaps the best treatment of this issue can be found in Richerson's and Boyd's *Not By Genes Alone: How Culture Transformed Human Evolution*.[62]

Second, biologically determinist accounts of behavior have rather narrow policy implications. Such accounts can lead to what Robert Cook-Deegan has described as "policy nihilism" – the conclusions that since behaviors and conceptualizations of the good are inscribed in human biology there is little we can do to change them. Therefore, we should not expend resources on education or on changing structural features of the surrounding society, since at the end of the day people are naturally disposed to whatever

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[61] Richard Dawkins. *The Selfish Gene.* (New York City: Oxford University Press, 1976).

[62] Peter J. Richerson, Robert Boyd. Not By Genes Alone: How Culture Transformed Human Evolution (Chicago, 2004).

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unfortunate convictions and behaviors they display. This line of thinking also supports racial theories of political development such as one finds in Lynn and Vanhanan's *IQ and the Wealth of Nations*, J Phillipe Rushton's *Race, Evolution, and Behavior*, or Richard Lynn's *The Global Bell Curve: Race, IQ, and Inequality Worldwide*.[63] Moreover, the positive policy interventions that this perspective does support are of two kinds – either force (which can overawe instrumental calculations) or medication (which can therapeutically change the biological determinants of behavior). If we cannot reason with people about the desirability of certain fundamental goods in the hope of persuading them, we must treat them more like animals unable to comprehend anything beyond their own self gratification and instinct.

5.6 Conclusion

A survey of the contemporary methods and emerging findings in biological-behavioral research suggests that the biological reduction of human behavior and beliefs is very far from being realized. Indeed there is reason to believe that the quest to provide an absolute foundation for the social sciences in biology will go unfulfilled. However, biological research has provided numerous insights into the nature of human irrationality as well as other physiological liabilities in human nature. As the universe of biological

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[63] Lynn and Vanhanan *IQ and the Wealth of Nations* (Greenwood, 2002), J Phillipe Rushton's *Race, Evolution, and Behavior* (Charles Darwin Research Institute, 2000), or Richard Lynn's *The Global Bell Curve: Race, IQ, and Inequality Worldwide* (Washington Summit Publishers, 2008).

research expands and important task will be to properly understand new findings and place them within a larger account of human agency.

If I am correct in my assessment of the limits of the reductionist project, what is my alternative account of how we are to understand the origins and nature of ethical convictions? I do want to emphasize that human biology must be the starting point of an adequate account. However, human biology gives rise to the human mind, which must be understood in its own terms, with references to consciousness, intentions, reflection, concepts, critical judgment and so forth. This is not the disembodied and radically free mind proposed by Descartes, but nonetheless a mind that enables us to be open to rational reflection with regard to our ends.

Our biology leaves us genuinely open to a range of convictions about the proper ends of life, convictions that may be grounded in biological necessities but capable of being permuted to the point of the very negation of natural drives. In so far as we can talk about a universal psychological architecture of the human mind, seeking to understand and realize what is "good" is perhaps the most fundamental orientation of the mind. Although "innate" desires form a starting point for orienting human being in the world, through language and training humans characteristically acquire faculties of reason and reflection that enable them to put their immediate desires into question and to pursue a wide range of ends. Reasoning about such ends is possible, and a process which I will describe as fundamentally hermeneutic.

My purpose in the next chapter is to sketch out the nature and implications of this alternative, non-reductive, conceptual account of ethical convictions. I do this in three

stages. First, I provide an outline of how capacities of practical reason characteristically emerge in human development, drawing heavily on the work of Alasdair MacIntyre. Second, I articulate and defend a concept of hermeneutic reason, which I believe best accounts for the way in which we reason about ultimate goods. Properly understood, "hermeneutics" confirms not only the possibility of ethical persuasion, but its centrality for social life. Finally, I examine debates concerning the social implications of a hermeneutic account of reason and the institutional forms that help sustain and mediate rational discourses about ultimate goods. I further try to show how an understanding the nature of ethical persuasion can complement knowledge gained from social sciences in service of pragmatic goals. In the concluding chapter of this project I then examine a number of examples of ethical persuasion in action, which show how ethical persuasion has been an important component of successful social change projects, able to transcending the pragmatic limits of social science research.

6. Nature, Goods, Reason, and Persuasion

6.1 Overview

In the preceding three chapters I have shown that the dominant analytic techniques of the social sciences all have intrinsic limits and have further illustrated how these limits often relate to the dynamism of ethical convictions. Moreover, I have argued that attempts to reduce ethical convictions in their entirety to structural, biological, or other non-conceptual sources have thus far failed. The question we then arrive at, once we accept that ethical convictions are important for social structure and yet not reducible within a scientific framework, is: where do ethical convictions come from? Moreover, are they corrigible and amenable to rational persuasion? And, if so, what are the pragmatic implications for our understanding of social institutions and our attempts to shape them?

In this chapter I sketch out answers to these three questions. Given constraints of space I can provide only a sketch, but this is enough to substantiate the main theses of an alternative, non-scientific account of ethics and its implications for social thought. First, I use Alasdair MacIntyre's work to suggest how it is that humans characteristically make the transition from being children driven by natural desire to practical reasoners able to stand back from and evaluate their desires and ends. Second, I use Hans-Georg Gadamer's work to argue that reason can indeed inform our reflections on the priority of various goods and that the character of such reason is fundamentally hermeneutic.

Finally, I explore the implications of this account of hermeneutic reason for social thought. An enormous amount of literature in political theory has dealt with this question. I use Charles Taylor's work to show how visions of the good and their social expressions are essential to our understanding of how modern liberal democracies function. This perspective casts doubt on a prominent line of thinking in contemporary political theory that idealizes "ethical neutrality" and hopes to make the state formally neutral with regard to conceptualizations of the good. With regard to debates concerning the institutional forms best suited to recognizing the ethical sources of social life, promoting their development, and managing their conflicts in politically productive ways I side with Bryan Garsten, Michael Waltzer, Malachi Hacohen and others who demur at the hope of promoting and managing ethical convictions through purely procedural institutions. There is no permanent procedural solution to the problem of ethics in society. There are, however, institutions that can better facilitate a politics of persuasion, even though, at the end of the day, social institutions are only as good as the persuasive resources we can muster in their defense. In this sense, politics always depends on ongoing ethical persuasion, something that we do well to recognize both at home in liberal democracies and in our attempts to improve societies abroad.

6.2 MacIntyre on the Transition from Natural Drives to Practical Reason

As mentioned in the preface to this study, Alasdair MacIntyre argues that they key to understanding the rationality of humans in contradistinction to the instrumental

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rationality of higher primates and other animals is that humans can progress from pre-linguistic natural desires to a condition in which they can put their given desires into question. Humans go beyond the reasoning of other animals "when they become able to reflect on and to pass judgment on the reasons by which they have hitherto been guided," – something that occurs as part of the natural maturation process of a child with good parents and teachers.[1] MacIntyre elaborates his account of this process in *Dependent Rational Animals: Why Human Beings Need the Virtues*.

It is of course essential that humans should be directed by nature to innately desire various primary goods, such as nutrition and shelter. These desires are crucial for directing our early development and they play an indispensible role in our ongoing attempts to secure the necessities of life. Moreover, were we not to share a common nature constituted by characteristic needs and desires with other humans we would have no basis on which to begin reasoning together about common ends and purposes.

We can perhaps imagine a life based on the satisfaction of natural desire, such as Rousseau's image of natural man – "I see him satisfying his hunger at the first oak, and slaking his thirst at the first brook; finding his bed at the foot of the tree which afforded him a repast."[2] However, upon scrutiny we find that the range of "natural" desires that well up in the human person is wide and the desires diverse and potentially chaotic. At some point we need to adjudicate between desires, and the question of "what should I

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[1] Alasdair MacIntyre. *Dependent Rational Animals: Why Human Beings Need the Virtues* (Open Court, 1999) 57.

[2] Jean-Jacques Rousseau. *Second Discourse* in *The Social Contract and the First and Second Discourses* edited by Dunn and May (Yale University Press, 90.

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do," however basic, enters into practical rationality. As MacIntyre explains "…the question 'Why should I do this rather than that?' becomes from an early age inescapable, and it is characteristic of human beings, that their replies to this question can themselves always be put into question, and that, when those replies are put into question, that further questions can only be answered, rather than avoided or ignored, by reflecting upon and evaluating the practical reasoning that issued in or was presupposed by their actions."[3] Thus, MacIntyre claims, "Human beings need to learn to understand themselves as practical reasoners about goods, about what on particular occasions it is best for them to do and about how it is best for them to live out their lives."[4]

MacIntyre's account involves an implicit critique of biological-behavioral research, which conceives of innate desires directing human actions without conscious or conceptual mediation. This is precisely what is suggested by studies that claim that complex behaviors like voting or charitable giving are driven by genetic variations or neurological imbalances. MacIntyre's account also challenges the common notion that human physiology provides conscious desire with primitive drives or innate longings that cannot be subject to rational evaluation or formation. Although this sort of perspective admits that desires rise to the level of conscious recognition, it nonetheless views such desires as biologically determined and unalterable. Against this view of unchanging,

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[3] Alasdair MacIntyre. *Dependent Rational Animals: Why Human Beings Need the Virtues* (Open Court, 1999) 67.

[4] Ibid., 67.

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innate desire, MacIntyre contends, "our affections and sympathies are generally, if not always, to a significant degree in our control, at least in the longer run."[5]

Although this is in some sense an "empirical question," MacIntyre not only suggests many examples of desires being transformed over time but shows that that this sort of transformation, along with the ability to critically evaluate our desires, is essential to our understanding of human agency. Desires are not basic and beyond reasoned reflection. Rather the development of rational agency requires the ability to put our desires and purposes into question. MacIntyre's treatment of this issue is worth citing at length:

> When someone gives a reason for doing this rather than that, it is never sufficient, either to explain or to justify one's action, to say 'I did x, because doing x enabled me to do, have, or be y and I wanted to do, have, or be y.' Why not? Because it is always relevant to ask why I should at this particular time in these particular circumstances choose to act on this particular desire rather than on some other. At any particular time I have some range of projects, goals, and desires. So, when I propose to myself to act on this particular desire, I have to ask 'Is it at this time and in these circumstances best to act so as to satisfy this particular desire?' And, if I do act on a particular desire, I either make or presuppose a judgment that it is best for me here and now to act so as to satisfy this particular desire. In our everyday speech of course the explanation or justification of some particular action by an agent often does terminate with his or her saying something such as 'I did it, just because that is what I wanted to do.' But, if this is what I say, I always invite the question of whether there was not some better reason for me to act in some other way. Hence if my reason for acting as I did was a good reason for so acting, it must have been not just that I wanted such and such, but that I wanted such and such *and* that there was no better reason for acting in any other way.
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> In so evaluating my desires I stand back from them, I put some distance between them and myself *qua* practical reasoner, just because I invite the

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[5] Ibid., 116.

question, both from myself and from others, of whether it is in fact good for me to act on this particular desire here and now. Most of the time deliberation does proceed and must proceed without bringing this question to mind. And if this question were raised too often and too insistently, it would paralyze us as agents. But without the ability to raise it we cannot function as practical reasoners, and the acquisition of this ability is possible only for those who have to some significant degree learned how to separate themselves from their desires and more especially from those desires in their primitive, infantile forms. The small child, if possible, acts on its desires, finding in them reasons for action, as dolphins do, as gorillas do. What the use of language enables it to achieve is, as Kenny, McDowell, and others have emphasized, the evaluation of its reasons. But the acquisition of language is not by itself sufficient. The child has to learn that it may have good reason to act other than as its most urgently felt wants dictate and it can do this only when those wants have ceased to be its dictator.

It is not of course that the child becomes able to act without desire. The notion of acting without desire is itself a phantasy and a dangerous one. It is rather that the child becomes open to considerations regarding its good. It develops a desire for doing, being, and having what it is good for it to do, be, and have, and in so doing becomes motivated by reasons that direct it towards some good. Notice however that in justifying our actions and our having acted from this or that desire for this or that object we make no reference to the desire for good *qua* desire. It is always and only some claim about the character of the good in question and about why it is better for us in this particular situation to act so as to achieve this good that is relevant to such justification.[6]

On MacIntyre's account, which I substantially endorse, the development of the

human mind marks a break with a biological nature ruled by instinct. The development of

the human mind can, of course, be thwarted in various ways – by physiological factors

(e.g. ingesting heavy metals) as well as by bad teachers or parents. However, generally

and for the most part, humans develop capacities of reason that enable them to reflect on

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[6] Ibid., 68-69.

and critically evaluate different possible goods, directing themselves to some ends rather than others.

MacIntyre calls attention to the crucial role that parents and teachers play in developing capacities of reason. Thus, education, broadly construed, is a necessary condition for rational agency. However, it is not sufficient. As MacIntyre argues: "What each of us has to do, in order to develop our powers as independent reasoners, and so to flourish qua members of our species, is to make the transition from accepting what we are taught by those earliest teachers to making our own independent judgments about goods, judgments that we are able to justify rationally to ourselves and to others as furnishing us with good reasons for acting in this way rather than that."[7] The search for an understanding of genuine goods and ways in which to rationally justify our evaluations of them is a recursive and permanent feature of mature human reasoning.

Although mature reasoners move beyond the initial judgments and direction supplied by their teachers, the process of engaging with and learning from others remains central to MacIntyre's account of rational reflection. Moreover, because progress in reasoning is essentially dialectical, cultivating reason means cultivating argument. Without the ability to reason rightly, persons may fail to achieve superior goods and ends open to them. We thus have a *prima facie* interest in cultivating our capacities of reason as well as social forums in which rationally productive debate can take place. This is

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[7] Ibid., 71.

because, as MacIntyre maintains, "Humans at times cannot flourish without arguing with others and learning from them about human flourishing."[8]

Moreover, since "everything that can inhibit, frustrate, or damage the exercise of the powers of reasoning is a potential threat [to human flourishing],"[9] cultivating our own capacities of reason as well as the capacities of fellow citizens and creating social spaces in which reason can be dialectically refined through argument is an important social project. The formation of independent practical reasoners is one of the most important tasks for all parents and all societies. Without such formation people may remain crippled in their adult life, lacking capacities to engage in meaningful reflection about their ends and enslaved, as Plato might say, to whim and passion. Such formation is also important for society, and not just because it is desirable to have well behaved, rationally capable citizens from the perspective of the state. Rationally capable citizens also benefit one another through their debates and disagreements. So, MacIntyre argues, the development of reason is both a personal and social good: "Independent practical reasoners contribute to the formation and sustaining of their social relationships, as infants do not, and to learn how to become a practical reasoner is to learn how to cooperate with others in forming and sustaining those same relationships that make possible the achievement of common goods by independent practical reasoners."[10]

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[8] Ibid., 68.

[9] Ibid., 68.

[10] Ibid., 74

Humans find that is possible to pursue and to be committed to variety of goods. Moreover such commitments need not be a matter of arbitrary choice, but are matters that are open to reflective consideration for and against which reasons can be marshaled. Indeed one of the most important questions for any person is the question of what goods deserve his or her allegiance. This is a question that lies at the heart of practical rationality, and one that we can characteristically only answer well by engaging in ongoing dialogue and disagreement with others. MacIntyre summarizes his claims about our unavoidable confrontation with the question of what goods we should pursue and the need for others to help us reason through this question as follows:

> What we need from others, if we are not only to exercise our initial animal capacities, but also to develop the capacities of independent practical reasoners, are those relationships necessary for fostering the ability to evaluate modify or reject our own practical judgments, to ask, that is, whether what we take to be good reasons for action are sufficiently good reasons, *and* the ability to imagine realistically alternative possible futures, so as to be able to make rational choices between them, *and* the ability to stand back from our desires, so as to be able to enquire rationally what the pursuit of our good here and now requires and how our desires must be directed and, if necessary, reeducated, if we are to attain it.[11]

MacIntyre's account of practical rationality is broadly Aristotelian in form, echoing the opening lines of the *Ethics*- "Every craft and every investigation, and likewise every action and decision seems to aim at some good"- as well as the *Politics* - "everyone does everything for the sake of what seems good."[12] However, this account of rationality and the general psychological claims that MacIntyre defends are, I believe,

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[11] Ibid., 83.

[12] Aristotle. *Ethics* and *Politics* in *Aristotle: selections* trans/edited by Irwin and Fine (Hackett, 1995).

rather minimal and widely shared by most accounts of ethics that believe in the possibility of rational reflection on human ends. The problem that all such accounts face is that there are a wide variety of apparent goods that may provide ends for human action. People appear capable of rationally reflecting on and arguing about which goods in fact deserve their allegiance, but how exactly does such reasoning take place? What can *reasoning* about ends really mean?

From these perspectives the most interesting questions in human affairs all revolve around our appraisals of goods. The major source of variation in human activities, if we can put it like that, derives from variations in the appearance of good. This is not to deny the distinct importance of technological knowledge. People can be in fundamental agreement about ends and still disagree about the best means to achieve them. Such technological debates concern questions of causality and efficiency, and modern scientific societies have deployed the majority of their intellectual resources in service of such debates. However, while technological knowledge has vastly improved the human condition, made projects in which there is ethical consensus more efficient, and even resolved the necessity of some conflicts in the first place, technological knowledge is not equipped to help us deliberate about questions of fundamental value and discriminate between greater and lesser goods. How, then, can reason come to grips with such questions at all?

I, like MacIntyre, would like to claim that we can reason about such questions, but the kind of reasoning involved is very different than the methods of reasoning employed in "scientific" investigations. Rather, reasoning about ultimate goods and

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values is a "hermeneutic" enterprise, which under certain conditions can make a claim to its own sort of rationality.[13]

6.3 Hermeneutics

Hermeneutics is a dangerous term to employ in formulating my thesis of because of the complex, diverse, and often opaque ways in which it has been used. Nonetheless, it is the best term and properly understood leads us to the heart of questions about the constitution of human reason.

At its most basic level, hermeneutics refers to the art of interpretation. The earlier uses of this term were associated with the specific challenges of properly interpreting legal and theological texts. At stake from the beginning were important questions about how human understanding operates – when someone makes a pronouncement or writes something down how do I know that I have understood it correctly given the ambiguities inherent in all language? However, problems of interpretation are obviously not limited to lawyers and biblical scholars, but extend to everyone engaged in any form of communication. Taken one step further, humans are confronted not only with the challenge of interpreting forms of linguistic communication, but also of self-interpreting their own life experiences. Thus, through a complex set of philosophical debates

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[13] In Aristotelian terms such reasoning involves *phronesis* rather than simple *techne*. Related to this distinction, I would like to claim that human society is ultimately an object of *praxis* – human action in the realm of the contingent – rather than *theoria* – contemplation of the eternal and unchanging (as partisans of "absolute science" might suppose). Regarding these issues, see the illuminating points made by Joseph Dunne in *Back to the Rough Ground: "Phronesis" and "Techne" in Modern Philosophy and in Aristotle* (Notre Dame: 1992).

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surrounding these issues, hermeneutics came to designate a much larger set of concerns beyond mere textual interpretation, ultimately encompassing the nature of human self-understanding writ large. So, at the end of the 20th century Frederick Lawrence described hermeneutics as "the structure of the overall human experience of the world and its articulation through language."[14]

One of the important debates in the philosophical controversies surrounding hermeneutics was whether and in what sense interpretations of texts, of artistic productions, of history, of other people's actions and of one's own experiences could be "objective," or "verified." In different ways important figures like Wilhelm Dilthey and, later, E. D. Hirsch sought to develop a "science" of interpretation, drawing on criteria such as authorial intent, contexts of reference, and psychological factors to determine definitive meaning. Without a doubt, such considerations are indeed crucial for what we believe are defensible interpretations of written texts. However, problems arise when the specific challenge of interpreting a text is analogized to the larger philosophical problem of hermeneutics as Lawrence describes it. To begin with, the idea of authorship does not transfer very well; nor does the idea of a discrete text when what one is considering is as grand as the history of human experience communicated through diverse literary/artistic/social media.

Dilthey maintained that the methods appropriate for proper understanding and interpretation, whether in reference to particular texts or to human experience at large,

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[14] Frederick Lawrence "Voegelin and Theology as Hermeneutical and Political" in Voegelin and the Theologian: Ten Studies in Interpretation. Edited by Kirby and Thompson (Edwin Mellen Press, 1983) 315.

were fundamentally different than the methods of investigation in the natural sciences. However, he also believed that interpretive methods could constitute something of a science in their own right. Thus, he claimed that there exists a certain universal structure to meaning and interpretation: "the process of understanding, insofar as it is determined by common conditions and epistemological means, must everywhere have the same characteristics."[15] Moreover, on his account, "hermeneutics determines the possibility of universally valid interpretation on the basis of an analysis of understanding."[16] This vision of hermeneutics as science that can provide universal criteria for the proper interpretation of meanings, however, has been largely rejected by the leading philosophical exponents of hermeneutics in the 20th century, including Martin Heidegger and Hans-Georg Gadamer. I likewise reject the "Diltheyian" account of hermeneutics and take Gadamer to have articulated the best way forward.

Very briefly, part of what was at stake in the different accounts of hermeneutics articulated by Dilthey and Gadamer was the problem of the so-called "hermeneutic circle." On one level this problem refers to recursive relationship between the meaning of "parts" and the meaning of the "whole." Characteristically, the meaning of any sentence or particular action only makes sense in reference to a larger narrative or overarching context. However, larger narratives are ultimately constituted and explained by their constitutive parts. Thus, there is a certain kind of circularity in the way that meaning

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[15] Dilthey, W. "The Rise of Hermeneutics" in Hermeneutics and the Study of History: Selected Works, Volume IV. Edited by R. A. Makkreel and F. Rodi. (Princeton University Press, 2006) p. 237

[16] Ibid. p.238

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takes shape. Dilthey was one of many who saw an epistemological danger in this potentially vicious circle. His science of interpretation was intended to head off "skeptical subjectivity" and to provide a foundation for the "universal validity of historical interpretation." [17] However, as many challengers pointed out, claims about how to verify or justify an interpretation are themselves open to interpretative dispute. There is no neat way out of this circle. Prior standards of coherence or intelligibility may confidently rule out some sorts of interpretations, but the domain of contestable interpretations generally remains quite large.

This problem is only magnified when extended to the question of human self understanding. This is because the "material" upon which we reflect is ever changing, and this includes not only the specific character of our individual lives but also the larger backdrop of world history. Gadamer claims that "in the linguistic character of our access to the world, we are implanted in a process of tradition that marks us as historical in essence."[18] That is not to deny a stable human nature, but rather to maintain that our nature as language innovating animals renders us open to a remarkable variety of ends, beliefs, behaviors, and practices – a variety only part of which has been displayed in the remarkable diversity of human history thus far. So, the dynamism of history presents an additional challenge to those who would like to stake out a stable set (or method) of meanings and value. Or, at least, it indicates that claims about meaning and value will

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[17] Ibid. p. 331

[18] Gadamer, "The Heritage of Hegel," in *Reason in the Age of Science*, trans. Frederick G. Lawrence (Cambridge, MA: The MIT Press, 1981) 50.

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have to provide an account of how they can be justified in light of our historical inheritance thus far and to do so in a way that is sensitive to the possibility that future historical change may provide radical new challenges.

Although Gadamer accepts that the grand interpretive enterprise in which we try to understand and articulate what is good, right, beautiful, meaningful, and such always takes place within a kind of justificatory circle, he argues that this circle is not a vicious one. The reality that we cannot ground these claims in some *a-priori*, ahistorical scientific methodology does not mean that anything goes. In fact, Gadamer thinks this way of putting the challenge is misleading, that it furthers an assumption prevalent in Western intellectual culture since Descartes that views truth as something to which we only have access through scientific methods. Rather, Gadamer argues that hermeneutic engagements can lay claim to a certain kind of rationality, which can approach a best-account-of-truth-thus-far, nonetheless cognizant that such claims to truth are provisional on being able to convincingly withstand future challenges.

Thus the problem of hermeneutics is, in part, the problem of how to philosophically conceive and justify those convictions and judgments that profoundly affect our lives but cannot be demonstrated through scientific methods. Hermeneutics after Gadamer stands on an ambiguous epistemological-ontological ground between two opposite ways of responding to this challenge. At one end there are philosophical schools of thought that embrace varieties of moral rationalism or normative cosmology. Their proponents aspire to show that our aesthetic and moral judgments can be demonstrably grounded either through *a-priori* arguments about the structure of reason, or by an

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understanding of the universe as teleologically ordered such that proper investigation and contemplation of it will reveal objective normative principles. Certain kinds of Kantians embrace the former view and certain kinds of Neo-Platonists the latter. However, for a variety of reasons, including what we could broadly characterize as the "historical turn" in philosophy, these "high" epistemological accounts are less compelling then they once were. Moreover, the fact that they are not compelling poses a problem for these approaches on their own terms.

At the other end of the spectrum lie interrelated schools of thought that take normative convictions to be entirely contingent and a-rational and find it inconceivable that one could aspire to justify them in any meaningful way. Nietzsche originates one sort of critique of this kind and did so in a way that builds upon the original insight of hermeneutics. "Interpretation" is ineluctably subjective and contingent in various respects, so if all we have in discussions of value are interpretations all the way down are we not stuck within a radically subjective and therefore "vicious" circle of justifications? There are also various structural theories that see normative convictions as epiphenomenal effects of some underlying determinants, be they economic modes of production or chemical imbalances in the brain. Hermeneutics has sought to stake out an epistemological ground between these two extremes of metaphysical rationalism and nihilistic contingency. What, then, defines a hermeneutic account of reason?

Gadamer singles out the experiences of philosophy, of art, and of history as "all modes of experience in which a truth is communicated that cannot be verified by the

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methodological means proper to science."[19] By this he means to draw our attention to those perceptions present in our everyday lives that posses an evidentiary truth because of the impact they have on us. Why is it that we are moved by certain kinds of encounters, experiences, and ideas? It is hard to give a simple account of such perceptions and effects. On one level they presume something about our nature – as humans we share, at minimum, a biological nature that first orients our concerns towards the world. They also presume something about our encounter with the world – that we can name and understand common elements of this encounter.

So, Gadamer claims, the world is already intelligible to us before we ever approach it through detailed scientific investigation. However, he also recognizes that our perceptions of the world are highly mediated through traditions of thought that we inherit. We never stand outside of such traditions, just as we never stand outside of language: "Language is not an instrumental setup, a tool, that we apply, but the element in which we live and which we can never objectify to the extent that it ceases to surround us."[20] This is the sense in which Gadamer admits to the reality of a hermeneutic circle: "The conceptual world in which philosophizing develops has already captivated us in the same way that the language in which we live conditions us."[21] Moreover, languages, communities, and traditions of thought characteristically provide ways of approaching

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[19] Gadamer, Hans-Georg. *Truth and Method*. Translated by Weinsheimer and Marshall. (New York: Continuum, 1999) xxii.

[20] Gadamer, "The Heritage of Hegel," in *Reason in the Age of Science*, trans. Frederick G. Lawrence (Cambridge, MA: The MIT Press, 1981) 50.

[21] Gadamer, Hans-Georg. *Truth and Method*. Translated by Weinsheimer and Marshall. (New York: Continuum, 1999) xxv.

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common human concerns that pose questions of meaning and value: "There is no one who does not form general views about life and death, freedom and human living together, about the good and about happiness…such theory is embedded within the practice of conditioned and lived life and is borne along with it."[22]

However, the crucial claim for Gadamer is that while we inherit traditions of ethical reflection, we also modify and innovate them based on ongoing encounters with the world (encounters with new events, alien traditions, or creative thoughts). So the circle is not a vicious one. New things come to light that challenge the adequacy of existing modes of thought, including historically contingent events, awareness of rival traditions, and existential epiphanies. One commentator explains this process in the following way: "As we come, through the work of interpretation, to understand what at first appears alien, we participate in the production of a richer, more encompassing context of meaning—we gain a better and more profound understanding not only of the text but also of ourselves."[23] Thus, there is ultimately an "interplay between our self-understanding and our understanding the world" in which "the past is handed over to us through the complex and ever-changing fabric of interpretations, which gets richer and more complex as decades and centuries pass."[24]

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[22] Gadamer, "The Heritage of Hegel," in *Reason in the Age of Science*, trans. Frederick G. Lawrence (Cambridge, MA: The MIT Press, 1981) 58-59.

[23] Bjørn Ramberg Kristin Gjesdal Stanford Encyclopedia of Philosophy entry on "Hermeneutics" <plato.stanford.edu/entries/hermeneutics>

[24] Ibid.

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Perhaps, however, this gloss presents an account of this process that is too "Hegelian," that is to say too convinced of the historical necessity of progress in understanding. Gadamer makes clear in his later work that he is not advancing an inevitability thesis concerning rational progress and the consilience of all values. Rather, he is trying to give an account of how rational progress is possible at all (and the circumstances in which it is possible) given the fundamental epistemological challenge indicated by hermeneutics. Can our arguments about meaning and value be more or less justified, even if they are not scientific? Yes, Gadamer claims, to the degree that they 1) are informed by traditions of inquiry that incorporate the best that has been thought and said within their purview of human history and 2) try to intelligibly engage new experiences and alien traditions, assimilating them where possible and rethinking their own commitments when not.

Others have characterized Gadamer's epistemological account as an "iterative process," in which new and superior understanding is developed through critical engagements with others. Although these and related paeans to dialogue may sound banal when put so crudely, they hint at the way in which Gadamer thought that, although our debates about meaning and value are fundamentally hermeneutic and not open to justification through some "scientific" method, they can nonetheless be more or less rationally defensible based on their overarching comprehension of historical traditions of thought and openness to entertaining new challenges to those traditions.

Even within the gamut of thinkers today that acknowledge Gadamer's conception of hermeneutics as the best characterization of how ethical judgments concerning the

meaning and value of goods take shape, there remain significant differences with regard to the ultimate ontological background that hermeneutics is thought to entail. Theorists such as Richard Rorty and Gianni Vattimo lean towards a "relativist" position. Vattimo argues that hermeneutics has an explicitly "nihilistic vocation," that it entails the dissolution of any ultimate ontological notion of truth.[25] Rorty is more circumspect, but likewise sees his own account of pragmatism as one that is anti-epistemological, in which he claims that truth is simply social convention and that the very notion of a difference between subjective interpretations and objective world is chimera. At the other end of the spectrum are theorists like Charles Taylor and Alasdair MacIntyre, who would like to fill out Gadamer's aspiration to provide an account of how hermeneutic understanding can hope to rationally progress toward some ultimate truth. Both MacIntyre and Taylor are committed to showing how hermeneutic engagements can, under certain conditions, claim to enable a fuller participation in some ontological reality.

One thing that unites almost all theorists influenced by Gadamer is the importance they attach to the phenomenon of persuasion. This is the crux of what needs explaining. Why do certain people find certain claims about and exemplars of beauty, goodness, truth, and justice compelling? Moreover, what role does human nature play in accounting for the persuasiveness of different ideals and judgments about such first order goods? And how important are capacities and habits of thought that require long periods of training and enculturation?

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[25] Vattimo, Gianni. *Beyond Interpretation: The Meaning of Hermeneutics for Philosophy.* (Stanford University Press, 1997).

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For the purpose of my basic argument, one can remain agnostic about the ultimate "ontology" underlying hermeneutics and still follow the conclusions a long way down the road– whether one is convinced that hermeneutic inquiries can never break out of radical subjectivism, or that hermeneutic engagements can progress by achieving a more complete participation in some ontological reality. That is to say that one can simply accept the brute fact that persuasion happens within hermeneutic frameworks and be interested in the behavioral implications of successful persuasion. However, if one adopts the view of radical subjectivism it is harder to conceive of an account of *rational* persuasion, as it is not clear why any persuasive appeal *should* be preferred to another. Persuasion then becomes a matter of mystery or of power. This position does present serious limitations on the ultimate conclusions I wish to draw and is, I believe, philosophically inadequate. Thus I need to be clear that, while a hermeneutic account of persuasion is a necessary and key component of my larger argument (and alone enables me to advance many of my claims), only an account of hermeneutics that entertains the possibility of rational progress is sufficient to establish all that I hope to in this study.

To return to the larger contours of the argument, the central question is how evaluative judgments about first order goods come to be formed and whether such judgments can lay claim to some sort of justification. It is undeniable that such judgments have profound behavioral implications. Can we say more about their formation and corrigibility that would be helpful for addressing social problems that preoccupy social scientists? Note that the particular claim that such judgments are largely shaped by traditions of hermeneutic reflection tremendously complicates this aspiration. This is

because, if these judgments are hermeneutically formed and hermeneutic frameworks emerge historically from debates internal to their own traditions of thought and the contingent conditions of practical life they confront, then it follows that, even if there is a kind of rational development within and between particular hermeneutic frameworks over time, the judgments about good that they accrue will be essentially historical in important respects. *That is to say that if judgments about goods are indeed hermeneutically constituted, it will not be possible to persuasively engage such judgments through an a-historical scientific method.*[26]

A great deal hinges on this point. I have argued that ethical convictions do matter for human behavior but cannot be (or at least have not yet been) understood and managed through scientific methods. The conclusion we arrive at is that there is no way of getting around the need to engage ethical convictions at the level of their hermeneutic constitution. There is no way of getting around the permanent need for ethical persuasion.

Of course, I do not mean to suggest that there are not scientific insights to be had regarding how to make people do what we want. There are drugs that will, for a short

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[26] I should note that this remains an exceedingly controversial thesis. Consider C. Mantzavinos' recent book, *Naturalistic Hermeneutics*, published by Cambridge University Press, which continues to defend the unity of the natural and social sciences against the idea of hermeneutics: "*Naturalistic Hermeneutics* proposes the position of the unity of the scientific method and defends it against the claim to autonomy of the human sciences. Mantzavinos shows how materials that are 'meaningful', more specifically human actions and texts, can be adequately dealt with by the hypothetico-deductive method, the standard method used in the natural sciences. The hermeneutic method is not an alternative method aimed at the understanding and the interpretation of human actions and texts, but it is the same as the hypothetico-deductive method applied to meaningful materials. The central thesis advocated by Mantzavinos is, thus, that there is no fundamental methodological difference between natural sciences, social sciences, and humanities. Advanced students and professionals across philosophy, social and political theory, and the humanities will find this a compelling and controversial book." C. Mantzavinos. *Naturalistic Hermeneutics* (Cambridge, 2005).

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time, make people pliant and obedient, psychotic and violent, or simply knock them out cold. There are techniques of "brain washing" that totalitarian regimes have used on dissidents, apparently with some success. Finally, there may be many natural goods, characteristically embraced by most hermeneutic traditions, instrumental appeal to which can motivate masses of people in ways that are useful and may appear law like. Credibly threaten people with torturous death and you will get many to comply (although, surprisingly, not all, as numerous contemporary examples from suicide terrorists to Burmese monks demonstrate).

Indeed as I suggested in my examination of statistical methods and formal modeling, much of the social structure and behavioral predictability that the social sciences profitably discover is ultimately derivative of certain overarching hermeneutic frameworks of value (configurations of incentives and power being somewhat endogenous to such frameworks). The question of how "incentives" motivate people to behave in certain ways is a crucial one. *But incentives only make sense as incentives within larger judgments about what is valuable.* In so far as frameworks of value are stable and shared, the design and analysis of incentives will be a fruitful enterprise. But even then it is paramount that we distinguish between illegitimate and legitimate forms of incentives (between say, extortion and voluntary exchange), even if the material circumstances may look similar in certain cases.[27]

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[27] For an excellent exploration of the problems and nature of "incentives" see Ruth Grant's "Ethics and Incentives: A Political Approach" in the *American Political Science Review* (100:0101, 29-39, Cambridge University Press, 2/2006); "The Ethics of Incentives: Historical Origins and Contemporary

To put this in the context of our concerns about hermeneutic persuasion, there is a significant difference between getting someone to do what you want by administering a drug or beating them into submission and getting someone to do what you want by convincing them of the goodness and desirability of that action on its own terms. Of course, this distinction admits of a spectrum that may make it difficult to distinguish tough cases concerning the difference between rational persuasion and powerful incentives at the middle.[28] Nonetheless, the distinction between rational persuasion and force is an essential one. To draw on Simone Weil's famous imagery from her commentary on the *Iliad*, force as it is used, for example, in war turns people into "stone."[29] It treats them as material objects that either submit or break without any purposeful agency of their own.

By contrast, rational persuasion characteristically aims to secure agreements in judgment with regard to criteria that each agent has good reasons to accept. These dynamics of persuasion are easier to perceive in contexts where there is broad consensus regarding the criteria of justification and a commonality of purpose – for example, the natural sciences.[30] But to return to the challenge that hermeneutics presents, what are we to make of the idea of rational persuasion with regard to ultimate questions of value and

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Understandings" in *Economics and Philosophy* 18 (1) 2002: 111-139; and her forthcoming *Strings Attached: The Ethics of Incentives* (University of Chicago Press).

[28] For an excellent treatment of this precise issue, see Grant, Ruth. "Ethics of Incentives" *Economics and Philosophy* 18:1:111-139 Cambridge University Press (2002).

[29] Weil, Simone. *The Iliad or the Poem of Force.* (Peter Lang Publishing, 2003).

[30] Operating, if one wants to push the point, in periods of what Kuhn calls "normal science."

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goodness if the criteria of judgment people employ are themselves internal to rival viewpoints? The question indeed extends to the notion of rational persuasion in any domain – claims about what constitute legitimate appeals in contrast to illegitimate exercises of force will always take for granted a prior hermeneutic framework with its particular notions of rationality and legitimacy.

While acknowledging all this, indeed because of such claims, many of those indebted to Gadamer's conception of hermeneutics hold that the persuasiveness of hermeneutic traditions – that is, their ability to draw and maintain the allegiance of various peoples – is itself *prima facie* support for the "truthfulness" of these traditions. It says something about a way of viewing the world if people continue to find it meaningful and resilient to challenges over time. This is not to adopt a simplistically "democratic" account of truth, nor is it properly an evolutionary account either, although some have sought to draw that kind of analogy. Rather it is inspired by the genuine question of why certain hermeneutic traditions stick and/or change as they do over time.

The ability of certain hermeneutic traditions to remain persuasive to various communities is one kind of epistemological-ontological justification. Or, as one of Rorty's intellectual biographers put it from another direction, since "there is no basis for deciding what counts as knowledge and truth other than what one's peers will let one get away with in the open exchange of claims, counterclaims and reasons," what survives from such exchanges over time has a certain claim to truth.[31] On the relativist reading, of

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[31] Guignon, Charles B. and Hiley David R. *Richard Rorty*. Cambridge, 2003.

www.manaraa.com

course, "truth" has to appear in quotations, because the "truths" that emerge from any age are tinged with the ironic recognition of their fragility and contingency. However, at the other end of the spectrum, theorists like Taylor and MacIntyre have sought to identify something like "meta criteria" of socially grounded rational persuasion, drawn (as they admit any criteria must be) from our particular historical philosophical inheritance, that would provide reason to believe that under certain kinds of conditions the debates within and between hermeneutic traditions could claim a kind of rationality and legitimacy. This sort of aspiration is not new, but the specific approaches of Taylor and MacIntyre need to be distinguished from prominent modern accounts of hermeneutic reason in politics.

6.4 Structuring a Politics of Persuasion

The question of the origins of ethical convictions and the possibility of rationally persuading others (or being persuaded) in matters of ethics is relevant to politics in two important respects. First, no political order can escape making ethical judgments. We understandably want to make sure that whatever ethical judgments are expressed can lay claim to some sort of rational legitimacy. Procedures such as voting may be a pragmatic way of trying to establish legitimacy, but they can only do so if they reflect deeper realities about the persuasiveness of the judgments expressed. Second, in order to function well, social institutions depend on a wide range of ethical convictions being present in a populace, convictions that provide individuals with motivations to uphold these institutions and to act appropriately within them. Beyond questions regarding the legitimacy of the particular ethical judgments of the state, there is this underlying issue of

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what ethical norms prove compelling sources of motivation for citizens at large. A hermeneutic account of ethical reason in line with Gadamer, MacIntyre, and Taylor's thought provides resources for addressing both of these challenges (and a helpful corrective to the way these concerns are approached in the literature on deliberative democracy and discourse ethics).

However, a prior issue stands in the way, namely the question of whether these two challenges really exist. A prominent tradition of liberal political thought suggests that ethical convictions are not particularly important for politics and, moreover, that liberal states can and should be neutral with regard to competing conceptions of the good. Kant's claim that a society of devils could be ruled well by the right institutions is emblematic of the former perspective, which considers the ethical convictions of citizens of little import (beyond indicating something that institutional designers need to take into account to engineer desired outcomes). With regard to the latter perspective, the political approaches of two of our most eminent contemporary theorists, Robert Nozick and John Rawls, idealize certain notions of neutrality.

Nozick envisions a "framework for utopias" in which "people are at liberty to join together voluntarily to pursue and attempt to realize their own visions of the good life in the ideal community but where no one can impose his own utopian vision on others (312)."[32] This is a sort of voluntarist paradise in which everyone gets to pursue whatever goods they want, and the minimal state, which consists only of non-interference rights,

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[32] Robert Nozick. *Anarchy State and Utopia* (Basic Books, 1974) 312.

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remains completely neutral with regard to these visions of the good. A version of the Lockean proviso, however, requires Nozick to admit the necessity of further limits on property rights, as well as the problems people confront in trying to negotiate a way of life together when they have to share a common political space. Nozick hopes to map out an account of rights that allows for the fullest pursuit of individual preference consistent with the equal pursuit of other citizens' preferences. But he gives insufficient attention to the ways in which the material constraints of living together with others require us to make compromises with and binding judgments over those who may not share our conception of the good. [33]

An ideal of neutrality is also central to Rawls' thought, both as a feature of the disinterested anonymity to be found behind the veil of ignorance and as a principle of the legitimate aims of the state. Although Rawls recognizes that the state can never be truly neutral in its effects, he maintains that it ought to be neutral in its aims, which is to say "the state is not to do anything intended to favor or promote any particular comprehensive doctrine [of the good] rather than another, or to give greater assistance to those who pursue it."[34] There is a notorious ambiguity in Rawls' thought concerning what count as "comprehensive" conceptions of goods (versus political or limited conceptions). Rawls' account of neutrality has been challenged by many political theorists (Raz, Nagel, Taylor, MacIntyre, Sandel, Tomasi, and Honig) from many different perspectives. Nagel's criticism cuts to the essential point:

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[33] Or be bound by the judgments of others.

[34] John Rawls. *Political Liberalism*. (Columbia University Press, 1993) 193.

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It is a fundamental feature of Rawl's conception of the fairness of the original position that it should not permit the choice of principles of justice to depend on a particular conception of the good over which the parties may differ.

The construction does not, I think, accomplish this, and there are reasons to believe it cannot be successfully carried out. Any hypothetical choice situation which requires agreement among the parties will have to propose strong restrictions on the grounds of choice, and these restrictions can be justified only in terms of a conception of the good. It is one of those cases in which there is no neutrality to be had, because neutrality needs as much justification as any other position.[35]

Notions of what justice requires, particularly in a post-Enlightenment (or post-Kantian) age, are parasitic on conceptions of the good. The idea of being genuinely neutral towards all conceptions of the good in the way we carve out political principles of justice is chimerical. That is not to say that the concept of neutrality has no meaning, only that its meaning, for example with regard to how a judge should review evidence in a court of law, is contextually limited. "Complete neutrality" would mean never being able to decide anything, a kind of species of disinterestedness.

Indeed the ideal of neutrality in politics has been persuasively attacked from a number of other directions. Formal theorists who work on problems of voting in the tradition of Arrow have drawn attention to the fact that democratic voting systems cannot avoid outcomes that many citizens disagree with.[36] Collective decision making always

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[35] T. Nagel. "Rawls on Justice" *Philosophical Review* 82 (1973) 220 reprinted in *Reading Rawls* ed N. Daniels (Oxford, 1975) 8-9.

[36] For an interesting treatment of this problem see A. Sen "The Impossibility of a Paretian Liberal" *Journal of Political Economy* 78 (1970) 153-157. Although it's practical implications may be slight, see Harmut Kliemt and Anthony de Jasay "The Paretian Liberal, His Liberties and His Contracts" in *Justice and Its Surroundings* (Indianapolis: Liberty Fund, 2002) 126-147.

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entails losers, except in the rare case of unanimity. There may be reasons to limit the scope of collective decision making, leaving a greater amount of choices up to individuals, but there are many social questions that only admit of a collective response, particularly with regard to criminal law.

Carl Schmitt launched a related critique of liberal democracies, arguing that their procedural rules and commitment to something like an ideal of neutrality led to indecision. This made liberal democracies unable to distinguish genuine social threats and act decisively against them. Liberal, in Schmitt's view, are too concerned not to make ethical judgments – the liberal, faced with the question "'Christ or Barabbas?,' [answers] with a proposal to adjourn the meeting or set up an investigative committee."[37] Thus, Schmitt ultimately claims, "there is no liberal theory of politics, only a liberal critique of politics."[38]

Against Schmitt, defenders of liberalism have argued that indeterminacy of judgment is not a practical problem for liberal democracies, despite the way in which neutrality is lauded by liberal theorists. All societies in fact reach a wide range of determinate conclusions that are coercively enforced about what is tolerable behavior, based on various notions of interests, justice, and the common good. A weakness of much late-modern liberal theory is its desire to hide or disavow the ethical judgments imposed by liberal societies.

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[37] Carl Schmitt. *Political Theology*. (Chicago, 2006) 62.

[38] Carl Schmitt. *The Concept of the Political*. (New Brunswick, NJ: Rutgers University Press, 1976) 70.

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We cannot get around the problem of having to make ethical judgments, which in a political context will, upon occasion, entail compelling others to live under rules inspired by conceptions of the good they do not share. Realizing this simply confirms how high the stakes of political judgment are, and this should increase our desire to see such judgments formed by procedures that reflect reasoned persuasion.

As for the other question of whether social institutions actually depend on widespread ethical convictions in order to function, we have excellent reasons to believe that, *pace* Kant and his society of devils, this is the case. I have already documented how the project of designing institutions through the structuring of incentives runs up against both the problem of diverse motives and an unmanageable regress of incentives. As Avner Greif helpfully explains, "studying political order or disorder requires examining the motivation of political actors to abide by the particular rules…in other words, a comprehensive understanding of political order or its absence and of the behavior of the state's agents requires considering the motivation that influences the behavior of the relevant individuals."[39] Such motivations may be diverse and complex. Common ethical convictions help direct the most important motivations that agents bring to social life, creating the possibility of harmonious interaction, or at least avoiding possible conflicts. Moreover, social interactions are always structured by some understanding of what we can expect from others and what is due to others. As MacIntyre points out, "every society

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[39] Avner Greif. *Institutions and the Path to the Modern Economy*. (Cambridge 2006) 9.

is constituted by members whose behavior embodies a set of beliefs about the workings of that particular society: how individuals are to be classified and ranked, who owes what to whom under what circumstances, what the consequences are likely to be of breaking rather than keeping different types of rules."[40] This insight, however, goes deeper than a simple point about the reasons people have for complying with rules.

Charles Taylor draws our attention to the way in which such expectations necessarily engage larger questions about the nature and meaning of our own good and the common goods we share with others: "Every person, and every society, lives with or by some conception(s) of what human flourishing is: what constitutes a fulfilled life? what makes life really worth living? What would we most admire people for? We can't help asking these and related questions in our lives. And our struggles to answer them define the view or views that we try to live by."[41] Moreover, individuals do not independently answer these questions in a vacuum. As the account of hermeneutic reason outlined above suggests, our ability to reason about such questions is always informed by a tradition of thought that precedes us, to which we are inducted as a member of any language speaking, cultural community. We no doubt innovate new responses in various ways, but the traditions of thought that precede us provide a prior horizon against which innovation can take place.

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[40] Alasdair MacIntyre. "The Indispensability of Political Theory" in D. Miller and L. Siendentop, *The Nature of Political Theory* (Oxford: Clarendon Press, 1983) 19-20.

[41] Charles Taylor. *A Secular Age*. (Harvard, 2007) 16.

Taylor argues that answers to such questions of meaning and their corresponding social implications come to be systematized in what he describes as "social imaginaries." By "social imaginaries," Taylor means to indicate "the ways that people imagine their social existence, how they fit together with others, how things go on between them and their fellows, the expectations that are normally met, and the deeper normative notions and images that underlie these expectations."[42] Social imaginaries are more than elite theory or mass ideology. Unlike elite theory, social imaginaries are broadly accepted and provide a common understanding "that makes possible common practices and a widely shared sense of legitimacy."[43] Unlike mere ideology, social imaginaries "also have a constitutive function, that of making possible the practices they make sense of, and thus enable."[44]

In Taylor's view modern liberal democracies are constituted by, indeed could not exist without, a number of social imaginaries. These include the notion of a market economy of mutual benefit, the idea of a public sphere in which debate can be freely conducted and citizens can criticize abuses of power, and the ideal of a self-governing people with the ability to bring into being new political orders through which their common purposes can be realized. Taylor shows how each of these imaginaries emerged in the modern period through a combination of explicit theorizing, political action, and

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[42] Charles Taylor, *Modern Social Imaginaries*. (Duke University Press, 2004) 23.

[43] Ibid., 23.

[44] Ibid., 183.

technological change, with the end result that these imaginaries mark a genuine innovation in or break with past beliefs and social orders.

Throughout Taylor's treatment of this phenomenon, in such works as *The Sources of the Self*, *Modern Social Imaginaries*, and *Secular Age*, he is keen to advance a simple, underlying thesis, namely: "that central to Western modernity is a new conception of the moral order of society."[45] Taylor notes that we are apt to lose sight of this fact and think that the order and self understanding we are familiar with is natural and permanent. However, Taylor emphatically argues that it is a mistake to "naturalize the features of the modern liberal identity," when in fact that identity is but "one historically constructed understanding of human agency among others."[46]

Commenting on the peculiar ethical features of contemporary Western societies, Taylor notes, "We live in an extraordinary moral culture, measured against the norm of human history, in which suffering and death, through famine, flood, earthquake, pestilence, or war, can awaken worldwide movements of sympathy and practical solidarity."[47] Not only is this exceptional from a historical point of view, it is still exceptional from the viewpoint of various societies to be found around the globe today. Many ethical judgments taken for granted by modern Western citizens were not widely held as recently as half a century ago in portions of the West, and continue to be alien to many in non-Western societies. Moreover, it is likely a mistake to think that those in non-

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[45] Ibid., 2.

[46] Charles Taylor. *A Secular Age*. (Harvard, 2007) 571.

[47] Ibid., 25.

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Western societies will be persuaded to adopt liberal ethical judgments merely as a result of adopting modern technology and being exposed to scientific knowledge, as prominent secularization and modernization theses have suggested. Against those who see Western modernity as the development of scientific enlightenment which dispels erroneous superstitions thereby enabling humans to concentrate on fulfilling their true nature and true desires, Taylor suggests "…the possibility that western modernity might be powered by its own positive visions of the good, that is, by one constellation of such visions among others, rather than by the only viable set left after the old myths and legends have been exploded."[48]

When we examine the way people in any society behave, much of that behavior has to be understood as being shaped by historically developed forms of moral self understanding. Visions of the self and of what constitutes ultimate goods, which Taylor documents and describes so well in his work, profoundly influence the way people act. Simply giving people new institutions, or placing them in new institutional contexts, may not direct behavior in ways that institutional designers hope. Institutions only work as they do in tandem with the motivations and convictions that agents bring to them. This is because, as Ruth Grant has pointed out, in elaborating an insight very close to Taylor's, "every political actor operates within a conceptual regime as well as within an institutional one."[49] And the most important conceptual regimes are those that express

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[48] Ibid., 571.

[49] Ruth Grant. "Political Theory, Political Science, and Politics" in *Political Theory*, Vol. 30, No. 4, What Is Political Theory? Special Issue: Thirtieth Anniversary (Aug., 2002), 577-595.

ethical visions that provide persuasive accounts of the nature and interrelation of fundamental goods.

Recognizing this forces us to think about institutions in a new way. We come to a better appreciation of the contingency of our own social institutions and the conditions for successfully spreading these institutions elsewhere. If institutions are able to facilitate desired social outcomes only when embedded in a particular social order of shared, ethical commitments it follows that in order to sustain desirable features of these institutions we also have to sustain the ethical visions that motor them. This sort of insight has led to a renewed interest in understanding the sources of ethical formation. Although various sorts of public institutions are themselves undoubtedly important for transmitting ethical visions, it is also the case that ethical commitments are forged at sites far removed from explicitly public or political institutions.

The political question that a hermeneutic account of ethical reason raises is how, on the one hand, to support the extra-political sites in which traditions of ethical formation are primarily located (family, religion, schools, voluntary associations) and, on the other hand, to structure public/political institutions such that they express and protect essential matters of ethical consensus that hold a society together – while also providing spaces, procedurally and otherwise, in which rival ethical perspectives can come into argumentative conflict. However, the nature of this problem needs spelling out.

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MacIntyre and Taylor both contend that ethical formation takes place at various sites, but ways of thinking about ethics tend to be systematized with reference to particular dominant conceptions of the good. Taylor's "social imaginaries" refer to very broad visions, widely shared within any society, about how basic social goods are interrelated. However, MacIntyre's thought has been more focused on the narrower/more extensive ethical commitments he sees rising out of particular traditions of ethical inquiry. Even within a society united by common social imaginaries there may be many rival, and incompatible, traditions of ethical reflection - utilitarian, Kantian, Catholic-Thomist, hedonist, etc.

MacIntyre argues, in a way characteristic of a hermeneutic account, that reasoning about fundamental goods takes place, in the first instance, *within* such traditions. Such traditions will always be working out issues and new problems from within their internal, interrelated understandings of reason and ethics. However, rational "progress" occurs not only internally to such traditions, in light of their own standards, but also in the conflict between traditions, as they come to illuminate each others' problems in new ways and provide hitherto unrecognized resources for dealing with such problems. In fact, in MacIntyre's view, rational inquiry is essentially social and depends on both having appropriated essential features of traditions of thought that precedes us and on trying to persuade and be persuaded by others. Thus argumentative conflict, both within, but primarily between traditions of ethical thought is epistemically productive:

> It is by having our reasoning put to question by others, by being called to account for ourselves and our actions by others, that we learn how to scrutinize ourselves and they scrutinize us and how to understand

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ourselves as they understand us. When others put us to the question and call us to account, it is generally in situations in which they are unclear about just what it is that we take ourselves to be doing or about why we take it to be reasonable to act in this particular way or perhaps both. They therefore invite us to make ourselves intelligible to them, so that they may know how to respond to our actions. And what we find when we attempt to make ourselves intelligible to such questioning others is that sometimes we also need to make ourselves intelligible to ourselves.[50]

Individuals and groups need, in the first instance, their own "private" spaces in which to transmit and develop traditions of ethical reflection. We have come to think of this sort of freedom and autonomy as essential to a liberal regime, although there are liberal misgivings about the conclusions that different ethical traditions may arrive at. Contemporary political theory is replete with efforts to delineate the proper boundaries between private and public, between the rights of groups to pursue their own ethical conceptions and the concerns others have with the social impact of such conceptions or the ways in which they circumscribe the capacities of children.

The general "solution" has been two fold. First, there are constitutional protections of rights and liberties that place ostensibly "minimal" constraints on the kind of ethical visions that can be pursued within a liberal regime. For example, communities that sanction sex with young children are outlawed. These minimal constraints are thought to reflect the areas of widest ethical consensus within the larger society.

The second aspect of the solution has been to provide various public and political forums that allow rival ethical traditions to debate competing conceptions, particularly

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[50] Alasdair MacIntyre. *Dependent Rational Animals: Why Human Beings Need the Virtues* (Open Court, 1999) 148.

when they pertain to the need for some collective decision. There must of course be procedural rules that enable a decision to be reached even if not all are persuaded, however the hope is that substantive debate can be incorporated as an important aspect of the procedure of decision.

This political problem shares interesting features with what MacIntyre describes as his vision of how rational inquiry and disagreement should be structured within a university. MacIntyre envisions the university as "a place of constrained disagreement, of imposed participation in conflict."[51] In this sense the university seeks conflict in a way that political society likely shouldn't. However, there are ways in which the problem of ethical reason may be similar for both, particularly with regard to the institutional implications. MacIntyre maintains that protagonists in debates are always protagonists from a particular, ethical point of view.

The idea of rational inquiry in the university involves two components, on MacIntyre's account. First, protagonists of a tradition "advance inquiry from within that particular point of view, preserving and transforming the initial agreements with those who share that point of view and so articulating through moral and theological inquiry a framework within which the parts of the curriculum might once again become parts of a whole."[52] However, confidence in one's conclusions requires going beyond the internal perspective provided by a tradition of thought. There is a second aspect of rational inquiry, namely the need "to enter into controversy with other rival standpoints, doing so

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[51] MacIntyre, *Three Rival Versions of Moral Inquiry* (Notre Dame Press, 1991) 231.

[52] Ibid., 231

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both in order to exhibit what is mistaken in that rival standpoint in the light of the understanding afforded by one's own point of view and in order to test and retest the central theses advanced from one's own point of view against the strongest possible objections to them to be derived from one's opponents. So systematically conducted controversy would itself contribute to systematically conducted moral and theological enquiry, and both would inform that teaching in which students were initiated into both enquiry and controversy."[53] This is an idealization of course, but indicates what MacIntyre takes to be the two fundamental aspects of rational debate, once we accept a kind of hermeneutic account of "tradition constituted rationality."

However, there is an additional, and essential, task for those engaged in debate within and between traditions of thought. They must contribute to and maintain an institutional system in which such debate is possible in the first place: "each of us would also have to play a second role, that not of a partisan, but of someone concerned to uphold and to order the ongoing conflicts, to provide and sustain institutionalized means for their expression, to negotiate the models of encounter between opponents, to ensure that rival voices were not illegitimately suppressed, to sustain the university – not as an arena of neutral objectivity, as in the liberal university, since each of the contending standpoints would be advancing its own partisan account of the nature and function of objectivity - but as an arena of conflict in which the most fundamental type of moral and

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[53] Ibid., 231

theological disagreement was accorded recognition."[54] This, of course, presumes that rival ethical traditions have an interest in seeing such debate take place. Those who want to shut down such debate become a threat to all participants, and there is a rationale for excluding or constraining the participation of such partisans.

Many contemporary political theorists have sought to model a hermeneutic approach to political disagreement and political institutions along the lines of an academic debate. There are both strengths and weakness to this idea. The turn to "deliberative democracy" and "discourse ethics" is in some sense necessary to deal with the collapse of enlightenment hopes of demonstrably grounding political morality in abstract, a priori reason. However, the devil is in the details of such proposals.

A way of thinking about politics advanced by Locke, Rousseau, and Kant, rooted in the idea of a social contract and human autonomy, hoped to ground and legitimate political institutions by showing what free, equal, and rational agents would consent to. But the demise of enlightenment reason and the truth of hermeneutics complicate attempts to universally ground politics in this way. The turn to discourse ethics and deliberative democracy, articulated and exemplified by Jurgen Habermas, represents an attempt to provide a new ground of politics secured by reasoned deliberation. As Seyla Benhabib explains, "Instead of asking what an individual moral agent could or would will, without self-contradiction, to be a universal maxim for all, one

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[54] Ibid., 231

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asks: What norms or institutions would the members of an ideal or real communication community agree to as representing their common interests after engaging in a special kind of argumentation or conversation?"[55]

Although Habermas's writings are voluminous, his thoughts complex, and the details of his position subject to periodic shifts, his basic proposal is that a collective choice is only valid and justified if everyone affected by it could accept it as the outcome of a reasonable discourse. However, everything then rests on what we take to constitute a "reasonable discourse." For Habermas a reasonable discourse is one that takes place within the "ideal speech situation," which requires, among other things, that: "(2.1) Every speaker may assert only what he really believes. (2.2) A person who disputes a proposition or norm under discussion must provide a reason for wanting to do so. (3.1) Every subject with the competence to speak and act is allowed to take part in a discourse. (3.2) a. Everyone is allowed to question any assertion whatever. b. Everyone is allowed to introduce any assertion whatever into the discourse. c. Everyone is allowed to express his attitudes, desires and needs. (3.3) No speaker may be prevented, by internal or external coercion, from exercising his rights as laid down in (3.1) and (3.2)."[56] Viewed from the perspective of a regulative ideal, these requirements indeed appear eminently reasonable, but viewed from the perspective of actual human discourse we see that they in fact impose rather high requirements.

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[55] Seyla Benhabib. "In the Shadow of Aristotle and Hegel: Communicative Ethics and Current Controversies in Practical Philosophy." *Philosophical Forum* 21 (Fall-Wint 1989-90) 1.

[56] Jurgen Habermas "Discourse Ethics: Notes on a Program of Philosophical Justification" in *Moral Consciousness and Communicative Action* (MIT Press, 1990) 88-89.

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But the problem with Habermas's ideal is not just that it may be practically unrealizable in certain respects, but that it also stipulates that a certain type of universality is expected and need apply to the decisions reached in deliberative discourse. Regarding this, Habermas asserts, "Every valid norm must satisfy the condition that the consequence and side-effects which foreseeable follow from its general compliance can, for the satisfaction of the interests of every individual, be accepted without force by those affected."[57] As Michael Walzer explains in a critical commentary on Habermas's proposal, "the ideal speakers have full self knowledge, but are committed to assent only to those interests and values that can be universalized."[58] Indeed, Michael Kelly views as Habermas's discourse principle "a modified dialogical version of the categorical imperative."[59]

Many theorists have pointed out that Habermas's and others' proposals for models of deliberative debate tend to have written into them fairly extensive liberal and universalizing commitments. Indeed, Selya Benhabib suggests of the Habermasian model that "One extracts from the ideal speech situation what one has…put into it."[60] Obviously, we would like to structure social institutions in such a way that they can support deliberative debate, but the prospects of "designing debate" ahead of time can

[57] Jurgen Habermas "On Morality and Ethics- what makes a lifeform 'rational'" in *Rationalitat Philosophische Beitrage* (Frankfurt: Suhrkamp, 1984) 219.

[58] Michael Walzer, "A Critique of Philosophical Conversation" in *Thinking Politically* (Yale, 2007) 26.

[59] Michael Kelly. "MacIntyre, Habermas, and Philosophical Ethics", *The Philosophical Forum*, Vol. XXI, Nos. 1-2 (Fall-Winter 1989-1990) 79.

[60] Quoted in Michael Walzer, "A Critique of Philosophical Conversation" in *Thinking Politically* (Yale, 2007) 26.

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quickly become incoherent. Debates that are structured by initial requirements of what

can be said, how it can be said, and stipulations on the type of permissible conclusions

are very peculiar debates. Indeed, Walzer notes that "Curiously, once one has a

conversational design, it is hardly necessary to have a conversation"[61] So, discourse

ethics in the tradition of Habermas runs into the problems both because it stipulates

exceedingly unrealistic conditions on the way debate takes place and presupposes

substantial ethical commitment in the very design of the discourse.

Along these lines, Malachi Hacohen notes:

> Critiques of discourse ethics have been legion. If some argue that it does
> too little, others suggest it does too much: under the guise of a tautology, it
> sneaks into the discussion a plethora of liberal values, which renders
> presumably neutral procedures tracks for substantive decisions swayed in
> particular direction. Efforts to elaborate procedures and state the
> obligations of participants in the debate lend themselves to such charges.
> The more, it seems, the proponents of discourse ethic try to say, the less
> convincing their claims to impartiality appear.[62]

And Bryan Garsten joins in the chorus of theorists who denounce discourse ethics

for ultimately being anti-political and contrary to the process of genuine persuasion:

> Discourse theory follows these philosophers [Hobbes, Kant, Rousseau] in
> asking citizens to substitute for private judgments ones that emerge from
> an authoritative public point of view – a sovereign set of procedures.
> Theorists of deliberative democracy who follow Habermas on this point
> this remain within the grip of the campaign against controversy and the art
> of controversy. In spite of their interest in disagreement, they often find

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[61] Ibid., 25.

[62] Malachi Hacohen. "Liberal Dilemmas and Moral Judgment" in *Naming Evil, Judging Evil* ed by Ruth Grant (Chicago, 2006) 180.

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themselves opposing rhetorical deliberation and the politics of persuasion.[63]

Moreover, Mathew McCubbins raises an eminently practical concern, suggesting it is far from clear that deliberation is always desirable in politics: "Deliberation is costly, difficult, and often inconclusive. We should not aim to have most of our policies subject to an intentionally deliberative process. Rather the goal should be that whatever decision that is made by experts meets a minimum criteria of claim on behalf of public good and that stakeholders who are most affected have the possibility of fighting it."[64] And this ultimately requires only that there be "complete and transparent disclosure rules." McCubbins shares with Habermas the notion that if some people are affected by a policy we should ask whether it is a policy they could reasonably be persuaded to accept, but McCubbins points out that actual deliberation is infeasible, and in any case there is no reason to believe people can be brought to agree on various matters through deliberation.

These legion problems, however, should not lead us to abandon the ideal of deliberation and persuasion in politics, indeed it's hard to know how these could be abandoned as regulative ideals. The task is to provide a realistic and ethically defensible account – informed as any account must be by the current range of dominant ethical perspectives in our society – that can suggest what sort of institutions enable ethical convictions 1) to be cultivated in accord with their own logics and 2) publicly contested with regard to the implications they hold for political judgments. We need a way of

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[63] Bryan Garsten. *Saving Persuasion: a Defense of Rhetoric and Judgment*. (Harvard, 2006) 191.

[64] Presentation at IHS Social Change Workshop, University of Virginia, Charlottesville VA 2007.

understanding the implications of a hermeneutic account of ethical reason for political life – one which illuminates what is possible for formal/procedural institutions to accomplish as well as what lies beyond the scope of institutional management (and depends simply on whatever persuasive resources emerge to motivate responses to social problems).

The sketch of such an account is, I believe, available in a related set of points made by Seyla Benhabib and Malachi Hacohen.

Benhabib notes that many of Habermas's deliberative ideals, particularly those based on universality and consensus, can and should be relaxed. Moving away from a high ideal of perfect deliberation, we can arrive at a more modest conception of deliberation, rooted in existing agreements and the search for ways to negotiate and make compromises with regard to ongoing disputes. That is:

> When we shift the burden of the moral test in communicative ethics from consensus to the idea of an ongoing moral conversation, we begin to ask not what all would or could agree to as a result of practical discourses to be morally permissible or impermissible, but what would be allowed and perhaps even necessary from the standpoint of continuing and sustaining the practice of the moral conversation among us. The emphasis now is less on *rational agreement*, and more on sustaining those normative practices and moral relationships within which reasoned agreement *as a way of life* can flourish and continue.[65]

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[65] . Seyla Benhabib. "In the Shadow of Aristotle and Hegel: Communicative Ethics and Current Controversies in Practical Philosophy." *Philosophical Forum* 21 (Fall-Wint 1989-90) 12.

This approach leads Benhabib to conclude against Habermas that conceptions of the good can be a matter of ongoing practical debate, even though such debate will be inconclusive and periodically draw lines that alienate some citizens:

> I see no reason why questions of the good life as well cannot become subject matters of practical discourses. It may very well be that discourses will not yield conceptions of the good life equally acceptable to all; however, there is a difference between assuming apriori that certain matters are questions of the good life and therefore inappropriate matters of moral argument, and assuming that a moral community will establish a line between individual conceptions of the good to be pursued freely and shared norms and values to be cultivated collectively. It is crucial that we view our conceptions of the good life as matters about which intersubjective debate is possible, even if intersubjective consensus, let alone legislation remains unattainable in these areas. Only through such argumentative processes, however, can we draw the line between issues of justice and the good life in an epistemically plausible manner, while rendering our conceptions of the good life accessible to moral reflection and moral transformation.[66]

Malachi Hacohen offers another kind of corrective to mainstream accounts of deliberative ethics, arguing that they "may need to be adjusted to recognize multiple formative sites, not all subject to the same level of rational control."[67] That is:

> We may need to change course in our thinking about discourse ethic and deliberative democracy by acknowledging that public debate may not be the major site where moral judgment and political decision are formed, although it remains the highest court. Discourse ethic need not insist that moral judgment is always a product of deliberation alone. Participants in debate rely on background knowledge, consisting of infinite judgments and decisions, which become a subject for public debate only when they seem to impinge on contentious problems on the agenda. This is neither to suggest that discourse ethic and deliberation have no role in settings such

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[66] Ibid., 16.

[67] Malachi Hacohen. "Liberal Dilemmas and Moral Judgment" in *Naming Evil, Judging Evil* ed by Ruth Grant (Chicago, 2006) 188.

as family, religious community, and school nor, certainly, to suggest that we ought not to extend the reach of discourse ethic to political agencies removed from the public eye. It is to acknowledge that participants in public debate may arrive with their minds already made up and that we should marvel at the ability of such debate to occasionally change their opinions and regard such changes as a great triumph for liberal democracy.[68]

I take Hacohen's account to be more "realistic" in many respects. He accepts a central insight that MacIntyre, Taylor and others have elaborated, namely that ethical formation takes place primarily at sites beyond public institutions, although such institutions may play a supportive role in enabling and protecting such sites.

As for the past failures of liberal regimes to constrain abuses of power, Hacohen argues, "Their failure has always been one of persuasion, not of conviction. Tocqueville had a number of palliatives. Religion was one. Secularists, like Mill, put their trust in education. But the dilemma remained alive, unanswered."[69] The dilemma – namely that the political power necessary for constituting a good society is also open to abuse, and there is no way of perfectly structuring institutions and incentives to prevent such abuse – has no solution other than in the ongoing ethical judgments that motivate elites to refrain from exploitation and motivate the masses to expose and condemn abuses. These judgments can be only be supported, but never secured, through institutional arrangements. They depend on sources that ultimately lie beyond the control of the state.

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[68] Ibid., 180.

[69] Ibid., 185.

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Thus Hacohen concludes, "The peculiar dilemmas of liberalism defy universal resolution," and he adds "We should be especially careful not to import the complexity of the present situation into procedural proposals for moral judgment…Instead, we should seek to modify the conditions enabling moral judgment through religious, educational, and other voluntary organizations."[70]

Hacohen thinks that, fortunately, liberal theorists are themselves increasingly persuaded by this perspective:

> If liberals remain skeptical about the prospect of liberal democracy in the developing world, they do not quite live on edge the same way, apprehensive about any sign of a weakening of liberal consensus in Europe of the United States. If many are still apprehensive about education for democracy and disregard the contribution – actual and potential – of communal religious life to the making of the liberal public, they seem less anxious about much of it and no longer suspect voluntary associations quite the same way for being closed societies.[71]

In any case, judgment in liberal-democratic regimes is indeed necessary but always involves a constant rebalancing act, adjudicating present dangers based on the most persuasive resources at hand.[72]

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[70] Ibid 189.

[71] Ibid., 185.

[72] And in this balancing act all liberal regimes walk a fine line between "Bigotry and Nihilism" as Thomas Spragens helpfully puts it.

The accounts of Benhabib and Hacohen support a judgment made explicit by

Michael Walzer and Bryan Garsten about the fundamental role that persuasion plays in

politics. As Walzer explains:

> There is no safe and sure conversational design that will protect us against
> bad agreements and bad disagreements. The continuing argument provides
> our only protection. Real talk is the conscious and critical part of the
> processes that generate our received ideas and reigning theories –
> reflection become articulate. Arguing with one another, we interpret,
> revise, elaborate, and also call into question the paradigms that shape our
> thinking. So we arrive at some conception of a just society (say) through a
> conversation that is constrained, indeed, by the ordinary constraints of
> everyday life: the pressure of time, the structure of authority, the discipline
> of parties and movements, the patterns of socialization and education, the
> established procedures of institutional life….In another sense, however,
> these same conversations are radically unconstrained, for while there may
> be ideas that are taken for granted by all the speakers, there are no
> stipulated ideas, none that has to be taken for granted if the conversation is
> to proceed (nor are the constraints taken for granted). There is no design.
> Real talk is unstable and restless, hence it is ultimately more radical than
> ideal speech. It reaches to reasons and arguments that none of its
> participants can anticipate, hence to reasons and arguments undreamt of
> (for better and for worse) by our philosophers.[73]

Persuasion and argument can be institutionally supported, but ultimately not

designed. Moreover, even our best institutions rely on persuasion for their ongoing

stability. There is no way of escaping the problem of persuasion and the necessity of

engaging in persuasion. We can only do so more or less adequately. Garsten makes this

point eloquently:

> There exists no sovereign authority to settle our disputes, neither a king
> nor an enlightened statesman nor a shared conception of public reason nor
> even a common public conscience deep within our hearts. Only once we

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[73] Michael Walzer, "A Critique of Philosophical Conversation" in *Thinking Politically* (Yale, 2007) 36.

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have acknowledged that fact will we find it necessary to engage in the work of trying to persuade one another…

What does that project require of us? Not that we become brothers or comrades, nor that we befriend those with whom we disagree, nor even that we join them in a contract. It requires instead that we pay attention to our fellow citizens and to their opinions. The politics of persuasion asks that we look to understand the commitments, beliefs, and passions of the other side if only for the purpose of trying to bring them to our side – or, more often, for the purpose of trying to rebut their views in front of people who have no settled position of their own. The effort of attention that persuasion requires is thus often motivated by our partial and political passions, but it nevertheless draws us out of ourselves. Trying to persuade others requires us to step outside our particular perspectives without asking us to leave our particular commitments behind.[74]

Thus the fundamental insight to be had about the nature of political order is that *ethical persuasion – persuasion about the nature and desirability of fundamental goods – is essential for preserving and extending such order*. There are ways we can structure and modify institutions to support sites of ethical formation and forums for public persuasion, but how this is done will reflect prudential judgments that again point to the primacy of persuasion. Simply put, we are stuck with persuasion, and realizing this is an important condition for responsible and effective political action. It is a mistake to try to avoid the need for persuasion by institutional artifices or scientific methods of management. Both may have their place, but neither can do away with the primary importance of persuasion.

The truth of the social importance of persuasion is not entirely good news. As Garsten admits in the conclusion of his book *Saving Persuasion*:

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[74] Bryan Garsten. *Saving Persuasion: a Defense of Rhetoric and Judgment*. (Harvard, 2006) 210.

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In thinking about how persuasion can engage judgment, I have painted a rather rosy picture of the politics of persuasion. But as was noted in the Introduction, efforts at persuasion do not always, and perhaps do not often, strike the golden mean that lies between manipulation and pandering. This is the fault not only of those who try to persuade, but also of those who listen and judge, or fail to judge. The truth is that there can be something exciting and gratifying about letting oneself, as a listener, be swept away in a momentary political passion or flattered into more stubbornly believing one's existing prejudices. Even if citizens were generally on guard against these tendencies, even if the exercised good judgment most of the time, the dispersal of judgment involved in the politics of persuasion would still generate heated a moral and political controversy. As even proponents of deliberation admit, public debate often tends to exacerbate and radicalize disagreements that divide us rather than mitigate them, creating enclaves of like-minded believers. The politics of persuasion is a risky enterprise. That is why the early moderns sought to avoid it in the first place.[75]

6.5 Conclusion

The politics of persuasion is indeed a risky business, but ultimately it cannot be avoided. Humans are by nature open to different conceptualizations of the good. And history demonstrates that humans can find a frightening array of goods compelling objects of allegiance. In order to construct a common life together on a basis other than pure violence and exploitation people have to be persuaded to share some common evaluative perspectives. Indeed, the order we witness in Western liberal democracies is at some level a moral order, constituted by common judgments about basic goods. A wide range of subsidiary disagreements of course remain, expressed by different traditions of hermeneutic reasoning. One of the ongoing tasks within Western liberal democracies is to

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[75] Ibid., 199.

negotiate these remaining differences in a civilized way, ideally, most would hope, in a way that reflects reasoned deliberation. Citizens of western liberal democracies need also to take note of the importance of continuing to identify persuasive resources to defend their overarching political order, despite the fact it does not always result in outcomes particular citizens prefer. The kind of reasoning involved in these tasks will be fundamentally hermeneutic in some respect. In the process of reasoning people will also of course incorporate "positive" claims about the efficacy of different arrangements or the factual details of certain situations. However, many disagreements are not exhausted by such questions. Persuasion will necessitate engaging in hermeneutic dialogue with others regarding the conceptualization of fundamental goods (which could take on various forms beyond that of a philosophical debate – e.g. artistic, literary, rhetorical). Whether persuasion is possible with regard to any particular difference always remains to be seen.

Against the backdrop of these considerations we can understand why efforts to aid peoples of third world and attempts to help them develop even minimally liberal-democratic political institutions present profoundly complex problems. Put simply, one must consider not only changes to the institutional regimes of the third world, but changes to their conceptual regimes as well. The two changes likely have to go hand in hand, but the role that persuasion must play is considerable, and in many circumstances it is not at all clear that those interested in seeing change have the requisite intellectual and cultural resources to successfully engage in persuasion. However, understanding the role that ethical convictions play in the constitution of any social order shows why ethical

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persuasion must often be a component of successful social change. Recognizing this should cause those involved in third world development projects to pay as much attention to the problem of ethical persuasion as they currently pay to social-scientific research. Indeed, social scientists who want to make their research of greatest practical value need to understand how to integrate their analytic findings with a recognition of the place and nature of ethical persuasion. In the next (final) chapter I briefly review some promising examples of this being done – and done with considerable success.

7. Ethical Persuasion and Social Change: Theory and Examples

7.1 Summary

I have argued that ethical convictions are crucial to understanding how social institutions work. Moreover, there is no way to perfectly manage or get around ethical convictions scientifically. Rather, we can only hope to persuasively engage with ethical convictions at a conceptual or hermeneutic level, although there are institutional arrangements and historical inheritances that facilitate such engagements. I have shown that a traditional account of the social sciences, which aspires to see them constitute an "absolute" science of human behavior, inherently leads away from considerations of ethics and hermeneutic persuasion. Social scientists devoted to the methodological ideals of absolute science are reluctant to deal with ethics and persuasion because these cannot be tractably understood and engaged from a strictly scientific perspective. However, the absolute ideal is a mistaken one for the social sciences to adopt; and I have illustrated in detail the systematic limits encountered by the dominant methodological approaches in the social sciences in trying to provide a scientific account of society. Moreover, I have suggested that if the social sciences are to be evaluated by the pragmatic value of the knowledge they provide (which I've argued they should be) then social scientists need to understand how a recognition of the importance and nature of ethical persuasion can complement their analytic insights into the structure of society.

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Social science methods are least well equipped to deal with large questions of social change, and social science research will remain handicapped with regard to its pragmatic aims if it remains concerned solely with what "scientific" methods reveal. In order to be most effective at accomplishing the ends for which social scientific research is pursued in the first place, social scientists and those who use their research need to incorporate some understanding of the role that ethical persuasion plays in constituting social orders and, once recognized, expand their efforts to include explicit attempts at ethical persuasion. There is no guarantee that such efforts will be successful, and indeed there are many ways in which they could fail. However, taking note of the importance of ethical persuasion and trying to engage in it opens up possibilities to which purely "positive" social science research is blind.

Central to the idea of ethical persuasion is the notion of changing people's evaluative judgments about what is right, good, desirable, and so on. It involves a claim that some ways of seeing and valuing the world are superior to others. This thought sits uneasily with various sorts of liberals and post-moderns who are committed to a notion of ethical neutrality and non-judgment. However, such a commitment is ironically a sort of ethical one, and in any case a closer examination of liberal values indicates that they do posit non-negotiable judgments about what is good, right, and tolerable, even if these judgments are ostensibly minimal.

When we come to the question of whether and how we can help peoples of the third world escape poverty, disease, famine, oppression, violence, and tyranny, the social sciences provide many useful insights. However, changing the ethical convictions held by

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those in the third world, particularly the elites, is likely to be essential to any long term melioration of these problems. In order to see the kind of social changes desired by many – Westerners and citizens of developing countries alike – institutional changes will have to go hand in hand with the persuasive change of a number of ethical convictions.

Note that I present this simply as a claim about effectiveness. If one wants to see human rights protected, markets developed, institutions of freedom and prosperity flourish, I simply want to claim that this will generally require that ethical transformations be part and parcel of institutional transformations. In particular this will necessitate changing common attitudes in third world regarding the abuse and subservience of women, the place of violence in resolving interpersonal disputes, and the prerogatives that attach to political power.

If someone believes that changing "native" convictions is too high a price to pay – that it is *prima facie* illegitimate to try to change the values of other cultures – so be it, but let them understand that they must thereby abandon hope of seeing many liberal outcomes develop in the third world. Upon realizing the how much persuasion is required to change the ways of life of a people, we may indeed decide this is more than we are comfortable with and choose to abandon our social change objectives. Or it may be that we find ourselves incapable of rousing the necessary resources of persuasion, in which case we find that our aspirations for and efforts on behalf of social change have to be correspondingly minimal. I simply mean to point out what will generally be required to see important forms of social change take place. I do not assert that such change needs to happen, nor do I want to claim that it is always or generally within our abilities to

accomplish the necessary persuasion and change. I mean only to affirm that ethical persuasion is likely to be a necessary component of accomplishing the social, political, and institutional changes that many would like to see take place.

In conclusion, I would like to consider briefly the interplay between institutional change and ethical persuasion in greater detail drawing on the thought of James Buchanan, Doug North, John Wallis, and Barry Weingast. Following this, I illustrate my basic claims with three contemporary examples of the way in which ethical persuasion can be an integral part of successful social change projects – the example of the Grameen Bank's "16 decisions," Gerry Mackie's work on ending female genital mutilation in sub-Saharan Africa, and Paul Romer's proposal for "charter cities."

7.2 New Thoughts from New Institutionalists and Public Choice

"Public Choice" and "New Institutionalism" are two schools of political-economic thought that in recent decades have revolutionized the way we think about the problems of political economy, particularly in development contexts. Public Choice draws attention to the way in which politicians respond to incentives, particularly economic incentives; and by documenting and modeling the incentives of political actors has shown how interest groups can gain favorable political advantages. Public Choice shows that there is a dangerous logic at the heart of democracy that enables the proliferation of state favors to special groups in ways that may undermine the long term stability of the political-economic order.

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New Institutional research has focused attention on the way that property rights and transaction costs are essential to understanding the performance of an economy. Both markets and politics always take shape within an institutional context, which can facilitate or hinder the ease with which people interact with one another and their knowledge about what they can expect from others. New Institutionalism shows that institutions determine social outcomes in ways not appreciated by traditional economic analysis, and identifies a number of institutional features that appear essential to the prosperity and stability of Western liberal democracies. James Buchanan, a pioneer of Public Choice, and Douglass North, a pioneer of New Institutionalism have both received Nobel Prizes in recognition of their original insights.

In the last decade those working in both schools have become keenly aware of the relevance of ethics to the social phenomena they study. Both have come to see ethical convictions as an irreducible component determining social outcomes, which means that the formal rules of political and economic institutions are insufficient for understanding the important aspects of social order.

In the last decade Buchanan has been obsessed, perhaps we could say haunted, by the problem of ethics as it relates to questions of constitutional politics. Buchanan realizes that in a wide variety of economic games good outcomes can only be reached if people refrain from exploiting the situation for all it is worth. Buchanan notes that to achieve success in dealing with particular problems presented by public goods "it becomes evident that the ethics that must be institutionalized here must be such as to

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generate behavior other than that dictated by opportunistic self interest."[1] This suggests that something is missing from the economist's traditional understanding of the kind of behavior and motives needed to recognize sustainable gains from economic interactions with others.

Buchanan is particularly interested in the implications of ethics for the central problem of constitutional governance that lies at the heart of Public Choice analysis. Such analysis alerts us to the logic by which special interest can monopolize state power through democratic means and transfer disproportionate tax wealth and other benefits to themselves at the expense of other sections of society. Democratic politics thus appears liable to turn into organized crime writ large. Moreover, attempts to limit such dynamics of exploitation through constitutional design always beg the question of whether constitutional rules can actually provide enduring constraints, since they are nothing more than words on paper. Having wrestled with this question for decades, Buchanan ultimately concludes: "Just as in market behavior…political behavior must be ethically constrained in supplementary extra- or supra- constitutional ways. Formal constitutional limits will be no more effective in constraining political actors than formal laws against fraud in the marketplace. The ethical culture that extends 'beyond law' must describe both the economics and the politics of a viable liberal society."[2]

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[1] James Buchanan. "Beyond Law: the instrumentalized ethics of liberal order" in *Why I, Too, Am Not a Conservative: The Normative Vision of Classical Liberalism*. (New York: Edward Elgar, 2005) 35.

[2] Ibid., 37.

That is to say the political-economic problem of liberal-democratic politics can only be solved theoretically as well as practically by the existence of a certain kind of ethical culture. Indeed, the more one examines the way in which social interactions of all types function, the more one comes to see that ethical convictions are essential for a wide range of desirable social outcomes. Thus, Buchanan suggest that ethical convictions contribute something unique to politics and economics which cannot be explained in other terms, but which continues to go unrecognized by most economists:

> What I am suggesting here is that, once the importance of ethical constraints on choices within markets is acknowledged, the possible variations in final productivity among political and economic structures otherwise similar are wider, and these variations are likely to be much harder to predict on the basis of empirical examination of the standard economists' measures, even those comprehensive ones that may include such variables as, say, the education levels of the labor force and the formal laws of property and contract. In sum, ethics matter also for the working of market economies.[3]

Buchanan's judgments about the importance of ethical convictions may seem mundane to many political theorists, but his recent work draws attention to the precise ways in which such convictions are essential for correcting shortcomings in economic and institutional theory. Buchanan's insights have been echoed by many working in the tradition of New Institutional Economics, who have become increasingly concerned with the way that "informal norms" affect the functioning of "formal institutions."

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[3] Ibid., 34.

The problem of political-economic development in the third world from the perspective contemporary New Institutionalists is not that we lack an account of what good institutions look like. We possess a very good understanding of how political competition, open and fair elections, property rights backed by an equitable legal system, a market economy and so on provide social benefits of prosperity, freedom, and stability in Western liberal-democracies. These institutions facilitate mutually advantageous exchange, they enable credible commitments and contracts to be made, they protect the security of persons, and pit factions against faction such that politicians need to compete for power by providing by providing public benefits. The problem, however, is that we have a very poor understanding of how such institutions can be transferred to new contexts and take root in ways that reproduce their benefits. Moreover, if ethical convictions and other "informal norms" are essential to the functioning of institutions, it follows that these institutions and their benefits cannot be effectively transferred without transferring these corresponding ethical convictions as well. Transferring both formal rules and informal norms is likely to require a great deal of persuasion along the lines I suggested in the previous chapter.

North, Wallis, and Weingast recently published what is perhaps the most insightful and stimulating study extending the framework of New Institutional Economics to address questions of political economic development in the third world. In *Violence and Social Orders* the authors argue, among other things, that violence presents a ubiquitous problem for all societies, and the way in which societies deal with violence is central to understanding the forms of social order witnessed across human history.

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Moreover, the problem of constraining violence is central for the transition to an "open access order," found in developed nations. The authors offer a rich historical examination of the emergence of crucial features of open access orders as well as typology of different strategies for dealing with violence that have characterized different political-economic forms of organization. Their main claim is that:

> The existing body of knowledge in social science can be transformed by a new conceptual framework that changes the way we think about traditional problems in economics, political science, sociology, anthropology, and history that results from an explicit consideration of the role violence plays in shaping social orders, institutions, and organizations and their development over time. Our recommendations for new research entail an in-depth understanding of violence, institutions, organizations, and beliefs in the natural state that we do not currently possess.[4]

The book has been widely praised by leading figures across the disciplines of economics, political science, history, and sociology.

Many of the claims made by North, Wallis, and Weingast are sympathetic to or directly support the conclusions I have drawn in the previous chapters. The authors note that "Social development, historically and in the contemporary world, is not simply a matter of changing institutions, adopting the appropriate governance structure, or constructing systems of property rights."[5] This is precisely because "beliefs" are a crucial co-determinant of institutional performance. And, the authors point out that the social sciences generally lack a way of dealing with the genesis and complexity of human

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[4] North, Wallis, and Weingast. *Violence and Social Orders.* (Cambridge, 2009) 271.

[5] Ibid., 260.

beliefs. The authors admit that they "do not have a general theory of belief formation and human cognition," but they "have tried to come to grips with two aspects of beliefs:"

> First, beliefs about causal relationships in the world intimately affect people's decisions. Second, the cultural environment – the political, economic, social context – fundamentally influences beliefs. Social structures that create fundamental inequalities among participants are reflected in the belief system and in forms of social relationships exchange – specifically personal versus impersonal exchange and the forms, types, and access to organizations that the society supports. These organizations range from the family to the church, to political, economic, and educational organizations. In large part, beliefs in impersonal identity derive from the structure of the organizations that a society supports and people live within.[6]

In particular, North, Wallis, and Weingast point out that in the present day beliefs about "freedom and inequality" in the developing world are essentially manifested as hopes, rather than as a reflection of lived experience.[7] In the first world it is easy to take norms of freedom and equality for granted because they are confirmed and expressed in social institutions. In third world contexts, where hierarchy defines social relations and networks of patronage, and political freedom is seldom encountered, ideals of freedom and equality may be particularly difficult to envision. It requires extra effort to persuade people that these ideals can be achieved as a political and economic reality, or indeed that they should be realized at all.

North, Wallis, and Weingast are also emphatic in pointing out that institutions work differently in different contexts. This is particularly the case with "democratic

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[6] Ibid., 262.

[7] Ibid., 263.

institutions," which when instituted in development contexts often lead to the rise of populist demagogues who help the poor expropriate wealth in ways that undermine the long term stability and growth of a country. Concepts such as freedom, equality, and fairness are essential for constraining what is democratically feasible in Western countries: "With all their myriad and sometimes offsetting costs and benefits, open access [i.e. developed] societies depend for their operation on impersonal identity and the associated beliefs in equality and fairness"[8]

The authors conclude that "social order [in developed societies] is maintained through the interaction of competition, institutions, and beliefs."[9] And the crucial question for them is how these supporting beliefs are formed. Ultimately, the authors judge that "we are still some distance from a deeper comprehension of the interaction of formal rules, informal norms, and enforcement characteristics that together determine the performance of the overall institutional framework."[10] However, they put their finger on the right set of issues. They suggest "a deep understanding of change must go beyond broad generalizations to a specific understanding of the cultural heritage of that particular society."[11] And in their discussions of violence they raise many pressing questions about the place of ethics within society and the way in which economic interests interact with non-material interests. Indeed, it is particularly with regard to problems of violence,

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[8] Ibid., 258.

[9] Ibid., 255.

[10] Ibid., 271.

[11] Ibid., 271.

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which the authors document so extensively, that ethical convictions and ethical persuasion have a large, and ideally constructive, role to play.

Both Public Choice theorists and New Institutional Economists increasingly call attention to the importance of ethical convictions for institutional performance in ways that support my main thesis. Moreover, rather than simply issuing platitudes that "ethics matter," their treatment of these issues is attuned to the specific ways in which ethical beliefs and material incentives interact. The challenge, for those who accept my claims about the importance of ethics and the need for ethical persuasion, is to articulate how efforts at ethical persuasion can be informed by, and pragmatically complement, insights provided by the social sciences. Ethical persuasion will seldom be a solution unto itself. It is idealistic to think that social change happens only at the level of ideas. There are material conditions that need to be considered alongside convictions regarding what is valuable and appropriate. Ethical persuasion and institutional change need to go hand in hand in order to realize the outcomes that many desire. This process will necessarily be complex, iterative, and recursive. However, this complexity should not dissuade those working for social change from doing the best they can to engage in ethical persuasion alongside attempts to address the more material conditions of a social order.

I conclude this project by highlighting three examples of social change projects in development contexts that have leveraged ethical persuasion, and arguably been more successful in attaining their goals by doing so. The basic concepts underlying these

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projects were developed from within the social sciences, but the effectiveness of these projects was magnified by the inclusion of explicit attempts to engage in ethical persuasion in ways that complement the logic and the desired outcomes of the projects. These three projects are all examples of the pragmatic importance of recognizing the ethical dimensions of social structures and of incorporating attempts to address them through ethical persuasion. They are examples that prove my point, although I refrain from calling them exemplary. There are faults one can find with each. However, they have accomplished what I judge to be genuinely good outcomes and it is unlikely they could have accomplished as much had they refused to engage with problems of ethics. Their efforts at ethical persuasion, although not perfect, have nonetheless been fruitful. Social scientist and program officers dealing with the third world would do well to take note of these successes and their explicit willingness to engage questions of ethics.

7.3 Grameen Bank and the "16 Decisions"

The Grameen Bank is a community development bank, founded in Bangladesh in the late 70's, which makes small loans to the extreme poor. Its founder, an economist named Mohammed Yunus, pioneered this concept of "micro-finance," extending small amounts of credit to the poor. The Bank's remarkably successful model has been widely praised by and copied in other countries. By the end of 2008 the Bank had lent some $7.6 billion to the poor, the vast majority women, and it currently has over 2000 "branches" in

Bangladesh.[12] It is credited with having helped some 50 million Bangladeshis rise out of subsistence poverty. [13]

The tremendous success of the Bank is often attributed to its economic model, based on the concept of "solidarity lending." Since the poor have no capital to put up as collateral for loans, the bank needed to identify an alternative way of ensuring that debtors have reasons to pay back their loans. The bank's solution was to leverage forms of social pressure and community bonds to hold people accountable. The Bank requires all those who receive loans to belong to a to a five member group in their local community (individuals choose with whom they want to form such groups). Although the members of the group are not liable for the repayment of each other's loans, no member of the group will be extended future credit if one of their members defaults. This provides an incentive for members to encourage each other to be responsible in how they deal with the loan and reliable in paying it back. Currently the bank reports a loan recovery rate of over 98%.[14] Also the bank itself is owned by its clients. Some 94% of the equity is owned directly by the borrowers, who thus have a long term interest in seeing the banking system flourish.[15]

The Bank also explicitly pursues community development goals. Loan recipients are required to set aside small amounts of their loan for an emergency fund and a group

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[12] Grameen Bank Historical Data. (http://www.grameen-info.org/index.php?option=com_content&task=view&id=177&Itemid=144) Retrieved June 22, 2009.

[13] Ian Fraser. "Microfinance comes of age". *Scottish Banker* magazine(2007-08-03).

[14] *Grameen Bank Monthly Update* in Taka, Issue Number: 334, Statement No: 1 Date: November 12, 2007

[15] "Grameen Bank At a Glance". *Grameen Communications*. Retrieved 2009-07-07.

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fund to help ensure against tough times. For various reasons the vast majority of loan recipients, some 97%, are women. Lending to women has by most estimates empowered them in new ways, creating both better conditions in their households and enabling the emergence of a class of small-time entrepreneurs.

One aspect of the Grameen Bank's model is often overlooked by economists seeking to copy its success elsewhere, namely the "16 Decisions" that the Bank requires all loan recipients to memorize and pledge to follow. Many of these decisions involve revisions of existing ethical norms and customary practices, and promoting these decisions is a form of both education and ethical persuasion. These are commitments that the Bank judges to be both substantially good for the borrowers themselves and conducive to responsible stewardship of loans. The decisions are:

1. We shall follow and advance the four principles of the Grameen Bank – discipline, unity, courage, and hard work – in all walks of our lives.

2. Prosperity we shall bring to our families.

3. We shall not live in a dilapidated house. We shall repair our houses and work toward constructing new houses at the earlier opportunity.

4. We shall grow vegetables all the year round. We shall eat plenty of them and sell the surplus.

5. During the plantation seasons, we shall plant as many seedlings as possible.

6. We shall plan to keep our families small. We shall minimize our expenditures. We shall look after our health.

7. We shall educate our children and ensure that they can earn to pay for their education.

8. We shall always keep our children and the environment clean.

9. We shall build and use pit latrines.

10. We shall drink water from tube wells. If they are not available, we shall boil water or use alum to purify it.

11. We shall not take any dowry at our son's weddings; neither shall we give any dowry at our daughters' wedding. We shall keep the center free from the curse of dowry. We shall not practice child marriage.

12. We shall not commit any injustice, and we will oppose anyone who tries to do so.

13. We shall collectively undertake larger investments for higher incomes.

14. We shall always be ready to help each other. If anyone is in difficulty, we shall always help him or her.

15. If we come to know of any breach of discipline in any center, we shall all go there and help restore discipline.

16. We shall introduce physical exercises in all our centers. We shall take part in all social activities collectively.[16]

While most of these "decisions" may strike a western reader as uncontroversial it is important to see that many of them involve a significant revision of existing habit, expectations, and commitments. Some of these decisions convey useful information and advice (plant seeds, use pit latrine, exercise), but the majority go deeper in asking borrowers to commit to certain values (discipline, hard work, education) and to refrain from existing customs (dowries, no attention to family planning). *In toto*, these decisions propose a way of life and particular values that are alien to many impoverished and rural communities, but ones that "outsiders" have reason to believe are superior and beneficial in various ways. The 16 Decisions constitute an explicit attempt to promote ethical change.

One of the most interesting things to note about the decisions is that they actually originated from poor Bangladeshis themselves. As Asif Dowla explains, "the Grameen

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[16] Retrieved from the Grameen Bank site:
(http://www.grameen.com/index.php?option=com_content&task=view&id=22&Itemid=109)

Bank holds a workshop for center chiefs of each branch every year. In these workshops the leaders share their achievements, review the problems they each face and examine ways of finding solutions to social and economic challenges."[17] The success of these workshops prompted Grameen management to hold a national workshop starting in 1980. At the first meeting, chiefs came up with the first four of the decisions as ones that promised to help address recurrent challenges they witnessed in their communities. Decisions continued to be added in subsequent years, and by 1984 these decisions totaled 16.[18]

A number of studies have suggested that the 16 Decisions have had an important impact in social outcomes above and beyond what might be expected simply from higher income levels, although it is ultimately hard to disambiguate these effects. In any case, research by the World Bank and others has shown that after receiving Grameen loans recipients have lower fertility rates, lower incidence of disease, and their children have better nutrition and access education at much higher rates.[19] It is reasonable to think that

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[17] Asif Dowla. "In Credit We Trust: Building Social Capital by Grameen Bank in Bangladesh" in the *Journal of Socio-Economics* (35: 2006) 112. I am indebted to Dowla's article for alerting me to many useful scholarly studies of the Bank's programs and effect.

[18] Ibid., 112.

[19] See Amin, R., Ahmed, J., Ahmed, M., "Poor women's participation in income-generating projects and their fertility regulation in rural Bangladesh: evidence from a recent survey." *World Development* 22 (4: 1994.), 555–564.; Khandker, S., Latif, M.A., 1995. "The role of family planning and targeted credit programs in demographic change in Bangladesh." Prepared for the Workshop "Credit Programs and the Poor", 19–22 Dhaka, Bangladesh, March 1995, Education and Social Policy Department, TheWorld Bank, and Bangladesh Institute of Development Studies (BIDS).; Schuler, S., Hashemi, S., 1994. "Credit programs, women's empowerment, and contraceptive use in rural Bangladesh" *Studies in Family Planning* 25 (2), 65–76.; Schular, S., Hashemi, S., Riley, A.,"The influence of women's changing roles and status in Bangladesh's fertility transition: evidence form a study of credit programs and contraceptive use." *World Development* 25 (4:1997), 563–575.; and Khandker, S. *Fighting Poverty with Microcredit*. (New York: Oxford University Press, 1998).

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the campaign by the 16 Decisions to promote these very goals has some impact in these improved outcomes.[20]

The 16 Decisions have been criticized by some as an illegitimate imposition of values meant to discipline borrows in service of the Bank's bottom line. Lamia Karim dismisses the 16 Decision as "NGO rhetoric" and reports that in some sections of rural Bangladesh recipients cannot recite any of the decisions.[21] Also, some anthropological research has suggested that very few recipients in fact change their behavior with regard to dowry customs.[22] True as these criticisms may be, they do not contradict the basic findings of the previously cited research, nor can they dismiss the remarkable success of the Bank at large.

The most focused study of the 16 Decisions to date is contained in Gayle Ferraro's documentary video *Sixteen Decisions*, which chronicles the impact of these rules on a young Bangladeshi woman named Selina, while exploring the larger challenge

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[20] With regard to why villagers didn't just adopt better norms and forms of collective organization on their own Woolcock (1998) argues "villagers struggle to engage in collective action of any sort because they do not have the organizational skills to do so, have a short radius of trust, and are so poor that they can afford to few little risks…An external agent is therefore needed to instill these skills and to provide a credible selection and enforcement mechanism (132)." (Quoted in Asif Dowla. "In Credit We Trust: Building Social Capital by Grameen Bank in Bangladesh" in the *Journal of Socio-Economics* (35: 2006) 113.) Woolcock's analysis may contain a good deal of truth, but it is premised on the thought better ways of doing things were always evident to villagers, who only lacked a coordinating mechanism. I would like to suggest that much of the importance of the 16 Decisions and solidarity model comes from the fact that they proposed thing the villagers had never thought about, or which were for various reasons previously inconceivable.

[21] Lamia Karim. "Demystifying Microcredit: The Grameen Bank, NGO's, and Neoliberalism in Bangladesh." *Cultural Dynamics*, 20(1); 5-29, (2008) 16.

[22] A. Rahman, *Women and Microcredit in Rural Bangladesh: Anthropological Study of the Rhetoric and Realities of Grameen Bank Lending*. (Boulder: Westview Press, 1999).

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the Decisions pose to detrimental social expectations in rural Bangladesh.[23] Based on many months of field research, the film endeavors to assess how the 16 Decisions act as a "social charter," which is essential for understanding the Grameen Bank's success in helping the poor. Although the documentary is nuanced in its treatment of the complexity of the cultural foundations of Bangladeshi poverty, Ferraro nonetheless presents the 16 Decisions as unambiguously empowering and transformative. In her judgment, "It was the guidance of the decisions and the bank that made the difference [in reducing poverty]."[24]

In light of both social science research and documentary/anecdotal evidence we have good reason to believe that the 16 Decisions – pioneered by Bangladeshis in response to the problematic habits and values witnessed amongst the poor and promulgated as a centerpiece of the loan program – have been instrumental in the Bank's success and in promoting desired outcomes such as health and education. Although simple, the attempt to change habits and values through the 16 Decisions, which recipients must memorize and pledge to follow, has, it seems, been a victory for the poor and a species of successful ethical persuasion.

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[23] Gayle Ferraro. *Sixteen Decisions*. 58 minutes, color video. (Aerial Productions: 2000).

[24] Quoted in "Banking on Poor Women" *Kennedy School of Government Bulletin*, Autumn 2000. (http://www.hks.harvard.edu/ksgpress/ksg_news/publications/alumbanking.html)

7.4 Gerry Mackie and Female Genital Mutilation

The next example comes from sub-Saharan Africa and the work that the political scientist Gerry Mackie has been involved with seeking to end the practice of female genital mutilation (alternatively known as female genital cutting or female circumcision). Mackie explains with great clarity the nature and history of the practice and as well as its effects:

> Female genital cutting (FGC) is a painful and dangerous practice, and irreversibly reduces a valued human capacity in the absence of meaningful consent. It affects at least 100 million women across some thirty countries in Africa and the Arabian Peninsula, and threatens three million young girls each year. The cutting is arranged by the girl's family, usually by the mother and close female relatives, and takes place between shortly after infancy to before the onset of puberty (rarely, on the eve of marriage or after birth of the first child), depending on the group. Age of cutting and type of cutting varies between groups, but varies little within groups. Depending on local custom it ranges in intensity from a mild pricking of the prepuce of the clitoris, to the removal of part or all of the clitoris and labia minora, to complete excision of the labia minora and the inner walls of the labia majora, followed by suturing of the vulva using thorns or stitches.
>
> Immediate and delayed health complications of the practice are more rigorously measured in recent years, and for some there may be psychological complications as well. Concern over the practice is prominent in international human rights discourse and the activities of international organizations. Dozens of programs have sought progress in its abandonment. Yet, the practice has been remarkably persistent.[25]

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[25] From Mackie's website devoted to the abandonment of female genital cutting
http://www.polisci.ucsd.edu/~gmackie/page5.html

Mackie had been concerned with the practice for years and conducted a comparative historical study that argued that there were strong parallels between FGC and the practice of footbinding in China:

> Each originated in ancient empires, in its origins was associated with chastity and fidelity, is linked to marriageability, is persistent across centuries, is general within the intramarrying group, and is practiced even by those who oppose it.[26]

The key to understanding the origins and persistence of both these practices, Mackie argued, was marriage incentives. These practices emerged as ways for imperial elites to control the promiscuity of their concubines and were eventually imitated by the upper and then lower classes. Once established as a social norm, footbiding and FGC both became necessary in order to for women to secure proper marriages. Over the course of centuries, legitimating mythologies, moral claims, religious apologies, and aesthetic fetishes developed to defend and explain the importance of these practices. Despite the incredible pain involved and the often crippling consequences, parents sought to inflict these procedures on their daughters because without them the prospects of a good marriage were dim.

Mackie noted, however, that despite the stubborn persistence of both these practices for long periods of time, footbinding ended rather rapidly:

> Female genital cutting was stubbornly persistent, continuing even among those who opposed the practice. Footbinding, however, which had been general in China for centuries, ended suddenly at the beginning of the 20th century. The people there organized Natural Foot Societies whose

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[26] Ibid.

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members pledged not to bind their daughters' feet, and if enough families joined a local society then they could safely marry their daughters to one another.[27]

In his historical inquiry into the ending of footbinding in China, Mackie found that the anti-footbinding reformers developed a threefold approach that proved effective in bringing the practice to a quick end:

> First, they carried out a modern education campaign, which explained that the rest of the world did not bind women's feet. The discovery of an alternative is necessary but not sufficient for change. Second, they explained the advantages of natural feet and the disadvantages of bound feet in Chinese cultural terms. New information about health consequences, again, is necessary but not sufficient for change. Third, they formed natural-foot societies, whose members publicly pledged not to bind their daughters' feet nor to let their sons marry women with bound feet. The problem is that if only one family renounces footbinding, their daughters are thereby rendered unmarriageable. The pledge association solves this problem-if enough families abandon footbinding, then their children can marry each other.
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> The first antifootbinding society was founded in 1874 by a local mission for its converts, who accidentally discovered the effectiveness of the public pledge. This local success went unnoticed until it was rediscovered and advocated on a national level in 1895 by the newly founded Natural Foot Society. The pledge societies and the cessation of footbinding spread like a prairie fire. By 1908, Chinese public opinion was decisively against footbinding, and footbinding of children was absent from urban populations by 1911.[28]

Mackie argued that FGC could possibly be ended in the same way, by collectively organizing communities to pledge to abandon the practice together and thus solve the

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[27] Ibid.

[28] Gerry Mackie. "Female Genital Cutting: The Beginning of the End," in Bettina Shell-Duncan and Ylva Hernlund, eds, *Female Circumcision: Multidisciplinary Perspectives* (Boulder, CO: Lynne Reinner Publishers, 2000), pp. 245-282, 256.

marriage coordination problem without anyone's daughter or son being the odd one out. In fact, Mackie thought Thomas Schelling's game theoretic model of conventions as coordination problems helped clarify the kind of public, coordinated shift that might enable a group to settle on a new norm. Mackie then discovered that there was a campaign aimed at doing something very much like this underway in Senegal. The campaign began as follows:

> In September 1996, women involved in the Tostan basic education program in Malicounda Bambara in Senegal decided to seek abolition of FGC in their village of about 3,000 people . The women went on to persuade the rest of the village-other women, their husbands, and the traditional and religious leaders-that abolition was needed to protect the health of their female children and to respect human rights. On July 31, 1997, Malicounda declared to the world its decision to abandon FGC and urged other villages to follow its example.[29]

Mackie contacted the group organizing this effort and spent four weeks doing field research in Senegal in 2004. He attended a public declaration of abandonment that brought together delegates from 96 villages and conducted many dozens of interviews with those involved.

According to Mackie, when locals are asked why they practice FCG, "Almost all say that FGC is required for a proper marriage, and many say that it is required for the virtue of the woman or for the honor of her family. Moreover, many have been unaware

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[29] Ibid., 256.

until recent years that other peoples do not practice FGC, and many have believed that the only people who do not do FGC are unfaithful women or indecent people." [30]

Mackie found upon closer scrutiny that there were a variety of reasons people seemed to practice FGC, many related in some way to marriage, but some not. He witnessed a combination of ignorance (people had no idea that others didn't practice it), medical misinformation (many believed it promoted fertility), unfortunate marriage incentives due to a sub-optimal coordination equilibrium (it was a necessary precondition for finding a desirable spouse). But he also found the practice was held in places by moral notions of virtue and purity, and by aesthetic ideals that saw it as attractive and beautiful.[31] It is easier to imagine how one might counter ignorance, misinformation, and coordination problems through education and collective action. However, it is less obvious how to try to change religious, moral, and aesthetic judgments.

As Mackie later noted in a report to the UN, "Religious obligation *is* an important factor in the decision to practice FGM/C, but is typically just one of several elements within what one WHO report (1999) calls a mental map that incorporates the stories, beliefs, values, and codes of conduct of society, and which are in fact "interconnected and mutually reinforcing and, taken together, form overwhelming unconscious and

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[30] Ibid., 254.

[31] These warrants could be complex and in many ways unintelligible to outsiders, such as Somali and Sudanese attitudes might seem: "The practice may additionally be associated with bodily cleanliness and beauty, such as in Somalia and Sudan, where infibulation is carried out with the purpose of making girls physically "clean" (UNICEF 2005: 12)." Cited in Gerry Mackie. "The Social Dynamics of Female Genital Mutilation/Cutting," with John LeJeune, March 2009 draft, for UNICEF Innocenti Research Centre.

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conscious motivations" for its continuation (Ahmadu 2000: 295; cited in Hernlund and Shell-Duncan 2007)."[32]

The way in which FGC came to be widely abandoned in Senegal provides an example of persuasion at work on all of these motivational factors.

Mackie reports that the initial decision of the Malicoundan village to abandon FGC was controversial (it was the first village to do so). Indeed, "some neighboring Bambara, Mandinka, and Sosse people, both men and women, were angry and sent hostile messages to Malicounda."[33] Eventually, the women who had led the movement to abandon the practice travelled to these villages to explain their reasons. What was it that motivated these women to seek to abandon the practice and to do so at this time?

The women had all been part of an educational program supported by various NGO's. As Mackie explains, these women:

> participated in the same basic education program designed and implemented by Tostan (which means "breakthrough" in the Wolof language), a nongovernmental organization (NGO) supported by the UN Children's Fund (UNICEF) and the government of Senegal, among others. The basic education program includes literacy training but goes well beyond that. The program is oriented toward women, but men are not excluded. There are six modules of learning, and each module contains twenty-four two-hour sessions carried out over two months. The six modules are distributed over eighteen months; there are also additional modules beyond the basic six (Tostan's new women's empowerment

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[32] Gerry Mackie. "The Social Dynamics of Female Genital Mutilation/Cutting," with John LeJeune, March 2009 draft, for UNICEF Innocenti Research Centre, 8.

[33] Gerry Mackie. "Female Genital Cutting: The Beginning of the End," in Bettina Shell-Duncan and Ylva Hernlund, eds, *Female Circumcision: Multidisciplinary Perspectives* (Boulder, CO: Lynne Reinner Publishers, 2000), pp. 245-282, 257.

program, six months in total, is proving effective in trials). The first module concentrates on problem-solving skills, the second module on health and hygiene, the third on preventing child mortality caused by diarrhea or lack of vaccination, the fourth on financial and material management for all types of village projects, the fifth on leadership and group dynamics, and the sixth on how to conduct a feasibility analysis to predict whether proposed group projects would result in net gains. Reading, mathematics, and writing are introduced in parallel, partly motivated by the substantive topics. The pedagogy uses local cultural traditions and learner generated materials, including proverbs, stories, songs, games, poetry, and plays. Technique and content are regularly tested and evaluated.[34]

…the thirty-nine Tostan participants [from Malicoundan] embarked on module 7, on women's health. Their facilitator was from the Wolof, an ethnic group that does not practice FGC. When this facilitator brought up FGC, the participants refused to take part and began speaking in Bambara. After several days of effort, the women started responding to the questions and comparing experiences. In the process they discovered a connection between FGC and negative consequences that had been attributed to other causes, realized that individuals believed that negative consequences were isolated because they had not been publicly disclosed, and thereby concluded that the negative consequences were not normal but avoidable. For example, a woman from a nearby village came who had once been a cutter but had stopped thirty years ago because her own daughter was almost killed by the procedure. The women were free to choose their own village projects, or none, and it was they who decided that stopping FGC would be their first project.[35]

After making the declaration to cease FGC in their own village and being harshly criticized by nearby villages, these women travelled to some of the nearby villages to explain their position. The women of Nguerigne Bambara, who had initially criticized the

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[34] Ibid., 259.

[35] Ibid., 260.

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declaration, were persuaded by the visiting women and on November 6, 1997, the village

of Nguerigne Bambara decided to formally renounce FGC as well.[36]

Mackie continues the story:

> On November 20, 1997, the president of Senegal decried FGC and called
> on the nation to emulate the women of Malicounda. At the same time, the
> people of much smaller Keur Simbara decided that they could not stop
> FGC without consulting with their extended family that lived in ten
> villages near Joal. Their decision to consult also supports the convention
> hypothesis: the Kent Simbarans were aware that a change would have to
> involve the population among whom they commonly intermarried. Two
> men, one a facilitator in the basic education program, the other a sixty-six-
> year old imam who had been a student of the basic education program,
> went from village to village over eight weeks to discuss FGC. The men
> were at first afraid of being chased out of the villages for talking about
> such a sensitive and controversial topic, but the fact of the Malicounda
> decision provided an opening for discussion. I infer that the demonstration
> effect was important: that the Malicoundans had succeeded at a collective
> abandonment and had avoided bad consequences. Three representatives
> (the village chief and two women) from each of the ten villages gathered
> in Diabougou on February 14 and 15, 1998, along with delegations from
> Malicounda Bambara, Nguerigne Bambara, and Keur Simbara. These fifty
> representatives of 8,000 people in thirteen villages issued the "Diabougou
> Declaration" [to jointly abandon FGC].[37]

The chain of events Mackie reports is striking, both in its speed and scope.

However, can we say more about the concrete dynamics of persuasion that led to such a

widespread change on such a sensitive and important matter? Mackie shows that "The

nexus of causal information, private experiences and attitudes made public, and the larger

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[36]Ibid., 257.

[37] Ibid,. 257.

context of the education program created a critical mass of women who then went on to persuade others in the village."[38] But how exactly did this happen?

The initial educational program was of utmost importance. This and other related programs were able to " 'build the capacity of women to participate actively in decisions affecting their lives' (Diop and Askew 2006: 127), engage entire communities, combine participatory human rights education with local development activities, and organize coordinated abandonment."[39] Without the organized educational initiatives it is hard to envision a similar spark being ignited or tipping point being reached.

However, Mackie argues that it was essential for the success of Tostan and other programs that they were undertaken and promoted by Senegalese themselves. Western aid organizations and NGOs provided many resources to help the Tostan program expand, and the program drew on outside medical expertise and provided knowledge of alternative ways of life. But it avoided a moralistic stance of condemnation or conspicuous propaganda. It was an exercise in what Mackie calls "non-directive education," although that's not to say that those who supported such programs did so without an agenda.

There were also other dynamics at play in the local community that lay far beyond anything that NGO's could influence. Mackie notes that an important development came when one of the village imams ruled that FGC did not constitute a religious obligation.

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[38] Ibid., 260.

[39] Gerry Mackie. "The Social Dynamics of Female Genital Mutilation/Cutting," with John LeJeune, March 2009 draft, for UNICEF Innocenti Research Centre, 1.

Moreover, the imam revealed that he did not have FGC performed on his own daughter. This helped assuage concern regarding the religious necessity of continuing this tradition.

The general approach of basic education, public discussion, and public declaration has been copied elsewhere, utilizing the twofold strategy of a coordinated program of community abandonment alongside deliberations concerning human rights.

For example, as Mackie notes:

> The NGO KMG in Ethiopia in 2000 held workshops for 50 women from different subdistricts on gender, democracy, and women's rights. It also coordinated with government and community organizations, and built its credibility with community projects. Its webpage in a 2002 entry states that, "We used to talk about helping girls one girl at a time. Now, our people have taught us that it is possible to seek not just change, but accelerating change." In early 2003, it implemented a vigorous community dialogue program which has led to many coordinated community abandonments (Dagne 2008, http://www.kmgselfhelp.org/hotissues.html). Both human rights deliberation and coordinated community abandonment are necessary for change. National programmes in Egypt and Sudan are promoting positive human rights messages and discussions at national, regional, and local levels, and are experimenting with a variety of coordinated abandonment through community dialogue efforts at the local level.[40]

The way in which the coordinated approach to abandonment works is easy to understand in light of work done in game theory on problems of collective action. However, the way in which human rights deliberation proves useful and persuasive is something that Mackie had a hard time explaining with traditional concepts from the

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[40] Ibid., 33.

social sciences. Mackie ended up conceiving of non-directive human rights deliberations in terms of a Gadamerian fusion of horizons:

> Transformative human rights deliberations should not be conceived of as the imperious transmission of informed and legitimated international norms to less informed and less legitimate local communities. Indeed, such an attitude would not respect the rights of the people making up those communities. Nor are such deliberations essentially a matter of opposing international moral norms to local social norms. Rather it is more a matter of what philosopher Hans Gadamer termed a fusion of horizons; in this instance, the joining of local values and practices, cultural and religious, with international rights discourse and experiences, each enlightening and improving the other. A general value, illustrated through a number of specific local examples, helps people identify which of their existing values are more fundamental, and which values derive from those fundamental values. Merry (2006) observes that the international human rights framework used by the international movement against violence to women is also enthusiastically appropriated by local, regional, and national movements to curb such violence. But, she notes, to be effective, human rights ideas need to be translated into local terms and be actively remade in the local vernacular.[41]

Ultimately, Mackie concludes, against those who think such deliberations are either imperialist or meaningless, that:

> A nondirective attitude, combined with the creation of fora, informal and formal, for discussion of arguments for and against any contemplated change, is more credible than missionary didacticism and harsh propaganda. International instruments that enshrine human rights are credible because they are widely endorsed, and deliberations on what human rights mean in terms of the local vernacular and how they relate to the most fundamental values of the local community, can be transformative.[42]

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[41] Ibid., UN 35. Also I should note that there were many criticisms of these efforts for being "imperious." An anthropologist at NYU had objected that the efforts Mackie documented wer an illegitimate form of cultural imperialism – the imposition of western values- and for some time held up UN funding earmarked to expand the program.

[42] Ibid., 22.

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www.manaraa.com

Mackie's understanding of the success of the Sengalese campaign was initially rooted in his game theoretic analysis of the marriage market and the need for a coordinated abandonment to achieve a different equilibrium norm. However, he also came to see how the success of this formal strategy crucially depended on deliberative discourses concerning human rights and related ethical/religious/aesthetic considerations that locals had about the status of FGC. The expansion of FGC abandonment campaigns has been informed by insights in both of these domains. The success of these programs is a testament to how social science knowledge and attempts at ethical persuasion can complement one another.

7.5 Paul Romer and Charter Cities

Until recently the economist Paul Romer was best known for pioneering an account of economic development called "endogenous growth theory."[43] Put simply, Romer criticized reigning theories of economic growth for focusing on "external effects" and easily available macro data while neglecting the incredibly important role that new knowledge and ideas – technological and otherwise – played in motoring economic development. His collaborative work for the World Bank in the 1990's increasingly emphasized the contribution that ideas made to economic growth above and beyond

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[43] See Paul Romer. "The Origins of Endogenous Growth" in *The Journal of Economic Perspectives*, Vol. 8, No. 1. (Winter, 1994), pp. 3-22.

technological innovation.[44] He pointed to the role that ideas play in constituting institutions such as legal regimes and interpersonal behavior, much in the vein of New Institutional Economics. In 2001 he surprised many colleagues by focusing the majority of his attention on a software company he founded that sought to provide educational resources for primary and secondary schools online in ways that would decrease costs, increase access to education, and improve learning. The venture proved extremely successful.

Today, however, Romer is best known for his proposal to develop "charter cities" as a way of helping societies in the third world transition out of poverty, violence, and oppression. Romer uses the analogy of Hong Kong to explain his vision of Western countries collaborating to build cities in the third world that would be administered for a limited time horizon by those Western countries. Being administered and protected by Western powers promises to provide the stability and credibility needed for foreign investment to take root in these places. Although the proposal and his example of Hong Kong may sound imperialistic, Romer stipulates a number of conditions that distance his proposal from imperial projects. First, he requires that a host country voluntary invite this collaboration. Second, he demands that the land on which a charter city will be built currently be unoccupied, which is eminently conceivable in many parts of Africa.

What are the benefits of building a city from scratch on unoccupied land and handing over rights of administration to a foreign power? In the first instance there are a

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[44] See Paul Romer. "Idea Gaps and Object Gaps in Economic Development", *Journal of Monetary Economics* 32 (3): 543-73, (1993).

number of material benefits to such an arrangement. Starting from scratch means that construction firms can efficiently build modern infrastructure from the ground up. Moreover, by avoiding the inevitable and deep conflicts that arise in trying to negotiate with pre-established populations and local interests, as well as the high costs of upgrading existing, outdated infrastructures, the process of building will be much cheaper and less troubled than in most contexts. Of course the location will have to be chosen carefully, and will likely have to be located on a seaboard that will allow port access.

The greatest benefits Romer foresees, however, pertain to more immaterial dimensions. Developing countries frequently attract low levels of capital investment because of political uncertainties about whether a regime will expropriate investments or whether investments will be destroyed by various forms of social unrests. By having the cities administered for a long time horizon by a Western power in accord with a clear charter, or administrative mandate, these cities will likely be able to overcome the credible commitment problems that plague investment in the third world. Western powers can help ensure security as well as fidelity to contracts.

Most importantly, though, Romer thinks that by building from scratch and having a clear charter, these cities will be able to promulgate a particular normative vision regarding the expectations and requirements of "citizenship." Since everyone who comes to the city is effectively an "immigrant," those who voluntarily locate there effectively "opt in" to a social order defined by a particular legal and ethical code. Such "rules," Romer argues are the key to a flourishing society. Setting them up from the get-go and making allegiance to them a condition of immigration provides a way of introducing

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these rules into the third world such that they only regulate those who voluntarily choose to migrate to and work in the city.

As Romer sees it, "Charter cities let people move to a place with rules that provide security, economic opportunity, and improved quality of life. Charter cities also give leaders more options for improving governance and investors more opportunities to finance socially beneficial infrastructure projects."[45]

The demographic and development trends of the third world make clear that cities will become increasingly crowded as populations grow and people continue to leave rural life. In the prelude to mapping out his vision Romer asks the question: "How can we maximize the number of people living and working under better rules?" "The default process," he notes, "involves change from within. A given group of people participate in a political process that can, in principle, generate change. Since a change in the rules applies to everyone, change from within always involves a mixture of consultation and threatened coercion. As a result, attempts at change from within regularly end in deadlock and persistence of the status quo."[46]

But Romer thinks he has a better answer to the question:

> Imagine an alternative process in which people can migrate from a society with bad rules to another society with better rules. In this case, the rules in both places stay the same but people move between them. The process of movement between can be more effective than the process of change from within. Just as important, the presence of movement between creates pressures that speed up change from within.

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[45] Paul Romer, concept statement on Charter Cities: (http://www.chartercities.org/concept)

[46] Ibid.

Today's world offers little chance for large-scale migration. The hundreds of millions of people who want to move to places with better rules aren't allowed in. Charter cities will become the places where they can go.

Cities are the right scale for implementing entirely new rules. A coherent set of rules can let millions of people work together and create enormous value on a small tract of land. Because cities are also relatively self-contained, the internal rules in one can differ from the rules in all of its trading partners.

Urbanization is the key to the predictable transformation from an economy where most people earn a precarious living in subsistence agriculture (doing great harm to the environment in the process) to one in which most people work in manufacturing and services. The transformation is inevitable; current estimates suggest that an additional 3 billion people will move to cities this century.

The quality of their lives will depend on whether these are well-run cities with good rules, or dysfunctional cities with bad rules. Many people continue to move into urban slums with no running water, high crime rates, few steady jobs, and sewage in the streets. The embedded, interlocking systems of bad rules that lead to this type of dysfunction will be exceedingly difficult for existing cities to change from within.

A new charter city offers a speedier path to better rules. People who live there, even people who start out earning very little, can live in housing that is safe and sanitary, send their children to school, find work, and live free from fear of crime.

All it takes is better rules. We already know what many of these rules are. We already know how to enforce them. Charter cities can create places where the hundreds of millions of people on the bottom rung of economic life could go live and work under these kinds of rules.[47]

Romer's proposal has been criticized by many, such as Elliott Sclar of Columbia University, for being essentially neo-colonial, notwithstanding it voluntary aspects.[48] The

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[47] Ibid.

[48] Ibid.

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development economist William Easterly has called Romer's idea "creative" but also suggested it is crazy.[49] Building a city is not like putting up a fairground, Easterly cautions. Moreover Easterly suggests that Romer's vision is characteristic of a long line of thought that mistakenly assumes, as one reporter puts it, "you can slough off debilitating customs and vested interests by constructing a technocratic petri dish uncontaminated by politics."[50]

A year after formally launching the charter cities initiative in 2008 Romer was in discussions with the government of Madagascar to develop a charter city on an uninhabited parcel of land on the country's southwestern coast. He met with the president, Marc Ravalomanana, in early 2009 but the plans fell apart along with Ravalomanana's government in a violent coup a month later.[51] At the time of this writing Romer is in discussions with two other developing countries exploring the possibility of establishing charter cities on their soil.[52]

Romer's vision raises a number of interesting questions with regard to ethics and persuasion, not to mention deeper questions about citizenship, sovereignty, and politics. Romer believes charter cities will be successful because of their ability to implement rules of good governance and to require particular normative commitments of the people

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[49] Sebastian Mallaby. "The Politically Incorrect Guide to Ending Poverty" in The Atlantic Magazine, July/August 2010. (www.theatlantic.com/magazine/archive/2010/07/the-politically-incorrect-guide-to-ending-poverty/8134/)
[50] Ibid.

[51] Ibid.

[52] Ibid.

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who move into them. Romer's idea of good governance obviously has very little to do with "democracy" as it has traditionally been understood. Romer imagines that many in the third world will be eager to come to charter cities because they offer economic opportunity, political stability, and security. However, immigrants to a charter city will not be full democratic citizens in the sense of having a say in government. But, Romer, points out this is a situation no different than that experienced by the 214 million or so migrant workers in the world today who have left their homes to work in places where they don't have a vote.

In order to be successful, Romer's project requires two colossal feats of persuasion. First he must persuade the many stakeholders of a charter city proposal to sign onto the project. This includes Western governments willing to serve as the administrative trustee and guarantor of security and good governance, as well as the government of the host country and its people, who may find the concept of ceding land for a generation extremely unpalatable. Second, Romer will have to figure out how to persuasively engage the masses who come to work in his cities – how to get them to adopt the informal norms and convictions essential to supporting desired outcomes that his good rules are intended to promote. We see that this is an exceedingly difficult problem in many western liberal democracies today, which have different experiences with regard to the assimilation an enculturation of immigrants. However, the comparative material advantages of a charter city and its "good rules" for the poor of the third world may be so dramatic as to elicit substantial allegiance to its public order.

With regard to the first persuasive feat, Romer has clearly enjoyed some success, having persuaded stakeholders from number of countries to come to the drawing table. However, the second feat is dauntingly complex, perilous, and perhaps impossible. And yet there are slivers of hope that perhaps it could be done.

Romer is eminently aware of the importance of cultural habits, behavioral norms, and ethical convictions for institutional performance. If his charter city plans go forward he will certainly try to address the issue and muster persuasive resources for cultural and ethical formation. Whether he can do so successfully, we'll have to wait and see. However, being aware of the challenge is a necessary first step. Romer's vision is still very much in an embryonic stage. It is not clear whether his hopes are foolishly utopian, hopelessly colonialist, or the most promising idea in development economics. If the last, it is an idea that, despite its considerable grounding in economic theory, can only be realized through a tremendous feat of ethical persuasion.

7.6 Conclusion

The preceding examples provide support for my central claim that ethical convictions are essential to any account of social structure and that ethical persuasion will generally be a necessary component of social change. Moreover I have shown that the social sciences are not able to do away with the need for ethical persuasion, which, as a fundamentally hermeneutic enterprise, escapes mastery through scientific methods. The social sciences do provide many useful insights into the nature of society, but social science methods are systematically limited in various ways.

I have shown how claims contrary to mine – claims that methods of the social sciences can provide an absolute perspective and that ethical convictions can be reduced, understood, and managed scientifically - emerge from a mistaken vision of science. The failure of this "absolute" vision supports an alternative, pragmatic account of the social sciences.

In order to realize the pragmatic aims for which social science research is pursued we will often have to integrate the insights provided by such research with explicit efforts to engage in ethical persuasion, particularly in situations where we hope to effect large scale social change. Although ethical persuasion cannot be scientifically mastered, it can be a rationally defensible enterprise under certain conditions.

Ultimately, although the social sciences may provide knowledge that can be used in service of ethical formation and persuasion, the more basic challenge is persuading others to adopt the evaluative judgments we believe are right, true, justified, and good. There is no way to get around the need for persuasion. If I have persuaded the reader of this, my project will have been worthwhile.

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Biography

William Edward English was born in Silver Spring, Maryland on May 18, 1981. He attended Duke University as an undergraduate, earning a BS in Economics, a BA in Mathematics, and minoring in Political Science. He graduated in 2003 with distinction and was awarded the Robert S. Rankin American Government Award for Leadership and Academic Achievement. From 2003-2004 he attended Worcester College, Oxford from which he received a Master of Studies in Christian Ethics in 2004. That year he returned to Duke to pursue doctoral studies in the Department of Political Science, from which he received a Masters of Arts degree in 2006.

William published a review of James Kalb's "The Tyranny of Liberalism" in ISI's journal *First Principles*, the short essay "What has Athens to do with Jerusalem? Political Philosophy and the Christian Church" in Religio, an essay entitled "The Compartmentalization of Moral Inspiration," in the *Proceedings of the 36th St. Gallen Symposium*, and most recently "Unlocking the Secrets of Human Biology: Implications for Diplomacy, Security, and War." In *H-Diplo/ISS Roundtable Reviews.*

William was awarded the Kenan Dissertation Fellowship in Ethics from the Kenan Institute for Ethics (2009-2010), a National Science Foundation Doctoral Dissertation Improvement Grant (2008), a Summer Fellowship with Duke's Center for Genomics, Ethics, Law, and Policy (2007), the Wings of Excellence Award of the St. Gallen Symposium (2006), an H.B. Earhart Fellowship (2005-2007), the Humane Studies Fellowship from the Institute for Humane Studies (2003-2010), and the Weaver Fellowship from the Intercollegiate Studies Institute (2003-2004).

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