The George Washington University
WASHINGTON DC

COLUMBIAN SCHOOL OF ARTS AND SCIENCES

June 26, 1997

I hereby certify that Thomas Clark Leonard has passed the Final Examination for the degree of Doctor of Philosophy on May 27, 1997 and that this is the final and approved form of the dissertation.

Christopher H. Sterling
Associate Dean for Graduate Affairs

Dissertation Research Committee:

Arjo Klamer, Research Professor of Economics and International Affairs, Director
Robert Stanley Goldfarb, Professor of Economics, Reader
Joseph John Cordes, Professor of Economics and International Affairs, Reader

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The Reason of Rules in the Intellectual Economy:
The Economics of Science and the Science of Economics

By

Thomas Clark Leonard

A.B. 1983, Harvard University
M.Phil. 1993, The George Washington University

A Dissertation submitted to

The Faculty of

Columbian School of Arts and Sciences of
The George Washington University in partial satisfaction
of the requirements for the degree of Doctor of Philosophy

September 30, 1997

Dissertation directed by

Arjo Klamer
Research Professor of Economics
and International Affairs

UMI Number: 9805088

UMI
300 North Zeeb Road
Ann Arbor, MI 48103

Acknowledgments

This dissertation has its genesis in many stimulating conversations with Arjo Klamer, my advisor and the person who, long ago, persuaded me that economics could still accommodate a scholar with interdisciplinary interests. Many of the ideas that I present here, certainly the most important of them, were born in those sessions, and ours has been a collaboration with genuine intellectual rewards. I thank Professor Klamer for his encouragement and patience.

Bob Goldfarb somehow managed to be, simultaneously, my sharpest critic and an indefatigable supporter, a graceful performance that I can only hope to emulate with my own students. Professor Goldfarb proved indispensable in big and small ways, and I thank him for his many contributions, which are manifest in these pages. Joe Cordes, the third member of my research committee, provided insightful commentary early and often, and was instrumental in a very fruitful reorganization of the material. I thank Professor Cordes for his good-natured criticism and his support.

I also owe a debt of gratitude to the other scholars who read all or part of the dissertation, particularly Richie Adelstein of Wesleyan University. Professor Adelstein closely read early and later versions, offering incisive criticism and encouragement in equal measure. The members of my Final Examination Committee — Professors Bryan Boulier, Steve Suranovic and Bill Griffith — all provided thoughtful comments, which I gratefully acknowledge.

I would also like to acknowledge the steadfast support of my family, especially Naomi, who patiently listened to my ideas, and to my sorrows, and who was unswerving in her encouragement. Thank you.

Abstract

This dissertation makes the case for an economics of science, that is, for using economic ideas to theorize about science, especially its agents (scientists), its output (scientific knowledge), and its internal rules (scientific institutions). Though under-researched relative to traditional (philosophy) and revisionist (sociology and rhetoric) theories of science, an economics of science offers a solution to a problem that plagues both traditions — how self-interested, fallible agents can produce the collectively beneficial outcome of reliable scientific knowledge — and thereby suggests a remedy for a central methodological dispute concerning the efficacy of science.

Science studies and economic methodology are introduced in the context of scientific goals and standards of appraisal. Economics as a science is considered in light of its methods and its unique status among the different sciences. The philosophy of science and the revisionist responses to it are surveyed, establishing the intellectual antecedents that underwrite the current dispute in economic methodology.

Revisionist ideas in the "new" economic methodology, particularly rhetorical economics, are critically reviewed and a case is made against its epistemological positions on scientific knowledge, and on the nature and function of scientific standards. Amendments are proposed, suggesting an economic view of science, with its more robust explanation for successful science and for the scientific institutions that, in part, enable that success.

A Smithian economics is developed, which entails agents who are rational but fallible, self-interested but prudent, and which emphasizes institutions — rules of thumb, norms, conventions, standards — as decision-making resources when optimality is undefined, unattainable or undesirable. Institutions are defined, discussed, and explained. Conceiving of science as a market-like process, we examine scientific institutions as evolved responses to market failures and other incentive problems that arise in knowledge production, and discuss their nature and function.

A case study of the recent minimum-wage controversy in economics is employed to identify and elaborate upon working institutions in the science of economics. The standards identified offer concrete instances of institutions as explained by the economics-of-science framework, and counterfactual evidence against claims that scientific standards are too ephemeral, too tacit or too variable to permit their articulation.

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Introduction

A few semesters ago, in an introductory course I was teaching, the discussion turned from economics to the production of economics. A student asked, in innocence I seem to recall, "what do economists do?" A difficult question to answer. After some discussion, an even harder (and more pointed) question arose. "Do economists maximize utility?"

What gave this query its bite was the implicit recognition that economists construe their own activities differently than they do those of their subjects, economic agents. We tend to think of economists, like other scientists, as disinterested, selfless seekers of knowledge. On the other hand, our theory says that economic agents are strategic, selfish and calculating; it's not the dismal science for nothing. The naive theory of science says that economists, like all scientists, are concerned only with the truth and the advancement of scientific knowledge. But economics says that everybody outside of the academy (or at least outside science) — producers of goods other than knowledge — are cold calculators of individual advantage.

The obvious problem is this: economists are people, too. If we believe that people are selfish maximizers, and that economists are people, then we cannot consistently claim that economists (more generally, scientists) are something other than selfish maximizers. (Colander 1989). This inconsistency of treatment is unsustainable unless one is prepared to argue that economists (scientists) are somehow fundamentally different from other people. Our naive philosophy of economic science is unavoidably at odds with our worldly science of economics.

As an illustration of this conflict, consider a place where the economist *qua* scientist and the economist *qua* economic agent meet: economic consulting. Consulting is the business of selling scientific knowledge. Consulting economists do many things, but they typically sell their knowledge as experts, i.e., as objective third parties in adversarial settings like courtrooms or regulatory proceedings.[1] The consultant offers her scientific opinion just as the psychiatrist opines on sanity or the pathologist on the cause of death. She is asked to testify on such questions as: is the proposed merger anticompetitive; was there a price-fixing conspiracy; will price-cap regulation lower firm expenditures; what kind of auction is most efficient; was the defendant negligent; and will a minimum wage increase have adverse employment effects for low-skilled workers?

A conflict of interest arises because, in the United States, experts are usually hired not by the adjudicating body, but by one of the disputing parties. When an expert is paid by a party with an obvious material interest in the outcome of her findings, there are clear incentives to tell the client what she wants to hear. Because economists understand incentives, we should not be surprised that consultants' expert testimony is virtually never at odds with their client's position. The economist whose testimony damages her client's position, will not long have that client.

Because the answers provided by economics can be bad for business, experts often look more like advocates than like objective third parties. It is of course possible that the "truth" and the client's preferred position coincide. But truly objective experts, those who regularly find against their clients, are as rare as hen's teeth. (See Huber 1991, cited in Mirowski 1994a).

I must emphasize that the fact that consultants rarely testify against their clients does not prove dishonesty, i.e, that successful consultants tailor their scientific views so as to obtain the desired outcome. There is some selection involved, as when consultants establish public positions on issues such that clients can select those experts most likely to support their side. And, when clients exercise ownership rights over data and results, this allows them to suppress any findings that prove inimical

[1] In this respect, economic consulting differs from management consulting, for example, where experts advise firms on how to make their practices more profitable.

to their interests. Nonetheless, selection and suppression cannot completely explain the regularity with which "objective" economic knowledge and clients' interests coincide.[2]

My present point is not that economists, *qua* scientists, have a moral obligation to reveal all relevant arguments and evidence, nor do I wish to claim that economists cannot advance their scholarship by working as consultants. (On these issues, see Weinstein 1992). Likewise, the obvious incentives to keep clients happy will not **necessarily** conflict with one's scientific position. My point is that conflicts of interest do arise in the business of selling expert knowledge, and this fact demonstrates that economists (and other scientists) are fully capable of the self-interested behavior that our naive model of scientific motivation denies. The consulting example merely illustrates that economists (and other scientists) can behave as something other than disinterested truth seekers.

The same sort of conflict — between what we take to be correct, and what we want to be correct — occurs when economists serve as policy experts. Any economist who has done a tour in Washington comes back with horror stories about how economics is used in a political setting. It is hardly news that politically popular policies are routinely at odds with what economists find sensible. (See Cordes, Klamer and Leonard 1993). There are, for example, virtually no academics who subscribe to the "supply-side" idea that marginal tax-rate decreases, at current levels, are self-financing. The 1996 Dole presidential campaign, at times, contended otherwise. Bob Dole's economic advisors were therefore in the uncomfortable position of defending (or at least distancing themselves from) a position they likely took to be wrong.

Again, the claim here is not that policy advisors are dishonest. Advisors can make their economic case privately, whether or not they ultimately prevail. And when they don't prevail, it's not clear that resignation is always the first-best response. Policy economists can serve a socially useful

[2] Nor does it explain how objective economic knowledge can defend both sides of an issue, which occurs when both parties hire economic experts.

function simply by countervailing the politicians' natural inclination for free-lunch arguments, a role that Charles Schultze refers to as partisan advocacy of efficiency. (Eizenstat 1992). Nonetheless, the fact that policy economists must **consider** trading off loyalty to the boss and loyalty to their objective beliefs demonstrates that economists (like all scientists) can and do have motives apart from the selfless pursuit of economic knowledge.

Economists, of all people, should be open to the idea that economic ideas such as self interested behavior can apply outside of ordinary markets. Our "imperialistic" intellectual forays have already pushed economic thinking far outside its traditional boundaries. Economics has made its way into politics, marriage, childbearing, law, religion, addiction, suicide, and even into animal behavior and other biological processes. Why should one fear economics brought to bear on economics (more generally, science) and on economic (scientific) knowledge?

One obvious reason to resist the reflexive application of economics is that the dismal science self-applied feels, well, dismal. I think George Stigler is correct when he says:

> Economists do not relish an explanation of their own behavior in ordinary economic terms. To tell an economist that he chooses that type of work and that viewpoint which will maximize his income is, he will hotly say, a studied insult. (1982: 60).

Clearly, *homo economicus* is not the most flattering of self portraits, and this partly explains the traditional disconnect between the way we view the behavior of economic agents and that of ourselves and other scientists. But it is not only vanity that works against economic explanations of economics. There is a second culprit as well, an uneconomic but widely held theory of science that is, in part, the topic of this dissertation.

The received-view theory of science and its critics

I conjecture that economists generally subscribe to a venerable philosophical theory about their own work, and that of scientists more generally. Rendered with far too much compression, this theory of science has two components: one, a view of the nature of scientific knowledge, and, two, a view about how scientific knowledge is produced. First, the theory says that science is special, a privileged realm that can be demarcated from other social arenas, such as politics or religion, and even from less "scientific" fields in academia, such as the humanities. It is special because it produces objective knowledge, that is, knowledge that is true in virtue of how the world actually is, and not merely someone's subjective preference or unsubstantiated ideology. This difference is, in fact, what demarcates truly scientific knowledge from mere opinion.

Second, science produces objective knowledge in large part because of its producers and their *modus operandi.* In particular, (1) scientists share the great cognitive goal of producing objective knowledge; and (2) scientists selflessly collaborate in order to achieve it. Most importantly, (3) scientists possess and follow known rules for developing and for appraising rival theories. These explicit methodological rules provide a kind of recipe for attaining the goal of objective knowledge. Finally, it is the methodologist's job to determine which rules best meet the goal of producing objective knowledge. In other words, science works because scientists are selfless seekers of objective knowledge; scientists have only cognitive goals; and scientists possess and observe a method (set of rules) that ensures the production of objective knowledge; and all of this redounds to the benefit of society.

Put in this radically elliptical fashion, we have what economic methodologists sometimes refer to as the "received view" in the philosophy of science (Suppe 1977), variants of which continue to thrive among working economists and elsewhere in science. So defined, it is clear why any economist might like to wear the mantle of "scientist." It certainly sounds preferable to "selfish constrained maximizer." Indeed, economic methodology has historically concerned itself with whether economics (or some school thereof) can indeed qualify as a proper science.

History, alas, has been unkind to the "received view." Much of the damage was done by the very field that spawned it, philosophy of science. Beginning in the mid-1950s, the "second generation" philosophers of science — Karl Popper, Stephen Toulmin, Norwood Hanson, Willard Quine, Paul Feyerabend, and, especially, Thomas Kuhn — assembled a rather compelling critique of the received view (Callebaut 1993), portions of which have become very influential in disciplines far outside the narrow confines of philosophy of science.

Because economics lags broader intellectual trends, economic methodologists came rather late to the second generation critique. By the time of its importation into economics (c. 1983)[3], second generation philosophy of science had been transformed by a long gestation in different corners of the academy, particularly in the Sociology, History of Science, and English departments. The transformation radicalized the already radical second-generation critique, to the point that the most influential second-generation innovator, Thomas Kuhn, could call some versions, "absurd" and "an example of deconstruction gone mad." (Kuhn 1992: 9). The theory of science that the "new methodologists" proposed for economics (and for science more generally) borrowed from the original second-generation critique, but it also embraced the more radical alterations of Richard Rorty, Stanley Fish, and the sociologists of knowledge who startled Thomas Kuhn himself.

The scholars most responsible for bringing to economics this critique of the received view of science were my teachers, Arjo Klamer and Deirdre McCloskey, and their fellow "new methodologists," Philip Mirowski and Roy Weintraub. In different, and in sometimes conflicting ways, their work has inspired a large and growing literature in economic methodology and in science studies more generally.[4] The motley labels that one must resort to in characterizing their work — rhetorical economics, post-positivist methodology, postmodernism, constructivism — reveals two things: one, the recondite nature of this kind of scholarship, and two, a certain lack of programmatic

[3] This date refers to McCloskey (1983) and is meant only as a convenient marker for the narrative.

[4] See, for example, Amariglio (1988), Lavoie (1990), Samuels (1990b), Rossetti (1990), Milberg (1991), Henderson et al. (1993), Coşgel (1994), and Burczak (1994).

unity. This is to be expected when considering pioneering work on such fundamental issues as the nature of knowledge. What unifies all of the new methodologists is their endorsement of a radicalized version of the second-generation critique of the received view.

The new methodological view of economics (and of science) disputes virtually every tenet of the received view. Again rendered with too much compression, the new methodologists argue that economics is not a science, because no science is a science in the received-view sense, including physics, the traditional exemplar. Science, they say, is not special nor privileged. Science may accomplish many things, but it cannot produce objective knowledge, and hence it cannot be cognitively demarcated from other kinds of inquiry, or indeed, from social practices of any kind, such as literature, or mythology, or religion. The beliefs of the modern physicist are indistinguishable from those of the Azande poison oracle. (Campbell 1988: 446).

The new methodological skepticism about the possibility of scientific knowledge follows from their view of how scientific knowledge is produced, a view influenced by work in the "science studies" literature, especially anthropologically motivated empirical studies of scientists in their native habitats. (See, for example, Latour and Woolgar 1986, Knorr-Cetina 1981). First, the new methodologists say that scientists are not the selfless, disinterested truth-seekers of the received view. Real scientists (economists) have goals beyond knowledge, such as professional esteem, wealth, even revenge, and they are fully capable of pursuing their own interests. Second, say the new methodologists, science is produced socially. In fact, the theoretical beliefs that scientists hold are determined as much by purely social factors, as by empirical evidence from nature. Third, the new methodologists argue that there is no universal set of procedural and appraisal rules (a Scientific Method) that enables the goal of objective knowledge. The rules prescribed for science (economics) by philosophers should not be seen as the royal road to scientific knowledge, but more as inefficient regulation of the intellectual marketplace (McCloskey), or as *post hoc* attempts to legitimate currently dominant theories. And, as a historical matter, actual scientific practice routinely flouts contemporaneous rules of science.

A useful summation of the new methodologists' perspective on science (economics) and on how it should be studied, is provided by Robert Nola (1988), who is characterizing their intellectual cousins in the sociology, history and philosophy of science:

> In general, many sociologists and some historians and philosophers of science have regarded science as a cultural effusion essentially no different from any belief systems and practices with which it has been traditionally contrasted, such as religion or myth. They have also alleged that science has no privileged status as a means of garnering information about the world, as many philosophers have claimed, and have insisted that it ought to be studied in the same way as one would study any cultural phenomenon. Just as anthropologists have recorded the beliefs and practices of alien tribes of people and have drawn relativist conclusions with respect to both beliefs and values, so sociologists, along with their fellow-traveling historians and philosophers, have entered the domains of tribes of scientists to study their beliefs and practices and have, similarly, drawn relativist conclusions with regard to their beliefs, norms and goals. (Cited in Hands 1993: 157).

Though far too simplified, this is the new methodological view of science, economics included. Following convention, and for convenience, I will sometimes refer to the new methodologists' view of science as the "sociological" or "rhetorical" view of science, a shorthand reference to their most important sources.[5]

When I first encountered this view of economics in Klamer 1983 and McCloskey 1983, it was presented in the language of classical rhetoric and of post-modern literary theory. I only later learned of its antecedents in second-generation philosophy of science, and of its intellectual connections to related movements in the sociology and history of science, and elsewhere. The new methodological view of science, which likely appears alien to the representative economist, is well established (orthodox even) in academic departments where post-modernism is already old hat. The academic

[5] I don't wish to suggest that the rhetorical and sociological perspectives on science are one in the same (they are not), nor do I want to overemphasize the programmatic unity among the new methodologists themselves, for they are often at odds. I do want to emphasize some commonalities in their theory of science (economics), especially their theory of knowledge, a point taken up below.

debate on science and scientific knowledge has even made its way into the popular press, most recently following physicist Alan Sokal's hilarious spoof of post-modern jargon, a hoax that Sokal was able to publish in a post-modern journal called *Social Text.*

Though trained as an economist, I found myself in broad agreement with the new methodologists. I agreed that scientists (economists) were people too; the special exemption from ordinary human impulses traditionally granted to scientists carried a burden of proof that was mostly unmet. I also concurred that science (economics) was as amenable to study as any other social phenomenon. It seemed appropriate to conceive of scientists (economists) as agents, and to expand the traditional philosophical emphasis on theories (what Ronald Giere 1981 calls theory-centrism) to consider the actual agents who produce and consume scientific knowledge, and their environment. It also seemed useful to consider scientific (economic) knowledge as an independent object of study. Scientific knowledge is, after all, "the main input to human progress and economic growth." (Diamond 1993: 245).

I also agreed that the actual practice of science looks rather different from what the philosophers say it **should** look like. In economics, it is clear that our Popperian preaching ("reject falsified theories") is hardly ever practiced. The rules of good scientific procedure, whatever their philosophical credentials, sometimes seem made to be broken. I also found the debunking skepticism with which the new methodologists approached science (and economics) to exemplify good scientific inquiry, and I applauded its naturalizing spirit. Finally, I agreed with McCloskey and Klamer that scientists (economists) are rhetorical creatures, engaged in persuasion of different audiences: colleagues, students, clients, administrators and policy makers.

And yet . . . notwithstanding these many agreements the new methodological premises, I could not follow them all the way to the conclusion that science is utterly non-objective, fundamentally equivalent to other social systems of belief, such as religion or myth. Like Donald Campbell, I found myself in the odd position of agreeing with most of the new methodological critique, while still believing that there is **something** special about science, and that science therefore has a **some** greater claim to objectivity than do other social systems of belief. (Campbell 1988: 490).

No review is required to see that the natural sciences, in particular, have enjoyed spectacular successes in explaining aspects of the physical world. Science, as David Hull (1988) points out, sometimes does exactly what it claims to do. One need not endorse received-view epistemology[6] to see that scientific knowledge has grown enormously over time.[7] Scientific knowledge is fallible, and always corrigible, but it is nonetheless more **reliable** (Ziman 1978) than are other species of knowledge, such as aesthetic or spiritual knowledge. Science regularly produces reliable knowledge, a fact that seems unintelligible on the assumption that there is nothing cognitively noteworthy in way that science proceeds. The production of reliable knowledge in science is either a miracle of happenstance, or there is something unique in the way the science proceeds.

The foregoing suggests a problem and a task. The problem is this: how to theoretically reconcile my view that scientists are like other people, with my view that science (sometimes) works to produce reliable knowledge? Taken together, these two claims are incompatible with both the received view in the philosophy of science, which denies that scientists are like other people, and with the new methodological view, which denies that science works. My task is to show that a realistic view of scientific motivation, which entails accepting many of the new methodologists' critical claims, can

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[6] "Epistemology" is a fifty-cent word for the theory of knowledge. "Epistemology, or theory of knowledge, is that branch of philosophy which is concerned with the nature and scope of knowledge, its presuppositions and basis, and the general reliability of claims to knowledge." (Hamlyn 1967: 9). The word derives from the Greek term *epistêmê*, discussed in Appendix 3A, which means "scientific knowledge," as distinct from mere opinion, or *doxa*. I will use the adjective forms "epistemological" and "epistemic" interchangeably.

[7] Economics is a harder case, but I think it's safe to say that economic knowledge has also grown over time, if at a vastly slower rate than the physical sciences, say.

indeed be reconciled with a belief that (some) science produces reliable knowledge, which entails denying several of the new methodologists' claims.

In particular, I want to investigate whether economics itself might offer the theoretical means for such a reconciliation, that is, whether an economic theory of science can provide an explanation of how science can produces the collectively beneficial outcome of reliable knowledge, given scientists who are people, too, i.e., who are fallible, self-interested making decisions under strong uncertainty. This dissertation is a preliminary and modest attempt to achieve such an explanation. The subject of this dissertation, then, is the social practice called science (and economic science in particular), but the puzzle that motivates my inquiry is quintessentially economic — how is it that fallible, self-interested, uncertain agents come to coordinate and to cooperate in a way that realizes collective benefits for all?

The organization of this dissertation is roughly as follows: the first half (the first four chapters) introduces, isolates, and ultimately rejects the new methodological arguments against the possibility of scientific success. The second half (the remaining three chapters) accepts the remaining new methodological arguments, and builds there upon, an economic view of science. The first and second halves are previewed in the next two subsections of this introduction.

A middle way between the received view and its new methodological critics

With the advent of the new methodology, economic methodology has become decidedly more interesting and also more fractious. McCloskey, Klamer, Mirowski and Weintraub have done economics the great service of invigorating methodological discourse, by introducing economics to larger debates about science and scientific knowledge. Their important contributions, however, have also lead to an intellectual fragmentation within economic methodology, with the new methodologists (who are themselves diverse) in one camp, and others (such as Mark Blaug), who retain fealty to (aspects of) the received view, in another camp.

In part, the current fractiousness in economic methodology reflects the novelty of the fields that the new methodologists have chosen to cultivate — classical rhetoric, post-modern literary theory, sociology, ethnography, and the like. It takes time for new literatures to be digested. However, it is my view that the key disputes which separate the new and the more traditional methodologists are not ultimately due to new jargons or different literatures. The key disputes derive from epistemological disagreements, some of which are quite ancient. The schism in economic methodology arises from divisions that are carried over from the larger debates on science within philosophy and science studies more generally. (Gerrard 1990).

In economics, the upshot has been the advent of a false dichotomy: one's choice is between, on the one hand, an antique version of the received view (generally a kind of watered-down falsification) or, on the other hand, a totalizing critique of the whole project of producing scientific knowledge. The received view says that selfless scientists (who have only cognitive goals) produce scientific knowledge **because** they following a set of known rules which infallibly guarantee objective knowledge. The new methodologists say that selfish scientists (who have non-cognitive goals), fail to produce scientific knowledge **because** there is no set of rules that infallibly guarantee objective knowledge, and hence all rules in science, to the extent we can even identify them, are to be seen as *post hoc* rationalizations of current practice. The current tenor of this debate notwithstanding, it is clear that there is vast middle ground between these two polar positions. Economics needs more options, and I will argue that they exist.

What does this middle ground look like? To answer completely, I would need to render the following chapters in a few paragraphs, which I cannot do. But let us briefly consider two important issues — the grounds for scientific belief, and the sources and applicability of methodological rules. The following will illustrate how polarized the debate has become. In their well-founded attempts to rework the received view's account of science and of scientific motivation, the new methodologists have adopted a theory of scientific knowledge, and a view of scientific practice, that is, in some aspects, as extreme as the received view is in the opposite direction.

Take the grounds for scientific belief. The received view argues that empirical evidence (Nature with a capital "n") unambiguously decides among theories. Scientists, who are selfless truth seekers, consider only inputs from nature, wholly unaffected by other considerations. The new methodologists argue the polar opposite. They endorse Harry Collins, who has said that "the natural world has a small or non-existent role in the construction of scientific knowledge." (Cited in Campbell 1988: 509). Only external factors (Society with a capital "s") determine beliefs, say the new methodologists.

A moderate alternative accepts that science is embedded in a larger social context, without concluding that empirical evidence is inconsequential when appraising theories — there is an enormous territory intermediate between "it's all nature," and "its all society." One can agree that confronting theory with evidence is a difficult and sometimes ambiguous process, particularly in economics, where there is limited recourse to controlled experiment, and where the data are conditioned by time and place. It is not always clear where "the finger of refutation" points. (Hollis 1994: 80). But it does not follow that all empirical work is necessarily useless, nor does it follow that scientists cannot have their minds changed by the evidence. (Hull 1988).

Consider next the debate on the applicability of methodological standards. The received view says that science is monolithic, that there is, in effect, a "rule book" of evaluative standards for appraising rival theories, which applies universally, that is, in all times and places. The new methodologists correctly see science as decidedly more plural in its standards of appraisal, which can change over time and also vary from place to place. But the new methodologists go beyond this well-founded critique, to a claim that standards are necessarily relative to a given scientific community, i.e., they can never apply in other communities. The new methodologists move from a sensible argument that methodological standards **may** differ, to the extreme claim that they **must** differ. In so doing, they move from pluralism, to relativism.

A more moderate position denies that the choice must be between one universal set of rules, and many sets of rules that are necessarily incommensurable. Some standards will prove useful in more than one community. One can agree that standards are influenced by local considerations, *contra* the received view, without concluding that more general standards are thereby impossible. To argue that there can be no general methodological rules is no less extreme than it is to argue for only one set of rules. And, in any case, an actual evaluation is required, both to determine the boundaries of a scientific community and to see if the standards in question exist in more than one community.

As a final illustration, consider the debate on the proper source of methods in science. The received view says that scientists should proceed by using the Scientific Method, the set of procedures which will lead to objective knowledge. The Scientific Method is deduced from philosophical first principles about knowledge — a task for epistemologists. The new methodologists counter by arguing that, historically, methods derived from received-view epistemology have failed to produce objective knowledge. Hence, rules of good scientific practice should come not from law-giving epistemologists, but should be derived from a study of what works in actual scientific practices. What counts as good economics, for example, is a matter for economists to decide, not philosophers of science (or other "outsiders").

This cogent critique recognizes that rules derived from bad theories of knowledge will tend to fail, and that rules of good practice can have many sources, including sources "internal" to a scientific community. But the new methodologists go from this sensible argument to the more extreme claim that **all** rules which are epistemically informed are objectionable (McCloskey 1985a). Rules that make reference to a theory of knowledge are part of Methodology (capital "m"), and are therefore objectionable; only rules that are (ostensibly) unconnected to epistemic matters — methodology (lowercase "m") — may be entertained.

A more moderate position says that rules ought to be judged by their performance, not by their pedigree. That previous prescriptions for good practice have failed does not rule all normative methodological work, any more than the (inevitable) failure of scientific theories rules out all science. Methodological rules that are epistemically informed are not **inherently** vacuous or ineffectual (though they may prove vacuous or ineffectual in practice). To the contrary, their efficacy, like that of any other methodological rule, will depend upon the scientific goals of the community, its size, its information environment, and other pragmatic considerations.

As the foregoing illustrations suggest, there is ample room for a more moderate theory of science (economics). One can, as the philosopher Philip Kitcher puts it, create a "framework for the study of science that combines the insights of [the received view] with the insights of its critics" (1993: 390, cited in Hands 1995b: 608). A moderate theory of science rejects as false many of the either/or dichotomies that plague contemporary methodological debate in economics. In so doing, it opens up the prospect for a perspective which reconciles the cognitive successes of science with a more sophisticated theory of scientific motivation. It preserves the idea that science (at least sometimes) actually produces reliable knowledge, while acknowledging the new methodological arguments that scientists are self-interested, have non-cognitive goals, and that science is an unavoidably social process.

A central part of my objective in this dissertation, then, is to critique and then to amend the new methodological view of science (economics). I believe that the heart of their important critique can be maintained, without resorting to what I and other sympathetic critics take to be a few extreme epistemological commitments. Once shorn of these commitments, the new methodological view is not only sounder, it also becomes decidedly economic in nature. Thus, one "middle way" between the philosophical position of the received view and the sociological (or rhetorical) position of the new methodologists is, in fact, an economics of science. Economics thus plays a dual role in this dissertation: it serves as the subject discipline to be investigated; and it also provides the framework within which that investigation will be made. Allow me to elaborate on the idea of an economics of science.

An economic theory of science

Science studies, or the theory of science, was once seen as the exclusive domain of philosophy, especially epistemology. To theorize about science was to do epistemology. Many still use the term "philosophy of science" as a generic description of any theorizing that is applied to science. As even a casual reader of the newspapers now knows, however, contemporary science studies is a much more plural enterprise. Historians, psychologists, sociologists, anthropologists, theorists of rhetoric and literature have all become students of science and of scientific knowledge. With plurality has also come diversity and controversy. The newer students of science (the new methodologists are no exception) are more skeptical of science's accomplishments, and decidedly less reverential towards both its practices and its practitioners.

Among the fields which have recently begun to study science and scientific knowledge, economics is conspicuously absent, or at the very least, under-represented. Despite the incursion of social science into science studies, only a few scholars have used economic ideas as a template for thinking about science, and an even smaller minority are themselves economists.

This lacuna is particularly puzzling in light of the many close connections between epistemology and economic theory. Any economics must theorize about what agents know, and how they act on this knowledge. And, as Phil Mirowski (1994a) has noted, the pantheon of great economists contains many who first cut their teeth on epistemology (or elsewhere in philosophy), and whose subsequent economic ideas were informed by that earlier work. Adam Smith, Karl Marx, Francis Edgeworth, Thorstein Veblen, John Maynard Keynes, Frederick Hayek, and Nicholas Georgescu-Roegen are prominent examples. But while this curious lacuna is an interesting puzzle in recent intellectual history, it is not one I will attempt to explain. My intention is to, in a small way, contribute to the small but growing economics of science literature.

It is useful to distinguish among two different literatures at the intersection of economics and science. Adapting a suggestion by Wade Hands (1994), we can differentiate between (1) the economics of science, and (2) the economics of scientific knowledge (ESK). The economics of science literature considers science in connection with technological innovation and economic growth. Its traditional interests are the labor market for scientists, how research expenditures affect innovation (usually measured by number of patents) and economic growth, and whether there are positive spillover effects from university research. (See, for example, Griliches 1979, Jaffe 1989, Stephan 1996).

The economics of science literature is wholly within mainstream economics proper. It represents the application of neoclassical economics to some issues which pertain to science. The economics of science literature is mostly unconcerned with science as social process, nor does it consider the nature of the product produced by that process — scientific knowledge. These other issues are traditionally the concerns of economic methodology.

The economics of scientific knowledge literature, in contrast, is more explicitly methodological in emphasis. It is more obviously concerned with science as a social process, and with the nature of its products, especially knowledge. Small as it is, the ESK literature can be seen as having two strands. The first strand is the scientist-as-economic-agent literature, which applies conventional rational choice models to scientists as actors. A very useful survey of these approaches is available in Arthur Diamond (1996). James Wible (1995), for example, adapts Isaac Ehrlich's model of criminal behavior to analyze scientists' choice between legitimate and fraudulent techniques. The optimal amount of fraudulent research is that which maximizes expected net benefits, where benefits are increased wealth and costs are the wealth penalty if caught.

Feigenbaum and Levy 1993 provide another interesting example in the scientist-as-agent genre. They model the incentives for economic researchers to replicate econometric results. They present (among other things) a time allocation model for researchers with differing returns to replication of other's results and to original research, and their empirical findings are consistent with the widespread lack of replication in economics. Though it is addressing methodological issues which are somewhat unfamiliar to economists, the scientist-as-agent literature has the outlook and formal apparatus of mainstream economics.[8]

The second strand in the economics of scientific knowledge literature also conceives of scientists as rational agents, if less frequently in the form of a constrained optimization model. The second strand uses economic ideas more than economic techniques. In addition, the second strand is more explicitly concerned with the nature of scientific knowledge itself — the vital good produced by science that economists have treated as a black box, or left to philosophers of science. There are several third-generation scholars in the philosophy of science who make use of economic ideas in their work. Michael Ghiselin (1987, 1989), David Hull (1988), Philip Kitcher (1993), and Nicholas Rescher (1989, 1990, 1993) are important examples. William Bartley (1990) and Gerard Radnitzky (1986) are Popperian scholars who also use an explicitly economic framework. Michael Polanyi's (1962) view of science as self-correcting in the sense that competitive markets are, is an important early account.

There are also a few economists who have used economic ideas to theorize about economic knowledge, and scientific knowledge more generally. Examples are Ronald Coase (1994), David Colander (1989) and D. McCloskey (1990), which all follow a pioneering public-choice view of science by Gordon Tullock (1966).[9] The unifying idea in this second strand of the ESK literature is

[8] For other papers in this vein, see Levy 1988, and the work of Arthur Diamond, who has estimated the returns to publication, citation and to research project selection. (See, for example, Diamond 1986).

[9] Antedating all of this work by a century is an obscure paper by the American polymath Charles Sanders Peirce, "The Economy of Research," first published in 1879. Peirce constructs a formal

that economics can be fruitful in tackling problems of scientific knowledge, which are traditionally consigned to epistemology. In particular, economics offers a way of understanding our own behavior (and that of other scientists), which generally looks quite different from that postulated by the received view. And, economics also offers invisible hand explanations — a means of understanding collectively beneficial outcomes in science, even with self interested scientists.

My own approach borrows from both strands in the ESK literature. I conceive of scientists as rational, that is, as agents who act purposefully in pursuit of both cognitive and non-cognitive goals. Scientists are also self-interested producers and consumers of scientific knowledge. But I am loathe to develop an economics of science solely with the optimality *cum* equilibrium apparatus of mainstream economics. This is because the mainstream approach cannot easily accommodate what I take to be two crucial aspects of science: (1) the inherent uncertainty of scientific research, and (2) the importance of the institutional structure of science (its rules, norms, conventions and standards) for producing good collective outcomes.

It is, ironically, uneconomic to treat scientists as constrained utility maximizers in all instances. This is because scientists, like all agents, have scarce cognitive resources; they are boundedly rational. In addition, scientists also face strong uncertainty — the outcomes of their varied professional and theoretical choices are hard to forecast, and they face a host other informational shortcomings that tend to make optimal choices undefined, unattainable or undesirable. The production of scientific knowledge is especially hard to forecast. Since the object of science is to produce novelty — ideas not yet conceived — scientific outcomes cannot be the direct product of optimal choice in the conventional sense. (Loasby 1989: 197). Science is a open-ended enterprise by its very nature, and it is no accident that its future path has regularly defied the predictions of even

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model of research project selection that maximizes the (expected) utility of a research project, subject to its expected costs. Jim Wible reprints Peirce's remarkable paper in Wible 1994.

those insiders best equipped to speculate.

It is my view, therefore, that while it is reasonable to conceive of the scientist as a rational, self-interested agent, it is less defensible to think of her as a maximizer. Of the many social settings in which economic theory has been applied, science is, I maintain, a place where *homo sapiens* is particularly far from *homo economicus*. What, then, is a boundedly rational scientist to do? One answer, I will argue, is to make use of institutions, the second aspect of science that an economics of science should address.

Institutions are decision-making substitutes for the unbounded rationality we wish we had but do not. Boundedly rational agents confronting Knightian uncertainty will find optimal choices undefined or unattainable or even undesirable. In these instances, scientists have incentives to look for useful substitutes. One important substitute, especially in a complex and uncertain environment, is institutions.

Broadly speaking, methodological institutions are rules, norms, conventions and standards, that help the scientist as decision maker. There are different kinds of institutions, and they serve different functions, but institutions work in science much as they do in markets. As such, some scientific institutions are best seen, not as philosophical edicts from the mount, but as sensible rules for coordination and cooperation in the production of scientific knowledge. An economic view of institutions sees them not as free-floating cultural posits, but as evolved responses to the very real problems of individual and collective choice in a world of scarce resources and strong uncertainty, where agents may not be inclined to follow the rules.

Methodological institutions serve many different functions. Some methodological institutions help the individual scientist, such as decision making heuristics. Other institutions in science, such as behavioral norms against fraud, error and plagiarism, are designed to meet cooperative goals, which may not be in the individual scientist's interest. The norm against plagiarism, for example, is an attempt to protect an intellectual property right. The reward conventions in science, awarding credit upon publication, for example, help to overcome the problem that scientific knowledge has public-good aspects (it is non-rivalrous in consumption). The rule that research results should be reproduced — the practice of replication — is a means of certifying product quality, or reliability. Whether all these institutions work well, or whether they are the most efficient means to a given scientific goal, are, of course, different questions.

As the foregoing suggests, the "economics" in my economics of science is one that emphasizes the importance of institutions as decision-making resources. There are several different literatures in economics, new and old, that emphasize institutions in economics, though often using a different terminology. The behavioral or psychological literature (Simon 1957, Kahneman and Tversky 1979, Thaler 1992), for example, begins with bounded rationality, and emphasizes the efficacy of decision-making heuristics or rules of thumb in overcoming finite cognitive resources and time. There is also an interesting literature that considers conventions as important devices for coordinating in strategic settings where uncertainty, especially in the form of multiple equilibria, obtains (Schelling 1960, Ulmann-Margalit 1977, Schotter 1981, Sugden 1986, 1989, Elster 1989, Young 1996).

The New Institutional literature (Coase 1960, 1988, Demsetz 1967, North 1990, Williamson 1975), a third example, has long emphasized the importance of formal rules, such as those in common and statutory law, recognizing their importance for economic processes.[10] There is also what we might call the collective action literature, which studies the incidence of cooperation where theory predicts

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[10] The New Institutionalists are somewhat eclectic, but they all begin with some measure of bounded rationality and treat institutions as the products of market processes, which induces them to explore the intersections of economics with law and with history. I cannot claim that I have selected the most inclusive name — other labels used, of varying relevance, are Property Rights economics, and Transactions Cost economics. (On programmatic differences and similarities, see the symposium in the March 1993 *Journal of Institutional and Theoretical Economics* 149(1), with contributions by North, Coase, Williamson, and Richard Posner). I merely identify notable economists who take seriously the ideas of fallible agents and the importance of rules to human action.

non-cooperation. Norms are integral to cooperative behavior in these settings: contributing to public goods rather then free riding, avoiding "the tragedy of the commons" with common property resources, and avoiding Pareto-inferior results in finite prisoner's-dilemma games (Ostrom 1990, Smith 1994, Axelrod 1984).

What unites these different literatures in economics is a recognition that (what I am calling) institutions — rules of thumb, conventions, laws, norms, standards — are vitally important decision making resources in a world where optimality is undefined, unattainable or undesirable. Economics lacks a general name for the different literatures that have institutions as a common element, so I adopt the name of the economist who has inspired, at least in part, all of these literatures — Adam Smith. The innovation I offer is to apply Smithian economics in a less familiar realm, science. In so doing, I attempt to bring together two rather different currents in the stream of economics — foundational work in economic methodology, and research on institutions as decision making resources.[11] Let us now turn to the idea of a Smithian economics applied to science (economics).

A Smithian economics of science

I must emphasize that this dissertation does not attempt a full-blown economic theory of science (or of economics). Such a project well beyond what can be accomplished in the space of a dissertation. There are entire disciplines dedicated to analogous efforts (philosophy of science, history of science, sociology of science), and I do not presume to **create** an economics of science. My purposes are more modest. I want to suggest, following Brian Loasby (1989, 1991), that there are sufficient resemblances between ordinary economic activities, and the activities of science, "to give scope for the application of a unified set of connecting principles to both." (1991: 9-10). The connecting principles I have in mind are economic, but, as the foregoing suggests, they are not strictly

[11] Very little work has been done in this area. For an exception, see the papers collected in Mäki, et al., 1993.

mainstream — they are, to coin a phrase, Smithian.

I invoke Adam Smith not merely as an appeal to authority, but because his economics is, I believe, especially well suited to the unique demands of theorizing about science and scientific knowledge. Smith's views on (1) self interest, (2) the fallibility of human knowledge, (3) the importance of institutions for coordination and other tasks, (4) the unintended consequences of human action (the Invisible Hand), and (5) the social benefits of competition, are all relevant to science as a social process. Taken together, they distinguish a Smithian economics from other theories of science, even those that share his emphasis on self-interested action. A Smithian economics of science is therefore different from mainstream economic approaches to science, and it also departs from the new methodological account, which is more sociological in spirit.

A Smithian economics differs from mainstream economics in two important ways: its insistence that human knowledge is fallible, and its emphasis on the resulting importance of institutions to markets and market-like settings. Like many Scottish Enlightenment thinkers, Smith was decidedly skeptical about human reason. He saw human knowledge and decision making as fallible. The idea that agents can optimize in a world of perfectly known alternatives would have puzzled Smith, particularly if applied to science, a subject he had studied in some measure.

Smithian agents are purposeful in pursuit of their goals, to be sure, but they are unlikely to have information sufficient for optimization. "Smith," Ronald Coase reminds us, "would not have thought it sensible to treat man as a rational utility-maximiser." (Coase 1994: 116). Had Smith known of maximization as a theory of human action, he probably would have been skeptical, on economic grounds — how can scarce cognitive resources be used costlessly? A Smithian view of decision making, which has Herbert Simon as its modern exemplar, insists that there is no cognitive free lunch.

As we have seen, a Smithian economics also differs from mainstream economics in its emphasis on institutions. Smith saw that the right institutional structure was essential to his most important argument — that self-interested behavior can (unintendedly) lead to beneficial social outcomes. Invisible-Hand outcomes depend upon agents having the right moral sentiments, and also upon more formal rules to protect property, to enforce contracts, and to promote competition, rules which Smith collectively referred to as "justice." For Smith, institutions like property rights are not just sand in the gears of an market mechanism that otherwise needs only prices to function. On the contrary, for Smith, institutions are the precursors to and the (partial) guarantors of the beneficial outcomes that the Invisible Hand promises.

The institutions that enable social cooperation and coordination are especially important in this regard. The problem of social cooperation and coordination, especially in an uncertain world where individual interests need not coincide with collective interests, is central to Smith's economics. The wealth of nations, he said, depends upon productivity, which, in turn, is principally caused by the division of labor (or specialization). But the benefits of specialization can be reaped only if subsequent exchange is possible, which requires extensive coordination among producers and consumers.

Self-sufficiency is radically less efficient than specialization and exchange, but the gains from specialization and trade can be realized only in a world where trade actually occurs (and is not too costly to transact). In a highly specialized economy, agents depend upon others for virtually everything, including the most basic needs. A failure to coordinate can lead to discomfort, and even to death, in the case of famines, which are paradigms of fatal coordination failures. The coordination of economic activities is therefore necessary for securing the wealth of nations that the division of labor enables.[12]

The problem of social cooperation and coordination applies in science, no less than in the more conventional setting of ordinary markets. Scientists must coordinate if the gains from intellectual trade

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[12] The foregoing two paragraphs are indebted to Loasby 1991: 10-11.

are to be realized; and cooperation is required to avoid certain types of market failure that inhere in the production of knowledge. The importance of cooperation and coordination is not altered by the fact that science is not trying to produce wealth *per se*. In fact, science presents an even greater challenge than do ordinary markets — the uncertainty is greater, the good in question (scientific knowledge) has public good aspects, rival producers are both competitors and collaborators, and all participants lack an explicit price mechanism, that most powerful tool for coordination. Perhaps even more so than with ordinary markets, the important question in science is: which institutions provide the incentives for scientists to cooperate and to coordinate in producing the collectively beneficial goal of reliable scientific knowledge.

The idea that greater specialization promotes higher output was not new with Adam Smith. Smith's innovation was to show that coordination was compatible with (more or less) self-interested individuals and with (more or less) free markets. Arguing an idea that is still counterintuitive to most, Smith said that coordination could best be accomplished by invoking individual self interest, rather than "benevolence" towards others or state central planning. The famous metaphor of the Invisible Hand refers to the process by which the motive force of self interest can unintendedly work to meet the collective goal of the wealth of nations. The possibility of Invisible Hand outcomes, that unintended consequences can be beneficial for society, is what distinguishes a Smithian economics, indeed most economics, from other theories of human action.

Sociology, for example, sees individual self interest as incompatible with collectively good outcomes. Hence, the sociologist of science argues that if scientists are self interested, then good collective outcomes, such as the production of reliable scientific knowledge, cannot be realized. An economist responds as follows: recognizing that scientists are people, too, does not mean that reliable scientific knowledge cannot be produced, because there may be Invisible Hand processes in science, analogous to those in competitive markets. Invisible Hand reasoning is a means of reconciling self-interested scientists with collectively reliable knowledge. "The problem," says Ian Mitroff (1974), "is not how objective knowledge results despite bias and commitment, but because of them." (Cited in Hull 1988: 32). Invisible hand reasoning permits a hard-headed view of human motivation that need not rule out the production of reliable scientific knowledge in science, and it is what differentiates an economic view of science from a sociological or rhetorical view.

There are, of course, no guarantees that a given institutional structure will be adequate for invisible hand outcomes. Without the right moral sentiments, effective competition, the protection of property and contract, and means for handling externalities, public goods, and informational asymmetries, markets tend to fail, and the same is true for science. Science, no less than ordinary markets, needs institutions that actually work.

The point remains, however, that under the right institutional circumstances, science can work even when we assume that its practitioners are mere mortals. Says the philosopher David Hull: "The objectivity that matters so much in science is not primarily a characteristic of individual scientists, but of scientific communities." (1988: 3). We should not be surprised that science is sometimes as imperfect as markets can sometimes be. In fact, this symmetry is only one of the many suggestive parallels between scientific and market processes. There are, says Bill Gerrard:

> [A] number of parallels between the scientist and the economic decision maker . . . . Both the scientist and the decision maker must make long term choices under conditions of uncertainty. Both make use of conventions determined, in part, by the institutional structures within which they operate. (Gerrard 1990: 216).

My objective is to exploit these and other parallels in a way that informs both our theory of science and also our economics.

My premise is that "understanding the scientific process has much to contribute to an understanding of human decision making in general and *vice versa.*" (ibid). When we theorize about science, we are theorizing about human agents, who produce, value and consume knowledge, hence a theory of science can learn from economics. When we theorize about economics, we must make theoretical commitments regarding what agents know, how they come to have this knowledge, and how this knowledge grows over time, hence economics can learn from theories of science. The theory of science can inform economics, just as economics can inform our theory of science.

Note to readers

This dissertation is unavoidably a contribution in economic methodology, because, for better or for worse, this speciality has traditionally been the locus of research on scientific knowledge in economics. "Methodology" is something of a dirty word in economics, and this invariably handicaps research that bears its imprimatur. Disciplinary realities notwithstanding, I hope that this research might, one day, find an audience among my fellow economists. Perhaps the emphasis on economics as a source of meta-theoretical ideas will entice a few practicing economists, especially those whose research interests include knowledge problems in rational choice theory, or institutions as decision-making resources. I would consider it a bonus, should scholars from the different science studies communities get past the inadequacies of my presentation, especially as it encroaches on their areas of expertise, to find something of value.

Taking a cue from McCloskey and Klamer, I have tried to remember my audience. But, given the history of theorizing about science, this dissertation unavoidably contains a large helping of philosophy, which makes it more costly for economists to read. I have endeavored to keep the philosophical material as non-technical and accessible as possible. Several especially methodological discussions are confined to appendices. I take seriously the scientific norm of clarity in writing. Still, the extra cost is unavoidable, and I ask my economist readers for their indulgence.

In trying to avoid the obscurity and scholasticism that sometimes plagues methodological writing in economics, I have no doubt created other intellectual costs. Interdisciplinary work, if I may use that term, is always high stakes, because, uniquely, it can alienate more than one audience. One danger is that too much philosophy will alienate the economist. There is the additional risk that my philosophical glosses are too egregious for the philosophically informed reader. In this regard, the best I can hope for is that the obvious costs of simplifying complex and subtle ideas from other fields, are exceeded by the gains from importing them in less than perfect condition.

Finally, if interdisciplinary work is risky, it is also, I think, important. In intellectual markets, no less than in ordinary markets, specialization is not its own end. On the contrary, the division of intellectual labor presupposes subsequent exchange. Without trade, specialization is barren, dangerous even. Consuming only locally produced ideas — intellectual autarky — is a recipe for dogmatism and sterility. For those who follow current fashion and say that ideas do not matter, I say that the passion with which they retail this idea betrays them. I think that Keynes ([1936]1953: 383-4) was right in his famous valedictory to *The General Theory*: ideas matter, more than we know, and ideas matter even when those who depend on them fail to recognize it.

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The text uses "she" as the gender-neutral pronoun. Apologies are due to Geoffrey Brennan and James Buchanan for appropriating, as my own, the title of their book (1985).

Organization

This dissertation is organized as follows. Chapter One introduces science studies and economic methodology, the fields that theorize about science and economics, respectively, as independent objects of scientific study. Scientific standards (what good theories should accomplish) are discussed in the context of scientific goals, and the perennial methodological question in economics — is economics a science — is considered in light of economics' methods and its unique status among the different sciences, natural and social.

Chapter Two locates the current dispute in economic methodology in a broader theoretical and historical context. The received view in the philosophy of science, and the varied reactions to it, especially those which rely on a constructivist epistemology, are surveyed, with an eye toward establishing the intellectual antecedents that underwrite the current dispute in economic methodology.

In Chapter Three, I consider the manifestation of constructivist ideas in economic methodology, particularly the rhetorical economics of McCloskey and Klamer. Their program is critically surveyed and different critical responses to it are presented, with an emphasis on the nature and function of standards of appraisal in science. Chapter Four discusses further the implications of constructivism in economics, pro and con, making the case that the epistemological excesses of constructivism (incommensurability and non-rationalism, especially) are unneeded, and can be abandoned without doing violence to the heart of the new methodological critique. The arguments from the first half of the dissertation — the nature of scientific knowledge, the nature of scientific motivation, and the nature of scientific standards — are then summarized.

The last three chapters of the dissertation are devoted to developing a case for an economics of science. In this second half of the dissertation, I argue that the new methodology, when shorn its constructivist excesses, can fruitfully be seen as an economic rather than as a sociological (or rhetorical) theory of science, and that, thereby, we acquire a more robust theoretical explanation for science that is successful, and for the existence and function of the institutions in science that, in part, enable those successful outcomes.

In Chapter Five, I argue that Adam Smith's economics (as against more neoclassical alternatives) is best suited to an economic view of science. Smith's emphasis on agents who are rational but fallible, and self-interested but prudent, is most appropriate for science, where strong uncertainty and endemic market failures tend to preclude optimal choices. A Smithian economics also emphasizes the importance of institutions as decision-making resources (especially for social coordination and cooperation) when optimality is undefined, unattainable or undesirable. Institutions — rules of thumb, norms, conventions, standards — are defined, discussed and explained as rational responses to bounded rationality.

This framework — the Smithian agent and market institutions — is applied to the production of scientific knowledge in Chapter Six. There I consider the scientist as Smithian agent, science as a market-like process, and scientific institutions as evolved responses to market failures and other incentive problems that arise in the unique setting of scientific knowledge production. In particular, I analyze reward conventions, intellectual property rights, peer review and replication, and other scientific norms, and consider how this institutional framework helps to explain invisible-hand outcomes in science, given self-interested scientists, and market failures due to public goods, intellectual property, and asymmetric information.

In Chapter Seven, using as a case study the recent minimum-wage controversy in economics, I attempt to identify and to elaborate upon working institutions in the science of economics. The economic rules identified are meant to serve as concrete illustrations of the different scientific institutions that are theoretically explained in economics-of-science framework developed in Chapters Five and Six, and also as counterfactual evidence against claims that scientific rules are too ephemeral, too tacit or too variable to permit their articulation.

Chapter 1. Economic methodology and the study of science

As long as there has been science, there has been theorizing about science.[13] Since the ancients, scientists, philosophers, historians and others have studied science in the same spirit that science studies natural and social phenomena. The scholars who study science and its product, scientific knowledge, are sometimes called meta-scientists or meta-theorists. The meta-theorist thinks about science and scientific knowledge in the same spirit that the scientist thinks about her own subject. Though the questions will often be different, thinking about science and scientific thought is no different, in principle, than thinking about plant taxonomy or quantum mechanics or macroeconomics. The economist Andrew Schotter puts it this way: "We can aspire to alter the world, or to alter the way we see the world. One approach is to theorize directly about the real world, whereas the other is theorize about the theory existing to describe the real world . . . ." (1981: 144).

In economics, economists who specialize in meta-theory have traditionally been economic methodologists and historians of thought.[14] Methodologists and historians of thought are those

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[13] By science, I will generally mean "inquiry," broadly construed. In the English language, "science" usually means something like "the natural sciences," especially the experimental and mathematical ones. This narrow construction is in contrast to the meaning of "science" in most other languages, which usually encompasses the social sciences and the humanities. (On this, see McCloskey 1985a: 54-55).

[14] "Meta-theory" or "meta-economics" are somewhat cumbersome terms, but they have the virtue of avoiding some of the confusion that surrounds the term "economic methodology." The confusion about "methodology" arises because all economists study economic methods, but few do economic methodology. (For more on the distinction between economic methods and economic methodology, see Machlup 1978). "Meta-theory" should be seen as encompassing any theoretical approach to science (e.g., philosophy of science, economics of science, sociology of science), while "meta-

economists who theorize about the knowledge produced in economics, as against economic phenomena *per se*. Their preferred tools, until relatively recently, have been provided by the philosophy of science and by intellectual history. In recent times, meta-theoretical sources other than history and philosophy of science (such as literary criticism and sociology) have made their way into meta-economics, with profound consequences.

One can think of meta-economics as the center of disciplinary self-analysis. Any thoughtful economist can reflect on the nature and significance of her own (and other economists') practices, and, at one time or another, most practitioners do. Prominent economists invariably pause at some point to make methodological statements. Kenneth Arrow, Kenneth Boulding, Milton Friedman, Nicholas Georgescu-Roegen, Fritz Machlup, Paul Samuelson, Robert Solow, and George Stigler are examples. But it is professional methodologists and historians of thought, for better or for worse, who specialize in the fundamental questions about economic knowledge and its production in economics. What are the fundamental questions?

1.1 Meta-economics: methodological standards and scientific goals

In economics, as in other sciences, a fundamental methodological concern is this: when appraising the output of economics, how can one sort the wheat from the chaff? How do economists appraise models, journal articles, research proposals, dissertations, books, theories and research programs, all of which are competing for the scarce resources of attention and shelf space? How do we know that theory X is superior to theory Y? On what grounds, if any, do we base our critical judgments of economic research? More succinctly, what are the standards in economics?

All fields invariably confront the question of standards. This is true whether they do so explicitly, in a methodological context, or whether they do so indirectly, in the everyday activities of

economics" refers to theoretical approaches to economic science in particular, e.g. philosophy of economics or economics of economics.

producing and evaluating new intellectual output. Whatever the unit of intellectual output — model, paper, theory, research program — it will be judged by the extent to which it satisfies (or doesn't) current methodological standards. Methodological standards, in turn, derive from a larger vision, whether explicitly stated or not, that sets out what goals economics should be attempting to achieve.

This instrumental formulation conceives of methodological standards as means to scientific ends. A methodological standard like "reject inconsistent theories" is merely a means to the scientific goal of producing consistent theories. When an economist says that, *ceteris paribus*, theories should be parsimonious, her standard invokes the scientific goal of parsimony. Whether a given standard is a cost-effective means of realizing a scientific goal, and whether it is superior to alternatives, are related methodological questions.

What, then, are the goals of science that determine what the standards should be; what makes for a good theory? There is, alas, no single, authoritative answer — no canon of methodological standards. Indeed, entire disciplines (such as philosophy of science) exist, in part, to propose, argue and debate alternative answers. The list of scientific desiderata is diverse because human inquiry is itself diverse, in methods and in aims. Science can have and historically has had many different goals. The following are some well known and less well known examples. (In some of the examples, I have attached a name sometimes associated with the goal). Good theories should:

- increase understanding of phenomena (Steven Toulmin);
- predict phenomena accurately (Milton Friedman);
- explain phenomena well;
- always be expressible in the notation of formal logic (A. J. Ayer);
- be chosen based upon their relative expected profitability (Charles Sanders Peirce);
- be beautiful;
- not refer to unobservable entities (Paul Samuelson);

- be deduced from true premises established by introspection (John Stuart Mill);
- have survived criticism (William Bartley);
- be logically consistent with other theories that we take to be true;
- be logically consistent internally;
- solve problems (Larry Laudan);
- be persuasive (D. McCloskey);
- not be beholden to any set of methodological rules (Paul Feyerabend);
- be empirically verified (Terrence Hutchison);
- aim for empirical adequacy (Bas van Fraassen);
- be parsimonious (Ernst Mach) or elegant;
- lead to the prediction of hitherto unknown phenomena (Imre Lakatos);
- aim for generality (Thomas Kuhn);
- produce reliable knowledge (John Ziman);
- be falsifiable, at least in principle (Karl Popper);
- be acceptable to theorists without excessive cognitive or personal cost;

(Some of these can be found in Laudan 1995: 18). Each of these scientific goals suggests a methodological standard. If, for example, one believes that economics should produce theories that predict well, then a **procedural standard** follows: economists should try to produce theories that predict well. (A set of procedural standards can be seen as a **method**). A **standard of appraisal** also follows: rival theories should be evaluated based upon on their relative efficacy in prediction.

There are three points about standards of appraisal to be made here: they pertain to consensus, ambiguity and conflicting standards. (See Kuhn 1977: 320-39). First, different scientists will frequently have different methodological standards. Some will argue that a good theory must be elegant in construction; others will prefer decent explanations in whatever form. Some scientific communities (groups of scientists) will be more unified than others with regard to methodological standards, and, in fact, methodological consensus is one way of defining a community of inquirers. But as the foregoing list suggests, standards are numerous and varied, and even the most apparently unified disciplines will have individual scientists who embrace different methodological positions. One likely consequence of different standards, or a lack of methodological consensus, is theoretical disagreement.[15]

A famous example in science studies concerns geologists and the theory of continental drift. Continental drift theory was first suggested in 1915 by Alfred Wegener. For many years, geologists were divided in two camps — the mobilists who supported drift theory, and the contractionists, who did not. Until the mid-sixties, eminent geologists and geophysicists could be found in both camps, and both camps were able to cite methodological standards which the opposing theories (and their evidence) did not meet. In the mid-sixties, and apparently rather abruptly, virtually all geophysicists and geologists came to accept drift theory, with the advent of plate tectonics. (New and compelling evidence was produced at that time). Both camps employed slightly different standards in evaluating theory and evidence, which lead to different theoretical choices before the consensus. (On this history and the implications for methodological procedures, see Miriam Solomon 1992. See also Laudan 1995: 231-43, and my discussion in Section 4.10.1).

A second aspect of methodological standards is that, whatever they are, they must be interpreted when applied to actual cases. If may not always be clear when a given standard has been met by a theory, which creates ambiguity. Thomas Kuhn has advocated four possible methodological standards[16] — accuracy, consistency with accepted theories, breadth of applicability (generality or

[15] Though the reverse is not true: disagreement is not conclusive proof of different methodological standards.

[16] At one time, Kuhn had a fifth criterion, "fruitfulness" (1977: 322), which he apparently abandoned.

scope), and simplicity. He goes on to point out that these theoretical desiderata are often "equivocal" in their application. Says Kuhn: "Accuracy is ordinarily approximate, and often unavailable. Consistency is at best local: it has not as a whole characterized sciences as a whole since at least the seventeenth century. Breadth of applicability becomes narrower with time . . . [and] [s]implicity is in the eye of the beholder." (1992: 13). Hence, whether a given theory (or other unit of intellectual output) succeeds in meeting a standard may be hard to determine unambiguously.

One implication is that "individuals [who share the same standards] may legitimately differ about their application to concrete cases." (Kuhn 1977: 322, cited Laudan 1995: 90). Supreme Court Justices who have very similar readings of the Constitution will sometimes reach different decisions in adjudicating a specific case. The same is true for scientists appraising rival theories.

Even if a community of scientists unanimously agrees on the relevant standards for theory appraisal, and are able to measure theoretical success against those standards, theory choice will remain problematic when there is disagreement on how the different standards are to be weighted. Say, for example, that all scientists in some community endorse Kuhn's four standards, and those alone. A theory (call it T_1) in this community is therefore appraised by judging the extent to which it meets those standards (S_1 .. S_4), and its value, then, is a function thereof:

$$V(T_1) = v(S_1,S_2,S_3,S_4).$$

But without an explicit functional form (also shared) that assigns relative weights to the four elements of theory appraisal, choices may still vary from scientist to scientist. As Kuhn argues, "two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions." (1977: 322). Theory choices will be uniquely determined, only if there is an (shared) **algorithm** of theory appraisal. If, for example, all scientists value theories with the same explicit function, say,

$$V(T_1) = \alpha_1 S_1+\alpha_2 S_2+\alpha_3 S_3+\alpha_4 S_4, \text{ where } \Sigma\alpha = 1,$$

then community consensus is guaranteed (given identical readings of theoretical success in meeting the standards). But in the absence of such an algorithm, shared standards may be insufficient for consensus on theory choice.

There is a third problem that arises with methodological standards, in addition to the problems of methodological consensus and of ambiguity in application. The third problem is that equally legitimate methodological standards can conflict. Say, for example, that economists want to produce theories that are very general and also very accurate. To the extent that scope and accuracy are substitutes, more of one will generally mean less of another. Hence, even if economists have consensus on standards, i.e., we unanimously endorse scope and accuracy as methodological goals, it may be impossible to satisfy both goals simultaneously. Unanimity on methodological goals, a rare thing, is therefore not sufficient for theory choice when there are conflicting standards among those unanimously endorsed.

The problem of conflicting standards is hardly unique to economics. It is a feature of most scientific disciplines. Even the grandest of scientific theories cannot meet all methodological goals. Newton's theory of gravitation predicts remarkably well in most circumstances, and covers a large range of phenomena. But Newton's theory offers no understanding; Newton could not say what gravity was, nor could he determine how it actually worked. Nonetheless, his theory remains extremely reliable knowledge in most settings, and Adam Smith was not alone in thinking that Newton's achievement was "the greatest discovery ever made by man." (Cited in Loasby 1991: 7). Charles Darwin's theory of natural selection, to pick another famous example, seems to explain a host of phenomena in evolutionary biology. But natural selection does not predict with precision, indeed it doesn't predict at all, at least not in sense of forecasting the future. Better theories will invariably meet more standards, but even the best theories will entail methodological trade offs.

Let us now consider methodological standards in economics, with reference to what I will call the "three questions" in economic methodology.

1.1.1 Meta-economics and the three questions

One can, without too much simplification, think of all methodological standards as having implications for one (or more) of what I will call the **three questions of economic methodology.** The three questions are as follows: (1) what do economists do?; (2) what should economists do?; (3) do economists actually do what they should?[17] These three fundamental questions comprise the traditional area of interest for economic meta-theorists. More recently, particularly with the advent of rhetorical economics, meta-theorists have come to consider an important, fourth, possibility: what do **economists** (as opposed to methodologists) say when answering the three questions? (See Klamer 1983). In other words, what do economists say about what they do, what they should do, and whether what they do is what they should do?

The first question — what do economists do — suggests a **descriptive** approach, one sometimes thought to be the province of historians of economic thought. The second question — what should economists do — involves a **prescriptive** approach, traditionally undertaken by economic methodologists. Methodological standards, as we have seen, are prescriptions that derive from a claim (or set of claims) about the proper goals of economics. The third question arises from the intersection of the first two: to what extent do economists actually observe methodological standards, or, do economists do what they should?

If one believes, for example, that economists should summarily reject theories that have been refuted by the data (i.e., falsified), and finds that, in practice, falsified theories are rarely rejected in economics, one has an interesting methodological problem to consider. The problem immediately suggests additional questions: if economists don't do what they should do, what does this say about

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[17] A more traditional catalog would simply substitute the word "theories" for the word "economists," and this substitution would suffice for most of our purposes. I will employ the broader formulation, however, which recognizes that economists do more than make theories.

what economists do? Does it mean that economists are Sabbath-only methodologists, who don't practice what they preach, perhaps because there are inadequate incentives for carrying out a worthy scientific goal? Or is it the case that economists are sensibly eschewing an impractical or inefficient rule, so that the problem is with the preaching, not the practice?

The three questions in economic methodology represent three kinds of theoretical work for the economic meta-theorist: (1) description, (2) prescription, and (3) judgment. The fourth issue — what do economists say when answering the three questions — can be thought of as providing evidence for or against our positions on the three questions.[18]

In offering this rudimentary taxonomy, I in no way want to claim that the three questions (and what economists say about them) somehow exhaust all methodological issues. They do, however, usefully organize a lot of work in economic meta-theory, and are convenient for purposes of exposition. Let us now turn to an illustrative example.

1.1.2 Friedman on the three questions

As an illustration, consider the most famous article in economic methodology, Milton Friedman's paper, "The Methodology of Positive Economics" (1953).[19] Let us consider first question two — what should economists do? Friedman says that economists should devise theories that predict well. Why? Because "the ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful predictions . . . about phenomena . . . ." (1953: 7). Friedman's methodological standard is good prediction: "theory is to be judged by its predictive power

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[18] Survey evidence should be treated with caution, of course. Economists' stated theoretical preferences may well differ from those "revealed" by their theoretical choices. The same is true for the methodological standards used in making those choices. That said, there is enormous value in meta-theoretical surveys of the kind pioneered by Klamer (1983).

[19] The philosopher of economics Dan Hausman refers to it as "by far the most influential methodological statement of the century." (1992: 162).

. . ." (ibid: 8). Theories are to be comparatively appraised by determining which predicts better. (His paper is famous not for this assertion alone, but for the instrumentalist claim that "unrealistic" (read: false) assumptions are of no consequence in economics, provided the theory predicts well).

Friedman the methodologist prescribes prediction as a standard, because he believes that the aim of science is good prediction. In fact, he claims that prediction is the **only** legitimate goal for a "positive" science, trumping all other possibilities, such as explanation or understanding. If Theory X's predictions are better than those of Theory Y, then Theory X is to be chosen.[20] Friedman offers a procedural rule — economists should create theories that predict, and an appraisal rule — economists should judge theories by their predictive efficacy. Only if two theories predict equally well are other standards to be considered. In case of ties, says Friedman, subsidiary methodological standards may be invoked. He offers theoretical parsimony (elegance) and logical consistency as tie-breaking possibilities (ibid: 10).

Now consider Friedman's answer to question one in economic methodology. Friedman is more or less silent on what economists do. We are left to presume that economists actually construct hypotheses and test predictions against the evidence in accordance with Friedman's method. As such, Friedman is really making normative rather than positive claims about economic practice, and, as Ronald Coase has noted, the title of the article is therefore misleading. (Coase 1994: 18). We should note that, if only implicit in Friedman's paper, there is the argument that economists are, like all human beings, susceptible to "ideological biases." This is the reason that economics needs methodological standards — to counter a natural tendency for economists to confirm their theoretical priors. In particular, Friedman hints, economists should attempt to refute (or falsify) rather than to confirm their

[20] I set aside the question of what "good" prediction entails. For example, do we opt for a theory that predicts more phenomena, though less accurately, over a theory that predicts fewer phenomena though more accurately, and how are such trade offs to be weighed?

hypotheses — an unacknowledged nod to Karl Popper.[21] Falsification demands far more of theories than does confirmation. If actually practiced, falsification is better suited to rooting out well-loved but poor hypotheses, and therefore to counteracting the bias towards confirming theoretical priors.

Regarding question three in economic methodology, Friedman doesn't consider whether working economists actually create and appraise theories using prediction as their paramount methodological standard. He offers no evidence on economic practices, except to make the following, tantalizing claim about the hypothesis of profit maximization by firms: "evidence for the maximization-of-returns hypothesis is experience from countless applications of the hypothesis to specific problems and the repeated failure of it to be contradicted." (ibid: 22). The trouble is that Friedman provides no instance of the "countless applications" which fail to refute the profit-maximizing hypothesis. (Nor does he specify the circumstances under which he would consider profit maximization falsified). Friedman is surely correct about the hardiness of profit maximization as a theoretical postulate in economics, but he produces no evidence whatsoever for his claim that profit maximization survives because it remains unrefuted.

Regarding the fourth issue — what economists say regarding the three questions — Friedman is likewise silent. Thirty years later, however, explaining why he did not write on methodology after 1953, Friedman did say: "I came to the conclusion that there was essentially no relationship between what people said about methodology and what they did in their actual scientific work." (cited in McCloskey 1994: 183). Having applied the three questions rubric to Friedman (1953), let us turn to the following question: where do the aims and standards of science (economics) come from?

1.2 Where do the goals and standards of science come from?

Friedman's answer to the second question in economic methodology — economists should only

[21] Says Friedman: "Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it . . . ." (ibid: 9)

produce theories that predict well — is asserted, not argued. What, an economist might wonder, are the grounds for arguing that prediction is the "ultimate goal" of science, trumping other possible goals, like explanation or understanding? And what of Friedman's (1953) other methodological claims: that the truth or falsity of theoretical assumptions is irrelevant to theory appraisal; and, that the greatest scientific theories are those which make the most "unrealistic" assumptions? Where did he get these methodological ideas about what economists should do? [22]

I want to argue that Friedman's methodological ideas, while they surely concern economics (and, in particular, the mid-century marginalism controversy), did not come from economics. His instrumentalist and predictionist ideas are, not surprisingly, philosophical in origin. Economic methodologists have traditionally acquired their theories from somewhere else, most often the philosophy of science. Whether cowed by the presumptive authority of philosophers, or reasoning that philosophers have a comparative advantage in such matters, or hoping for a benediction that would certify economics as properly scientific, economic meta-theorists have regularly turned to philosophy for help in devising meta-theories. In particular, economics has relied on philosophers of science, where "science" means something like "mathematical sciences, particularly those that reside in the Physics Department." Until very recently, twentieth century economic methodology has largely entailed **applying** philosophy of science to economics.[23]

The traditional reliance on philosophy of science for meta-economics has, for some, been

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[22] I do not attempt a complete answer here. The interested reader can consult Hirsch and DeMarchi, 1990 or the relevant papers collected in Caldwell 1984.

[23] This was not always so. Philosophy of science is recognizable as a distinct field within philosophy only since the 1930s. (Callebaut 1993). In the 19th century, before the advent of academic specialization and rise of "economics" as a separate discipline, economists and philosophers of science were often one in the same person. Prominent examples include William Stanley Jevons, who wrote a distinguished treatise on science, *The Principles of Science* (1958 [1874]) and the philosophers of political economy, such as John Stuart Mill (1877 [1836]), John Cairnes (1888), and John Neville Keynes (1917).

excessive. McCloskey says, for example, "[I]t's time to put away the philosophical tools, misunderstood and misused by most self-described philosophers of economics, and pick up the historical and sociological and rhetorical ones." (1995: 1322). For others, it has represented a fruitful research area. Whatever one's position, there is nothing inherently wrong in making use of philosophy for methodological arguments in economics. It would be surprising if one did **not** exploit professional scholarship in the philosophy of science, given that philosophy of science was, for many years, the principal locus (for better or worse) of meta-theoretical work.

Still, economists' traditional reliance on the philosophy of science seems to have breed a certain credulity regarding philosophy. In particular, economists have too often believed (wished?) that the philosophy of science has somehow succeeded in settling the hard questions about scientific knowledge.[24] It has not. That we economists might prefer otherwise is entirely understandable: methodology is an enterprise that produces seemingly interminable debates about recondite questions, and too little in the way of determinate guidance for economists and other scientists. As Bob Coats says: "Unlike old soldiers, basic methodological issues in economics do not usually fade away; more often they reappear in new uniforms." (Coats 1983: xii). This can have the effect, as the economic methodologist Bruce Caldwell says, of making methodological matters "agonizing." (Caldwell 1982: 1).

But wishing won't change that fact that philosophy has not produced an authoritative text where the "correct" scientific goals and corresponding methodological standards are clearly spelled out, and the ancient debates about scientific knowledge are settled. There is, alas, no canonical rule book, and hence the appraisal of economics cannot consist in referring to some established, infallible

[24] Though economics has changed a great deal in the intervening 65 years, Lionel Robbins, for example, claimed that "How economics should be pursued . . .may be regarded as settled between reasonable people." (1932: 72 cited in Coats 1983: 1).

authority — what Wade Hands has called "the shelf of scientific philosophy."[25] (Hands 1995a: 147).

In addition, even if a canonical rule book existed, it could not have come from philosophy of science. The great changes in economic methodology reflect the intellectual turmoil that has characterized philosophy of science over the last generation or so. Even contemporary defenders of the received view acknowledge that "philosophy's sense of certainty about what science is has disappeared." (Rosenberg 1994: 216). Philosophy of science has undergone profound changes in its various twentieth century movements — from logical positivism to logical empiricism to Karl Popper's growth of knowledge to the sociology of Thomas Kuhn to the anarchism of Paul Feyerabend. Any text that could accommodate this diversity would produce a confusing and contradictory welter of methodological positions, not a methodological rule book.

The belief (hope) among some economists that philosophy of science had somehow resolved the fundamental questions about science and knowledge, relies implicitly upon another proposition — the unity of science hypothesis. The unity of science hypothesis says that all fields of inquiry should have the same goals, standards and methods. What works in physics, the traditional exemplar, will work in economics, and indeed in any human science.[26] The question, of course, and this is a staple of methodological debate, is whether human sciences can sensibly be modeled after natural sciences such as physics. Even if philosophy of science had somehow produced a canonical rule book, it would apply to economics only on the additional assumption that economics and physics can be essentially

[25] And, as noted earlier, even if a canonical rule book actually existed, methodological disputes would not thereby disappear, due to the problem of ambiguity in application. The U.S. Constitution is canonical, but there are, of course, competing interpretations of Constitutional rules, and even learned experts (Justices and constitutional scholars) cannot come to consensus on many important issues. (This is not to say that an authoritative text might not help in economics).

[26] I say "implicit" because many economists simply take the unity hypothesis for granted. In part, this assumption reflects the fact that most philosophy of science read by economic methodologists (Popper, Lakatos, Kuhn) is predominantly philosophy of physics — the hardest of the "hard" or natural sciences.

similar enterprises.

Eminent economists such as Frank Knight and Frederick Hayek have argued that the methods that have made the natural science so successful simply cannot be applied to the more complex realm of society. To do so is to commit a methodological category mistake that Hayek called "scientism." A great many economists, in contrast, are happy to conceive of economics as a kind of social physics. Whether it is possible to treat natural and social sciences alike remains a contentious issue. My point here is that the wish for an end to methodological controversy requires more than a canonical rule book, it also requires a defense of the unity of science hypothesis.

Whatever one's position on the unity of science, economics makes a superb case study for the student of science, because economics has always blurred the traditional lines that separate the "hard" sciences and the rest of inquiry. Economics is an interesting case because it perches so precariously between the "hard" or exact sciences that refer to nature, and the "soft," inexact sciences that refer to human society.[27] Philip Mirowski argues that economics is the paradigm case of a field that is both "hard" and "soft": "Economics née political economy, née moral philosophy, has been a prime locus of the hashing out of definitions of both the Natural and the Social in Western culture . . . ." (Mirowski 1994b: 6).

We now turn to the issue of "locating" economics along the continuum of different sciences, "hard" and "soft." Economic methodology has typically rendered the location issue as some variation on the question: is economics a science, and, if not, what is it? I offer what can only be a preliminary answer, with the intention of laying some conceptual groundwork for heavier philosophical weather

[27] The scare quotes on "hard" and "soft" are deliberate because the distinctions between hard and soft sciences are often ambiguous. If, for example, we take "hard" to refer to sciences that predict, or to sciences that undertake controlled experiments, we are left with some anomalies. Natural sciences like evolutionary biology and geology, for example, are essentially historical; they do not predict. Similarly, astronomy, which is carried out in that "hardest" of natural science departments, physics, does not undertake controlled experiments.

that lies ahead.

1.3 Is economics a science: formal deduction, empirical testing, and replication

Is economics a science or is it something else? The question is not as scholastic as it first appears. Important worldly considerations turn on the answer we provide. In particular, there are the matters of funding and of prestige. If the answer is yes, then economists can continue to receive funding from the National Science Foundation (NSF) and the private sector, and avoid the fate of sociologists and anthropologists, for example, who fare less well in obtaining public monies and private consulting fees. The honorific of "science" also affects public and academic perceptions of our discipline, and this, in turn, may affect employment opportunities and wages. Influential public agencies (such as the Federal Reserve Board) rely on the counsel of economists, in part because they regard our advice as scientific. There is no Council of Sociological Advisors counseling the President. More generally, and recent post-modern debunking notwithstanding, science is still widely regarded as a prestigious enterprise and as a material benefit to society. Non-science is, well, something else. As long as this perception remains in force, correct or not, it is in the interests of economists to be regarded as scientists. Economics has a material stake in its scientific status. Now to the question itself.

Answering whether economics is a science requires answers to two more fundamental and even more difficult questions. They are: (1) what is economics, and (2) what is a science? I shall not here attempt a complete answer, for the simple reason that an answer cannot possibly be rendered in a few pages, and even an incomplete answer will take up a good part of the subsequent chapters. Instead I offer some preliminary discussion that is designed to consider economics' rather unique position — at once inside and outside "hard" science — without pretending that there is a simple, clear and succinct criterion by which we can demarcate "hard" science (or Science with a capital S) from other kinds of human inquiry. In so doing, I will introduce some important concepts which will prove useful in later chapters.

First, what is economics? This is not a trivial question, and for several reasons. First, a definition presupposes that a disciplinary boundary can meaningfully be defined. Frank Knight doubted that it could be done. "It is impossible to draw a clear-cut boundary around the sphere or domain of human action to be included in economic science." (Cited in Kirzner 1960: 1). Second, definitions invariably imply disciplinary goals, and we have already seen that goals can differ, can conflict, and can change over time. Third, even if one is not yet discouraged, a definition depends upon whether economics is to be distinguished by its subject matter (traditionally, markets, material wealth and resource allocation), by its techniques (constrained optimization, statistical inference, axiomatic deduction) or by its ideas (scarcity, opportunity cost, rationality, self-interest).

Lionel Robbins' classic statement (which owed much to the Austrian economists) is partial to a definition based upon economic ideas: "Economics is the science that studies human behavior as a relationship between ends and scarce means which have alternative uses." (Robbins 1932: 15, reprinted in Hausman 1981: 116). The familiar ideas are rational behavior, scarcity, choice and opportunity cost. Gary Becker's bold definition takes Robbinsian choice under scarcity and adds to it a set of empirical claims about what economic agents know. Says Becker: "The combined assumptions of maximizing behavior, market equilibrium and stable preferences, used relentless and unflinchingly, form the heart of the economic approach as I see it." (1976: 5).

Becker's definition essentially takes Robbins, and adds the strong information assumptions needed for maximizing (as opposed to rational) choice. Optimal choices generally require large amounts of information — an ordering of preferences that is singular, complete and transitive, complete knowledge of future utility functions, complete knowledge of the relevant probability distributions, complete knowledge of the other players' rationality and knowledge, and the like. Even setting aside claims about fixity of preferences and market clearing, Becker's optimality requirements require assumptions that, while in the spirit of Robbins, go well beyond the idea of rational choice under scarcity. (Neither Robbins or Becker are especially modest in their definitions; both imply that the scope of economics includes virtually all human activity, or at least that part attended to by the human sciences).

Alfred Marshall, for his part, adopted more of a subject-matter approach when he offered: "the study of mankind in the everyday business of life," by which he meant an inquiry into how one gets and then uses income. (1920: 1). Marshall accordingly emphasized intimate knowledge of actual production and exchange practices. I am partial to the definition, attributed to Jacob Viner, which says: economics is what economists do. Viner's definition is only partly tongue-in-cheek; it recognizes the difficulty of defining economics, and the inevitable inadequacy of any definition. (Boulding 1948: 3). Viner's definition also reminds us that our own behavior as economists (and that of scientists more generally) is itself evidence which can be brought to bear on our theoretical claims.

1.3.1 Economics as formal deduction

I suspect, however, that for the laity, and perhaps even for the working economist, what makes economics "scientific" are its techniques, in particular the use of mathematics and statistical methods. As against the other social sciences, economics has a history of attempts to bring the tools of the "hard" sciences into the study of social processes. It is clear that late-19th century visionaries in economics like William Stanley Jevons and Leon Walras wanted to produce a kind of social physics. Walras, in particular, saw his nascent general equilibrium theory as the means by which economics could achieve the status of a natural science. The very title of Francis Edgeworth's 1881 book, to pick another example, *Mathematical Psychics*, succinctly conveys this aspiration.

The implicit theory underwriting an emphasis on mathematics is that science consists in logically valid deduction from axioms (first principles) that are taken to be true. We can characterize this rationalist approach as Cartesian or Euclidean in recognition of its desideratum of certainty —

with true axioms and valid logic, our theorems must be true — and its emphasis on scientific knowledge as a product of introspection and logic alone. The only role for empirical work is to determine the applicability of the true theory, where applicability is constrained by the variables not specified in the theory, what John Stuart Mill termed "disturbing causes," what we today call the *ceteris paribus* variables.[28] (This capsule characterization is developed more fully in Appendix 2A of the next chapter).

The influx of scientists and engineers into economics during the 1930s brought with it new mathematical techniques and, in some measure, the ethos of the Cartesian or deductive approach to science. The halting but ultimately successful ascendancy of formal, especially mathematical technique in economics is a fascinating disciplinary history that remains to be fully documented. (Excellent beginnings are available in Ingrao and Israel 1991 and in Mirowski 1989). In the case of contemporary economics, an exhaustive survey is not required to see the preeminence of the deductive approach.

If formal deduction is the hallmark of a hard science, as Walras imagined, then modern economics looks like a hard science. To an outsider, a scholar from the humanities, say, an economics journal is too technical to easily comprehend. The scientific paper of an economist looks formidably formal. It is most often an exercise in deductive logic that uses the mathematics of constrained optimization — employing techniques developed by natural scientists and mathematicians such as LaGrange, Hamilton, and Liapunov. "Proof," "theorem" and "lemma" are the obligatory headers. The mathematics are sometimes complemented by econometrics, which is technically demanding in its own right.[29]

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[28] The preceding paragraph owes much to Gerard 1990: 198-200.

[29] A philosopher of social science who assayed economics in the mid-seventies, characterized it as a branch of applied mathematics. (Rosenberg 1976).

Our primary means of disciplinary communication is by scientific paper, presumably because rapid changes on the scientific frontier tend to make longer works (monographs and books) obsolete. Graduate school training is increasingly taken up with mastering the techniques of constrained optimization and of statistical inference. Klamer and Colander (1990) document that graduate students at leading universities overwhelmingly cite mathematics, not economics, as the best preparation for their economic studies. A mastery of mathematical techniques is vastly more important in graduate training than a knowledge of actual economic issues.

Economics apparently looks "rigorous" even to scientists steeped in a mathematical culture.[30] A distinguished group of physicists and other scientists, convened at the Santa Fe Institute, were stunned by the mathematical sophistication of their economist colleagues. (Waldrop 1992: 140-43). The Santa Fe physicists were equally surprised by the extent to which the economists relied on strictly axiomatic approaches. Asked to solve the same problems, the physicists found that the economists began, rather than ended with algebra, and that the economists were notably less interested in considering intuition, computational approaches and, most striking, empirical support. (ibid).

1.3.2 Economics and empirical testing

The Santa Fe physicists were surprised by the economists' relative aversion to evidence because modern physicists are not pure Cartesians. The obvious importance of mathematics to physics notwithstanding, physicists are keen to empirically test their theoretical conclusions. They tend to think that a proper science must make sense of an external reality. Truth, on this view, is not (merely) a matter of consistency, but of correspondence to the world. Call this the empiricist approach. For the empiricist, economics looks "soft." The empirical hallmarks of a "hard" science — testing, controlled

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[30] The modern emphasis on formal expression in economics has lead to some curious euphemisms. "Rigorous" means something like very mathematical, especially in the axiomatic mode. "Theoretical" has come to be a synonym for "mathematical," so that a very theoretical economist is not doing a lot of theory *per se*, but is doing a lot of mathematics.

experiment, and replication of results — are relatively rare in economics.

Take testing first. The ascendency of technique in economics has had an effect on the amount of empirical work.[31] Wassily Leontief (1982) surveyed ten years of articles in the *American Economic Review* (AER) (March 1972 to December 1981) and found that more than 50 percent of them contained mathematical models without any empirical data. Analyses with no mathematics and no empirical data were an additional 21 percent in the first five years (March 1972 to December 1976) and 12 percent in the following five years. In other words, about two-thirds of AER papers contained no empirical work. Theodore Morgan (1988) updated Leontief's six years later. Morgan found an increase in empirical analysis in the AER from March 1982 to December 1986 — rising from roughly one third to one half of all papers. Morgan also surveyed the *Economic Journal* (EJ) over Leontief's ten years and Morgan's five years. There was no trend towards more empirical work in the EJ; non-empirical papers consistently (i.e., in each of the three five-year intervals) constituted about 58 percent of the total.[32]

Morgan also compared economics (as represented in the AER and the EJ) to a sample from four other social and physical sciences — politics, sociology, chemistry and physics — for the 1982-86 period. The results are striking. Economics is more mathematical (no surprise) than political science and sociology, which produce strictly mathematical papers only in 18 and one percent of all cases, respectively. But economics is less empirical than our two sister fields — non-empirical work in political science and sociology is 42 and 22 percent, respectively, well under the 50-60 percent in Leontief's and Morgan's surveys. Papers without empirical work are unheard of in chemistry (zero

[31] Many critics of modern economics' emphasis on formalism do not criticize mathematical approaches *per se*, but emphasize the opportunity costs of relying on formal approaches, in particular the crowding out of empirical work. (Woo 1986, Mayer 1993a).

[32] These references are from Blaug 1992: xxi-xxii. Note that Blaug's bibliography has an incorrect citation to Morgan 1988.

percent) and rare in physics (12 percent). Within the empirical category, these two fields also do vastly more experimental and simulational work — 83 percent in chemistry and 48 percent in physics. (ibid).

To the extent that economists actually confront theories with evidence, we use "soft" statistical inference, an attempt to find regularities in historical observations, not "hard" experiment. (Morgan (1988) reports about 6 percent of economic papers were experimental or simulational). There is a difference. Controlled experiment tries to locate invariances in nature that are outside of historical time; economic data is always of a time and place. Since social time and place varies widely, our data are capable perhaps of ascertaining "tendencies" (in John Stuart Mill's sense of the term), but not "laws." In discussing the status of economics, Sir John Hicks emphasizes this difference, saying that economics is "on the edge of science and on the edge of history":

> It is on the edge of science, because it can make use of scientific or quasi-scientific methods; but it is no more than on the edge because the experiences that it analyses have so much that is non-repetitive about them. If a scientific theory is good, it is good now, and it would have been good a thousand years ago, if it had been available; but the aspects of economic life which we need to select in order to make useful theories can be different at different times . . . . Economics is in time, and therefore in history, in a way that science is not. (cited in Redman 1991: 106).[33]

In recognizing a difference between experiment and inference from history, I do not mean to ignore the recent increase in experimental work in economics, nor the importance of its results. (See, for example, Kagel and Roth 1995). But economics has never been a center of randomized controlled experiment, and current experimental work represents a very small proportion of research output. Economics experiments (if they are to be true to real-world situations) have several handicaps. First,

[33] Most sociologists of science will object to the idea that experiments are outside of their historical context. They would agree that economics is "in time," but they would dispute Hicks's claim that the natural sciences are not. Collins (1985), for example, disputes the idea that experiments are evidentially superior to historical observation. He argues that even "crucial experiments" are unavoidably historical, because perfect replication is unattainable.

and perhaps most important, economics experiments tend to be very costly, since the financial incentives to participants must be real and significant. Second, there is usual question of whether laboratory results (generally using students as subjects) can legitimately be generalized to real market settings. And, third, experiments outside the laboratory are likely to entail ethical problems, as when the "control group" gets a placebo rather than, say, a transfer payment.

Clearly, not all of these shortcomings are unique to economics, but I conjecture that they are important enough that economics will continue to rely on historical observations for the empirical research we do attempt. Kenneth Arrow told the physicists at Santa Fe that this difference in empirical emphasis constituted the crucial difference between a science like economics and one like physics. Economists, Arrow said, cannot easily undertake randomized controlled experiments (Waldrop 1992).

In addition to the relative dearth of empirical work, especially the underemphasis on controlled experiment, there is also the matter of how economists treat hypotheses and published research results in practice. The issues arising here are (1) specification search and publication bias, and (2) replication of research results. Though econometrics uses the Popperian terminology of "falsification," most work that is actually published takes the form of "innocuous falsification," i.e., results that confirm rather than reject the preferred hypothesis.[34] Blaug (1992: xx, ff. 5) cites Canterbery and Burkhardt's (1983) report on their survey of 340 articles in the *AER*, the *EJ*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics* (all from 1973-78), which finds that only three papers actually rejected the author's preferred hypotheses. This result obviously suggests that, among published papers at least, confirmation is what is rewarded, whatever our rhetoric about rigorous falsification in the Popperian sense.

This startling asymmetry between published positive results (theory confirmed) and published

[34] By "preferred hypothesis," I mean the hypothesis that comports with the theorist's theoretical priors, which is generally the alternative hypothesis (H_A) to the tested or null hypothesis (H_0).

negative results (theory falsified) has been widely noted (see Denton 1988), and seems to occur in other social sciences as well. In reviewing over 300 articles in psychology, for example, Smart (1964) found that less than 10 percent of these articles presented falsifying results; the remainder supported the preferred hypothesis. (Cited in Hull 1988: 344). Denton (1988: 174) argues that this strong tendency towards confirmation (or innocuous falsification) is the product of a biased selection process both before and during the referee process.

Denton offers several economic explanations: specification search (or "data mining") is what occurs when individual researchers try alternative models (or altered data sets) until one with robust test statistics emerges (especially *t*-ratios above 2). But this kind of improper practice is not the only source of bias in published results, there is also the "file-drawer" problem. The file-drawer problem arises when researchers (honest researchers and "data miners" alike), fail to produce the desired results and this useful information — the number and specification of tests that fail — is filed rather than submitted for publication, and thereby lost to other researchers. There is also "editorial filtering," or publication bias, the tendency for editors a referees to prefer confirming results, or to prefer results with greater statistical significance. (Denton 1988: 176-79).

Whatever combination of these practices leads to bias in publication, one result is that economists tend to steeply discount the hypothesis-testing value of published econometric results. As Denton says:

> If I have carried out a test of some economic hypothesis at the 5 percent level of significance and the statistic succeeds the critical level, I do not sell my holdings of French francs and buy Japanese yen, move to Florida, or change my occupation. I may decide to write a paper . . . . The point (well known) is that the results of *hypothesis tests are not used in economics as decision criteria but rather as indicators suggestive of correspondence between real world observations and particular economic theories* [emphasis added] . . . (1988: 172).

Fancier econometrics do not help to overcome the skepticism bred by experience. As the labor economist Richard Freeman points out: "[A]s all practitioners know, any single piece of complex econometric analysis rarely convinces anyone, for the more sophisticated the analysis, the greater the danger the results derive from the model than from the world."[35] (Freeman 1989: x).

Paul Samuelson, reflecting on fifty years of econometrics, confesses that experience has turned his great expectations for econometrics into a wiser, but rueful skepticism. Says Samuelson:

> Back when I was 20 I could perceive the great progress that what being made in econometric *methods*. We would be able to test and reject false theories. We would be able to infer new good theories. My confession is that this has not worked out . . . . I never ignore econometric studies, but I have learned from sad experience to take them with a large grain of salt. . . . It seems objectively to be the case that there does not accumulate a convergent body of econometric findings, convergent on a testable truth. (Samuelson 1992: 243).

There are other sources of skepticism regarding econometric results, one of which is the fact that, whatever their merits, they are rarely replicated.

"Hard" sciences traditionally insist on replicability and replication. Results are contingent until they have been replicated by other researchers. "Soft" sciences, in contrast, are content with unreplicated (and even unreplicable) results. A famous recent attempt at replication, funded by the NSF, reviewed empirical papers submitted to the *Journal of Money, Credit and Banking* (JCMB) from 1980 to 1982 (some papers were already published, others were accepted, others under review). The results were dramatic.

Out of 154 JMCB authors notified, only in 90 cases were authors willing and able to supply data and programs. DeWald et al. reviewed 54 of these data sets, and found that only eight were sufficiently free of problems to permit an **attempt** at replication. And, only in two of the remaining eight papers were the results actually reproduced in full. The review team was thus able to fully reproduce econometric results only 3.7 percent (2/54) of the time. (DeWald, et al. 1986). And, one should note, DeWald et al. were attempting replication only in the narrow sense of reproducing

[35] There is, of course, a large literature on econometric practices. See, for example, Hendry 1980 Leamer 1983, Lovell 1983 and McCloskey and Ziliak 1996.

econometric procedures (using original data sets and statistical techniques) — what is sometimes called "checking." DeWald et al. were not undertaking replication in the stronger sense of the term, i.e. generating new data and determining whether the theory holds up under more general conditions.

Feigenbaum and Levy's (1993) replication exercise selected some older empirical papers for their simpler econometrics, with the hope that replication would be less problematic. They asked their students to replicate (again in the sense of reproduction) econometric results in thirty-six papers from "first-rate journals" of the 1960s and 1970s, where the principal econometric technique was ordinary least squares. As a measure of replication, they calculated the median absolute deviation (MAD), which is = Median$|(b_i-b^*_i)/b_i|$, where b_i is the paper's reported coefficient and b^*_i is the replicator's obtained coefficient. Feigenbaum and Levy found, for all the papers replicated, that the mean MAD was 64 percent, though this number reflects one outlier, a paper with a MAD of 757 percent! Nonetheless, even the median MAD was 22 percent, a magnitude far too large to be attributed to rounding or to other innocuous sources of error. (Feigenbaum and Levy 1993: 226-27).

In sum, then, economics looks like "hard" science in some respects, especially the predominance of formal, deductive techniques. In other respects though, economics looks "softer": the relative paucity of empirical work, the limited extent of controlled experiment, and the rarity of replication. Contemporary economics is certainly done differently than is physics, the traditional exemplar. I hope this brief survey is sufficient to show that economics, as currently practiced, is truly "in between" in Hicks's sense, at once historical and scientific, at once soft and hard. I now want to suggest, in Section 1.4, that many of longstanding methodological debates in economics owe their origin to this unique duality in the nature of economics.

1.4 The scientific status of economics and methodological disputation

Because economics is both "hard" and "soft" it has produced a great deal of intramural methodological dispute regarding the scientific status of its claims to knowledge. One eminent economist goes so far as to say that "no science has been criticized by its own servants as openly and constantly as economics" (Georgescu-Roegen 1971: 1). The methodological debate has taken many forms in its varying contexts since the Menger-Schmoller *Methodenstreit* of the 1870s, but all of these forms can be seen as variations on the traditional methodological theme: is economics a ("hard") science, and, if not, what is it.

Carl Menger ([1883] 1985) argued that economics could indeed be a "hard" science, at least in the sense of being an "abstract" or "theoretical" science. On Menger's view, it is reasonable to think of some economic propositions as (at least) analogous to natural laws. The Law of Demand, for example, should hold regardless of historical time or social location. Gustav Schmoller, a younger member of the German Historical School, countered that markets vary in time and place, hence it is not possible to abstract economic "laws" that would hold under more general circumstances. The most economics should do, Schmoller says, is to attempt descriptions of how markets happen to function in their respective eras.

When Terrence Hutchison (1938) brought the ideas of logical positivism to economics, he argued that economics, if it is to be scientific, must be empirical — in particular, economists must attempt to verify their hypotheses. If economics would not subject its theories to testing, then it was not science. Frank Knight (1940, 1941a, 1941b) rejected Hutchison's empiricist standard by arguing that economics was a **social** science, and that the empirical methods of natural science were therefore inappropriate.[36] Echoing what Hayek condemned as "scientism" in the social sciences, Knight said:

> The fallacy is that social science is a science in the same sense as the natural sciences, in which the revolution has occurred, that its problems are to be solved by carrying over into the social sciences the methods and techniques which have produced the celebrated triumphal march of science in the study of nature, and that this procedure

[36] Said Knight: "This [Hutchison 1938] emotional pronouncement of value judgments condemning emotion and value judgments seems to a reviewer a symptom of defective sense of humor." (1940: 151).

will lead to a parallel triumph in our own field . . . (1941b: 121).

Hutchison and Knight disagreed on what economists should do, because they disagreed on what constituted the appropriate aims and standards of a social science like economics.

Not long thereafter, Richard Lester (1946) set about to test some key behavioral postulates of neoclassical economics— in particular whether firms actually maximize profits. Lester found (based on surveys of 58 CEOs), for example, that most firms determine employment principally as a function of product demand, with far less concern for wages and non-labor costs. Firms, in other words, don't deliberately maximize profits when making hiring choices. Hence, marginalist price theory was, Lester maintained, based on false assumptions, and was therefore unscientific.[37] Lester's survey, in effect, took Hutchison's empiricism seriously.

The defenders of neoclassical marginalism (especially Alchian 1950, Machlup 1946, and Friedman 1953) responded by conceding Lester's point — firms don't maximize profits, hence the claim that they do is false. They then boldly asserted that this concession doesn't make economics unscientific. Alchian argued that competitive market forces work, in the long run, to weed out unprofitable firms so that only the maximizers survive. Even if the assumption of intentionally maximizing behavior is false in some (or even most) instances, competition works to ensure that only maximizers survive, and hence the theory is reasonable. Economics, said Alchian, is better seen as a theory of market outcomes than as a theory of individual behavior.

Milton Friedman embraced Alchian's argument from natural selection, and then added, as we have seen, an instrumentalist twist. Friedman argued that science consists in good predictions only, and that the truth or falsity of assumptions is irrelevant. The assumption that firms knowingly maximize is false, but the prediction that only maximizing firms will survive in the long run is borne

[37] Hall and Hitch (1939) had, in a similar paper, found that firms tended to set prices not optimally, but by marking up costs.

out by experience. Ergo, price theory is therefore scientific after all.[38]

What all of these famous methodological controversies have in common is an attempt to answer difficult questions that are, I am suggesting, closely connected to a more fundamental question, namely, "is economics a science?." The debates concerning the extent of abstraction possible in economics, the possibility of a unity of natural and social sciences, the efficacy of testing in a *ceteris paribus* framework, the "realism" of assumptions, and the nature of our most basic theoretical postulates, are all hardy methodological perennials precisely because they bear on the question we posed at the beginning of this chapter: when can economics legitimately claim to have produced genuinely scientific knowledge? The answer is of more than academic interest, given that prestige, legitimation, funding, corner offices, the ear of the Prince, and other worldly goods are at stake.

The rub, as I having been suggesting, is that any sensible answer will turn on what one means by "genuinely scientific knowledge." And, theorists of science have failed to produce a knock-down, true-for-all-time demarcation criterion that would enable a unambiguous yes-or-no response. One result of this failure is turmoil in the theory of science and the recent ascendency of theorists who think the whole project of demarcating science is impossible, and misguided in any case. In economics, these skeptical students of science are known as the new methodologists.

1.4.1 The new methodologists on the old methodological question

In economics, there has recently arisen a different kind of answer to the perennial question — an answer due to the "new methodology." The new methodologists — economists like McCloskey, Klamer, Weintraub and Mirowski — are those who have imported into economics the radical changes that have occurred in the philosophy of science in the last generation or so. The new methodological "incursion" in economics borrows from the revolutionary work of Karl Popper, Imre Lakatos, Thomas

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[38] For a discussion of the marginalist controversy in the context of evolution and selection-style arguments, see Vromen 1994.

Kuhn, Paul Feyerabend, and Richard Rorty (to name but a few) and also from the new fields in science studies inspired, in part, by these thinkers — postmodern literary theory, rhetorical studies, and the sociology of scientific knowledge.

The "new methodology" is certainly part of the long tradition of methodological dispute in economics that I have briefly documented above. On this score, the new methodologists' say: economics is not a science. What sets them apart from the earlier methodological disputants is the nature of their critique. Economics, they say, is not unscientific because it fails to meet their criterion for science. No, the new methodologists make a more radical claim. They argue that economics is "unscientific" because **all** disciplines are unscientific, in the sense proposed by traditional philosophy of science. For the new methodologists, *science itself is not scientific.*

The new methodologists' position is therefore distinguishable from traditional critiques of economics in that its critique is far more ambitious — it indicts not merely economics, but all science. Traditional methodological appraisals of economics took the following form:

(1) A field is scientific if it does X [a demarcation criterion];

(2) Economics does (does not do) X;

(3) Hence, economics is (is not) a science.

Let X be "prediction" and we have Friedman; let X be "verification" and we have Hutchison; let X be "falsification" and we have "early" Popper, and so on. The post-modern or new methodological critique doesn't quarrel with a particular statement of X *per se*, it denies that any statement of X is possible. The new methodologists argue that there is no meaningful way to **demarcate** science from non-science.

For the new methodologist, science is not a separate, special realm of inquiry, because we cannot distinguish goals, standards or methods that are meaningfully different from those in other types of inquiry. As a result, the traditional dichotomies that underwrite demarcation — "hard" and "soft," "objective" and "subjective," "positive" and "normative," "fact" and "value" — do no epistemological work (see McCloskey 1985a: 42); they are best seen as *post hoc* rationalizations by successful scientists who wish to legitimate their practices.

Paul Feyerabend, who has provided much grist for the new methodological mill, pushes this logic farthest. Feyerabend (1975) argues that actual scientific practice routinely flouts contemporaneous methodological standards. What scientists do, says Feyerabend, is different from what philosophers say they should do. (In fact, he argues, the "best" science, historically, is that which **most** violates the scientific standards of its day). Hence, Feyerabend concludes, no standards can be shown to better promote the goals of science, and the only methodological rule that can be applied across history is a kind of anti-rule: "anything goes."

For the new methodologist, the economic methodologist's perennial question — is economics a science? — is utterly misplaced, even moot. Economics, says McCloskey for example, is no different from poetry, nor, for that matter is mathematics or physics. Phil Mirowski argues, in a similar vein, that "Mathematics [should] be treated neither as pure logic, nor as convenient language, but as yet another form of trial-and-error practice on all fours with congressional testimony, tenure evaluation, literature searches and arbitrage operations." (1994a: 57). The new methodologists are radical egalitarians when it comes to appraisal of the knowledge produced by different disciplines. They reject the traditional concern with demarcation because they deny that there are any epistemically meaningful differences between what physicists produce, for example, and what literary critics produce. Hence, the traditional methodological task of determining what the rules of science are (what should scientists do), and whether a given discipline successfully meets them (do scientists so what they should), should be replaced by the more limited project of studying how science actually proceeds (what do scientists do).

1.5 Conclusion

Like the new methodologists, I want to move past the perennial question of demarcation in economic methodology. But, unlike them, I do not believe that this move somehow entails the end of all interest in the nature of the knowledge called "economics." Even if demarcation is something of a chimera, we still need to ask many of the usual, normative, epistemic questions, such as, when is a economic claim well founded, and does a particular rule for doing economics tend to promote reliable knowledge?

The point is that demarcation of science from non-science is not the same thing as distinguishing reliable from unreliable knowledge. As Larry Laudan argues, one should avoid conflating what are two distinct questions: (1) what makes a belief scientific, and (2) what makes a belief well-founded. (1995: 222). There are beliefs considered scientific that are not especially well founded, and there are "nonscientific" beliefs that are very well founded. What is called science in our culture is "epistemically heterogeneous": some theories are well tested, others are not; some sciences are progressing at a rapid rate, others are not; some theories make successful prediction of surprising phenomena, others never do; and some hypotheses are *ad hoc*, while others are not. (ibid: 221).

So I am not especially concerned with the project of sorting beliefs into "science" and "non-science" boxes, but I think it is a mistake to believe that foreswearing demarcation requires abandoning inquiry into the nature of scientific knowledge. On the contrary, it is worth knowing why the social practice traditionally labeled "science" tends to produce more reliable knowledge than do other social processes. And in particular, it is worth knowing whether the institutional structure of science — the rules which reward and sanction scientists — helps to explain science's relative efficacy in producing reliable knowledge. Theorists of science are not obliged to do philosophy of science, but neither can they completely avoid its traditional questions.

An economics of science should, following a suggestion in Coase (1994), theoretically investigate which institutional structures are most likely to produce collectively beneficial outcomes, given more or less self-seeking scientists. But an economics of science should not be agnostic about those outcomes — especially the production of reliable knowledge — and here it departs from the new methodologists. An economics of science maintains the idea that reliable knowledge is worth having, that it is distinguishable (at least in part) from other sorts of knowledge, and that it is connected by more than happenstance to the social processes that produce it.[39] Different disciplines produce different kinds of knowledge, and some disciplines are better at producing reliable knowledge.

We have now come to the point when we can no longer beg the many questions raised by our current discussion. We need to know more of the philosophical antecedents to the current methodological dispute, especially as they bear on the new methodology. This task is undertaken next, in Chapter Two. In Chapters Three and Four, the new methodological position is reviewed and critiqued in light of the (sometimes ancient) arguments developed in Chapter Two, with an eye towards developing, in the second half of the dissertation, an economic view of science.

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[39] It in no way requires "privileging" one type of knowledge over another.

Chapter 2. The dispute in economic methodology: its antecedents in the philosophy of science

In 1982, the economic methodologist Bruce Caldwell informed economists what philosophers of science had known for some time, that positivism was dead (Caldwell 1982). In a now famous article, D. McCloskey (1983) warned economists that positivism was alive and well (he called it "modernism"). They were both right.

McCloskey and Caldwell were writing near the beginning of a revival of interest in methodological work in economics, rekindled in the mid-1970s. (A 1974 conference at Nafplion, Greece on the status of scientific theories in physics and economics was particularly influential; the edited proceedings on economics are in Latsis 1976). Caldwell was correct in claiming that logical positivism was long dead in philosophy. It had been more or less superseded by a variety of historicist (and other) reactions to its tenets, beginning in the mid-1950s.[40] McCloskey's point was that this was news to economists. Economics, said McCloskey, lags broader intellectual trends; the spirit of positivism (or at least a variant hybridized with Karl Popper) was alive in economics.[41] What is positivism and what are the implications of its demise for economics?

2.1 The positivist project

In the first part of the twentieth century, a group of philosophers (The Vienna Circle) began

[40] And, as we will see, there is now a third-generation reaction to the historicist reaction, one that makes use of aspects from both positivism and the historicist reaction to it. (Callebaut 1993).

[41] Rudi Visker (1990) quips that economics has functioned as a kind of nature preserve for outmoded philosophies of science.

an undertaking that has become a famous chapter in a much longer history of attempts to determine what makes science different from other sorts of inquiry. That chapter is typically called logical positivism, though it (and its subsequent cognates, logical empiricism and neo-positivism) are often collectively referred to with the shorthand term "positivism." This is a somewhat imprecise[42] convention I will nonetheless observe. What was positivism, as broadly defined, attempting?

The positivists were attempting to find a way of demarcating scientific knowledge from other kinds of knowledge. In particular, scientific knowledge was to be distinguished by its special methods of knowledge production. These methods are a set of procedural and appraisal standards. The positivists were, in short, attempting to create a canonical rule book for science. As the philosopher Steven Fuller defines it, positivism is "the project of deriving universally applicable formal principles for the conduct of inquiry." (1993: 1). In textbook representations of science, the positivist principles of inquiry are sometimes loosely referred to as **the** Scientific Method (SM).

"Universally applicable" means what it says. Positivism conceived of its methods as applying (1) at all times and (2) in all places. This implies, for example, that modern scientific methods would have worked in ancient Greece, were they available, and, indeed, will also function in contemporary, non-Western cultures. Also implied in the notion of a universal scientific method is the unity of science hypothesis — SM will work in the natural and social sciences alike. What works in physics is appropriate for sociology too. "Universal" means at all times, in all places, and across all disciplines.

It is important to point out that the different proponents of what I am calling positivism have often disagreed on the details of precisely which principles of inquiry constitute the rule book. But, as

[42] See Hacking (1983: 41-57) for a useful sorting out of the different varieties of positivism. Economists who usefully tackle the subject in an economic context are Blaug (1992), Caldwell (1982) and Redman (1991).

Philip Kitcher argues, they concur on the essential points. In particular, there is agreement that: (1) there are universal, objective rules of theory appraisal; and (2) scientists are (at least tacitly) aware of the rules, and (3) scientists systematically apply the rules in developing and testing their theories. (Kitcher 1993: 3). The rules exist; scientists know them; and scientists follow them. Kitcher's point three raises the issue of scientific motivation.

Positivism's principal unit of appraisal is a theory or hypothesis. Positivists are far more concerned with theories than with human beings who make and appraise them — Ronald Giere calls this **theory centrism**, as against **person centrism**. (1990: 22-3). Hence, positivism has a rather undeveloped view of scientific motivation. But, as point three suggests to the economist, the positivist take on scientific motivation is naive: it casts the scientist as a selfless, dispassionate seeker of purely cognitive goals. Newton-Smith characterizes this naive view of scientific motivation as follows: "[I]n the noble . . . pursuit of some worthy aim (variously characterized as truth, knowledge, explanation, etc.) the members of the community dispassionately and disinterestedly apply their tools . . . on the royal road to the much esteemed goal." (1981: 1).

On the positivist account, scientists are selfless, and utterly without non-cognitive ambitions, i.e. they seek only the advancement of knowledge. In the language of sociology of science, positivism implies that only factors "internal" to science influence the positivist scientist. She is either unaware of or wholly immune to "external" influences — matters like income, security, prestige, the esteem of colleagues, the larger social climate, and the actions of rival researchers. (Kitcher 1993: 73). The positivist scientist pursues knowledge heedless of her own self interest.

Finally, positivism says that it is the philosopher of science's role to discover and articulate the universal rules that scientists are to follow. Analogous to the physician-patient relationship, the methodologist prescribes to scientists the proper course of action to achieve the desired goal. This methodological guidance helps to forestall ill-conceived choices by working scientists (improves efficiency). (Ibid: 9). It also provides a benchmark with which the historian of science can evaluate previous choices by scientists.

Let us elaborate more on the positivist view of science and scientists, especially in its mid-century guise as the received view in the philosophy of science.

2.2 The first generation: positivism and the *received view*

The foregoing aspects of positivism, when admixed with other constituents of philosophy of science regnant until the mid-fifties, yield an amalgam that has come to be called **the received view** of science among philosophers of science. (Suppe 1977). Emphasizing its mythic (and somewhat elusive) status, Philip Kitcher calls it "Legend" (Kitcher 1993). Thomas Kuhn's revolutionary book referred to it as "the image of science." Whatever label one employs, the received view embodies several philosophical ideas about science that will be important for our discussion. Ian Hacking's (1981) list of what he takes to be the essential characteristics of what we will call the received view of science is a useful place to begin. The essential ideas are:

1. **Scientific realism**: science is an attempt to find out about one real world; and there exists a unique best description of that world.

2. **Theory** and **observation** are separable.

3. There are empirical **foundations** to knowledge. Observation and experiment provide foundations for and **justify** theories.

4. The **context of discovery** is separable from the **context of justification**;

5. Science is **progressive**; new knowledge builds cumulatively on what is already known.

6. **Demarcation**: scientific beliefs are distinguishable from non-scientific beliefs.

7. **Unity of science:** there should be just one science about the one real world.

8. Theories have a **deductive structure.**

9. Scientific concepts are **precise**, and have fixed meanings.[43] (1981: 1-5, with minor modifications).

From Hacking's nine tenets, which covers a vast terrain in philosophy of science, I would like to emphasize several aspects. Take item one, scientific realism, first. There are several key ideas here about the nature of reality (ontology) and our knowledge of it (epistemology). **Realism** is the doctrine that there really is an objectively existing world "out there." Says philosopher John Searle: "I have defined realism as the view that the world exists independent of our representations of it." (1995: 153). A realist believes, therefore, that the earth existed before there were humans to perceive it and to think about it. A realist perspective also implies that "when we die, and all our representations die with us, most features of the world will remain totally unaffected; they will go on exactly as before." (ibid, p. 154). Thus defined, realism is an ontological position, not an epistemic one. (ibid).[44]

Scientific realism, however, adds an epistemic component. It claims the objective existence of an external world, but it also argues that scientific claims about that world are true. (See Kitcher 1993: 169). Scientific realism conceives of scientific knowledge as representations of a singular reality, and sees theoretical representations as corresponding to that one world — the better the correspondence, the truer the theory. Hence, for the scientific realist, theories are true or false in virtue

[43] McCloskey provides a similar list for economics, which comprises the "precepts of modernism." McCloskey argues that the following are believed by a majority of economists: (1) prediction and control is the point of science; (2) only prediction matters to the truth of a theory; (3) only (experimental) observation decides truth; (4) introspection (unlike observation) is not objective and is therefore worthless; (5) only the quantitative matters; (6) the context of discovery is separable from the context of justification; (7) science and non-science can be demarcated; (8) normative and positive can be demarcated; (9) explanation requires covering laws; (10) all statements are analytic or synthetic. (McCloskey 1985a: 7-8). McCloskey's list differs from Hacking's especially in its instrumentalist rather than realist emphasis, probably reflecting the wide influence of Friedman 1953.

[44] Things get a bit more complicated when theories refer to things that are unobservable, like quarks. The realist will insist that entities one cannot "see" are nonetheless real. Says Hacking: "protons, photons, fields of force, and black holes are as real as toe-nails, turbines, eddies in a stream, and volcanoes." (1983:21). Some philosophers of science deny the existence of such entities, and adopt a non-realist perspective, such as Van Fraassen (1980).

of how the world really is, and, at the limit, there is a theory which best corresponds to reality. (On scientific realism, see Hacking 1983).

Scientific progress (item 5), then, consists of better and better representations of reality. New science builds on existing scientific knowledge (this is economizing), and, in so doing, provides deeper or broader correspondence. A common analogy (popular in introductory economics textbooks) proposes seeing theory as a map, and reality as the terrain to be mapped. Maps can provide more detail for a given region (deeper correspondence) or can map a larger region, encompassing *terra incognita* (broader correspondence). (On the map analogy in economic theory see Goldfarb and Griffith 1991).

New maps are better than old maps when they provide better correspondence. A new theory T_2 is better than an old theory, T_1, when, for example, it explains the all phenomena that T_1 does, and also makes whatever true predictions T_1 does, and more. "More" means that T_2 should exclude some part of T_1 that is erroneous or cover a wider range of explanation and prediction than T_1, ideally both. In other words, progress occurs by subsumption — T_2 should **subsume** T_1. (Nagel 1961, as interpreted by Hacking 1983: 67-8). Newton's theory of gravitation, for example, can be seen as being subsumed by Einstein's more general theory. The fact that T_1 and T_2 can be meaningfully compared implies that rival theories are **commensurable**. Theories are commensurable when there is a common measure for comparison, a means of joint evaluation.

There are, as we saw in Chapter One, many criteria by which one can appraise theories, but the received view puts special emphasis on empirical testing. The received view is decidedly empiricist. Scientific theories often (if not always) have implications which can be compared with the world. "Maps," after all, refer to a world that is "out there" and knowable. Hacking's items two, three and four all obtain here.

Item three is the most fundamental, and is discussed at greater length in Appendix 2A. It says that there are empirical foundations to knowledge, i.e., that data (observations and experimental results) are the foundations upon which science is built. Facts are, at once, prior to theories, for theories are built to explain them, and they also (once a theory is built) can be used to test theories. A theory (or any belief) is justified by its concordance with the facts, which are objective. What makes facts objective — so called "brute facts" — are two claims: one ontological and one epistemic. The ontological claim is realism again— there is only one world out there, hence aspects of that world are everywhere identical. The epistemic claim is that facts about the world are accessible to and indubitable for all inquirers. (Kuhn 1992: 4-5). Facts are given. Anyone can get out there and look for herself.

Item two restates the idea that facts are objective in terms of theory testing. If facts are to serve as an independent arbiter among theories, then it is clear that facts cannot be themselves influenced by or the products of the theories they are meant to test. Theory and observation must be separable, i.e. facts must be generated by a process that is independent of the theories they will be asked to confront. That facts must be independent of theory is a crucial assumption for the received view, and for any empirically-based theory of science.

Item four — the distinction between discovering theories and justifying then — likewise matters for the an empirical theory of science. The distinction, which is due to philosopher Hans Reichenbach, says that the creation of theories is fundamentally different from the process of justifying them. Inspiration, creativity, the social milieu, the amount of funding, the scientist's birth-order, are all examples of things which may influence a given theory's development, or discovery. None of these things is of interest to the traditional philosopher of science — they belong to the murky domains of psychology, sociology and history. Philosophers traditionally care only about what happens to a theory after it is born — in particular how it is justified. The received view wants to know: is the theory logically consistent, is it supported by the data, confirmed by experiment, etc. (Hacking 1983:

5-6).

We have already touched on two other aspects of the received view in science: demarcation (item six) and the unity of science hypothesis (item seven). Demarcation, we saw, is the attempt to distinguish science from other kinds of knowledge, accomplished by identifying, using philosophical first principles, criteria which distinguish science from non-science. The unity of science hypothesis says that all sciences should have the same methods, standards and goals, that the human sciences, for example, should proceed like physics.

Finally, we have also seen, the received view of science is **theory centric**. The theory, not the individual scientist as actor, is the unit of analysis. The scientist *qua* actor appears only as the corporeal embodiment of what theories do. Scientists, it is assumed, understand the rules and selflessly obey them. Apart from this mostly implicit assumption, the received view is content to more or less ignore scientific motivation.

Appendix 2A locates the received view in a larger philosophical and historical context. This context accomplishes two purposes. First, it permits some expansion on the foundational aspects of the received view, especially the its emphasis on justification and certainty. Justification and certainty are important issues in the debate begun by the new methodological critique in economics. Second, since I will be referring to the new methodological position (among others) as anti-foundational, some additional discussion on foundationalism may be clarifying, especially for the economist. Third, some history helps to show that current methodological dispute in economics has ancient antecedents, which can inform the current debate, however *au courant* today's disputes otherwise appear.

Whenever debate concerns ideas as fundamental as knowledge, one can be sure that venerable arguments remain pertinent, however far from everyday economics they appear at first glance. As Warren Samuels reminds us: "All economics, indeed all inquiry, is a series of footnotes to, or derivations of, various philosophical positions, each of which [has] . . . their strengths and weaknesses

. . . ." (1990a: 309).

2.3 The second generation: the erosion of the received view

As Appendix 2A suggests, the received view in the philosophy of science comprises ideas with a long and distinguished pedigree — most date to the Enlightenment, and others are ancient. It was not until the 1950s that philosophy of science began to seriously quarrel with own project. Mid-century theoretical developments in the philosophy of science challenged the received view on several fronts. Second generation philosophers of science (Popper, Toulmin, Hanson, Quine, Lakatos, Feyerabend and, especially Thomas Kuhn) attacked the received view by removing one pillar at time.

This second generation in the philosophy of science eventually challenged all of the accepted ideas in the received view. Let us review what are the most important arguments in the second-generation arsenal, continuing to use Hacking's list for reference. The key challenges, we will see, are addressed to the received view's empiricism, in particular the nature of facts and their role in testing theories. Let us start with Karl Popper, the philosopher of science probably most familiar to economists.

2.3.1 Popper on testing: falsification

Karl Popper is best known in economics for his emphasis on theory testing via falsification. Properly scientific theories, said Popper, are those which are potentially refutable, i.e. at least capable of being tested against empirical reality.[45] Science is demarcated from pseudo-science by its willingness to risk refutation: "the criterion of the scientific status of a theory is its falsifiability, or refutability or testability." (Popper 1962: 37). Successful theories are those that have survived

[45] Theories like Marxism and Freudianism are, in contrast, pseudoscientific, Popper argued, because they do not specify the conditions under which they might be falsifiable or refutable. (Popper 1962: 37-38). Ironically, Popper's program is vulnerable to reflexive application. Popper's theory of falsification cannot be falsified, and if, as he argues, only falsifiable theories are scientific, then Popper's theory is itself unscientific.

attempts to falsify them. Scientists therefore should make "bold conjectures" and attempt to refute them "mercilessly." (ibid: 33-65). Scientists who merely attempt to "confirm" their hypotheses are not doing science — any hypothesis, no matter how half-baked, can locate some confirming evidence.

Evidence can never confirm our hypotheses; they can only fail to disconfirm them. Hence, the following is an illegitimate inference on Popper's account (Hollis 1994: 74-75):

(1) H → O
(2) O
therefore (3) H.[46]

If my hypothesis is that "all swans are white" and I subsequently observe a white swan; I have not thereby confirmed the truth of my hypothesis. I cannot **know** that my hypothesis is true. The next swan might well be black. If, in fact, I do observe a black swan, then my hypothesis is refuted — I **do** know that my hypothesis is untrue.[47] In notation, then:

(1) H → O
(2) ~ O
therefore (3) ~ H

Hence, if I continue to observe white swans only, while I cannot regard my hypothesis as confirmed, I can regard it as not yet disconfirmed. For Popper, even continued failure to refute does not justify a theory, it constitutes nothing more than a kind of corroboration.

2.3.2 Popper: knowledge without certainty

In economics and elsewhere, Popper is often mistaken for a first-generation positivist. There are obvious similarities: he retains the emphasis on the importance of empirical testing, he wants to demarcate science from non-science and he generally accepts the discovery/justification dichotomy. His philosophy likewise remains theory-centric. Popper, however, saw his work as overthrowing

[46] "H" refers to a hypothesis, "O" to observations. "→" means "entails" and "~" means "not."

[47] More precisely, I know that my theory is false to the extent that I can rely on my data — the observation of a black swan. I may be mistaken, or the bird may be white, but stained.

positivism. Where Popper departs from the received view is in his insistence that certain knowledge is unattainable, and hence, theory justification, traditionally defined, is impossible.

Popper is an antifoundationalist. There is, for Popper, no foundation of certitude that can underwrite our science, only an endless process of error elimination. Any theory, no matter how long it has survived, may eventually be refuted — most, in fact, are. Our scientific knowledge, even that produced by strict Popperian standards, is unavoidably fallible. (Popper is sometimes referred to as a fallibilist). Popper puts it this way: "I am on the side of the search for truth, and of intellectual daring in the search for truth; but I am against intellectual arrogance, and especially the misconceived claim that we have the truth in our pockets, or that we can approach certainty." (1987: 141).

Popper denies that certainty is ever possible. Scientific knowledge can be enormously valuable, but it is always corrigible, hence the traditional definition of scientific knowledge as certainty is doomed to failure. His famous metaphor for the uncertainty of scientific knowledge makes Popper's antifoundationalism clear:

> The empirical basis of science has nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on the piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure." (cited in Redman 1991: 118).

Knowledge is possible, but certainty is not. Our knowledge is real but it is also fallible and therefore corrigible. The best we can hope for is to eliminate error, provisionally accepting theories that have yet to be refuted.

As we will see in Chapter Four, Popper does not take the "end of certainty" to be fatal for science, as do more radical anti-foundationalists, including the new methodologists. Reconceiving knowledge as fallible need not imply that science is somehow irrational. On the contrary, Popper argues, rational choice in science does not require certainty. His principal contribution is to recognize that knowledge without certainty is possible, and indeed valuable. Popper abandons the ancient program that equates scientific knowledge with certitude, but remains committed to the idea that fallible knowledge is worth having.

2.3.3 Can facts serve as foundations to science?

Following Popper's attack on the foundationalist doctrine that equates knowledge and certainty, there arose an even more radical critique of the idea that facts are objective and can therefore serve as a foundation to science — adjudicating among rival theoretical claims. Especially important in propounding this critique were two philosophers, Willard Quine and Norwood Hanson.

What Quine argued against in his famous "Two Dogmas of Empiricism" paper (1953) is the received view's claim that empirical data can be wholly objective. The reason facts are not objective Quine argues, is epistemic: facts are not given to us, prior to all interpretation. In effect, says Quine, Locke was wrong; the empiricist notion that experience is prior to belief won't hold water. Beliefs and experience are, rather, interactive. As an illustration, consider the following example from Alan Musgrave 1993a.

Say I believe that oars are straight. When out rowing, I observe that a partially submerged oar appears bent. My experience (the evidence) conflicts with my belief (the theory). Suppose I ascertain that the oar is not really bent, but only appears to be, because light propagates through water differently than through air, sometimes creating an optical illusion. Hence I reject the facts rather than the theory, and sure enough, once free of the water, the oar is straight.

Note the ambiguities that this creates for the idea of empirical testing. If facts are not necessarily prior to theory, then they cannot serve as an objectively existing foundation. Interpretation of facts opens the door to **different**, subjective views of the same data. I opted to interpret the facts in light of other theory (the propagation of light) **before** applying them. The brute fact of a bent oar would otherwise been disconfirming — a bent oar falsifies the straight-oar theory. Another observer who did not have access to the propagation of light theory would not have read the data as I did. She would have applied a different interpretation, perhaps, or would have, as a naive falsificationist, accepted the facts and discarded the straight-oar theory.

The idea that facts may be "theory-laden" is central to Norwood Hanson's (1958) argument. Hanson argues that different observers may construe the same objects differently — seeing is **not** believing, at least not in the sense that everyone will "see" the same thing. The problem becomes particularly acute when "facts" are generated by sophisticated experimental apparatuses rather than by observation. The design and construction of lab equipment is itself governed by theory, so the data cannot be neutral; they are, in effect, "contaminated" by theory. Facts must be objective, uncontaminated by theory, if they are to test theory. If facts are not objective — that is, directly and indubitably accessible to all inquirers — then there is no truly neutral court in which to try rival theories.

2.3.4 The Duhem-Quine thesis and testing

A related second-generation volley against the received view, also due to Quine, attacks the possibility of unambiguous theory testing on another front. Even if scientists can reach agreement on what the facts are, there is the problem of determining exactly what the facts say regarding the theories being confronted. The fundamental ambiguity of test results goes under the banner of the Duhem-Quine hypothesis — a nod to Quine (1953) and to Pierre Duhem (1954 [1905]), a physicist with rather prescient views in the philosophy of science.

The Duhem-Quine hypothesis applies to all attempts to "test" theories with evidence — whether data are used to confirm or to falsify. The problem is this: when evidence contradicts theory, should one reject the theory or reject the evidence? (Reject my straight-oar theory or the bent-oar evidence?). The naive Popperian says, reject the theory. Otherwise, we risk "immunizing stratagems," the *ad hoc* attempt by theorists to preserve their pet theories in the face of disconfirming evidence.

Without the demanding requirements of strict falsification, Popper worries that scientists will resort to innocuous falsification, or to mere confirmation, which provides no test at all.

The rule "reject upon refutation" assumes, however, that "crucial" or decisive tests are possible. But knock-down crucial tests are hard to come by, because there is almost always uncertainty as to what has gone wrong. Was it the theory, or the evidence? And, if it was the theory, which aspect? Data problems are familiar to all scientists — an experimental apparatus fails, or the viral sample is contaminated by wild bacteria (Ziman 1984), or the econometrics software automatically "adjusts" some problematic fields in the data set, or the "black" swan turns out to an oil-caked white swan.

Even if the data are deemed sound, it is still uncertain what exactly has been disconfirmed. This is because data do not test a single hypothesis in isolation — also being tested are auxiliary hypotheses, core propositions, background knowledge, etc. In Quine's elegant phrasing: "our statements about the external world face the tribunal of sense experience not individually, but only as a corporate body." (Quine 1953: 41). Thus, when tests are disconfirming, it the theorist, not "nature," who must determine which part (or parts) of the "corporate body" has been refuted.

When a econometric result, for example, is odds with "theory," it is difficult to determine what element of "theory" has gone wrong. Is it a core proposition that has been refuted, such as consumers maximize utility, or is it an auxiliary hypothesis particular to the model in question, say, consumers live for two periods? Or perhaps the culprit is some generally accepted background knowledge, such as the idea that agents discount the future, i.e., that rates of time preference are positive. Worse, it could be some combination.

In answering these common questions, the theorist is obliged to **choose** which aspect (or aspects) of the theoretical "web" has been refuted. Referring again to Hollis's notation, we have:

(1) (H_1 and H_2 and $H_3 \ldots H_n$) $\rightarrow$ (O_1 and O_2 and $O_3 \ldots O_n$)

(2) $\sim(O_1$ and O_2 and $O_3 \ldots O_n)$
therefore (3) $\sim(H_1$ *or* H_2 *or* $H_3 \ldots H_n)$

Testing failure cannot unambiguously reveal which theoretical component went wrong, so the theorist must make her own interpretation. "The choice of where exactly to point the accusing finger of refutation," Hollis remarks, "is ours, not nature's. . . ." (1994: 80). Falsification is designed to discipline the scientist, to enforce the unpleasant task of rejecting one's own theory. But to the extent that individual hypotheses are embedded in much larger webs of belief (Quine 1953), refutations are not decisive, and the theorist can rescue her theory by choosing the most favorable interpretation or by *ad hoc* modifications.

2.3.5 Is falsification an efficient rule?

The Duhem-Quine argument against the possibility of unambiguous testing has economic consequences. First, if crucial tests are not possible, then "naive" falsification — reject at the first whiff of disconfirmation — will likely not be a good long-run strategy. Naive falsification has the great virtue of avoiding Type II errors (accepting false hypotheses) altogether, but this comes at the expense of inducing Type I errors (rejecting true hypotheses). (Hands 1992: 61). It is surely desirable to have a mechanism that weeds out bad theories, but naive falsification is too blunt an instrument for delicate things like theories. Naive falsification will kill off virtually **all** theories. As Feyerabend says: "every moderately interesting theory is falsified . . . . Applied resolutely, Popperian criteria would eliminate science. . . ." (1981: 160).

Feyerabend has a point. No theory is without **some** empirical anomalies or internal inconsistencies or other transgressions. In this sense, as Thomas Kuhn says, all theories are born refuted. Can you think of any important economic theory that is not at least partially refuted? To an economist, it seems reasonable to suppose that killing a theory in its infancy may well prove less profitable than letting it survive until it becomes more robust. It is rash to jettison theories at the first bit of disconfirming evidence; provisional acceptance may provide higher returns over time. (See Coase 1994). One can agree with Popper that scientists are loath to reject their own theories without concluding that all attempts at theory modification are necessarily immunizing stratagems.

Popper's goal is to prevent immunizing stratagems on the grounds that they are investment in unproductive theories. Whether or not this is necessarily true, the economic insight remains: individual scientists have selfish interests (bad theory preservation) that can be inimical to the collective goal of producing good theories. Hence, quite apart from the merits of falsification (or any other rule) there is the question of incentives and enforcement. Will scientists obey methodological rules (solely) on the grounds that they are good for science, and, if not, what are the mechanisms for enforcing rules that don't enforce themselves?

2.3.6 Will scientists obey methodological rules?

Let us continue with falsification as a representative methodological rule. Is there evidence that scientists actually practice something like falsification in their work? Can we produce historical evidence that scientists ruthlessly falsify? The answer, says Imre Lakatos, a distinguished student of Popper's, is "no." The history of science is no kinder to Popperian falsification than it was to the positivism Popper sought to displace. This is hardly surprising, says Lakatos, given what we know of human nature:

> "Popper's [demarcation] criterion ignores the remarkable tenacity of scientific theories. Scientists have thick skins. They do not abandon a theory merely because facts contradict it. They normally invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain the anomaly, they ignore it. . . . (Lakatos 1978: 4).

Lakatos' answer should be, I submit, sensible to most economists. Lakatos recognizes that even if falsification is a recipe for good scientific practice, it does not follow that individual scientists will find it in their interest to mercilessly refute their own theories.

Lakatos thus touches on a crucial point for an economic view of scientific motivation. Even when we set aside the epistemological questions of the type raised by Duhem and Quine, we still need a theory of scientific motivation. Even if we know what scientists should do, it matters whether they will in fact do what they should. The naive view of scientific motivation that is implicit in the received view simply assumes away the problem by making scientists into demi-gods — they are selfless seekers of knowledge. A more sophisticated view of scientific motivation does not have this luxury.

It is important to note that Popper was not unaware of the Duhem-Quine problem. In fact, he proposed a number of additional rules that were designed to forestall the immunizing strategies that scientists deploy to save their favorite theories. (See Blaug 1992: 19 for a compendium) But, of course, these auxiliary rules, which Popper called "conventions," suffer from the same incentive problem. Unless scientific rules are self-enforcing, or viewed by scientists as inviolable norms, they will not be observed without some means of enforcement.

Popper was concerned mainly with the normative arguments for his rules, and less so with their likely efficacy. He did not attend to the economic question of whether there were sufficient incentives to ensure that his (or any other) rules were observed. Like the philosophers of the received view, Popper was less interested in what scientists actually do, and more concerned with what they should do. This attitude in the philosophy of science was completely altered by Thomas Kuhn.

2.3.7 Thomas Kuhn: the scientist as agent and incommensurability

Of all the second-generation philosophers, Thomas Kuhn, a trained physicist who became a philosopher who advocated history, is surely the most influential. Kuhn's original account was published in 1962, and a postscript was added in a second edition in 1970.[48] *The Structure of Scientific Revolutions* influenced an entire generation intellectually. In it, Kuhn quarreled with virtually all the received view's tenets as represented by Hacking, though I will want to emphasize

[48] Kuhn 1996 is the third edition, which adds only an index to the second edition of 1970.

three aspects in particular: (1) the idea of **incommensurability**; (2) **person centrism**, the emphasis on the scientist as an agent, especially as a social agent; (3) **historicism**, the importance of studying actual scientific practice when evaluating science.

Kuhn's account is too familiar to bear much review, but recall his emphasis on the idea of "paradigms." A paradigm has two meanings that are important: (1) a model or exemplar of good scientific procedure, and (2) the name for a set of shared standards and values within a research community, what he later termed a "disciplinary matrix." (Kuhn 1996: 176-8). The former is a "way of doing" recognized as exemplary by scientists within a community undertaking **normal science**, Kuhn's term for the quotidian, puzzle-solving nature of most scientific work. In contemporary economics, the exemplar is constrained optimization with equilibrium outcomes. If you want to publish your ideas, they must be represented in a formal model that makes use of maximization and equilibrium. The disciplinary exemplar of maximization *cum* equilibrium is what is taught in graduate programs in economics.

The latter sense of "paradigm" is more like the scientific *ethos* of a given research community. (Hacking 1983: 7-13). Kuhn refers to this meaning of paradigm as "the entire constellation of beliefs, values and techniques, and so on, shared by the members of a given community." (Kuhn 1996: 175). Speaking loosely, one can think of paradigm_1 as referring to modeling strategies or methods, and paradigm_2 as referring to scientific norms, standards, and shared culture.

Note that Kuhn is already talking of scientists and communities, not just theories. (His subsequent appeal to sociologists and other social scientists is not accidental). What differentiates research communities for Kuhn are their different paradigms. To this conception, Kuhn adds his notion of scientific change — normal science, crisis, revolution, new normal science. (Hacking 1983: 7-8). Scientific groups spend most of their lives engaged in normal science, says Kuhn, patching up theoretical anomalies, extending mathematical techniques, acquiring needed data, and the like. Normal science requires that the central elements in a body of knowledge are stable and widely accepted by scientists, as with, for example, Newtonian physics from about 1700 to about 1900. (Storer in Merton 1973: xxviii).

When, over time, empirical or conceptual anomalies accumulate, a theoretical crisis may occur, eventually resulting in a competing paradigm. The new rival may attract adherents, and eventually the old model is overthrown by mass defection, a scientific revolution. Following the revolution, Kuhn says, normal science is reestablished, with the new body of accepted knowledge serving as the stable ground for inquiry.

What makes Kuhn radical is not the idea of intellectual revolution, which is ancient, but his theory of revolutionary shift in paradigms. Kuhn argues that a "paradigm" switch is like a religious conversion or a psychological gestalt-switch — all at once the scientist sees the world from a different perspective. Kuhn refers to optical illusions like Wittgenstein's duck-rabbit picture, the birds/antelopes pictures and vase/two profiles picture to metaphorically illustrate the sudden and essentially arational nature of a paradigm switch.[49] (Hacking 1983: 11-12). Kuhn argues that a paradigm switch involves changing one's perspective altogether — living in a different (cognitive) world. The transition is such that there is no going back. The point for Kuhn is that different paradigms are, he summarizes, **incommensurable**. Their respective exemplars, their ethos, and their very languages are so different as to be untranslatable. Once you begin speaking Newton, there is no understanding Aristotelian physics.

I wish to emphasize three implications of Kuhnian incommensurability. First, the impossibility of meaningful comparison of theories across paradigms, implies that any standards for theory appraisal

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[49] The idea is that, without "new" evidence, and without the intervention of reason, the viewer of an optical illusion of this type, can see the same thing in an altogether different way. What once looked like a rabbit, though it is the very same thing, now appears to be a duck, and, importantly, this shift in our perspective is sudden and non-rational.

must be local to some paradigm. Different communities will employ different standards of appraisal so there can be no methodological unity between rival paradigms. Standards of appraisal, for Kuhn, are particular to a given paradigm, and will be the same in different paradigms only by coincidence. Sociologists, for example, will have different standards of what constitutes a good theory than will applied mathematicians. Likewise, the Keynesian macroeconomist will appraise theories using a different set of standards than will her colleague, the Monetarist.

Second, Kuhn argues that paradigms are incommensurable in the sense that a given community cannot make it discourse (theories, world-views, ethos, etc.) intelligible to a different community of inquirers. (Laudan 1990: 121). Incommensurability is not just a matter of different standards of appraisal in different communities, it also entails different conceptual languages. Different conceptual languages work to impede inter-disciplinary (or inter-paradigmatic) communication. Says Kuhn:

> "Proponents of different theories are, I have claimed, native speakers of different languages . . . . I simply assert the existence of significant limits to what the proponents of different theories can communicate to each other . . . . (Kuhn 1977: 320-39, cited in Hacking 1983: 13).

A key implication of incommensurability (in both the different standards and different languages senses), is that scientific progress cannot be defined, at least in fields where there have been scientific revolutions. The reason is that incommensurability rules out the joint evaluation of theories from (once) rival paradigms — the Newtonian physicist cannot understand Aristotelean physics, and she uses different standards of appraisal in any case. Hence, says Kuhn, it is not reasonable to think of scientific progress in the traditional, subsumptive fashion emphasized by the received view.

Third, Kuhn's religious or psychological characterization of a paradigm switch suggests that such changes are largely non-rational. When a scientist embraces a new paradigm, it is not so much due to reason and evidence. When you buy into a new theory, Kuhn says, "you begin to speak the language like a native. No process like choice has occurred." (ibid). Theory choice is therefore more like a "conversion experience" than a reasoned selection. Imre Lakatos (somewhat uncharitably) characterizes it as follows: the Kuhnian "*scientific revolution is irrational, a matter for mob psychology.*" (1970: 178, italics original).

Notice how Kuhnian incommensurability goes well beyond the idea that facts are theory-laden and cannot serve as objective courts in which to try theories. Even if facts are not inherently "neutral," there remains the possibility that a community could conceivably develop a set of agreed upon phenomena. There are, in fact, many scientific communities where there is widespread agreement on what the facts are. Incommensurability, however, has the effect of obviating this consensus on the facts, because incommensurability denies that there is any way to meaningfully compare rival theories about those facts. Kuhn takes the second-generation point that facts are theory-laden, and, by adding incommensurability, goes much farther.

2.3.8 Kuhn: history of science and naturalism

The first line of Kuhn's revolutionary book announces his historical intent: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed." (1996). Kuhn and other second-generation philosophers were reacting to the "armchair" approach of the received view. "To preach the virtues of *the* scientific method, while utterly ignoring the question of whether scientists now or in the past have actually practiced that method is surely arbitrary," summarizes Mark Blaug. (Blaug 1992: 31). The received view's emphasis on prescription over description seems more than arbitrary to the historicist; it is self-contradictory. The contradiction is that the received view, so enamored of testing scientific theories, never bothered to test its own theory's implications. Werner Callebaut says:

> It is a major paradox that the philosophical movement which . . . paid most lip service to "scientific philosophy" . . . turned away from the actual study of science and even committed itself to a foundational project which turned it into yet another "prima

> philosophia" [first philosophy], i.e. a philosophy purporting to be logically and methodologically prior to science." (1993: 3).

The received view did not see its own project as empirical. Though it prescribed good scientific practice, by ignoring the actual practice of science, it was itself unscientific on its own terms.

The second generation believed that what scientists actually do is relevant to any evaluation of science. The idea, largely credited to Kuhn, is that history can, in effect, **test** philosophies of science. Larry Laudan says that Kuhn in particular advanced the concept that "[h]istory is philosophy teaching by examples." (Callebaut 1993: 11). Knowing what scientists actually do (description or history of science) can influence our theory of what they should do (prescription or philosophy of science). If you believe that economists should, after Milton Friedman, concern themselves solely with prediction, then it is surely relevant to see whether economists have, in fact, done so. The doctor who prescribes to the patient presumably has some evidence that such prescriptions have worked in the past.

The move from logic to history was the beginning of a trend that has continued to this day in philosophy of science and in theoretical study of science more generally — the trend towards **naturalism**. Naturalism is the idea of taking a scientific (especially empirical) approach to the theory of science, as opposed to armchair epistemology. A naturalistic approach says that those who study science should proceed like other scientists, which requires, among other things, gathering evidence in support of one's theoretical claims.

Naturalism was not new with Kuhn and the second generation. Charles Sanders Peirce had proposed it nearly a century earlier, saying, "philosophy ought to imitate the successful sciences in its methods . . . ." (cited in Wimsatt 1981: 124). Nonetheless, Larry Laudan, among others, gives Kuhn the credit for reviving naturalism in post-war philosophy of science. William Wimsatt says, in this regard: "Kuhn liberated us (philosophers) not only to do history of science, but also science. History of science is after all just science looked at after it has happened." (Callebaut 1993: 24).

Even those contemporary scholars most hostile to science — the sociologists of scientific knowledge — prefer to conceive of their own work as being scientific. David Bloor, to pick a prominent example, proposes that sociologists of science should "transfer the instincts . . . acquired in the laboratory to the study of knowledge itself." (cited in Hands 1992: 159). And, "[t]he search for laws and theories in the sociology of science," says Bloor, "is absolutely identical in its procedure with that of any other science." (Cited in Laudan 1995: 189).

2.4 Kuhn transmuted: the theory of science outside philosophy

Second generation philosophy of science and, especially that of Kuhn, appealed to intellectuals far outside the narrow confines of philosophy of science. Kuhn's work attracted social scientists in particular; they were drawn to several aspects of his critique. First, Kuhn's theory was person- rather than theory-centric; Kuhn in particular emphasized the social aspect of science that was largely absent in received-view accounts. Second, the naturalism of the Kuhnian approach opened the door not just for history, but also for sociological and anthropological and rhetorical approaches to science. Finally, we should not underestimate the appeal of second-generation debunking of the traditional view of science. The received view clearly saw science as special — the uniquely rational part of human inquiry — and therefore privileged. Given the intellectual Left's hostility to elite privilege, and to authority (scientific and otherwise) more generally, the second-generation critique of science must have seemed liberating.

The wider influence of the second-generation philosophy of science matters for economics, because the "new economic methodology" brought second-generation ideas to economics only after they were transformed by a long subsequent gestation in different corners of the humanities — particulary the English and Sociology departments. McCloskey and Klamer's early work was strongly influenced by Wayne Booth, a distinguished scholar of rhetoric and literary theory, and by Richard

Rorty, a philosopher most influential in modern language departments. Roy Weintraub, for his part, was most influenced by his Duke University colleague, Stanley Fish, a literary theorist who has become the avatar of "critical studies" (and, allegedly, served as the model for the character Morris Zapp, who appears in the academic satires of David Lodge). And Philip Mirowski's methodological research has strong affinities with that produced by the sociology of scientific knowledge.

Before we turn to the advent of antifoundationalism in economics, it will be worthwhile to briefly survey the transmutation of the second-generation philosophy of science by following its migration into the humanities. We will focus primarily on Richard Rorty, owing to his influence on rhetorical economics, and will also consider the sociology of scientific knowledge.

2.4.1 Antifoundationalism and Rorty's indictment

Rorty's project is extraordinarily ambitious. His intellectual agenda is nothing less than to announce and then bring about the End of Philosophy, at least as it is conceived in the Anglo-American, or "analytic," tradition. (Gottlieb 1991). Such a critique obviously extends well beyond the issues in economic methodology we want to tackle, so I consider only the relevant arguments from Rorty's much longer list of indictments against foundationalism.

We consider Rorty, because he served as one of McCloskey's (1983, 1985a) chief intellectual guides in introducing a radical antifoundationalism to economics. Since I mean to use Rorty principally as an expositor of antifoundationalism as it has manifested in economics, I do not evaluate the many other aspects of Rorty 1979.[50]

Rorty argues that foundationalism fails because its theory of knowledge is wrong. Foundationalism goes astray because it accepts two influential but "uncashable" philosophical metaphors: (1) that the mind is the mirror of nature (Locke's theory of knowledge), and (2) that the

[50] For a sustained argument that Rorty's history of modern philosophy is defective, see Philipse (1994).

book of nature is written in mathematics (Galileo's theory of reality). The foundationalist believes, with Locke, that the mind is simply a passive instrument, a mirror that unerringly reflects the world, and, with Galileo, that the external world has an singular, immutable nature that can be uncovered by science. "The Book of Nature," said Galileo famously, "is written in the language of mathematics." (Musgrave 1993a: 110). But, says Rorty, Locke was wrong about the mind, and Galileo was wrong about nature.

Take Galileo first. Galileo meant for his figure to be taken literally; he believed that Enlightenment science had actually uncovered the language which nature itself uses. (Rorty 1981: 571). He did not see mathematical theories as part of a human attempt to understand nature, but as actually revealing the heretofore hidden essence of nature itself.

> When Galileo said that the book of nature was written in the language of mathematics he meant that the new reductionist, mathematical vocabulary didn't just *happen* to work, but that it worked *because* that was the way things *really were*. He meant that the vocabulary worked because it fitted the universe as a key fits a lock. (Rorty 1981: 570).[51]

Without saying how he could know this, Galileo believed that science unlocked Nature's secrets. Like most Enlightenment scientists, he simply assumed that, because his theories predicted some phenomena, they were therefore uniquely true. Rheticus, a student of Copernicus's, went so far as to **identify** the theory with Nature itself. Said Rheticus: "The hypotheses of my learned teacher correspond so well to the phenomena that they may be mutually interchanged, like a good definition with the thing defined." (Cited in Christopher Lehmann-Haupt's review of *After Thought*, by James Bailey, in *The New York Times*, July 15, 1996, p. C14).

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[51] By "vocabulary" Rorty means "theory." But because "theory" implies the kind of objectivity and neutral description that he is at pains to deny, Rorty prefers a term that doesn't suggest a metaphysical distinction between science and other forms of human activity, such as politics. Generally, Rorty argues for a "Deweyan approach," i.e. one that emphasizes "the utility of narratives and vocabularies rather than the objectivity of laws and theories." (1981: 573).

Galileo's mistake, says Rorty, is to conflate **an** explanation with **the** explanation. Theories may be successful — providing good predictions, say — but this does not mean that they have thereby uncovered nature's essence, nor does it mean, as Rheticus believed, that theory and phenomena are one and the same thing. The Rortian antifoundationalist does not deny that scientists sometimes experience epiphanic understanding when happening upon a good explanation, but she does deny that this feeling is sufficient grounds for supposing that such an explanation is the one, true explanation. (Hacking 1983: 53).[52]

In refuting Galileo, Rorty is careful not to devalue the discoveries of Enlightenment science. He acknowledges that they are the very basis of modern technological civilization. What Rorty challenges is not so much the idea that science is successful, but rather the inference that there is some secret to the success of science, an epistemological moral to be drawn.[53] (Laudan 1990: 167). The antifoundationalist might well agree that Newtonian mechanics, for example, is successful — where "successful" means usefully serving human purposes, like building bridges or sending a machine to the moon and back. She would deny, however, that these achievements require a special epistemic story. Newton's physics is deemed superior to Aristotle's not because it better corresponds to reality, but because it helps human beings to "cope" better. (Rorty 1979: 269).

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[52] The spirit of Galileo is still deeply entrenched in the natural sciences and, I suspect, in economics. The mathematician Eugene Wigner marveled, in 1967, that "the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious." (Rescher 1990: 56). The physicist Stephen Hawking claims that when modern physics discovers the Unified Theory — that is, a grand theory that unites gravity, electromagnetism and the strong and weak nuclear forces — "we [will] know the mind of God." (Hawking 1988: 175)

[53] In other places, Rorty is less careful, and there he is skeptical about the success of science. He argues that the term is question-begging — successful at what? One answer is that modern science is successful at prediction and control of natural phenomena, a rather unobjectionable claim, even for the antifoundationalist. Rorty's rejoinder is: "what's so special about prediction and control?" (See Laudan 1990: 167, and the citations therein). Rorty here objects not to prediction and control as goals of science, but argues science is social and therefore may have multiple goals, and that prediction and control cannot, by themselves, help a society "decide what to do." (Rorty 1981: 575-6).

In believing otherwise, foundationalists, says Rorty, make two related mistakes. The first mistake (Galileo's) is to infer that because scientific theories sometimes work, they therefore reveal nature as it really is. Second, it is also mistaken to conclude that there is some secret to the successes of Enlightenment science, i.e., something unique in its methods that enables its success. Galileo didn't succeed by carefully following some scientific recipe; rather, says Rorty, he simply lucked out.[54] (1981: 571). Had Galileo claimed merely that his theories predicted rather well, he probably would not have run afoul of Cardinal Bellarmino and the Inquisition.[55] (Hausman 1992: 286). What made Galileo press the stronger Book-of-Nature claim? The answer, Rorty suggests, is that he was under the spell of the Lockean metaphor, which he took literally. (Rorty 1979: 143).

Locke (recall from Appendix 2A) proposed that the mind could directly experience nature, without the intermediation of already existing concepts. The Lockean metaphor says: the human mind is a mirror of nature. Knowledge consists of correct representations. In Locke's hands, knowing becomes a quasi-visual faculty; we are to think of knowledge as an assemblage of accurate representations. (Rorty, 1979: 163).

Locke, says Rorty, conflates explanation with justification; that is, he confuses a causal explanation of how a belief is acquired with an explanation of how beliefs are to be justified. (Rorty 1979: 136-48). In describing a physiological process by which beliefs are formed — the world directly impresses itself on the mind — Locke fails to say anything about how beliefs are to be justified. Rorty is rather traditional in insisting that beliefs must be justified. Beliefs are justified by offering reasons, a relationship between persons and propositions, says Rorty, which is quite different from Locke's causal account, which is a relationship between persons and objects. (Rorty 1979: 141-42). If

[54] On the reliability of Galileo's reports of his own science, see note 68.

[55] Cardinal Bellarmino did not dispute the accuracy of Galileo's predictions. Bellarmino's argument was that only the Church could pronounce on the nature of the world.

knowledge is properly seen as justified true belief, then Locke's theory of knowledge cannot succeed, whether or not it accurately depicts sensory processes.[56]

For the Rortian antifoundationalist, scientific knowledge does **not** consist of true representations of an objective reality. Knowledge is not justified by the mirror metaphor. And because Rorty rejects Locke's Mirror, he also rejects the argument that facts can serve as a objective foundation to scientific knowledge.

2.4.2 Some implications of Rorty's antifoundationalism

Generally, antifoundationalists prefer to think of scientific knowledge as what William James called "good in the way of belief" (Rorty 1982: 162). Theories should not be seen as true or false, but as more or less useful to the tasks of inquiry. Says Rorty, theories are "instruments for coping with things, rather than representations of their intrinsic natures." (Rorty 1981: 576). This is a clear rejection of the received view's scientific realism (Hacking's item one)..

Because theorizing is not the business of making better and better representations of reality, it makes more sense to say that scientific inquiry **makes** worlds rather than **discovers** them. (Nelson Goodman 1978). Scientific knowledge, says the radical antifoundationalist, is constructed not discovered — hence the term **constructivist**. This antifoundational idea is unexpectedly endorsed by Joseph Schumpeter, who says: [S]cience is not simply progressive discovery of an objective reality — as is for example, discovery in the basin of the Congo. Rather it is an incessant struggle with the creations of our own and our predecessors' minds . . . ." (1954: 4).

Furthermore, because antifoundationalists in the Rortian vein don't think of knowledge as a neutral depiction of nature as it really is, they tend to emphasize the social determinants of what

[56] In Rorty's (1979) account, the other Enlightenment heavy is played by Descartes. Descartes bifurcated mind and world, and, following his famous aphorism — *cognito ergo sum* — made thinking indubitable. Locke clearly accepts Descartes' distinction between "inner" and "outer space," since his theory of knowledge is an attempt to explain the relationship between the two.

scientists believe — hence the term **social constructivism**. Having challenged the direct connection between thought and nature; antifoundationalists usually turn to the connection between thought and society — both society at large, and particular scientific communities. Rorty argues for the social aspects of science as follows:

> If we [conceive] . . . of knowledge as what we are justified in believing, then we will not imagine that there are enduring constraints on what can count as knowledge, since we will see 'justification' as a social phenomenon, rather than as a transaction between the 'knowing subject' and 'reality'. (Rorty 1979: 9).

For the antifoundationalist, knowledge is produced by human beings situated in a practice and a historical context, not by the disinterested automatons of the received view. Society, no less than nature, affects what scientists think and do.

2.4.3 Kuhn exploded: the sociology of scientific knowledge[57]

Along with Rorty, it is the sociologists who study science and scientific knowledge who have taken the second generation work of Kuhn and pushed it far beyond even his radical revision. The received view of science is only barely visible in their accounts. For simplicity, we emphasize three aspects of the sociology of scientific knowledge (SSK): (1) the idea that science studies should itself be scientific, at least in the sense of being empirical, and (2) the idea the scientific beliefs are determined wholly by social factors, and (3) the idea that scientists can have non-cognitive goals, like fame, prestige, and wealth. The first two notions can be assigned to two different traditions in SSK, the ethnographic school and the "strong program," respectively.

The ethnographers clearly get their inspiration from the naturalistic turn initiated by Kuhn and others. These sociologists forgo the armchair and generally undertake laboratory studies (Latour and Woolgar 1986, Knorr-Cetina 1981). The idea is partly anthropological in spirit — to observe and

[57] This subsection makes extensive use of Wade Hands's (1994) and Uskali Maki's (1992) useful introductions to the sociology of scientific knowledge.

understand scientists in their native habitat — and partly empirical — to determine whether scientists actually do what their methodologies say they should do. The idea is that on-site observation produces evidence superior to that which relies on secondary sources (interviews with scientists outside their daily context) or tertiary sources (published papers). We can, say the ethnographers in the SSK, learn most about science by studying how it actually done, as against studying how scientists say it is done (interviews) or how they report their results in official outlets (scholarly journals).

The second idea I am emphasizing is pervasive in the SSK, though most associated with the "strong program" in SSK. Strong program members and admirers (Barry Barnes, David Bloor, Steven Shapin, Mary Hesse) have rather different views (Laudan 1995, chapter 10), though they tend to believe that scientific knowledge is determined by social factors alone. This is a departure from the older Mertonian tradition in the sociology of science (see, for example, Merton 1973), which, though it conceived of scientists as working in societies, did not investigate the **content** of scientific knowledge, because it did not question the objective nature of scientific beliefs. The Mertonian claim is that science is a social activity (and, by implication, that social science may help to explain its activities). This is (today) a relatively uncontroversial position, though it clearly moves beyond the received view, which ignored any sort of agency on the part of scientists. The strong program, as the name suggests, is not nearly so demure. The sociologists of scientific **knowledge** argue that scientific knowledge is itself socially determined.

Kuhn, recall, argued that science has an unavoidable social element. Nature does not dictate theoretical choices to passive, neutral scientists, because facts must sometimes be interpreted, and therefore cannot serve as a certain foundation to science. Hence, the traditional view — only nature determines scientific belief — is too simplistic. The strong program takes Kuhn's insight to the opposite extreme. It argues that scientific beliefs are determined **solely** by social factors. Theories are so underdetermined by the facts (owing to the Duhem-Quine problem), say the strong programmers, that theory choice is **strictly** a matter of subjective preference and one's ability to negotiate, i.e., to enforce theoretical beliefs on others.

In this hyperbolic view, the data can never work to affect what a scientist believes. Harry Collins says that sociologists of science "must treat the natural world as though it in no way constrains what is believed to be" (cited in Hull 1988: 4), and that "the natural world has a small or non-existent role in the construction of scientific knowledge." (Cited in Laudan 1995: 250). However scientists respond to problems of the Duhem-Quine variety, they will ultimately choose without regard to the evidence.

This is a complete inversion of the received view in the philosophy of science, and a radical departure from Kuhn himself. Kuhn, Hanson and Quine argued, in different ways, that "the facts" cannot be wholly objective and therefore cannot, by themselves, adjudicate among competing theories. But even theory-laden facts do not rule out the prospect of scientific consensus on what the relevant facts are. Scientists unavoidably collect data where the street lamps of theory shine, but communities can still achieve some kind of objectivity by agreement on what the facts are.

The strong program, and many other elements in the SSK, say that the facts are simply irrelevant — in effect, scientists can believe whatever they please, and their beliefs will never be threatened by empirical evidence. It is this kind of thinking that made Kuhn include himself among those who find "the claims of the strong program absurd: an example of deconstruction gone mad." (Kuhn 1992: 9). Kuhn acknowledges that "interest, politics, power and authority undoubtedly do play a significant role in scientific life," but vigorously denies the SSK idea that "power and interest are all there are." (Kuhn 1992: 8).

For the SSK, it is not an external reality that determines what scientists believe, but rather scientists' social interests. Hence, says the strong programmers, when scientists cite reason and evidence as grounds for their beliefs, this is to be seen as mere window dressing, a rhetorical dodge to pretty up the powerful in science. For example, the labor economist who believes that the employment elasticity of the minimum wage is negative, is never persuaded by the data, nor by reason, but, **solely** by political considerations, or by ideology or by the opportunity for personal gain.

The SSK takes the same view of methodological standards. Methodological standards, on this view, have no cognitive function; they work only to provide, *post hoc*, a favorable image of the winners in a competitive struggle for preeminence. Words like "true" or "false" or "rational" don't explain beliefs, they are deployed rhetorically to persuade other scientists to adopt one's beliefs. The strong program thus turns the received view of science on its head. The received view of science says: scientists agree because there are grounds for thinking a claim is valid. Consensus comes from validity. The strong program inverts the causality: it says a claim is valid **because** scientists agree. Says Bloor:

> Instead of defining it as true belief, knowledge for the sociologist is whatever men take to be knowledge . . . . Of course knowledge must be distinguished from mere belief. This can be done by reserving the term "knowledge" for what is collectively endorsed, leaving the individual and idiosyncratic to count as mere belief. (1976: 2-3, cited in Mäki 1992).

Validation of claims consists in consensus.

A final point. The reason why scientists want others to adopt their beliefs is that goods flow to successful persuaders in a scientific context — esteem, fame and wealth. The idea is that scientists, like everybody else, have non-cognitive goals. They are not truth-seekers, **merely** curious about how phenomena work. Scientists are, rather, purposeful pursuers of more ordinary goals.

In sum, there are three key ideas from the SSK that I want to emphasize: (1) meta-science should be scientific (empirical); (2) all scientific beliefs are determined solely by social factors, and (3) scientists can have non-cognitive goals. The alert reader will have noticed that these ideas are in conflict. In particular, (2) seems to rule out (1) — a problem of reflexivity. Why put any credence in the claims of the SSK, if its claims, like all scientific beliefs, are entirely determined socially?

Indeed, there is something contradictory in an enterprise that presents lots of evidence to show that evidence is irrelevant (Laudan cited in Hull 1988: 4). For now, however, we set aside the reflexivity issue. It is time to examine modern antifoundational thinking, in the Rorty/SSK guise we have just reviewed, as it has manifested in economics.

Appendix 2A Foundationalism: demarcation, justification and certainty

The received view in philosophy science did not emerge *ex nihilo* in the twentieth century. The philosophical ideas it contains have a long and distinguished lineage. For lack of a more elegant term, we can categorize the received view as part of the foundationalist program in epistemology. What is foundationalism? Foundationalism is the attempt to found science on a base of certitude, as well as an attempt to determine when beliefs achieve the status of scientific knowledge — demarcation. The notion that scientific knowledge should be certain, and can thereby be demarcated from ordinary opinion, dates to the very beginning of Western thought.

The foundationalist argues that beliefs become scientific only when they have undergone a process of justification — that is, the provision of proof and other evidence. Unjustified claims are condemned to remain mere opinion. A properly justified claim qualifies as scientific knowledge, but foundationalist justification sets the bar rather high — it requires certainty.

Suppose I claim that some proposition C is true because I have proven that it follows from proposition B, which I know to be true. The obvious question — "how do you know B is true" — is successfully answered only by reference to another proposition, "because it follows from A, which is true." An infinite regress is halted only if, "at bottom," there are propositions which are self-evidently true.[58] Since most knowledge is inferential — it builds on other propositions we take to be true — the foundationalist argues that this inferential chain must terminate with knowledge that is beyond doubt. Scientific knowledge requires a foundation of certainty. (Hollis 1994: 67-71, and passim).

In the next few subsections, we unpack these ideas in more detail. First, we take up the epistemologist's conception of justification, in particular the idea that scientific knowledge consists of justified true beliefs (JTBs). Then we distinguish two species of foundationalism — classical

[58] Foundations are therefore propositions that "can be known without proof or evidence," i.e., propositions (1) we know to be true, and that (2) we know self-evidently. (Hollis 1994: 68).

foundationalism, which is rationalist in character, and modern foundationalism, which is empiricist. (Philipse 1994: 25-26). In passing, we say something about Enlightenment science.

Appendix 2A.1 Scientific knowledge as justified true belief[59]

The ancient Greeks were concerned with demarcating scientific knowledge from opinion. They reserved the term *epistêmê* for genuine scientific knowledge, to distinguish it from mere opinion or belief, *doxa*. The Greeks generally thought of *epistêmê* as a belief that is also true and justified. Their definition of scientific knowledge survives today.[60]

Each word matters to the epistemologist: justified, true and belief. Take "belief" first. To illustrate, suppose I say that "the monkey is on the dresser," a proposition we may call P. To count as knowledge, I must actually **believe** that the monkey is on the dresser. If I am lying, for example, then it cannot be said that I know that P, **even** if there happens to be a monkey on the dresser. Logically, one can't be said to know something that she doesn't actually believe.

Of course, belief by itself is not enough to count as knowledge; this is the point of the Greek demarcation between *doxa* and *epistêmê*. Belief is necessary, though not sufficient for knowledge: a belief must also be true, i.e. the monkey must actually be on the dresser. If I find that the dresser is monkeyless, then I cannot claim to know that P. If, instead, I discover that the monkey is indeed on the dresser, then my statement is a true belief. Note that I am using "true" here in its sense of correspondence, i.e. the idea that things in the world (a monkey on the dresser) correspond to

[59] This subsection is indebted to Musgrave (1993a, pp. 2-10), an especially lucid introduction to the history of the theory of knowledge.

[60] Knowledge as justified true belief has a long pedigree indeed. It was first propounded by Plato in the dialogue *Theatetus* (Fuller 1991: 109). More recently, knowledge as JTB has taken some lumps. See, for instance, Gettier 1963.

propositions about the world (a claim that a monkey is on the dresser).[61]

On the traditional definition, a belief that happens to be true is insufficient for scientific knowledge. A third condition for knowledge is also required: that I can **justify** my belief. This last condition obtains in order to avoid the problem of non-demonstrable true beliefs, like lucky guesses. Suppose, for example, I experience a vivid premonition that, at 10:00 p.m., the roulette wheel will come up zero. I believe it. The ball lands on zero, so my belief is true. Can I claim to have **known** that zero would be the outcome, or was it merely happenstance? In order to claim *epistêmê*, I must be able to **justify** my belief in some fashion.

What does it mean to justify a belief? Do I justify my roulette forecast by stating that I saw it in a dream? It depends on how we are to construe "justify." In its weaker sense justify can mean something like "to offer reasons," or "to provide warrant." On this weaker interpretation, a premonition might serve to justify my belief in the roulette wheel's outcome. It is, after all, a reason offered for belief. But epistemologists have long insisted on a far stronger reading of what it means to justify a belief. For epistemologists, to justify is to prove beyond doubt. Justification requires certainty.

Appendix 2A.2 Aristotle's pyramid: justification requires certainty

For the classical philosopher of knowledge, if I cannot have certainty, I cannot claim scientific knowledge. Dreams don't meet this strict test. Genuinely scientific knowledge requires the certainty provided by proof.

By proof, Aristotle meant logical deduction from first principles (premises or axioms) to arrive at conclusions (theorems). To use a shop-worn example, consider the following syllogism:

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[61] This is no small matter for philosophers. Hilary Putnam (1984: 265), for one, refers to the problem "of how words 'hook onto' the world" as "*the* problem for analytical philosophy in the twentieth century." (Emphasis original).

(1) All men are mortal [major premise];
(2) Socrates is a man [minor premise];
Hence, (3) Socrates is mortal [conclusion].

The logic of the reasoning is valid. Hence, if the premises are true, then the conclusion **must** be true.[62] If we are certain that the premises are true, then the knowledge derived from them will also be certain. If the premises are not certain, then all knowledge built thereon will be suspect. This is the Aristotelian method.[63]

The great rationalist exemplar of the Aristotelean method is Euclid.[64] Euclid's *Elements* founded geometry. He built an entire discipline on theorems that derived from a few axioms. Among these axioms were innocuous propositions such as "if equals are added to equals, then equals result," and "any two points lie on a straight line," and "all right angles are equal." (Musgrave 1993a: 178, Davis and Hersh 1980: 218). He proved, for example, the somewhat unintuitive theorem of Pythagoras, by showing it could be deduced from a few axioms. (Musgrave 1993a: 177-78). If one accepted Euclid's axioms as certain, then the theorems **had** to be true, incontrovertibly. Euclid's geometry seemed unshakable.

Indeed, it was the apparent certainty of Euclid's conclusions that drew centuries of admiration. Musgrave recounts the remarkable impression that Euclid had upon Thomas Hobbes, for example.

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[62] Validity is distinct from truth. Valid conclusions need not be true. For example: (1) all philosophers are women; (2) Socrates is a philosopher; hence (3) Socrates is a woman. The conclusion is logically valid but false, due to a false premise.

[63] Aristotle thus conceives of knowledge as a pyramid, with first principles (premises or axioms) at the apex, and conclusions (theorems) at the base. Metaphorically, knowledge flows top-down, and justification flows bottom-up. A theorem can be justified if it is deduced from "higher" premises that are also true, which, in turn, can be justified only if its own premises are true, and so on. The regress is halted by the first principles (axioms) at the apex. Aristotle's first principles are self-evidently true — the foundations. (See Philipse 1994: 12-14).

[64] The discussion of Euclid in indebted to Musgrave 1993a: 177-81 and to Davis and Hersh 1980: 217-223.

who stumbled upon Euclid's proof of Pythagoras' theorem at age 40, declared that geometry was 'the only science that it has hitherto pleased God to bestow upon mankind,' and spent the rest of his life trying to erect a politics (*Leviathan* is the result) as Euclid had founded geometry— on a foundation of certitude (1993a: 177-180). Einstein was no less moved by Euclid. He declared: "Here were assertions . . . which — though by no means evident — could nevertheless be proved with such certainty that doubts appeared to be out of the question. The lucidity and certainty made an indescribable impression upon me." (ibid).[65] The idea of certainty, of indubitability, is what so impressed Hobbes and Einstein, and millions since.

Certainty is also what attracted (ancient and modern) epistemologists to Euclid. The epistemologists saw geometry as more than a strictly formal[66] set of relationships. They also interpreted Euclidean geometry as a true description of objective physical reality. For epistemologists, geometry had a dual aspect: it was a consistent set of logical deductions, and it was also a "true and accurate statement of the world of spatial experience." (Davis and Hersh 1980: 218). Euclid's theorems were not only valid; they were true — a model of *epistêmê.*

The epistemic crux, of course, rests upon the axioms, the foundation of Euclid's entire edifice. The theorems are only as strong as the axioms, and Euclidean geometry describes the real world only

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[65] Today, the prestige of the Euclidean method is such that it still confers a kind of scientific legitimacy. This can be confirmed by a glance at the stylized "scientific paper" format found in the pages of the *American Economic Review* or the *Journal of Political Economy*, for example. Arguments, even mostly non-formal ones, are invariably couched in the form of a proof.

[66] The formalist says that mathematics is just a "combination of meaningless symbols," a self-contained game that does not refer to the real world and is therefore primarily concerned with internal consistency. The formalist approach is characteristic of the Bourbaki school of twentieth century mathematics. The "realist," in contrast, thinks of mathematics as referring to the physical world; mathematical objects are real things that exist independent of the minds that imagine them. The "Platonist" is a realist *in extremis*: she believes that the physical world is actually **comprised** of mathematical forms. (Davis and Hersh 1980: 318-322). Galileo, to pick a prominent example, said that the book of nature is written in mathematics.

if the axioms are certain. Euclid believed that his axioms were self-evidently true, knowable by "the natural light of reason." One could just "see," for example, that all right angles are equal.

It turns out that Euclid (and the generations of epistemologists who made him their exemplar) was wrong. One of his ten axioms proved uncertain, and neither could it be derived from the other nine — 2000 years of subsequent effort notwithstanding.[67] The discovery of non-Euclidean geometries in the mid-nineteenth century demonstrated that alternative descriptions of the world were possible. Euclid's system remained valid — the theorems were properly deduced — but it could no longer serve its other role as a uniquely true account of physical space. Einstein, whose theory of relativity made use of Reimanian geometry, as Newton had used Euclid, (Musgrave 1993a: 234-5) said famously: "As far as the laws of [geometry] refer to reality, they are uncertain, as far as they are certain, they do not refer to reality." (cited in Musgrave 1993a: 237).

It well to remember, however, that the apparent overthrow what Davis and Hersh call the Euclid Myth was two millennia in coming. The appeal of indubitable knowledge has proved to be rather resilient. Indeed, the spirit of Euclid survived the non-Euclidean revolution well into our century, and it lives on. "Even now," say Davis and Hersh, "most educated people believe in the Euclid Myth." (1980: 325). The lure of certainty, the prospect of infallible knowledge, continues to be irresistible, even if Euclid can no longer serve as its exemplar.

The Classical foundationalists, then, thought of scientific knowledge in deductive, logical terms. Scientific knowledge (*epistêmê*) is demarcated from mere opinion (*doxa*) by its techniques of justification, which depend upon the existence of foundations to knowledge — indubitable propositions that are self-evidently true. In Aristotle's representative view, to have science one must

[67] The problematic axiom is usually rendered in an equivalent statement created by the English mathematician John Playfair: given a line L and a point P not on L, there is through P one, and only one, parallel line that can be drawn. (Davis and Hersh 1980: 219).

have certainty, and it is this infallibility that most clearly distinguishes scientific knowledge from opinion. (Laudan 1995: 211).

Appendix: 2A.3 Modern foundationalism: Locke's mirror and certainty

Modern foundationalism is also concerned with demarcating scientific knowledge from other types, but it attends to a somewhat different class of epistemic problem, one thrown up by the advent of experimental science during the Enlightenment. Where Aristotle's conception of science was deductive and essentially rationalist, Enlightenment science was inductive and empirical in spirit. The new science called for testing theories against nature, not merely against reason.

Experiment, in particular, was deemed to be the royal road to knowledge. Francis Bacon, whom Ian Hacking calls the first philosopher of experimental science (Hacking 1983: 246), taught that "not only must we observe nature in the raw, but that we must also 'twist the lion's tail', that is, manipulate the world in order to learn its secrets." (ibid: 149). For the Enlightenment scientists, justifying theories required empirical testing.

Galileo, an Enlightenment paragon, belittled Aristotle's science precisely because it was not experimental. In his famous *Discourses*, Galileo (speaking through characters named Salviati and Sagredo) said:

> I seriously doubt that Aristotle ever tested by experiment whether. . . two stones, one weighing ten times as much as the other, if allowed to fall, at the same instant, from a height, say of 100 cubits (150 feet), would so differ in speed that when the heavier reached the ground, the other would not have fallen more than 10 cubits . . . But I, who have made the test, can assure you that a cannonball weighing 100 or 200 pounds, or even more, will not reach the ground by as much as a span ahead of a musket ball weighing only half a pound . . . . (cited in Clower 1988: 89).

Modern science sought to compare propositions about the world (theories) with evidence from the world, especially experimental data.[68] Newton's laws of mechanics and gravity were considered

[68] Galileo, it is interesting to note, is not the experimentalist paragon he makes himself out to be. In fact, Galileo provides a useful example of the important difference between what scientists say they

scientific not because they were deduced from self-evidently true propositions, but because they more or less comported with existing astronomical (and other physical) observations. (Musgrave 1993a: 213-4).

Despite these differences, Enlightenment science retained the Greek emphasis on certainty. Enlightenment scientists counted their beliefs as scientific for the same reason that Aristotle did: they were certain of their results. The idea that science required certainty was widespread during and after the Enlightenment, says Larry Laudan. "Bacon, Locke, Leibniz, Descartes, Newton and Kant . . . may disagree about how precisely to certify the certainty of knowledge, but none quarrels with the claim that science and infallible knowledge are coterminous." (Laudan 1995: 213). Isaac Newton could not say what gravity was, nor did he know what caused it. But, says Laudan, he nonetheless "regarded his noncausal account as [scientific] because of the avowed certainty of its conclusions." (ibid). Science changed during the Enlightenment, but its philosophy continued to make certainty the badge of demarcation.

Maintaining the certainty criterion created an epistemic problem for Enlightenment philosophers.[69] What can serve as a foundation in an empirical science? The appeal to empirical

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do, on the one hand, and what they actually do, on the other. According to Dudley Shapere: "Galileo . . . did not base his views on experiments, and even when he performed then (which was more rarely and ineffectively than had previously been supposed, he did not draw conclusions from them, but rather used them to illustrate conclusions at which he had already arrived— ignoring, in the process, any deviations therefrom." (1981: 33-4).

[69] It is worth noting that my use of terms like "science" and "philosophy" is decidedly anachronistic. The contemporary specialization which locates the physicist and the philosopher of knowledge in wholly different realms would have been inconceivable to an Enlightenment thinker. Scientists and philosophers were typically one in the same person, like Descartes, who thought of his work in what we now call science, mathematics and philosophy as of a piece, inextricably bound together. (Musgrave 1993a: 194). What we today call a "scientist" was known in the 17th century as a "natural philosopher." The English word "scientist" was not coined until 1834, by William Whewell. (Hull 1988: 37). The term "physics," referring to the united study of mechanics, heat, light, etc. was not generally used in English until the mid-19th century. (Mirowski 1988: 16).

evidence gave new impetus to the age-old problem of the external world. The Aristotelean scientist required reason and logic alone; the Enlightenment scientist also needed facts, experimental and otherwise. The Enlightenment foundationalist is therefore obliged to answer the skeptic's question, which asks: how can one be sure that one's ideas (and other mental events) actually reflect events in the real world? How can Newton, for example, be sure his observations are actual physical phenomena, and not delusions or dreams or misperceptions? Idealist philosophers like Berkeley argued in this skeptical fashion.[70]

Modern foundationalists, John Locke in particular, answered the skeptics by arguing, in effect, that the mind records nature directly. Locke argued that the mind was a *tabula rasa*, waiting for experience to inscribe its slate. Locke said that Nature speaks to us directly; scientific beliefs do not exist prior to facts obtained by experience. The only epistemic authority for the empiricist is sense-experience. (Musgrave 1993a: 60-64). Facts are obtained directly from the real world, by a mind that records it neutrally, like a camera or a mirror.[71]

Objective facts are thus Locke's foundation. The key empirical elements of the received view in the philosophy of science have their direct antecedents in Locke. Locke argues that data from the world are self-evident, i.e., known to all inquirers without proof or evidence. Since the mind neutrally records the world, and there is only one world, any scientist can obtain the facts merely by

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[70] They proposed that only ideas exist, as there is no way to ensure that one's ideas about the external world are actually **caused** by the world. (Musgrave 1993a: 88). Berkeley pushed the idealist logic farthest. He argued that if one cannot show that ideas and perceptions are caused by objects in the world, then one must conclude that an independent (of mind) material reality does not exist. An ordinary skeptic would argue that I cannot say that "the monkey is on the dresser" (proposition P), I can only claim something like "it appears that P." Berkeley concludes that the monkey and the dresser do not exist — all that exists is mental (Hacking 1983: 96).

[71] In fact, the word "information" derives from a Socratic metaphor used in *Theatetus*, whereby the world literally informs itself upon the mind, like a seal makes an impression in (informs) hot wax.

observation.[72] As long as any scientist's mind can record Nature like a mirror, then certainty is obtainable in the form of brute facts.

[72] Locke's schema also requires a kind of realism: the claim that external objects really exist independent of our representations thereof (contra idealists like Berkeley).

Chapter 3. The new methodology: constructivism comes to economics

A generation after the early work of the second-generation philosophers of science, the radical form of antifoundationalism known as constructivism came to economics. We can date its arrival to 1983, when Donald McCloskey invited literary and rhetorical theory to the table of economics.[73] McCloskey (1983, 1985a) and Arjo Klamer (1983), her collaborator, have championed an important and influential view of economics called "rhetorical economics." A second variant of constructivism, also with an English department postmark, arrived a few years later, due to E. Roy Weintraub. (See, for example, Fish 1988 and Weintraub 1989).[74] Along with Philip Mirowski, whose work has affinities with the SSK, Klamer, McCloskey and Weintraub lead what Roger Backhouse terms the constructivist camp in economic methodology (1992).

"Constructivism" is a useful term, because it allows us to distinguish this radically skeptical form of antifoundationalism from other varieties of antifoundationalism (such as those of Karl Popper or C.S. Peirce), which retain a belief that science can accomplish some of its traditional goals. I will refer to the new methodologists as constructivists, in order to differentiate their epistemology from less

[73] McCloskey (1983) was not the first to evaluate economics with the tools of the English department, only the most influential. See, for example, Henderson (1982).

[74] Weintraub now works in the constructivist mode, though he was once a proponent of the Lakatosian approach in economic methodology (Weintraub 1985, for example), a tradition that, while not wholly foundational, is nonetheless at odds with the constructivist approach of the type Weintraub now embraces. Weintraub traces his 'conversion' to a conversation where McCloskey successfully "baited" him, followed by the conference on the Consequences of Economic Rhetoric held at Wellesley College in 1986 (see Klamer et. al. 1988), and an ensuing "reeducation." (Weintraub 1995: 223).

radical forms of antifoundationalism, such as Popper's, which, while agreeing that scientific knowledge is fallible, retain an emphasis on the efficacy of empirical work, and on the prospect for rationality and progress in science.

There are now several economists working within the idiom of literary criticism or with constructivist theory more generally — Amariglio (1988), Lavoie (1990), Warren Samuels (1990b), Rossetti (1990), Milberg (1991), Henderson et. al. (1993), Coşgel (1994), and Burczak (1994) are examples. These theorists make use a host of untraditional sources in meta-economics — linguistics, post-modern literary theory, sociology of scientific knowledge, rhetorical theory, hermeneutics, and feminist theory, for example. Though this is sometime lost in the welter of unfamiliar sources and different jargons, all the new methodologists, and many of their colleagues, are united by a constructivist view of scientific knowledge. The new methodologists all embrace the totalizing critique of science that is characteristic of constructivism.

Because rhetorical economics is the entry point for constructivist thinking into economics, McCloskey and Klamer will be the focus in this chapter. Their important work is presented, critically, in Sections 3.1 through 3.4. Three classes of critics of McCloskey and Klamer are surveyed — the "traditionalists" (Blaug, Rosenberg), the "fellow travelers" (Fish, Weintraub), and the moderates (Hausman and McPherson, Hollis, Solow) in Section 3.5. My survey of McCloskey and Klamer and of their critics emphasizes the importance of standards in economic science. McCloskey and Klamer "respond" in Section 3.6, acknowledging the existence and importance of standards of theory appraisal and of behavioral norms. In Section 3.7, I suggest that their response is sensible but incompatible with their constructivist epistemology.

3.1 The rhetoric of economics: what McCloskey and Klamer are saying

McCloskey and Klamer are tackling questions that lie within the traditional purview of economic methodology: (1) what do economists do; (2) what should economists do, and (3) do economists do what they should, i.e. to what extent do practicing economists actually observe methodological prescriptions? Let us take each of them in turn.

The rhetorical perspective answers the first question — what do economists do — by arguing that economists are engaged in persuasion. What economists do is persuade. McCloskey adopted the label "rhetoric" because, at least in its classical sense, rhetoric is the study of how persuasion works. Rhetoric was, for centuries, a staple of the university curriculum. Rhetoric has languished in modern times, to the point where its contemporary everyday meaning is something like "ornament" or worse, "demagoguery" or "deception." (For an account of the decline of Rhetoric as an independent discipline at Harvard University, see Heinrichs 1995).

Appealing to the more honorable ancient meaning of the term, McCloskey argues that economists are rhetorical creatures no less than everyone else. We attempt to persuade our colleagues, our students, university administrators, policy makers, clients and others, albeit in different ways. Economics is no different from poetry in this regard, nor, for that matter, are politics or even mathematics, which are likewise to be seen as exercises in persuasion.

McCloskey and Klamer document that economists use many rhetorical devices in their work — most notably metaphor and narrative. In trying to persuade our different audiences, economists may also adopt a character, or *ethos*. We may appeal to authority, or resort to other tropes. For example, seeing the complex social process of goods production as a mathematical function, $Y = a(t)f(L,K)$, is metaphorical, says McCloskey. "L" is metonymous because "it reduces the human attentiveness in making bread to an hour of work. The hour is a mere emblem, no more the substance of the matter, than the heart is of emotions, or a bottle is of wine." (McCloskey 1985a: 84).[75]

[75] Note that McCloskey's own example uses argument by analogy, a (intentional?) rhetorical move that makes his point. Metonymy, incidentally, "is a figure in which the name of an attribute or adjunct is substituted for that of the thing meant. 'Buckingham Palace denied the allegations' or 'this department needs some new blood', for example." (Klamer and Leonard 1994: 46). Klamer and

Human capital is another important metaphor in economics, one that suggests seeing human learning as analogous to investment in physical capital. (ibid: 77). Many of our favorite pedagogical devices are also metaphorical. Klamer and Leonard argue that when an economist draws supply and demand curves on the blackboard and calls it "the labor market," she is being metaphorical. We argue that "[t]he diagram is a kind of *icon*, which itself stands in for an elaborate and systematic metaphor on the nature of work in a commercial society." By connecting a diagram with a market, and a market with work, an economist deploys two metaphors. (Klamer and Leonard 1994: 23-24). Even more "rigorous" forms of argument, such as a logical proof, are best seen as rhetorical devices, for they too are designed to persuade. The scientific paper itself is but a genre. The impersonal voice and the scientistic language can be seen as literary devices fashioned to convey the idea of objectivity and rigor.

3.1.1 Rhetoric: persuasion or the study thereof?

Before turning to McCloskey and Klamer on question two in economic methodology — what should economists do — I want to pause for a brief consideration of an ambiguity in the meaning of the term "rhetoric." Aristotle, who is frequently cited by McCloskey, defines rhetoric as "the faculty of observing in any given case the available means of persuasion." (Aristotle 1941: 1329, which is *Rhetoric*, Book I, Chapter 1, 1355b). This suggests that rhetoric is the study of (the art of) persuasion. But "rhetoric" is also used to mean persuasion itself. It is easy but incorrect to run together these two distinct meanings.

Let us maintain the distinction as follows: the "rhetoric of economics" refers to what economists do when they persuade; but "rhetorical economics" is the theoretical program that studies persuasion by economists. The difference is more than semantic. It gets to the issue of consequences:

Leonard (1994) discuss metaphors in economics; a glossary of rhetorical terms is appended. Lanham (1991) is a complete reference for rhetorical terms.

in particular, is the study of economic rhetoric merely revealing (and it is certainly that), or can it serve to improve one's economics? Another way to put this is to ask: does the study of rhetorical practices make one more effectively rhetorically?

McCloskey's position is that the study of economic rhetoric is, indeed, ameliorative — studying persuasion in economics will make one a more persuasive (i.e., a better) economist.[76] The implication is that by making economists' practices intelligible, we can thereby make them better. (Madison 1991: 192-93). A rhetorical analysis can help us see how successful economists persuade their audiences — by appealing to authority, by employing irony, by constructing an appealing narrative, by demonstrating a mastery of key metaphors, and so on.

I agree that analysis of this kind is interesting and valuable for its own sake. An effective rhetorical analysis helps me to better understand how economists persuade and how they come to be persuaded. What I dispute is McCloskey's claim that what I learn is **necessarily** ameliorative, i.e. that it makes me a more persuasive economist. This **could** be so, but only if I can translate my theoretical knowledge of what persuades into the practical knowledge of how to persuade. Just as studying hydrodynamics does not make one a better swimmer, studying economic rhetoric need not make one a more persuasive economist. I venture that many skilled practitioners of persuasion know nothing of rhetorical theories — many persuasive speakers surely didn't know a synecdoche from a simile.

One explanation is that skilled rhetoric is best seen as a kind of practical knowledge or know how, as distinct from the theoretical knowledge ("know that") possessed by the expert theorist of rhetoric. Unless one is prepared to assume that the former is reducible to the latter, the study of economic rhetoric will not be automatically ameliorative. Studying rhetoric will make one more

[76] Klamer is more equivocal on whether a rhetorical economics will make for better economics. He would argue, I believe, a weaker claim: studying economists' persuasive practices yields insights into the problem of communicating what we know.

persuasive only if one can show that the theory of rhetoric is a good substitute for rhetorical practice itself. And, if the theory of rhetoric is a good substitute, we would expect distinguished professors of rhetoric to be among the most persuasive of all persons.

Setting aside the question of whether studying rhetoric improves rhetoric, we consider McCloskey and Klamer's position on question number two in economic methodology.

3.2 Rhetorical economics and question number two: prescription

If what economists do is persuade other economists, then it makes sense to find tools helpful to the task of understanding how they persuade. McCloskey and Klamer propose post-modern literary theory. "Literary criticism does not merely pass judgments of good or bad; in its more recent forms, the question hardly seems to arise. Chiefly it is concerned with making readers see how poets and novelists accomplish their results." (McCloskey 1985a: xix). Note the non-normative emphasis. The idea is to describe economic rhetoric and how it is accomplished, not to appraise it.

McCloskey explicitly disavows any normative intent, as when he quotes Stanley Rosen, for example: "[Rhetoric] is not offered as a new theory of how to philosophize, but as an account of what we actually do." (McCloskey 1985a: 51). And Klamer and McCloskey issue the following demurral:

> "The point [of a rhetorical perspective] was *merely* to note that all economists, mathematical or not, use analogies, appeals to authority, and other rhetorical devices, using them as thoroughly as poets and preachers. . . . (Klamer and McCloskey 1988: 3). [Emphasis added].

That literary criticism might be useful for studying how economists persuade one another is an unconventional but relatively innocuous suggestion. Indeed, in the many critical responses to rhetorical economics (see McCloskey 1994 for a sampling, with responses), I cannot locate any direct objection to this proposal. But rhetorical economics is after bigger game, and its demurrals are unpersuasive.

Rhetorical economics did not engage its methodological critics because it advocated the importation of literary criticism for meta-theoretical work. On the contrary, rhetorical economics engaged methodological critics because it rejected the very idea of methodology, traditionally conceived of as identifying good rules of scientific practice. McCloskey and Klamer answer question number two — what should economists do — by saying that economists should ignore the contemporary rules of good scientific practice identified by methodology. Popper (develop falsifiable theories) and Friedman (develop theories that predict well) are bunk, because falsification is impossible (see the Duhem-Quine problem) and prediction is ruled out by economic theory itself. (On the latter, see McCloskey 1990). Says Klamer: "In the absence of uniform standards and clear-cut tests, economists have to rely on judgments . . . ." (1983: 238).

Traditional standards of theory appraisal fail because they are derived from a defunct theory of knowledge. Received view epistemology is defunct because it has failed to provide economics (or any science) with a method for achieving Truth (with a capital "T"). As constructivists in the Rorty/SSK vein[77], McCloskey and Klamer therefore reject all evaluative standards which they take to be derivative of received-view philosophy of science. In fact, they can read as rejecting any standards of appraisal that retain a connection to the concerns of epistemology. (Hausman 1992: 265). In short, McCloskey and Klamer prescribe doing away with epistemically informed standards. As we will see in Section 3.6, McCloskey and Klamer do not deny that standards of appraisal exist; they argue that such standards should not be connected, as has been traditional, to epistemology. This leads to a terminological distinction (also used by Weintraub) between "method" (small "m"), which is legitimate, and "Method" (capital "M"), which is to be avoided.

One obvious implication is that the role of the economic methodologist should be limited to description. If methodological standards don't promote better economics, then rule-promulgating is,

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[77] "There are," say McCloskey and Klamer, "no timeless claims for or against a particular discursive practice . . . there are no foundations." (Klamer and McCloskey 1988: 17).

at best, an empty exercise, and, at worst, an impediment to good economics. Methodologists, say McCloskey and Klamer, should give up prescription, and confine their meta-theoretical work to question one — describing what economists do (when persuading).

In rejecting the "very idea" of methodological standards, McCloskey and Klamer go beyond the idea that economists are engaged in persuasion. They are making a different and far stronger argument: that economists are engaged **solely** in persuasion. This latter argument is, protestations to the contrary notwithstanding, an epistemic claim. It says that persuasion is not incidental to, nor even in the service of traditional scientific goals such as explanation or understanding; **persuasion is all there is**. Claims are not valid independent of their persuasiveness; it's persuasion "all the way down." (McCloskey 1990: 8).

The irony, noted by several writers, is that McCloskey and Klamer, in advocating the end of methodological prescription, are, well, prescribing. Janet Seiz, among others, has detected this paradox:

> It is one thing to investigate which arguments economists have found persuasive at particular times, and why — to study, as does the Strong Program in the sociology of science, 'how scientists choose what to believe', without judging these choices. It is another matter altogether to offer assessments of how scientists' beliefs 'really are warranted.' Klamer and McCloskey, critics complain, are not being clear about whether they are pursuing only the first task or also the second. (Cited in McCloskey 1994: 196).

This dual purpose is manifest in McCloskey's first book (1985a). The first few chapters are a withering indictment of traditional economic methodology, and its inspiration, the received view in the philosophy of science. McCloskey's conclusion is prescriptive: because economic methodology, as practiced, is impossible, economists should ignore it.

The next three chapters are actual exercises in literary criticism — including a masterful deconstruction of John Muth's famous article on rational expectations. McCloskey's rhetorical analysis of Muth's techniques of persuasion is revealing and enlightening. But Muth may be deconstructed without reference to epistemology, or to foundations, or indeed to any of the traditional concerns of philosophy of science surveyed in Chapter Two. Literary analysis is possible without invoking epistemology.

The difference is between arguing that rhetorical analysis **can be done**, and arguing that rhetorical analysis is **all that can be done.** We can think of these different positions as the weak and strong forms of rhetorical economics, respectively. The former claim is mostly innocuous. The latter claim is far more important, precisely because it embeds the constructivist theory of knowledge. One can, as McCloskey and Klamer do, forswear an interest in things epistemological, but their program still relies on a (constructivist) theory of knowledge.

3.2.1 The impossibility of prescription-free description

The point of this line of reasoning is not merely to indict McCloskey and Klamer for being inconsistent by prescribing less prescription. There is a larger point, namely, that one cannot avoid being prescriptive — saying what should economists do — if one wants to say anything meaningful about what economists actually do. While one might prefer to limit meta-economics to descriptions of what economists do, and eschew prescriptions based on outmoded epistemology, one cannot avoid prescription altogether. As Dan Hausman says, methodology cannot avoid its normative calling. (1992: 318).

Why is prescription unavoidable? The reason is that it is impossible to merely describe while disengaged from one's judgment and appraisal. "Objective" description requires precisely the kind of neutral, view-from-nowhere perspective that McCloskey and Klamer are otherwise at pains to deny. History, to pick an obvious area of description, does not provide objective facts. As Bruce Caldwell notes: "History is theory impregnated . . . [w]hat we include within the category of "history" depends on prior theories of what is deserving of attention, and *that* is influenced by a discipline's prior methodological commitments." (1991: 9). Even Mirowski concedes this point (in a slightly different context), saying: "There is no neutrally objective history of economic thought. There must always be some organizing principles of selection, since no work can adequately summarize all thought even within a narrow range of issues and controversies." (Mirowski 1985: 1). All description is theory-laden.

That history of science should inform one's philosophy of science lay at the heart of the second generation's reaction to the received view's armchair approach to the philosophy of science. The idea cuts two ways, however. Just as a theory of science needs history to be relevant; history without, a theory of science is impossible. (On this, see Blaug 1992: 31). Science is the business of knowledge production, and a theory of knowledge, even if implicit, is unavoidable.

I must reemphasize that the inevitability of prescription is not somehow unique to rhetorical economics. It derives from the fact that any position, even one that denies prescriptive intent, entails some appraisal. My point is only that McCloskey and Klamer do not sidestep the matter by foreswearing an interest in philosophy. Pure description, uninformed by a theoretical position, is not possible. One can sensibly refer to one's position as rhetorical rather than as philosophical, but it remains prescriptive whatever it's called. Rhetorical economics prescribes, as it must if it takes its own judgments seriously.

All description involves appraisal, and all appraisal requires standards. We cannot say an article (a theory, a model, an argument, a methodology) is bad or wrong or inelegant without some developed conception of what is good or right or elegant. Evaluative judgments like these are sensibly made only by reference to standards, whatever their origin. Dan Hausman says persuasively:

> [T]he normative role of methodology is unavoidable; whether methodological rules are garnered from imitation, methodological asides, or systematic methodological treatises, there is no doing economics without some standards or norms. Furthermore, if economics is to make any rational claim to guide policy, these standards or norms cannot be arbitrary. (1989: 123).

Indeed, McCloskey's early statements (1983, 1985a) are enjoyable precisely because they abound with rich, critical judgments and prescriptions for better practice, as informed by a very specific world view, that of the Old Chicago School.[78] When, for example, McCloskey makes the convincing and important case for distinguishing statistical significance from economic significance in econometrics, this is a prescriptive argument. McCloskey appraises an aspect of econometric practice, and finds that is usually falls short of a key standard — to wit, don't confuse statistical significance with economic significance. (1985a: 138-53). Here is evidence that prescription is unavoidable: one doesn't write a survey of econometric practices unless one has already judged that there is a problem worth investigating. McCloskey's excellent descriptions of econometric practice (see also McCloskey and Ziliak 1996) are motivated by and continually informed by a judgment that econometrics has failed to meet some important standards of economic science.

3.3 Rhetorical economics and question number three

McCloskey's first manifesto begins with the following claim: "economists do not follow the laws of enquiry their methodologies lay down. A good thing too." (1983). The point is two-fold. First, McCloskey argues that what economists actually do is quite different from what traditional methodology says they should do. Take the Friedman/Popper view as representative. Do economists make ruthless attempts to falsify their theories by comparing predictions with the evidence? No. In Section 1.3, we saw that at least half of journal articles have no empirical content whatsoever, and, of the remainder, the huge majority are exercises in innocuous falsification. As Vernon Smith says, economists preach falsification, but they are practicing "verificationists to the core." (Smith 1985: 287, ff. 1).

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[78] McCloskey is an avowed Chicago-School economist, at one point indicating sympathy for a variety of Chicago theorizing that Melvin Reder (1982) dubs "tight-prior" equilibrium theory — the idea that markets are nearly always Pareto optimal. (McCloskey 1985a: 9, ff. 2). Some writers, otherwise sympathetic to the antifoundational view, see McCloskey's meta-theoretical stance — don't appraise theories — as an attempt to immunize Chicago-school economics from criticism on methodological grounds. Mirowski is a proponent of this view (1988, chapter 8).

I think that most economists would agree that there is a gap between actual economic practice and methodological rules. Working economists know that no important economic hypothesis ever gets falsified, the Popperian credo notwithstanding. Core theoretical propositions (like utility and profit maximization) are generally seen as part of a Lakatosian hard-core, i.e., as given, and therefore as immune from testing. What distinguishes McCloskey's claim is the second point: it's a good thing that economist don't follow methodological prescriptions. The problem is not with the rule-breaking practice, but with the rules themselves. If everybody is violating the law, goes the argument, perhaps it is the law that it is at fault.

For the methodologist, the interesting question is how to assess the gap between actual economic practice and preaching. The assessment will depends upon one's view of the rules preached. A traditional methodologist like Mark Blaug says that the problem is with economists, not with the methodological rules they flout. His solution: try harder. Economist need to "stop playing tennis with the net down," Blaug maintains. (1980: 256). The rules are simple: create theories with testable empirical implications; discard the refuted theories and keep the others. Economists, Blaug suggests, should obey methodological authority even when history tells us they can't or won't (or both).

McCloskey offers the opposing interpretation: economics is fine as practiced; it is the rules that are wrong. McCloskey's counter-argument says, in effect, know your history. A philosophy of science that doesn't comport with what real scientists actually do, is wrong (or, at least, falsified). It's a **good** thing, McCloskey argues, that economic practice diverges from philosophical preaching. Many useful results in economics would not have been attained had economists dogmatically obeyed methodological rules. Reform the theory, McCloskey argues, not the practice. Or, more precisely, abandon the whole project of promulgating methodological rules that derive from epistemology of any kind.

In discussing McCloskey and Klamer I have thus far argued a minor and a major point. The minor argument partly anticipates the even stronger antifoundationalist position of Stanley Fish and Roy Weintraub. It questions whether studying the rhetoric of economics will necessarily, as McCloskey claims, make for a better economics. I suggest that the study of rhetorical practices in economics, which is important in its own right, may not lead to better economics. It depends on the extent to which one believes that better theory (study of persuasion) is a good substitute for better practice (persuasion).

The more important argument says that, claims to the contrary notwithstanding, rhetorical economics is prescriptive. Indeed, it must be. A constructivist theory of knowledge underwrites rhetorical economics, for better or for worse. As Pascal's *tu quoque* argues: to criticize philosophy is to philosophize. To repeat: this is not an argument that rhetorical economists must do philosophy. It is an argument that description unavoidably entails appraisal, and that (reasoned) appraisal requires standards, whatever their source, and that prescription-free description is impossible.

3.4 McCloskey: the status of methodological rules

McCloskey attacks traditional methodological rules in two related but distinct ways. On the one hand McCloskey argues from philosophy. The traditional philosophy of science is hopelessly reductive — McCloskey calls it the "3 by 5 card view" in economic methodology. Judgment and appraisal are complicated affairs; theory choice is not reducible to an algorithm. Standards of appraisal are "bromidic," sometimes contradict, tend to be implicit, and, most importantly, change over time. (See Section 1.1). More than that, standards are also to seen as **tacit** in some sense, hence, "the reasonable rhetorician cannot write down his rules." (1985a: 52).

On the other hand, McCloskey makes an economic argument, which says that methodological rule-making is akin to government regulation of ordinary markets. On McCloskey's Chicago-School view, regulation, however well intentioned, is likely to be inefficient. By implication, then, the intellectual marketplace of economics will fare better when left to it own devices. How, McCloskey asks, could a bunch of philosopher-lawgivers (qua regulators) have more information than the market participants (economists) themselves? (1985a: 20). An economic view of market regulation suggests that excessive methodological rule making will be bad for the intellectual marketplace of economics.

So far, so good. There can be no doubt that some methodological "regulations" will do more harm than good, i.e., will impede progress in economics. And likewise, it is clear that too much regulation can choke off intellectual innovation. But McCloskey leaps from this economic insight, to which I am sympathetic, to the idea that **all** methodological rules are therefore objectionable. McCloskey says: "any rule-bound methodology is objectionable." (op. cit.).

Economic thinking alone will not sustain this extension. The mistake, I believe, is to assume that all methodological rules in science can be seen as inefficient regulation. This is not so. Ordinary markets, to pursue the analogy, will not function in the absence of some regulatory rules. In particular, markets depend altogether on certain fundamental rules, such as rules that protect persons and property, rules that enforce contracts, and, rules that address other market failures. Laws that protect persons, property and contracts are freedom-restricting, to be sure, but they cannot be seen as inefficient. To the contrary, such rules are better seen as the necessary preconditions for the successful operation of markets themselves. Free markets cannot sustain too much freedom. In this important sense, then, not all rules should be regarded as inefficient.

There are two points here, one economic and one philosophical. The economic point is that not all methodological rules are alike in their nature and function. The analogy between intellectual and ordinary markets is helpful in this regard: it distinguishes fundamental rules (of property and contract), from, say, rules that regulate prices, which may well be inefficient in the sense that McCloskey intends. One can agree that some methodological rules are inefficient, without concluding that all methodological rules must be. On the contrary, some fundamental methodological rules are essential to the operation of the intellectual marketplace, just as they are in ordinary markets.

Secondly, to take the philosophical point, it is important to distinguish the idea of methodological standards from the epistemic provenance of a given methodological standard. McCloskey can condemn particular methodological standards on the grounds that they are based on an epistemology deemed inadequate, but it does not follow that any epistemically-informed standard is therefore suspect. This goes too far. A more pragmatic view is that performance matters more than pedigree.

These points will be taken up at greater length in Chapter Six. In Section 3.5, we review three very different species of critique of McCloskey and Klamer (and of constructivism more generally), finding that, important differences notwithstanding, they all share an interest in whether a constructivist theory of knowledge can accommodate any standing for methodological rules. In Section 3.6 we see that McCloskey and Klamer relax their opposition to methodological rules *per se*, partly in response to the different criticisms we now take up.

3.5 Critical responses to McCloskey and Klamer on standards

I want to characterize the many critical replies to rhetorical economics as taking one of three positions — traditional, sympathetic, and moderate. Traditionalists, like the economist Mark Blaug (1992) and the philosopher Alexander Rosenberg (1988, 1992), reject the constructivist view of science altogether. They worry that the new methodology's hostility to the received view entails abandoning the methodological standards that they take to be characteristic of good science. The traditionalists fear, in particular, that abandonment will result in anarchy (anything goes) and in conventionalism (the ratification of whatever currently goes).

The sympathizers, in contrast — we consider Stanley Fish (1988, 1994) and Roy Weintraub (1989) — embrace constructivism and endorse the main thrust of the new methodologist's critique. Fish and Weintraub, however, read the consequences of that constructivism more radically. They argue that McCloskey and Klamer don't go far enough.

A third group of critics are somewhere between the traditionalists and the sympathizers. The moderates include Daniel Hausman and Michael McPherson (1988), Martin Hollis (1985), and Robert Solow (1988). This group of critics is sympathetic to the new methodological critique of economic methodology, and they share an antifoundational theory of knowledge. They disagree with McCloskey and Klamer's conclusion that antifoundationalism necessarily entails the end of all methodological standards, and the impossibility of rational choice in science. In particular, the moderates want to preserve some epistemic standing for the methodological standards economists use in appraising theories. In short, these critics fear that McCloskey and Klamer are in danger of going too far.

Let us take in turn each set of critical responses to McCloskey and Klamer, with an eye towards the implications for the nature and status of methodological standards in economics.

3.5.1 The traditionalist response: Blaug and Rosenberg

Mark Blaug, we have already seen, is, in his own words, an "unrepentant Popperian," by which he means a proponent of falsification. (Blaug 1994). Economists should, says Blaug, practice falsification. He concedes, however, that economists don't, in practice, take falsification seriously: "my fond belief that economists could be goaded into taking falsification seriously has received some hard knocks over the last ten years." (1992: xxi). But Blaug is unbowed. The solution is still more jawboning: "Try harder," he exhorts. Falsification is a good law, even if economists insist on breaking it at every turn.

Blaug laments the willingness of some economic methodologists to abandon Popperian falsification. He says in this vein: "in refusing to prescribe they end up with economics just as it is . . . . it is not much more than accepting economics as it is, for better or worse."[79] (Blaug 1994: 129).

[79] Blaug's indictment of post-Popperian thinking in economic methodology as promoting the status quo in economics, makes for some strange bedfellows. This is precisely the charge (though for different reasons) that Mirowski levels against McCloskey (1988: chapter 8).

(In "they" he includes not just constructivists like McCloskey and Klamer, but also Wade Hands and Bruce Caldwell, writers in the Popperian tradition who accept many critiques of Popperian ideas, especially of naive falsificationism). When Blaug, in passing, notes that Roy Weintraub has "traveled from great sympathy with Popperianism all the way to . . . antifoundationalism," he seems not to recognize that Popper was an antifoundationalist (though not a constructivist) *par excellence*. (ibid: 109).

This lapse is curious, because Blaug is an astute reader of Popper, and is knowledgeable about the "different" Poppers — the falsificationist, the more tolerant critical rationalist, and the advocate of "situational logic" in social science. (On these different strands in Popper's thought see Caldwell 1991 and Hands 1993, chapter 6). Blaug is also well aware of the important critiques of naive falsification, particularly the Duhem-Quine thesis. (Blaug 1992: 17-21).

My reading is this: Blaug fears that the abandonment of the *de jure* standard in economics, falsification, will mean that economics has thereby lost **any** basis for theory appraisal. Abandoning falsification, however problematic and difficult it is to implement in economics, will lead to anarchy and to **conventionalism** — the simple ratification of whatever standards happen to prevail. For Blaug, the cost of losing falsification (even if it is, he concedes, the *de facto* standard in economics) is the uncritical acceptance of economic practice, in whatever form it may take.

Though Blaug is known as proponent of falsification, increasingly, he can be read as an opponent of what he takes to be a no-standards or whatever-standards-operate approach. Foreswear falsification and you are left with anarchy or conventionalism, Blaug seems to imply. He has taken to endorsing Tom Mayer's (1993a) project of a more empirical, less formal economics, even recognizing Mayer's pragmatic view that good economic practice comes from doing economics, not from a reading of philosophy of science.

For my part, I think that Blaug is half right. He is correct that economics needs methodological standards — no standards would indeed mean anarchy. He is also correct to recognize that self interested economists will obey methodological rules only when it is their interest to do so. (Blaug, should not, therefore, be altogether surprised at the gap between economic preaching and practice). He is wrong, however, to conclude that abandoning his preferred rule, falsification, invariably leads to anarchy or to conventionalism. We know this because economics does not practice falsification, and yet economics is far from anarchic. On the contrary, economics is far more unified on method and appraisal than are other social sciences, for example. Blaug's mistake is to assume that the methodological choice is between falsification on the one hand and chaos on the other.

Alexander Rosenberg describes himself as a positivist manqué (Callebaut 1993: 85), and, indeed, he is something of a throwback to the first-generation received view in philosophy of science. His philosophy of economics is theory-centric and justificationist. He emphasizes the value of empirical testing, asking for "the proper esteem [for] the role of observational testing in the certification of knowledge . . ." (Rosenberg 1988: 131). Rosenberg also accepts the unity of science hypothesis, and says: "I still think there is a distinction between the context of discovery and the context of justification." (Callebaut 1993: 85).

Rosenberg also believes that science is cumulative, in particular, that "a scientific discipline should be expected to show a long-term pattern of improvements in the proportion of correct predictions and their precision." (1992: 18). Applying this last criterion to economics, Rosenberg concludes: economics is not science, and it has not progressed since Adam Smith (1992: 224-7). The "cognitive status" of economics, says Rosenberg, is therefore puzzling (1994).

Rosenberg, like Blaug, wants to preserve some cognitive difference between disciplines like physics, economics, mathematics, literary criticism and astrology. He rejects the new methodologists' egalitarianism — that there are no meaningful epistemic differences between these disciplines — while conceding that it is difficult to identify litmus tests that can accomplish demarcation. (1994: 217).

"I persist in believing," Rosenberg says, "that there are important differences between the cognitive status of these various enterprises. It is just very difficult to establish what those differences are." (ibid).

Rosenberg believes that there are cognitive differences between, say, physics and astrology, while admitting that these differences are not reducible to a single criterion. He therefore rejects Blaug's falsification, saying "falsifiability is unacceptable as a test for the scientific respectability of a theory." (Rosenberg 1994: 217). Nonetheless, Rosenberg is keen to demarcate science from non-science, and he adopts precise prediction as his demarcation criterion. (1992). If economics wants to be a science, he argues, it needs to make predictions that become more accurate over time. Otherwise, economics is non-empirical, best seen as a branch of mathematics.

Rosenberg's rationale for prediction derives from his view of economics as a policy science. Without predictive accuracy, he argues, policy makers cannot sensibly rely on economic advice. Hence, if it is to be a policy science, economics must provide increasingly accurate predictions over time. Rosenberg implies that if economics cannot meet his criterion, then it is worthless.

There are, I argue, two broad problems with Rosenberg's argument. They are: (1) prediction is not sufficient for control of economic phenomena, in part because prediction has special problems in social science, and, (2) economics can be useful to policy making without meeting Rosenberg's demarcation criterion of increasingly accurate predictions. Let us take each objection in turn.

First, even good prediction may not be sufficient for control of economic phenomena, which is Rosenberg's main desiderata and his measure of whether a discipline is progressing. The problem here is that theories can have predictive success without being especially useful to policy making. This situation can arise in two ways, (1) when successful predictions involve spurious correlation, i.e. when there is correlation without causality, and (2) when agents change their behavior in response to theory.

Edward Tufte provides an amusing example of non-causal prediction: the number of radio receivers per 1000 population "explains" almost all the variation ($R^2 = .99$) in the number of registered British mental "defectives."[80] (Tufte 1974). Though the number of radios successfully predicts the number of mental defectives, the inference that policy makers can reduce the number of mental defectives by reducing the number of radios is clearly absurd. Prediction, in this sense, is clearly not sufficient for control. Policy making also requires some understanding of **causal** processes. Without knowledge of causal effects, control of economic phenomena is difficult, and control is the policy goal that Rosenberg makes paramount in his account.

In addition, social science encounters the special problem of agents' changing their behavior in response to policies based upon theories of their behavior. Physicists need not worry that protons will consider changing their behavior in response to physics. Economists, in contrast, must consider this very problem explicitly, especially in a policy context. Ken Binmore puts this special difficulty as follows:

> A theory of human behavior which does not take into account the effect that it would have itself upon human behavior if it became widely accepted cannot, of necessity, become widely accepted without contradicting its own predictions . . . (1990: 13).

When economic theory exploits regularities in human behavior, and then becomes the basis for policy, the very regularities that gave the theory its original success are likely to change. If, for example, the FED successfully devises a way to predict how agents will respond to a decrease in the money stock, and make policy accordingly, there are now incentives for agents to change their behavior in light of the FED's actions. This change in behavior will tend to make the FED model obsolete. (See the Lucas critique). The interaction of theory and its objects (economic agents) creates special difficulties for economics as a policy science.

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[80] Tufte continues: "And the relationship between the number of British mental defectives and the first names of American presidents during 1924 to 1936 does not gain in credibility because the length of the name "explained" 87 percent of the variation in the number of mental defectives. (1974: 90)

This raises the question of whether, particularly at the macro level, accurate prediction of the kind Rosenberg insists upon is attainable in economics — i.e., knowing when, where and by how much changes will occur. It is here that Rosenberg runs afoul of Dan Hausman's extended argument (1992) that economics can never be more than an "inexact" science, capable of short-term and direction-of-change (i.e., signs not coefficients) predictions, but no more. Rosenberg has set the bar too high for economics, because he doesn't recognize the value of an inexact science. Hausman, in contrast, argues that there is social (and scientific) value in an inexact science.

Hausman points to my second objection to Rosenberg's argument, the claim that economics cannot serve a useful social function without increasingly accurate prediction. Rosenberg's argument takes the following form:

> (1) Economics is influential; it affects society;
> (2) Society wants only improving prediction from economics (even if economists don't);
> (3) Therefore, economics can succeed only when it produces improving predictions. (Rosenberg 1992: 51-2).

I have no present quarrel with the major premise, but the whole syllogism falls apart under closer scrutiny. In particular, Rosenberg offers no evidence for his minor premise. Rosenberg may want only increasingly precise prediction from economics, but he has not made the case that this is true for policy makers. It is entirely reasonable to suppose that policy makers want other services from economics as well. Governments can benefit from economic reasoning that doesn't entail increasing precision in Rosenberg's sense.

Economists can, for example, serve a useful social function simply by opposing radically inefficient policy choices. Even with imprecise estimates, economists can sensibly insist (in their roles as "partisan advocates of efficiency" (Charles Schultze)), that $10 billion should not be invested in a project that will produce $1 billion in benefits. In working against the obvious political incentives for free-lunch arguments, economists add social value. Even very rough, order-of-magnitude predictions can provide social value-added, notably when they are included in cost-benefit analyses that would otherwise assume that costs are zero.

One can agree with Rosenberg that policy makers would like to know the precise magnitude as well as the sign of the relevant coefficients, but it does not follow from this that knowing only the sign is worthless. It is useful, for example, for policy makers to know that a strong monetary tightening will tend to reduce output, and that such a move is, therefore, counterproductive in a deep recession. (FED officials did not have this knowledge in the aftermath of the Crash of 1929, and the social costs were very large indeed). Obviously, it would be nice to know exactly by how much output will decline for a given reduction in the money stock (and when), but knowing that it will (probably) decline is valuable knowledge, whether or not it meets Rosenberg's demarcation criterion for science.

Rosenberg's claim that economics can be a policy science only if it meets his definition of science is therefore wrong. His mistake is to conflate two distinct questions: is economics a science, and is economics useful for policy making? If we must use Rosenberg's demarcation criterion, one can answer no and yes. One can accept that economics is not a science, and still believe that is useful for policy makers. One can also agree that economics might provide greater social value were it to achieve the predictive success of physics, but this does not require believing that anything short of this makes economics socially worthless.

As McCloskey and others have noted, there are many successful scientific disciplines, such as evolutionary biology and geology, that also fail the Rosenberg demarcation test. I argue that economics, whatever its predictive shortcomings, can be a useful (albeit limited) policy science. To argue otherwise, Rosenberg would have to demonstrate that the contributions of economics to policy are worthless, and this he does not, and I argue, cannot do.

Rosenberg fears that economics will be unable to function as a policy science, if it does not follow his model of increasingly successful predictions. I disagree: economics can be valuable to policy makers even as an inexact science. Regarding standards, Rosenberg and Blaug are of one mind. Though they endorse different methodological standards — prediction and falsification, respectively — Rosenberg and Blaug both agree that the new economic methodology is corrupting. If their preferred rules don't apply, they assume that methodological anarchy and conventionalism will result.

3.5.2 The radical response: Fish and Weintraub

Stanley Fish and Roy Weintraub are constructivist fellow travelers. Fish says: "I have bought into and am in the process of retailing, selling and pushing the antifoundationalist-rhetorical epistemology." (1988: 27). (By antifoundationalism, Fish means what we are calling constructivism). Fish and Weintraub do not indict rhetorical economics for its constructivism, on the contrary. The Fishian view indicts the rhetorical perspective for missing constructivism's self-denying consequences. In effect, Fish and Weintraub critique Klamer and McCloskey by being more Catholic than the Pope (Fish 1988).

Fish's argument begins with the position is that "theory" is different from "practice." The gist is this: talking about X is different than doing X. (Fuller 1993: 348). We can think of theory as propositional knowledge ("knowledge that") and practice as practical knowledge ("knowledge how"). Theorizing about music composition is different from actually composing music, just as theorizing about economics is different from doing economics, and theorizing about market behavior is different from actually participating in markets.

Fish's second proposition is that this distinction is insurmountable — theorizing about X cannot influence the practice of X. Theory can never "have consequences" for the practice it studies. A musicologist cannot influence music. The art critic cannot influence art. What economists say about markets will have no effect on how markets actually function. Likewise, methodological theorizing should have no effect on the way economics is done. Practical and theoretical are categorically different kinds of knowledge in Fish's scheme.

Fish takes these two arguments — theory is different from practice, and theory cannot affect practice — and says that they apply to theories of knowledge, too. Fish's theory of knowledge is constructivist (he calls it "the Ur text for the rhetorical line") — he believes we cannot get outside ourselves to seize the Archimedean point that enables objective choice among different theories. (1988: 25). But constructivism is also a theory, and like all theories in Fish's scheme, has no consequences. Klamer and McCloskey are guilty of believing otherwise, what Fish calls "antifoundationalist theory hope" (ATH) — "the hope that by becoming aware of the rhetoricity of our foundations we gain a [nonrhetorical] perspective on them that we didn't have before. (1994: 172).[81]

ATH makes the error of thinking that "because we now know that we are in a situation, imbedded, constituted socially, we can use that knowledge to escape the implications of what we now know." (Fish 1988: 27). No such luck, says Fish. Seeing economics as rhetorical may create a kind of critical self-awareness, but self-awareness has no epistemic consequences. As I suggested in Section 3.1.1, self-awareness does not, by itself, compel therapeutic improvement. Says Fish:

> Critical self-consciousness . . . is the idea that you can in some way step back, rise above, get to the side of your beliefs and convictions so that they will have less of a hold on you than they would had you not performed this distancing action, thereby entitling you to survey the field . . . unencumbered by the beliefs and convictions whose hold has been relaxed. (1994: 295).

In short, the problem of knowledge does not lend itself to a kind of "twelve steps" therapy. Admitting alcoholism may be the first step toward sobriety, but embracing constructivism will not thereby free one to ascend to the Archimedean point.

Fish recognizes that this is a difficult conclusion for those who think of their epistemic commitments as superior. He says, in this regard:

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[81] Traditional methodologists experience antifoundationalist theory **fear**, "the fear that if lots of people get persuaded by all that [antifoundational] talk, no one will any longer have standards, norms, procedures, and so forth." (Fish 1988: 29).

> This is hardest of lessons for the cultural and intellectual left, whose members want very much to think that what they take to be their epistemological sophistication . . . makes their reasons different *in kind* from the reasons of those who maintain their faith in objectivity. But this is a flat-out misreading of the lesson antifoundationalism preaches, for . . . even [constructivism cannot] . . . claim an epistemological superiority that would give its proponents an advantage independent of the hard work of presenting evidence, elaborating analogies, marshaling authorities, and so on. (1994: 20).

A constructivist says that all views are rhetorically equal in the sense that none can be shown to be objectively superior. Fish insists that this logic applies to constructivism itself. Hence, there is nothing liberating or therapeutic in adopting a constructivist view of knowledge. As Janet Seiz points out, the constructivist is stuck in a quandary: she believes passionately that her ideas are superior, but she also adopts an epistemology view that denies these ideas any chance of objective superiority. (Klamer 1992: 325). The constructivist can never be right, only more persuasive.

Fish's devilishly subversive position underwrites Roy Weintraub's argument that "methodology doesn't matter but the history of thought might." (Weintraub 1989). Weintraub's point is that no methodology in economics, whether traditional or rhetorical, can affect the practice of economics, for better or for worse. He argues that traditional prescriptive methodology ("Methodology with a capital "M") lacks influence over economics for two reasons. The methodologist Kevin Hoover summarizes them:

> The reason that Methodology cannot succeed in reforming the practices of economists is not only that it does not, indeed cannot occupy a privileged position, but also that it does not belong to the same interpretive community as the economists whose practices it hopes to reform." (Hoover 1994: 291).

Since methodology (theory) cannot affect economics (practice), "a better economics will emerge from economics" alone. Methodological rules, Weintraub says, can come only from within economics itself — appraisal criteria from outside communities (philosophy of science or literary theory) have no force. Call this the inside-outside view, and note its affinity with Kuhn's idea of incommensurability.

Methodological standards, says Weintraub, are irretrievably local. Methodological standards are produced within scientific communities, not imported or imposed from without. What counts as good economics is determined within economics proper. Standards of appraisal can never be imported from other communities, such as economic methodology, or literary criticism. Traditional philosophers like Rosenberg, and even tolerant methodologists like McCloskey and Klamer (*qua* meta-theorists) cannot sensibly propose standards. Their status as outsiders precludes it, so all methodological prescription from "outside" is vacuous — it has no consequences for the practice of economics.

Weintraub concludes that what meta-theorists should do, since prescription is impossible, is intellectual history. Like McCloskey and Klamer, Weintraub would reduce the methodologist's role to question one, the study of what economists do. Weintraub says:

> The issue is what constitutes a good theory, and that is, we have seen, not a matter of comparing the theory to some standard of scientific goodness. We have to ask more complex questions of the theory and its interpretations: How was it developed, how was it presented, what do its terms mean, who was its audience, etc. (ibid: 490).

A prescriptive methodology is not equipped for such inquiry, but history of economic thought may be.

The sympathetic critique of McCloskey and Klamer thus makes two points I wish to emphasize. First, constructivism compels (by embedding) a kind of relativism. A constructivist must accept the subversive implication of her theory of knowledge, which is that her own views cannot be shown to be objectively superior to competing theories. As much as we believe in the validity of our own theoretical positions, that validity does not exist independent of our rhetorical success — persuasion is all we can have. The constructivist economist cannot, therefore, simultaneously be right and unpersuasive. I think that Fish is correct in insisting that constructivism has this self-denying element. He, however, is happy to adopt the sobriquet of "Sophist" (Fish 1994: 291). McCloskey, who wants to argue that a rhetorical perspective can influence economics for the better, cannot be content with sophistry, and therefore should be less content with constructivism.

Second, there is the claim that methodological standards are local (not universal), and that they will not travel, i.e., they cannot apply in more than one scientific community. This is the inside-outside view, which derives from a strong reading of Kuhnian incommensurability. Strong incommensurability is problematic, and I will take issue with it in Chapter Four, arguing that standards may apply generally (if not universally).

3.5.3 The moderate critique: antifoundationalism doesn't preclude standards

The moderate critics of the rhetoric perspective (Hausman and McPherson, Hollis and Solow) all sympathize with McCloskey and Klamer, but fear they go too far, just as Fish insists that they don't go far enough. In all cases, the moderate critics join McCloskey and Klamer in rejecting the received view of science. They are all antifoundational in that they deny the possibility of certain foundations to knowledge. But the moderate critics deny that the end of foundational thinking requires abandoning the methodological enterprise in economics. Take Hausman and McPherson (1988) first.

Hausman and McPherson comment on rhetorical economics by reflecting on the quality and standards of refereeing reports they observed as founding editors of *Economics and Philosophy*. What sorts of methodological standards, they asked, were revealed by their referees' work? Their conclusion is two-fold: first, there seem to exist evaluative criteria of what constitutes good economics (and philosophy). Second, these methodological standards are best characterized as occurring in "an important middle ground between discipline-specific (yet often unsatisfied) methods and nearly platitudinous quasi-ethical constraints on all conversations, and that this middle ground is occupied by something that looks rather like the Methodology McCloskey opposes." (1988: 5).

On their account, standards of appraisal are neither norms of good intellectual conduct ("be civil," "don't sneer," etc.) nor are they discipline-specific procedural methods (how to derive a demand curve, handle inequality constraints, or run a chi-square test). The "middle ground" that Hausman and McPherson envision comprises standards of theory appraisal. These evaluative standards are not, they recognize, reducible to a 3" X 5"card. There are "partly tacit and . . . flexible;" they "evolve in the course of inquiry." Hausman and McPherson's rules of appraisal are not always "narrowly rule-bound in the way that rightly offends McCloskey." (ibid: 6).

In short, Hausman and McPherson argue that (1) standards of appraisal exist, (2) economists (and philosophers) tend to observe them, and (3) the observed standards of appraisal are not rigid, fixed and explicit, but, rather flexible, evolving, and partly implicit. They argue that "what's left when the formulas and rigid rules are gone is the exercise of informed judgment, guided by broad and evolving principles of assessment, which in turn still rest on implicit or explicit epistemological theories." (ibid).

Martin Hollis's response to McCloskey 1985 makes a similar argument. The following claims are made by Hollis:

> 1. You don't have to be post-modern to reject positivism; virtually everyone does;
> 2. Yes, as Quine said, "experience yields no unvarnished news;" we scientists have a choice in the order we impose on it. But the fact of interpretation doesn't do away with the need for theory appraisal; it only makes it harder;
> 3. Antifoundationalism creates the threat of relativism. (1985).

Hollis acknowledges that the received view "will do as an account neither of what economists do nor of what it makes philosophical sense for them to attempt." (1985: 128). He also agrees that facts are unavoidably theory-laden, and that foundational certainty cannot be had. But these concessions from the second generation of philosophy of science, says Hollis, do not, by themselves, entail the end of methodology. Like Hausman and McPherson, Hollis argues that antifoundationalism makes theory choice harder, not impossible. Even if there is no algorithm of theory choice, appraisal is still required, and reasoned choice will necessarily refer to some standards of appraisal.

This is why Hollis is correct to worry that "doing without foundations for knowledge is harder than it is fashionable to suppose, owing to a threat of relativism, which worries even pragmatists." (ibid: 132). By **relativism**, Hollis means the idea that scientists are never in a position to legitimately claim that a given theory is superior to its rivals. Unlike the traditionalists (Blaug and Rosenberg), Hollis does not take the overthrow of foundational epistemology to automatically entail relativism. But if relativism does not automatically follow from antifoundationalism, it remains a risk for the constructivist, who denies that standards of appraisal can apply outside their local community.

Robert Solow's response to McCloskey and Klamer evinces the same concern with methodological standards. (Solow 1988: 31). Solow accepts the idea that one can see economics as discourse, and agrees that economics comprises a far richer variety of argumentation than "syllogism and measurement." Solow likes McCloskey and Klamer when they function as anthropologists, "descriptive scientists" who study Life among the Econ. And McCloskey and Klamer are correct, he says, to criticize economists for preaching Method while practicing something altogether different.

But Solow is less comfortable with the egalitarianism suggested by their metaphor (from Michael Oakeshott) of science as conversation. He argues that "All arguments are equal but some are more equal than others." (ibid: 32). The conversation metaphor is therefore "too permissive," because "some modes of persuasion are more worthy than others." In particular, Solow makes the case for formal representation in economics, because mathematical expression works to avoid arguments that are sloppy, or fraudulent, or obscure. Solow says:

> [L]ogical or mathematical deduction from explicitly stated assumptions is better than reasoning by assertion, allusion, suggestion or rough analogy, mainly because, in the former case, what you see is what you get. It goes without saying that mathematics can be erroneous or misleading or tendentious just as analogy can be irrelevant or misleading or tendentious. I only mean that *ceteris paribus*, to coin a phrase, chains of reasoning ought to be <u>exposed</u> as possible, and mathematics does that. It will not transform bad assumptions into good conclusions . . . but mathematics makes it a little easier to know when you're being had. (It also lends itself to browbeating, I admit). (ibid: 33).

The methodological convention in economics that says "render your arguments deductively in formal notation" is useful, says Solow, because it promotes clarity, and clarity, in turn, promotes honesty, since it is easier to scrutinize mathematical reasoning than other types of reasoning.

I read Solow as arguing for two important methodological standards in economics — (1) be clear and (2) be honest — rather than for mathematical argument *per se*. Solow prefers mathematics not for its own sake, but because he believes it promotes clarity and honesty better than other forms of argument. Mathematical reasoning promotes honesty by lowering the cost of checking work, since, with formal notation, "what you see is what you get."

I agree with Solow that clarity and honesty are two vitally important standards in science. I disagree that mathematical expression is invariably "clearer" or more "checkable" than other forms of expression. Sometimes it will be, and sometimes it won't. If mathematical expression is clarifying in the sense Solow suggests, then it makes sense to employ it. But economists are famous for using heavy mathematical artillery for relatively simple arguments, a method which can often obscure more than clarify. As Solow recognizes, notation-heavy arguments can be costly in readers' time and effort. Fancy math also can be used to dress up or disguise a weak argument, a kind of deceit that undermines rather promotes honesty.

Whether or not formal expression is always the superior means for promoting clarity and honesty (and I think it is not), Solow's larger point remains: methodological standards are not always local. "Be clear" and "be honest" are examples of methodological standards which are likely to apply in many disciplines. It is entirely reasonable, because it is economizing, to prefer theories (arguments, models, papers, monographs) which have greater clarity. Likewise, it is reasonable to prefer theories (arguments, models, papers, monographs) which are more amenable to "checking," or, more generally, to replication. (The nature and function of these standards are taken up in greater detail in Chapter Six).

The moderate critics don't argue for a return to the received view: they do not believe that methodological rules are universal; nor do they believe that they constitute an algorithm for theory choice. They do believe, however, that methodological rules exist, and that the operation of these standards work to keep relativism at bay. Denying a universal or algorithmic method of inquiry does not countenance relativism. It does recognize that the task of choosing better theories (and theorizing about those choices) is far harder than words like algorithm suggest.

In sum, then, the traditional and moderate critics of McCloskey and Klamer, though they come from different traditions, share a common objection: rhetorical economics (constructivism, more generally), in foreswearing methodological standards of any kind, risks anarchy. All agree that some standards of appraisal must be operative if economics is to make reasoned theoretical choices. The difference is that the moderates are less wedded to the efficacy of falsification and prediction as the appropriate standards for economics.

The moderates agree that (1) that standards are not universal, and therefore may vary across communities, (2) that standards can arise internal to a community, (3) that standards may change over time, and (4) that standards are sometimes tacit and hard to unambiguously interpret. The moderates do insist, however, that standards of appraisal exist, that they are underwritten (if only implicitly) by a theory of knowledge, and that, correctly conceived, they can work to keep antifoundationalism from lapsing into relativism, a crucial function. I emphasize the moderate's pragmatic stance on standards: whatever the origin of standards, what matters is their efficacy in promoting the production of knowledge. That fact that one set of standards owes its rationale to an defunct epistemology clearly doesn't imply that **any** set of epistemically-informed rules is therefore objectionable.

Let us now consider McCloskey and Klamer's response on standards in economics.

3.6 Rhetorical economics replies: maybe there <u>are</u> methodological rules

McCloskey and Klamer, to their credit, have taken their critics' arguments on methodological standards seriously, and have amended their earlier views to some extent. Like all good rhetoricians, they anticipate their critic's arguments regarding standards, this in "Economics in the Human Conversation" (1988: 16). The arguments they want to rebut — (1) the Chaos Argument, (2) the Hitler Argument, (3) the Fallacy Argument — are really arguments about methodological rules and the consequences of antifoundationalism.

The Chaos Argument is similar to that of traditionalists like Blaug and Rosenberg. The traditionalist says: "if we abandon all standards and deny the existence of any criterion of truth, anything goes. And everything will." (ibid). The McCloskey and Klamer response is to acknowledge that methodological standards do, in fact, exist. "The conversations of a scholarly community such as that of economics are disciplined," though "by a wider and more difficult set of standards" that derive more from "a practice of speech" than from "a philosophy." (ibid: 17). McCloskey and Klamer recognize that standards are an unavoidable part of any evaluative enterprise.

While recognizing that **some** methodological standards must operate when appraising competing theories, McCloskey and Klamer do not identify any such rules. They do offer a view of where methodological rules come from, and a glimpse of what they look like. In particular, McCloskey and Klamer argue (1) that methodological rules arise internally in economics, which is also Weintraub's position, and (2) that these methodological rules are tacit, i.e. hard to articulate. Their first point is that standards in economics come not from philosophers of science or other "outsiders" but from economists themselves. The second point is that the standards are hard to interpret ("fuzzy"), and hard to articulate ("tacit"). If there is no rule book, then interpretation and articulation can be difficult.

McCloskey has also, from the beginning, emphasized the importance of behavioral norms for intellectual conduct, sometimes characterizing them as "Habermasian." (A reference to Jürgen Habermas, the German philosopher and social scientist). The Habermasian norms of conduct are: "Don't lie; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence in aid of your ideas." (McCloskey 1985a: 24). These rules are meant to apply to all disciplines.

Klamer and McCloskey also propose another kind of moral rule in rebutting what they call the "Hitler" argument. Here they take up the charge of relativism. "According to the rhetoricians, everything is relative, so Hitler would be irrefutable." "No," say Klamer and McCloskey, "he would not; he was no *vir bonus* [good man], however much a *peritus decendi* [persuasive speaker]." (1988: 17). However persuasive Hitler was, he was not a good man, and therefore is refutable. Rhetoric, McCloskey and Klamer argue here, also requires moral purpose, of which (presumably) only good men are capable. Persuasion by itself is necessary but not sufficient for good rhetoric.

The "good person" proviso is added because, otherwise, they would obliged to concede that Hitler himself offered the better argument. Requiring persuaders to be good persons helps to save a rhetorical perspective from the idea that what is persuasive, is best. Klamer and McCloskey cite Plato in support[82]: "Rhetoric is to be used for this one purpose only, of pointing to what is just, and so is every other activity." (ibid: 16). McCloskey and Klamer thus add a substantive ethical requirement for economists: you must be more than persuasive, you must also be a good person.

Let us sum up the McCloskey and Klamer amendments. First, they agree that there are standards of appraisal in economics that enable us to choose better from worse theories, though they do not specify what these standards are. Second, their standards of appraisal are distinguishable from more traditional standards in two respects: origin, and interpretability. McCloskey and Klamer's standards of appraisal originate within economics, not from philosophy of science. And the standards of appraisal which operate in economics are hard to articulate; they are tacit in some sense. Admitting to the existence of criteria for theory appraisal clearly relaxes the earlier claim that "any rule-bound methodology is objectionable." (McCloskey 1985a: 20).

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[82] Appealing to Plato as an authority is a curious choice here. Plato may have detested rhetoric absent moral purpose, but he also would have been actively hostile to Klamer and McCloskey's modern version of rhetoric, which renders meaning and virtue context-dependent. Indeed, as they note elsewhere (Klamer and McCloskey 1989: 156), Plato's universal constructs are the seminal antecedents of the "modernist" program Klamer and McCloskey oppose.

Third, McCloskey and Klamer also relax their position that persuasion is all there is. Persuasion is no longer sufficient for good rhetoric; moral goodness is also required. This implies, presumably, that we must judge economists as well as what they say. Fourth, McCloskey acknowledges behavioral norms in science — the "Habermasian" standards. The requirement that scientists be civil (and tolerant and respectful) is not quite the same as the requirement that they be good. One can easily conceive of a person who behaves civilly without being a truly good person.[83]

Having amended rhetorical economics to admit standards of appraisal, and also behavioral norms, McCloskey and Klamer recover a means to intellectually differentiate their position from truly radical positions (e.g., Feyerabend) which embrace rather than eschew anarchy. Klamer and McCloskey won't go all the way, and sensibly so.

3.7 Can constructivism accommodate McCloskey and Klamer's sensible amendments?

I applaud McCloskey and Klamer for their recognition of the importance of methodological rules in economics, and for their willingness to amend their earlier position in light of criticism. I likewise endorse most their amendments. Unfortunately their constructivist theory of knowledge will not accommodate these sensible changes.

In particular, the moral rules that they have adopted, both the behavioral norms and the good person rule, are incompatible with other commitments entailed by constructivism. The problem I see is twofold and can be posed as follows: (1) can a rhetorical position, which says that standards are always relative to some locale, consistently advocate a universal ethics of inquiry (the Habermasian norms)?; (2) why invoke norms unless you believe that science has purposes worth protecting? I consider the first problem in Section 3.7.1 and the second problem in Section 3.7.2.

3.7.1 Are moral rules universal?

[83] The reverse is also probably true: we can conceive of a good person who is not especially civil or tolerant of other views. Mother Theresa comes to mind.

The first problem with McCloskey and Klamer's amendments concerns the range of application of their moral rules — are the rules universal or local? It seems contradictory for a contextualist to conceive of scientific norms as universal, i.e., applying in all places and at all times. This moral absolutism is at odds with the rhetorical program of opposing a universal method. Again it is Stanley Fish who insists that constructivists live with the implications of constructivism.

Fish says that a constructivist cannot sensibly propose universal principles. He criticizes Richard Rorty on these very grounds, saying "[Rorty begins] by asserting . . . the concomitant unavailability of overarching principles, but then [goes] unaccountably to the proclamation of an overarching principle . . . the principle of ever more tolerant inquirers." (Fish 1994: 218). Moral claims, Fish argues, need a context no less than knowledge claims. The binding force of norms like "be tolerant" depends upon what is at stake, which is the point of the old Hitler argument. Hence, when Rorty proposes the following norm, "prevent the actual and possible humiliation of others," Fish responds:

> 'Avoid cruelty' is a directive that fairly cries out for contextualization and when you put it in a more qualified way — avoid cruelty when you can, or avoid cruelty, all other things being equal, or avoid cruelty except when the alternative seems worse — it is even clearer that its force depends upon how everything is filled in, on what is already felt to be at stake in the situation. (Fish 1994: 217-8).

Fish insists that constructivists cannot be in the business of promulgating universal rules. This does not argue against the prospect of widely applicable norms *per se*, it only means that constructivists (like McCloskey and Klamer) cannot consistently make such claims.

A universal ethics may well be good for scientific inquiry. I think it is reasonable to suppose that **some** norms will indeed be useful in a number of different scientific communities, and will discuss such norms in Chapters Six and Seven. And, as a practical matter, it seems that many scientific communities do, in fact, endorse norms that resemble the Habermasian norms. But McCloskey and Klamer also endorse the idea of incommensurable discourse communities, which rules out the prospect of norms (or any other methodological rules) applying outside a given community, never mind applying more generally.

The conflict is this: Habermasian norms are incompatible with incommensurability. To the extent that McCloskey and Klamer mean for their norms to apply generally, this directly contradicts their view that standards are relative to specific communities and cannot travel. If, instead, they mean for their Habermasian norms to apply only locally, then the very motivation for a *sprachethik* is lost. The Habermasian norms cannot serve their civilizing function if they vary too much from place to place.

3.7.2 Advocating moral rules presupposes (the possibility of) scientific progress

The second problem with McCloskey and Klamer's moral rules can be put as follows: why bother with scientific norms (or any methodological rules) unless you believe that science has purposes worth promoting? Is "be civil" invoked to promote civility for its own sake, or does civility work to serve a scientific goal? The answer matters. For if science doesn't accomplish any goals save self-perpetuation, as McCloskey and Klamer sometimes seem to suggest in following Rorty, then it is hardly necessary to protect science with norms. Without some notion of what science can accomplish, an argument for norms is puzzling.

The problem for McCloskey and Klamer is that there is no reason to tolerate (never mind respect) other views, unless one believes that they might be superior to one's own. It is not a question of intellectual humility alone, but rather a belief that there is some non-arbitrary way of choosing among rival theories. In this sense, tolerance, civility and other Habermasian norms[84] are parasitic on our ability (however imperfect) to compare and appraise theoretical alternatives. If scientists cannot

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[84] Richard Rorty's equivalent for McCloskey's Habermasian norms is the "Socratic virtues." (1982: 169). McCloskey cites Rorty as the principal source for his arguments on the norms of science. (1985a: 25, ff3).

sensibly compare and appraise theories, then science cannot progress (except by accident), and promulgating moral rules seems pointless.

So, why advocate norms to protect science, if you believe that science accomplishes nothing worth protecting? Rorty's answer says that even though science has no purpose — "we do not know what success would mean except simply continuance" — it is worth protecting science **for it's own sake**: "Socratic conversation is an activity which is its own end." (1982: 172). For Rorty, science doesn't progress; it simply proceeds. Hence, promoting behavioral norms doesn't require that one believe in scientific progress: "Loyalty to our fellow humans [need not presuppose] that there is something permanent and unhistorical which explains why we should continue to converse in the manner of Socrates, something which guarantees convergence to agreement." (Rorty 1982: 171).

Rorty's error is to assume that progress is undefinable without recourse to permanent, universal goals of science. Behavioral norms and other methodological rules are designed with scientific goals in mind, but there is no reason why these goals must be permanent and invariant. Goals in science may well change over time, just as they may vary across communities. But, the longevity and generality of scientific goals are empirical questions. Progress **can** be defined over periods in which goals are relatively stable. Only if Rorty is prepared to argue (and he does not) that scientific goals are hopelessly ephemeral over time can his thesis be maintained. The point is that goals need not be universal (nor permanent) in order to produce valuable knowledge here and now, or over a generation or two. The argument for a standard's usefulness lies in its ability to meet a given goal (e.g., producing reliable knowledge), not in its pedigree or its universality.[85]

Rorty also errs when implying that scientific progress can be defined only with reference to received-view, "mirror" epistemology. As Larry Laudan has argued at length, there is no reason that

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[85] This is not to suggest, however, that widely-employed rules of inquiry are not more efficacious. Widespread use may indeed be evidence that a set of rules is better at producing knowledge.

progress must be yoked to Truth (with a capital T). Scientific progress can also be defined in more pragmatic terms, such as success at solving problems. We can think of progress, for example, as occurring when science allows us to treat more disease, or to alleviate more poverty, or to raise living standards.

Laudan's pragmatic definition of scientific progress says: "the aim of science is to secure theories with a high problem-solving effectiveness." (Laudan 1995: 78). As such, "*science progresses* [when] . . . *successive theories solve more problems than their predecessors*" [emphasis original] (ibid). Without denying that a pragmatic definition of progress creates its own difficulties, let us recognize that scientific progress does not require received-view "mirror" epistemology.

One can certainly argue that science, for its own sake, is worthwhile. But to argue that science is **only** for its own sake is too narrow: (1) it denies that science accomplishes certain goals worth achieving, and, in so doing, (2) it makes it hard to understand the rationale for behavioral norms and other methodological rules in science. Behavioral norms in science are important precisely because they work to promote specific scientific goals.

To anticipate Chapter Six, consider, for example, the scientific goal of producing reliable knowledge. Knowledge becomes more reliable the more it is tested, hence, reliability is promoted by theories that are easily replicable. A norm like "be clear" promotes replication because clarity lowers the cost of replication. (Recall Solow on this point). Clarity is not its own end, it is an instrument for promoting the goal of replication (among other goals), which, in turn, promotes reliability.

Note that it may not be in the individual scientist's interest to be clear, both because clear writing is difficult, and because obscurity can help to camouflage a weak argument. To the extent that the norm can actually be enforced (an open question), perhaps by editors and referees, it works to promote a specific scientific goal that is not in the individual scientist's self interest. A norm like "be clear" does not merely promote clarity for its own sake, it promotes a specific goal of science that we take to be worthwhile.

To summarize the two objections of Sections 4.7.1 and 4.7.2: (1) universal norms (i.e., the Habermasian variety) cannot be accommodated by constructivism, which is wedded to incommensurability; and (2) the very idea of norms is incompatible with the view that science does not have substantive goals worth promoting. Hence to the extent that the new methodologist's embrace incommensurability and deny any substantive goals in science, its promulgation of norms is puzzling and contradictory. Consistency requires choosing.

Since I agree that norms and other methodological institutions **are** vitally important in science, I argue for rejecting (1) incommensurability (at least in its strong form) and (2) the idea that science has no substantive goals. This enables an explanation of the nature, status and function of methodological rules in economics, without seriously threatening the more fundamental elements of the new methodological critique.

3.7.3 Enforcing methodological rules

In Sections 3.7.1 and 3.7.2, I have argued that norms of inquiry set forth by McCloskey and Klamer are sensible, but incompatible with their constructivist theory of knowledge. Without criticizing McCloskey and Klamer in particular, I want to briefly note a third objection, which will be taken up in more detail in Chapters Five and Six. The objection is practical, not substantive: even if norms (and other methodological institutions) are good for science, is it reasonable to expect that scientists will observe them? Collective goals often require behavior that is not in the individual's best interest. If, as McCloskey and Klamer argue, economics needs economists who are civil (the Habermasian norms) and good (the "good person" rule), what incentives ensure that economists will actually comply? Specifying norms of good intellectual conduct is one thing; expecting that economists will observe them is quite another.

I must emphasize that the issue of enforcement is by no means unique to the rules proposed by McCloskey and Klamer. As we saw in Section 3.3.6, the effectiveness of **any** methodological rule will depend upon whether it is enforceable. Whether or not a given rule **should** be observed by scientists, there is the question of whether it will be.

My point is that a more sophisticated view of scientific motivation, such as that proposed by the new methodologists, requires a more sophisticated view of methodological rules. Once we conceive of scientists as agents just like anyone else, it is clear that the individual scientist's interests can conflict with the goals of her science, and that the right incentives are required to ensure collectively beneficial outcomes. These themes will be taken up in Chapters Five and Six. In Chapter Four, we complete our critique of the new economic methodology, pro and con. I argue that a few epistemic amendments transform the new methodology into an economic rather than a sociological (or rhetorical) theory of science, and that we thereby acquire, among other things, a more robust theoretical explanation of the existence and function of institutions in science.

Chapter 4. The new methodology and constructivism: pro and con

> [S]*cience is fallible because science is human.* But . . . it does not follow that the choice between theories is arbitrary or non-rational: . . . that our knowledge cannot grow. (Karl Popper, cited in Sah 1991: 81).

In Chapter Three I introduced rhetorical economics as an exemplar of constructivist epistemology in economics. I presented a critique and those of a variety of interlocutors, with a focus on standards of theory appraisal and behavioral norms in economics. What separates McCloskey and Klamer (and other new methodologists) from their critics, sympathetic and otherwise, are some small but decisive commitments required by the constructivist theory of scientific knowledge. It is these constructivist commitments that create the divergent views identified in Chapter Three, particularly those concerning standards of theory appraisal and other rules in science. And it is these commitments that I will explore and critique in this chapter.

The commitments that I take to be decisive derive from an idiosyncratic reading of the consequences of an antifoundational view of scientific knowledge. The new methodologists, among constructivists more generally, go beyond a skepticism regarding the prospects for certain knowledge (a skepticism we share), to two varieties of relativism: (1) epistemic relativism (also known as non-rationalism), which is a skepticism regarding the possibility of reasoned choice by scientists and (2) and cultural relativism, the idea that scientific beliefs and standards must be relative to different cultures (or communities), and that, therefore, comparison of theories or standards across communities is impossible.

Constructivists argue, in effect, that antifoundationalism somehow entails rejecting the possibility of reasoned choice in science (epistemic relativism or non-rationalism), and also precludes the possibility of theoretical and methodological trade across community boundaries (cultural relativism). I disagree on both counts, and will present my case in Sections 4.5 to 4.10.

Let me begin, however, where the new methodologists (and constructivists more generally) and I concur. Our area of agreement is vast, the critical tenure of the discussion notwithstanding. We all agree, for example, that: (1) there are no certain foundations to scientific knowledge (antifoundationalism); (2) like any other social process, science is amenable to scientific study; (3) scientists are not the selfless, truth-seeking paragons of the received view; and (4) there are no guarantees that methods which work well in the natural sciences will necessarily prove useful to the social sciences (contra the Unity of Science hypothesis). I discuss these areas of agreement in Sections 4.1 to 4.4.

4.1 Fallible knowledge: there are no certain foundations

The new methodologists and I agree that foundations to knowledge, traditionally conceived of as providing certain justification for theories, are impossible. This is so whether we think of certain foundations as self-evident first principles (the rationalist approach) or as objective brute facts (the empiricist approach). For an empirical science, the facts do not exist independently of the theories meant to explain them, and therefore facts cannot serve as a perfectly neutral court in which to try rival theories. Scientists may come to agree on which data shall constitute "the facts," which will provides some measure of objectivity, but facts cannot serve in their foundationalist role as a independent guarantor of certainty.

The ancient epistemological desire for certainty in human knowledge remains unrequited. McCloskey, who echoes John Dewey, is correct — the 2500 year quest for certainty has failed. The failure, says philosopher David Hull, comes from asking too much of human knowledge:

> [N]either the content nor the methods of science can be "justified" in the sense that

> generations of epistemologists have attempted to justify them. The reason that epistemologists have not been able to justify knowledge-claims in their sense of "justify" is that no such justification exists. They want the impossible. (1988: 12-13).

The philosopher Hilary Putnam characterizes epistemology's long quest for certainty in even stronger terms. He says: "The enterprises of providing a foundation for . . . Knowledge — a successful description of the Canons of Justification — are enterprises that have disastrously failed." (cited in McCloskey 1995: 1322). To insist that knowledge requires certainty is to require the impossible, because certainty is unattainable. All human knowledge is fallible. The new methodologists and I have no differences here — we are all antifoundationalists in this sense.

4.2 Science is amenable to scientific study

The new methodologists and I also agree that science (or scientific field, like economics) is amenable to study by other scientists. Science is an important aspect of modern societies, and this very centrality makes it important as an object of scholarly study. That science itself could be studied — the naturalistic turn initiated by Kuhn — remains a novel and still somewhat controversial idea. Robert Merton, a somewhat lonely pioneer in science studies, was able to write in 1957: "When the Trevelyans of 2050 come to write that history [of sociology] . . . they will doubtless find it strange that so few sociologists (and historians) of the twentieth century could bring themselves, in their work, to treat science as one of the great social institutions of the time." (1973: 286).

Given the recent explosion of work in science studies, a Merton might well revise his view, but there is still, I would argue, a lingering sense that science studies are somehow inherently misguided. This is nonsense. Science is not only too important to be left unexamined; there are real gains to be had in understanding how it functions. So, while I disagree with many of the interpretations of science offered by students of science, I do not disagree with the naturalizing spirit of the enterprise.[86] Science is a social process, and social scientists can have useful things to say about it.

4.3 Scientists are people too: self-interested and epistemically-sullied[87]

Perhaps the most fantastic of all the formulations of science offered by the received view is its depiction of scientists as selfless truth seekers. Scientists themselves were never this naive about their work and motivation, and this reveals how unscientific was the received-view depiction. The sociology of science (especially Merton) and the ethnographically-oriented studies of laboratory life have done much to debunk the largely mythical view of scientists. Recall the scientist as depicted in the received view:

> (1) The scientist is an asocial monad. The received view conceives of "knowledge acquisition in terms of an individual subject who confronts the objects of knowledge in total isolation from other subjects" (Hull 1988: 13). What scientists believe is guided only by direct inputs from nature; they are not influenced by anything but the data, and are immune to ideology, personal biases, and larger social factors.
>
> (2) Scientists have only cognitive goals, i.e., scientists are truth-seekers interested in knowledge alone. They are wholly unaffected by the non-cognitive goals that we readily ascribe to all other human beings — collegial esteem, wealth, security, approbation, fame, prestige, and revenge on enemies. (Kitcher 1993: 73).
>
> (3) Scientists are wholly unselfish. Not only is the scientist concerned only with cognitive goals, she is indifferent whether she or someone else publishes a discovery first. If collective knowledge is advanced, the individual scientist (or scientific team) does not care who gets priority and credit.

These myths are remarkably enduring, even in economics, where we otherwise endorse a rather hard-headed view of human motivation. Under the slightest scrutiny, however, the myths dissipate.

Take item one first. Strictly speaking, even the received view didn't believe that scientists actually worked in glorious isolation. This 19th-century view of the scientist as lonely genius was

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[86] I reject, in particular, the SSK's claim that, because science is social, sociology is the only appropriate meta-theoretical discipline. This formulation conveniently leaves out all other social science, especially economics.

[87] The term "epistemically sullied" refers to the idea that scientists have non-cognitive goals, and is due to Philip Kitcher 1993.

unavoidably disturbed by the advent of Big Science during and after World War II. Nonetheless, this aspect of the naive view of scientific motivation persisted. If one could no longer claim that scientists actually worked in isolation, the received view implicitly claimed that scientists were immune to the unambiguously social aspects of their work — that scientists could **function** as isolated, disinterested interlocutors of nature. The received view implicitly claims that the scientist can somehow remain above "external" factors — heroically able to get beyond her politics, her personal biases and the influence of larger social forces.

Next consider item three. No economist can be comfortable with the idea that agents are entirely heedless of their own self-interest. Say, for example, that two scientists, X and Y, are racing to isolate a gene that determines the production of leptin, a protein that regulates the production of fat cells. Isolating the gene will permit the control of leptin, which, in turn, permits the control of body fat. If Y beats X, then, according to received view, X is delighted. Because her only goal is to acquire knowledge, once Y has provided it, she selflessly accepts the result.

If this outcome seems unlikely on the face of it, it is. As Philip Kitcher puts it: "A [scientist's] . . . goal is not just to reach a particular cognitive state, but to arrive at it in a particular way — either as a results of his own efforts or though the work of a team to which he belongs" (1993: 72). The point is that even the scientist who has purely cognitive goals, can also be self-interested. I might care only about knowledge while preferring to acquire it through my own effort.

A desire for esteem is but one of many possible non-cognitive goals for scientists. Often it is an even less flattering aim that drives scientists. David Hull's (1988) detailed study of biologists in action finds that scientists are frequently motivated by revenge — a desire to "get that son a bitch." Says Hull: "Time and again, the scientists whom I have been studying have told stories of confrontation with other scientists that roused them from routine work to massive effort. No matter what the cost, they were going to get even." (1988: 160). Science is not the place of quiet contemplation and fraternal cooperation that lives in the popular imagination. Hull argues that "the language of the battlefield is as appropriate for science as is the language of camaraderie and cooperation." (ibid: 160-1).

The new methodologists and I agree that scientists are people, too. We reject the received view's account of scientists as selfless agents with only the advancement of knowledge in mind. We agree that scientists are self-interested and are also epistemically-sullied. Let us briefly pursue this economic view of scientific motivation in some more detail.

4.3.1 The nature of the scientific motivation

It is important to realize that making scientists into real people in no way rules out genuine intellectual curiosity, or socially motivated goals. It only rules out these noble motives as the **exclusive** determinants of scientific behavior. Scientists' may care intensely about their reputations, or their wealth, or their prizes, or other non-cognitive matters, and still be concerned with knowledge for its own sake. To see this, consider the Figure 4.1, which is derived from an argument in Kitcher (1993: 72-3).

Figure 4.1 The Nature of the Individual Scientist's Motivation

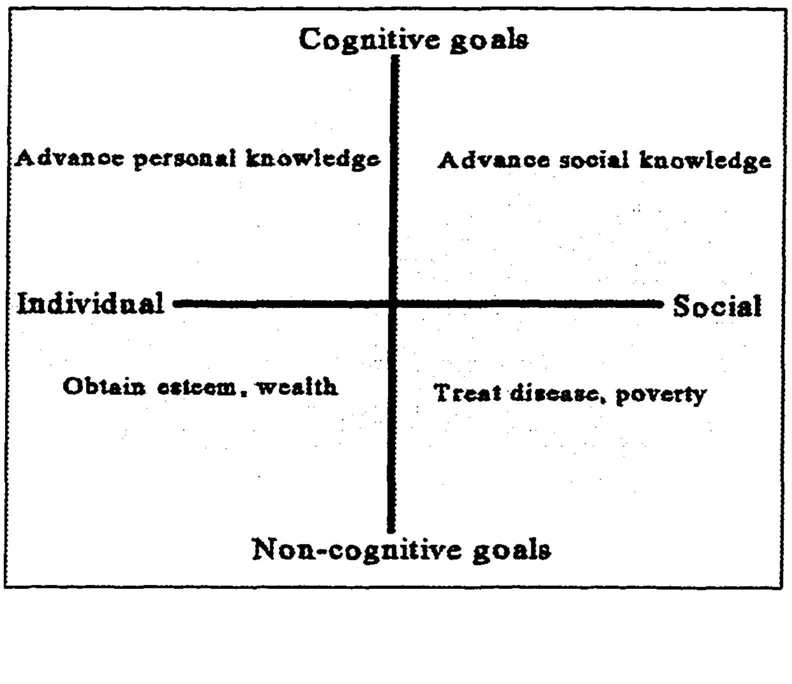

Figure 4.1 puts the nature of a scientific goal (cognitive versus non-cognitive) on the vertical axis and it orientation (individual versus collective interests) on the horizontal axis, yielding the four-fold grid.

The received view, for example, locates scientific motivation in the northeast quadrant — the scientist is selfless, i.e. interested in advancing collective goals, and has cognitive goals only. Some popular depictions of scientists makes them out to be selfless, which admits non-cognitive goals, but only those that involve the betterment of society — this comprises the two eastern quadrants. The new methodologists, among post-modern views more generally, would probably locate the scientist in the southwestern quadrant. This result obtains because they deny any prospect for realizing scientific knowledge. Emphasizing non-cognitive goals, they also tend to think of scientists as self interested. Economists, who traditionally make self-interest a central feature of human motivation, will locate scientists as agents in the western half of the diagram, with further refinements depending upon their epistemic views.

It is important to emphasize, as Kitcher notes, that these different goals need not be mutually exclusive. (1993: 72-3). The scientist's motivation is as complex as any human being's. The physician who immunizes Sudanese children may also be possessed of a desire for fame or recognition. A Wall Street economist who seeks wealth may also want to genuinely understand, for its own sake, how markets for financial derivatives work. In private correspondence, McCloskey offered an elegant characterization of the variety of factors that motivate an economic scientist:

> A particular economist will be motivated by a dream of immortality, a desire for a quiet or exciting life, anger at this or that doctrine, a need to be as good as Harry X, a genuine puzzlement (this is rare) about some part of the economy, a childish enthusiasm for big machines, a pride in Being There, a cultural ambition.

All of us agree that scientists, as human agents, are more interesting than the selfless truth seekers of the received view.

In addition, we should be careful to note that this is a discussion of individual motivation.

Collective outcomes are an entirely different matter, and we should not assume that what is individually rational will coincide with what is collectively rational. In particular, it is possible that entirely self-interested scientists with mostly non-cognitive goals can produce, in the aggregate, reliable scientific knowledge. (See Goldman and Shaked 1991). This will occur when there are invisible-hand processes in science, analogous to those in competitive markets.

4.4 The difficulty of a unity of method

I also think that the new methodologists are correct when they question the unity of science hypothesis of the received view. There is no obvious reason why the research methods successful in physics, for example, will be successful in economics, or in sociology. And there are good reasons why they may not be. The success of the physical sciences clearly invites emulation. Imitation is often a good strategy, and this surely was what motivated Walras, Pareto, and Edgeworth in their attempts to erect a social physics. But "success" is defined only with reference to disciplinary goals, such as good explanation, or accurate prediction, or theoretical elegance. To the extent that other disciplines have (or are ontologically obliged to have) different goals, one cannot assume that the methods from physics will be as efficient in realizing those different goals.

Given that different disciplines may have different goals and that the phenomena under study (human consumers, plants, protons) can be ontologically quite diverse, we should expect some variation in methodological rules across disciplines. In biology, as physicist and theorist of science John Ziman points out, "a good sketch diagram may be no less valid epistemologically, and much more comprehensible, than a whole book of equations. There is no general philosophical demonstration that the final, most mature state of all science must be similar to theoretical physics in this respect." (1984: 50). Ziman's point is that we cannot assume that economics or any other science **must** be constituted as physics.

Those who say that economics is simply awaiting its Newton, forget that modern physics and modem economics both came into being at about the same time. (Hayek 1974). It could well be that economics is less well developed than physics because it concerns vastly more complex phenomena. Seen this way, economics, as Paul Krugman suggests, is simply harder than physics (1994: xi), and, by the same token, sociology is harder than economics.[88]

The unity of science approach in effect denies that diversity of goals and differences in tractability, can lead to alternative institutional structures. It appeals, instead, to a kind of reductionism that wants to explain higher-order processes in terms of more basic elements: biological processes are reducible to chemistry, which is reducible to physics, which will one day reduce its constituent parts to a grand Unified Theory of everything. A universal set of rules — the Scientific Method — will follow only if one is prepared to endorse reductionism on this scale. The new methodologists and I are in agreement that there is no necessary unity of science.

Still, let us not leap to the opposite conclusion that there is nothing in physics or other disciplines worth borrowing. Imitation is sometimes a good intellectual strategy. Intellectual autarky is generally a poor disciplinary strategy. To argue that different disciplines can have different goals is not to argue that they must, nor is it to argue that they (necessarily) should. Diversity of goals and differences in the tractability of phenomena can help to explain why there are different methodological institutions in different fields, but they do not justify that diversity, nor do they, by themselves require it.

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[88] Keynes's anecdote on Max Planck is relevant here. "Professor Planck, of Berlin, the famous originator of the Quantum Theory, once remarked to me that in his early life he had thought of studying economics, but had found it too difficult! Professor Planck could easily master the whole corpus of mathematical economics in a few days. He did not mean that! But the amalgam of logic and intuition and the wide knowledge of facts, most of which are not precise, which is required for economic interpretation in its highest form is quite truly, overwhelmingly difficult for those whose gift mainly consists in the power to imagine and pursue to their furthest points the implications and prior conditions of comparatively simple facts which are known with a high degree of precision." (Keynes 1963: 158, ff 1).

In sum, then, I agree with new methodologists (1) that knowledge is fallible, that (2) that science is amenable to scientific study, (3) that scientists, like everybody else, are self-interested and can have non-cognitive goals, and (4) that there is no necessary reason why all sciences (human and natural) should proceed on the model of physics. Now we turn to the areas of disagreement.

4.5 Where we disagree: The consequences of antifoundationalism

I argue that the new economic methodology goes astray when it embraces the radical theory of knowledge which we have been calling constructivism. McCloskey's attempt to introduce moral rules into science, for example, founders not because such moves are wrong-headed *per se.* They are not; norms are crucial to science. Rather, as I tried to show in Section 3.7, norms are incompatible with constructivism. It is constructivism that makes mischief for rhetorical economics and related new methodological programs — and the problem is epistemological.

The crux of the problem is this: constructivism goes beyond a skepticism regarding the prospects for certain knowledge, to two varieties of relativism: (1) epistemic relativism (also known as non-rationalism), which is a skepticism regarding the possibility of reasoned choice by scientists and (2) and cultural relativism, the idea that scientific theories and standards **must** be relative to different cultures (or communities), and that, therefore, comparison of theories or standards across communities is impossible.

Constructivists argue, in effect, that antifoundationalism somehow entails rejecting the possibility of reasoned choice in science (epistemic relativism or non-rationalism), and it also precludes the possibility of theoretical and methodological trade across community boundaries (cultural relativism). These two arguments together imply a third conclusion: that scientific progress cannot be defined. If what counts as good science varies widely from place to place (and from time to time), then progress will be hard to define.

In the next few sections I want to dispute each of these arguments: epistemic relativism, cultural relativism and the implication that scientific progress is impossible. Epistemic relativism is considered and critiqued in Sections 4.6 and 4.8. Cultural relativism is critiqued in Section 4.7, and the possibility of scientific progress is examined in Sections 4.9 and 4.10. A summary of results from Chapters Three and Four is offered in Section 4.11.

4.6 Epistemic relativism: does skepticism about certainty require skepticism about rationality?

The new methodologists in economics, among constructivists more generally, argue, as we have seen, that antifoundationalism has radical consequences for science. In particular, they conclude that the end of certainty must also mean the end of rationality in science. It is here that constructivism departs from other varieties of antifoundationalism.

The constructivist argument, recall, proceeds as follows:

> (1a) scientific knowledge requires justification, and (1b) justification requires certain foundations;
> (2) certainty is unobtainable [antifoundationalism];
> hence, (3) scientific knowledge is impossible, and, as a result,
> (4) methodological rules designed to produce it are therefore *post hoc* rationalizations by the winners; and "rationality" is just an honorific given to rhetoric that proves successful [non-rationalism].

What's wrong with this argument is not antifoundationalism (item 2), but the received-view hangover that scientific knowledge entails justification and certainty (items 1a and 1b). This premise argues, in effect: if it ain't certain, it ain't nothin'. This is precisely how the foundationalist would put it, with the only exception that the foundationalist thinks that certainty is obtainable. The constructivist's implicit reliance on a received-view definition of knowledge exemplifies Larry Laudan's (1995) point that "radical" positions often have more in common with traditional, received-view conceptions, than do "moderate" alternatives.

Thomas Kuhn himself is alert to this incongruity in the constructivist position. He says: "[Constructivists] are taking the traditional view of scientific knowledge too much for granted. They seem, that is, to feel that traditional philosophy of science was correct in its understanding of what *knowledge* must be . . . . If science doesn't produce knowledge in that sense, they conclude, then it cannot be producing knowledge at all." (Kuhn 1992: 9). Kuhn correctly objects to the constructivist's implicit reliance on the received-view theory of knowledge.

If one can conceive of knowledge in a more modest fashion, then the constructivist conclusion does not obtain. Rather than equating scientific knowledge with certainty, one can conceive of knowledge as fallible, i.e., as uncertain but nonetheless worth having. This is precisely where many antifoundational thinkers, who are not constructivists, begin.

Prominent examples are the critical rationalists — fallibilists like Karl Popper, William Bartley and Alan Musgrave, and also third-generation philosophers of science who favor an economic approach to science, such as Michael Ghiselin, David Hull, Philip Kitcher, Larry Laudan, and Nicholas Rescher. What unites these scholars is a more modest view of scientific knowledge, one which rejects the received-view requirement of certainty, while retaining the prospect of reasoned choice in science. Let us consider the antifoundational alternative offered by the critical rationalists.

4.6.1 Critical rationalism: rationality without certainty

Critical rationalists (or fallibilists) like Popper, Bartley and Musgrave are all antifoundationalists — they agree that infallible knowledge is unattainable. But they deny that the fact of fallibility means that scientists cannot proceed rationally, i.e. in a reasoned way that works to produce reliable knowledge. Scientific knowledge **can** be possible for the critical rationalist, once one accepts a more modest conception of what scientific knowledge is.

Alan Musgrave argues against the constructivist position by arguing that constructivists, conflate two different kinds of skepticism: what he calls "certainty skepticism" and "rationality skepticism." Certainty skepticism says: any (non-trivial) belief is uncertain and unjustifiable. Rationality skepticism says: any (non-trivial) belief is irrational. The constructivist (Musgrave uses the term "relativist") accepts both, claiming that the latter follows from the former. This is indeed where McCloskey, Klamer and Weintraub end up.

But irrationality does not logically follow from uncertainty, Musgrave points out, without an auxiliary premise. Irrationality follows from uncertainty only when one adds an additional claim (call it R), which is: (R) a belief is rational only if it is certain. (Musgrave 1993a: 280).

This is where the constructivists embed the received-view foundationalism. Ironically, both the foundationalist and the new methodologists accept R (or something like it) in their respective positions. The foundationalist pursues certainty in order to obtain rationality; and the new methodologists deny certainty in order to deny rationality (ibid). But (R) is incorrect, because it confuses rationality with certainty. They are different things. It can be rational to accept theories, even when they are not certain. Indeed, we have no other choice in an uncertain world (if we are to accept anything). The critical rationalist, says Musgrave, severs the received-view linkage of skepticism and irrationality (that is, she denies R).

Critical rationalists are skeptics who think that rational choice is possible, even under uncertainty. Musgrave calls this intermediate position a "mitigated skepticism." Mitigated skepticism says that scientists can have no certainty; but it does not follow that it is reasonable for them to believe anything. (ibid). On the contrary, certainty is not a prerequisite for reasoned (rather than, say, random) choose among theories.

In place of proposition R, the critical rationalist substitutes: (R*) it is reasonable to believe [some proposition] P if P has withstood serious criticism, and survived. Beliefs are reasonable to hold not because they are certain or justified, but because they have survived criticism. Critical rationalists are more interested in an idea's "criticizability" (and the actual criticisms) than in its putative epistemic credentials. (ibid).

4.6.1.1 Rationality: a characteristic of the process of belief generation

Critical rationalism also offers a subtle but important distinction: the difference between justifying a theory and justifying a belief in a theory. (Musgrave 1993a, chapter 15; 1993b). This has the happy result of allowing for rational belief in propositions that turn out to be false (as virtually all do). If I once believed that (P_1) the Earth is flat or that (P_2) the solar system is geo-centric; these may well have been rational things to believe at the time. When I revise my beliefs — I no longer believe that P_1 or that P_2 — I reject the propositions, not my former belief in them. If new evidence leads me to believe that the earth is, in fact, round, then I reject $P_{1.}$ Rejecting P_1 does not mean that my former belief was irrational — on the contrary, it may well have been warranted by the best evidence. The point is this: it can be rational to hold beliefs that we later find to be false.

When we find out that a proposition is false, we say that what we believed was wrong, not that we were wrong to believe it. (Musgrave 1993a: 282). The difference is that between beliefs, and the **process** by which we come to hold beliefs. Rationality, therefore, is not a characteristic of beliefs; it is, rather, a characteristic of the process by which one comes to hold beliefs.

In this sense, critical rationalism is able to accommodate the fact that the most certain of theories (Euclid, Newton) are eventually overturned, or at least shown to be a special cases. As Philip Kitcher points out, beliefs may be true or false, but this is not the same thing as saying they are rational or irrational. One can rationally hold false beliefs (the earth is flat), and irrationally hold true beliefs (The earth is roughly spherical. Why? Because that's the way my dog arranged it). Rationality refers to a process by which people come to hold beliefs, not the truth or falsity of those beliefs. (Kitcher 1993: 182-88).

In sum, then, skepticism about rationality (non-rationalism) does not follow from skepticism about certainty. As antifoundationalists, the new methodologists should not accept the foundationalist equation of knowledge and certainty, and therefore their argument against rationality fails, as Kuhn recognized. This leaves open the prospect that science **can** be a rational enterprise, or at least a partially rational enterprise, even if it cannot produce certainty. Once we recognize the value of fallible knowledge, which entails abandoning the received view equation of knowledge and certainty, scientific knowledge **is** possible. And, if scientific knowledge is possible, then it cannot be a matter of indifference as to how science proceeds.

The prospect for rational inquiry in an uncertain world is a question that divided American pragmatists a century ago, much as it divides their descendants today. The current intramural quarrel among antifoundationalists has a history in the debate among pragmatists, which sheds some light on the current dispute. I briefly sketch this history in the Appendix 4A. We now turn to the constructivist argument for cultural relativism.

4.7 Intellectual autarky: does cultural relativism follow from antifoundationalism?

The second crucial area of disagreement concerns the possibility of inter-disciplinary (or across communities) trade of both scientific beliefs and of methodological standards. Here again the new methodologists arrive at a result (cultural relativism) that does not inhere in antifoundationalism. The new methodologists say that such trade is impossible. They reason as follows:

> (1) There is no universal set of standards; scientific beliefs and methodological standards can vary from one scientific community to another [pluralism];
>
> (2) different communities are so embedded in their individual languages, cultures, and disciplinary exemplars, that exchange among or comparison across scientific communities is impossible [strong incommensurability (see Section 3.3.7)];
>
> hence, (3) scientific beliefs and methodological standards are always relative to the scientific community in question; standards **must** be local, i.e. cannot travel [cultural relativism].

In short, the new methodologists argue that cultural relativism follows from pluralism. They argue from the correct premise that standards may be local, to the rather extreme conclusion that standards **must** be local.

Pluralism is a position that many antifoundationalists endorse, as I do. Pluralism follows when one rejects the positivist dream of a universal, even algorithmic, Method. The pluralist argues that different cognitive perspectives (theories) and different cognitive methods (methodological standards) are an unavoidable feature of a world without certainty. Nicholas Rescher captures the idea of pluralism with a helpful metaphor, as follows:

> There is no simple, unique, ideally adequate concept-framework for 'describing the world.' The botanist, horticulturist, landscape gardener, farmer and painter will operate from diverse cognitive 'points of view' in describing the self-same vegetable garden. It is mere mythology to think that the 'phenomena of nature' lend themselves to only one correct style of description and explanatory conceptualization. . . . [This] is the myth of the God's-eye-view. (1990: 87).

The error in the traditional conception (and the temptation is understandable) is to assume that since there is only one nature, only one science of nature is possible— the one-world one-science argument. (ibid: 90). But many "thought-worlds" are possible, since scientific knowledge is the result of an interaction between knowing subjects and nature. Hence, while there is a world independent of mind, there is no knowledge of it independent of mind.[89] Plural beliefs and methods are going to be unavoidable in many instances, and scientists must learn to live with this fact, what Rescher calls **dissensus.**[90] (1993: 5).

But pluralism is not the same the thing as relativism. Pluralism suggests that different communities **may** use different methodological rules. Relativism argues that different communities **must** use different rules. The pluralist accepts that the rules may vary in different communities. A community of physicists, for example, may well have different standards of what constitutes a valid theory, or of what kinds of testing are legitimate, than does a community of economists. But the relativist says that there must be different rules, that the differences are insuperable.

In the two sections (4.7.1 and 4.7.2) that follow, I will show that relativism is but one possible

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[89] This formulation is ontologically realist (a world exists independent of mind), but it epistemically plural; it admits different conceptions ("thought worlds") of that world.

[90] "Dissensus" is Rescher's coinage. He means by it the opposite of "consensus." The 'dis' construction cleverly suggests dissent as well as disagreement.

response to pluralism, and that, contrary to the new methodologists' implication, it is not the only possible response to pluralism. Relativism follows from pluralism only if one embraces a totalizing version of incommensurability. The constructivist argument for relativism derives not from pluralism *per se*, but from strong incommensurability.

4.7.1 Strong incommensurability goes too far

I want to argue against the idea of strong incommensurability, and, thereby, to argue that relativism does not follow inexorably from pluralism. We have already discussed Kuhnian incommensurability in Section 2.3.7, and considered it in a methodological context as Roy Weintraub's inside-outside view in Section 3.5.3. In its strong form, incommensurability makes scientific communities wholly autarkic. They cannot trade scientific beliefs or methodological standards because they are so embedded in their own peculiar languages, ethos, and disciplinary matrices, that an alternative conceptual scheme (or paradigm) is, literally, beyond understanding.

Broadly speaking, incommensurability is a catch-phrase for the idea that scholars and scientists in different communities can never understand each other. (Laudan 1995: 9). Incommensurability is why, Weintraub argues, methodologists cannot influence economists (1989), and why Post-Keynesian economics (1985) cannot influence mainstream economics. If external theories and standards are vacuous, then only immanent criticism is possible.

There is nothing in antifoundationalism that requires incommensurability. "Rejection of foundationalism," says Isaac Levi (1981: 294) need not be accompanied by a commitment to incommensurability." Because relativism is the product of incommensurability, not of antifoundationalism, if we can argue against strong incommensurability, then relativism does not result.

I will object to strong incommensurability on two counts, one theoretical, and one practical. The theoretical argument, which is due to Laudan 1995, says that incommensurability relies on yet another positivist holdover — the idea that comparison of different theoretical schemes somehow requires an exercise in translation. The practical argument notes some of the difficulties in defining a scientific community, and asks who, as a practical matter, will be doing such law-making. Take the theoretical point first.

Laudan is persuasive that strong incommensurability relies upon and derives from a positivist holdover: the idea that theories have their own associated languages (not merely their own jargon), "replete with syntax, semantics and pragmatics." (Laudan 1995: 8). Hence comparison of rival theories is said to require **translation** of one into the other. (Positivists, who were instrumental in implementing the linguistic turn in 20th century philosophy, were keen to reduce all theories to a formal language). By the time Kuhn arrived on the scene, says Laudan, the "language of theories" and translation were already part of the philosophical vernacular. (ibid).

As we saw in Section 3.3.3, the second-generation philosophers of science (Kuhn, Quine, Hanson) challenged that idea that facts are theory-neutral. Scientists cannot render facts in a language that isn't theory laden. If there is no common "observational language" in which to compare, then the putative requirement of translation is impossible. And, if translation is impossible, then rational theory comparison is impossible — how can one choose among alternatives which cannot be compared? Kuhn makes theories and methods from different paradigms into ontological apples and oranges.

The error, Laudan points out, is not in rejecting the possibility of a theory-neutral language of comparison. This second-generation critique is compelling: facts are indeed "tainted" by the theories which try to explain them. Rather, the error lies in uncritically accepting the positivist idea that theories have individual languages that **must** be suitably translated for comparison of any kind. This simply isn't so. An instrumentalist like Milton Friedman argues, for example: compare the predictions and don't worry too much about translation of conceptual terms. A pragmatist like Laudan argues that we should prefer theories that solve more problems than their rivals. If rival theories address similar problems, then they can be comparatively assessed whether or not they are translatable in the positivist sense. The point is clear: there are pragmatic bases for theory comparison that in no way require positivist translation exercises.

Ironically, Kuhnian incommensurability is so strong a position that it rules out other aspects of Kuhn's own work. Recall, for example, Kuhn's key hypothesis that scientific paradigms "shift" during "revolutionary" periods of change, as when the Copernican model replaced the Ptolemaic model of the solar system. Strong incommensurability, however, rules out Kuhn's hypothesis of paradigm shift *a priori*, for to talk of change in theories is to necessarily compare theories.

The historian of science cannot argue that the Copernican model is radically different from the Ptolemaic model, without having undertaken an evaluative comparison to reach this judgment. And this kind of comparison is precisely what strong incommensurability will not permit. When Kuhn attempts to understand Aristotle's physics not as bad Newtonian physics, but as a viable conceptual scheme unto itself, he is undertaking the very comparison that strong incommensurability says he cannot. Two genuinely incommensurable theories cannot be compared, says Kuhn, whether one concludes they are different or similar.[91]

That Kuhnian incommensurability seems to rule out Kuhn's own thesis of paradigm shifts reveals how radical a position incommensurability entails, at least its strong form. It completely rules out conversation across community borders. In so doing, it merely substitutes a "no conversation" view for the equally extreme view of "perfect conversation." In part, this reflects the influence of the later Wittgenstein and his followers, who argue that all thought must occur within language, hence any comparison of anything entails translation — we are trapped by language in "language games."

This position has been overdone. Language **is** constraining (how could anything so

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[91] See Laudan 1995: 9-10. Laudan credits Dudley Shapere with this insight.

fundamental not be?), but it is not a prison that precludes conversation across community borders. First of all, there is compelling new evidence from the cognitive sciences which suggests that thought does not always require language (see Pinker 1994 for a readable and informative survey). Secondly, notwithstanding one's position on cognitive science and Wittgenstein, there is the possibility of examining language serially, if not from "outside" language. As David Hull puts it: "As constraining as language is, we can become self-conscious about its influence . . . . We cannot suspend all language all at once, but we can examine parts of it serially, in ways that are partially independent of language." (1988: 10).

Secondly, scientists **are** sometimes able to talk across disciplinary boundaries, albeit imperfectly. The example of Roy Weintraub, an interdisciplinary scholar *par excellence,* belies his own argument from incommensurability. Weintraub is well read in economics, mathematics, history and philosophy, and contributes in all of these fields. While there is no doubt that there are different rules and vocabularies in different scientific communities, the interdisciplinary scholar can rhetorically "toggle" in these different communities, once she has learned something of their peculiar folkways. (Klamer and Leonard 1992). Interdisciplinary travel is surely arduous, but to argue that it is impossible goes too far, as the example of Weintraub shows.

There is a kind of hyper-specialization in the academy today, which makes interdisciplinary trade increasingly harder to undertake. But all economists who subscribe to the idea of comparative advantage know that the benefits of specialization cannot be realized without subsequent exchange. To argue that intellectual trade is impossible only reinforces the trend towards specialization without trade. The mutual gains from intellectual trade can be obtained only with subsequent exchange.

Having offered a theoretical objection to strong incommensurability, let us now proceed to more practical objections. In particular, I want to pose the following questions: how are communities defined, and who draws the boundaries?

4.7.2 Incommensurability: how are communities defined and who defines?

The matter of defining scientific (or discourse) communities is not a mere detail for those who would argue for strong incommensurability. This is the "how big is big" question posed by McCloskey in another context. If one defines the community as, for example, all humanity or as all English-speakers, then incommensurability has no bite. If a community has five or one billion members, then theories and methodological standards obviously apply widely. Generally, the larger the definition of a community, the smaller will be the opportunity cost in terms of foregone gains to intellectual exchange.

To go to the opposite extreme — solipsism — we can also make each individual a "community" unto herself. On this narrowest definition of community, incommensurability is clearly overwhelming. If every individual constitutes a paradigm unto herself, then whole cognitive enterprise can never get off the ground. Given this narrowest definition of community, science cannot exist.

It is reasonable to suppose that most scientific communities will have a size somewhere in between. A more intermediate definition permits more exchange than small communities (one or two people), and less than large communities (one billion people). Say, for example, the relevant community is defined as all economists. Even here there are obvious problems. As we saw in Section 1.3, it is no trivial matter to define economics. Does economics encompass both theoretical and applied work, and what of heterodox economists like Austrians, Post-Keynesians, Institutionalists and Marxists? Is economics to be defined by a set of ideas, by a set of techniques, or by a subject matter?

It is revealing that leading advocates of incommensurability themselves offer differing criteria for demarcating communities. Kuhn proposes shared paradigms; Quine proposes "webs of belief," and Wittgenstein suggests "language games" as bases for distinguishing among communities. (Munz 1987: 361). Which shall we use? There is no unambiguous answer.

Whatever one's criteria, it seems reasonable to suppose that any definition of community will likely result in communities that are overlapping (like intersecting sets), rather than sharply bounded along all dimensions. If, for example, we use Kuhn's definition of "paradigm" in its sense of "the entire constellation" of beliefs, values and techniques shared by a community (see Section 3.3.7), then it is reasonable to suppose that different communities within economics (however defined) can share some values and techniques.

I think of economics as an amalgam of overlapping schools or groups, any two of which may share certain values, while not sharing others. Austrians and Post-Keynesians, for example, share a commitment to the idea of incomplete knowledge (Knightian uncertainty), but have very different views of the correct unit of appraisal — individual or group. Neoclassicals and Analytical Marxists use a very similar technical apparatus, optimization with equilibrium — what Kuhn called a "puzzle solution" — but have widely divergent views on production, labor markets, and income distribution. Strong incommensurability will not admit this kind of overlap. It presumes sharply-delineated boundaries that rule out the very idea of "overlapping" values or techniques. This goes too far.

There is also the question of **who** determines community boundaries. Are boundaries drawn by the social scientists who study scientific communities or by the members themselves? And, if the latter, by what mechanism (voting?) are individual views aggregated into a community consensus? Weintraub has argued, for example, that Nicholas Kaldor's critique of general-equilibrium (GE) theorizing was correctly ruled out of court by the GE community, because Kaldor was not a *bona fide* member the GE community. It is certainly possible that Kaldor was wrong on the merits, and even that he was wrong because, being outside the GE community, he was less well-informed. But it seems dangerous to endorse dismissal of criticism based on putative memberships — saying, "he's not one of us." This is so for two reasons.

One, it gives comfort to those who would substitute membership in groups, however defined, for reasoned argument. In economic terms, it helps to legitimize restraint of trade. Second, given the ambiguities, just noted, of defining a community, the door is opened to strategic definition. Define your community narrowly enough, and no criticism will ever threaten your theories — a very effective immunizing stratagem. This is what happens when economists say "he's not serious" or "that's just not interesting." Ruling out of court and avoidance of criticism are far less promising than intellectual engagement.

In sum, a practical objection to strong incommensurability says that community definition is ambiguous, and susceptible to strategic abuse. This does not mean, I must emphasize, that distinct scientific communities don't exist — they do. Nor does it mean that the difficult task of definition should be abandoned — on the contrary. And, likewise, there is no doubt that communication of scientific beliefs and methodological standards can often be difficult, and that differences can and do persist over time. What I object to is the idea that more-or-less distinct communities can **never** trade intellectually. Sometimes they can, as entrepreneurs like Weintraub himself demonstrate. And intellectual trade tends to be liberalizing, in the sense of making communities better able to communicate ideas and methods.

Equally important, I would argue that meta-scientists should not put the theoretical cart before the empirical horse. Make your community definitions, and then investigate whether scientific beliefs and standards can ever travel, or not. Strong incommensurability is a testable hypothesis. If one can show that (even a few) standards apply in several communities, then one has a counterfactual to strong incommensurability. In Chapter Six, I will attempt to identify some methodological rules which I believe apply rather widely, and which, thereby, provide a counterfactual to strong incommensurability.

I have argued that strong incommensurability is too extreme, and that a less restrictive view of interdisciplinary exchange helps prevent pluralism from lapsing into cultural relativism. This more moderate stance preserves the possibility that ideas and methodological rules can travel, without insisting that they are universal. Pluralism avoids lapsing into cultural relativism when one rejects incommensurability in its strong form.

4.8 Responses to pluralism: what are the grounds for choice

Rejecting strong incommensurability rules out cultural relativism as inevitable, but pluralism remains nonetheless. All antifoundationalists agree that there is no magic recipe for producing the one best, ideal theory in all settings, hence different theoretical perspectives are possible. Science, like all human endeavors, must settle for fallibility in this form. The problem this section takes up is the following: how should scientists proceed in a world with multiple theoretical alternatives; on what basis should they choose?

There are, as Nicholas Rescher argues in his compelling account (1993), essentially four ways of responding to theoretical pluralism, each with its proponents. They are:

> 1. Radical Skepticism (Nothing goes); No alternative should be accepted; no single proposition is justified; the alternatives simply cancel one another out;
>
> 2. Syncretism (Anything goes); all the alternatives should be accepted; all those seemingly discordant positions are in fact justified; they must, somehow, be cojoined and juxtaposed;
>
> 3. Relativism (non-rationalism); only one alternative should be accepted, but this acceptance cannot be based on rationally cogent grounds, it emerges solely from nonrational factors — matters of taste, personal inclination, social tradition, etc.
>
> 4. Rationalism: only one alternative should be accepted, and this acceptance has a basis of rational cogency, albeit this basis may differ from community to community and era to era. (This reproduces Table 5.1 in 1993: 80, with minor modifications).

Radical skepticism, response one, is an ancient position. The skeptic says: we can't know anything with certainty hence there's no basis for preferring any one position over another. Though skepticism is hard to refute logically[92] (see Musgrave 1993a: 19-29), it is vulnerable to economic

[92] As we have seen, if knowledge is taken to require certainty, then there is, indeed, no knowledge. Of course, one can ask the skeptic: how do you *know* there is no knowledge?

argument. The skeptic who accepts nothing, maximizes Type I errors (rejecting hypotheses one should accept), even as she minimizes Type II errors (accepting hypotheses one should reject). In short, skepticism is a non-starter if one wants to reason about one's environment, that is, to "enter the cognitive enterprise." (ibid: 80-88).

Human beings enter the cognitive enterprise not out of curiosity alone, but because survival depends upon it. Not all theoretical knowledge is scientific in the sense that we conceive of it today, and all but the most resolute skeptics make routine use of their knowledge. Rescher argues that entering the cognitive enterprise is less a choice than an imperative:

> Man is *homo quarens*. We have questions and want (nay, *need*) answers. The need for information and thought-orientation in our environment is as pressing a human need as that for food itself. . . . The quest for cognitive orientation in a difficult world represents a deeply practical requisite for us. (ibid: 87).

Pluralism requires choosing. A radical skepticism, which retains the knowledge-must-be-certain requirement of the received view, says that choice is meaningless. Radical skepticism, therefore, is a non-starter.

Syncretism, or Anything Goes, is equally nonoperational as a response to pluralism, Rescher argues. In accepting everything, syncretism also precludes meaningful choice. Syncretism is "an attempt to 'rise above the quarrel' of conflicting doctrines, refusing to take sides by taking all sides at once. It is a Will Rogers kind of pluralism that never met a position it didn't like." (ibid: 91). The syncretist refuses to accept the principle of non-contradiction, preferring to believe that all theories can get at some aspect of the truth in some way, hence they are "cojoinable" into one grand theory. (ibid: 90-95). But to refuse to discriminate, to opt for not opting, is as much as refusal to enter the cognitive enterprise as is radical skepticism. To accept all theoretical alternatives is also a non-starter. This leaves two candidates as responses to pluralism — relativism (or non-rationality) and rationality — both of which entail choosing.

Pluralism requires choosing; the very meaning of the word "science" is, in part, discrimination. The issue is, on what grounds are theoretical choices to be made. On what bases do scientists make their choices? In the next section, I will argue that those choices can sensibly be made only on rational grounds — a view that the new methodologists dispute.

4.8.1 The logical impossibility of non-rationalism

The fact is that scientists **do** make theoretical commitments. They choose precisely because they have entered into the cognitive enterprise, which rules out radical skepticism and syncretism alike. Any explanation of choice under pluralism will turn on **how** theoretical choices are made.

The non-rationalist is **indifferent** as to how choices are made, since she denies that any rational basis exists for one's theoretical commitments — choices are based on non-rational factors, such as personal taste or fashion. The non-rationalist is therefore indifferent, in principle, among alternative theories and methodological rules.[93] The rationalist, in contrast, argues that the non-rationalist position cannot be coherently sustained. Non-rational choice, says the rationalist, is logically self-refuting, and inconsistent with our own experience in forming theoretical beliefs.

If the pluralist must accept the inevitability of different theoretical positions, she needn't accept the non-rationalist's claim that all positions are cognitively equal. In fact, once a scientist or scholar makes a choice, her own commitment tends to undermine the non-rationalist's egalitarianism. The reason is this: if the choice was made thoughtfully i.e., with careful, reasoned deliberation, one believes, by definition, that her choice is superior to the rejected alternatives.

Whether she is correct in this belief is an entirely different matter. Reasoned choice doesn't guarantee that the chosen theory is indeed the better theory. But when scientists believe that their

[93] The converse is not true; that is, it doesn't follow that indifference makes one an non-rationalist. One can be indifferent among theories without being indifferent **in principle.** This can occur when one lacks information sufficient for a reasoned choice, or because one has sufficient information but believes, on rational grounds, that the alternatives are equal.

theory choices are superior, it is not because of some psychological propensity, but because of the of the very process — deliberation and weighing of evidence — that has lead to the choice. For the thoughtful person, it is this rational process that works to self-certify our belief. (Rescher 1993: 98-106). Again, rationality consists in a **process** of arriving at scientific beliefs, it does not refer to the aspects of the beliefs themselves.

If the decision-making process were not rational in this sense, then our theoretical choices would be thoughtless (random), or wholly subjective (dependent upon the day of the week, or upon yesterday's shoe color) or, as Peirce feared, the product of dogma or coercion. If I believe that the earth is roughly spherical because my dog has so arranged it, then I haven't arrived at my belief rationally. And when theoretical choices are made in non-rational fashion, one cannot (honestly) put much stock in one's beliefs. We do not self-certify beliefs that were produced non-rationally. (This doesn't rule out the possibility that my non-rational belief could nonetheless prove correct, as we saw in Section 4.6.1.1). Indeed, who would openly seek to present her views as determined randomly or subjectively or dogmatically? Have you ever heard a scientist or scholar (seriously) defend her position by reference to whim or to talking dogs or to shoe color? The answer, I submit, is no.

This takes us to the second, related objection to non-rationalism — the fact that it is self refuting. I recognize that this argument is old hat, but it bears repeating: one cannot consistently advance a rational argument for the impossibility of rational argument. If you are correct, then your very success denies your position, and if you are incorrect, then your argument for non-rationalism fails. Rescher says: "[non-rationalism] is self-frustrating in that to whatever extent it is correct it cannot be seriously maintained to be so." (1993: 104).

One cannot say, with any cogency, that "my basis for making this claim is ultimately no better than other, conflicting bases that may underwrite other, conflicting conclusions." (ibid). Even the non-rationalist thinks she is right. This is the quandary that Janet Seiz alluded to in Section 3.5.2: she believes passionately that her (well-reasoned) views on economics are correct, and yet she also subscribes to a constructivist epistemology that appears to rule out any reasoned basis for choosing her position, or any other.

My response to Seiz is this: pluralism doesn't require non-rationalism. If you examine **why** you think that your beliefs are correct — why you self-certify them — it will be because you have arrived at them via a process that is rational, i.e., a process that involves careful deliberation and weighing of evidence. One can view all beliefs as on a par in some abstract sense, but we cannot sensibly believe that **our** belief is not superior, provided we have done the hard work that reason requires. We put credence in our own scientific views when they are the product of careful deliberation and weighing of evidence; we do not (honestly) put credence in our scientific views when they are not. In this sense, introspection provides evidence against the argument for non-rational choice.

Introspection is not the only way to see this. Consider colleagues who make the case for non-rationalism. The passion, intelligence, energy and (yes) careful reasoning of these scholars is inexplicable without our hypothesis: they believe they are correct because they have developed their beliefs with careful deliberation and weighing of the evidence. The only alternative hypotheses are (1) mass delusion or (2) rank dishonesty.

The mass-delusion hypothesis proposes that all these scholars think that their views are rationally devised when, in fact, they are nothing of the kind. On this account the non-rationalist arrives at an argument against rationalism by non-rational means — e.g., by consulting the dog, or yesterday's shoes. The dishonesty hypothesis says that the non-rationalist doesn't actually believe in non-rationalism, but finds it in her interest to pretend that she does. Neither of these hypotheses — which imply that **all** avowed non-rationalists are fools or knaves — is remotely plausible, and I reject them. What is left — that non-rationalists **do** believe their arguments, and do so because they are produced rationally — unavoidably undermines non-rationalism. Non-rationalism is sustainable only on the absurd premise that all its proponents are fools or knaves.

A final point regarding honesty and self-interest. "Rationality" has taken on specialized meanings in economics that differ from the everyday sense — deliberation, the application of reason, the weighing of evidence — in which I am using the term here. In economics we use "rationality" to mean: choosing what one most prefers (utility- or profit-maximization), or, in more casual usage, acting in one's self-interest. I want to be clear that in this context, reason, *qua* deliberative process, should be distinguished from self interest.

What one believes and what one wants are often different things. The snake-oil peddler knows that her tonics will not work as advertized, though it is in her self interest to pretend otherwise. Her actual beliefs — what she takes to be the case regarding the efficacy of her tonics — are different from her stated beliefs. Similarly, I can rationally believe that tariffs and non-tariff trade barriers are a net loss for society, and, nonetheless, be delighted to have my domestic industry (or those I hold shares in) protected. In the context of this discussion, rationality refers to process of belief generation, not to the truth or falsity of beliefs, nor to self interest *per se*. Rationally obtained beliefs can be false, and they can also be contrary to one's self- interest. What one takes to be true can be wrong, and also can be bad for business.

Having argued against cultural relativism and epistemic relativism (or non-rationalism), we are now in a position to consider the new methodological position on scientific progress.

4.9 The possibility of progress in science

The new methodologists, we have seen, are reluctant to consider progress in science, because constructivism seems to rule out the traditional way of thinking about progress, as asymptotic convergence on Truth, with a capital "T." (See Section 3.2). But it is possible to conceive of scientific progress in humbler, more practical terms. Progress can be seen as more efficient fulfillment of scientific goals, or in meeting more of these goals, without reference to Truth *per se*. Consider by analogy, an ordinary good such as an automobile. A superior automobile represents progress over its predecessor by better accomplishing certain goals. The better auto is faster, or more reliable, or more comfortable, or safer, or more pleasing to the eye, or more envy-inducing among the neighbors, or some combination. Likewise theories, they too are improving to the extent they better accomplish scientific goals.

As we saw in Chapter Two, there are many possible goals for theories. In Chapter Three, we saw that the received view of science is traditionally preoccupied with one transcendent goal for theories, Truth. Truth is, for the received view, meant in the sense of correspondence. Truth as correspondence presupposes a kind of scientific realism; theories mirror reality, and theories improve by asymptotically converging toward a truer and truer correspondence with the world. For illustration, recall the theory-as-map metaphor.

The theory-as-map is meant as a metaphor for truth consisting of a correspondence between theory and reality. Maps are generally scale models of a known reality. We can, in some sense, compare the scale model with the known reality. Is the border correctly drawn; are the mountains at the right altitude, and is the coastline correctly depicted — these are all possible queries. But there are also important disanalogies between theories and maps.

One disanalogy is that theories are often not small-sized substitutes for a known terrain, but, rather, the very means by which we try to apprehend an unknown, or poorly known domain. (See Klamer and Leonard 1994: 39-42). In theoretical physics, for example, super-string theory postulates the existence of exotic, tiny bits of matter known as "strings." But there is, as yet, no empirical means of inspecting matter at a level small enough to evaluate the claims of super-string theory. There is no Archimedean point from which to measure the distance between theory and reality if theory is our current means of apprehending reality.

A second disanalogy is that theories are often nothing like little scale-models. When an economist thinks of how prices are determined in a market, she is likely working with a mental construct, such as a supply and demand curve diagram. A blackboard diagram is often called a "model," but it is not anything like a scale model of actual markets or of market exchanges or of production processes. In these instances, "models" are more like metaphors, a lens through which the theorist conceives of some reality, rather than a scaled depiction of that reality. (ibid). Maps abstract from reality, to be sure, but not all theories abstract in the same, representational way that maps do.

Thirdly, though not a disanalogy, the thing to be mapped can change. In the case of actual maps, political borders change yearly, and even geological features are wholly transformed over a long enough period. In the case of theories, this means that the target is always moving. A theorist can speak of one reality without claiming that this singular reality is somehow invariant. Aspects of reality **may** be invariant, but, particularly in the social sciences, there are no guarantees of stability. (See Mirowski 1989). The relevant question for the theorist as cartographer is, how much stability, and for how long?

Finally, maps can serve many representational goals — the cartographer can depict topographic features alone, or political boundaries alone, or road systems, or population densities, or bulk commodity production, or mean temperatures, or the path of Lewis and Clark. Different maps serve different goals, and can be judged only with reference to these goals. The point is that there is no one best map, no "uniquely perfect representation." (Martin Rudwick, cited in Callebaut 1993: 224, ff 28).

It is difficulties such as these that have lead pragmatists like Larry Laudan and Bas Van Fraassen to endorse a notion of scientific progress that does not require the realism that a correspondence theory of truth demands. Laudan recall, considers progress in terms of problems solved, both empirical and conceptual. In economics, Tony Lawson and Uskäli Mäki have argued against this idea, and for predicating economics on a epistemically realist foundation.

For my part, I would prefer to remain agnostic on the realism issue. It seems clear that, absent a fixed Archimedean point, it is very difficult to measure the distance between a current theory and a true theory. On the other hand, I think it is the case that, even if we reject the possibility of a fixed Archimedean point, given the difficulties just enumerated, there nonetheless is a singular reality out there that theories "get at." (Kuhn 1992: 14). Even if there is no one best description for all time, there are descriptions which accomplish scientific goals better than others, and often by virtue of their connection to the real world. Dudley Shapere says, in this regard:

> [T]here *is* something in experience which is independent of our presuppositions: no matter how much selection and interpretation we impose, a result of experience or experiment can still disagree with what we expected in light of those presuppositions. Depending on the character of the conflict, that can lead us to change our methods and our goals as well as our substantive beliefs about nature. (In Callebaut 1993: 67).

Nature can work to influence our beliefs, even if we are skeptical about obtaining a fixed, permanent Archimedean point. Scientists cannot reasonably believe whatever they like, for the world will not admit all interpretations, even if it admits many. Hence, for purposes of thinking about scientific progress, I am content to invoke the usual weasel words that are deployed as euphemisms for the Truth (with a capital "T") — increasing truthlikeness, more adequate referents, more complete or more correct schemata. (Kitcher 1993).

4.9.1 Progress defined by comparative choice

Fortunately, progress does not require taking a position on realism, on Truth. As Thomas Kuhn (1992) argued later in his career, scientific progress only requires the possibility of comparative evaluation. Kuhn's model of progress eschews a fixed, permanent Archimedean point for theory evaluation, instead proposing an Archimedean point that is historically contingent, enabled by a given community's agreement on the facts (theory-laden though they are), and by Kuhn's proposal for changing the unit of appraisal.

Instead of evaluating a given belief, Kuhn proposes evaluating an incremental change in belief. Kuhn says that "seldom can one compare a newly proposed law of theory directly with reality. Rather, for purposes of evaluation, one must embed it in a relevant body of currently accepted beliefs . . . ." (1992: 13). Rather than asking, is T_1 true, which requires the permanent Archimedean yardstick, Kuhn says we should ask, is $T_1 + \epsilon$ better than T_1.

For "better" Kuhn suggests the following standards of appraisal: accuracy, consistency with accepted beliefs, breadth of applicability (generality), and simplicity. Kuhn says:

> To ask which of two bodies of belief is *more* accurate, displays *fewer* inconsistencies, has a *wider* range of applications, or achieves these goals with the *simpler* machinery does not eliminate all grounds for disagreement, but the comparative judgment is clearly more tractable than the traditional one from which it derives." (Ibid).

Like Laudan, Kuhn argues against Truth as attainable goal, but wishes to preserve, *contra* the new methodologists, the possibility of rational evaluation, and therefore of scientific progress. As Laudan says: "[the comparative approach] relativizes the acceptability of a theory to its competition." (1995: 86). Kuhn obviously does not deny that the standards of appraisal may be disputed, nor does he deny that theory appraisal is a messy, contentious business. (See Chapter Two). What he does deny, again *contra* the new methodologists, is that theory choice is never meaningful, that reason and evidence are inconsequential. Says Kuhn: "Nothing [I have suggested requires] replacing evidence and reason by power and interest. Of course power and interest play a role in scientific development, but there's room for a great deal else beside." (1992: 15).

Advocating progress in science does not require settling the ancient epistemic debate on scientific realism. There are proponents of progress in science who are realists (Kitcher 1993, Popper 1959) and those who are not realists (Laudan 1995; Van Fraassen 1980). In avoiding the realism problem, I don't wish to abandon the idea that successful theories refer to the world, only the extreme view that correspondence consists in a **complete** mirroring or capturing of reality. The notion of asymptotic convergence on truth implies that, at the limit, completeness is possible, and, therefore, that the ideal theory exists.

I agree with Donald Campbell, who objects to the implication that knowledge somehow requires a "*complete exhaustion of the referent.*" (in Callebaut 1993: 416-17).[94] Campbell points out that theories can be right or wrong in more limited senses, even if they can never be complete. Some maps are better than others (given cartographic goals), but there is no one best, ideal map. "We have got to," Campbell says, "get rid of the notion that anything that is *usable* knowledge is complete." (ibid). Campbell's idea is that we can get some things right, and that this knowledge is valuable, without believing that there is a single way of capturing truth that will be exhaustive. (ibid). The best need not be the enemy of the good, if we can accept that something short of the best is worth having.

William Wimsatt see theories in a similar way. "It is not the aim of theories to capture all phenomena." Rather, "*Our theories are cost-benefit structures adapted to dealing with real-world problems.*" (In Callebaut 1993: 173). This is an economic view of the work that theories do: theories are not machines for generating Truth; they are more like cost-benefit structures for accomplishing certain scientific goals at the expense of others. Even if Truth were within our grasp, i.e. even if the ideal theory existed, finite intellectual resources would still require some trading off among different scientific goals.

4.10 Progress and the problem of theoretical (dis)agreement[95]

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[94] Campbell (in Callebaut 1993: 417) relates an anecdote about two competitive cartographers, due to Lewis Carroll, on the absurdity of complete reference. "The Englishman says: 'We've mapped all of England 1 mile to the inch!' The German says: 'That's nothing! We've mapped all of Germany 1 inch to the inch, but the farmers won't let us unroll it.'"

[95] This section and Section 4.10.1 rely heavily upon Laudan 1995, especially chapters five and thirteen, the latter which is co-authored with Rachel Laudan.

In this section, I want to connect scientific progress (and standards of theory appraisal) to the issue of theoretical disagreement. Recall from Chapter One the discussion on standards of appraisal. With reference to Kuhn 1977, I noted several possible difficulties of applying evaluative standards to theories. The difficulties concerned consensus, ambiguity and conflict. First, there may be disagreement among scientists (even within a community) on what the standards are or on how specific standards are to be weighted in theory appraisal, a problem of divergent standards. Second, there may be ambiguity when applying (even agreed upon) standards to actual cases. And third, standards may conflict among themselves — more generality may come at the expense of accuracy, for example.

The new methodologists correctly focus on the most serious threat to theoretical consensus — divergent standards of theory appraisal. They reason that if scientists employ different standards of appraisal, then theoretical agreement is problematic. And, since scientific progress requires theoretical agreement, how can science ever progress with divergent standards of appraisal? It is no surprise, the new methodologists argue, that scientists routinely disagree. The new methodologists reason that methodological disagreement on standards of appraisal explains the problem of theoretical disagreement. Plural or divergent standards are why scientists disagree — call this the problem of theoretical disagreement.

On the other hand, however, the argument cuts two ways: how can a proponent of divergent standards explain theoretical consensus? It is clear that scientists agree on a great many things. Theoretical consensus exists in many places. If standards vary widely, then theoretical agreement is hard to explain. Laudan makes the point rhetorically, "if different scientists have (at least partially) divergent and conflicting aims and standards, then how is the high degree of consensus often exhibited by the natural sciences to be explained?" (Laudan 1995: 231). Call this the problem of theoretical agreement.

The received view, of course, could readily explain theoretical consensus. All scientists (the unity of science hypothesis) use **The** Scientific Method, which, given like information, guarantees theoretical agreement. The received view therefore cannot explain persistent theoretical disagreements, just as the new methodologists — who argue that standards cannot apply outside a local community — cannot explain theoretical agreement.[96] Plural standards seem to rule out theoretical agreement and a universal set of standards seems to rule out theoretical disagreement. But science, of course, has both agreement and disagreement. Is there a way out? Laudan 1995 says yes.

The first point is to note that the problems of consensus, ambiguity and conflict among standards are not inevitable. Many standards are, for example, reasonably unambiguous. Laudan offers an example of a standard which is not hard to interpret: theories should be internally consistent. Most scientists understand what is meant by internal consistency, and are able to apply this standard to concrete cases. (1995: 92). There are other examples. Consider the following two: theories should lead to predictions of phenomena that are unknown to the theorist, and theories should be subject to controlled experiment. Most scientists are clear on what is meant "phenomena unknown to the theorist," and by "controlled experiment." It is also easy to conceive of appraisal standards which do not conflict. But let us concede that lack of consensus, ambiguity and conflict among standards can and do occur in science. Since I agree with the new methodologists that there is no Universal Method, is there a way in which theoretical agreement is nonetheless possible?

Theoretical consensus can occur with divergent standards. One way is a timely resolution of disagreements about standards. If scientists can resolve their differences over the relevant standards, then theoretical consensus is possible. But this is only a partial explanation, and obviously cannot

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[96] Strictly speaking, the new methodologists can admit theoretical agreement, but only within a smallish community; they cannot explain more general theoretical agreement, i.e., across different communities. Likewise, the received view can explain theoretical disagreement in the short run, perhaps different scientists have differential access to the latest results, but they cannot explain persistent theoretical disagreements.

explain persistent disagreements over standards. A more promising alternative is the hypothesis of **theory dominance** (Laudan 1995: 234). Assume a community with two different standards, S_1 and S_2, and two rival theories, T_1 and T_2. Proponents of S_1 emphasize accuracy, and proponents of S_2 emphasize parsimony, say. Even with this disagreement on standards, the respective proponents can still reach a unified theoretical choice, if one theory (say T_1), dominates the other. Theoretical consensus occurs when T_1 is superior by both sets of standards, that is, if T_1 is more accurate and more parsimonious than T_2. More generally, theory dominance occurs when a theory is superior to all rivals with respect to all relevant standards.

Consensus with divergent standards is therefore possible when a given theory strictly dominates its rivals. Hence, *contra* the new methodologists, different standards do not rule out theoretical consensus in all cases. And, *contra* the received view, one can explain theoretical agreement without recourse to a Universal Method, or even to general agreement on standards. (Laudan 1995: 234-5). Theory dominance does not overcome the problems of ambiguity in applying standards, nor does it work in the absence of theoretical domination, but it does show that theoretical consensus is possible even with divergent standards of appraisal. Unanimity on standards is therefore not a necessary condition for theoretical agreement. And, divergent standards do not guarantee theoretical disagreement.

4.10.1 Divergent standards and theoretical innovation

The foregoing is closely related to problem of theoretical innovation, as Laudan 1995 (chapter 13) demonstrates. Argued first by Paul Feyerabend, Laudan formulates the problem of innovation as follows: "If one insists . . . that standards for accepting a theory should be pretty demanding epistemically, then how can it ever be rational for scientists to utilize *new* theories, which (in the nature of the case) will be likely to less well tested and well articulated than their older and better established rivals?" (1995: 232). Feyerabend's argument was: (1) (a truly binding) set of universal standards precludes rational theoretical innovation. But (2) theoretical innovation is crucial to science, hence (3) innovation can only occur when science abandons its standards and embraces the anarchy of anything goes. (ibid).

Kuhn agrees with Feyerabend, saying that, strictly speaking, it is always non-rational to embrace a theoretical upstart. On this view, theoretical innovation necessarily involves a kind of folly by the scientists who switch to a newer rival. Kuhn, recall, argues that embracing an upstart theory, at least in the early stages, should be seen as non-rational, as a kind of conversion experience, rather than as a reasoned choice. (See Section 3.3.7). But if theoretical innovation is a good thing for science, it seems odd to characterize it a wholly non-rational. If science wants innovation — competition in the ideas market — then there ought to be a way of making the development of theoretical rivals a reasonable thing for scientists to do.

Laudan proposes just such an alternative. He explains theoretical innovation by reference to divergent standards of theory appraisal. Imagine the emergence of a new theory that meets the standards of some scientists, but not of others. The new theory, for example, might be internally consistent, or predict a wider set of phenomena, without having accumulated much empirical support. The early adopters (innovators) accept the theory because their standards are met, and they develop the theory further, with a eye towards meeting the (as yet unmet) standards of their colleagues. (ibid).

In the case of theoretical disagreement upon innovation, a testable hypothesis results: the early adopters (innovators) subscribe to different standards than do those who accept the new theory (if ever) at a later stage. (ibid: 233-4). We would expect later converts to employ standards that differ from those of the innovators. Laudan presents the case of continental drift in geology as an example of his thesis that divergent standards can explain theoretical innovation.

As we saw in Section 1.1, geologists and geophysicists were long divided on whether the continents were immobile or whether they had drifted over time. By 1966-7, however, virtually all scientists opted for the theoretical innovation, drift. Before 1966, the evidence for drift was not too different from that cited by Wegener (the originator of the hypothesis) fifty years prior: (1) the continents appeared to fit together, like jig-saw puzzle pieces (especially South America and Africa), (2) there was remarkable similarity of rocks, flora and fauna at the corresponding locations on either side of the Atlantic ocean, (3) major mountain chains, like the Alps, were foreshortened, and (4) the poles had apparently moved over time. (Laudan 1995: 237). This evidence was sufficient to convince the early adopters, who argued that drift theory explained a wider variety of phenomena than its rivals.

The other camp, however, was unimpressed. They agreed that drift theory met the standard of explaining a wide variety of phenomena, but they argued that drift theory could explain only those phenomena it was invented to explain. Drift theory could not meet the more demanding standard of predicting new phenomena, nor was it readily testable.

The deadlock was broken in the mid-sixties. Vine and Matthews predicted that if one form of drift theory, sea-floor spreading, were true, then the magnetism of recently discovered mid-ocean ridges ought to be symmetrical, mirror-like. In 1965-6, this prediction, wholly incompatible with immobile continents, was confirmed. As second, unexpected, prediction was also borne out at about this time. Known submarine fracture zones were hypothesized to be a new kind of fault, one that would allow for drift, and convincing confirming evidence was presented in 1966.

Laudan argues that the confirmation of prediction of new phenomena (in two cases) was decisive. The immobilists (and the agnostics) accepted drift theory almost immediately, because their standards had been met, and convincingly. Early adopters accepted drift theory earlier because drift theory was able to meet their standard early on: it explained a variety of phenomena. The later adopters had different standards — surprising prediction and independent testability — which were met only in the mid-sixties. Post-1966, Laudan argues, drift theory was **dominant** with respect to all its theoretical rivals (1995: 239-40).

The point I wish to emphasize here is that divergent standards, can not only accommodate theoretical agreement in some instances, it can also explain theoretical innovation. The received view, which posits a Universal Method, cannot. The received view obviously implies that later adopters will have the same standards, and therefore it cannot explain theoretical innovation (and the disagreement it may create), as Feyerabend pointed out.

I must emphasize that non-cognitive factors can also come into play during scientific debates, a point the new methodologists would obviously endorse. It's possible to argue early adopters are less risk-averse, or that they have less invested in the older theory, or that they come from a different culture, or that they are behaving strategically out of self-interest. Laudan's hypothesis that divergent standards can accommodate theoretical disagreement and can explain theoretical innovation does not, by itself, rule out non-cognitive explanations. Without denying that these factors play a role, it is important to recognize that differences in theory appraisal do not depend upon them. Just as it is possible to explain theoretical agreement without requiring consensus on standards, it is also possible to explain differential assessment of theories without invoking non-cognitive factors.

4.11 The middle way in economic methodology: a summary outline

In this section, I offer a summary of what my critique of the new methodological view entails, that is, a summary of the "middle-way" position in economic methodology. These are, in succinct form, the arguments I have made in Chapters Three and Four. I summarize my position on scientific knowledge and on scientists in Section 4.11.1 and on standards of theory appraisal in Section 4.11.2.

4.11.1 Conclusions regarding scientific knowledge and scientists

The following are my conclusions on scientific knowledge, as argued in Chapters Three and Four.

1. The new methodologists are correct; there are no certain foundations to knowledge. Certain knowledge is unattainable, and if theories must be justified by recourse to certainty, then no theory can

ever be justified. However,

2. Skepticism about certainty, which is the hallmark of antifoundationalism, is not the same thing as skepticism about rationality, and the new methodologists err in conflating the two. Non-rationalism follows only if one implicitly accepts the received-view equation of scientific knowledge and certainty, a claim that antifoundationalists should reject. (See item one). If, instead, one sees knowledge as fallible, then the prospect of reasoned inquiry can be retained. Prominent antifoundational alternatives that retain the prospect of reasoned choice by scientists are offered by Charles Sanders Peirce, scholars in the tradition of critical rationalism (the later Karl Popper, William Bartley, Alan Musgrave), and third-generation theorists of science in the naturalistic mode, such as Michael Ghiselin, David Hull, Philip Kitcher, Larry Laudan, and Nicholas Rescher.

3. Rationality is best seen as a characteristic of the **process** that generates beliefs, not as a characteristic of the beliefs themselves. One can rationally hold false beliefs and irrationally hold true beliefs.

4. Ontological realism is compatible with epistemic pluralism. One can believe in the existence of a real world that exists independent of human representations of it, without concluding that one world admits only one, best theoretical explanation. Conversely, multiple theories (epistemic pluralism) do not require multiple worlds.

5. There is nothing in antifoundationalism that requires cultural relativism, the idea that all beliefs and methods are necessarily incommensurable, unique to a given community and beyond all external evaluation or comparison. A middle way recognizes the **possibility** of incommensurable systems, but denies that incommensurability is inevitable. Scientific beliefs and methodological rules **can** travel across the borders of scientific communities, contrary to the new methodologist's strong incommensurability. The extent to which beliefs and methods do travel is best approached as an empirical question.

6. Scientific theories can refer to the world, even if one rules out the prospect of complete or exhaustive description of phenomena. Evidence from nature therefore is not, as some sociologists of scientific knowledge claim, wholly irrelevant to the formation of scientific beliefs.

7. Rival scientific theories can be comparatively appraised. One can sensibly ask which of two or more rivals better meets current disciplinary standards, such as accuracy, simplicity, generality and consistency.

8. Scientific progress is possible because progress does not require Truth in the received-view sense of **complete** or exhaustive correspondence, nor does it require permanent or invariant goals of science. Progress can be considered in more pragmatic terms, such as an increase in problems solved. And, to the extent that disciplinary goals are reasonably stable, progress can be assessed. Hence, there can be growth in scientific knowledge.

The following conclusions pertain to scientists.

9. Scientists work in a social setting; they do not interrogate nature directly, independent of all external influences. On the other hand, scientists do consider empirical results in forming and in appraising their hypotheses. Both social and natural factors influence scientific belief (see conclusion 6). Scientists are sometimes obliged to accept views they set out to refute.

10. Scientists are not exclusively seekers of knowledge. They can and do have non-cognitive goals as well, such as esteem, prestige, wealth, and even revenge upon enemies. In Kitcher's (1993) terminology, scientists are "epistemically sullied."

11. Scientists are self interested. This implies, for example, that they abide methodological rules only to the extent that it is in their interest to do so, and that they will prefer to be first in a race for discovery, and that, if priority entails material rewards (esteem, wealth, prestige), there will be priority battles.

4.11.2 Conclusions on standards of appraisal in science

The following summarizes my conclusions from Chapters Three and Four on the nature, source, and function of methodological standards in science.

1. Methodological standards exist.

2. Standards have different sources. They may deduced from an epistemologist's model of good scientific practice, or they may be induced from a study of effective scientific practices. One cannot dismiss all epistemically-informed standards on grounds that some such standards have previously failed (*pace* McCloskey). In either event, standards should be judged by their efficacy (relative to other standards) in meeting scientific goals, not by their pedigree.

3. Standards are not, contrary to the received view, universal, i.e. fixed for all times and places. Standards, like the different sciences that employ them, are plural; they evolve over time, and can vary across disciplines. Thus, it makes no sense to speak of a universal set of rules for inquiry. But,

4. Plurality ought to be distinguished from cultural relativism. That different communities **can** have different standards doesn't mean that they **must**. Standards can apply more generally, once one eschews incommensurability in its strong form. (See conclusion 5 above). And,

5. Since standards **can** persist over time and **can** apply in different scientific communities, the longevity and scope of standards are empirical questions, not matters to be determined in advance. If one can identify standards which **do** apply more generally, then one has a counterfactual to the claim that they cannot.

6. Standards may be ambiguous when applied to concrete cases of theory appraisal, and, some standards (even those with a consensus) may conflict with one another. The greater is the ambiguity and internal conflict among standards, the greater is the likelihood of theoretical and methodological dispute.

7. There may also be disagreement on what are the appropriate standards for theory appraisal

(or on the relevant weights of the appropriate standards). But,

8. When there is theory dominance, divergent standards need not lead to theoretical disagreement. Divergent standards do not, therefore, guarantee theoretical disagreement.

9. Theoretical innovation, a special case of theoretical disagreement, can be explained with reference to divergent standards. The (testable) hypothesis is that early adopters employ a different set of standards than do others, and will convince these others only by improving the new theory so as to meet these (as yet unmet) standards.

10. Standards may be tacit, hard to articulate (the new methodologists), or they may be explicit, even codified (the received view). There are instances of both types, and both types are influential. Again, it is an empirical question as to whether standards are necessarily tacit. If one can correctly articulate a standard, then one has a counterfactual to the claim that standards cannot articulated, or "written down."

This summary concludes my theoretical attempt to critique the new methodology in economics. A more moderate view of science, I have argued, emerges when one forgoes the more extreme requirements of the constructivism. In particular, I argue that the new methodologists, among constructivists more generally, can safely abandon (1) strong incommensurability (and its implication of cultural relativism), (2) non-rationalism, (3) skepticism towards the possibility of scientific progress, and (4) hostility towards all epistemically-informed standards. I hope to have shown that there is nothing in a more realistic conception of science that requires these constructivist outcomes.

Once shorn of its the excesses which I argue are due to constructivism, the new economic methodology becomes quite moderate, even as it retains the heart of the new methodological critique in economics. It retains, for example, the more sophisticated view of scientific motivation — scientists are self-interested and may have non-cognitive goals. It retains the idea that science is a social process. It retains the antifoundational emphasis on the fallibility of human knowledge and on the impossibility of certainty. It retains the idea that scientific beliefs and methods can and do vary across time and place, *contra* the unity of science hypothesis. It retains the idea that standards of theory appraisal may be divergent, ambiguous, or conflicting. And it retains the idea that scientific norms are essential to science.

More intriguingly, the new methodological view of science, as amended, begins to resemble economics as much as sociology or as literary criticism. The ideas that agents are self-interested and possess non-cognitive goals are decidedly economic, as are the Smithian ideas that there can be mutual gains from trade and that self-interested agents can produce collectively beneficial goals. In the remaining three chapters, I want to pursue these economic ideas as they bear on science. We depart the precincts of philosophy in order to consider science as an economic process, rather than as a strictly philosophical or rhetorical or sociological process.

In Chapters Five and Six I will argue that the analogy to a market process is fruitful for understanding key aspects of science and scientific knowledge, i.e. that we can conceive of scientists as economic agents, science as a market-like process, and, in particular, scientific institutions — its norms, standards and conventions — as helping to overcome market failures that inhere in the production of knowledge. Scientific institutions enable science, like (functioning) markets, to produce invisible hand outcomes, and also make it possible for science to be a self-adjusting process.

Given the right institutional circumstances, science is a self-adjusting process (albeit an imperfect one) that can realize its collective goals even with self-interested, epistemically sullied scientists. As with successful markets, successful sciences work not because their members are morally superior, or because they are motivated by knowledge alone, but because the institutional structure of science provides incentives for socially advantageous outcomes.

In Chapter Seven I take up as a case study a recent controversy in economic thought, the "new economics of the minimum wage." Having made a theoretical case for scientific institutions as enabling coordination and cooperation in the scientific market in Chapters Five and Six, Chapter Seven attempts to identify and explain some of the actual standards in economics which are revealed by the minimum-wage controversy. Let us turn to the second half of this dissertation and the economics of science.

Appendix 4A C.S. Peirce: antifoundationalism without indifference among rules

The new methodologists, and Richard Rorty, are sometimes given to referring to their position as "pragmatic," a reference to the American, antifoundational philosophy that arose roughly a century ago, pioneered by the American polymath, Charles Sanders Peirce. But their choice of exemplars — John Dewey and William James, not Peirce — is revealing, and helps to illustrate the differences between constructivism and other, more rationally oriented varieties of antifoundationalism, which has a history in pragmatic thought.

Peirce offers the original example of an antifoundationalism that doesn't reject reasoned choice by scientists. Though Peirce founded pragmatism, which was later popularized by his friend William James (Hacking 1983: 58), he is mostly ignored or dismissed by the new methodologists. Rorty in particular dismisses Peirce, saying that "his contribution to pragmatism was merely to have given it a name, and to have stimulated William James."[97]

Given that Peirce was a founding antifoundationalist, and a pragmatic theorist *par excellence*, a dismissive response is puzzling at first glance. Peirce conceived of truth not as privileged representations of nature (the mirror metaphor), but as the "limit of inquiry," what an ideal community of inquirers settles upon in the long run. (Rescher 1993: 23-4). Truth, for Peirce, is belief that has become fixed in the minds of a community — a stability in interpretation. (Peirce 1991: 144-59). Knowledge, for Peirce, was unavoidably fallible, though the stability provided by consensus could make knowledge reliable. Peirce, then, rejects both mirror epistemology and certainty, and even endorses something like the community-consensus view of Kuhn and the new economic methodologists.

Where Peirce departs is in his insistence that not all methods of obtaining consensus (of fixing

[97] Cited in Hoover 1994: 287. This article is a useful introduction to Peirce in the context of economic methodology. Rorty's dismissal of Peirce does not, as near as I can tell, reflect a widely-held view, indeed there seems to be a renaissance in Peirce scholarship. Popper, for one, considered Peirce to be the greatest American philosopher. (Hoopes in Peirce 1991: 4)

belief) are equally valid. After all, religious cults and totalitarian regimes also produce consensus. Peirce explicitly rejects such methods. He rejects what he calls the methods of authority (of the church, say), of dogma (what he called "tenacity"), and of *a priori* reasoning, believing whatever first enters your head (ibid; see also Hoover 1994: 296-97). These species of consensus derive from thoughtless conformity, or from knuckling under to authority, or from apathy. (Rescher 1993: 14-15).

They are inferior, Peirce argued, to consensus that results from considering the "weight and presence of the evidence, and the merit of the case." (ibid: 15). This last he calls the scientific method, by which he meant a rational process of deliberation. Says Hacking: "If you can have a method which acknowledges permanent fallibility and yet at the same time tends to settle down, then you will have a better way of fixing belief." (1983: 59).

Peirce's insight is that consensus, by itself, is insufficient. As Rescher points out, it also matters **how** consensus is produced: "It is never *just* consensus that we want, but the *right sort* of consensus — that is, a consensus produced in the right way — one that obtains for good and cogent reasons. And then we are back to putting rationality at the base of it." (Rescher 1993: 16). It is here that Peirce differs from his fellow pragmatists James and Dewey, the pragmatists who do much of the heavy lifting in Rorty's account. And it is here that we glimpse the source of Rorty's hostility.

Where Peirce is not indifferent among the ways that we make our theoretical choices, Rorty is. Ian Hacking points to this difference, when he locates the indifference in Rorty and in Rorty's intellectual ancestors:

> [James and Dewey] did not care what beliefs settled on in the long run. . . . [Rorty says that] Nothing is more reasonable than anything else in the long run. James was right. Reason is whatever goes in the conversation of our days, and that is good enough. (1983: 64).

Peirce would not have liked Rorty's rendering of pragmatism, just as he rejected its predecessor versions in James and, to some extent, in Dewey. He went so far as to change his program's name to the unwieldy "pragmaticism," in order to distinguish it from James's rewriting of the pragmatism Peirce founded (ibid: 58, see Peirce 1991: 253-9).

In particular, Peirce objected that James's pragmatism could lapse into conventionalism — whatever goes is reasonable. (Hoover 1994: 288). Conventionalism is, by definition, indifferent to the methods of inquiry. Why not merely submit to the party's line, or to the church's dogma? Peirce believed that some methods were demonstrably better. Science could progress only when it avoided inferior methods like bowing to authority or reverting to dogma. To be indifferent among the possible ways of establishing consensus was, for Peirce, irrational.

In short, if science has goals, it is unreasonable to be indifferent among the varied ways of achieving those goals. Peirce understood that methodological rules presuppose some ends of inquiry, even if those ends no longer include truth and certainty. He asks: "Ferdinand Schiller informs us that he and [William] James have made up their mind that the true is simply the satisfactory. No doubt; but to say 'satisfactory' is not to complete any predicate whatsoever. Satisfactory to what end?" (Peirce 1991: 253).

Thus does pragmatism have two branches — James and Dewey on one side, Peirce on the other. (Hacking 1983: 62). The descendants of James and Dewey — Rorty and McCloskey and Fish and Weintraub — admit no goals of inquiry. There is no purpose to science, Rorty said, save continuance. And because the constructivists don't concern themselves with what science should aim to, they are indifferent to the ways in which scientists choose. The modern descendants of Peirce, like the philosopher Nicholas Rescher, insist that such indifference is unreasonable, and perhaps self-contradictory (see Section 4.9.1 below).

The example of Peirce provides us with an antifoundational alternative that rejects irrationalism. He makes it plain that scientists cannot sensibly be indifferent among their methods if some are better than others. Abandoning the unattainable requirement of certainty is the beginning, not the end of any discussion on knowledge production. The fact of uncertainty does not compel indifference. On the contrary, if one believes that fallible knowledge is worth having, then how scientists make their choices will be important.

Chapter 5. A Smithian economics of science, part one: boundedly rational agents and institutions

> [O]ur knowledge of the way things work, in society or in nature, come trailing clouds of vagueness. Vast ills have followed a belief in certainty. Kenneth Arrow 1992: 46.

Any economics of science will need an economics, and the first question I address in this chapter is: **which** economics of science? The answer I propose is, a Smithian economics. Adam Smith's economics has several aspects that I believe are especially conducive to theorizing about science, its members (scientists), and its output (scientific knowledge). Since aspects of Smith's economics differ from neoclassical economics in ways that are important for an economics of science, I will discuss them in the context of the neoclassical framework.

First, the Smithian agent is rational, if fallible. She is rational in the sense of being purposeful and evaluative, i.e. she has goals, and evaluates the costs and benefits of alternative means thereto. She is fallible in the sense that choices are not always optimal. Second, Smith conceives of economic actors as self-interested, though not wholly self-regarding. The Smithian agent is best described as prudent: primarily self-regarding, but also concerned with the welfare of others, especially close family and friends. The Smithian agent has moral sentiments, a learned disposition to honest behavior, and will sometimes make choices that are not to her material advantage, i.e., which are not opportunistic. Third, Smith emphasizes the importance of institutions to social and economic processes; his most important theoretical results (such as invisible-hand outcomes) depend on a robust institutional structure — good rules and effective enforcement.

In this chapter I take up these three aspects of Smith's economics, in the context of modern rational choice theory. My aim is to develop a view of the rational but fallible economic agent, and also a theory of market institutions as epistemic resources that help rational but fallible agents make decisions when an optimal choice is undefined, uneconomic, or undesirable. In Chapter Six, I will apply this theoretical framework to science, which will call on other aspects of Smith's economics relevant to an economics of science (especially unintended consequences and the benefits of competition). Let us begin with a view of the boundedly rational agent, by examining her opposite, the unboundedly rational agent of neoclassical economics, which is the home port of rational choice theory.

5.1 The neoclassical model: unboundedly rational agents

As usefully presented by Geoffrey Hodgson 1994, neoclassical economics can be characterized by three core propositions. They are:

> (I) the assumption of rational, maximizing behavior by agents with given and stable preference functions;
> (II) the focus on attained or movements towards (unique) equilibrium states;
> (III) the absence of serious information problems (no Knightian uncertainty).

These core propositions are what David Kreps, in a recent survey of economics, calls the "canonical principles" of neoclassical economics. (1997).

The canonical principles are interconnected. In many settings, proposition (I) implies proposition (II), i.e., a market of rational maximizers will generally tend towards a unique equilibrium, though, as we will see, this is less likely with paradoxical results that sometimes obtain in game theory and in others models with expectations. Proposition (I) also, to some extent, assumes proposition (III), because it is only without serious information problems that agents can calculate their way to optimality — maximization generally requires complete (if not perfect) information. (Hodgson 1994:

60). With reference to the canonical principles, let us consider rationality, maximization, and information.

5.1.1 Rationality and maximization

Take proposition (I) first. "Rationality" has many different constructions. In our discussion to date, we have used rationality in its everyday, ordinary-language sense — the application of reason. Economists typically mean something different by "rationality;" sometimes we use "rationality" as a synonym for self interest, more often, we use rationality in the sense of instrumental or "thin" rationality.[98] Instrumental rationality consists of choosing the best means to realize (given) goals or objectives. The philosopher Bertrand Russell puts it this way: "'Reason' has a perfectly clear and precise meaning. It signifies the choice of the right means for the end that you wish to achieve. It has nothing whatever to do with ends." (Cited in Black 1990: 97). It is no accident that Robbins' definition of economics — "the science which studies the relationship between ends and scarce means which have alternative uses" (Robbins 1932: 15) — relies on this instrumental conception of rationality.

Note Russell's assertion that objectives or ends are "given," which is to say, they are not themselves the product of reason. With an instrumental conception of rationality, reason's role consists only in selecting best means, not in determining or judging ends themselves. This is what David Hume refers to in *A Treatise of Human Nature* when he characterizes reason as the slave of the passions. In economics, actions are rational to the extent that they satisfy already existing objectives, which are said to be exogenous.

Rational choice theory is sometimes silent on the **basis** for preference orderings. Modern consumer theory, for example, uses utility indices, which are rank orderings without substantive

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[98] Edgeworth argued in *Mathematical Psychics* (1881), that "the first principle of economics is that every agent is actuated solely by self-interest." (Cited in Hollis 1987: 60).

content. The basis for the ordering of preferences is unspecified. Hence, there is no actual quantity maximized, only a function. This is in contrast to classical economics, which had a more substantive view of rationality. Jeremy Bentham, for example, also uses the term "utility," but he conceived of utility in substantive terms, as the quantity of pleasure over pain. The Benthamite agent is trying to maximize an actual (hedonic) quantity, net pleasure.[99] Of course, modern rational choice theory retains substantive goals in the theory of the firm. Firms are said to maximize profits, which is an actual quantity.

Modern rational choice theory is instrumental, but gives "rationality" a more specialized meaning, which leads to the idea of "maximization." Choice theory begins with some assumptions about an agent's objectives, or, in modern terms, preferences over objectives. If preferences meet a few axioms — (1) commensurability (2) complete specification, (3) transitivity, and (4) continuous substitutability — then they can be rank-ordered into an index of preferences, and one can represent this index of preferences as a mathematical (utility) function, up to an increasing monotonic transformation.

In expected utility theory (EU), agents order prospects, where a prospect is a combination of an expected outcome, and the estimated likelihood (or probability) that it will occur.[100] Because intertemporal decision making requires making a forecast of future utility effects, it is conventional

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[99] Though a pervasive practice, it is misleading to refer to a preference ordering as a "utility" function. The reason is that "utility" traditionally refers to pleasure or some other quantifiable (or cardinal) entity, as with Jeremy Bentham's psychological hedonism. (See Georgescu-Roegen 1973, for a characteristically erudite discussion that finds antecedents in Plato). Edgeworth, in the Benthamite tradition, advocated modeling human beings as "pleasure machines" (1881: 15). In modern rational choice theory, however, consumers do not choose X over Y because the utility of X is greater than that of Y. On the contrary, it is because agents prefer X to Y, that a "utility" function satisfying $u(X) > U(Y)$ is chosen. (Binmore 1992: 97-98). For more on the conflation of (1) maximizing pleasure (or some other substantive element) and (2) choosing higher-ranked alternatives, see Broome 1991.

[100] Subjective expected utility theory, as outlined in Savage (1954) especially, refers to the case where agents form subjective probability distributions over future outcomes.

to assume that preference orderings are stable over time. This assumption is more a concession to analytical tractability, than it is a theory of the fixity of human wants.

In modern choice theory, the rational agent is one who behaves consistently in accordance with this single, complete, transitive preference ordering, and, in so doing, it is **as if** the agent is maximizing a (utility) function. Rationality in this sense consists of choosing the highest attainable point on a preference ordering, i.e., doing what one most prefers, subject to any constraints. Note well, however, that rationality and maximization are not the one and the same thing, which is why proposition (I) requires both terms.

Behavior can be rational without being optimal, and optimal behavior is sometimes irrational (what Amartya Sen 1977 calls rational foolishness), as we will see below. Generally, we should be careful to distinguish sub-optimality from irrationality (or non-rationality) in the ordinary language sense. Economists sometimes conflate the ordinary-language meaning of rational — acting with reasons — with our own technical definition, choosing what one most prefers. But when agents fail to choose optimally, it does not follow that they are acting without reason. There are many ways in which agents fail to choose optimally that don't entail irrationality — with incomplete information or under excessive complexity, for example. The point is simple, but it bears repeating: human beings can apply reason to their circumstances, whether or not optimality is attainable.

5.1.2 Unlimited information or Knightian uncertainty?

Proposition (III) in neoclassical economics assumes that agents don't face serious information problems when making decisions, i.e. that information is always sufficient for maximization. In the context of rational choice theory, this requires a number of assumptions about the information that agents' possess. The neoclassical agent (1) is assumed to have preferences which are accurately described by the axioms of rational choice theory. In addition, the neoclassical agent (2) has complete information regarding the "external" environment as well, that is, knows income, price, time and other constraints. In a strategic setting, the neoclassical agent (3) is assumed to possess information about the structure of the game (payoffs, timing) and about other players' beliefs and rationality. In an intertemporal setting, the neoclassical agent (4) is assumed to know all possible future events and the associated probabilities. Once this information is in place, i.e., once an objective function and constraints are specified, rational choice is a strictly mechanical procedure.

The adjective "Knightian" refers to Frank Knight's (1921) famous distinction between risk and uncertainty. In Knight's scheme, situations of uncertainty are those in which agents cannot define a complete probability distribution over all possible outcomes, or states of the world. Knight argued that uncertainty occurs when "the conception of an objectively measurable probability or chance is simply inapplicable." (1921: 231). Risk is measurable; uncertainty is not. (ibid: 205). John Maynard Keynes understood uncertainty in a similar fashion — the **absence** of a fully specified probability distribution, the case where "there is no scientific basis to form any calculable probability whatever." (1937). Risk, on the other hand, characterizes situations where agents **do** possess a complete probability distribution over all possible outcomes, and, therefore, optimal calculation is always possible.[101]

Twentieth century advances in modeling what agents can know almost wholly rely on the probabilistic approach. Economics in the last fifty years has seen an explosion of different theoretical projects that relax the traditional assumptions of perfect knowledge and perfect foresight. In most instances, however, theory has simply replaced complete knowledge about future events with complete knowledge about the probabilities of those events. (Winston 1988: 3).

In developing EU theory, for example, Von Neumann and Morgenstern (1944) did just this,

[101] The adjective "Knightian" would seem redundant, but modern economics (EU theory, for example) has mostly ignored Knight's distinction between risk and uncertainty, so much so that "decision making under uncertainty" has come to mean "decision making under risk."

exchanging known bundles of commodities for known bundles of prospects.[102] Their analytical accomplishment was to preserve the formal structure of the old models with deterministic certainty, while accommodating risk (Winston 1988: 38). Rational expectations theory does much the same thing. As Kenneth Arrow points out, rational expectations are simply a stochastic form of old-fashioned perfect foresight (Arrow 1987).

The introduction of risk acknowledges that agents do not have perfect foresight. Their information may be imperfect. Agents may bet red only to have the roulette wheel come up black. But neoclassical agents **are** assumed to know all possible outcomes (or states of the world), and the relative likelihood of each outcome. In other words, information is always complete, if not always perfect. When the neoclassical agent bets red, she is not certain that the wheel won't come up black or green, but she is certain that, *ex ante*, her choice was optimal. The agent with complete information may not know what **will** happen, but she knows everything that **could** happen (and the respective likelihoods).

This difference between **imperfect information** and **incomplete information** corresponds to the distinction between choice under risk and choice under uncertainty. The agent who chooses with complete information never experiences Knightian uncertainty. As long as information is complete, she knows that her choice was (*ex ante*) optimal. With merely imperfect information, optimality is never threatened — choice remains determinate, even if the computations are a bit more tedious.

The assumption that agents possess complete information is especially demanding in situations where agents must form expectations, as in strategic environments. In game theory, the neoclassical agent must possess information about the game structure (payoffs, timing of moves) and also must forecast other players' actions by forming expectations about their information sets, their objective functions and their "rationality" (i.e., how they will act, given their information and objective

[102] Von Neumann and Morgenstern's terminology is revealing: prospects or expected values are called "certainty equivalents." (Winston 1988: 37).

functions). Though complete information of this variety is often hard to come by, modern game theory assumes that agents possess it.

Following John Harsanyi's innovation, game theory treats incomplete information (which could entail Knightian uncertainty) as always reducible to complete but imperfect information (which entails only risk). An agent may not, for example, know a fellow player's "type" (always defects, always plays tit-for-tat), but she is assumed to know a probability distribution of player types in the population. Even a Bayesian game, which permits players to have subjective probability distributions over player types, and to "learn" via Bayesian updating, requires a strong, auxiliary assumption — that all players begin with identical probability distributions. This assumes that all players begin with "common priors." (See Aumann 1987). The Harsanyi "transformation," by positing a known probability distribution that allows a determinate (maximizing) calculation to be made, renders Knightian uncertainty into mere risk. (See Rasmusen 1989: 55-60).

Game theory makes still greater informational demands, however, because it is not enough that agents know the game structure, information sets, objective functions, and "rationality." Agents must also know the extent to which this all this information is shared among the other players. That another player knows some fact X, is itself a fact worth knowing (Werlang 1987), as is the fact that I **know** that another player knows fact X. If players are to optimize, the higher-order knowledge of knowledge seems to result in an infinite regress.

Game theorists have addressed the infinite regress problem by assuming "common knowledge." (See Lewis 1969, Binmore 1990). A fact is common knowledge if it is known to all players, each player knows that all of them know it, each of them knows that all of them know that all of them know it, and so on, *ad infinitum*. (Rasmusen 1989: 50). Common knowledge is therefore mutual knowledge of degree infinity.[103] In particular, it is assumed that players possess "common knowledge" of both the game's structure, and of other players' information, objective functions, and "rationality."

These very strong assumptions demonstrate how demanding the requirement of complete information can be. Game-theoretic settings, and other situations which require expectations formation, such as intertemporal choice, are especially taxing of the third canonical proposition of neoclassical economics.

We now turn to an examination of the ways in which maximization can go wrong, i.e., the ways in which the canonical propositions of neoclassical economics might not be met.

5.2 Five ways that maximization can go wrong: boundedly rational agents

Optimal calculation can fail in a variety of decision making settings, which, without too much simplification, can be organized under five different scenarios. The five ways in which maximization can go wrong are as follows (I have included in parentheses a representative instance):

(1) maximization is impossible (incomplete information);
(2) maximization is possible but insufficient for choice (multiple equilibria);
(3) maximization is theoretically possible, but practically impossible (complexity and time constraints);
(4) maximization is possible but uneconomic (costly decision making);
(5) maximization is possible but leads to Pareto-inferior outcomes (Prisoner's Dilemma games).

Let us take each scenario in turn.

In the first scenario, the decision-making problem is poorly defined, generally a problem of incomplete information. There are, of course, many ways in which information can be insufficient for optimality, but this scenario will most often arise in complex decision-making environments, where

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[103] If I know that you know what the game form looks like (and you know that I know what the game form looks like), then there is mutual knowledge of degree one. But mutual knowledge of degree one is far less extensive than common knowledge. I do not know whether you are aware that I know you know the game form, which would entail mutual knowledge of degree two.

the informational requirements of rational choice theory are most demanding. Information will be incomplete when, for example, agents cannot devise a single, complete, transitive preference ordering, or when they lack complete probability distributions over future states of the world, or when they lack knowledge of their future utility functions, or when they lack knowledge of what other players know, or when they lack common knowledge of the game structure (payoffs, timing of moves), or some combination of these and other informational shortcomings.

As noted, the problem of incomplete information is most acute in intertemporal and in strategic decision-making environments, i.e. when agents must form expectations about the future and about the knowledge and rationality of other agents. These information environments tend to be complex and are potentially unstable. As computer scientist and complexity theorist John Holland puts it, "[nature] doesn't care whether problems are well-defined or not." (Waldrop 1992: 254).

In the second scenario, the problem is well-defined, i.e. agents have complete information, but optimal calculation is insufficient because it does not yield a unique choice. The most common manifestation of this scenario is the problem of multiple equilibria. With multiple equilibria, agents face more than one acceptable outcome, and have no unambiguous basis to prefer one to the other. Take the example of the "driving" game (which side of the road should I drive on?), a two-person coordination game with symmetric payoffs that yields two pure-strategies Nash equilibria, and no strictly rational basis to select either. A (known) convention like "drive left" solves the game so trivially that it's easy to forget that this "simple" coordination problem cannot be solved by unbounded calculation alone.

Multiple equilibria are common in economic models with a recursive structure, such as models with rational expectations. (Hahn 1980, Hargreaves Heap 1991). In strategic settings, multiple equilibria are routine. It is now a commonplace in game theory to recognize the "Folk Theorem." The Folk Theorem says that, at least for any infinitely-repeated game, there are essentially an infinite number of (subgame-perfect) equilibria. (Rasmusen 1989: 92). Optimality is very difficult to achieve in this situation, because even costless, unbounded calculation is insufficient for a unique choice.

In the third scenario, optimal calculation is possible and is also theoretically sufficient for locating a unique solution to a given choice problem. The problem here is that, though a theoretical solution exists, it cannot be calculated. Some very well-defined problems are nonetheless beyond the practical limits of human computation. The game of chess is a paradigmatic illustration here. David Kreps sketches the problem as follows:

> Chess is a finite game of complete and perfect information. It has a finite game tree. So all we need to do is to draw out the game tree for chess and, starting from the back (one move from the end of the game), work out the best move for whomever has the move, and then, node by node, work our way to the start of the game, finding the 'optimal strategy' for both sides. Instant grandmastership! (1990: 54-5).

All one must do is reason backwards to the root of the decision tree (and assume that one's opponent is capable of and is, in fact, doing the same thing). A determinate, optimal strategy exists, no less than it does in the game of tic-tac-toe.

The catch, of course, is that the number of game-tree branches to search lies somewhere around 10^{20} or 10^{40}, numbers vastly beyond the limits of human computation. (Simon and Schaeffer 1992). A theoretical solution exists in chess, but it cannot be determined in practice. Hence, "the game is too complex for backwards induction . . . [because it] strains the ability of players to examine completely all the options they have." (Kreps 1990: 136, 146). In fact, it is the practical impossibility of obtaining the theoretical solution which ensures that chess remains an interesting game, unlike, for example, tic-tac-toe.

There are also some settings where a solution can be obtained, but not in the time allotted for making a decision. In these situations it is time, rather than cognitive limits, that creates the computational constraint, and the ensuing suboptimal outcome. In either case, the key insight here is economic: cognitive resources are scarce. The upshot, in Herbert Simon's (1987) terminology, is that agents should be seen as boundedly rational. "[H]uman beings," says Simon, "are limited in knowledge, foresight, skill and time." (1957: 199).

This takes us to the fourth scenario, which says that even decision-making problems which don't exceed the constraints of finite cognitive resources (as chess does), may be undesirable to solve optimally. The economic insight here is that decision making (information processing and computation) is costly. Whether or not a decision exceeds our physical cognitive resources, decision making should be seen as costly — otherwise we are assuming a cognitive free lunch.

This economic insight is no different, though less familiar than the idea that information collection (search) is costly. (Stigler 1961; Conlisk 1996). When the marginal net benefits of more decision making are deemed negative, then the agent should not allocate any more resources to a given decision.[104] A decision-making rule of thumb may provide an adequate decision more cheaply than more elaborate methods (when the costs of decision making are included). The idea of costly decision making is unfashionable, but it is not new in economics. Frank Knight said in 1921: "It is evident that the rational thing to do is be irrational, where deliberation and estimation cost more than they are worth." (Knight 1921: 67, cited in Conlisk 1996: 686).

An ancillary point to note here is that "bounded rationality" is a something of a misnomer (though, observing convention, I will sometimes use the term). Generally, rationality is not bounded; it is information and or cognition that is bounded. Agents can be fully rational (i.e., purposefully evaluating costs and benefits) without obtaining optimal results. Optimality is not a characteristic of rationality *per se*; it is simply the limiting case when agents costlessly deliberate with a complete information set.

Scenario five is not, strictly speaking, a problem with optimal calculation *per se*. It is not so

[104] Analogous to optimal search, the marginal benefits of additional decision making are likely to be hard to estimate.

much that optimality is unattainable, or even uneconomic, as it is, paradoxically, undesirable. The most famous instance that scenario five refers to is the Prisoner's Dilemma, wherein the (apparently) optimal individual choice leads to a Pareto-inferior outcome. The (finite game) outcome in Prisoner's Dilemma games is vexing for rational-choice theorists because, while it is (in some sense) logical, it is clearly not sensible (Schotter 1996). Pareto-inferior results also occur in a (finite) centipede-type game, again owing to the logical, but "unreasonable" results of backwards induction.[105]

We should also note that the five scenarios need not be mutually exclusive. Optimality can fail in multiple directions, especially in complex decision-making environments. An optimal choice may be undefined (scenario one) or unattainable (scenario two and three) or undesirable (scenarios four and five), or it may entail some combination of these possibilities.

5.3 What to do when optimality is undefined, unattainable or undesirable?

There is, of course, a large experimental literature that documents the many ways in which *homo sapiens* routinely falls short of *homo economicus*. This literature is so large that there are books and long survey papers written just to summarize it. (See the survey references in Conlisk 1996: 670. For small surveys directed to economists, Conlisk cites Lowenstein and Thaler 1989, Tversky and Thaler 1990, and Kahneman et al. 1991).

Our own behavior as economists also provides evidence that maximization is often unrealizable. Economists, Milton Friedman 1953 argued famously, don't know enough. We confront an endlessly complex world, and it is therefore necessary to simplify, to form unrealistic or even false assumptions, to ignore certain information (the *ceteris paribus* condition), and to adopt other such procedures, generally in the name of analytical tractability. In other words, we use the fiction of

[105] In their influential 1957 text, Luce and Raiffa argue that, at least for repeated games, the Pareto-inferior backwards-induction result is "not 'reasonable'" as a solution, saying "it is not 'reasonable' in the sense that we predict most intelligent people would not play accordingly." (1957: 101)

maximization et al. because it is easy to implement, not because it theoretically optimal. Economists therefore, as Oliver Williamson notes, are "analytical satisficers." (1997: 130). When the cost of solving theoretical problems become prohibitive, it makes sense to use imperfect tools. (Ghiselin 1989: 35).

The experimental literature and our own choices as economists are compelling evidence against maximization, but I want to emphasize that the **theoretical** objections to maximization I have enumerated are themselves economic in nature. Most fundamental is the idea that human cognitive resources are scarce. Hence, decision making is necessarily costly. The claim here is that scarcity is a more fundamental economic idea than is maximization, which assumes a cognitive free lunch. In addition, I make use of another fundamental economic idea, that agents respond to incentives. If agents know that maximization is undefined, unattainable or undesirable, then there are incentives to locate alternative decision-making resources.

Hence, the point of the catalogue of the ways in which maximization can fail, is not only to argue that the canonical assumptions are unrealistic. All economists know that the neoclassical propositions are a fiction; we don't really suppose that agents set up and solve complicated LaGrangean and Hamiltonian problems. The relevant question is whether they are a **useful** fiction, i.e., does they work to meet worthwhile scientific goals within economics, and do they do so in a way that is superior to other theoretical strategies? So while this debate is red meat for methodologists and for critics of conventional rational choice theory, I take a slightly different tack, and ask the following question: if agents don't optimize, but they are rational, what other decision-making resources (if any) do they have?

One possible answer is "none." The implication of this response is that if an optimal choice is undefined, or unattainable, or undesirable, then the only alternative for the agent is random or irrational choice. Robert Lucas 1977 seems to argue in this fashion. Lucas is discussing whether it is reasonable to suppose that agents have complete probability distributions. He suggests that a complete frequency distribution can be developed over time, when "there are repeated instances of essentially similar events." (1977: 11). But agents will sometimes encounter decisions that don't look very familiar and that don't entail essential similarity.[106] As Richard Thaler argues, "the most important life decisions: human capital formation, marriage, saving for retirement, job-search, home purchase, etc., are all both difficult and *rare*." (1996: 235, emphasis added). In these instances, agents face Knightian uncertainty, not just risk. Lucas's response is dismissive: "In cases of [Knightian] uncertainty, economic reasoning will be of no value." (ibid: 15).

The problem with this response is that actual economic agents don't have the luxury of abandoning economic reasoning. Economic life continually imposes choices, and doesn't pause when agents lack the resources (such as a complete probability distribution) for optimal choice. Human agents must act, even when they are uncertain, a point that Keynes emphasized.

More importantly, it is not reasonable to suppose that rational agents are unaware of their informational (or cognitive) shortcomings. Sir John Hicks makes the point as follows: "one must assume that the people in one's models do not know what is going to happen, and know that they do not know what is going to happen." (Hicks 1979: 91). Nor is it reasonable to suppose that agents do not act on the incentives to make good decisions when they know that optimality is precluded. If agents know that they cannot make optimal choices, then there are strong incentives for them to locate other decision-making resources. If we grant that, in many settings, agents cannot maximize, it is reasonable to assume that agents themselves know this, and that, in response, they have devised (or

[106] The answer will depend, of course, on what qualifies as "essentially similar." George Shackle, a proponent of radical uncertainty, argues that many important decisions in life (what he called "crucial decisions") cannot be seen as "essentially similar." Other (most?) economists believe that all decisions are essentially similar in kind. Gary Becker, for example, argues that choosing a spouse is no different in kind from choosing a can of paint. (Becker 1976).

make use) of potential substitutes.

I want to propose institutions as a rubric for these alternative decision-making resources. There are several different literatures in economics, new and old, that conceive of institutions in this fashion, if sometimes employing different terminology. The behavioral or psychological literature (Simon 1957, Kahneman and Tversky 1979, Thaler 1992), for example, begins with bounded rationality, and emphasizes the efficacy of decision-making heuristics or rules of thumb in overcoming finite cognitive resources. There is also an interesting literature that considers conventions as important devices for coordinating in strategic settings where uncertainty, especially in the form of multiple equilibria, obtains (Schelling 1960; Ulmann-Margalit 1977; Schotter 1981; Sugden 1986, 1989; Elster 1989; Young 1996).

The New Institutional literature (Coase 1960, 1988; Demsetz 1967; Langlois 1986; North 1990; Williamson 1975), a third example, has long emphasized the importance of formal rules, such as property and other rights in the common law, recognizing their importance for economic processes.[107] There is also what we might call the collective action literature, which studies the incidence of cooperation where theory predicts non-cooperation. Norms are integral to cooperative behavior in these settings: contributing to public goods rather then free riding, avoiding "the tragedy of the commons" with common property resources, and avoiding Pareto-inferior results in finite prisoner's-dilemma games. (Ostrom 1990; Smith 1994; Axelrod 1984).

What unites these different literatures in economics is a recognition that institutions — rules

[107] The New Institutionalists are somewhat eclectic, but they all begin with some measure of bounded rationality and treat institutions as the products of market processes, which induces them to explore the intersections of economics with law and with history. I cannot claim that I have selected the most inclusive name — other labels used, of varying relevance, are Property Rights economics, and Transactions Cost economics. (On programmatic differences and similarities, see the symposium in the March 1993 *Journal of Institutional and Theoretical Economics* 149(1), with contributions by North, Coase, Williamson, and Richard Posner). I merely identify notable economists who take seriously the ideas of fallible agents and the importance of rules to human action.

of thumb, conventions, laws, norms, standards — are vitally important decision-making resources in a world where optimality is undefined, unattainable or undesirable. Because economics lacks a general name for the different literatures that have institutions as a common element, I adopt the name of the economist who has inspired (at least in part) all of these literatures — Adam Smith. We now turn to three related tasks concerning institutions: a definition, an examination of different types of institutions and their characteristics, and an examination of the extent to which institutions enable agents to overcome (or at least to mitigate) situations where optimality is unattainable, uneconomic or undesirable.

5.4 What are institutions?

"Institution," like "rationality" and "utility," is a term that has multiple, overlapping meanings in economics. The "old" institutionalist literature[105] (Thorstein Veblen, John Commons, Wesley Clair Mitchell) generally conceived of institutions as persistent individual habits that are shared and reinforced within a social group, i.e., socially habituated behavior. (Hodgson 1994: 64). I prefer to conceive of institutions not as conventional behavior *per se*, but as the rules that guide or constrain behavior. Institutions are rules, what Douglass North terms "the rules of the game in a society or, more formally, [the] humanly devised constraints that shape human interaction." (1990: 3). Sometimes it will be useful to conceive of an institutional framework as the rules plus any associated enforcement mechanisms. I will employ "rule" as a synonym for "institution."

Institutions are rules, and should also be distinguished from **organizations** such as firms, trade unions, political parties, governments, universities, clubs, etc. Conceptually, the distinction between organizations and institutions is the difference between the players and the rules. (North 1990: 4). Though economists sometimes speak colloquially of some organizations as institutions (as in, "the

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[105] For a useful, book-length treatment of Old Institutionalism, New Institutionalism, and their similarities and differences, see Rutherford 1994.

FED is an influential institution"), an organization is an agent, or more precisely, a group of agents. A firm, for example, can be said to observe the law; it will likely have internal norms, and it may behave conventionally, but firms are not themselves institutions in my sense. Organizations such as firms make rules, follow rules, enforce rules, misunderstand rules, and are ignorant of rules. But we should not say that they **are** the rules.

As already noted, there are several types of institutions — heuristics, conventions, norms, and standards are all different types of institutions. Though there is some overlap, I will distinguish among them as follows. A decision-making **heuristic** is a rule of thumb that agents follow when they lack the ability to reach an optimal decision, perhaps due to computational complexity, or to incomplete information.

Say I am faced with *n* possible states of the world, but I lack a probability distribution. One rule of thumb (for estimation) is to assume the all outcomes are equally likely, i.e., assign each outcome a likelihood of $1/n$. Or, if I am uncertain as to the optimal level of portfolio diversification, I may opt to follow the old rule of thumb that allocates one-third of total assets to equities, debt, and cash, respectively. Heuristics generally work to facilitate individual decision making in the face of cognitive or informational shortfall, and have no social intent.[109] (See the papers in Kahneman, Slovik and Tversky 1983 for a compendium of decision-making heuristics).

A **convention** is an institution that specifies behavior, often in a social coordination setting. Conventions emerge spontaneously from some social process, and they are customary, expected and self-enforcing. (Lewis 1969; Young 1996). All agents observe the convention (conform), all expect others to do likewise, and all have incentives to conform, given the expectation that everyone else will

[109] They may, however, have social **consequences**. When individuals make use of decision rules to guide their decisions, their behavior becomes more predictable. The predictability of other agents is important when making decisions in strategic settings. (See Heiner 1983).

too. (Young 1996: 105).

A **norm** is an institution with ethical connotations, often intended to promote social cooperation, where there are individual incentives to behave opportunistically. Norms may also emerge spontaneously, and in some accounts are seen as conventions that have been invested with ethical import over time. (David Hume, for example, conceives of morality as conventional). There is evidence from experimental division games, for example, which suggests that subjects rationalize divide-the-pie outcomes as fair, even when the outcomes are peculiar to the particular history of play in a particular group. (Young 1996: 116; see also Roth 1987). Unlike conventions, however, norms are not self-enforcing; some agents will deem it in their interest to break such rules, especially in the Prisoner's Dilemma and related strategic situations.

Standards take two meanings. A standard may be a common industrial technology or process, such as VHS or Beta, DOS or Macintosh — call this a **technical standard**. A standard may also be a **critical standard**, that is, an evaluative benchmark that underwrites informed judgment. For example, one might (correctly) say that VHS is the standard in the video cassette recorder (VCR) market, while, at the same time, making the critical judgment that Beta is the technically superior system. The judgment that Beta is better requires at least implicit reference to a critical standard — whether that standard is technological, or aesthetic or ergonomic, or some combination.

In economics and in other sciences, we have already seen, there are critical standards which apply to theories — standards of theory appraisal. When appraising rival theories, an economist will consider which theory best meets her methodological standards of good theories. Standards, in this sense of informed judgment, are meant to distinguish better from worse theories. Generally, all reasoned criticism or judgment will make use of critical standards, whether such appraisals occur in science, or elsewhere.

5.4.1 Explicit and implicit institutions

Institutions may be formal constraints, like statutory law, or they may be informal constraints, like conventions or norms. (North 1990: 3). Formal institutions, in this sense, are rules which are explicit, and which have an established enforcement mechanism. Explicit rules are codified and written down; a rule book exists. Examples are the U.S. Constitution and the Rules of Major League Baseball. Implicit rules are not necessarily less binding, but they are probably not written down, and are less likely to have an established enforcement mechanism.

Explicit rules typically come with a designated set of paid enforcers, such as law enforcement officials or baseball umpires. Those charged with enforcing the rules generally have incentives to do so — their income, security, and employment are at stake. (Elster 1989). In settings where there are no (or few) professional enforcers, enforcement can be more difficult, as it can be costly for individuals to enforce the rules. Will a student, for example, report violations of a university honor code, if doing so requires public testimony, say? To the extent that implicit rules are less likely to have paid enforcers, any required enforcement will probably be less frequent.[110]

In prior chapters, I argued that implicit rules are often no less important than those which are written down. The "rules" of economics exist, even though there is no rule book where they are enumerated. Economics has no canonical document where there are commandments on how to do good economics (procedural rules), or on how to choose among competing economic theories (standards of appraisal). Nonetheless, these institutions exist; there are standards of what constitutes good economics. Although implicit, these standards are those that referees and editors and dissertation

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[110] Enforcement does, however, occur. I have witnessed truck drivers risk their place in the traffic queue (owing to road construction) in order to block opportunists from advancing in the emergency lane. These agents are engaging in costly behavior to enforce a norm against queue jumping, behavior which can be seen as observing a meta-norm, which says that norms should be enforced. Incentives for enforcement probably attenuate at higher orders. Will, for example, student X report student Y for not reporting a cheating incident? Will Z report X for not reporting Y, who failed to report the cheating? (Axelrod 1986).

supervisors employ in making their critical judgments. Absent such standards, all judgment would be random or entirely subjective. So methodological standards in economics exist, even if they are not assembled in explicit, rule-book fashion.

In some settings, explicit and implicit institutions coexist. In these situations, knowing only the explicit rules of a game will be insufficient for informed play. As an illustration, consider the game of baseball. Baseball is awash with formal rules. A rule book exists, and umpires are employed to enforce the explicit rules during play. For example, if the batter gets three strikes, she is out, unless the third strike is a foul ball, or is dropped by the catcher. A knowledge of baseball, however, is not obtained by mastering the rule book.

To know baseball, one must also learn the many implicit rules, the constituent knowledge sometimes called "inside baseball." A newcomer to the game can understand the formal, codified rules perfectly, and have no understanding of the game whatsoever. What are some of these implicit rules? Let me offer a few examples.

5.4.1.1 Implicit rules, an example from baseball

Implicit rules in baseball are the rules that are not formally codified, but which exist and function as rules. Consider the following examples.

1. Base runners cannot overtly attempt to distract the pitcher with verbal abuse ("razzing") though distraction in the form of feigned base stealing is permissible.

2. If a pitcher is perceived to deliberately attempt to hit a batter with a pitch, the opposing pitcher will retaliate, in kind.

3. If a successful batter struts, taunts or otherwise addresses the pitcher, the pitcher will later retaliate by deliberating attempting to hit the batter.

4. At the beginning of the game and in the middle of the seventh inning, the crowd will rise *en masse* and sing a designated song.

None of these rules is anywhere written down, though they are widely observed, and I believe that "insiders" would be able to identify and confirm them. We can consider them conventions or standards. In Chapter Seven, I will attempt to identify some implicit standards in economics, by an examination of the recent minimum-wage controversy.

What makes an institution an institution is not its substantive content, nor its transcription somewhere, but, rather, the fact that agents tend to observe it. (Fuller 1993: 321). This is not to say that all rules are always followed; in some cases, especially with norms, there are clear incentives not to. The point is that rules can be implicit, and be no less binding for that fact.

Note that violations of the implicit rules in baseball are policed (see examples 2 and 3) not by umpires, the official enforcers, but by the players. Some implicit rules, then, also embed a "meta-norms" of enforcement, or a rule about how to enforce implicit rules. (See Axelrod 1986). This leads to an additional point regarding institutions, one we have already made in the discussion of standards of theory appraisal. It is this: all rules must be interpreted, even explicit ones.

5.4.2 Institutions and the necessity of interpretation

Institutions, like all rules, must be applied to actual cases. In that application, interpretation is unavoidable. This is clearly true for implicit institutions, which usually take time to learn, and cannot be obtained merely by reference to the rule book. But the fact of interpretation also applies to those instances where a rule book is at hand, i.e. when the rules are explicit. Supreme Court Justices and Constitutional scholars, for example, make careers interpreting the explicit rules written in the U.S. Constitution. If the rules "spoke for themselves" there would be no need for Constitutional jurisprudence, nor would there be competing views on how the Constitution should be interpreted. But there are, of course, competing views on Constitutional interpretation.

Activists tend to want to interpret the Constitution loosely, to hew less strictly to the "letter of the law." Non-activists prefer to read the Constitution narrowly, to rule out readings that are only within "the spirit of the law."[111] Curiously, the Constitution itself is silent on how it is to be read.[112] Hence, all theories of Constitutional interpretation are extra-textual, that is, are derived from outside the document itself. Even if the Constitution were explicit on how it was to be interpreted, those explicit rules of interpretation would themselves need interpreting (though the range of possible theories of interpretation might be narrower).

The point is that interpretation is unavoidable. Interpretation of explicit rules may be easier and less contentious than interpretation of implicit rules, but interpretation is required nonetheless. That is why there are markets for professional interpreters of the rules — markets that employ judges, Justices, scholarly referees, review boards, and baseball umpires. Let us return to the humble baseball example for a final illustration of the consequences of interpretation over time.

In baseball, decades of interpreting the explicit rules have lead to *de facto* rules that are quite different from the *de jure* rules found in the rule book. Years of interpretive "precedent" in baseball have created *de facto* rules that are different from the *de jure* rules enumerated in the rule book. Consider the examples presented in Table 5.1

Table 5.1. The consequences of rules interpretation: *de jure* and *de facto* rules in baseball

De jure rule (what the rule book says):	*De facto* rule (what the prevailing interpretation says):
1. The pitcher may not balk	1'. The pitcher may not balk too egregiously
2. A fielder must always touch the base to force the base runner out	2'. When attempting a double play, the fielder must at least feign a touch of second base

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[111] Activism is a preference regarding interpretation; it has no necessary political implications. Activists may be politically conservative or liberal. The activist Warren Court of the 1960s is considered politically liberal, the activist Lochner Court of the 1920s is considered conservative.

[112] In fact, the U.S. Constitution nowhere grants the Supreme Court an explicit charter to review the constitutionality of Congressional legislation. This power, the power of judicial review, was, in effect, arrogated by the Court, when (in the famous case of *Marbury v. Madison*) it **interpreted** the Constitution as granting it the power of judicial review.

Table 5.1. The consequences of rules interpretation: *de jure* and *de facto* rules in baseball	
De jure rule (what the rule book says):	*De facto* rule (what the prevailing interpretation says):
3. A base runner may never interfere with the fielders	3'. When attempting to foil a double play, the base runner must at least pretend not to tackle the middle infielder
4. The batter must always bat inside the batter's box, as marked by chalk lines	4'. The batter must not bat too far outside the batter's box, which she will erase with her foot as soon as the game commences

There are many other examples. Occasionally, baseball will use its authority to attempt to reestablish a *de jure* rule that has been superceded by its *de facto* version. This occurred when the American League tried to enforce the "balk rule" (item number one in Table 5.1) as it is actually written, rather than as it actually operated. The attempt was a failure. Players and umpires were too well adjusted to the *de facto* rule, even though it is only implicit, and could not make the switch. The attempt to resurrect the "official" balk rule was quickly abandoned.

Now we turn to conventions and norms, two types of institution that are especially important for social purposes, coordination and cooperation, respectively. Decision making heuristics, in contrast, are institutions that help the individual to make decisions when she is unable to choose optimally. The same is often true for conventions and norms, but they work, in addition, to serve the social (or collective) goals of coordination and cooperation. Conventions are taken up in Section 5.4.3 and 5.4.4.; norms are considered in Section 5.4.5. The focus is on coordination and cooperation, along with the issue of enforcement.

5.4.3 Conventions and coordination

Consider a coordination game again. In the "driving game," to use our recurring example, agents must choose which side of the road to drive on, a decision which naturally depends on what other drivers choose. The strategic form is depicted in Figure 5.1. There are two pure-strategies Nash equilibria, {L,L} and {R,R}, neither of which is a unique solution, a classic problem of multiple equilibria.[113] Even the unboundedly rational agent cannot reason to a determinate optimal solution. But a convention like "drive on the right side of the road" can, once established, enable a solution.

Figure 5.1. The Driving Coordination Game

		Beta	
		L	R
Alpha	L	0,0	-1,-1
	R	-1,-1	0,0

The convention, in this instance, works where even unlimited calculation cannot. Conventions like "drive right" are devices that enable the rational goal of successful coordination when maximizing calculation is inadequate to this task. The drive-on-the-right convention enables social coordination; all parties are made better off by knowing and observing the convention.

The coordinating value of conventions such as "drive right" is sometimes obscured by their very familiarity. To see this, consider another coordination problem with (symmetrical) multiple equilibria but with no established convention. You are on the telephone and are cut off mid-conversation. If both parties call back simultaneously, or if neither party does, then you have failed to coordinate. If one but not the other party calls back, then you have successfully coordinated. But **which** party should call back — the party who originated the call, perhaps, or the recipient? The older party? In the absence of an established convention, coordination is not a trivial problem it can appear to be.

In some instances, however, uncertain agents may be able to coordinate even when there is no

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[113] For simplicity, I ignore mixed-strategies equilibria (here, play L and R with 0.5 probability), which only compound the multiple equilibria problem.

convention. We digress briefly to consider the case of coordination without established conventions.

5.4.3.1 Focal points: coordination without established conventions

People generally pass one another in crowded corridors without collisions. (Hollis and Sugden 1993). How do they accomplish this? Generally, by observing a rule which, like driving in the U.S., says "stay right." The "stay right" convention is probably less well established for pedestrians than for drivers, which accounts for those instances where two oncoming pedestrians each do multiple side-to-side moves, before successfully coordinating. But there is a convention nonetheless, and it is more or less observed.

One way to think about the existence of the pedestrian convention is as an analogue to the driving convention. Agents see the problem situation and find it is analogous to another setting where the convention is well established. I observe "drive right" when driving, hence I should do the same when walking. If conventions can spread by analogy, this implies that there may be a family relationship or similarity among conventions. (Sugden 1989). When two cars approaching from opposite directions come to a single-lane bridge, and the convention is not common knowledge, who should go first? Sugden argues that "first come-first served" suggests itself, by analogy. (1989: 93-4). First possessors and first arrivers often get precedence in coordination settings, as in the formation of queues, or in the claims to temporary property rights, like a parking space or a movie house seat, or a beach location.

But what is to ensure that the other agents see the same family resemblance, and make the same inferences? Coordination requires that **all** agents know the convention, and what seems plausible to one may not to another. One agent's analogical reasoning will be useless unless others have reached like conclusions. Thomas Schelling's (1960) answer is that certain equilibria are "prominent," or "salient" or otherwise a "focal point."

In a series of informal experimental games, he found that subjects were able to coordinate without communication and without an established convention. One coordination problem was this: write down a positive number. (If the players choose the same number, they have successfully coordinated and are rewarded). The responses were not randomly distributed. Thirty-six out of forty-two respondents selected the number one. There is, Schelling reasoned, something "salient" or "focal" about the number one. (ibid: 94). Another coordination problem asked subjects to pick a place to meet in New York City, with no arranged location and no communication. The majority responded with Grand Central Station. With a virtual infinity of possible outcomes, and no communication, coordination was nonetheless achieved in many instances.

Schelling's experiments were replicated in Mehta, et al. 1994, along with some additional coordination-without-convention experiments. They also found that subjects were unexpectedly able to coordinate without conventions, even with open-ended questions like "write down any color" and "write down any boy's name," and (again) "write down a positive number." (The respondents, British undergraduates, coordinated successfully far more often than random choice would suggest. The most popular answers were "Red" (59%), "John" (50%) and "one" (40%), respectively). Schelling-type salience suggests, at least, that there is a family resemblance among coordinating conventions, and that, therefore, there is a means by which coordination can be achieved even in settings with no established convention.

Leaving the issue of focal-point coordination behind, the reader can think of other "rules of the road" conventions, some of which have been codified into law. When sailboats cross paths, the convention is to yield to the boat on the starboard tack. At (U.S.) traffic circles, one yields to the car already in the circle (or, alternatively, to the car on the right). When two queues merge into one, the convention stipulates that cars (or people) should merge "zipper-style," alternating one car (or person) from each queue at a time.

Some conventions are more important than others. All modern economies depend upon that most integral of social conventions, money. The good that serves as money (shells, precious metals, salt) is not itself a convention; the convention is the social agreement to accept a certain commodity as a medium of exchange. The convention is: "accept this commodity as money."[114] Carl Menger uses money as the paradigmatic case of a spontaneously emerged convention, as against a designed rule. (See Section 5.4.6, below). Language itself, as Menger argued, is another important example of a vital social convention (see Adelstein 1995). Even prices tend to have a conventional aspect. One observes prices like $12.99, or $16,500.00, but not like $127.84. Markets themselves (in the sense of market-places and market-days) can be seen as a kind of convention.[115] (See Schotter 1981 for a game-theoretic treatment of the emergence of these economically important conventions).

Classical game theory begins by postulating a set of rules (regarding what agents know, their options, their rationality, etc.). An institutional approach, following a suggestion by Oskar Morgenstern (1973), recognizes that rules may also be the outcomes of games. Rules like the conventions we have identified are not the antecedents to strategic settings in society; they are the consequences.

5.4.4 Conventions are not always efficient

Though an institution can serve the vital purpose of social coordination, there is no guarantee that it will always be first-best. Conventions that enable stable outcomes may well be Pareto-inferior. To see this, consider another type of social situation— as depicted in the "Stag Hunt" game in Figure 5.2. (Lewis 1969 attributes this game to David Hume).

[114] With fiat money, the convention is codified by the state. The rule says accept this commodity as money because we deem it so.

[115] This is not to suggest that all institutions can be explained from an economic perspective (Elster 1989: 100). Consumption norms, for example, are coordinating devices, but may not have an identifiable economic explanation — men wear ties, women wear pantyhose.

Figure 5.2. The Stag Hunt Game

		Beta Cooperate	Defect
Alpha	Cooperate	2,2	0,1
	Defect	1,0	1,1

Alpha and Beta are hunters who can, if they team up, bring down a stag that will feed both of them handsomely, or, should they not cooperate, can individually catch rabbits, which are adequate but inferior to venison. Unlike the Prisoner's Dilemma situation, the first-best outcome {C,C} is an equilibrium; there is no special incentive to defect. But there is also a non-trivial coordination problem. Which Nash equilibrium to select, {C,C} or {D,D}? As in the driving game, there is the uncertainty of multiple equilibria, but this game is not symmetric; {C,C} is unambiguously preferred to {D,D}.

The hunters are clearly better off cooperating to bag a deer, but there is no strictly "rational" basis for selecting this superior outcome.[116] A convention like "look after yourself and you will never starve" can just as readily emerge as the preferred "hunt together and you'll eat better." Some experimental work suggests that, indeed, the "obvious," Pareto-superior solution {C,C} is often not realized, even with repeated play. (See Van Huyck et al. 1990).

There are also social situations which require coordination, but also involve competing preferences — an example is depicted in the so-called Battle of the Sexes game. Here, Alpha prefers boxing to ballet, and Beta prefers ballet to boxing, but both prefer to attend either event together. The game is depicted in Figure 5.3 below.

[116] Game theorists, of course, don't take this lying down. There have been many efforts to develop a theory of rational selection among multiple equilibria, especially where there are Pareto-superior alternatives, as in the Stag Hunt game. See, for example, Harsanyi and Selten (1988).

Figure 5.3. The "Battle of the Sexes" Game

		Beta Boxing	Ballet
Alpha	Boxing	4,3	1,1
	Ballet	0,0	3,4

Even if the players succeed in coordinating, one of them would prefer the other option — the equilibrium not selected. Note how this differs from the Stag Hunt game, where successful coordination {C,C} makes **both** players better off than they would be otherwise. A convention of "always hunt together" is Pareto-improving when it works. This is not so in a Battle-of-the-Sexes situation. Here a convention unambiguously makes society better off by enabling coordination (designating one of the two pure-strategies Nash equilibria), but, in designating one equilibrium over the other, it distributes those gains differently. This is the result of conflicting preferences.

Say the convention is "Alpha always gets her way," hence {Boxing, Boxing} results. The convention solves the uncertainty of multiple equilibria, but, in so doing, makes Beta worse off than the alternative rule. The convention is efficient; it maximizes the total payoff. But, in this setting, the convention also has distributional implications. Any coordinating convention is good for society, but **which** convention actually operates will affect the distribution of gains achieved by having the rule in the first place.

There are therefore two points here. First, institutions enable coordination but need not be first-best. A convention like "hunt rabbits and you will never starve" in the Stag Hunt game is coordinating but second-best. Second, institutions may have distributional consequences, even when they are efficient. "Alpha always gets her way" is coordinating, and jointly-maximizing, but makes Beta worse off than the alternative convention. Coordination rules can be inefficient or they can be efficient, but with differential distributions.

Douglass North once argued that evolutionary forces tend to select more efficient coordination rules over time, but he has recently "abandoned the efficiency view of institutions" (North 1990: 6), in deference to history's many examples of inefficient institutions that persist. Why would inefficient rules persist, when there are unambiguous incentives for agents to switch to the superior convention? Some insight can be had by referring to the literature on network markets (or "network externalities"), markets where consumers want to buy products that are compatible with those bought by others.

Network markets tend to be "tippy," that is, different standards don't easily coexist, since one standard is likely to drive out the other. (Besen and Farrell 1994). VCR consumers, to pick the classic case, want to buy the technically superior VCR, but they also want to buy the "winning standard," so as to have a better selection of video tapes, the complementary good. Beta technology is better than VHS technology, but consumers opted for VHS because it was better established (it came to market sooner and licensed its technology more widely), and therefore promised a wider videotape selection. A similar story can be told for the Macintosh versus IBM operating systems. Another example is the QWERTY keyboard, which was developed to slow down typists in order to prevent key-jamming. QWERTY is inferior to the faster Dvorak design, but remains the standard despite being second-best.[117] (David 1985).

In all these instances, a given standard won out, not because it was technically superior (to the contrary), but because historical circumstances gave it an early lead, which was reinforced by positive feedback. (Young 1996: 106). This phenomenon, sometimes known as "path dependency," recognizes that inferior standards may well persist over time, due to the "lock-in" effects described above. (Arthur 1989). A standard's history matters, because that history shapes consumers'

[117] See, however, Liebowitz and Margolis (1990) for an interesting history that suggests the there is little evidence for the claim that the Dvorak design is technically superior. They argue that the QWERTY standard may indeed be efficient, denying David's (1985) premise.

expectations (about the winner and size of its network), which ultimately determine the outcome. (Besen and Farrell 1994: 118).

To get an idea of why certain inferior conventions might persist in a non-technological setting, consider the following thought experiment: what if the U.S. government decided that a switch to "drive left" would be desirable? Legislation is passed to mandate the change. Anyone who has driven, or been a pedestrian, in a country with a different driving convention, knows how difficult, and potentially dangerous, an abrupt change can be.[118] The cost of such a switch in lives and injuries, to say nothing of the cost of re-engineering automobiles and roadways, would be enormous. Hence, to the extent that conventions are costly to overturn, i.e. to the extent that agents are adapted to them, we would tend to expect a certain inertia. (See Schotter 1981 and Farrell and Saloner 1985).

If, on the other hand, a change in convention is low-cost relative to the gains of a more efficient convention, then the likelihood of evolution towards more efficient rules increases. Though there are clear incentives to adopt more efficient rules, better rules are not therefore inevitable. More efficient rules will be realized only when transactions costs are relatively low, when network externalities are not present, and when there are not significant collective action problems in inducing change.

5.4.5 Norms and the problem of enforcement

A key issue with the institution I am calling a norm is this: it may be in the individual's interest to flout the norm and behave opportunistically. In coordination-type settings, as with the driving game, there is no incentive to deviate from the convention, once it is established. The convention is therefore self-enforcing. But in many social situations, institutions don't merely coordinate — which makes all

[118] There is the story, probably apocryphal, of Winston Churchill stepping off the curb to cross a New York City street. Churchill looks right, and begins to proceed. He narrowly misses being hit from behind by a bus, saved only by an alert member of his party who grabs him at the last possible moment.

better off — they also attempt to constrain certain individual behaviors — which makes some worse off. In these instances of mixed motive, agents **do** have incentives to deviate. A classic illustration is the Prisoner's Dilemma (PD) game, which is depicted in Figure 5.4.

Figure 5.4. The Prisoner's Dilemma Game

		Beta	
		Cooperate	Defect
Alpha	Cooperate	3,3	1,4
	Defect	4,1	2,2

In a social setting characterized by the Prisoner's Dilemma payoff structure, society is best served by the cooperative outcome {C,C}. The collective payoffs are maximized there. The catch, of course, is that each player has an individual incentive to defect from a cooperative strategy, and because all players have the incentive to cheat, the result (in one-shot and finite repeated games) is a Pareto-inferior outcome of {D,D}. The PD outcome is vexing, because all players can be made better off by cooperating, yet neither can they, in classical game theory, resist the temptation to cheat — Amartya Sen calls such behavior "rational foolishness" (1977).

There are, of course, many economic situations that have the PD structure, such as cartel production behavior, common-property resource problems, and public goods contributions. Contributors to a public good do best collectively when they contribute to its provision, but, as individuals, they are tempted to free ride. If all free ride, then the inferior result obtains; no public good is provided. (On the provision of collective goods, see Olson 1965). Cartel members have individual incentives to produce more than their quotas, though collectively they fare better (achieve monopoly profits) by cooperating. Common property users have individual incentives to over-exploit the resource (grazing land, water pool, fishery), though cooperation can help avoid the "tragedy of the commons" that results from over-exploitation.

The prisoner's dilemma succinctly represents one of the oldest and most important problems in social theory: to wit, what to do when individual and group goals are incompatible? In social contract theory, the problem is how to induce agents to play by the rules (rules that protect persons, property and expression) that will make all better off, when, as individuals, they would prefer not to surrender their freedoms (though they are happy to have others do so). How to induce agents to behave cooperatively, when it benefits them to behave opportunistically? A contract is an obvious candidate. Agents who sign the social contract agree to observe its rules. In the absence of an actual contract, a norm of cooperation may substitute.

The key problem, of course, is enforcement. Norms can enable cooperation only when they are observed. A norm like "always cooperate" will not be self-enforcing as it was in the symmetric coordination game, precisely because there are material incentives not to observe it. The cartel member produces more than its quota, the potential public-goods contributor free-rides, and the common property user over exploits the resource. The best outcome, from the individual agent's perspective, is not to cooperate (breach the contract) while everyone else does (abide by the contract).

5.4.5.1 How to enforce norms with self-interested agents?

One enforcement solution, made famous by Thomas Hobbes, is to invest a third-party with dictatorial powers — to create a Leviathan state. ([1651] 1968). Hobbes did not propose a dictatorship lightly; he believed that the benefits of society were unobtainable without a credibly powerful mechanism of enforcement. Norms without credible enforcement are, for Hobbes, ineffectual — he dismissed them as "covenants without the sword." The Hobbesian solution to the problem of social order is to create a state that can (credibly) threaten rules-breakers with punishment. In effect, Hobbes's solution changes the payoffs in the PD game so that the payoff to defection is less than that for cooperation.

The problem with the Hobbesian solution is twofold. First, the obvious: what is to prevent the all-powerful state from usurping all rights from the people who created it? This is the problem of who is to police the police, once absolute power is surrendered to the state. History is littered with examples of governments abusing the power entrusted to them by the governed. Nor should we be surprised by this history, since governments are made up of the very same people who, Hobbes argued, needed a firm hand to restrain their anti-social impulses towards theft, murder, mayhem and the like. I want to emphasize a second, less conspicuous problem with Leviathan: given Hobbesian (opportunistic) agents, the Hobbesian state is not particularly cost-effective.

If agents are truly opportunistic in Hobbes's sense, they will cheat or rob or kill any time the net expected benefits are positive. The butcher regularly has her thumb on the scale, the cashier will generally attempt to shortchange you, and the customers are all potential shoplifters. When it pays, promises will be broken, contracts will be breached, property will be stolen, and the blind will be mugged. Small wonder that life in a Hobbesian state of nature is "nasty, poor, solitary, brutish and short." Effectively enforcing rules in such a milieu requires raising the (expected) costs of anti-social behavior sufficient to make the net benefits thereof negative. This is no small matter, with truly opportunistic agents; it requires supervising every transaction, exchange and interaction. The monitoring costs alone are staggering, to say nothing of costs for arrest, trial, and punishment.

Though I cannot produce any decisive cost data, history shows that repressive regimes do tend to be expensive. And the more extensive the repression, the higher is the cost of maintaining the regime. Totalitarian regimes, for example, are especially costly to maintain. The problem is this: if agents are truly opportunistic in Hobbes's sense, then no authority is strong enough to economically enforce the rules. And, as obedience breaks down, even law-abiders will have increasing incentives to abandon the rules. Hence, even backed by Leviathan, effective enforcement of rights to persons, property and contract depends upon a citizenry that is already disposed to observe these fundamental rules. (See Rapaczynski 1996).

A more economical solution is to devise an ethics. It's far cheaper for a society to make use of norms of cooperation — don't murder (abuse rights to person) don't steal (abuse rights to property), don't cheat (breach contract; commit fraud). Norms are clearly more cost-effective than a repressive regime. But the normative "solution," of course, requires agents who are not Hobbesian, i.e. who are not pure opportunists. Effective enforcement presupposes agents who (mostly) obey moral rules because they believe that they **ought** to, not merely because they have calculated the expected net benefits of being caught and punished. But can moral norms work? Hobbes said no; there are no covenants without the sword. Adam Smith, who called such norms moral sentiments, said yes.

5.4.5.2 Smith not Hobbes: covenants without the sword

Smith developed an elaborate theory of moral rules (he was, after all, a professor of Moral Philosophy) in his **other** book, *The Theory of Moral Sentiments* (1759) (TMS). Today, TMS is vastly less well-known than its renowned successor, ignored even. But Smith published six editions of TMS (the last just before he died) and they were extremely successful, and influential in their day. Indeed, one way to read the TMS is as an essential analytical precursor to the *Wealth of Nations* (1776) (WN). In TMS Smith lays down a theory of the moral rules which he believed were essential to social order, which, in turn, was a prerequisite to the successful operation of free markets. (This is Heilbroner's view, among others. See his remarks in Heilbroner 1986: 58, an expurgated version of TMS and WN which I shall refer to in this discussion. I will also make use of Jerry Muller's excellent account (1993)).

The WN remains on bookshelves today because of its famous, and unintuitive thesis: that self-interested behavior can result in socially beneficial outcomes.[119] Smith used the invisible hand as a

[119] Smith's other great idea is this: there are mutual gains from voluntary exchange. Extending this idea to nations, Smith arrived at the argument for free trade, and against Mercantilism. Incidentally, the idea of base private motives producing beneficial social outcomes is, as has been noted in many places, not original with Smith. Bernard de Mandeville makes the argument, for example, in his

metaphor for this thesis of positive, unintended consequences.[120] The invisible hand works, Smith cautioned, only if there exists the right institutional framework — competitive markets, protected rights to person and property (what Smith meant by "justice"), provision of other public goods (especially national defense and education), and a measure of individual morality. Smith wrote TMS, in part, to explain how human agents (in a commercial society) come to be moral.

This aspect of TMS was part of a larger analytical project of the Scottish Enlightenment: to wit, how to reconcile traditional (especially religious) demands for virtuous behavior by agents with more modern arguments (especially those of Hobbes and Mandeville) that self-interest and egoism were paramount in human motivation. (Muller 1993: 17). Smith agreed with Hobbes that agents were primarily self-interested. He says, flatly, that man is "by nature first and principally recommended to his own care . . . and every man . . . is much more deeply interested in whatever immediately concerns himself, than in what concerns any other man." (Muller 1993: 101, TMS II,ii,2.1 82-83).[121] But Smith denied that agents were entirely self-regarding in the sense that opportunism requires. In his scheme, agents are self-interested but fundamentally social beings. (The asocial Robinson Crusoe agent is as far from Smith as one can get). One aspect of their social character is a profound psychological need for the approval of others, what Smith called "approbation." The desire for attention and praise, is, for Smith, a basic human propensity — part of our God-given psychological makeup.

A second key element in Smith's theory of morality is the idea that human agents can reflect

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notorious *The Fable of the Bees: Private Vices, Public Benefits* (1715).

[120] This most famous Smithian metaphor appears but twice in all his published works, once in each book.

[121] Regarding producers, he said acidly, "people of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in some conspiracy against the public, or in some contrivance to raise prices." (Cited in Heilbroner 1986: 322)

upon their own actions. We can, said Smith, consider ourselves as if from outside (albeit imperfectly). The metaphor he devised for this self-reflective capacity was the "impartial spectator." The impartial spectator, which resides within all agents, evolves from another basic human propensity — the capacity for sympathy. Smith argued that agents are hard-wired for some measure of fellow-feeling. "However selfish man may be supposed," begins TMS, " there are evidently some principles in his nature, which interest him in the fortunes of others, and render their happiness necessary to him . . . ." (In Heilbroner 1986: 65). We cannot feel the full depth of what others really feel, Smith argues, but neither are we immune to their condition. Says Smith: "When we see a stroke aimed and just ready to fall upon the leg or arm of another person, we naturally shrink and draw back our own leg or our own arm; and when it does fall, we feel it some measure, and are hurt by it as well as the sufferer." (ibid: 66).

With practice considering others in this fashion, and knowing that they are likewise considering us, agents develop the art of critical self-reflection. "Though we are naturally inclined to prefer our own interests to the interests of others," says Muller, "our egoism is restrained to the extent that we learn to judge our own actions as they appear to others who do not share our egoistic partiality toward ourselves." (1993: 102). The capacity for internal spectating, for Smith, grows from this ongoing internal conversation regarding others (ibid).

Thus, our morality is **learned** in a social milieu, says Smith. Human agents do not have, as his teacher Frances Hutcheson argued, an innate moral sense. Smithian moral sentiments evolve in the play of social life. (Taylor 1930: 209-210). Human morality is therefore made, not given. (Heilbroner 1986: 59). Human agents develop a conscience through the interaction of the selfish desire for approval and the ability to impartially spectate. (Muller 1993: 101).

In effect, then, Smithian agents observe moral norms because they want to. In Smith's moral psychology, the development of a conscience means that there are real (psychic) rewards for observing moral rules, and real (psychic) punishments for failing to observe moral rules. Virtuous behavior earns self-approval and violation of moral rules leads to dread and self-condemnation (ibid: 107).

In modern terms, doing or not doing (what one takes to be) the right thing, has utility effects. Agents obey norms when they find that internal sanctions — guilt, shame, anticipatory remorse — are sufficient to make disobedience less attractive than obedience. (James Coleman 1987: 141-42). Smithian morality is not so much a question of duty, nor does it derive from an innate love for all humankind (what Smith called "benevolence"). Moral rule-following derives more from providential design — God has outfitted human nature with a conscience that weighs in during moral decision making, if not always decisively. (ibid). In a real sense, Smithian agents act morally because they want to act morally.[122]

There are several points to be made regarding Smith's theory of moral rules and rule following. First, although human nature is reasonably uniform, actual norms, because they are evolved and not given, will be relative to the society (and individual) in which they are developed. There is no universal morality in Smith. The existence of norms may be universal, but their actual content can vary with social location.

Second, Smith understood that the social ties which bind human agents vary with social distance. Social proximity matters to fellow-feeling. Says Smith: "After himself, the members of his own family, those who usually live in the same house with him, his parents, his children, his brothers and sisters, are naturally the objects of his warmest affection." (TMS, part VI, section II, chapter I; Heilbroner 1986: 136).

Agents care most about themselves and their immediate families, a bit less about close friends

[122] It is important to note that these processes are not wholly rational. Smith's moral psychology is mostly instinctive, of the "passions" he would say. Smith was widely suspicious of reason in these matters.

and more distant kin. Acquaintances, business associates, fellow citizens and fellow humans move increasingly farther away in social distance. Smith refers to social distance in a chapter of the TMS called "The order in which individuals are recommended by Nature to our care and attention."[123] (ibid). Given this variance of affection, it is reasonable for the Smithian economist to expect that moral norms are more likely to be observed in smaller communities, where agents are nearer in social distance. The reverse is also true: cooperation will be harder to achieve in larger, or less intimate groups. (See Olson 1965).

Third, Smithian moral rules are not "trumps" that always and everywhere override consideration of material self-interest. Smithian agents will not simply default to the cooperative outcome in PD games because we now ascribe to them a moral aspect. For Smith, agents consider their interests as whole, and moral preferences are **part** of self-interest. As such, some agents will act cooperatively in a given situation, while others will be tempted to behave opportunistically. (On trading off moral and ordinary interests, see Dowell, Goldfarb and Griffith 1993.)

The Hobbesian says that agents **always** behave opportunistically, hence payoffs can only be altered by credible third-party threats to material interests (no cooperation). The moralist, on the opposite extreme, says that norms always trump material preferences; agents **never** behave opportunistically (always cooperation). Smith's position is intermediate between these extremes — Smithian agents will sometimes find it in their interest to obey norms, sometimes not (sometimes cooperation).[124]

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[123] In a paper on Smith's view of social proximity, Nieli (1986) uses the evocative term, "spheres of intimacy".

[124] In modern terms, the Smithian account of morality can be seen as a kind of meta-preference, an ability to take a strategic view of our "ordinary' preferences. (See Sen 1977). See also Robert Frank (1988) for a evolutionary treatment of Smith's idea that moral (and immoral) behavior has unavoidable utility consequences. Frank argues that the emotions of guilt and shame are evolved mechanisms that function as unconscious, internal sanctions against immoral behavior.

Smith would therefore not be surprised that most taxpayers voluntarily pay their taxes, and that most citizens refrain from murder, mayhem, theft and trespass, even though the expected net benefits of such crimes can be large. Nor would he be surprised, for example, that agents can sometimes cooperate to overcome the over-exploitation problem inherent in management of commonly-owned resources such as irrigation water, fisheries and grazing land. (See Ostrom 1990, and the Symposium in the Fall 1993 *Journal of Economic Perspectives* 7(4): 87-134). Adam Smith would predict that, in "ultimatum games," the "allocator" regularly gives the other player an amount well above the non-cooperative result.[125]

Any of these cooperative outcomes are inexplicable in Hobbes's setting (where interest does not admit moral considerations), absent an enforcement threat that changes the material payoffs. The Hobbesian agent obeys only when the risk of being caught and punished is a marginal deterrent. The Smithian agent, in contrast, will sometimes honor norms, covenants without the sword.

Note well, however, that tax cheating, overexploitation of common property resources, under-contribution to public goods, and other partly non-cooperative outcomes would also not surprise Adam Smith. Smithian moral sentiments are not robust in all people, and they do not, by their mere presence, induce cooperative behavior everywhere. But where moral sentiments are robust they can work to change the incentives in the PD game as surely as can Leviathan, and at lower cost.

Ronald Coase's summary of the Smithian agent, puts it well. Smith, says Coase, "thinks of man as he actually is: dominated, it is true, by self-love but not without some concern for others, able to reason but not necessarily in such a way as to reach the right conclusion, seeing the outcomes of his

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[125] In the ultimatum game, two players are paired anonymously. Player one, the allocator, has \$10 to divide between herself and player 2. The allocator offers \$x to player 2 (thereby getting \$10-x), an allocation which player 2 may accept, ending the game, or veto, in which case both players get \$0. The non-cooperative prediction is that the allocator offers player 2 \$.01, which player 2 accepts. In experiments, however, the modal offer is \$5, with means around \$4.38. (See Vernon Smith 1996).

actions but through a veil of self-delusion." (Coase 1994: 116).

Now we turn to the question of origin. Where do institutions such as conventions and norms come from, and does the source matter for their efficacy?

5.4.6 Where do institutions come from, evolution or design?

I have argued that some institutions, particularly conventions, evolve spontaneously, through repeated social interactions over time. Friedrich Hayek, defining what he called "spontaneous order," liked to cite Adam Ferguson: spontaneously evolved institutions are those that "are of human action, but not of human design." (Hayek 1973: 20). Robert Sugden, who has used evolutionary game theory to consider spontaneous orders (1986, 1989), echoes Hayek in arguing that most conventions evolve spontaneously: "[conventions] are rules that have never been consciously designed and that it is in everyone's interest to keep." (Sugden 1986: 54. See also Ulmann-Margalit 1977 and Young 1996). But institutions may also be consciously designed, as with statutory law. Designed or invented rules result from a deliberate choice — an act of creation, often by the state or other agent of enforcement.

Hayek saw the difference — spontaneously emergent rules versus rationally designed rules — as profound. The different views of institutions, he argued, derived from two very different intellectual traditions, Scottish Enlightenment thought (David Hume, Adam Smith, Adam Ferguson) on the one hand and the French rationalist tradition (Hobbes, Voltaire, and especially Rene Descartes), on the other. (Hayek 1973: 8-34; see also Gray 1988). The Scots, as we have seen with Adam Smith, were skeptical about human reason, and took the problem of incomplete knowledge as a given. The rationalist tradition is less skeptical about of human knowledge. "If you want good laws," Voltaire advised, "burn those you have and make your own." (cited in Hayek 1973: 25). It is probably no accident that, following the French Revolution, it was decreed that carriages should stay right, and pedestrians left, a reversal of the prevailing convention. (Young 1996: 106).

These two intellectual traditions collided in economics during the great Socialist Calculation

Debate. Lange and Lerner, playing the Cartesians, argued that central planning was entirely feasible; it was simply a matter of getting the marginal conditions correct. Economic knowledge, they argued, was sufficient for central planning of entire economies. Mises and Hayek, standing in for the Scots, argued that no planner could ever have knowledge sufficient for such a task, even if such incorruptible worthies could be found. They argued that the knowledge required for central planning was not only vast, it was unavailable. The knowledge required by the central planner does not exist **before** it has been "discovered" or generated by the market process. Prices, said Hayek, are produced by market processes; they don't exist prior to that process. (Hayek 1937).

Hayek, then, saw rules in the Scottish tradition, as spontaneously emergent or unplanned. In this respect, he followed Carl Menger, the founder of the Austrian School.[126] Menger conceived of **all** the important institutions in economic life — "language, law, the state, money [and] markets" — as unplanned.[127] (Menger 1985 [1883]: 147). Menger probably goes too far here, as Vanberg and Buchanan (1990) have pointed out. Some important rules are not self-enforcing, particularly norms, and are therefore less likely to have evolved spontaneously. (ibid: 181).

Whatever a given rule's provenance, the distinction between spontaneously evolved and rationally designed rules remains important for two reasons. First, designed rules, such as legislation or regulation, may conflict with already established conventions or norms. Second, and related, designed rules may be unsuccessful without an existing convention to "piggy-back" on. Consider the first problem, when designed rules conflict with established conventions.

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[126] The term "spontaneous order" is due to Hayek. Menger used the term "organic" to designate evolved rather than designed rules.

[127] It is therefore perhaps unsurprising that Menger took spontaneously evolved rules to be "perhaps the most noteworthy problem in the social sciences." The question he wants to consider is this: "How can it be that institutions which serve the common welfare and are extremely significant for its development come into being without a common will directed towards establishing them?" (Menger 1985 [1883]: 146).

Agents already observing the current convention will find it in their interest to switch to a new convention, only on the belief that all others can and will also switch. Absent this belief, a switch is undesirable. To see this, consider again the Stag Hunt game, and assume that the established convention is to hunt alone. Say that the government passes a law that requires hunters to hunt cooperatively, i.e., in teams. For the individual hunter, it will make sense to obey the new law, and overturn the old convention, only if she expects other hunters do likewise. If others don't obey the new law, then the obedient hunter will go hungry. The law (and enforcement thereof) may be sufficient to convince hunters that all other hunters will cooperate, but it may not be. Even well-intended legislation will sometimes be ineffectual when it conflicts with established conventions.

Government-issued money is another example. If agents don't accept fiat money, then it is not money, no matter what the issuer says. Generally, when governments do issue new money (following a bad inflation, for example), they link it's exchange value to the old money— new rubles are worth 1,000 old rubles, for example. Fiat money that is completely severed from former monies is unlikely to be accepted. The same is true for units of money. If agents prefer not to use unfamiliar units of money — $2 bills and dollar coins are examples — then U.S. Treasury attempts to promote their acceptance will fail, and the costs of production (and subsequent destruction or warehousing) will be a total loss.

Language is another example of a spontaneous evolved convention that legislation will be hard pressed to overturn or significantly alter. If agents prefer to speak English, then French will no longer serve as a *lingua franca,* no matter what the French Culture Ministry might say. The French government prefers that it's citizens avoid English terms like "weekend" and "software" in their everyday usage, but regulation against such imports is unlikely to overturn this established practice. Languages are not invented or designed, they are spontaneously evolved. That is why rationally designed languages, like Esperanto, have never caught on, their greater "efficiency" (fewer irregular verbs, fewer gendered nouns, etc.) notwithstanding. (Adelstein 1995).

Some designed rules simply piggy back on established conventions. There are traffic laws, after all, that say: drive on the right side of the road. The laws are long antedated by the convention.[128] A convention like "drive right" may be **codified** by law, but it is not **created**, *ex nihilo*, by law. Had the U.S. traffic laws been written "drive left" — that is, had they opposed the established convention — they would have been a dismal, and costly, failure.

If there is no convention on which to piggy back, designed rules may have difficulty. Consider a traditional college campus with lots of green grass but no built walkways. Left to their own devices, agents will gradually develop paths, generally in ways that reflect the best routes between busy locations. If the college erects a system of built walkways that doesn't map well onto the conventional paths already established, we would expect to see bare patches and mud where pedestrians ignore the built walkways in favor of the more efficient (for them) paths.

The walkway planner can try to enforce the designed system on recalcitrant pedestrians, but temporary or passable barriers will quickly be overrun. Truly impassible barriers, like high fences, will be expensive, as are other means of enforcement, such as policing the grounds. A designed rule works best when it accommodates the existing order. This is not to claim that evolved conventions are invariably superior. They can be inferior, as was discussed in Section 5.4.4. The claim is, rather, that to ignore the knowledge embedded in existing conventions will invariably be costly when trying to

[128] Colin Camerer offers one view of how the driving conventions originated. He reports: "The American convention was established by farmers driving large teams of horses to the market. They sat on the left rear horse so they could lash the team with a whip, right-handed. Since they were sitting on the left, accidents were best avoided if other teams passed on the left; they drove them on the right. English drivers sat up on smaller carriages with a load behind them. A whip lashed right-handed would get caught in the load if the drivers sat on the left, so they sat on the right. Drivers passed on the right. Historical context matters: on an otherwise identical planet with more left-handed drivers. Americans would drive on the left, the English on the right." (Camerer 1990: 327). I owe this reference to Tayfun Gurler.

design new rules. The rule designer can learn a great deal from the knowledge embedded in existing conventions.

In fact, the complexity of existing market conventions can make full replication impossible. Take property rights: can the state simply establish them *de novo*, or must there already be some property conventions in place? Rapaczynski, referring to the prospects for market economies in Eastern Europe, argues that "property rights are too complex to be 'put in place' in advance of the development of a market economy." (1996: 89). Property rights in advanced economies are highly complex; there are rights to future income streams, such as pensions or mutual fund returns, expectations of Social Security payments, licenses to practice a profession, derivative instruments, tax shelters, mortgage-backed securities, and other sophisticated forms of property. (ibid). The problem "with most of these sophisticated forms of property is that even the most powerful, rational and benevolent states cannot fully define or protect them." (ibid).

None of this is to argue that markets do not benefit from legal establishment of property rights, nor that the law is not an important instrument of enforcement. The opposite is true. The point is that designed rules like laws sometimes cannot serve as a complete substitute for the market conventions it wishes to codify (or to transplant).

Let us now summarize this chapter's arguments on rationality, maximization, uncertainty and institutions. The emphasis will be on how institutions help when optimality is undefined, unattainable, or undesirable.

5.5 Summary: how institutions help individuals when optimality is undefined, unattainable, or undesirable

An emphasis on the limits of human knowledge and the importance of institutions in markets is hardly new in economics. It was central to Adam Smith's economics. As Vanberg and Buchanan remind us, "[a] central theme of classical political economy has been the way the character of a social and economic order depends upon the framework of rules . . . within which individuals act and interact." (1990: 174). This theme has been obscured in modern economics, largely because neoclassical economics generally posits agents who meet the canonical propositions — neoclassical agents possess complete information, and an unbounded (and costless) ability to calculate optimal, determinate decisions.

By way of summary, let us reconsider the five ways in which optimality can fail, and institutions as possible remedies. Most important for an economics of science, will be institutions with a social aspect — conventions as coordinating devices and norms as incentives for cooperation. These institutions generally arise in scenario two (multiple equilibria, where a determinate solution is unavailable) and in scenario five (PD and other settings where the "optimal" choice is undesirable), respectively. But let us briefly reconsider the other three scenarios as well.

In scenario one, maximization is undefined, owing to incomplete information. The missing information may be non-existent, unavailable, or uneconomic. Depending upon the information deficit, agents have incentives to resort to decision-making heuristics, or rules of thumb. (See Kahneman, Slovik and Tversky 1983). One rule of thumb already noted (for estimation when lacking probability distribution), is to assume that all (*n* possible) outcomes are equally likely, i.e., assign each outcome a likelihood of $1/n$. Also noted was the old rule of thumb for portfolio diversification — allocate one-third of total assets to equities, debt, and cash, respectively. Another heuristic is "don't accept offers that look to good to be true." (Thaler 1996: 229).

Rules of thumb such as these are substitutes for optimal choice when maximization is impossible, and they clearly cannot be optimal in most instances. Hence, there are many situations when heuristics will prove self-defeating — by not accepting "too good to be true" offers, agents will occasionally forgo very profitable opportunities. Another common rule of thumb, "don't waste," can lead to the sunk-cost fallacy, Thaler 1996 notes. Following this rule, agents are more likely to finish a rich, mediocre dessert when they have paid for it, rather than if it were offered *gratis*. (ibid: 229-30).

But from an evolutionary perspective, the question is not whether a heuristic is sub-optimal, but whether, on balance, it provides more good than harm to the decision maker, and whether there are better (equally feasible) alternatives. Sub-optimal rules of thumb will persist when they provide net benefits greater than those of known alternatives.

In situations described by scenario three (complexity overwhelms human cognitive limits), rules of thumb may prove superior to (necessarily futile) attempts at optimization. In the game of chess, we saw, complexity precludes identifying the theoretically optimal solution. The upshot has been the creation of many rules of thumb that prove useful (if not optimal) in chess.[129] One such rule is "maintain an adequate pawn structure." Another is "control the center of the board." More refined rules of thumb will be situational, such as, "in situation Y, castle."

Following these rules of thumb in chess may well be a better alternative to attempting the brute search that an optimal solution requires (and that computers perform so adeptly). As a practical matter, even the best human players can project scenarios only a few moves into the future, and attempts to optimize (that is, to search a search space of 10^{20} to 10^{40} branches) may well be less effective than rule following. When there is also a time constraint (a game clock), the advantage of rule following becomes even more compelling. Humans beings have a comparative advantage in the situational expertise that recognizes patterns or situations, and then applies a learned rule of thumb. (Simon and Schaeffer 1992). In chess, at least, the best is the enemy of the good.

In situations described by scenario four, where optimization is physically possible but uneconomic, owing to costly decision making, a paradox results. If agents economize on decision making, in recognition of decision-making costs, then they should also (if they are maximizers) be

[129] I am indebted to Bob Goldfarb for this point.

economizing on their economizing. (Conlisk 1996: 687). To go another order higher, the maximizing agent should also economize on the economizing economizing, and it is clear that a regress results. The regress problem seems to block, as Conlisk notes, "any effort to maintain optimization as the ultimate logical basis for all behavioral modeling." (ibid).

Generally the extent of regress issue will depend upon the cost of decision making (say, *C*) relative to the decision-making payoff. Where *C* is high, agents will tend to rely on rules of thumb. Where it is very low, agents will tend to the optimal choice absent decision-making costs, perhaps by treating *C* as a small, fixed cost. In between, Conlisk suggests, agents will rely on some mix of deliberation and rule following. (1996: 688). Agents may use a rule of thumb as a substitute for decision making, when *C* is very high relative to the benefits of decision making, or they may use a kind of "stopping rule." A stopping rule limits the time of deliberation, with reference to the importance of the decision at hand. For example, the agent may observe a rule that says: "for trivial problems, allocate no more than a minute."

In all of these three scenarios (one, three, and five), a decision-making heuristic is the institution that an individual agent makes use of when optimality is undefined or unattainable or undesirable. Generally, these rules of thumb have no social intent; they serve the mostly individual purposes of boundedly rational agents. (Though, as noted, to the extent that rule-following leads to more predictable behavior, it does have social consequences, as emphasized by Ron Heiner 1983). Conventions and norms, in contrast, do have broader consequences; they work to promote coordination and cooperation, and are therefore important to collective outcomes.

5.5.1 How institutions help society: conventions, norms and law

It is the rules with a social aspect that are most important for markets and for science. Explicit rules, such as laws, and implicit rules, such as norms and conventions, are meant to promote beneficial collective outcomes, which may or may not be compatible with individual goals, as we saw in Section 5.4.5. Perhaps most fundamental are the institutions that protect persons, property, and contracts.

In market economies, there are laws, backed by the enforcement power of the state, that enforce private property rights by making it illegal to abuse or to steal property. These are laws against trespass, vandalism, and theft, for example. There are also laws which protect the sanctity of one's person — murder (and other forms of killing), rape, assault, etc., are all punishable crimes. And there are laws that guarantee the right to free contract, enforcing legitimate contracts, while banning fraud and other forms of commercial deception.

There are exceptions to such laws, of course. In some instances even killing is legal (in self-defense, for example). Most governments grant exemptions to themselves when it comes to respecting private property — the power of eminent domain, for example. And governments routinely regulate commerce in ways that restrict free contracting — minimum wage laws, and laws that prohibit the sale of "inalienable" goods (sexual services, body parts, freedom), are examples.

But the protection of persons, property and contracts is the foundation upon which free markets stand. Without some means of protecting persons, property and contracts, the production and exchange of goods will be radically curtailed. This is because potential producers will be obliged to devote disproportionate resources to protect lives and property. In the absence of institutions (and enforcement thereof), a society suffers the state-of-nature PD outcome (scenario five), as discussed in Section 5.4.5.

As argued in Section 5.4.6, important laws (including those that pertain to persons, property and contracts) are usually antedated by (and piggy back on) more informal institutions, especially norms and conventions. Virtually all societies have norms that condemn murder, theft and fraud — thou shalt not kill, thou shalt not steal, thou shalt not bear false witness. In the absence of a state (or other third-party enforcer), we also saw, the extent to which norms of cooperation hold (i.e. that opportunism is eschewed) will be a function of individual honesty — the robustness, that is, of moral sentiments in the population.

Conventions are less visible than norms, perhaps because they tend to be self-enforcing, but they are no less important for promoting beneficial collective outcomes. As we saw in Section 5.4.3, some conventions are especially important to markets, because they enable coordination under uncertainty, such as that presented by multiple equilibria (scenario two). Because agents need to coordinate in many settings, conventions can be quite useful — our running example has been the rules of the road. There are more important conventions of course. A language is a set of conventions for communication; it enables the exchange of ideas, concepts and meanings. A money is a convention for the exchange of goods and services. There are other conventions which are less fundamental than language and money, but that likewise enable coordination in markets — the Monday to Friday (9:00-5:00) work week, for example.

Conventions are economically important because they enable coordination, which greatly economizes on transactions costs. Conventions reduce uncertainty by working to fix expectations in strategic settings, which greatly reduces the costs of negotiation, contracting, monitoring and enforcement. A medium of exchange is only the most conspicuous example — monetary economies are vastly more efficient than barter economies.

In this chapter I have tried to develop two themes. The first theme is the idea of a boundedly rational economic agent, i.e., rational but fallible. The Smithian agent is rational but fallible, self-interested but prudent. Second, is the idea that institutions are decision-making resources to economic agents. Some institutions, such as heuristics, aid the individual decision maker when optimality is undefined or unattainable. Other institutions, such as conventions and norms (and the laws that codify them), work to promote collectively beneficial goals in markets; in particular, they enable coordination and cooperation. In Chapter Six, I apply these themes to science. The idea is that the scientist can be seen as a Smithian agent and that science can be seen as a market-like social process. If this is so, then we would expect to find institutions in science similar in function to the market institutions discussed in this chapter. We now turn to these tasks.

Chapter 6. A Smithian economics of science, part two

Man is not an optimizer, but a scientist. (Brian Loasby 1989: 208)

In Chapter Five I argued that economic agents should be seen as Smithian, rational but fallible, self-interested but prudent. I also argued that institutions provide decision-making resources that can aid the individual agent, and also enable social goals like cooperation and coordination, in situations where optimality is undefined, unattainable or undesirable. In this chapter I want to apply that framework to science. This entails seeing the scientist as a Smithian agent, science as a market-like process, and scientific institutions as analogous to those that operate in markets. The substantive argument for an economics of science begins below, in Section 6.1.

In Appendix 6A, I make a logical argument for an economics of science: for economists, theoretical consistency requires it. If we believe in our economic theories of human action, then they should apply to ourselves (and to other scientists), and the naive view of scientists as selfless truth seekers is therefore contradictory. Economists might understandably prefer the flattering depiction of the received view to the not-so-flattering depiction of the dismal science, but intellectual consistency will not permit this preference. In Appendix 6A I take up and reject three arguments against the claim that economics should apply to science (and therefore to itself).

6.1 The scientist as a Smithian agent

Let us begin with the scientist as a Smithian agent. I argued in Chapter Five that one can reject maximization as the (only) basis for thinking about choice, without rejecting rationality *per se*. In that chapter, I discussed five scenarios in which economic agents cannot realize optimality.

Scientists are not immune from these difficulties. If one agrees that people are boundedly rational, and that scientists are people, too, then scientists are also boundedly rational. (See Appendix 6A). Like all agents, scientists' cognitive resources are scarce, hence, decision making will be costly (scenario four), and some problems will demand solutions that exceed cognitive limits (scenario three).

In fact, that science is a setting where agents face even greater obstacles to optimality than in ordinary markets, hence the arguments for bounded rationality (and for institutions) apply with even greater force. The unique uncertainties of producing scientific knowledge, as against ordinary goods, can be seen with reference to the other three scenarios. Coordination is difficult in science (scenario two), especially given that knowledge is not explicitly priced, and the unusual nature of knowledge as a good also tends to create market failures (scenario five). These two problems I take up in the next section. Here I emphasize the uncertainty that scientists face in trying to form expectations about the future, a problem of incomplete information (scenario one).

Because the production of scientific knowledge entails novelty (knowledge must be new in some fashion), it is inherently difficult to forecast the future course of scientific research. The outcomes of scientific research are difficult to anticipate because science is an open-ended enterprise by its very nature. Since the object of science is, in part, to produce novelty — ideas not yet conceived — scientific outcomes cannot be the direct product of a completely informed, maximizing choice in the conventional sense. (Loasby 1989: 197). It is no accident that the future path of science regularly defies the predictions of even those participants best equipped to speculate.[130]

The Smithian agent, we have seen, is rational but fallible, self-interested but prudent. In the context of science, this implies that scientists cannot be seen as (exclusively) selfless seekers of

[130] One can argue that this uncertainty is true of all innovation, in ordinary markets as well as in science. I don't disagree. I merely emphasize that, unlike ordinary markets, the production of scientific knowledge always entails some innovation, which is not the case with ordinary goods.

knowledge (as we saw in Chapter Four). As such, the Smithian scientist can have and pursue non-cognitive goals like professional esteem, wealth, and prestige. In Philip Kitcher's terminology, Smithian scientists are "epistemically sullied." This conception in no way rules out intellectual curiosity (knowledge seeking) or cooperative (i.e. norm-following) behavior in science; it only rules out selfless truth-seeking as the exclusive determinant of scientific motivation, this by insisting that scientists are people too. "Scientists," as Donald Campbell puts it "are thoroughly human beings: greedily ambitious, competitive, unscrupulous, self-interested, clique-partisan, biased by tradition and cultural memberships, given to mutual back scratching, and the like." (1988: 320).

These considerations suggest that it is reasonable to model scientists as Smithian agents. Next we consider science as a market-like process, that is, we consider the extent to which the social processes of science can be seen as analogous to market processes.

6.2 Science as a market-like process

Ronald Coase 1974, among others, has argued that it is appropriate to conceive of science as a market-like process. Says Coase:

> I do not believe that the distinction between the market for goods and the market for ideas is valid. There is no fundamental difference between these two markets . . . . In all markets producers have some reasons for being honest and some reasons for being dishonest; consumers have some information but are not fully informed or even able to digest the information they have; regulators commonly do a good job, and though often incompetent and subject to the influence of special interests, they act like this because, like all of us, they are human beings whose strongest motives are not the highest. (1974: 389).

In this section I want to take up Coase's claim, asking: in what sense can we think about the production and consumption of scientific knowledge as market-like processes? The "marketplace of ideas" is a much-abused cliche, but we should not let this deter investigation of the ways in which science can be seen as market-like.[131] The discussion begins with the question: what kind of good is scientific knowledge?

6.2.1 What kind of a good is a scientific idea?

Scientific knowledge is obviously not a garden-variety commodity — it is somewhat amorphous, hard to bound, and difficult to value. But knowledge is a real thing, it has boundaries and it has value. Knowledge exists, and it exists because scarce intellectual and material resources were expended to produce it. In this limited sense, at least, knowledge can be seen as a good. In the interest of concreteness, we can consider an idea as a unit of knowledge. (The Law of Demand, for example, is an idea, as is the notion that the Law of Demand can be tested empirically). Knowledge consists of a body of interconnected ideas. A "contribution" to knowledge is supposed to include at least one idea, and that idea is supposed to be new, however much it is derived from and connected to older ideas.

A scientific idea is still a pretty exotic beast. But it has aspects which are familiar to economists, and it is those aspects I wish to emphasize in this section. An idea can be seen, in some measure, as a public good, as a capital good, as intellectual property, and as an experience good.

The idea that ideas are public goods is not new; it was a staple of the early economics of science literature. Nelson 1959, Arrow 1962 and others have observed that scientific knowledge has public good aspects — its consumption is non-rival, and it is difficult to exclude non-payers once it is made public.[132] Like all public goods, an idea has a collective consumption characteristic — one consumer's use of it does not reduce the amount available to others (non-rival consumption). And,

[131] It will not do to object that the "marketplace of ideas" is more a metaphor than a physical reality (Weissberg 1996: 107). This is true of most labor markets as well. The "labor market" is a metaphorical way of conceiving an important economic phenomenon. (Klamer and Leonard 1994: 23-4).

[132] Not any consumer can consume scientific knowledge, which may require training and other knowledge not possessed by all consumers. Nonetheless, scientific knowledge does have the non-depletability aspect that is characteristic of collective consumption goods.

after publication, it is impossible (or very costly) to exclude non-payers. Clearly, scientific ideas are goods that consumers want, both lay consumers of scientific research (society more generally) and professional consumers, scientists themselves. Because economic theory predicts that public goods will be un- or under-supplied by markets — why produce a good that free riders will consume without paying — the early economics of science literature advocated government funding of science as a means of overcoming the market failure.

New scientific ideas are new, but they are not invented *ex nihilo*. The stock of existing scientific knowledge is clearly a key input to the production of new scientific ideas. Even the great Isaac Newton, who was anything but humble, felt obliged to acknowledge that "if I have seen further, it is only by standing on the shoulders of giants." (cited in Merton 1973: 303). Scientists invariably use the output of other producers in their own research, and technologists use knowledge to develop goods and services. Established scientific knowledge therefore can also be seen as a capital good, which is used to produce new ideas, which themselves may prove productive of future ideas. (Ghiselin 1989: 9). Not all ideas become accepted of course; only those which are actually used will serve as capital goods.

Scientific ideas also have aspects of an intellectual property good. Intellectual property is characterized by high development costs, but low costs of production. Once the product is created, an expensive, risky process, the cost of actually producing it is low — books, films, software, drugs are examples. The danger is that if firms cannot appropriate the returns to their investment in development, because rivals can pirate their creation and produce it cheaply, then firms will not produce intellectual property goods. It is quite easy to claim another scientist's output as one's own; all that needs to be done is to affix one's name. The danger that producers of intellectual property will be unable to appropriate the returns to their investment is what has lead to the advent of legal protections like patents, copyrights, and trademarks.

Finally, scientific ideas also have aspects of an experience good. Unlike a search good, an experience good cannot easily be judged for quality before purchase. Search goods, like clothing or furniture, can be inspected and evaluated before purchase. Experience goods, like a used car, or a book or software, can be appraised only after consumption. Experience goods lead to an informational asymmetry, which induces consumers to consider the producer's reputation, or to consider price as a signal of quality (Akerlof 1970), or to hire independent evaluators, such as critics, or consumer-products magazines. A scientific idea is like an experience good to the extent that it doesn't necessarily reveal all its attributes at once, and, therefore, potential consumers may consider producer reputation, and or independent evaluation as proxies for quality.

Many of these economic aspects of the good called a scientific idea (public good, intellectual property, experience good) involve a kind of market failure. Absent institutional means of addressing these failures — inducing supply of a public good, protecting intellectual property rights, and overcoming information asymmetries regarding product quality — the quantity of scientific knowledge produced will tend to be less than socially ideal. Before we turn to the institutions that science has evolved to overcome these potential market failures, it is important to consider another, related matter in the economics of science — how are scientific ideas valued?

6.2.2. How is the good called a scientific idea valued?

The great advantage of markets is a price system — prices economize spectacularly on the knowledge that agents need to possess (Hayek). Much of the coordination that the Invisible Hand requires is enabled by the information on relative scarcities that prices can provide. Science has no explicit price mechanism. Scientific ideas to not trade on a formal market with explicit prices attached, which is not altogether surprising, given the unusual aspects of knowledge as a good just discussed.

But since scientific ideas obviously can and do have different values, and scientists have strong incentives to know the value of the knowledge that they rely upon (or will come to rely upon) in their research. Without explicit prices, then, the question is: how are ideas valued? The answer is that ideas are valued in a two-stage process: first they are peer reviewed by editors and referees who act as agents for potential consumers (principals), then (if published) ideas are valued by a kind of market test, traditionally measured by citation counts. Post-publication, an idea is valuable to the extent it is used by others who also produce ideas (and products based on ideas).

Scientists rely upon peer reviewers in the same way that consumers of experience goods rely upon independent evaluators (such as critics), to provide information regarding quality that is difficult or costly to obtain. Scientific journals will have different reputations, depending upon the perception of how demanding their standards of appraisal are, as will critics. Scientists themselves will also develop reputations, like those who create films and plays. In fact, as Hayek argues when he characterizes markets as a discovery process, "the function of markets is here precisely to teach us *who* will serve us well." (Cited in Loasby 1989: 199). Consumers rely upon the reputation of both the producer and the independent evaluator in order to make a preliminary evaluation of the good in question. Science values ideas, then, by employing a reputational system.

Professional evaluators provide the first measure of quality; scientists use the journal's reputation as a signal of quality. Once it clears the first hurdle of peer review, an idea also is valued by the reputation of its producer. Ultimately, however, scientific ideas, like ordinary goods, are valued by a kind of market test — the extent to which other scientists actually use it. Author and journal reputations can be used as a initial proxy for actual quality, but the "true" value of an idea consists in the use that others make of it. Particularly useful ideas greatly enhance the reputation of their creators.

As Brian Loasby puts it: "just as the market rewards knowledge which enables someone to offer goods and services which customers wish to acquire, so the reputational system rewards those who produce new ideas which others can put to use: and if the goods are or ideas are unwanted or defective, they will be ignored or criticized." (1989: 39).

In academia, for example, rewards are based upon reputation. More publication is better than less, presumably because this signals production of high-valued ideas, and publication in "elite" journals is considered better still, presumably because this signals even higher-valued ideas. At some universities, citation counts are also considered, on the premise that particularly valuable ideas tend to be used more (and cited when used).[133] Arthur Diamond (1986) has estimated the present value of an additional publication (in 1994 dollars) to a 35-year-old (academic) mathematician as about $6,750. (Cited in Stephan 1996: 1203).

I will have more to say on the costs and benefits of a reputational system as a way of valuing scientific ideas below. For now, I want to note a fundamental difficulty in valuing scientific knowledge. Upon publication, the value of idea is approximated by some combination of the reputation of the producer and of the journal. But the ultimate value of an idea often lies in the future — the use that current and future scientists make of it.

An ordinary capital good, such as a machine tool, has a fairly predictable future course. The future of an idea is far harder to predict, and therefore to value. This is not to suggest that research cannot be analyzed for expected net benefits, only to argue that it will often be difficult as a practical matter.[134] While society surely wants to promote those research projects with the highest expected net benefits, it will be particularly difficult to evaluate expected benefits.

Future benefits are hard to meaningfully quantify *ex ante*, and not merely due to the ever-

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[133] Rewarding citation counts also assumes that citations are not negative, i.e. that other scientists are not attacking rather than using the idea.

[134] Some writers, such as Gerard Radnitzky, maintain that science can indeed be seen as the attempt to achieve efficiency, in the sense of maximizing expected net benefits. Alternative research projects, goes this reasoning, should be chosen based upon expected net benefits. I am not averse to thinking about optimal resource allocation in science. But my view is that most such calculations will be difficult in the extreme.

present risk of failure. As Coase 1974 notes, a scientific idea is likely to have positive (and perhaps even negative) spillover effects in the future, a kind of intertemporal externality that is exceedingly hard to forecast (as the discussion in Section 6.1 emphasized). Spillovers make expected benefits hard to forecast. First, research can produce purely serendipitous discoveries, in the sense of unexpected answers to unposed questions. Second, even anticipated knowledge can have indirect benefits for other research that simply cannot be anticipated. Even retrospectively, the economic value of research can be difficult to gauge. (Daspgupta and David 1994: 490).[135]

Scientific ideas can be valued by the peer-review reputational system. Journal and author reputation provide a kind of preliminary valuation in the absence of an explicit price system. But the ultimate value of a scientific idea will be a function of its future use, the nature of which generally defies forecasting. The ultimate value of an idea is its usefulness **over time**, which is revealed only as the future unfolds. And though scientific reputations are often made by providing particularly valuable ideas, a glowing reputation does not, by itself, guarantee that future output will be equally valuable.[136]

Now we turn to the ways in which scientific institutions help to overcome the market failures that seem to result from the nature of knowledge as a good, and from other incentive incompatibilities in science.

6.3 Rules in science: how institutions help in the production of scientific knowledge

[135] The philosopher William Bartley has emphasized this point about the value of scientific knowledge. (1990). Knowledge is, he argues, hard to value, because it is never altogether fathomable. Those of us who produce ideas have no way of knowing whether and in what fashion our output will be used by future producers. We may not even understand the full contours of our own product, says Bartley, here echoing Karl Popper.

[136] In fact, it is the case that already eminent scientists (e.g., Nobel Prize winners) are awarded recognition disproportionate to their actual contribution (relative to those of fellow contributors), owing to lingering reputation effects. This phenomenon Merton refers to as the "Matthew effect." (1973: 443-47).

Like markets, science, has evolved a set of informal institutions — norms and conventions — that work to overcome certain types of market failure. As the foregoing suggests, especially worrisome are the problems of knowledge as a public good, knowledge as intellectual property, and asymmetric information regarding product quality. In this section, I want to propose that some familiar norms and conventions in science should be seen as evolved institutional responses to these very real problems of market failure.

Let us take the public-good problem first, followed by the potential failures arising from intellectual property and from asymmetric information regarding product quality.

6.3.1 Scientific knowledge as a public good: the conventions of credit, priority, and open publication

Science has evolved a set of institutions to encourage the production of scientific ideas, which have a public-good aspect. Paramount are the institutions of credit, priority, and open publication. First, science rewards scientists who make important discoveries or create innovative ideas with **credit**. In the reputational market of science, credit is the coin of the realm. In their sociological investigations, Latour and Woolgar (1986) found that scientists repeatedly refer to "obtaining credit" as a central motivational goal (cited in Hands 1994). In David Hull's account, the scientist's purposeful pursuit of credit is a primitive concept; Hull makes the pursuit of credit a psychological propensity analogous to Adam Smith's desire for approbation.

Whatever its source, credit, once earned, buys a number of desirable non-cognitive goods: eponymy (Kuhn-Tucker conditions, Plank's constant, Halley's Comet, the Philips Curve, the Euler-LaGrange equation), prizes, promotion, higher wages, election to learned societies, and collegial esteem. (See Merton 1973: 297-303 and Stephan 1996). A scientist, then, is not wholly uncompensated for producing ideas. To the extent that other scientists make use of her output, (and, importantly, acknowledge this use), a scientist will receive more credit, a greater reputation, and the non-cognitive goods which attach to reputation. In effect, free riders pay for the public good with citation.[137]

Note well that the indirect compensation that credit provides accrues only if scientists who use ideas acknowledge that use, in the form of citation (or other acknowledgment). You can use the idea, but you must cite its creator. Failure to cite properly is a failure to recognize the (generally informal) property right held by the original producer (Merton 1973: 291, ff. 19), a kind of theft. Failure to cite may be plagiarism (knowing theft), or it can result from laziness or ignorance (unknowing theft). Credit provides indirect compensation to public-goods producers only when scientists actually observe the citation norm designed to protect property rights, and, thereby, facilitate the awarding of credit. (See Section 6.3.2 below).

The public-good problem of scientific knowledge is addressed by the convention of awarding credit, provided the norm of proper citation is observed. The institutions of credit and citation help to ensure that scientific knowledge is actually produced. But science also wants its ideas to be produced rapidly (sooner is better than later), and to ensure that they are diffused widely. The reputational system in science has evolved two other conventions that work to meet these additional goals, which are not always in the individual scientist's interest.

The two conventions are priority and open publication. Science generally award credit not just to those who produce a valuable scientific idea, but to those who produce it first. Robert Merton 1973 observes that scientific credit usually is awarded based upon **priority** — being the **first** to publish. Credit is awarded only or primarily to first publishers. The convention of priority creates incentives for more rapid production of valuable ideas. In many settings, priority results in a winner-take-all structure. Thus, while priority clearly encourages more rapid innovation, it also may also have

[137] Citation has direct pecuniary consequences when, for example, academic promotion decisions consider citation as well as publication counts.

adverse effects.

When priority has a winner-take-all structure, it creates incentives for rapid innovation, a good thing. But a winner-take-all structure also increases the likelihood of fraud and error in the rush to be first, and, in addition, may result in wasteful *ex post* gamesmanship (Merton 1973: 317), and in socially inefficient expenditures analogous to the patent-race problem. (On this point, see Daspgupta and David 1994). From the individual scientist's perspective, there is a tradeoff between the danger of waiting too long to publish (and thereby losing priority), and rushing into print, which risks errors, and reduces useful early feedback from colleagues. (Hull 1988: 352).

What is clear is that scientists respond to the reward incentives that priority creates. As Merton 1973 nicely documents (discussed in Section 4.3), the battles over priority are fierce, even bloody, and involve some the greatest scientific names. "During the last three centuries in which modern science developed," says Merton, "numerous scientists, both great and small, have engaged in acrimonious controversy." (1973: 287).

If scientists were selfless or indifferent to non-cognitive goods (collegial esteem, wealth, prestige, etc.), then we would expect few or no disputes over priority. But the history of science is unkind to the received-view conception of scientific motivation. The great struggles over priority that characterize much of scientific history are compelling evidence that scientists are people too— agents who are self-interested (if prudent), who possess non-cognitive goals and who, therefore, respond to the institutional incentives in the structure of science. Says Merton: "the recurrent struggles for priority, with all their intensity of affect, far overshadow . . . cases of noblesse oblige . . . ." (ibid: 290).

Another institution in science, open publication, arises from the fact that scientific knowledge is a quasi-public good — it is non-rivalrous in consumption, but exclusion of non-payers is possible. A scientist who has produced a good idea can opt to keep it secret, rather than publish. Other scientists (and technologists) can make use of an idea, only if it is published following its production. The potential ability of knowledge producers to exclude non-payers gives rise to the institution of open publication.

While open publication of new scientific ideas is clearly desirable for science collectively, it is may not be for the individual scientist. Publication risks revealing procedures or results to rivals seeking priority on other fronts; it risks theft, and, in some situations, it may also undermine lucrative commercial opportunities. The chemists Fleischman and Pons, to pick a recent example, claimed to have produced cold fusion in bottle, a spectacular discovery they initially refused to publish, presumably because the potential profits were so large. Scientists prefer to get credit for the ideas they produce, without revealing too much.[138]

To meet this potential incentive incompatibility, science has evolved a convention analogous to priority ("credit is awarded to first discoverers"). It is the convention of open publication: "credit is awarded only upon publication." Generally, one must publish to obtain credit. In academia, for example, it is published research, not research *per se*, that is the main determinant of academic advancement, and therefore of wages.

I don't wish to claim that one cannot find examples of credit awarded to scientists who were second in a race (or who made an independent discovery at roughly the same time), or (eventually) to scientists who never bothered to publish their results. Not all scientists respond to the incentives created by the ancillary institutions of priority and open publication. But it is clear that most do.

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[138] Scientists would prefer to obtain the benefits from priority, without the potential costs of open publication. Merton documents practices designed to meet this desire, such as publishing in code, and depositing sealed and dated manuscripts with scientific societies. David Hull reports that "Galileo, Newton, Hooke, and Huygens translated statements of a new law or principle into an anagram and then made it public. While others tried to unscramble the anagram, the author could continue working undisturbed." (1988: 323). Priority is why some contemporary journals print the manuscript submission date, as well as the acceptance and publication dates. (Merton 1973: 315-16).

The reward conventions which indicate how credit (and therefore reputation) is to be obtained, generally work precisely because scientists care about getting credit. Scientists **do** care who gets there first, and do publish openly, because science is structured so that these collectively beneficial behaviors are individually rewarded.[139] Priority battles in science are a compelling counterfactual to the claim that scientists are selfless truth seekers.

Each of these scientific institutions — credit, priority, and open publication — can be seen as evolved responses that attempt to overcome a market failure inherent in the nature of scientific knowledge — ideas are a public good. The reward conventions of science help to induce Smithian scientists to produce and publish scientific ideas, when there are incentives not to produce at all (the public goods problem) or not to publish any production. Market failures such as public goods are instances where optimal behavior creates undesirable outcomes. In science, no less than in markets, institutions can work to mitigate this outcome.

Now we turn to the second source of potential market failure, the intellectual property aspect of scientific ideas.

6.3.2 Respecting intellectual property: the institution of citation

As with the public-goods problem, the reward conventions of science provide incentives for scientists to produce scientific knowledge. But, as we have seen, the indirect compensation provided by credit depends upon scientists giving credit where credit is due — that is, observing the norm of citation. Citation recognizes a kind of property right, thereby helping to facilitate the indirect payment scheme of the reputational system in science.

As in all markets, the danger is that individual scientists may not observe the norm of citation, that is, they will fail to respect the intellectual property rights of other producers. Since valuable credit

[139] Sometimes, of course, the incentives to withhold (for potential commercial gains) will overwhelm the institutional incentives to publish openly.

can be obtained merely by attaching one's name to someone else's work, we can, with Hobbesian (opportunistic) scientists, expect some theft, depending upon how efficiently property rights are enforced. (Hull 1988: 342).

As noted above, failure to cite can be plagiarism (knowing theft), or it can result from laziness or ignorance (unknowing theft). Plagiarism takes two forms: (1) theft of published work, and (2) theft of unpublished work, such as lecture notes or working paper ideas, or submissions to journals, conferences or funding entities. Theft of published work is probably more likely to be detected than theft of unpublished work (especially grant submissions) during the peer-review process. A subtler form of theft occurs when a senior scientist adds her name to a paper to which she has not contributed, but which was produced by junior scientists or scholars in her laboratory or otherwise under her aegis. All are attempts to acquire valuable credit (and its attendant goods) for intellectual property that belongs to others.

Generally, we should expect plagiarism to be more easily detected than ordinary thievery. An ordinary good has value even when hidden, but an idea gets credit only upon (re-)publication, which puts the idea thief at more risk. Here is a case where scientific institutions are partly reinforcing — open publication works to reduce the expected gains from theft of intellectual property.

The institution of citation is designed to protect intellectual property rights (see Kohn 1986: 1-4), but it will work only if most scientists are honest (don't plagiarize) and careful (don't inadvertently steal). There are quality controls that help to enforce the norm of citation — papers are checked by reviewers and then by a more general readership.[140] Quality control via the review process and norms regarding errors avoidance are considered in the next section.

6.4 Information asymmetries: institutions that promote quality

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[140] There are also, in some settings, incentives to trade cites — a practice known as logrolling. This can be seen as a kind of dishonest citation, if not especially harmful.

The third important market failure which derives from the production of scientific knowledge pertains to product quality, in particular the informational asymmetry between the consumer of an idea and its producer. The producer knows far more about the quality the idea, because she knows the details of its production. Generally, we can think of the quality of scientific knowledge in terms of its reliability.

Knowledge that is unreliable, due to error or to fraud, will beget more error in all the new knowledge production that uses this "tainted" stock— a kind of perverse multiplier effect. Because science relies so utterly on existing knowledge — ideas are capital goods — the most damage occurs when scientists make use of knowledge that is low-quality, or unreliable.

The information asymmetry makes it impossible (or too costly) for individual scientists to certify the quality of every result she that she uses, directly and indirectly. The stock of scientific knowledge that a scientist relies upon is far too vast, even in a specialized area, for any one scientist to completely assay. Indeed, science progresses precisely because it increases the amount of knowledge that the scientist can simply **accept** as reliable. Most of what a working scientist believes she simply accepts as correct, without undertaking independent verification. Scientific knowledge, seen this way, is spectacularly economizing, as recognized by Charles Sanders Peirce. "[K]nowledge that leads to more knowledge is more valuable in proportion to the trouble it saves in the way of expenditures to get that other knowledge." (cited in Rescher 1989: 5-6).

Scientists accept established knowledge because they trust that these findings were produced without fraud or error. "[S]cience," says Jacob Bronowski, "involves an implicit social contact between scientists so that each can depend upon the trustworthiness of the rest . . . ." (cited in Kohn 1986: 1). Trust is a moral sentiment that is economizing. Trust economizes by reducing the spectacular expenditures that would be required for every scientist to independently assay every result she makes use of. For the scientist, it's far cheaper to trust one's colleagues. But, of course, science does not depend upon integrity alone. It trusts because it verifies.

Verification is the public scrutiny of research results. As we have noted, it generally takes two forms. First, an idea (submitted for publication or for grant funding) is peer reviewed, scrutinized by editors and referees. Second, published work is then checked, or replicated by other scientists. Not all work is scrutinized equally in the review process and in subsequent checking, and some review processes are more demanding than others. It is probably the fate of most scientific papers never to be read, much less checked.[141] Still, as Merton points out, "Scientific inquiry is in effect subject to rigorous policing, to a degree perhaps unparalleled in any other field of human activity." (1973: 311). In this sense, trust is a belief that there exists an adequate, if imperfect, process of quality control, both before, during and after peer review.

As noted above, when it is costly or difficult to judge product quality, consumers have incentives to overcome the adverse-selection problem that may result. Like firms who establish a reputation for quality goods, or for "honest dealing," scientists who are especially scrupulous in their work may develop trustworthy reputations to signal quality to potential consumers. The producer's "brand name" can help consumers evaluate quality. The second line of defense is peer review, and the third line of defense is post-publication checking, or replication. Let us consider the institutions that work to promote quality, at each stage of review.

6.4.1 Institutions that promote quality before peer review

The institutions that scientists are supposed to observe in undertaking their research are those that are probably most familiar — the dos and don'ts designed to promote reliable knowledge before it reaches the peer review process. These norms concern how a scientist is to work with her evidence, how she is to obtain her results, and how she is to present her ideas. Reduced to their essence, these

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[141] Collins 1993 estimates (though provides no source) that 90 percent of published papers are never even cited, never mind replicated.

scientific norms are: don't lie; don't cheat; don't obfuscate; don't err. (These norms can also be rendered in an affirmative fashion: be forthcoming; show integrity; be clear; be careful).

The norms in science against lying and cheating are familiar. Take cheating first. Merton cites a wonderful classification of data-manipulating sins, by Charles Babbage, the prescient 19th-century mathematician and inventor of computing machines. (1973: 309-11). They are: "forgery," "trimming," and "cooking."

Forgery is the outright fabrication of data; using wholly invented results. Forgery is first-order scientific fraud. Trimming consists in throwing out troublesome data in a given data set; in modern terms this is known as purging the "outliers." There can be legitimate theoretical reasons for "cleaning" data sets, of course, but trimming refers to the practice of deleting evidence only because it is inconvenient for one's hypothesis. Cooking is a related practice that consists in the strategic selection of data most hospitable to one's hypothesis. It is a more egregious form of data mining, which makes use of the fact that **some** confirming data can be found for almost any idea. Says Babbage: "the cook must be very unlucky if he cannot pick out fifteen or twenty [observations] which will do for serving up." (Cited in Merton 1973: 310-11). Fabrication and fudging will not always produce unreliable results, but *ceteris paribus*, they will obviously create more of it.

The norm against lying says "don't misrepresent procedures and findings." This means that scientists should accurately and fully report their work. Note that this norm includes sins of commission (lying about one's procedures) and of omission. Selective reporting may entail a sin of commission, an attempt to disguise, for example, specification search when undertaking hypothesis tests. But even procedural omissions which are not designed to mislead may be problematic. This will occur when there is valuable information which did not find its way into the paper actually published, such as rejected hypotheses, or known problems with data not employed.

At a minimum, empirical economists, for example, should make publically available the means

— data and programs — for others to replicate their results. (Mayer 1993a). (On the issue of specification search and publication bias, see Section 1.3.2 and the discussion of empirical standards in Chapter Seven). The norm of full disclosure is intended to facilitate the important policing function of peer review. But the norm imposes "compliance" costs on individual scientists, and will function best when there are incentives that work to overcome these costs.

Another norm in science that pertains to the production of knowledge before peer review is, "be clear," or alternatively "don't obfuscate." "Be clear" is a norm that pertains less directly to the reliability of a scientific idea; clarity influences the cost of consuming ideas (though, in extreme cases, egregiously unclear writing can affect reliability as well). Economics, like any science, benefits collectively when its published research is written with clarity. Consider an example, due to Leland Yeager 1995. The premise is that clearer writing by producers conserves on reading time by consumers. With enough readers, there will be high returns to clarity. If, for example, a scientist works eight additional hours to save 500 readers 12 minutes of time, the (net) gain to society is 92 man hours. (Yeager 1995: 24-5).

Absent incentives to invest time into clearer writing (in effect, to internalize a negative externality), some writers will not do so, and the superior collective outcome is not realized. Seen this way, "be clear" is not merely a stylistic injunction, it is a norm that promotes greater efficiency in science, by attempting to overcome an incentive incompatibility problem in the production of scientific knowledge. It for this reason that Karl Popper said, "Aiming at simplicity and lucidity is a moral duty of all intellectuals: lack of clarity is a sin." (Cited in Coats 1983: 32).

If clarity is a methodological virtue, it is a virtue not always observed in academe. It may be that the incentives to do the socially efficient thing are inadequate, or, it may be that there are conflicting institutions. All too common in academe is a perverse convention, which can be rendered as follows: "in the absence of other means, use obscurity to signal erudition or cleverness." Obscurity takes many forms and is by no means unique to economics, where the preferred form is arcane mathematics. As Thomas Mayer (1993a) points out, other disciplines have their own forms of obscurity — inscrutable jargon and footnote-itis (conspicuous erudition) being the favorites in less technical fields.

The final norm which pertains to the production of knowledge before peer review is "be careful," or, alternatively "work to avoid error." Scrupulousness in procedure, and in the accounting of those procedures, is obviously important to the quality of scientific knowledge. Other things equal, careful work is more likely to produce reliable knowledge. Clearly there are incentives for the individual scientist to be careful, since errors work to undermine success. As mentioned however, a reward system with emphasis on priority creates a countervailing incentive to publish sooner rather than later.

Let us summarize these scientific norms, with reference to Rescher 1990:

> *Be honest*: Do not cheat (manipulate data), or lie (distort findings, misrepresent work), or steal (plagiarize);
> *Be clear*: Formulate your findings intelligently, avoid imprecision, equivocation, obscurity.
> *Be careful*: Strive to avoid error. Do not be sloppy, indifferent to precision, or heedless of pitfalls. (Rescher 1990: 49, with modifications).

Now we turn to the scientific institutions that work to enforce quality after the good has been produced, the practices of peer review and replication.

6.4.2 Peer review and replication

Peer review and replication are the crucial policing functions in science. They serve double duty — they help to overcome the informational asymmetry between producers and consumers of scientific knowledge and they also attempt to enforce the norms of knowledge production just discussed. Peer review and replication help to signal product quality to other scientists, and, they also provide some incentives for scientists to produce reliable knowledge in the first place.

While vitally important to science, the review process is necessarily imperfect. Unlike in some ordinary goods markets, it provides no enforceable warranty to the consumer of scientific knowledge. The consumer can still have product quality information that is inferior to that of the authors, who know whether they cheated, or were careless. (Feigenbaum and Levy 1993).

There is also a familiar agency problem between peer reviewers and consumers. The agents charged with peer review (editors and referees) may have different standards than the principals they are charged with serving, for example. There are other, related, incentive problems at the review level. Refereeing is a demanding and time-consuming job, analogous to contributing to a public good, i.e. referees must observe similar norms. Referee reports, like original research, can also be late, or sloppy, or insufficiently diligent. And, in the case of editors, there are obvious incentives for corruption.[142] This gives rise to the old question, who is to monitor the monitors?

A partial answer is post-publication replication, an additional level of monitoring. Additional replication can help assure consumers of the reliability of published research, and of the review process. This is accomplished by independent efforts to check results after publication — the practice of checking or reproduction — and more indirect replications, as when results are used in new research.

Replication takes us to an important issue: are their sufficient incentives to observe scientific norms, and to what extent do scientists, in practice, observe scientific norms?

6.5 Getting the incentives right: do scientists play by the rules?

Replication, like peer review, is costly and it helps if there are incentives for scientists to undertake it. An important mechanism in science for promoting replication is competition. The

[142] If a published paper is worth about $5,000 to its author, then the editor of a journal which publishes 50 articles a year can develop a remunerative sideline, particularly where the marginal differences among published and unpublished papers are small. Less egregious forms of corruption would be nepotism and mutual back-scratching.

scientist can expect more post-publication checking of results, *ceteris paribus*, when science is competitive. It is unrealistic, economics suggests, to expect scientists to ruthlessly falsify their own hypotheses. But, as David Hull (1988: 4) points out, their rivals will be happy to do so. Competition promotes replication, because competitors clearly have an incentive to refute results that are inimical to their own work.

There is, for example, a new literature in economics that argues that the minimum wage does **not** result in adverse employment consequences for low-skilled workers (Card and Krueger 1995). Those who find this result pleasing, such as political proponents of minimum wage increases, are unlikely to closely scrutinize Card and Krueger's results. But those who believe that a minimum wage does have adverse employment consequences, and have built scientific reputations on this view, are likely to examine Card and Krueger's findings rather skeptically, and have, in fact, done so. (See the discussion in Chapter 7).

Competition promotes replication, which promotes reliability and helps to sustain the socially useful norm of trust, which, in turn, greatly economizes on the production of scientific knowledge. Competition is therefore crucial to science. As in ordinary markets, less competition reduces the incentive for producers to provide what consumers actually want. If science wants reliable knowledge, competition works to promote it. Less competition means less independent checking, which, *ceteris paribus*, means less reliable scientific knowledge.

Competition, argues the economic view, is good for science. Given the right institutional framework, we **want** scientists to be partisans of their knowledge products. Popper (1975: 93) argues that "should individual scientists ever become 'objective and rational' in the sense of 'impartial and detached', then we should indeed find the revolutionary aspect of science barred by an impenetrable obstacle." (cited in Hull 1988: 359). An economic perspective helps to see that science succeeds not in spite of competition, but because of it. Low-quality output — whether due to error or fraud — is more likely to be revealed, hence discouraged, when there are parties with an interest in replication.

As with ordinary markets, too much competition (i.e., unhealthy competition) is destructive. The Hobbesian (opportunistic) scientist will, when advantageous, sabotage a rival's experiments (destroy data, contaminate samples), actively undermine her funding or promotion opportunities, slander her, arrange to have her papers rejected and so on. This kind of "competition" is no more good for science than it is good for ordinary markets. Hence the idea of beneficial competition is partly parasitic on agents who are capable of eschewing rank opportunism. Like ordinary markets, science benefits from competition principally when agents are Smithian, i.e., self-interested but prudent. The Smithian scientist has moral sentiments ("internalized" norms) that will constrain opportunism to some extent, and thereby work to keep competition on the healthy side of the fence.

But science also has an advantage in enforcing norms that ordinary markets do not, which derives from the special nature of knowledge production. It is this: scientists need the output of their fellow producers, including that of their rivals. Unlike most ordinary producers, the scientist uses the output of her competitors as an input to her own production, and, as a result, values her rivals in a way that producers of ordinary goods do not. Scientists thus have an additional incentive to refrain from misconduct that ordinary producers do not.

The producer of ordinary goods is delighted to have fewer rivals, or to have existing rivals weakened. Other things equal, this entails higher profits, and there is therefore the risk of unhealthy competition. The producer of knowledge, in contrast, benefits from her rivals' output. The scientist has an incentive not to abuse her competitors (by slander, say, or an unfair referee report, or other sabotage), that goes well beyond the incentives facing the ordinary producer. David Hull makes the point as follows, "Because scientists must use the work of other scientists, they are forced to "cooperate" in a metaphorical sense with even their closest competitors, i.e. use their work." (1988: 514).

6.5.1 What evidence is there of misconduct in science?

Is there evidence of misconduct in science? The short answer is yes, though there is very little empirical research on the extent to which working scientists fail to observe the scientific norms we have reviewed. Broad and Wade 1982 and Kohn 1986 are good introductions to the issue.

Broad and Wade report 34 cases of known or suspected fraud in science. What makes their reporting interesting is that the list of suspects includes some of the most eminent scientists in scientific history: Ptolemy, Galileo, Newton, Bernoulli, Dalton, Mendel, among others. (Cited in Wible 1995, Section 3.10). Kohn likewise surveys many famous cases of fraud (Piltdown man; Sir Cyril Burt), and some less well known of plagiarism (Dr. Elias Alsabati), and other cheating episodes, especially in clinical medical research. There is new evidence, following the recent release of his laboratory notebooks, that the great Louis Pasteur published results quite different from those recorded in his private notebooks. (Geison 1997). James Watson has confessed that, in the rush to discover the structure of DNA, he actually stole a colleague's spectrographic photographs. (Watson 1968, cited by Donald Campbell in Callebaut 1993: 295).

Apart from these famous examples, there is very little detailed research on misconduct in science (or by scientists outside science). Kohn cites one 1976 study in the *New Scientist*, which surveyed it readers on the incidence of "intentional bias" (a euphemism for deliberate data manipulation). Of 201 valid questionnaires returned, 194 reported having known of this kind of misconduct, of which one-third involved direct or indirect personal contact. Of the incidents actually reported, about one-fifth involved catching the perpetrators in the act. (Kohn 1986: 8). Generally, however, the data on misconduct in science are very limited.

The scarcity of data suggests two possibilities: (1) either there is very little misconduct in science, or (2) there is misconduct and science fails to detect it or to report it. (The latter is Broad and Wade's (1982) position). Let us consider the latter position. Apart from self-reporting or whistleblowing by collaborators (the incentives against which are clear), the main device for detecting misconduct in science is, as noted, peer review and replication, both direct and indirect. Absent these screens, the expected costs to misconduct decrease. Hence, to the extent that peer review and replication are rare (or undemanding), we would expect more misconduct.

The problem is particularly acute in the social sciences. Where methods are less developed, phenomena are less regular, and environments are less stable over time, peer review and replication are less likely to serve their enforcement function. As Donald Campbell says:

> [T]he social system [of science] which enforces honesty in communicating data from the laboratory . . . is the precious secret of scientific validity, and it is the one we should worry about losing, especially in an area such as the social sciences in which there is pressure for publication in an area where no one can check your work anyway. (In Callebaut 1993: 295).

As we saw in Chapter One, there is relatively little replication (of the direct, "checking" kind) in economics. Feigenbaum and Levy 1993 suggest that this is true because, for the individual economist, the returns to replication are far lower than the returns to publishing original research. Economic journals only rarely publish positive replications (in the sense of reproduction), and even negative replications (such as the Social Security episode with Martin Feldstein) are rather scarce. (Tom Mayer (1993b) reports that only two journals in economics will consider publishing positive replications). As long as publication is the key to academic advancement, and replication is unlikely to lead to publication, we should expect very little replication.[143] Replication is good for science, but, as in economics, it may not be in the individual scientist's interest to replicate.[144]

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[143] I am referring here to replication in its narrow sense of "reproduction." As Bob Goldfarb has pointed out to me, the paucity of published reproductions does not preclude more indirect kinds of replication, which occur when a particular regression equation becomes a standard in a literature. Goldfarb suggests that, in labor economics, "[Jacob] Mincer-type" earnings equations, having been run in many settings, and with different data sets, may be an example of indirect replication.

[144] Of course this begs the question of **why** the returns to replication are low in economics. One possibility, noted in Section 1.3.2, is that economists are sophisticated about the nature of econometric

6.6 Methods as coordinating conventions: neoclassical economics

Not all scientific institutions are norm-like, that is, not all institutions in science are working to induce cooperative outcomes when individuals have incentives to "defect" to the individually preferred (but socially undesirable) choice. Some scientific institutions can be seen more as coordinating conventions. An example, I suggest, are methods.

Consider the canonical principles of neoclassical economics, as discussed in Chapter Five. The maximization *cum* equilibrium approach characterizes the method of neoclassical economics. Agents are not really maximizers and markets are not continually in equilibrium, but neoclassical economics assumes that they are, which is a methodological convention. (I noted in Section 5.3 Oliver Williamson's point that economics itself therefore offers some evidence against the idea of maximization). Economists use theoretical conventions like the maximization *cum* equilibrium framework, in part because our bounded rationality demands such shortcuts. But a conventional method also helps researchers to coordinate

The convention of putting all human action into the maximizing *cum* equilibrium framework comes at a cost, as all theoretical shortcuts do. But the convention also provides the benefit of coordinating research endeavors, particularly when there is uncertainty as to which methods are best. A widely shared analytical framework is a kind of convention that is coordinating, and reduces the transaction costs of dealing with many (or more) frameworks. Substantive considerations to one side, then, coordination on a given method may well be superior to a failure to coordinate, which characterizes fields (sociology, e.g.) where researchers use a welter of different methods and approaches.

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work, and don't take its results all that seriously. Economists don't regard econometric results as truly "testing" some hypotheses, but, rather, as lending support to or as illustrating the theory. (Mayer 1993b: 271).

One can think of coordination on research in the simple game-theoretic terms of Sections 5.4.3 and 5.4.4. Consider Figure 6.1 in this regard.

Figure 6.1. The Methods Coordination Game

		Beta Method 1	*Beta* Method 2
Alpha	Method 1	y,y	0,0
Alpha	Method 2	0,0	x,x

The game is written so that coordination on methods is better than not, i.e. $y > 0$ and $x > 0$. A shared framework (coordination) is better than none, as in the Driving or Stag Hunt games.

Importantly, however, and this is where substantive considerations reenter, there is no guarantee that the conventional method is first-best, an outcome we considered in Section 5.4.4. Depending on the values of x and y, (and, obviously, the game can involve more than two choices), the current convention may be Pareto-inferior.

As discussed in Section 5.4.4, conventions can be inefficient, perhaps due to lock-in effects that arise from path-dependency situations, as when there are network externalities. A shared analytical framework, such as the maximization *cum* equilibrium framework of neoclassical economics, has the virtue of enabling coordination, which reduces transactions costs. But it may well be second-best, or third-best. If a method may be seen as conventional, then we can think of it as beneficial but not (necessary) best in the production of scientific knowledge.

6.7 Science as an invisible-hand process

Virtually alone among intellectuals, economists see the virtue of (functioning) markets. Economists like markets because (1) markets promote invisible hand outcomes; that is, they take mostly self-interested agents and produce unintendedly beneficial social outcomes, and (2) markets are self-correcting processes, spontaneous orders that accomplish (1) without the need of expensive central planning. Other intellectuals disagree, principally because they find our invisible-hand arguments counter-intuitive. They prefer to see human beings as mostly collective-minded creatures, or believe that human beings can be so improved by the right kind of social engineering. Hence, most intellectuals are not so impressed with the value of a mechanism that aligns private interests with the common good.[145]

Connected is the idea that markets (commerce, more generally) are corrupting — intellectuals generally see markets as morality-free zones of unconstrained greed. (Haskell 1984: 214). But this is not the case, as I have tried to show, and, here, at least, economists are partly at fault for not emphasizing the importance of institutions in enabling invisible-hand outcomes. Markets depend altogether on institutions that protect persons, property, contracts, and that address other sources of market failure, such as public goods, informational asymmetries, and intellectual property. Markets also need the partial restraint (from opportunism) of agents who have the right moral sentiments (Smith not Hobbes), if they are not to incur crippling transactions costs.

Max Weber made the point as follows: "Unlimited greed for gain is not in the least identical with capitalism, and is still less its spirit. Capitalism may even be identical with the restraint, or at least the rational tempering, of this irrational impulse." (Cited in Haskell 1984: 214). Markets need rules, and can only prosper when those rules function.

The situation in science is directly analogous, I have argued. Like markets, science functions best with freedom (to innovate, to investigate, to be skeptical, to dispute, to criticize), but not so much freedom that collective goals are unrealizable. There must be some rules in science, and, in many instances, these rules will restrict freedom. The paradox, a staple of political philosophy, is that the surrender of some freedom is required to maintain the order which sustains freedom. The rules which

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[145] As Coase 1974 points out, intellectuals are keen to regulate ordinary markets, with the self-serving exception of their own — the intellectual market.

limit individual liberty can be seen as the price of avoiding anarchy.

In this limited sense, then, Feyerabend (1975) is right. Science, like markets, **is** somewhat anarchic. Overly rigid (algorithmic) methods may stifle innovation, and the history of science does have examples where great scientists innovate by flouting the rules. But science is **not** chaotic in the sense that "anarchy" implies; it is not random, crazy, unreasonable, or dangerous like a Hobbesian state of nature. Science is more like what Hayek called a spontaneous order, where the powerful motive force of self-interest is channeled by a robust institutional framework.

This is not to claim that all conceivable institutions in science are efficient, but is to argue that the crucially important norms and conventions discussed in this chapter are what enable science to produce the invisible-hand result of reliable knowledge. Hence, it is not the case that "*any* rule-bound methodology is objectionable" (McCloskey 1985a: 20, emphasis added). To the contrary, some rules are what keep freedom from descending into anarchy.

The great value of markets, Hayek argued, is that its **processes** provide incentives for fallible agents to "find and make use of knowledge, while simultaneously affording checks on its misuse." (Loasby 1989: 37-8). The same is true in science; it is the process of science that validates its results, and the efficacy of that process depends upon the institutional framework. (ibid: 198). Scientists respond to incentives, many of which, I have argued, are created by institutions.

The conventions of the reputational system induces scientists to (publicly) produce ideas by rewarding them with credit. Peer review and replication (direct and indirect) provide incentives to produce ideas that are reliable, i.e. ideas which other scientists can use, and signal other scientists regarding product quality. Even with Smithian, epistemically-sullied scientists, the right institutions can work to provide the collectively beneficial outcome of reliable scientific knowledge.

The economic idea that incentives matter in enabling invisible-hand outcomes is foreign to both the traditional philosopher of science, who begins with the received-view premise that scientists are selfless truth-seekers, and to the sociologist of scientific knowledge, who denies that invisible-hand outcomes are possible in science. Both the sociologist and the traditional philosopher take the selfless truth-seeking scientist as a necessary condition for the realization of valid scientific knowledge.[146] Hence, both believe that a showing that scientists are self-interested and epistemically-sullied necessarily rules out the possibility of reliable scientific knowledge. The only difference is that the traditional philosopher fears such a result (and therefore defends an uneconomic view of scientific motivation), while the sociologists embraces it, with post-modern glee.

The economist, however, argues that collective outcomes can often differ from individual intentions. The theorist of science needs to examine both individual behavior **and** the institutional setting in order to determine collective outcomes. (Goldman and Shaked 1993: 252). We cannot assume that what's individually rational coincides with what is collectively rational. It's possible that entirely self-interested scientists with mostly non-cognitive goals can produce, in aggregate, reliable scientific knowledge. (See Goldman and Shaked 1991).

Polanyi (1962) argues that science is a invisible-hand process because it is structured so that scientists are induced to coordinate their independent initiatives by mutual adjustment to the output of others. In fact, says Polanyi, coordination in markets using prices is a special case of a more general kind of coordination, mutual adjustment.

> [T]he coordinating functions of the market are but a special case of coordination by mutual adjustment. In the case of science, adjustment takes place by taking note of the published results of other scientists; while in the case of the market, mutual adjustment is mediated by a system of prices broadcasting current exchange relations . . . . (1962: 56).

Scientific ideas do not carry prices, but they do have values, established first by reputation, and ultimately by use over time.

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146 For the sociologists, of course, the selfless truth-seeking scientist is not sufficient.

When a scientist uses another's idea, she is, in Polanyi's view, unintendedly promoting the collective goal of producing reliable knowledge. This is because scientists want, no need, knowledge that is reliable, and they are unlikely to sabotage their own work by choosing ideas on other grounds. In the long run, scientists will use good ideas and eschew bad ones, even if this means "buying" the idea of a rival. It is in the scientist's individual interest to consume reliable knowledge.

I don't wish to claim that the reliability of a given idea is instantly known, or that scientists cannot be stubborn in ignoring ideas that they would prefer not to accept. For the scientist, as for all agents, there is sometimes a discontinuity between what she wants to be true and what she takes to be true. Self-interested scientists are, like all human beings, adept in ensuring that the latter comports with the former, and some will never change their minds, even in the face of overwhelming evidence to the contrary.

Still, in the long run, scientists, unlike ordinary consumers, are sometimes obliged to consume goods they would rather not. It is painful to admit that a rival has a better idea, or to concede that one's own output is inferior, but there are situations in where it is rational to swallow one's pride and do just that. In fact, it is these instances that science can be said to be "objective." As the philosopher Helen Longino argues, science can be seen as objective precisely in those instances when scientists choose ideas they would prefer not to. In her view, objectivity in science consists not so much in exact representations of nature, but in developing a "*nonarbitrary* account of natural processes that doesn't simply impose our own wishes for how the world is on our description of the world." (In Callebaut 1993: 25-6). Science succeeds as an invisible hand process not just because of the unique institutions that science has evolved in response to market failures, but because the individual incentives to produce reliable knowledge sometimes oblige scientists to make use of their rivals' (and others') output.

Finally, I do not wish to claim that science always works, nor that it works perfectly, nor even that it works most of the time. Scientific rules, norms and conventions are imperfect, they often require trade offs, and they are regularly flouted. Producer (or evaluator) reputation is often an imperfect proxy for product value. Plagiarism, lazy practice, data massaging, misrepresentation of results, and outright fraud are part of the everyday stuff of science. Some of the greatest scientists are known to have cheated, lied, and stolen. Even a good institutional structure works to meet beneficial collective outcomes only some of the time. But this should not surprise us, for it is true of markets as well. David Hull notes, "the important feature of science is not that it *always* produces increased knowledge, but that *sometimes* it does." (1988: 26). It is because science is **sometimes** successful in producing reliable knowledge that its enabling institutions merit close scrutiny.

6.8 Conclusion: the nature of the rules in science

The most important results of Chapters Five and Six are contained in the following two ideas. First, science works because it has an effective institutional framework, not because scientists have superior moral probity (see Section 6A.2), or are exclusively truth seekers, or are, in some other way, fundamentally different from everybody else (*contra* the received view). If science is more successful in realizing its collective goals than are other social processes (such as politics), it is because science has evolved an institutional framework that provides individual agents with incentives to promote collective goals.

Second, there is a reason to rules. A Smithian economic view of science offers a theory of rules in science, hence a rationale for their existence. The conventions and norms (and enforcement mechanisms) that constitute the institutional framework of science are best understood as evolved responses by Smithian agents to market failures (and other incentive incompatibilities) and coordination problems that arise in the unusual conditions of producing scientific knowledge. Scientific rules are not inexplicable cultural imperatives or matters of mere politeness, as they appear to be in more sociological accounts. McCloskey, for example, invokes a subset of scientific rules, the

Habermasian norms, as important to science, but offers no theoretical rationale for them.[147] Important scientific rules have both a history and a purpose.

It is equally important to recognize that agents who observe rules are not irrational, nor does rule following imply unthinking choice, as economists might conclude. If one conflates rationality with maximization, then rule following will be regarded as irrational, but this will only be the case when an individually optimal choice is defined, attainable and desirable. In all other instances, which is to say most instances, rule following can be rational indeed.

The point is that scientific institutions do not exist for their own sake; nor are they are inexplicable phenomena. Institutions are crucial to science as a market-like process. They are the social structure that enables science to produce reliable knowledge.

Seeing institutions in economic terms reinforces some of the conclusions at the end of Chapter Four. Scientific institutions, it is now easier to see, do not exist for their own sake; they need not be tacit (one **can** write them down), and they can apply more generally, traveling across communities. Let us take each of these points in turn.

Recall from Section 3.7.2 Richard Rorty's argument that scientific norms exist for their own sake. An economic view of institutions disagrees. "Be honest" is not invoked to promote honesty for its own sake, but to increase the likelihood that science will produce reliable knowledge. The convention of awarding credit upon publication and the norm of recognizing intellectual property rights with citation are not for their own sake, but are attempts to overcome market failures that inhere in the nature of knowledge production. An institutional framework (however imperfect) exists to promote collective goals in science, just as ordinary market institutions exist to promote collective economic

[147] Recall the "Habermasian" norms: "Don't lie; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence in aid of your ideas." (McCloskey 1985a: 24).

goals.

The fact that many scientific norms and conventions can be given an economic explanation also suggests that they are likely to be general rather than wholly local in their application. Science is a social process, and it is no accident that some scientific institutions resemble those found elsewhere in society. This outcome is contrary to the new methodological view that scientific rules are invariably local in their application. The reason that some scientific institutions sound so familiar, even trite, when cast in everyday terms (don't lie, don't cheat, don't steal), is that they are attempting to solve social coordination and cooperation problems that are quite common, and that have therefore given rise to common (attempts at) solutions.

The scientific institutions I have discussed are likely to evolve in any knowledge-production setting, where agents are Smithian (or at least something short of the heroic model of the received view), and where are there market failures which derive from the special nature of knowledge production. Hence, one upshot of an economic view (as against a sociological, or rhetorical view) of science is that some scientific rules can indeed travel across communities, with general application.

An economic view of scientific institutions also is at odds with McCloskey's claim that the rules of good science are tacit — so varied, complex, and changing that they cannot be written down. "The reasonable rhetorician," says McCloskey, "cannot write down his rules." (1985a: 52).[148] I do not disagree in principle, but I think I have identified a number of scientific institutions that are not especially tacit. This does not, of course, rule out the existence of other, tacit rules in science — which no doubt exist — but it does suggest that some important rules are explicit and can be communicated.

Others have also recorded what they take to be important institutions in science; see Rescher 1990 and even McCloskey 1985a. It seems clear that scientific institutions exist, that they are not

[148] This might be called the Potter Stewart view of institutions; Stewart was the Supreme Court Justice who said of obscenity, (something like) "I can't define it, but I know it when I see it."

always tacit, that they can apply across communities (and over time), and that they serve important goals in science. An economic view of science allows us to see that some institutions are not just inefficient regulation, but are indispensable resources in the intellectual economy.

In Chapter Seven, we turn to an empirical investigation of rules in the science of economics. Rules are an important but elusive quarry. They exist, and they are very influential, but they are hard to observe directly. And, any catalogue will probably be incomplete. The hope, however, is that by identifying and elaborating on some of the working institutions in economics, we can provide concrete examples of the types of institutions discussed in this chapter. As a case study, I make use of a recent dispute in contemporary economic thought — the minimum wage controversy.

Appendix 6A Why logical consistency requires an economics of science

Start with McCloskey's injunction: "Economics should apply to itself." (1994, 71). What does this mean? In a broad sense it means that theorists of human action, economists and others alike, cannot logically exempt themselves from their theories. To the extent that theorists actually subscribe to their theories, they are in them. Karl Marx, for example, argued that all mankind suffers irretrievably from a "false consciousness" induced by the structure of economic relationships in society. But Marx is part of mankind, how did he manage to see through his false consciousness to locate this hidden truth about the world? Apparently we are to see Marx alone as exempt from the implications of his own theory.

When social scientists theorize about human behavior, they are necessarily theorizing about themselves. Hence, we should reserve special scrutiny for any position that argues: my theory of human action applies to everyone, except to me (and my fellow scientists). Consider the following illustration of the point:

(1) (economists say that) human beings are utility maximizers;
(2) economists are human beings;
therefore, (3) economists are utility maximizers.

Or, to take another example:

(1) (sociologists say that) human beings conform to social norms;
(2) sociologists are human beings;
therefore, (3) sociologists conform to social norms.

Logic dictates that an economist who believes that human beings are utility maximizers, must therefore believe, upon pain of inconsistency, that she herself is a utility maximizer. Similarly, the sociologist who believes that human actors conform to social norms, cannot claim to be a non-conformist, for this would undermine her major premise.[149]

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[149] I don't wish to claim that these major premises realistically portray what all economists or sociologists believe to be true about human behavior. I claim only that, whatever they believe, they

A related example from McCloskey can also be cast in this syllogistic fashion: (1) (economists say that) people cannot profitably predict the future: (2) economists are people; hence (3) economists cannot profitably predict the future. In other words, an economist who believes that $500 bills do not lie unclaimed on sidewalks cannot consistently offer advice to clients that purports to locate unclaimed $500 bills. (McCloskey 1990: 111-134). This doesn't mean that economists don't sell forecasting services. They do. It means only that economists who purport to do what their theory says they cannot are inconsistent — either fools (unknowingly inconsistent) or knaves (knowingly inconsistent).[150]

There are, of course, arguments which attempt to get around the logical requirement that theories of human action apply to theorists too. For convenience, let us identify three classes of such argument: (1) limiting the domain of theories (2) exempting scientists on grounds of greater moral probity; (3) denying that scientists must believe their theories in the first place. I will raise each of these arguments, and argue that they each fail to circumvent the requirement of logical consistency.

6A.1 Limiting theories' domain

The likelihood of self-reference varies directly with theoretical ambition — the larger the domain that the theory attempts to explain, the more likely it is that the theory will apply to the theorist herself. Say, for example, my theory proposes that "all human beings always and everywhere successfully maximize utility, where 'utility' is a function that represents a single, complete and transitive ordering of preferences over states of the world." Clearly, this theory will be more likely to apply to me than a version which restricts its domain by substituting theoretical qualifiers like "some

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must logically believe it about themselves as well.

[150] Incidentally, the fact that some economic consultants get rich selling advice says something about the buyers of such advice, but it does not imply that economic consultants can profitably predict the future — they got rich by selling advice, not by using it. (McCloskey 1990: 118-19).

human beings," or "at some times" or "in market settings only." If I say that agents maximize utility in market settings only, then it is at least possible that my theory does not apply to my theorizing.

The range of economics may be (and traditionally has been) construed narrowly. Economics has traditionally applied to market settings (production, allocation and distribution of material resources) but not to political, religious or family realms, for example. A narrower theoretical ambit is, indeed, less likely to apply to the theorist. Only if economic theory is narrowly construed can Robert Lucas, for example, consistently claim that "I . . . don't use economic principles at home." (Klamer 1983: 48).

If, on the other hand, economics is defined broadly — as the science of choice under scarcity, for example — then its theoretical scope is very large indeed.[151] Economists such as Gary Becker who endorse this broad mandate and work outside the traditional boundaries of economics — theorizing about suicide, marriage, childbearing, intra-family transfers, and so on — are virtually certain to find themselves in the model. One can consistently hold altogether different views of economists and economic agents only if one restricts the theory's domain. Lucas, for example, would want to claim something like "my theories apply in market settings only; they are not meant to describe choices made within families." This requires believing that human behavior in markets is different from human behavior elsewhere.

Carefully delimiting the domain of economics (or of any other social science) can, in principle, avoid theoretical self reference. Clearly, however, the trend in economics is away from theoretical modesty. "Imperialism" in economics, the quest for a unified social science, makes theoretical self reference more rather than less likely. The more ambitious economics becomes, the more glaring is the

[151] This is Lionel Robbins's definition, which we encountered in Section 1.3: "the science which studies the relationship between ends and scarce means which have alternative uses." Since virtually all resources are scarce, economics so defined will apply to any and all aspects of human action, not just those which happen to occur in market settings, for example.

contradiction between our conception of ordinary agents and our conception of scientists. Theoretical ambition works to undermine a motivational distinction between economists and economic agents.

6A.2 Exempting theorists on grounds of superior moral probity

A second attempt to circumvent the logic of an economics of science, takes a page from the received view, and argues that science is categorically different, because scientists are more ethical than other people. This is the view that scientists are special, somehow able to get outside their own biases, preferences, and idiosyncracies in ways that other mortals cannot. Hence social-science theories apply to everybody, except the social scientist.

As David Hull points out, this self-excluding gambit reminds one of the U.S. Congress that passed civil rights legislation requiring all employers to institute fair-hiring practices, with the sole exception of Congress itself. (1988: 3). The argument from specialness distinguishes theorizing from other human activities by claiming that scientists are disinterested truth seekers. One unlikely proponent of this view is the economist James Buchanan. Buchanan, writing with Geoffrey Brennan, argues:

> [We] make a distinction between two types of social interaction: between science and politics of the ordinary sort. . . . Science is a social activity pursued by persons who acknowledge the existence of a nonindividualistic, mutually agreed-on value, namely *truth*, and who, furthermore, accept this value as the common goal of all participants in the enterprise. Science, therefore, cannot be modeled in the . . . [public choice] paradigm. Science is categorically different from the relationship that is the domain of economics. (1985: 38).

I say "unlikely" because Buchanan is a distinguished founder of Public Choice economics, a discipline organized around the conviction that economic ideas should be applied more outside their traditional boundaries. (For an early example, see Buchanan and Tullock 1962).

Public choice theorists apply economic ideas to the political realm. They reason: (1) human beings are rational and self-interested; (2) political actors like politicians, bureaucrats and voters are human beings; hence (3) political actors are rational and self-interested. Politicians try to maximize votes; bureaucrats try to maximize budgets; and voters vote their pocketbooks, to the extent they find voting rational at all, and so on. The logic of Public Choice says that the traditional view of political actors as beneficent public servants fails because it is inconsistent with more basic premises about human nature — that human beings are rational and self-interested. The error of traditional political theory, argues the Public Choice view, was to treat political actors as if they were different from everyone else.[152]

But this is precisely the error that Buchanan himself commits when he and Brennan imply that scientists are different from everybody else, that they seek truth under all circumstances, including when it is not in their self-interest. The advocate of a clear-eyed view of political motivation suddenly loses faith in his economic reasoning when asked to apply it to science. Methodological individualists, Buchanan and Brennan even goes so far as to argue that individual scientists behave as if they were part of a truth collective, with "non-individualistic" goals. But Buchanan and Brennan offer no evidence that scientists behave as members of a truth collective. Buchanan and Brennan abandon economic reasoning at the point where it threatens their view of science and of scientific motivation.

I want to be clear that one can legitimately claim that science works differently from politics. But I think it is incorrect to locate the source of difference in the make up of scientists, rather than in the different incentive structures of science and politics. It is **logically** possible that scientists are categorically different from other people, but the burden of proof surely rests on those who would maintain such strong distinctions *a priori*. Of course an economist **can** seek truth, since cognitive and non-cognitive goals coexist. But, *ceteris paribus*, she is no more likely to do so than is the politician to serve the public interest.

Gordon Tullock agrees. He argues that "[T]here is no reason to believe that scientists are

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[152] Buchanan refers to the traditional view of political motivation as the "myth of benevolence." (Brennan and Buchanan 1985: 33-45).

much more truthful and honest than other men." (1966: 130). Even George Stigler, who, unlike Tullock, was reluctant to think of economists in economic terms, said: "we ought to occasionally remind ourselves that there is no presumption that intellectuals possess less or more courage and integrity than the rest of the population." (1982: 61). It is more plausible, I suggest, to argue that science and politics differ not because scientists are inherently more honest, but because science and politics have different institutional structures, i.e. other things are **not** equal. David Hull makes this important point as follows:

> Whatever is true of people in general had better apply to scientists as well. Scientists are people. One cannot claim simultaneously that people in general cannot sustain widespread, altruistic behaviors and that scientists can, without offering some explanation for this peculiar state of affairs. . . . Because no one has suggested that any unique features of science as a social institution are due to the peculiar genetic makeup of scientists, the only plausible answer must lie in the peculiar structure of science as a social institution. (1988: 304).

The point is that if one believes that science is superior to politics in achieving its goals, this does not require a received-view claim that scientists are selfless truth seekers. A more compelling and more economic interpretation, I suggest, is that scientific institutions are relatively more effective in channeling self-interested behavior towards desirable collective outcomes. The invisible hand works better in science because it operates in a superior institutional framework, not because scientists are inherently more honest.

An economic view of human motivation **does** subvert the naive belief that scientists are (only) disinterested truth-seekers, somehow immune to ordinary human desires. That is why Buchanan's discomfort is understandable. But an economic view of scientific motivation does **not** require that science cannot be successful, as Buchanan implicitly seems to fear, and as critics of science (such as elements in the SSK) often conclude. On the contrary, the motive force of self-interest can be as potent in science as it is in markets, provided there exists an institutional structure sufficient to produce invisible-hand outcomes.

Acknowledging that scientists are self-interested and have worldly ambitions in no way undermines the possibility of scientific success. Given the right institutional framework, self-interested actors can produce socially beneficial results, in science no less than in markets. The point is two-sided: *pace* Buchanan, special dispensation for scientists is not required for science to succeed; and, *pace* the new methodologists, denying special dispensation does not ensure that science cannot succeed.

Let us now turn to the final argument against the logical requirement that economics should apply to itself.

6A.3 Theories are only instruments: scientists don't believe their own theories

In addition to (1) limiting theories' domain and (2) exempting scientists on grounds of superior moral probity, there is another argument for treating scientists and agents differently. It is suggested by the following example:

> (1) (behavioral psychologists say that) human beings are stimulus-response machines;
> (2) behavioral psychologists are human beings;
> therefore, (3) behavioral psychologists are stimulus-response machines.

What makes this conclusion false is not the logic, which is valid, but the major premise, which is false. Human beings may be rational maximizers, or they may be social conformists, but they are not machines. Confronted with an absurdity, the theorist must take a different tack. She can claim, for example, that theories are not to be taken literally. That is, we should not consider theories as true or false accounts of reality (scientific realism), but rather as instruments for accomplishing scientific goals, such as prediction. The scientist uses a theory like a mechanic uses a wrench, and either the wrench works or it doesn't.

Milton Friedman's (1953) variation of this position, known as instrumentalism, said that economic theories are instruments, to be judged by the accuracy of their predictions alone. Theories, Friedman argued, are only useful fictions. It is clearly false, he concedes, to assume that billiards players continuously solve sets of differential equations while making shots, but, for the theorist, it may be useful to pretend that they do. Thus, when the economist says that firms maximize profits by producing where marginal revenue equals marginal cost, she really means it is **as if** firms maximize returns by producing where marginal revenue equals marginal cost. By inserting the "as if" construction, Friedman makes a theoretical expression into a figure of speech (a simile), indicating that it is not to be construed literally.

The instrumentalist's theoretical maneuver is more radical than merely restricting the theoretical domain or exempting scientists on grounds of superior moral probity. The instrumentalist says that theories don't even attempt true descriptions of the world; they are merely tools for prediction (or other goals). Scientists don't have to believe that their theories are correct in a meaningful sense, they simply see them as useful, the way a wrench is useful. Friedman even goes so far as to claim that "unrealistic" (read: false) assumptions are a theoretical virtue: "the more significant the theory the more unrealistic the assumptions . . . ." (1953: 14). Hence, the theorist need not actually believe her theory, and the instrumentalist gambit seems to, thereby, circumvent the problem of theoretical self-reference.[153]

Though this is not the place for an involved discussion of instrumentalism, I will argue that theorists are not indifferent to the nature of their beliefs, which will often require more than prediction can provide.[154] Scientists sometimes act as if they were instrumentalists in Friedman's sense,

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[153] Friedman (1953), recall from Section 1.4, was defending marginalist theory of the firm from charges that they were "unrealistic," that is to say, false. (The charge was leveled by Richard Lester (1946), who had surveyed businessmen on how they make hiring decisions, and found that they made little consideration of wages or other non-labor costs, i.e., did not think much about marginal costs.) Friedman's defense conceded that businessmen don't consciously optimize, then boldly asserted that this didn't matter. Theories are to be judged not by the truth or falsity of their assumptions, but rather by success of their predictions. The assumption that firms knowingly set marginal revenue equal to marginal cost is false, Friedman agrees, but the prediction that only profit-maximizing firms will survive in the long run is accurate.

[154] Because instrumentalists don't care whether theories are true or false, they are in opposition to scientific realists, who argue that science aims to provide theories which are true or false in virtue of

indifferent to whether or not their theories are correct. This will occur especially in fields where the phenomena are more or less unobservable, such as quantum mechanics, and or where causal processes are hard to ascertain.

But where possible, I submit that scientists also want to know **how** things work. They inquire not merely to predict phenomena X, but to find out what causes X. When they have the opportunity, scientists will also want knowledge of causality, not just of correlation. If scientists want to understand the way in which things work, and the phenomena "permit" such an understanding, scientists will not be satisfied with prediction alone.

This is the position of the philosopher of science Stephen Toulmin (1961), who argues that understanding, not prediction, is the more fundamental goal of science. For Toulmin, prediction is only "an application of science rather than the kernel of science itself." (cited in Redman 1991: 79). What matters, argues Toulmin, is "understanding — a desire to make the course of Nature not just predictable, but intelligible . . . ." (ibid). On Toulmin's account, prediction is not a goal for its own sake (never mind **the** goal), as the instrumentalist argues, but a means to a more fundamental goal, understanding.

If scientists care about understanding, then prediction is not the sole end of science, and scientists cannot be seen as wholly agnostic towards their own beliefs. Few economists believe that agents actually formulate and solve constrained optimization problems. But I submit that there are more basic economic ideas that many (perhaps most) economists take to be true, not merely useful. Among these fundamental economics ideas are: rationality (in the sense of purposeful evaluation of costs and benefits), scarcity, and self-interest, at least in Smith's sense. Without claiming that this list

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how the world actually is. A scientific realist accepts a theory when she believes it to be true, not merely because it predicts well. (See Van Fraassen 1980, which is anti-realist but not strictly instrumentalist. For a discussion of Van Fraassen's "constructive empiricism" in the context of economic methodology, see Boylan and O'Gorman 1994).

canonical or exhaustive, I argue that it is theoretical beliefs of this sort that distinguish economists from other theorists of human action, such as sociologists or psychologists. And it is these basic ideas that economists take to be true.

Thus, while it is logically possible for economists not to believe aspects of their theories, I argue, as a positive matter, that economists are not indifferent to the truth or falsity of their most fundamental ideas. Hence, to the extent that economists actually believe their theories of human action — that is, to the extent they take them to be true — those theories are unavoidably self-referential.

6A.4. Theories are only instruments, part two: consequences for theory testing and modification

In this section, I don't take issue with Friedman's argument that scientists are indifferent to the truth or falsity of their theories. In fact, I will try to show that the truth or falsity of theoretical assumptions matters for theory testing, even in an world where scientists don't believe their own theories.

As an illustration, consider a theory of output which asks us to assume rigid nominal wages, and which predicts that inflation is pro-cyclical. Under one interpretation, we might presume that pro-cyclical inflation occurs **only** in those instances when nominal wages are in fact rigid. When nominal wages are not rigid, i.e. when the assumption is false, the prediction isn't meant to hold. Call this kind of assumption a domain assumption.[155] The theory applies only in the domain specified by its assumptions; it is not intended to apply more generally.

Friedman the instrumentalist is less cautious. He argues that false assumptions need not limit the theory's domain, provided they support a theory that predicts well. Galileo's law of falling bodies (falsely) assumes a vacuum, yet predicts rather well (if the objects involved are not much affected by air resistance — cannonballs not feathers — and don't encounter any gravitational fluctuations).

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[155] These terms for different types of assumptions, and much of the argument are due to Alan Musgrave's (1981) critique of Friedman's paper.

Hence, though the macroeconomist will be making an assumption that is known to be false — nominal wages **do** vary, after all — but Friedman's implication is that this falsehood will have negligible effects upon the prediction of pro-cyclical inflation. Call this, again following Musgrave, a negligibility assumption.

The difference between domain and negligibility assumptions is crucial for testing theories (among other things), a distinction that Friedman misses when he argues that "the more significant the theory the more unrealistic the assumptions . . . ." (1953: 14). In the case of domain assumptions, the **opposite** is true. The more unrealistic are one's domain assumptions, the more likely it is that one's theory will be insignificant. If nominal wages are never or rarely rigid, then the theory can never (or rarely) be tested and will not be very significant. (Musgrave 1981: 381-82). This is hardly a methodological virtue, particularly for Friedman, whose predictionist program cannot go forward without testability.

One might argue, in response, that Friedman never meant for "assumptions" to be understood in the domain sense. But this is not the case. (ibid). Friedman says, in fact: "the 'assumptions of a theory' . . . are sometimes a convenient way of specifying the conditions under which the theory is expected to be valid," which clearly conceives of assumptions in a domain sense. (1953: 19, cited in Musgrave 1981: 382).

Nor is it the case that economists, in practice, don't make use of domain assumptions. In fact, domain assumptions are routine in economics. Our theoretical conclusions are often of the if-then form that domain assumptions entail. **If** nominal wages are rigid, **then** inflation will be pro-cyclical. In fact, assumptions are often transformed from negligibility to domain when the theory fails to predict under the more demanding negligibility construct. If it turns out that nominal wages **do** influence the pro-cyclicality of inflation (i.e., if nominal wages are not negligible), then the economist may opt to revise the theory such that it predicts pro-cyclical inflation **only** when nominal wages are rigid.

Furthermore, given that theories are often altered in this sense, it is clear that the truth or falsity of assumptions does have consequence for theories. Theory modification depends on the possibility of assessing which theoretical component has gone wrong, assumptions included. If we proceed by assuming that assumptions can never go wrong (i.e., as if the truth or falsity of assumptions is irrelevant), then we may fail to diagnose the theoretical problem. This is, as Daniel Hausman (1992) suggests, like trying to fix a car without opening the hood.

To sum up: I've introduced three objections to the idea that economics should apply to itself, and found them all wanting. Restricting the domain of theories could, in principle, work, but the trend in economics is clearly running towards a larger and larger theoretical domain. Exempting social scientists on grounds of superior moral probity is a non-starter, until one can make the difficult case that social scientists are inherently more ethical than those who inhabit our theories. The production of reliable scientific knowledge does not require selfless truth seekers, any more than the wealth of nations requires selfless seekers of the common good.

Finally, the instrumentalist claim that economists need not believe their own theories is undermined by a plausible counter-argument, which says that economists want to understand, not merely to predict. In addition, Friedman's confusion on the nature of scientific assumptions works to subvert his own program of theory testability, and to seems to rule out useful kinds of theory modification. This concludes the case that economists should support an economics of science.

Chapter 7. The minimum wage controversy: rules in economics

In the two previous chapters, I have attempted to show, theoretically, that science can be seen as a market-like process, a process is characterized by a variety of methodological rules, and that these different rules, depending upon their function and efficacy, can work to promote collectively beneficial (invisible hand) scientific outcomes. In this chapter, the focus changes from science in general to the science of economics in particular, and from theoretical arguments to empirical support. This chapter presents an investigation of an important episode in contemporary economic thought — the minimum-wage controversy. My aim is to use a case study of the minimum-wage controversy to illustrate the theoretical arguments I have made in Chapters Five and Six, while providing some empirical evidence of actual methodological rules in economics.

The minimum-wage controversy begins with the advent of a relatively recent literature in empirical labor economics, which suggests that increases in minimum wages **do not**, contrary to conventional price theory, lead to adverse employment outcomes for low skill workers. If anything, this research suggests that the elasticity of employment with respect the minimum wage is **positive**. Much of the literature is collected in or summarized by David Card and Alan Krueger (hereafter CK) in their influential 1995 book, *Myth and Measurement: The New Economics of the Minimum Wage* (hereafter *Myth*). *Myth*, a book that Ronald Ehrenberg (1995) called "the most important labor monograph of the 1990s," provides a convenient touchstone for the controversy.

Unlike most economic policy debates, the minimum wage controversy has had an impact far beyond the usual academic confines. Its influence was fueled by contemporaneous political developments. A prominent political debate on pending minimum wage legislation (since signed into law) to raise the Federal minimum wage from \$4.25 to \$5.15 per hour coincided with *Myth*, as did a strong and mostly negative response from the profession itself, a response that carried over into Op-Ed pages, opinion magazines, and other popular forums. The literature I will draw on for the case consists of *Myth*, and the varied responses to what CK call the "new economics of the minimum wage," especially academic responses (Neumark and Wascher 1995, 1996; the *Industrial and Labor Relations Review* Symposium 1995; Kennan 1995), but also those written by economists for a lay audience.

I am presenting this case not as a labor economist doing labor economics, but as an economist who is interested in the question of whether, by examining a notable episode of contemporary economic thought, one can identify some working methodological rules in economics — rules of theory, of empirical work and of policy advising. I do not attempt, in one chapter, to write a "thick history" of the economics of minimum wages, but, rather, to offer a preliminary characterization. Likewise I do not attempt a full-blown rhetorical analysis of the kind that would reveal and dissect the means by which protagonists persuade to their audiences. What this case study attempts is an answer to the following question: what methodological rules, if any, are revealed in the minimum wage controversy?

I begin with a proposition, which goes as follows:

> Proposition 1. *The intellectual and political capital expended on the minimum wage debate, inside and outside the academy, is disproportionately large relative to its economic importance.*

On the macro level, the predicted economic effects of even a \$0.90 increase (21 percent) in the minimum wage are rather unimpressive. In the first place, only 9 or 10 percent of the employed labor force is affected at all, and these workers are concentrated in few industries, especially retail trade.[156]

[156] This assumes 90 percent coverage of about 12 million otherwise affected workers, and an employed labor force of about 110 million, a figure which omits the self-employed. "Affected"

CK estimate, for example, that $5.5 billion was transferred to low-wage workers by the $0.90 Federal minimum-wage increase of 1990 and 1991, which is only 0.2 percent of the total U.S. annual earnings. (1995: 277). The likely effects on output prices are also relatively minor. CK estimate that if firms had somehow passed along **all** of the proposed 1990-91 labor cost increases in the form of higher prices to consumers, prices would have gone up (once-and-for-all) only about 0.3 percent. (ibid: 393). Employment effects, whether negative or positive, are also relatively small; most estimates of employment elasticities are fairly close to zero.

This is not to minimize these very real economic effects, nor to rationalize the policy which creates them. Rather, I am arguing that the economic consequences of minimum wage legislation are far less significant to the economy than are those of other economic policies under debate, such as entitlement reform, health care reform, or the Consumer Price Index calculation debate. Both opponents and proponents of minimum wages tend to overrate their effects, says Charles Brown. His 1988 survey argues that opponents of minimum wages tend to overestimate job losses — employment effects are small — and that proponents tend to overestimate distributional gains — "even under the most favorable assumption that the minimum wage had *no* disemployment effects, its effect on poverty or the income distribution is low."[157] (1988: 143). Neither are very significant. This raises the question, what's all the fuss about?

I will argue that while the sound and fury generated by the minimum wage controversy is indeed excessive relative to its importance to the economy, it is by no means inexplicable. The reason

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workers are those whose current wage is below the new proposed minimum wage.

[157] Brown interpreted the evidence as follows; the employment elasticity of minimum wages for **teenage** workers is on the order of -0.1 or -0.3, and -0.1 to -0.2 when coverage and trend issues are included in the analysis. They are lower for older workers (1988: 139). Distribution effects are minimal because there is a surprisingly weak relationship between being a low-wage worker and living in a low-income household. (1988: 143).

is that, for economists and policy makers, there is more at stake than the consequences of minimum wages for employment, income distribution, other wages, output, prices, and firm values. The minimum wage controversy engages economists and policy makers because it brings to light fundamental questions about economics as a science: such as, (1) how should economists do theory, (2) how should economists do empirical work, and (3) how should economists convey their knowledge to policy makers, and to other consumers of their output. The minimum wage controversy has created intellectual fireworks precisely because it touches on these fundamental methodological issues.

Most conspicuously, "the new economics of the minimum wage" appears to challenge what is perhaps the most deeply held scientific belief among economists — the Law of Demand. But the controversy also touches on a host of other issues that run deep in economics: (2) should economics proceed inductively or deductively; (3) can data be used to "test" neoclassical price theory; (4) how should economists proceed when theory and evidence conflict; (5) how do economists and policy makers know when a consensus has been achieved within economics; (6) how should the goals of efficiency and equity be traded off; (7) is economic knowledge secure enough to legitimately guide policy makers, especially when intervening in functioning markets; (8) given the political economy, do economists have a special obligation to promote efficiency or to ensure that "policy entrepreneurs" and special interests don't abuse academic research (seller beware)?

It is because the minimum wage controversy provokes these fundamental questions that it makes a fruitful case study for the economist stalking methodological rules. The right kind of controversy induces responses to these fundamental questions, going beyond the everyday, technical concerns of the labor specialist, which pertain to coverage issues, possible heterogeneity among affected workers, and the influence of teenage school enrollments. Especially heated controversy induces participants to speak their minds in a way that ordinary academic discourse typically discourages, thereby uncovering beliefs about methodological rules that are not ordinarily on display.

I hope to exploit this unusual conjunction of events in order to locate some evidence of what the rules in economics are.

In order to introduce these fascinating issues, a brief introduction to the economics of mandated minimum wages, and to its history, is presented next.

7.1 A brief introduction to economics of minimum wages and its history

The issues that pertain to mandated minimum wages are, of course, much older than the contemporary debate, and they also antedate the passage of the Fair Labor Standards Act in 1938, which introduced the first **federally** mandated minimum wage in the United States. The progressive era was very much concerned with improving labor market conditions in the first two decades of the century, and progressives saw wage regulation as of piece with other labor market reforms, which sought to regulate child labor, quantity of hours worked, and working conditions. Minimum wages were seen a means of guaranteeing a "living wage," that is, an income sufficient for meeting the essentials of life. Economists were frequently among the reformers, especially Wisconsin-school progressives like Richard Ely, who wrote the forward to John Ryan's 1906 book, *A Living Wage*.

State legislation to regulate labor markets was, for many years, struck down by the U.S. Supreme Court as an unconstitutional violation of the 14th Amendment, which guarantees the right to free contract. Prominent among several Supreme Court decisions affecting labor legislation was the 1905 decision of *Lochner v. New York* (198 U.S. 45, 25 S.Ct. 539), which ushered in a period of Court opposition to labor-market regulation and other New Deal-type reforms, known as the Lochner era.[158] Routine Supreme Court opposition is, in fact, what lead Franklin Roosevelt to his infamous attempt to "pack" the Supreme Court with more sympathetic justices. In a radio address of March 9,

[158] *Lochner* concerned a New York law that prohibited the employment of bakery workers for more than 10 hours per day or 60 hours per week. Justice Peckham delivered the opinion of the court, saying: "There is no reasonable ground for interfering with the liberty of a person or the right of free contract by determining the hours of labor. . . . (Gunther 1980: 512).

1937, Roosevelt said "one-third of the nation [is] ill-nourished, ill-clad and ill-housed" and that therefore "we must take action to save the Constitution from the Court, and the Court from itself." (Gunther 1980: 151). Roosevelt's scheme failed, but the composition of the Court changed of its own accord, and reforming legislation gradually received more hospitable treatment from the Court. The minimum wage provisions of The Fair Labor Standards Act of 1938, which imposed a minimum wage of $.25 (about $2.50 in 1995 dollars), passed constitutional muster in 1941, in *United States v. Darby* (312 U.S. 100, 61 S. Ct. 451). (See Gunther 1980: 162-67).[159]

Over time, new Congressional legislation has increased the minimum wage and expanded the number of industries and workers covered by (that is, not exempt from) the law. The federal minimum wage has never been indexed to prices, and therefore, the minimum wage in real terms has fluctuated with the vagaries of inflation. Plotted against time, real minimum wages increase in discrete upward jumps that correspond with statutory increases in the nominal minimum, then gradually decrease with the decline in purchasing power. In real terms ($1995), the minimum wage peaked (at about $7.00) in 1968, declining to roughly $4.00 prior to the 1990 federal increase. (See Figure 2 in Kennan 1995: 1955, and also Card and Krueger 1995: 6).

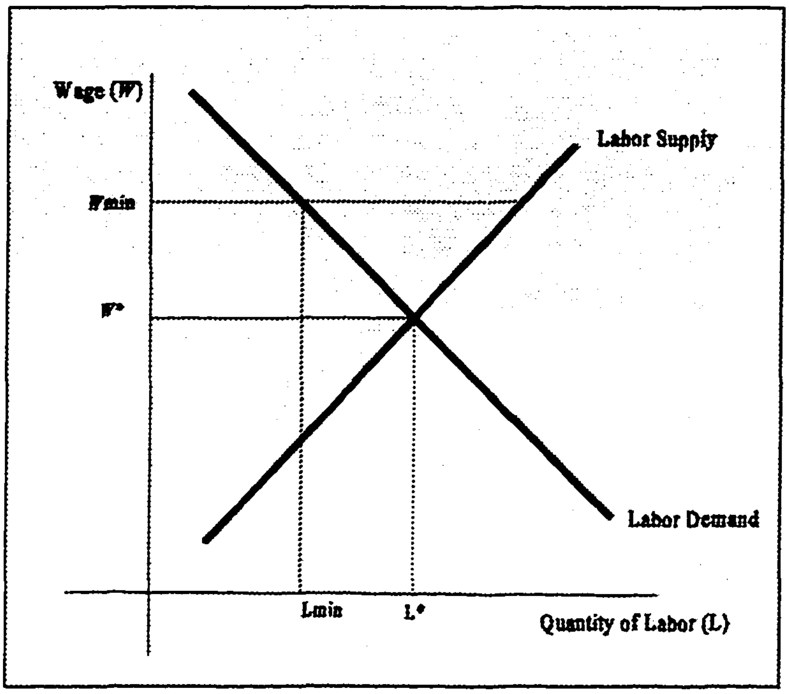
7.1.1 The textbook theory of minimum wages

Economists treat mandated minimum wages as they would any other kind of price regulation. Most often we use a simple, Marshallian model of the kind that will be familiar to any introductory student. If the mandated wage (W_{mn}) — a price floor — exceeds the equilibrium market wage (W^*), then, given a downward-sloping demand curve, we should expect firms to reduce the quantity of labor demanded, from L^* to L_{mn}. (For now we set aside the question of what the correct quantity (L) is, labor hours or employment). The magnitude of the reduction in labor demanded ($L^* - L_{mn}$) will depend

[159] The foregoing three paragraphs owe much to the discussion in Burkhauser, et al., 1995: 2-7).

upon the wage increase and the wage elasticities of demand and supply, but the direction of the change is theoretically unambiguous. The model is depicted below in Figure 7.1.

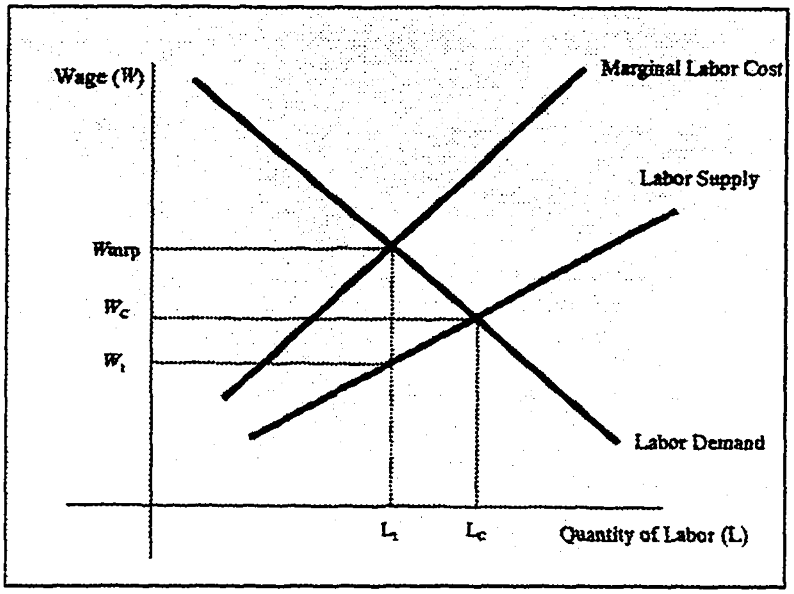
Figure 7.1. The competitive "textbook" model of a labor market with a mandated minimum wage



This story, along with its price ceiling analogue of rent control, is practically obligatory in introductory textbook discussions of neoclassical price theory. Charles Brown quips: "An introductory textbook without a discussion of the minimum wage may not be like a day without sunshine, but it certainty would rank with a morning without caffeine." (1988: 134). Importantly, however, and unlike other classic devices in introductory texts, economists themselves conceive of minimum wage effects in this very simple but powerful fashion. As Brown says, "with relatively few refinements," this model "really does summarize the heart of the matter." (ibid).

The textbook model assumes that output and input markets are competitive. Firms are assumed to be price and wage takers; firms can hire as many workers as they like at the prevailing wage (but have no power to set wages), and output choices are efficient. But what if labor markets are not competitive? If labor markets are not competitive, a situation of monopsony, then firms may exercise their market power over workers and pay a wage that is less than the workers' marginal (revenue) product. This is often portrayed as a company-town scenario, where firms must offer higher wages to attract new hires, which leads to a higher wage bill and marginal (labor) cost curve that is higher than labor supply. The monopsony situation is depicted in Figure 7.2 below.

Figure 7.2. The monopsony model of a labor market



The profit maximizing monopsonist chooses quantity L_1, which results in a wage of W_1 that is less than the marginal (revenue) product W_{mrp}. A judiciously chosen minimum wage (i.e. set $W_{min} = W_C$) can, in this scenario, actually increase employment to the competitive level (i.e., from L_1 to L_C), overcoming the monopsonist's socially inefficient choice. Hence, in the monopsony setting, a (well chosen) minimum wage can lead to employment **increases**. Until the "new economics of the minimum wage," however, labor economists have regarded the company-town story as a theoretical curiosum (Brown 1988: 134, ff 1; CK: 373), perhaps because there is little empirical evidence that a single firm hires anything but a small percentage of workers in low-wage markets. (Brown et al. 1982: 489).

The *locus classicus* in the minimum wage literature is Stigler 1946.[160] Stigler argued from economics and from policy. The economic argument had two parts. First, he said that a (binding) minimum wage would reduce employment in covered sectors (unless the labor market were monopsonistic), and that this welfare loss could exceed the benefits of higher wages to those who kept their jobs (when the employment elasticity is greater than 1.0). Second, Stigler argued that disemployed workers from the covered sector would seek work in the uncovered sector, and that this increase in labor supply would reduce wages in the uncovered sector, another welfare loss to be weighed against higher wages in the covered sector. With universal coverage, the disemployed workers would become unemployed or they would leave the labor force altogether.

Stigler's policy argument said that minimum-wage policy should be evaluated with respect to its goal, the reduction of poverty. This suggests two questions: (1) does a minimum wage reduce poverty and (2) are their superior alternatives? Stigler's answers are, famously, no and yes. (See Burkhauser et al. 1995). The minimum wage will not reduce poverty, he argued, if there is a weak relationship between minimum wages and family income. This will occur when minimum wage earners are members of non-poor households, as with some teenagers. The important point here is that, regardless of whether there is disemployment sufficient to offset higher wage gains, minimum-wage policy may fail to redistribute income effectively, i.e, may fail to help the (working) poor.

A more effective policy, Stigler argues, would be a negative income tax for the poor, a policy that today is carried out in the form of the Earned Income Tax Credit (EITC). Rather than subsidize the wages of the potentially non-poor, and risk creating job losses for the least productive workers, a

[160] Stigler was a life long opponent of minimum wages, who made one's position on the minimum wage into a kind of acid test for disciplinary membership (a rhetorical device that, we will see, lives on today). He said, in 1976, that "one evidence of professional integrity of the economist is the fact that it is *not* possible to enlist good economists to defend . . . minimum wage laws." (Stigler 1982: 60).

better policy simply transfers, via tax credits, payment to the workers who are known to be poor. All government interventions, Stigler argues, create inefficiencies, but direct subsidies of the poor are more efficient than minimum wages, which, by regulating wages, are doubly distorting.[161]

Stigler's emphasis on disemployment effects, and on whether minimum-wage earners are poor, remain, 50 years later, the touchstones of the contemporary debate. There are, of course, a host of other issues which pertain to minimum wage policy, but, for economists, these two issues remain central to the contemporary controversy. CK make it clear, for example, that they regard their findings of a (small) **positive** employment effect to be strongest and most important of the many different results they present under the banner of the new economics of the minimum wage. Employment effects and the poverty status of affected workers will also be our focus.

Legislators (and the general public) have tended to be skeptical about the disemployment effects predicted by economists, and ignorant of or indifferent to the question of whether minimum wages effectively target the working poor. Legislative skepticism may well be self-interested — minimum wage laws are very popular politically. But some skepticism among policy makers is always justified when the empirical evidence is less than decisive. Though the textbook theory has unambiguous predictions, it is another matter to measure actual employment changes, especially when exogenous changes in the minimum wage (Δw_{min}) are small relative to endogenous changes in wages, employment, and other features of labor markets. In 1946, Stigler could say "no precise estimate of the effects of a minimum wage upon aggregate employment is possible." (1946: 361).

[161] Even economists who recognize that low-wage workers need not be poor, sometimes lose sight of this possibility. Richard Freeman, for example, who is a very thoughtful commentator, states that "this paper takes as its premise the goal of redistributing earnings to the lower paid in response to the massive increase in earnings inequality of the 1980s-1990s. If you don't want to redistribute, or don't consider redistribution a potential social goal, then stop reading." (Freeman 1996: 640). But the goal is not to redistribute earnings to low-wage earners *per se*, for they may not be poor, but to the poor, regardless of their wage.

Goldfarb's survey (1974) of the empirical minimum wage literature points out that even policy makers inclined to accept disemployment effects in theory, would have been hard pressed to find decisive empirical support in the literature. The empirical evidence for employment effects on **adults** (ages 20 and up) was, at that time, particularly sketchy, Goldfarb argued. There was, in addition, little empirical work on the income distribution of minimum wage beneficiaries, so that economists could not yet empirically address Stigler's policy argument. (ibid).

Since the early seventies there has been a great deal of empirical research, which addresses the lacunae identified by Goldfarb. Mincer 1976, and Gramlich 1976 are influential early examples, and many others are surveyed in Brown et al.1982, for example. By 1980 or so, Goldfarb (1981) could report empirical progress, especially with regard to income distribution effects. By the early eighties, the empirical evidence on minimum wages had a much firmer footing. Let us review this evidence with reference to the following questions: (1) what does the empirical evidence say about predicted disemployment effects and about poverty reduction from minimum wages?; and (2) what do economists believe about these effects?

7.2 What does the evidence (before 1990) show?

In the early eighties, two influential volumes on the minimum wage were produced — the five volume report of the Minimum Wage Study Commission (1981), commissioned by a Democratic U.S. Congress, and the American Enterprise Institute's (AEI) 1981 study entitled *The Economics of Legal Minimum Wages*, (Rottenberg 1981). As Eccles and Freeman (1982) point out in their summary of the two volumes, the "liberal" (MWSC) and "conservative" (AEI) reports reveal a remarkable consensus among labor economists. They argue that "If one did not know which study had been funded by which group, one could not guess from the results" (1982: 227). The influential survey of Brown et al. 1982, which is contemporaneous with the MWSC and AEI volumes, seems to confirm that empirical work on the minimum wage supports the textbook model's prediction of adverse employment effects. Using Brown et al. 1982 as our primary reference, let us summarize the state of the evidence — regarding employment effects and (income) distributional effects — fifteen years ago. Take employment effects first.

7.2.1 Empirical research on employment effects of minimum wages

Brown et al. 1982 survey focuses on employment effects — they survey time-series and cross-sectional results for teenagers (ages 16-19) and for adults (ages 20 and up). A typical piece of work in the empirical literature is a regression with something like the following specification:

$$Y = f(MW, D, X_1 \ldots X_n),$$

where Y is labor force status (e.g., labor force participation ratio, employment-population ratio, unemployment rate), MW is some measure of the minimum wage (often weighted by coverage and average wages), D as a business cycle variable to account for changes in aggregate economic activity, and $X_1 \ldots X_n$ are a variety of exogenous variables that are designed to control for labor supply, school enrollment, participation in the armed forces, etc. (Brown et al. 1982: 497). Some specifications add a time trend as well, particularly when data for the X_i variables are scarce (see Mincer 1976).

Most of the empirical work surveyed by Brown et al. are time series studies of teenage low-wage workers. They survey 25 empirical papers published from 1970 to 1981, most with time series that begin in 1954 (some in 1947-48) and end a year or two before their respective publication dates. There are different functional forms to the specifications, but all 25 studies use data from the Current Population Survey (CPS), and are concerned with teenage workers only. The summary results are presented (sometimes disaggregated by sex and race and age) in Table 3 of Brown et al. 1982.

In all the studies (eighteen), save one, the results find that **employment** elasticity of the minimum wage is **negative** for teens, i.e., consistent with the textbook model's predicted disemployment effect. The range of elasticities runs from -0.052 to -0.30. For specifications which used the unemployment rate (twelve) as the dependent variable, the change (in **percentage points**) was nonnegative, also as predicted, in nine instances, with changes ranging from 0.0 to 3.65 percent. (Note well, however, that statistical significance is available in only two or three instances, because the "all workers" coefficients they summarize were generally calculated from more disaggregate data, which are also reported and which are sometimes significant).

Brown et al. 1982 report that cross sectional studies are far rarer, in part because of the analytical difficulty of disentangling **Federal** minimum wage effects from the state and local differences that cross sectional research tries to exploit. The cross sectional studies are surveyed in Brown et al. 1982, Table 5. Of the four studies which permit measuring the employment elasticity for all teenage workers, three report negative results, as predicted, which range from -0.024 to -.048. The fourth study reports a slightly positive employment effect of .015. (The same caveat regarding statistical significance applies in all four analyses).

There are, curiously, far fewer studies which focus on adult (age 20 and up) employment effects of minimum wages. (Curious, because adults constitute the majority of low-wage workers). Brown et al. 1982 tabulate seven papers which study minimum wage effects on youth aged 20-24, and find that, as with teens, there are consistently negative employment and positive unemployment effects (1982: 512), though with somewhat lower elasticities than with teens (-0.03 to -0.07). (One exception found positive employment effects). There are, with workers aged 20-24, more mixed results when different demographic groups are considered separately. (ibid). Wachter and Kim (1979), for example, found disemployment effects for males and whites females (of which only the latter was statistically significant), but strong positive employment affects for non-white females (elasticity of 0.3, statistically significant).

In their conclusion, Brown et al. summarize their review of the empirical literature as follows: minimum wages reduce teenage employment; employment elasticities are on the order of -0.1 to -0.3, but "the lower half of that range is to be preferred." (1982: 524). For young adults (20-24), the effect of the minimum wage on employment is "negative and smaller" than that for teens, though this conclusion rests on a much smaller literature. For adults more generally, Brown et al. say: "uncertainty about the effects on adults is a serious gap in the literature . . .," echoing Goldfarb 1974.

If we take Brown et al. (1982), and the MWSC and AEI collections as representative, its safe to say the empirical evidence supports the theoretical prediction of a disemployment effect, especially for teenage workers, and that the employment elasticity of the minimum wage is on the order of -0.1 to -0.3 for teens, less for young adults. In 1988, as we saw, Brown could report similar, if slightly lower employment elasticities for teens, -0.1 to -0.2, depending upon the econometric specification.

To get a rough idea of the actual magnitudes involved, say that about 12 million workers are affect by a 20 percent increase, of whom 25 percent are teens.[162] If we assume a teenaged employment elasticity of between -0.1 and -0.3, disemployment for teenaged workers will therefore be 60,000 to 180,000. For the nine million adult workers affected, assume an elasticity of -0.05 to -0.1. Disemployment for adults is therefore 90,000 to 180,000, for a total employment loss of between 150,000 to 360,000, given a 20 percent increase in the minimum wage.

7.2.2. Empirical research on income distribution effects of minimum wages: are low-wage workers poor?

Stigler's (1946) policy argument asked, do minimum wages reduce poverty as efficiently as other policy options? Empirical researchers have attended to this question for a shorter period of time (Goldfarb 1974), due to a lack of data before the mid-seventies. Gramlich 1976 is a useful place to begin. He asks, "are the low-wage workers being helped or hurt by minimum-wage legislation also in low-income families?" (1976: 444). Gramlich's answer is, not really; there is a generally "loose correlation between wages and family incomes [which] implies that minimum wages will never have

[162] Mishel et al. 1995, using the 1993 CPS, estimate that 12,284,000 workers are affected by the increase of the Federal minimum wage from $4.25 to $5.15, of whom 25.6 percent are teens.

strong redistributive effects." (1976: 445).

There are many reasons why low-wages may not correlate with poverty; they include "irregular hours, low-wage secondary workers in high-income families, varying family sizes and numbers of earners per family, [and] varying amounts of unearned income . . . ." (op. cit.) Gramlich merges the 1972 U.S. income distribution and the 1973 U.S. wage distribution and finds that the correlation between low wages and low family income is modestly positive for adults, and practically non-existent for teens. Among adult low-wage (1973 wage less than $2.00 hour) earners, fully 23 percent were below the poverty line, for example. And both male and female low-wage workers had family incomes that were only 47.5 and 51.3 percent, respectively of high-wage (1973 wage more than $4.00 per hour) worker incomes. (1976: 448) Still, fully 25 percent of low-wage adults were above the median income for the economy ($12,620 in 1972), a "spillover" which redistributes regressively.

Among teens the correlation is between low wages and low family income is weaker still, Gramlich found. Low-wage teens, for example, come from families with **higher** median incomes than do high-wage teens (1976: 448), and tend to be dispersed across the income distribution. Gramlich summarizes the distributional dilemma as follows, with a kind of rough and ready calculation:

> For every billion dollars that a boost in the minimum wage brings to low-wage workers, $0.3 billion goes to teenagers, who either do not benefit at all [owing to disemployment effects] . . . or who are so spread out along the income distribution so as to prevent effective redistribution. Of the $0.7 billion received by adults, 25 percent goes to families with income above the median, requiring 25 percent to families below the median just to cancel the distributional impact of this leakage, leaving only half as a net absolute gain to the latter group. Hence this net gain from the minimum wage boost is only $350 million. (1976: 445-49).

Knieser (1981), working with the 1978 CPS, asked "who are low-wage workers?," and, in particular, "are they poor?" (By "poor," Knieser means below the poverty level for a family of five in 1977). The federal minimum in 1978 was $2.65 per hour, scheduled to rise to $2.90 in 1979 and to $3.10 in 1980. Because, in real terms, the federal minimum was relatively high in this era, Kniesner found that 27 and 35 percent of all workers had wages at or below the federal minimums for 1979 and 1980, respectively. Fully two-thirds of low-wage workers were women, less than 25 percent were heads of households, and less than 40 percent were teens (here ages 14-19). (ibid).

Comparing wages with family income, Knieser found that only one quarter to one third of all low-wage workers were living in poverty, conservatively using the 1977 poverty level for a family of five ($7,320). He breaks the sample into workers who report an hourly wage, and those for whom an hourly wage must be imputed, given that they are salaried. The relationship between wages and income, for both groups, is shown Table 7.1, which essentially reproduces Knieser's table 10 (1981: 474).

Table 7.1 Distribution of low-wage workers, by hourly wage and family income, May 1978 (percent)

Family Income	$2.65 or less	$2.90 or less	$3.10 or less	$2.65 or less (imputed)	$2.90 or less (imputed)	$3.10 or less (imputed)
$0-7,499	26.9	26.5	26.0	35.8	34.4	33.1
7,500-14,999	27.9	28.5	28.9	28.7	28.8	29.7
15,000+	41.5	41.4	41.4	32.0	33.1	33.5
Missing data	3.7	3.6	3.7	3.5	3.7	3.7
No. of workers	5,291	7,133	9,235	1,664	2,035	2,425

Source: Knieser 1981: 474

The results show that, among workers with reported hourly wages (78 percent of those in Knieser's sample), nearly three-quarters of low-wage workers affected by the 1979 and 1980 increases are not poor (ignoring the missing data). Among workers for whom an hourly wage must be imputed (22 percent of the sample), about two-thirds are not poor. Hence, concludes Knieser: "although low-wage workers certainly cannot be classified as wealthy, it is also difficult to call them poor . . . when two-thirds to three-fourths of those with low wages have family incomes above the poverty line [for a family of five in 1977]." (1981: 475).

This is not to claim that low-wage workers are prosperous. Low-wage workers clearly have a family income distribution that is skewed towards the lower end relative to all families. Nonetheless, the relationship between minimum wage increases and the income of poor families is rather tenuous. Working with the same data set as Knieser, Bell (1981) reports that 41.6 of all low-wage workers have 1978 incomes $15,000 or higher (which comports with Knieser's results), this when the median income for **all** families is only $16,000. (1981: 457). Her data also reveal that it is not merely teens who constitute non-poor minimum wage earners; also in this group are the elderly and spouses. (1981: 458). Knieser's and Bell's evidence suggests that minimum wage increases redistribute the bulk of income to families who are not poor, and, indeed, to many families whose income exceeds the U.S. median.

Smith and Vavrichek (1987) conduct a similar analysis which uses the March 1995 CPS wage data, and the 1984 family income data. Of workers earning the minimum wage or less (then $3.35 per hour), they find that 19 percent are poor and an additional 12.3 percent are near-poor (defined as income 100 to 150 percent of the poverty level). When the analysis is extended to include higher-wage workers likely to be affected by an increase in the minimum wage (workers earning $3.35 to $4.35), Smith and Vavrichek find that 13.7% percent of "affected" workers are poor and an additional 12 percent are near-poor. In other words, 69 percent of minimum wage (or less) workers in 1985 are neither poor nor near-poor, and 75 percent of "affected" workers (read: up to $4.35 per hour) are neither poor nor non-poor.

Since the minimum wage declined in real terms during the 1980s, we might expect, by 1990, that families with minimum wage workers would be worse off, i.e. less likely to resemble the family income distribution as a whole. Is this expected change born out in more recent data? Only to some extent. There is no doubt that income inequality has increased in the 1980s at all levels, but CK themselves find that minimum wage beneficiaries are not from especially impoverished families, and tend to be distributed across the family income spectrum.

In chapter nine, CK compare (using the March 1990 CPS) workers affected by the April 1990 federal increase (those earing less than $4.25 per hour) with the overall family income distribution, by family income decile. CK find that affected workers are disproportionately clustered in the bottom three family income deciles — 42.8 percent. (1995: 285, Table 9.2, column 5). Nonetheless, they also find that about 38 percent of affected workers live in families who are among the top 50 percent (i.e., in the upper five deciles). This outcome roughly comports with that of Knieser's.

A replication of CK's distribution analysis by Burkhauser et al. 1996 reproduces CK's results, but argues that CK have chosen a flawed measure. First, Burkhauser et al. note that family incomes are not adjusted for family size — so that a one-person family in the second decile ($8,000 to $13,559) is not poor, while all individuals in a four-person family in the second decile are poor. (This is a charge that can be leveled at most of the analyses that relate individual wages to family incomes). Second, Burkhauser et al. argue that minimum wage effects are better measured with respect to the legislation's traditional goal — the reduction of poverty.

Burkhauser et al. conduct an analysis that relates family income to poverty-level income for all workers, those affected and not. Some of their results are reproduced in Table 7.2 below:

Table 7.2 Wage distribution of all workers by ratio of family income to poverty-level income, March 1990

Ratio of family income to poverty-level family income	Percent of all workers	Percent of affected workers (wage below $4.25)
Less than 1.00 (poor)	6.1	22.8
1.00 to 1.25 (near poor)	2.8	6.1
1.25 to 1.50 (near poor)	3.3	6.9
1.50 to 2.00	8.2	11.9

Table 7.2 Wage distribution of all workers by ratio of family income to poverty-level income, March 1990		
Ratio of family income to poverty-level family income	Percent of all workers	Percent of affected workers (wage below $4.25)
2.00 to 2.99	17.9	20.3
3.00 and above	61.7	32.8
Total	100.0	100.0

Source: Burkhauser et al. 1996: 550.

It's clear that affected workers come from families that are less well-off than all families, i.e. the affected-worker family-income distribution is skewed downward. But while, affected workers are more likely to be poor (and near-poor) than are all workers, the great majority of affected workers are neither poor nor near-poor. Only 22 percent of affected workers live in poor families, and only 35 percent of affected workers live in poor and near-poor families. (1996: 550). Another way of stating this result is: about two-thirds of income redistributed by minimum wage increases (assuming **no** employment effects) will go to families who are neither poor nor near-poor.

Table 7.2 also shows that (nearly) as many affected workers live in families with income triple the poverty-level (32.8 percent) as live in poor and near-poor families. The reason, often overlooked, why the majority of minimum wage workers do not live in poor and near-poor families is that only 12.2 percent of **all** workers live in poor and near-poor families. (ibid: 550-1). And, as Smith and Vavrichek 1987 note, about 70 percent (3.6 million) of the 5.2 million minimum wage workers in 1985 lived in families in which at least one other member held a job during the survey reference month. (1987: 27-9). Small wonder that even CK acknowledge that the minimum wage is a "blunt instrument" for redistributing income to the poorest families. (1995: 285).

There are other reasons why minimum wages are a limited instrument of poverty reduction. First, a minimum wage can help only the employed (and those in their families), and, second, at current levels, it can help only a small subset of the employed.[163] CK note that about two-thirds of adults in poor families don't work at all. (1995: 305). Burkhauser et al. 1996 agree, and add that, even more significantly, 25.7 percent of poor families have no working adults. (1996: 551). It is no surprise, then, that CK find that "the effect of the minimum wage on the *overall* poverty rate of adults is statistically undetectable." (1995: 280). Having reviewed the evidence (before 1990) on the employment and anti-poverty effects of minimum wages, we turn now to the question of how economists interpret the evidence. (Additional evidence on distribution effects of minimum wages can be found in Horrigan and Mincy 1993).

7.2.3 What do economists believe about minimum wages — is there a consensus?

Economist jokes are generally of two species, those that pertain to our unrealistic assumptions ("assume a can opener") and those that pertain to our penchant for intramural disagreements ("all the world's economists laid end to end wouldn't reach an conclusion.") Jokes don't work without a measure of truth, but the latter charge is, in some ways, a bum rap. Economics, while disputatious to be sure, has far more agreement than outsiders realize. The consensus is strongest in micro matters — price theory — and especially regarding the consequences of interventions to regulate wages and prices. (See Kearl et al. 1979). There is generally less agreement about macroeconomic issues and about more normative concerns.

The difficulty, and the attendant reputation problem, is that policy makers and the lay public generally associate economics with the very (macro and normative) issues around which there is far less agreement. This leads to what Alan Blinder, a distinguished macroeconomist and policy maker, calls the Murphy's Law of Economic Policy: "Economists have least influence where they know the most and are most agreed; they have the most influence where they know the least and are least

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[163] This first drawback also applies to employment-oriented subsidy programs, such as the EITC.

agreed." (1987: 1). George Stigler lamented the same phenomenon in 1976, saying "when we are most agreed, our success is often small, as the free trade example illustrates. Contrariwise, where agreement is small — as in the current state of Monetarist vs. Keynesian theories of the level of aggregate activity — a theory (now the monetarists') receives substantial (verbal) acceptance." (1982: 59).

Surveys of economists tend to support this conclusion. U.S. economists especially tend to agree on positive, micro issues, such as the effects of minimum wages, and disagree on normative and macro issues. Let us review the opinion survey literature. Kearl et al. (1979) is the beginning of the modern surveys of economists.

In 1976, Kearl et al. surveyed 600 U.S. economists on 30 different economic propositions, allowing three responses: (1) generally disagree, (2) agree with provisos, and (3) generally agree. (211 economists responded). Individual propositions classified by Kearl et al. as either micro or macro and as "can" or "should" (read: positive and normative). They measured consensus using a relative entropy (ϵ) measure, where 0 represents perfect consensus and 1 represents no consensus.[164] The minimum wage question was deemed microeconomic and positive. In response to the following proposition — "a minimum wage increases unemployment among young and unskilled workers" — 69 percent of economists agreed, and 21 percent agreed with provisos. Only 10 percent of U.S. economists generally disagreed that minimum wages increase unemployment among the young and unskilled. (The mean response was 2.6).

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[164] "Relative entropy is defined as observed entropy divided by the maximum possible entropy for the number of outcomes considered, where entropy is the sum of the probability of a particular outcome multiplied by the log (base 2) of the probability [i.e., $(-\sum p_i \log_2 p_i)$]. A relative entropy of 1.0 would result if the respondents were equally distributed across the three response options." (Alston et al. 1992: 203, ff1). Note well, however that the relative entropy measure is non-linear, hence large changes in the distributions of responses lead to small changes in ϵ. (Fuller et al. 1995: 228). Generally, values of ϵ less than 0.7 indicate a "substantial degree of consensus among respondents." (Ibid).

The Kearl et al. study has been updated at least twice since 1976, in Frey et al. 1984 and in Alston et al. 1992. Frey et al. sampled 2,072 economists (with 936 replies) in the United States and also in Austria, France, Germany and Switzerland. (Frey et al. do not report the dates of their survey, but 1982 is a reasonable guess). Responding to the same minimum wage question, 66.4 percent of U.S. economists (n = 211) agreed that a minimum wage increases unemployment among young and unskilled workers, and 21.3 percent agreed with provisos. Again, as with the 1976 survey, only 10 percent generally disagreed with the proposition. (Totals don't sum to 100 percent owing to responses that did not answer). The economic consensus on employment effects of minimum wages (for the young and unskilled) is mostly unchanged from 1976-1982.[165]

Colander and Klamer 1987 surveys graduate students at leading schools, using the Kearl-style format, but permitting a fourth response category, "not sure." They found that graduate students respond somewhat similarly to economists, qualifying their responses a bit more. Overall, 34 percent agreed with the minimum-wage-increases-unemployment proposition; and 39 percent agreed with reservations. Eighteen percent disagreed and nine percent were not sure. (1987: 101). Intriguingly, Colander and Klamer found, amid this moderate overall consensus against minimum wages, sharp differences across programs. Chicago students agree with the proposition overwhelmingly, 98 percent of students agreeing (70) or agreeing with reservations (28). At Harvard, in contrast, only 15 percent agreed, 41 percent agreed with reservations, and 35 percent disagreed (nine percent were not sure). (ibid: 104).

Alston et al. 1992 updates Kearl et al. 1979, to investigate whether the views of U.S. economists had changed in the years between 1976 and 1990. Asked the same minimum wage question in 1990, U.S. economists responded as follows: 61 percent agreed with the proposition that

[165] The responses are very different from European economists, who are much more favorably disposed to minimum wages. I bypass this interesting result to focus on U.S. opinion.

minimum wages increase unemployment among the young and unskilled, 21 percent agreeing with provisos. Eighteen percent of U.S. economists generally disagreed with the proposition ($\epsilon = 0.74$). Between 1976 and 1990, then, the mean response on minimum wage effects fell from 2.6 to 2.4. Alston et al. found the change in response pattern to the minimum wage proposition from 1976 to 1990 to be statistically significant (at the .01 level).[166] Thus, in 1990, there is still strong consensus among U.S. economists that minimum wages cause unemployment among young and unskilled workers, but that consensus has eroded some since 1976 (Kearl et al. 1979) and also since 1982 (Frey et al. 1984), at which times the consensus was stronger still.

The tripartite survey response format doesn't allow for much subtlety in response, so some context may help to shed light on the nature of the consensus on minimum wage employment effects. Alston et al. 1992, to use the most recent survey, asked for responses to 40 different economic propositions. Of these 40, only twelve responses showed more consensus (a lower ϵ) than that regarding the adverse employment effects of minimum wages. If we eliminate the two propositions from these twelve which pertain to historical events (economists agreed that the rise in gasoline prices following the Iraqi invasion of Kuwait was **not** due to the monopoly power large oil companies), to organizations (economists disagreed that reducing the regulatory power of the Environmental Protection Agency would improve the efficiency of the U.S. economy), we are left with only ten economic propositions with more consensus than that on the minimum wage.

Of the ten remaining propositions, most are similar in nature: they involve the benefits of a price system for allocating resources, and the costs of intervening to regulate prices. Four of these propositions are macro-oriented: (1) economists **disagree** that "wage and price controls are a useful

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[166] Of the 21 propositions common to the 1976 and 1990 surveys, 10 responses patterns had statistically significant changes from 1976 to 1990, eight at the 0.01 level and two at the 0.05 level. (Alston et al. 1992: 206).

policy option in the control of inflation" (ϵ = 0.53, mean response = 1.34); (2) economists **disagree** that "wage contracts are the primary factor that prevents the economy from continuously operating at full employment" (ϵ = 0.57, mean response = 1.32); and (3) economists **agree** that fiscal policy can have a "significant stimulative impact" when the economy is operating at less than full employment (ϵ = 0.67, mean response = 2.51). (Economists also agree that the federal budget should be balanced over the business cycle rather than yearly, ϵ = 0.72, mean response = 2.48). A fifth proposition with high consensus (tied with the minimum wage) is that markets are superior for handling market failures: economists agree that effluent taxes or marketable pollution permits represent a better approach to pollution control than imposition of pollution ceilings" (ϵ = 0.74, mean response = 2.36).

Four other propositions are more micro in nature, and they too, involve the benefits of a price system. Economists agree (1) that rent ceilings reduce the quantity and quality of housing available (ϵ = 0.52, mean response = 2.7), (2) that tariffs and import quotas usually reduce general economic welfare (ϵ = 0.57, mean response 2.65), (3) that cash increases welfare more than cash-equivalent payments-in-kind (ϵ = 0.72, mean response 2.43), and (4) that "flexible and floating exchange rates offer an effective international monetary arrangement" (ϵ = 0.70, mean response 2.49). A final high-consensus view, germane to the minimum wage debate, is explicitly normative: economist agree that the redistribution of income within the U.S. is a legitimate role for government (ϵ = 0.73, mean response = 2.4). (All these results are from Alston et al. 1992: 204-5).

I review the other economic ideas with wide support within the U.S. economics profession because they reveal, with some exceptions, the result emphasized by Kearl et al. 1979 — that consensus is strongest around non-normative propositions that derive from price theory, and which tend to support the efficiency of markets, and to oppose government intervention.[167] Many of the most

[167] One obvious exception is U.S. economists' support for income redistribution. The consensus that fiscal policy can be stimulative (at less than full employment) is not, by itself, interventionist. One can

strongly held beliefs are, like the minimum wage disemployment proposition, variations on the idea that price regulation has real quantity effects, and therefore results in efficiency losses. These core beliefs are, indeed, right out of an introductory price theory text: tariffs reduce welfare, wage and price controls are ineffective; cash payments are better than payments-in-kind; flexible exchange rates are good; rent control reduces the quantity and quality of housing.

7.2.3.1 What do *labor* economists believe about minimum wages?

The most recent and most targeted survey results available are of labor economists. (Whaples 1996). Using the same tripartite response format, Whaples mailed surveys to 193 randomly selected labor economists (members of the AEA), obtaining a response rate of 41 percent. Whaples points out that his survey was undertaken after publication of the revisionist research that constitutes much of *Myth*, hence is able to address the question of how labor economists have responded to the new minimum-wage research. Whaples finds that an "immense majority" of labor economists (87 percent) accept (or accept with provisos) the proposition that "minimum wages increase unemployment among young and unskilled workers." (1996: 730). Even with the advent of the "new" minimum wage literature, labor economists are even more likely than economists in general to believe that minimum wages create disemployment.[168] (ibid).

Whaples also asked respondents to estimate how much a "'10 percent increase in the minimum

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believe that expansion is likely without believing that expansionary fiscal policy is well-founded and should be undertaken. Expansionary fiscal policy is well-founded only if one also believes that the inflationary costs are less than the output gains, and that the policy can be enacted at the correct moment in the business cycle, i.e. there are no major problems with timing the stimulus.

[168] Whaples' survey was done before the publication of *Myth*, but after the publication of most of the revisionist research, generally in the ILRR. Whaples found that ILRR readers were more likely to believe (17 percent) that minimum wages have no effect on employment, versus 10 percent of labor economists who don't read ILRR. The median estimated employment elasticity is -0.15 for ILRR readers, -0.2 for others. This may reflect the influence of the revisionist research, Whaples argues, or it may reflect already different beliefs among ILRR readers. (1996: 733, ff 11, 12).

wage from its current level' will affect 'teenagers' employment.'" (ibid). The median estimate was two percent, which comports with the empirical consensus, as reported in Brown et al. 1982 or Brown 1988, for example. Intriguingly, Whaples found that 57 percent of labor economists support an increase in the current minimum wage, which provides a clue as to how labor economists trade off the welfare effects of job loss and wage gain. Whaples notes that estimates of the employment elasticity (negatively) correlate with the likelihood of endorsing an increase. Those who oppose an increase estimate the employment elasticity at about -0.3, whereas proponents estimate the employment elasticity at about -0.1.

Also relevant are Whaples' findings on monopsony. Most labor economists believe that workers' pay "usually approximates" their marginal revenue product (72 percent), but do not believe that labor markers function competitively — two-thirds of those who accept that $w \approx$ MRP do so with provisos. (ibid: 726). Labor economists generally rule out monopsony as an explanation for those instances where w diverges from MRP. "Six in seven" labor economists agree that "'few American workers are employed in monopsonistic labor markets.'" (ibid). Respondents were not surveyed on whether minimum wages effectively target the poor, and I can locate no formal survey evidence of economists' beliefs on this issue.

The lay public has also been surveyed on minimum wages. What the public thinks about minimum wages is briefly surveyed in Appendix 7A.

7.2.4 Summary of empirical evidence (before 1990) and of consensus studies

In sum then, the evidence before 1990 suggests that minimum wages do indeed produce disemployment, especially for teens, and that the relevant elasticities are on the order of -0.1 to -0.2, though less for young adults and adults. Most of the evidence is from time-series studies. When the 1980s are included in the time series, the employment effects become somewhat attenuated, though they remain negative. The evidence on income distribution is that affected workers have disproportionately lower family incomes, but that, nonetheless, most (roughly two-thirds) affected workers are neither poor nor near-poor.

U.S. economists overwhelmingly believe that minimum wages create unemployment for the young and unskilled (though this is not the same thing as reducing employment, see Mincer 1976). The consensus among economists is less firm by 1990, perhaps coincident with the lower elasticities found in the newer time-series studies, or with the higher incidence of younger PhD's, who are less likely to concur with predicted disemployment. (Colander and Klamer 1987; Alston et al. 1992). Among labor economists, however, and even after much of the revisionist research had been published, a large majority (87 percent) continues to believe that minimum wages create disemployment. (Whaples 1996). There is little direct evidence for what economists believe about income distribution effects.

Given this strong consensus, especially among labor specialists, what gave rise to the revisionist minimum research? This consensus on disemployment is already fairly established by the early eighties. (Brown et al. 1982). The empirical literature was perceived as strongly supporting the theoretical predictions of the textbook model, so much so that Eccles and Freeman (1982) could title their review, with mock exhaustion, "What! Another minimum wage study?". There may have been quibbles about the exact magnitude of the disemployment effects, but none about whether they existed. (Deere et al. 1995: 222). Indeed, there was very little empirical work on minimum wages undertaken during the mid-eighties. What changed?

First, the few new time series estimates which did incorporate data from the eighties seemed to suggest that employment elasticities with respect to minimum wages were attenuating, i.e. becoming more inelastic. Wellington (1991), for example, reports an elasticity of only -0.06 for teens over the period 1954-1986 (not statistically significant, however); and Klerman (1992) finds an (significant at 0.1 level) elasticity of -0.052 for teens over the period 1954-1988. (Cited in CK: 182). Secondly, because the nominal value of the federal minimum wage stayed at $3.35 per hour between 1981 and

1990, its real value decreased dramatically, by over 50 percent. In the intervening years, several states raised their minimum wages, which created a research opportunity for cross sectional or panel studies. Finally, the increase in income inequality during the 1980s spurred political action for redistributive measures. In the nineties, minimum wage research revived, and with dramatic consequences.

7.3 The new economics of minimum wages: what are Card and Krueger saying

Much of the "new" empirical work has been carried out by David Card and Alan Krueger (together and separately), sometimes in collaboration with Lawrence Katz. Their research is collected and addressed in *Myth*. I want to emphasize three lines of attack that CK mount on the traditional textbook view of minimum wages. First, they present cross sectional evidence from four studies, most famously from the New Jersey-Pennsylvania fast food "experiment." Their conclusion is that the employment elasticity of the minimum wage is **positive** (but generally insignificant) — jobs are gained not lost. Second, CK review the existing empirical literature and judge it altogether inadequate. They conclude that previous findings are unreliable, tainted by unacknowledged structural changes over time, or by a publication bias in favor of negative elasticities, or by specification search by researchers. Paul Osterman calls this aspect of *Myth*, "a damning indictment of how labor economics has been practiced over the past three decades." (1995: 839). Third, CK review the (income) distributional consequences of increases in minimum wage, finding progressive but "relatively small" (1995: 393) effects.

In addition to its empirical findings, review of the evidence, and consideration of distributional effects, *Myth* is important for its emphasis on empirical methods, in particular natural experiments (NE).[169] Let us consider each of these four aspects, in turn.

7.3.1 The employment elasticity of minimum wages is positive

[169] These four area do not begin to exhaust all the research in *Myth*, which also evaluates the effects of minimum wages upon firm values and output prices, and, in the penultimate chapter, offers some theoretical possibilities to account for their empirical results.

Of their many results, CK say that their "strongest and most important finding" (1995: 387) is the absence of disemployment effects from minimum wage increases. The four cross-sectional studies presented all find a small, but nonnegative employment elasticity of the minimum wage. (They also find, consistent with textbook theory, that minimum wage increases worked to increase low-wage worker wages).

The New Jersey-Pennsylvania (NJ-PA) fast-food study, for example, compares employment responses in the NJ-Eastern PA market, where New Jersey raised its state minimum from $4.25 to $5.05, while Pennsylvania's remained at $4.25. The "natural experiment" compares the employment effects on affected (NJ) workers, against the PA "control" group. Survey data on wages and employment were collected in two "waves" (February 1992 and November 1992), the former before the minimum wage increase took effect (April 1992), the latter after. If all differences between NJ and PA remain unchanged before and after the minimum wage increase, then any "difference in differences" can be attributed to the minimum wage effects.

Conventional theory predicts that, *ceteris paribus*, employment should fall in NJ, and remain unchanged in PA. CK find that (for workers at the entry-level wage) that NJ employment actually **increased**, by 4 percent. (PA employment, it turns out, fell by nine percent).

The other three studies exploit changes in state or federal minimum wages, and make use of a "control" group analogous to the PA fast-food restaurants. They are: (1) the increase in the federal minimum wage to $4.25 in April 1991, studied in Texas; (2) the increase in the California minimum wage to $4.25 in July 1988, studied in California (for teens); (3) the increase in the federal minimum, from $3.35 to $4.25 in two steps (April 1990 and April 1991), studied across several states, for four groups. Their results are summarized on Table 12.1 (1995: 388), part of which I reproduce in Table 7.3.

Table 7.3 Employment and wage effects from minimum wage increases, in *Myth*, by study, and analysis		
Study	Proportional effects on wages	Proportional effects on employment
1) NJ-PA fast-food restaurants	0.11*	0.04
2) TX fast-food restaurants	0.08*	0.20*
3) CA teenagers	0.10*	0.12
4a) Cross-states, teenagers	0.08*	0.00
4b) Cross-states, workers with low predicted wages	0.07*	0.02
4c) Cross-states, retail	0.05*	0.01
4d) Cross-states, restaurant	0.07*	0.03*

Note: * Indicates that estimate is statistically significant at the 0.05 level.
Source: Card and Krueger 1995: 388.

Of the seven separate analyses of employment effects (the California study disaggregated teens, low-wage workers, retail trade workers and restaurant workers), none of the employment effects, significant or otherwise, are negative, contrary to the "textbook" theory prediction. On the other hand, only two of their estimates are significantly different from zero (at the 0.05 level).[170]

CK clearly understand the implications of their results. They recognize that their minimum-wage research has an importance within the profession that exceeds its actual consequences in labor markets, and they suggest that this is because their analysis offers a clean test of the "textbook" model. CK say, in this regard: "the minimum wage provides a simple and direct test of the kind of theoretical reasoning that economists routinely apply to other, more complicated phenomena, and to many policy questions." (1995: 396-7). If CK are right, they are suggesting, then conventional price theory must be wrong, or at least flawed.

7.3.2 CK's evaluation of previous empirical work

[170] The wage effect is statistically significant (.05 level) in all seven analyses.

In chapters six through eight of *Myth*, CK review earlier empirical work on minimum wage effects, and judge it inadequate. I will focus on their evaluation of the domestic time series literature. Evaluating time series research, CK replicate (by extending the time series) some earlier work (Brown et al. 1983; Wellington 1991) and find that the employment elasticities attenuate when more recent years are included. In addition to discussing some of the usual problems with time series econometrics (structural change, *ceteris paribus* omissions, possible endogeneity of minimum wages), CK also conduct a meta-analysis of previous time-series estimates that suggests that earlier work is tainted by (1) publication bias and or (2) specification search.

They argue that time-series estimates, over time, involve larger and larger samples, which, *ceteris paribus*, should increase *t*-statistics. If the additional data in newer studies are independent of the older data, "then a doubling of the sample size should result in an increase in the absolute value of the *t*-ratio of about 40 percent."[171] (1995: 187). If the *t*-statistics don't increase with sample size, then, CK suggest, something is fishy. Using the time-series studies reviewed in Brown et al. 1982 (see Section 7.2.1), CK find that the *t*-statistics are, in fact, **declining** in sample size.[172] (1995: 188).

CK also conduct a related meta-analysis that compares the relationship between the estimated coefficients (of employment elasticity) and their respective standard errors, for the same 15 time-series studies. If the estimated coefficients (*b*) are truly unbiased, CK argue, then there should be no systematic relationship between them and their respective standard errors (*se*). If, on the other hand, editors will only publish studies with robust test statistics ($t > 2$, say), then this publication bias should be reflected in the meta-analysis, given that $t = b/se$. Plotting "implicit" standard errors against the

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[171] CK note the lack-of-independence problem that occurs with time-series data more generally, but point out that previous authors themselves assume independence following adjustments for serial correlation in errors, and similar procedures.

[172] Actually, they plot the absolute value of the *t*-statistic against the square-root of the degrees of freedom, or $(n\text{-}k)^{½}$.

coefficients, CK find a rather good fit (for $b = 2se$), with only two or three exceptions. The relationship between coefficients and standard errors does suggest a problem with independence.

From this meta-evidence, CK offer three possible explanations, none of which reflects well on previous empirical studies. One explanation is that structural change made the effects of minimum wages decline over time. But structural change calls into question the validity of time-series work to begin with. (ibid: 191). The alternative explanations, which CK deem more likely, are even worse; they are specification search and publication bias. CK argue that the early literature was contaminated by a preference (on the part of data mining researchers and or biased editors) for (statistically significant) results that comported with the disemployment predictions of the textbook model.[173]

The econometric specifications which became standard were chosen, CK suggest, because they tended to produce the right sign (negative) and got the t-statistics above two. Later researchers tended to reuse these specifications. Because the statistical significance was overstated in the earlier research, however, more recent work (presumably not so susceptible to temptation) discovered weaker effects (ibid: 192-3). Three of Finis Welch's studies are offered by CK as examples of specification search, as influenced by publication bias.

CK conclude their review of previous empirical work with a surprising conclusion. Once corrected, CK argue that "the bulk of the empirical evidence . . . [is] consistent with . . . if anything . . . a small positive, effect on employment, rather than an adverse effect." (1995: 236).

7.3.3 Distribution effects: are minimum wage beneficiaries poor?

In chapter nine, CK discuss minimum-wage effects in terms of income distribution. Their analysis finds that minimum wage earners (and other workers affected by increases) are disproportionately poorer, that is, their family incomes tend to be lower than that of all families. On

[173] Kennan asks the following cheeky question: if CK's meta-analysis did not comport with their priors regarding specification search and publication bias would they have published it in *Myth*? (1995: 1963).

the other hand, CK also find that most workers affected by minimum wage increases are not poor or non-poor.

As already reported in Section 7.2.2, CK cross-tabulate minimum wage earners (and other affected workers) by family income decile. They find that workers affected by the April 1990 federal increase (those earing less than $4.25 per hour) are disproportionately clustered in the bottom 30 percent of the family income distribution — 42.8 percent. (1995: 285, Table 9.2, column 5). CK also find that about 38 percent of affected workers live in families who are among the top 50 percent (i.e., in the upper five deciles).

Though CK do not explicitly relate affected workers to poverty (or near-poverty) family income, it's evident from their tabulation (and from Burkhauser et al.'s (1996) replication shown in Table 7.2), that about 35 percent of affected workers (in April 1990) are poor and near-poor. This means that the minimum wage only mildly redistributes income. This result is intriguing because CK have argued that their employment results are robust enough to shift the debate away from efficiency and towards distribution: "the minimum wage is mainly a distributional issue — at least in the range of the current minimum wage. (1995: 276). They repeat this claim on page 393: "For moderate levels of the minimum wage, we believe that our findings suggest a reorientation of policy discussions away the efficiency aspects of the minimum wage and toward distributional issues . . . ."

7.3.4 Natural experiments and model free empiricism

Myth is notable for it methodological emphasis on natural experiments, and, more generally, on the possibility of "model-free" empirical work. This emphasis is at the core of their approach to minimum wage economics. It is safe to say, for example, that they take the NJ-PA study as their most important result, precisely because they believe that its NE setting is closest to the ideal of classical (randomized controlled) experiments, and therefore offers superior empirical evidence of minimum wage employment effects. The idea is that NEs can "test" the predictions of the textbook model, even if they are imperfect substitutes for the classical (i.e., randomized controlled) experiments of the "hard" sciences.

CK are careful to acknowledge that NEs are different from the classical experiments they seek to emulate. (Their discussion is 1995: 21-5). Most importantly, NEs do not **randomly** assign of subjects into treatment (get higher minimum wage) and control groups (do not get higher minimum wage), which is essential for attributing any differential outcomes to the intervention (higher minimum wage), rather than to biasing factors. NEs are facilitated by exogenous forces, but the "assignment" of subjects to treatment and control groups are not necessarily random. Minimum wages are, of course, the outcome of a political process, and there may be motivating factors which bias the control group. CK recognize that states which enact minimum wages may differ from those that don't; they may have more robust economic growth, for example. (Ibid: 23). CK also recognize that, unlike classical experiments, NEs may entail some interactive effects, as when control-group firms gain a comparative advantage as treatment firms incur higher costs. (Ibid: 22).

There are other differences between NEs and classical experiments, most of which CK are careful to note. NEs are not "blind;" subjects know when they are receiving the intervention, and when they are not (a placebo). And the NE method also requires assuming that any current differences between treatment and control groups would persist in the absence of interventions (e.g., both groups would have identical rates of growth). (1995: 23).

But CK are confident that these departures from classical experiments are small relative to the advantage NEs provide over traditional empirical approaches in economics. In particular, they cite the advantage of a control group, even given its differences from than a randomly selected one. The NE approach, they argue further, is also model free, in that its results do not hinge on a particular econometric specification, nor, therefore, upon the theoretical assumptions that underwrite a given specification. (Though results can be used to test a theory). And, in principle, NEs can reduce the incentive for specification search, the temptation to fit the model to the data. (Provided the NE can be set up in advance of the intervention, and is "approved" by some kind of consensus among researchers). (ibid: 24-5).

7.4 Replies to Card and Krueger

There are many replies to CK and to *Myth*, testimony to the influence of CK's research. I will focus on a handful of responses intended for a mostly academic audience: Neumark and Wascher 1995, 1996, Welch 1995, Brown 1995, Osterman 1995, Freeman 1995, Hamermesh 1995 and Kennan 1995. My "sample" is probably biased toward critics, but I believe that most reviewers are critical, and CK's allies rarely have more to add beyond endorsement, given how exhaustively researched *Myth* is.

Neumark and Wascher (1995, 1996) are CK's most conspicuous interlocutors within academia. This is because they have amassed a great deal of similarly motivated research that has produced different results (reviewed in their 1996 paper), and because they authored a replication (1995) of CK's most important paper — the NJ-PA fast-food study.[174] Neumark and Wascher 1992, for example, was intended to "check" whether the time-series employment-elasticity estimates (-0.1 to -0.2) hold up in state-level panel data. They find that for young adults, the estimates are similar, but, for teens, the results are less robust (though negative), unless school enrollments are included in the specification. (1996: 67).

Neumark and Wascher's (1995) replication of CK's NJ-PA is superior, they argue, because it makes use of actual payroll data from affected fast-food firms, not less reliable surveys. Neumark and Wascher find a negative employment effect from the NJ increase, which contradicts CK's finding. Neumark and Wascher have not yet released their data (which is proprietary to the EPI), and do not

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[174] Robert Barro, in a *Wall Street Journal* article, refers to Neumark and Wascher as the "heroes" of the minimum wage controversy. (January 11, 1996)

say whether their payroll sample is sufficiently random.

Neumark and Wascher also note that textbook theory refers to labor hours, not employment (bodies) — firms could well reduce labor hours while leaving employment unchanged. (See also Boulier et al. 1996). The point of this critique is that the "textbook" model should be seen as measuring labor quantities in hours, not persons. CK measure quantity of labor in persons (employment), when, in fact, the correct quantity is labor hours. Boulier et al. argue that affected firms may reduce hours for their employees, without discharging them — hence CK's results, right or wrong, need not be inconsistent with the "textbook" model. The quantity of labor (manhours) can fall, even if the firm's payroll contains the same number of employees.

For Finis Welch, a strong critic, the issues are data quality, data release and control-group validity. He accuses CK of (1) sloppy survey methodology, and (2) an improper unwillingness to release their primary data. Welch also argues that CK's NEs are not natural enough, in particular he says that (3) CK's control groups are invalid. (1995)

Welch's strongest argument is that the survey used in the NJ-PA study was flawed, as was the survey methodology, and that the raw data reveal these flaws. The questionnaire is flawed, Welch argues, because it is ambiguous. How for example, should a fast-food restaurant manager respond to the first question: "How many full-time and part-time workers are employed in your restaurant, excluding managers and assistant managers?" Is the respondent to give a compound response (e.g. three full-time and eight part-time), or should the total be given (e.g., eleven). And why can't assistant managers work part-time, Welch asks rhetorically. (1995)

The raw data also reveal several anomalies, Welch says. For example, most of the PA firms paying entry-level wages above the minimum wage (before the NJ increase) are recorded as having **reduced** the entry wage by the time of the second interview (27 out of 46 restaurants). (1995: 844-45). The average NJ restaurant has 21.14 employees (part-timers are coded as 0.5 an FTE) at the first survey, and 21.27 at the second, but firm-specific changes were far larger — the standard deviation of the change is nine employees per firm, says Welch. (ibid: 845). He also notes that the simple correlation between first- and second-interview employment is 0.552, very low for two surveys administered in the same year. (ibid: 846).

Welch could not obtain the data for the Texas study, so he notes only that the results are statistically insignificant, saying that "an inconclusive result does not prove there is no effect" (ibid: 846). But here Welch also upbraids CK for not delivering the data so that he (or other researchers) could attempt a replication (in time for his review). He generally seems to accuse CK of dragging their feet on public provision of the materials needed for replication.

Welch also quarrels with the validity of CK's control groups. He notes, for example, that the CA study, which compares various CA workers with workers from different states and cities is flawed because the "control" areas (Georgia, Florida, New Mexico, Arizona, Dallas) are economically stagnant while California' economy was expanding. The fourth study (see Table 7.3 above) is flawed for the same reasons. In this study, Card assumed that, if minimum wages reduce employment, employment reductions should be greatest in states with the lowest wages (hence the highest cost of compliance). The problem, Welch argues, alluding to Deere et al. 1995, is that low-wage states are not proper "controls"; Southern and Southwestern states (the low-wage states) experienced rapid employment growth in the relevant period. In other words, Welch argues that the *ceteris paribus* requirements of a NE are not met.

If Neumark and Wascher and Welch are opponents, Charles Brown is more of an agnostic. Brown 1995 is less hostile to CK; he takes their results as evidence that employment effects are small, and that the evidence for negative employment effects is increasingly tenuous. Brown emphasizes, however, that CK's results are unavoidably short-run, the nine-month interval between first- and second-round surveys are too short for anything else, he argues (1995: 829). Brown argues that the longer run effects could well be different (and will probably be more negative for employment), particularly as capital is substituted for labor, concluding that more investigation is needed.

Brown also notes that CK have not developed a convincing theoretical alternative for the textbook model they want to replace. CK offer, Brown notes, a variation on the monopsony story (less than perfectly elastic labor supply) due to information imperfections rather than to limited competition among firms hiring labor. He observes that if employment doesn't decrease after minimums rise (or if it increases), then it is reasonable to expect output to increase and output prices to fall. "But the gods do not smile," says Brown: CK's estimates, if imprecise, point towards output price increases. (Ibid: 830).

Paul Osterman 1995 is comfortable with CK's empirical findings, but he also wishes they had done more work in support of their theoretical alternatives. Osterman notes that CK adduce no evidence in support of the different ("ad hoc") theoretical alternatives they broach — efficiency wages, loyalty, or the high wage-low turnover model. Osterman clearly admires the work in *Myth* (and is favorably disposed to its results), but he laments the absence of an institutionalist research style — detailed field-work interviews with actual market participants. (1995: 841)

Osterman is also impressed with CK's meta-theoretical work, finding that this part of *Myth* "damns labor economics as it has been practiced" (1995: 840), an echo of Ehrenberg's remark that *Myth* is a "devastating critique . . . of empirical research methods in economics." (1995: 827). Osterman is pleased to see economists ("the most arrogant of social scientists") get their comeuppance and concludes as follows: " [T]he lesson of this book is that an economist, when asked about the effect of the minimum wage, must first of all report that most past research has been seriously flawed in a somewhat unseemly haste to assure that the results conform to the theory." (Op. Cit.: 841).

Richard Freeman is an enthusiastic supporter of CK; he says *Myth* is "the most careful and wide-ranging analysis of the empirical evidence on minimum wages in the United States that any social scientist could ask for." (1995: 831). Freeman reads CK's employment effects a bit more cautiously than they do, however. He reads the results as rejecting negative employment effects rather than as indicating employment gains. (ibid: 832). Freeman also cautions that, for larger magnitudes, we know that employment will probably fall. Still, he accepts CK's claim that *Myth's* results shift the debate from employment effects to distributional effects.

Hamermesh 1995 is a critic. His main critique of CK is that their NEs are "neither natural nor experiments." Hamermesh seems to agree that NEs are fine in principle, but argues that they are hard to carry out in practice. NEs of the kind that CK advocate must meet three criteria, Hamermesh argues: the first survey measurement must be sufficiently before the policy intervention (P) so as not to miss any adjustment by employers who are anticipating P; (2) the second survey measurement must be sufficiently after P to capture slower-developing effects (otherwise all one measures are the most rapid adjustments); and (3) the control group must be valid, i.e. all change in differences between the treatment and control groups must be attributable solely to the intervention.

Hamermesh argues that CK have failed on all three counts. The NJ minimum wage law was enacted in 1990; CK's survey measurements are in February 1992 and in November 1992, with the law taking effect in April 1992. Isn't it possible that fast-food employers could have already acted on their knowledge that minimum wages would be increasing? Can't one sensibly argue, Hamermesh suggests, that the policy intervention arguably occurred long before its actual effective date?

Arguing in the alternative, Hamermesh also worries that the second survey was too early to capture all employment effects. This is the long run-short run problem raised by Brown 1995. Hamermesh argues that firms adjust their capital slowly relative to labor, which means that firms' employment responses may come well after the second survey. "The simplest theoretical rationalization for CK's results," says Hamermesh, " . . . is that they are observing very short run responses." (1995: 837). The NJ-PA study is valid, Hamermesh argues, only "if employers will not preadjust to the policy change and will adjust very quickly at the time of treatment." (Ibid: 836).

It is also unreasonable, Hamermesh argues, to assume that any differences between treatment and control groups at the first survey, will necessarily remain unchanged absent the policy intervention. This is, Hamermesh says, "nonsense on its face"; and, he suggests, argues for more precise modeling of the variables in question. (ibid: 837).

Finally, Hamermesh also quarrels with CK's interpretation of their meta-analyses of previous empirical work. He finds "astonishing" their conclusion — that the previous evidence, taken together "is consistent with . . . if anything . . . a small positive, effect on employment, rather than an adverse effect." (*Myth*: 236). The reason, Hamermesh says, is that "*Every* estimate they cite or generate is negative, though not all significantly so. No unbiased reader could conclude from Chapter 6 anything other than that the effect is small and negative and thus inconsistent with results from CK's NEs." (ibid).

Kennan 1995 agrees with Hamermesh 1995 that the timing of policy interventions is a problem for CK (he wishes that governments would randomize the timing of the increases to forestall prior adjustments) and that it is difficult to keep control groups pure in NEs. He also agrees with Welch 1995 that noise in the data may be overwhelming. (See his scatter plots of the CK's raw data for an illustration of this, in Kennan 1995: 1959). I will focus on his argument regarding control-group validity.

Regarding the problem of valid control groups, Kennan notes, regarding the NJ-PA study, that the "differences in differences" approach masks an unexpected change in PA employment per fast-food firm. The average number of FTE workers per restaurant increased slightly in NJ, from 20.4 to 21.0, the unexpected result most important for CK. But the average number of FTE workers **decreased** in PA, from 23.3 to 21.2. The first-survey difference (NJ vs. PA) is -2.9 FTE workers, the second-survey difference is -0.2 FTE workers, so the difference in differences is 2.7 FTE workers. Hence, while it is indeed very important that the NJ employment did not fall with the minimum wage treatment, the increase in the "difference in difference" is due mostly to the decline in PA employment. The puzzle is why the "control" group (PA) would have such large employment changes, and the worry is that this casts doubt on the its validity as a proper control. As Kennan notes: this outcome "is like having a drug trial in which the drug has no effect but the placebo makes people sick." (1995: 1958).

7.5 What rules are revealed in the minimum wage controversy

In identifying and discussing what I take to be some of methodological rules revealed by the minimum wage controversy, I am endeavoring to report them rather than to comment on them. Clearly, I cannot hold my normative views permanently in abeyance, and, no doubt, these judgments may reveal themselves in the presentation. It is my intent in this chapter, however, to be descriptive rather than prescriptive.

For organizational purposes, I have organized the rules revealed in the minimum wage controversy into three categories: empirical rules, theoretical rules, and policy making rules. These categories are for convenience in exposition only; some rules will apply in more than one of the categories.

7.6 Empirical rules

The prefatory discussion in Freeman 1989 provides a useful introduction to the special methodological problems that face the economist who wants to do empirical work, and therefore to the matter of empirical rules of practice in economics. Freeman, a labor economist, recalls his graduate-school roommate, a physicist, and the disparaging remarks the roommate made about economic research:

> Empirical economics? How could you learn anything? You can't do controlled experiments, so you can never be certain what you know. You can't test theory because the Invisible Hand, being invisible, can explain whatever you observe. And

> the way the economy changes, parameters are time-variant instead of constant. No one in his right mind can hope to find the truth in economics. (Freeman 1989: ix).

Freeman, swallowing hard, mostly agrees. Empirical economics is not a laboratory science; it uses data generated for non-scientific reasons; it must make use of circumstantial evidence; and it regularly faces self-interested claims by market participants. (Ibid). Economists, Freeman says, are more like detectives than physicists.

The question, then, is how can labor economists, who practice in the most quintessentially inductive of economic fields, learn anything about actual markets? Freeman's answer is that economists must make do with what they have. Given their constraints, economists can only approximate the conditions enjoyed by physicists. To do so, Freeman proposes the following rules for good empirical work in labor economics:

> 1. Exploit natural experiments;
> 2. Focus on "first-order economic principles," i.e., use simple Marshallian analyses, and eschew high theory ("the latest chalkboard squiggles");
> 3. Replicate empirical results (reproduce results, but also replicate in the stronger sense of new data and specifications);
> 4. Develop primary data, especially when government sources are inadequate to questions of interest;
> 5. Discuss issues with actual market participants; surveys can be very useful, and are often superior to "introspection" and "blackboard contemplation." (Freeman 1989: x-xi).

It is interesting to note how closely *Myth* hews to these very rules. CK conduct natural experiments; they keep it relatively simple theoretically; they replicate other work; and they develop their own data. They do not offer much interpretive discussion with market participants, but otherwise they meet virtually all of Freeman's criteria. Small wonder, then, that Freeman's review of CK, finds it "a model of how do to empirical analysis," work that is "careful and searching" (1995: 832, 831).

Freeman's list of s introduces a host of empirical issues in the minimum wage controversy. Of particular interest are the following: (1) how should data be developed (i.e., what kinds are legitimate to use), (2) how should data and results be reported, (3) how should results be obtained (what methods are legitimate), and (4) how should empirical findings be treated by other researchers? Let us take these issues in sequence to introduce what rules are revealed in the controversy.

7.6.1 What kinds of data are legitimate: to survey or not to survey?

Let me begin by identifying empirical rules which pertain to how economic data should be developed. At issue is a venerable dispute in economics — do surveys produce legitimate data? Economists officially prefer data that consist of actual market transactions rather than of responses to questionnaires. "Unlike other social scientists," McCloskey points out, "economists are extremely hostile towards questionnaires and other self-descriptions." (1985a: 181). The reasoning is twofold: in survey settings, (1) agents may have incentives to misrepresent or to withhold information, and (2) agents will tend to be less thoughtful than in market situations, where there are material incentives to get it right. Hence, goes the argument, watch what agents do, not what they say.

Voters might lie to a political pollster, and consumers might misrepresent (or be lax in detailing) their demographic profile to market research firms. There is evidence, for example, that experimental futures markets in political candidates predict actual voting outcomes better than does traditional polling. (See the University of Iowa College of Business Administration's Electronic Markets). Political market participants have an incentive to get it right that ordinary survey respondents do not.

Official hostility notwithstanding, empirical labor economists have traditionally relied on, yes, surveys, especially the Current Population **Survey**. Empirical researchers make direct and extensive use of other government data that are also survey generated, such as the Consumer Expenditure Survey and the Panel Study of Income Dynamics. As such, CK's critics within labor economics, who have all used the CPS extensively, cannot sensibly criticize them for using survey data *per se*, and do not do so. On the other hand, not all surveys are alike, so not all survey data are equally suspect.

Surveys differ in their efficacy. Better instruments try to control for known problems in measurement — framing effects, bias to the first response alternative, excessive complexity, changes in meaning, errors in recall, and so on. All surveys must deal with these threats to reliability — the rule is garbage in, garbage out. But this true of all empirical work — experiments and econometrics have analogous pitfalls that must be confronted. The economist's traditional objection to surveys centers on the incentives for veracity and for accuracy, not on the efficacy of a given survey.

None of this is to say, however, that survey data are worthless. On the contrary — they are simply less reliable than transactions data. Transactions data have their own problems, of course; they are often unavailable (e.g., with untraded goods), measurement can be difficult, and they may be more expensive. The foregoing suggests a data rule, and a corollary:

> (ER1) Economists should prefer more reliable data to less. Since survey data are deemed less reliable than market data, surveys should be acceptable mainly when admittedly superior alternatives are unavailable (or uneconomic).
>
> (Corollary 1 to ER1) Survey data collected where there are greater incentives to be accurate and truthful (e.g., penalties for false statements) are more reliable than those without such incentives.

The idea of the first corollary is that payments or penalties to respondents may help to overcome (or at least mitigate) the incentive problem. Settings where respondents have fewer incentives to lie and face lower costs of participating are likewise preferable.

Note that survey reliability is not just a function of the incentives for veracity and accuracy; it also depends upon the nature of the information that respondents are asked to supply. Hence there is another corollary to ER1, which is:

> (Corollary 2 to ER1) Incentives equal, surveys which ask respondents to speculate are less reliable than those which ask them to report data they already possess.

By "speculation" I have in mind "contingent valuation" surveys, which ask respondents how much they would be willing to pay for certain goods not traded in markets. Asking respondents their party affiliation or their hourly wage involves a reporting task that is more straightforward than asking respondents how much they would pay to keep a certain wildernesses pristine. (See the symposium on contingent valuation in the *JEP* 8(4), Fall 1994). This difference can affect data reliability.

CK are not criticized because they use surveys for primary data development, nor because they advocate NEs. CK are criticized for being sloppy in their survey design and execution, especially in their most important case, the NJ-PA fast-food survey. As we have seen, Welch 1995, Hamermesh 1995 and Kennan 1995, for example, all argue that the NJ-PA case is flawed due to survey weaknesses. This aspect of the minimum wage controversy suggests another data rule:

(ER2) When economists develop their own data, they should be careful.

Economists, like all scientists, should be careful in all aspects of their work. ER2 simply applies the "be careful" rule in the data development context. It has special relevance in contemporary economics, however, because, unlike other social sciences, empirical economics has turned away from primary data development in favor of fancier (econometric) techniques of data manipulation, which has increased reliance on secondary data sources. One upshot is that even distinguished empirical researchers, such as CK, may be less skilled at primary data development than government (and other) professionals who do it for a living. Empirical economists are far less adept at data development — dirtying one's hands — than they were a generation ago, hence the pertinence of ER2.[175]

There is also the fear, quite apart from expertise, that it is easier for economists who generate their own data to produce data that are friendly to their preferred hypotheses, or to otherwise cook the results to fit their theoretical priors. Independently collected data (e.g., government statistics) are, the argument continues, less vulnerable to such manipulations. Rules of disclosure (see ER4 and ER5 below) are intended to help circumvent this problem.

[175] Some have suggested (see Mayer 1994) an economic explanation for the emphasis on data manipulation over data development. The argument is the cost of data manipulation has fallen relative to the cost of data development. Faster computing and better econometric software make data manipulation lower cost than data development, where the painstaking techniques of collecting and assembling information have not changed much over time.

The caution of ER2 notwithstanding, I believe that most economists applaud the spirit (if not the execution) of CK's emphasis, following Freeman, on developing more (and better) primary data. I cannot locate any objection to the idea that minimum wage research could benefit from better data. Perhaps more controversially, it also appears that economists, including CK's critics, agree with CK that the returns to better data are higher than the returns to more sophisticated techniques of data manipulation. Kennan's (1995) review of *Myth* makes this point as follows:

> "[CK's] lasting contribution may well be to show that we just don't know how many jobs would be lost if the minimum wages were increased to $5.15, and that we are unlikely to find out by using more sophisticated methods of inference on the existing body of data. What is needed is more sophisticated data. The fast food data . . . show the potential benefits. . . . (1995: 1964, emphasis added).

Rather than continuing to drive a Mercedes down a cow track (Mayer 1994: 132) it may be preferable to improve to road.

The foregoing suggests a related empirical rule:

> (ER3) Economists should invest more resources in data development relative to fancier (econometric) techniques of data manipulation.

This is not to say that economists will observe ER3; rules are not always self-enforcing. Should the disciplinary incentives that favor data manipulation over data development persist, then a gap between what economists say they should so and what they actually do will also tend to persist.

I want to make a final point on surveys and data development: not all survey data are meant to be a substitute for transactions data. There are things that direct investigation can reveal that transactions data cannot. This is point of Freeman's fifth empirical rule — economists should talk to market participants. Conversations with low-wage workers, managers and other actual participants can reveal subtleties that may not be apparent in transactions data. Interviews may also suggest alternative theoretical avenues when results are anomalous.

That investigators should talk to their subjects is, of course, second nature to empirical researchers in other social sciences. Detailed interviews were also a staple of labor research during the institutionalist (what CK call "social economics") heyday of John Dunlop, Clark Kerr, Richard Lester, and Lloyd Reynolds (called DKLR by Freeman in 1989: 317). But labor economics has changed enormously since the 1940s and 1950s (see Freeman 1989 chapter 16, for an appraisal of whether it has changed for the better). Interviews as part of detailed case studies are no longer practiced in modern labor economics, as CK perhaps inadvertently demonstrate.

Myth, which is dedicated to Richard Lester, and which invokes the name and ethos of the institutionalists, does not interview participants except to generate quantitative data obtainable in other ways. (Neumark and Wascher, for example, obtained their replication data from the payrolls of the NJ-PA firms). Card and Krueger challenge the conventional wisdom on minimum wage effects, and thereby join the DKLR tradition of dissent against orthodoxy. But CK do not truly follow in the ethnographic footsteps of the institutionalists, as Kennan (1995) points out.

Paul Osterman laments the decline of institutionalist methods in labor economics. Osterman suggests, for example, that interviews might have offered some empirical support for CK's "speculative" theoretical explanations for the positive employment effects they find, citing the example of Lloyd Reynold's (1951) interviews with employers. (1995: 841). But most of the commentators on the controversy, even the more empirically-minded, do **not** endorse Freeman's fifth rule, which calls for more actual discussions with market participants. Economists welcome more sophisticated data (see ER3), but my reading is that, by "data," economists mean something quantifiable. Conversations with subjects may be interesting, but, in economics at least, they are not deemed "data." *Myth* may well provoke more emphasis on primary data development (and on empirical work more generally), but I see no evidence that any such revival will stimulate a revival of the institutionalists' ethnographic methods.

7.6.2 How should data and results be reported?

Beginning with its September 1989 issue, the *American Economic Review* publishes in all regular issues an "editorial statement" that reads as follows:

> It is the policy of the *American Economic Review* to publish papers only where the data used in the analysis are clearly and precisely documented, and are readily available to any researcher for purposes of replication, and where details of the computations sufficient to permit replication are provided. The Editor should be notified at the time of submission if the data used are proprietary, or if, for some other reason, the above requirement cannot be met.

The statement appears to have been inserted in the aftermath of Dewald et al.'s (1986) famous attempt (and failure) to successfully replicate empirical papers from the *JCMB*. The idea, of course, is that economists should observe the following rule and corollary for open disclosure:

> (ER4) Open disclosure: economists should make their data (including raw data) and methods openly available, so that other researchers can attempt to reproduce their results.
>
> (Corollary to ER4) Data and methods should be available to all, not merely to intellectual allies.

The idea is that researchers should openly publish their results, in order to provide the means for others to replicate them. This is the norm of "be forthcoming" discussed in Chapter Six, and it also works to promote the institution of replication.

The minimum wage controversy has repeatedly returned to the open disclosure rule. Both "sides" of the debate have invoked it when accusing the other of methodological obstruction. Welch 1995, upbraids CK for not making available to him the raw survey data, coder instructions, and analytical data file for the Texas study (authored by Katz and Krueger). Kennan reports that only part of the *Myth* data advertised as available by anonymous FTP (*Myth*: 18), are actually there. (1995: 1957, ff. 6). Neumark and Wascher's replication of the NJ-PA study makes use of propriety (to the Employment Policies Institute (EPI), a lobby of firms organized against minimum-wage increases) payroll data that they and the EPI will not release. Regarding the latter, Kennan says: "[Using] these data to attack the credibility of data supplied to them by Card and Krueger, while refusing to release their own data . . . [is a] kind of hit and run scholarship [that] will not get us very far." (1995: 1961, ff. 9). Finally, CK nowhere publish the questionnaire used in their most important study, the NJ-PA paper — an odd omission in a book that is scrupulously complete in most respects.

These violations of ER4 from the minimum wage controversy are far from uncommon. Very few results in economics are actually replicated, so the *AER*'s stated policy is rarely put to the test. Only a fierce controversy like the minimum wage debate, where professional reputations (and core beliefs) are perceived to be at stake, induces researchers to take the time and trouble to replicate. Ordinarily, as Feigenbaum and Levy (1993) argue, the professional returns to replication are lower than to original research. It is clear that in the minimum-wage controversy, the failure to observe the rule of open disclosure (ER4) provides fuel for mostly unfounded charges of ideological bias.

Even though regularly violated, the *AER* policy is, we should note, not as strict as it might be. As my added emphasis indicates, the *AER* insists only that other researchers provide the means to reproduce **the** analysis, i.e., the analysis that is reported in the paper. This neglects the rather common practice of researchers conducting more analyses than are actually reported in their write up, a practice known as "selective reporting." Selective reporting is sometimes pernicious, as in the case of specification search, a data sin when testing hypotheses. But even relatively innocuous omissions will tend to deny other researchers valuable information, which could prevent wasteful duplication of effort, among other things. This suggests a rule not just of open disclosure, but of full disclosure.

> (ER5) Full disclosure: economists should make openly available **all** data, methods, tests, etc., not merely those used to obtain reported results (at least up to the point where the marginal social benefits of disclosure equal the marginal private costs of complying).

Given that ER4, open disclosure, is generally observed only in the breech, it is probably not reasonable to expect that ER5 (full disclosure), a more demanding requirement, will be widely observed. Nonetheless, it seems to be a empirical rule in economics (see ER8 below).

7.6.3 How should empirical findings be treated by other researchers?

Clearly implied in the ER4 rule are two closely-related empirical rules that pertain to how economists should treat published empirical findings:

> (ER6) Economists should replicate published empirical findings, in the narrow sense of reproduction, using the same data and methods (checking).
>
> (ER7) Economists should replicate published empirical findings, in the broader sense of extension: using bigger data sets (e.g., additional years in a time series), or using alternative models with the same data (robustness), or both.

Theses are, of course, variants of the scientific institution of replication. In the minimum-wage controversy, there is, as noted, a relatively large amount of replication, and in all senses of ER6 and ER7.

CK, for example, attempt to replicate the results in Wellington (1991) and also in Brown et al. 1983. (They augment the time series through 1993, and use different statistical software). CK report "exact" reproduction of Brown et al.'s results, and "extremely close" replication of Wellington's results. (1995: 194). Neumark and Wascher's (1995) replication of CK's NJ-PA fast food study involves a completely different data set (payroll versus survey data) for the same period. CK also replicated (calling it a "reanalysis") Neumark and Wascher's 1992 cross-sectional study. CK's different results turn on the question of specification — should the variable for "the proportion of age group enrolled in school" be included as an independent variable. (1995: 211-15).[176] Burkhauser et al. 1996 reproduce CK's income distribution analysis.

All the participants in the minimum wage controversies either explicitly endorse, endorse indirectly (by practicing it) or do not explicitly oppose replication. Burkhauser et al., for example, take issue with CK's income distribution analysis, but explicitly applaud their efforts as improving

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[176] For more on this particular debate, see the arguments and replies in the *Industrial and Labor Relations Review* symposia in the October 1992 issue (volume 46), and in the October 1994 (volume 48) issue.

economists' "poor track record" at replication. (1996: 547). Neumark and Wascher, are CK's most direct interlocutors, but they endorse (in practice) CK's emphasis on replication. Welch's strong critique (1995) is based, in part, on his inability to replicate all of CK's results, indicating a belief that replication is good economic practice.

The amount of replication in the minimum wage controversy is vastly higher than is ordinary in economics, a good thing if one agrees with Feigenbaum and Levy 1993 that there is too little replication in economics. One can, of course, also have too much replication, and I don't wish to claim that replication of the magnitude observed in the minimum-wage controversy is optimal for economic research more generally. But, especially given current disciplinary incentives, the controversy reveals the benefits of more replication.

It also shows, I believe, the benefits of intellectual competition in promoting disciplinary virtues such as replication. As we saw in Chapter Six, this implies a rule:

> (ER8) Economics should be intellectually competitive; (healthy) intellectual competition increases the likelihood of replication and other scientifically valuable practices.

Healthy competition promotes replication. By "healthy," I mean competition constrained by the right institutional structure, which includes some of the rules under discussion. Obviously, slander, libel, destruction of research materials, physical violence and the like are beyond healthy competition, as economists understand it. Given the right institutional structure, however, an intellectual discipline like economics can benefit from competition, even that which is motivated by partisan ideology.[177]

Ideology is clearly present in this debate. The evidence is compelling — in particular the fact

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[177] Both "sides" have been accused of ideological bias. Krueger, for example, was charged with being a lackey of Robert Reich, the pro-minimum wage Secretary of Labor at the time. As Alan Blinder points out, this charge is absurd — most of the *Myth* research was done before Krueger took the job of chief economist at the Dept. Of Labor, and before he had even met Reich. (Alan Blinder in *The New York Times* Op-Ed page, May 23, 1996). Neumark and Wascher have been called pawns of the low-wage lobby, because they have used data collected by the EPI. This is an equally silly charge.

that, for most part, "liberal" economists are predictably for CK, and "conservatives" predictably against. This outcome can be read as bad or as neutral for economics as an objective discipline. The "bad" reading is that economists make their theoretical choices based on their ideological priors, evidence and theory be damned. The "neutral" reading is that the evidence is equivocal (finding minimum wage effects is like locating a needle in a haystack, says Kennan), and that more value-oriented considerations thereby come into play, and appropriately so. My point is ideological bias is unavoidable and what matters is having an institutional structure that tends to promotes less rather than more influence of ideology in theoretical choices.

Healthy competition, I believe, is part of that structure, even if it ideologically motivated. Whatever their motives, CK have done economics a service by reexamining an issue most economists considered closed. This research would probably not have been done by proponents of the status quo. By the same token, we can be fairly certain that CK's allies (inside and outside the profession) will not scrutinize *Myth's* results too closely. Fortunately, competition ensures that there are number of researchers (Neumark and Washer 1995, Welch 1995, Deere et al. 1995) who see their own research threatened, and who therefore have an incentive to replicate and otherwise scrutinize the revisionist minimum wage literature. Competition cannot guarantee that research will be subjected to greater empirical scrutiny, but it does help.

The foregoing suggests that ER8 can be seen as a particular instance of a more general meta-rule, that is, a rule regarding rules:

> (ER9) Meta-rule: Economics should have a disciplinary structure that reduces the incentives for specification search, selective reporting, publication bias and other methodological sins, and increases the incentives for open and full disclosure, better data development, replication and other methodological virtues.

I believe that the meta-rule ER9 applies in economics even if I am incorrect about the particular rules which it designed to promote. The idea is to increase the incentives for good economic practice, and reduce the incentives for bad economic practice. ER9 is less an application of the general norm "be honest," as it is a recognition that, *ceteris paribus*, honesty will increase when the cost of honesty is lowered (or the cost of dishonesty is increased).

7.6.4 How should results be obtained?

The minimum-wage controversy also suggests some empirical rules which pertain to empirical opportunities, i.e. ways in which economists can take advantageous of favorable research conditions, in particular "natural experiments." The following rule and corollary are, I believe, revealed in the controversy:

> (ER10) Natural experiments: given limited access to quasi-experimental opportunities, economists should exploit exogenous changes in markets, especially those with large effects.
>
> (Corollary to ER10) Economists should exploit exogenous changes in markets, especially where a "control group" can be identified.

Even CK's most vigorous interlocutors seem to concur with ER10 and its corollary. Minimum wage researchers of all stripes have long exploited the fact that minimum wage changes are (sometimes) exogenous to labor markets, and therefore offer a quasi-experimental opportunity. Among labor economists, all of CK's critics have done research that attempts to exploit exogenous changes in minimum wages (or other labor market variables).

I also cannot locate any objection to the corollary to ER10: the idea that "control groups" make NEs especially compelling. Freeman obviously considers NEs a key empirical rule. Even those who question whether CK's work can be considered a proper NE (Welch 1995; Hamermesh 1995), agree that NEs are empirically advantageous to labor researchers. Hamermesh, for example, argues that the analyses in *Myth* are not legitimate NEs, but he favorably cites two other papers (Card 1990 and Angrist 1990) which more successfully carry out a NE (1995). Neumark and Wascher 1995 obviously obtain different results than do CK, but they too are committed to the idea that NE settings present especially fruitful opportunities for empirical researchers. And Kennan wishes that legislatures would make minimum-wage NEs even more authentic by randomizing the timing of increases (1995: 1950).

7.7 Theoretical rules

We now turn to the theoretical rules which are revealed in the minimum wage controversy. For convenience, I have categorized these rules as follows: (1) pertaining to the Law of Demand, and (2) pertaining to the Duhem-Quine problem, i.e., what to do when data disconfirm theory. Let us take them in sequence.

7.7.1 The Law of Demand and minimum wages

The most conspicuous aspect of the many critical responses to *Myth*, is the idea that CK's results are incompatible with the Law of Demand, a core, perhaps **the** core belief among economists. "Belief in the Law of Demand," says McCloskey, "is the distinguishing mark of the economist, demarcating him from other social scientists more even than his other peculiar beliefs. Economists believe it ardently." (1985a: 59). If the employment effect of minimum wages is positive, as CK argue, then labor is that most elusive of goods, a Giffen good, and there is an empirical counterfactual to the Law of Demand.

This perceived threat to the most fundamental of economic beliefs has lead to some extraordinarily intemperate reactions, sometimes by the most distinguished of economic scholars. James Buchanan, for example, a 1986 Nobel Laureate, said in the *Wall Street Journal*:

> The inverse relationship between quantity demanded and price is the core proposition in economic science . . . . Just as no physicist would claim that 'water runs uphill,' no self-respecting economist would claim that increases in the minimum wage increase employment. Such a claim . . . [denies] that there is even minimal scientific content in economics, and that, in consequence, economists can do nothing but write as advocates for ideological interests. Fortunately, only a handful of economists are willing to throw over the teachings of two centuries; we have not yet become a bevy of camp-following whores." (*Wall Street Journal*, April 25, 1996).

In the same issue, Merton Miller, a 1990 Nobel Laureate said: "[E]conomists used to believe that there was no such thing as a free lunch. Some now seem to have found one . . . . Is this too good to be true? Damn right. But it sure plays well in the opinion polls. I tremble for my profession." Such reactions are not confined to academic luminaries. Camp-following whores? Trembling professors? CK obviously touched a tender nerve.

Even the less dramatic responses to *Myth* routinely make recourse to the Law of Demand — the idea that downward-sloping demand curves will not logically permit positive employment elasticities. William Poole, Milton Friedman, David Bradford, Martin Feldstein, Bradley Schiller, William Niskanen, Arthur Laffer and Robert Barro all invoke the Law of Demand in the opposition to CK (*Wall Street Journal*, April 24, 1996). The professional reaction to *Myth*, especially that from outside labor economics, suggests the following theoretical rule:

> (TR11) Failing some extraordinary (if unspecified) evidence to the contrary, economists should regard the Law of Demand as true.

> (Corollary to TR11) The Law of Demand applies in all markets; i.e. economists should assume that labor markets are not different in kind from financial and goods markets.

Some economists would go even further, suggesting an even stronger alternative to TR11, which says:

> (TR11a) The Law of Demand is true, and should be regarded as beyond empirical refutation, i.e. as part of a Lakatosian hard core.

Robert Higgs, for example, is in this latter group. He regards the Law of Demand as logically necessary, a self-evident truth. Higgs says: "*no* study can overturn this economic logic." (June 21, 1996 "The Madness of the Minimum Wage," The Independent Institute, emphasis added). Any study which suggests the Law of Demand is untrue, should be seen as flawed by definition.

Most economists, I conjecture, are closer to TR11. Robert Barro, to pick an example, says that the Law of Demand is contingently true until disconfirming evidence can be found. Barro says that while the Law of Demand "is not a mathematical theorem, it is an empirical law that has avoided any clear violations in all of recorded history." (*Wall Street Journal*, January 11, 1996). CK, Barro suggests, are arrogant to believe that their work could be "the first documented case in which demand curves fail to slope downward." (Ibid). Barro offers no evidence for his sweeping historical assertion, but I think most economists would agree with TR11, as the survey evidence in Section 7.2.3 suggests.

The corollary to TR11 contains an important qualification. It says that neoclassical price theory (as depicted in the "textbook" model) should apply everywhere — in labor markets no less than financial and goods markets. The idea is that markets are markets are markets. The institutionalists (DKLR) would have disagreed; their point was that labor markets are different in kind, characterized by tradition, fairness, loyalty, seniority and other differentiating bugaboos. While most economists would probably agree labor markets are less likely to be efficient, they do not, the corollary to TR11 argues, think that this difference merits abandoning price theory.

Of course, CK's results are not, strictly speaking, incompatible with the Law of Demand, nor even with the "textbook" model of low-wage labor markets. As Freeman notes, with irony, "economic theory is so 'rich' that it offer us monopsony models that predict increases in employment in response to minimum wages." (1995: 831). One can argue that monopsony is unlikely in low-wage markets, but that is another matter, and does not entail refutation of the Law of Demand.

CK's results are even compatible with a properly dynamic version of the "textbook" model, as Hamermesh 1995 points out. Given the relatively short interval (nine months) between the two surveys of the NJ-PA study, for example, it's reasonable to regard firms' employment choices as short term — the "after" measure may have occurred too soon to capture long term (negative) adjustments. In the long term, firms may gradually substitute other inputs (capital) for labor, which is, Hamermesh argues, "consistent with rule economic analysis in the presence of adjustment costs in factor demand." (1995: 837).

Several other commentators also worry that CK are measuring only short-run effects of minimum wage increases. (See, for example Brown 1995: 829; Freeman 1995: 833). This objection does not mean that CK's results are wrong, but it recognizes that long-term results may differ. It also suggests a familiar theoretical rule in economics:

> (TR12) Given that they can be different, economists should be careful to distinguish short-run from long-run effects.[178]

CK themselves recognize that their results may well be short-run in nature (1995: 387). The catch, as usual, is measurement. As Freeman 1995 points out, longer-run analyses will likely run afoul of the usual time-series problems, such as inflation or a change in the structure of labor markets. Hence, while it is theoretically reasonable to suppose that long-run effects will differ; empirically it remains a challenge to estimate them. Rules of what economists should do, do not always come with the means of attainment.

7.7.2 When data disconfirms: how should economists handle the Duhem-Quine problem?

Economists, of course, have many reasons for their belief in the Law of Demand — logic, introspection, analogy from other core theories, and evidence. (On the variety of reasons, see McCloskey 1985a: 57-62). Barro's defense argues from evidence; he says that Law of Demand has never been empirically refuted. True or not, this raises a vital methodological issue in economics: what should economists do when the evidence **does** appear to contradict the theory? How, in other words, should economists handle the Duhem-Quine problem?

The Duhem-Quine problem, recall from Section 2.3.4, is that testing failure cannot unambiguously reveal what has gone wrong. Are the data incorrect, or is the theory, and, if it is the theory, precisely which aspect thereof — a core proposition (firms maximize profits), an auxiliary hypothesis (agents live for two periods), a simplifying assumption (labor is the only input), or some

[178] Related is the magnitude problem. CK's results cannot readily be generalized; even if they are correct; their results hold mainly for changes in minimum wage that are similar to those they studied — about 20 percent. Larger increases in the minimum (100 percent?, 500 percent?, Freeman asks) will almost certainly cause disemployment, as CK acknowledge (Freeman 1995: 75, ff 9).

implicit background knowledge (rates of time preference are constant)? Since theorists must choose what has gone wrong, one cannot assume that the data "speak for themselves." The Duhem-Quine problem is inevitable, particularly in the social sciences, and the key issue is not whether it arises, but how scientists respond to it. Any rule for handling the Duhem-Quine problem will involve trading off the value of currently useful theories versus the value of having theories that are well supported empirically. Karl Popper, recall, proposed one rule for these instances — eschew *ad hoc* modifications to rescue theories refuted by the data.

Much of the minimum wage controversy has turned on the question of whether CK's results, especially positive employment effects, are sufficient to refute the "textbook" minimum wage theory. Critics generally argue that CK's results are far too fragile to constitute anything like a decisive refutation of the "textbook" model — hence the theory should stand. (Neumark and Wascher 1995, Welch 1995). Supporters find *Myth's* evidence compelling, and suggest that "textbook" theory must be reconsidered (Freeman 1995, Osterman 1996). The "agnostics" say that the theory should stand, pending further research, particularly on longer-run effects. (Brown 1995).

My reading of these different reactions suggests the following theoretical rule:

> (TR13) Duhem-Quine problem: when evidence is at odds with theory, reject the evidence, not the theory (especially when the theory involves core propositions).

As I suggested in an earlier chapter, there are few important economic ideas that have been overturned by data **alone**.[179] Melvin Reder 1982, in his survey of Chicago-school economics, identifies its method as one that virtually always opts to save theory from contradictory data. He says, flatly:

> "Because . . . it is presumed that the currently accepted theory is valid, new findings are accepted far more readily if they are consistent with the theory's implications than if they are not . . . [and] the evidence required for acceptance of a finding is greater

[179] Few, of course, does not mean zero. An example of where the data did prove decisive, is the slope of the long-run Phillips Curve.

. . . if any of its implications are inconsistent with [the theory]." (Reder 1982: 21).

Theoretical adherents simply "distrust reports . . . of behavior incompatible with the implications of economic theory." (ibid). This hostility to unfriendly data, extends to scholarly reputation. Reder observes that Chicago faculty who do applied economics are favorably evaluated only when their evidence comports with the theory. Unfriendly evidence is taken to be a sign of lack of ingenuity on the part of the economist. Reder says: "Inconsistency of empirical findings with the implications of [the theory] . . . is considered to be poor performance." (ibid: 33).

Chicago is perhaps an extreme case, but it does seem that economists are more attached to their theories, than are other social scientists. I suspect that this attachment is more than mere unthinking dogmatism; it is more of a convention that has its roots in two factors: (1) a recognition that data in the social sciences are relatively inferior, and or (2) a tendency to invest relatively more intellectual resources into theory. These factors are related.

Data **are** different in the social sciences. They are often harder to come by, less likely to be accurate, and, by their very nature, susceptible to change over time, owing to the effects of time and place. One result is that our empirical knowledge tends to depreciate over time (Freeman 1989). Unlike in physical sciences, say, it's harder to build on knowledge we can take as given, or at least as stable. There are two possible responses for a social science — invest more resources into improving the data, so as to narrow the empirical disadvantage, or invest relatively more in theory, presumably on grounds of comparative advantage. Without claiming to know the what the optimal mix is, I think it is clear that economics has chosen the latter course — the disciplinary structure traditionally rewards theory more than empirical work.

Because economists have historically invested more intellectual resources in theory than in data — which is one response when empirical testing is difficult or costly — there may be a selffulfilling process at work. Having invested relatively more in theory, economists are loathe to reject their costly theories when data are disconfirming, and generally, having invested less in data, are already somewhat skeptical of the data that does exist.

Andrew Schotter 1996 pursues this line of reasoning. Compared with psychology, Schotter proposes, economists invest far more in their theories. In psychology, he says, hypotheses are a dime a dozen, so psychologists are more inclined to collect data and to use that data for testing hypotheses (rather than for estimating parameters, or for calibration). "The problem with economists and models," says Schotter, "is that we tend to take them too seriously," and the reason is that they are, for us, more valuable.

This tendency to overvalue theory relative to evidence is a recurring theme in modern economics. The mid-century Lester-Machlup (and Friedman, and Alchian) debate comes to mind. This debate, already discussed in the context of prediction and of false assumptions, was less about the merits of (Lester's) surveys as instruments for generating data, than it was about whether any data can be used to test (core) theoretical propositions, such as maximization.

There are two related theoretical rules which also emerge from the minimum-wage controversy, and which pertain to economics as a theory-intensive field, and to Duhem-Quine problems. The first is:

> (TR14) It takes a theory to beat a theory; economists should not reject the prevailing theory absent a viable alternative.

The idea is that it's not enough to criticize the prevailing theory, nor is it enough to produce contradictory evidence (given TR13). One must also offer a better theory, an idea which can be found in Kuhn [1962]1996: theories don't compete with the data, they compete with theoretical rivals.

This is an old rule in economics. The key to the rule's operation, of course, lies in what is implied by "viable." Hamermesh, for example, explicitly invokes this rule when he says: "The authors

[CK] challenge economic notions that make logical sense with new evidence, but they never offer a convincing theoretical explanation for why the old logic fails," i.e. they don't offer a viable theoretical alternative. (1995: 838). It's clear that a rule like TR14 can help to ensure that worthy (if partially disconfirmed) theories are not abandoned due to naive falsification. But, as we have seen, overly aggressive falsification is not a problem in economics. The rule can also work, as Popper feared, to immunize current theoretical favorites from criticism, which destroys theoretical innovation.

In addition, there is the matter of what counts as a empirical refutation decisive enough to topple a theory. All the principals in the minimum wage controversy have made a judgment about whether *Myth* is sufficient to overturn (or at least to require modification of) the textbook theory — some say yes, most say no. But, none of the controversy's commentators have indicated precisely what it would take for them to consider the textbook model (or any model) refuted, only that it would take more than CK and the other revisionists have provided. In economics, for better or worse, criticism, and even empirical disconfirmation are rarely sufficient to topple an established theory.[180]

The historical preference among economists for theory over data has bred (or reflects) a certain skepticism among economists regarding econometric (and other) evidence. This skepticism is not total, however, which leads to the following rule:

> (TR15) Meta-analysis: Econometric evidence should always be treated skeptically, but more econometric evidence for a theory (many papers with similar results) is evidentially preferable to less.

I believe that this rule is correct, if imprecise, because "more" is not well defined in contemporary economics.

In the minimum wage controversy, one question was: why have CK not treated their own

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[180] This point suggests some interesting future empirical work: survey economists on what, in their mind, constitutes a decisive refutation. In the context of the minimum-wage debate, the question might be: "what evidence would you require in order to conclude that the textbook theory is to be rejected?"

findings with the same skeptical scrutiny with which they treated the (far larger set of) findings that they debunk in their meta-analysis. (See, for example, Kennan 1995: 1964). One answer is self-interest; we can't reasonably expect researchers to be as critical of their own (or allied) work as they are of their "opponent's" work. This reasoning is what supports the argument for intellectual competition. Another answer is that CK consider their results superior, because of the greater power of their empirical methods (especially NEs) over more traditional (say, time-series) analyses.

The latter answer points to an important issue that is has not received much attention in economics: how should economists make sense of a large empirical literature, especially when there are conflicting results? (See Goldfarb 1995). One way is to count studies — sixteen for, three against disemployment; disemployment wins (given majority rule or some super-majority rule). Magnitudes come from some kind of averaging across different results. But there are obvious problems with a "counting" approach: not all studies are created equal. As Kennan points out: if only one study is "impeccable" but inconclusive, why toss the inferior studies into the pot and take an average? (1995: 1955).

Another approach, more formal, is to undertake statistical meta-analysis. Meta-analysis is rule practice in other fields that are more data-oriented than economics, like epidemiology and psychology (Goldfarb 1995: 206). It is far less common in economics, perhaps because economists are more skeptical about the inherent value of our empirical results. I notice that none of CK's critics quarrel with the execution or the results of their meta-analyses of the empirical literature, and that none object to the idea that meta-analysis can shed light on a large empirical literature.[181] In some respects, CK's meta-analyses are more telling against the conventional wisdom in minimum-wage economics than are their own findings, which are far better known, and which they themselves make more

[181] Hamermesh 1995 is only a partial exception; he objects to CK's **interpretation** of their meta-analysis results.

prominent.

Even if economists are relatively unsophisticated in meta-analytical matters, and even if they are, more generally, skeptical about econometric evidence, they do consider the preponderance of evidence when making theoretical choices. "Preponderance" remains to be defined clearly in economics (which would allow a more refined rule), but I believe that TR15 remains valid even in its imprecise form.

7.8 Policy making rules

The impact of the minimum wage controversy derives, in large measure, from its connections with economic policy making, a separate matter from the empirical and theoretical issues we have already discussed. At issue is the intersection of economics with politics. The first policy-making rule I have identified is:

> (PR16) Because economic policies can have unintended consequences (good and bad), it's the job of economists to identify and estimate the value of these unintended consequences, in order to help rationalize policy making.

The idea of unintended consequences, good and bad, is a favorite trope of economists, with roots that antedate Adam Smith. Charles Brown observes that:

> Economists' function is to point out unintended consequences. Our habitual refrain is that simple policy fixes have more fizz than fix and may do unanticipated collateral damage. Our cheerful side — the 'invisible hand' demonstration that greed has unintended benefits — goes unappreciated, so our dismal side dominates public perception." (1995: 828).

The unintended consequences of minimum wages in the textbook story are: (1) creating job losses for the very people the policy intends to help, and (2) subsidizing workers who are not poor.[182] An increase in the minimum wage will not only be inefficient, it will also tend to be perverse, hurting the very individuals (low-skilled) whom the policy is intended to help, and helping those whom the policy

[182] These two issues have been our focus, but there may be, of course, other unintended consequences for output, prices, firm values, labor force composition, and so on.

does not intend to help. (For more on the idea of perverse outcomes as a trope in economics see Hirschman 1991).

The word "unintended" suggests well-meaning but ignorant policy makers. On this view, the role of the economist is as educator; economists provide theoretical knowledge that enables the policy maker to make the superior, enlightened choice. (See Cordes, Klamer and Leonard 1993). But there is more: economics also has a view of politics which suggests that policy makers are not always well-intentioned. On the public-choice view, the problem is not policymaker ignorance *per se*, it is the inefficient incentives that plague the political process.

The economic view of political motivation suggests a related policy rule (and corollary) revealed in the minimum wage controversy:

> (PR17) In advising policy makers, economists should not only endeavor to count all costs and benefits of policy (PR16); they should also be **advocates** of efficiency, because no one else will.
>
> (Corollary to PR17) Economic policies which comport with free-lunch arguments are therefore immediately suspect, and should be subject to stricter scrutiny.

Embedded in this rule is the implication that the political process encourages inefficient policy choices, that politically popular policies tend to be inefficient. Ignorance matters, but even fully-informed policy makers have political incentives toward inefficient policy choices.

The familiar public choice reasoning says that policy makers are no different from other economic agents; they are self-interested and respond to incentives. The problem for the economist is that political incentives are biased towards inefficient policy choices. This bias toward inefficiency derives from two facts: first, reelection depends upon net benefits conferred on supporters, and second, organized special interests are more influential than the national interest. The upshot is that policy choices tend to confer economic gains on small, well-organized special interests, such as producers, while imposing economic costs on large, unorganized and diffuse groups, such as consumers.

Government subsidies of milk production, for example, confer benefits on milk producers which are exceeded by the costs incurred by milk consumers, who pay higher prices, and taxpayers, whose taxes fund the subsidies. Imported automobile consumers lose far more than automobile producers and the Treasury gain with protective tariffs. (To say nothing of dead-weight losses). The politician doesn't care whether the aggregate social costs exceed the benefits, what matters is the effect on political support.

In making policy, then, politicians have strong incentives to emphasize benefits, and to obscure costs. There are two ways of obscuring costs. One is to be make them less evident to those who must bear them. This is why politicians prefer "mandates," for example, which are harder to see, to taxes, which are more conspicuous. The other technique, more pernicious, is to spread costs over a large population, so that the cost inflicted on any one voter is relatively small, ideally smaller than the cost of organizing to oppose the policy. Concentrated benefits and diffuse costs are a recipe for political success, and for economic (efficiency) failure.

The policy rules PR16 and PR17 emphasize the importance of efficiency, and also the reality that efficiency has few friends in the political process. Ignorance is part of the problem, to be sure, but unwillful ignorance can be overcome. As we saw in Appendix 7A, voters are much less enthusiastic about minimum wages when they are informed about unintended consequences (potential higher prices don't faze them but potential job losses do). Willful ignorance is a harder problem, and leads to the PR17 rule. Given the politician's natural inclination for free lunch arguments, economists can be socially useful when we insist that benefits should exceed costs. Who will argue for the national interest (in efficiency), if not economists?

One reason CK faced such strict scrutiny (i.e., ran afoul of PR17) is because their conclusions seemed to suggest a free lunch. The government could raise minimum wages (moderately) without incurring employment losses, *Myth* suggested, thereby achieving a redistribution with no obvious efficiency losses. Some benefits, no costs. Set aside this issue for the moment. Even economists who accept the *Myth* idea that employment losses are close to zero, and that the redistributive effects are mildly progressive are entitled to ask Stigler's policy question: are there alternative policies that achieve the same goals more efficiently? And, if there are, is it inappropriate to endorse a second- or third-best alternative like the minimum wage?

Burkhauser et al. 1995, for example, argue that the minimum wage is, in fact, inferior to the EITC, which more directly targets the working poor, and doesn't risk disemployment. Their simulation results find that the EITC, which adjusts for family size and is means-tested, directs 41.3 percent of benefits to the working poor, as compared with 19.3 with a minimum wage. (1995: 16). If the goal of minimum-wage policy is to reduce poverty, why not opt for a policy alternative that is better at poverty reduction, and, in addition, doesn't risk disemployment?

CK sympathizers, like Alan Blinder, reason as follows: "yes there are other ways to help the people at the bottom of the economic ladder. But every solution sacrifices some economic efficiency, and Congress is not about to enact any of them." (*New York Times* Op-Ed page, May 23, 1996). On this view, half a loaf is better than none; given the vagaries of Washington, economists should take what they can get.

The alternative view argues that there are adverse consequences to endorsing policies thought to be second-best. First, it gives aid and comfort to policy entrepreneurs who are political creatures, indifferent to efficiency considerations. This is not to say that economists cannot endorse inefficient policies that achieve other social goals, such as poverty reduction. Such a position would be absurd, and most economists believe that the government has a legitimate role to play in redistribution (see Section 7.2.3). The point is that minimum-wage policy, whatever its efficiency consequences, is also inferior at poverty reduction, and that economists have limited political capital to expend. (See Goldfarb and Baldwin 1996).

Though I am less sure that the following rule is widely observed enough to be deemed a policy rule, I offer PR18 nonetheless. It says:

> (PR18) Because their political capital is scarce, economists should not promote second-best or third-best alternatives; more efficient poverty-reduction policies are to be preferred.

The danger that PR18 addresses is that minimum-wage policy will be sold, as it has been in the past, as a tool of poverty reduction. If policymaking resources are scarce, then poverty reduction efforts on other, presumably superior policies will fail. A Republican senator, for example, can claim to have supported poverty reduction by endorsing a minimum, and thereby purchase (undeserved) political cover for opposing (or failing to support) more efficacious poverty reduction policies.

7.9 Interpretation: what manner of rules are these?

The eighteen rules (and corollaries) that I have inferred from the minimum-wage controversy take a variety of forms. In fact, most of them may be seen as belonging to one of the institutional categories proposed in previous chapters, notably norms and conventions. Consider the empirical rules first. Most of the ten empirical rules can be seen as norms — that is, as general rules of intellectual conduct where individual incentives may differ from collective goals — as they manifest themselves in a particular science, economics.

ER2, for example, says: when developing your own data, be careful, a straightforward application of the "be careful" norm discussed in Chapter Six. This means not only be scrupulous in one's procedures, but also avoid the temptation, when collecting data for hypothesis testing, to produce data that is friendly to one's hypothesis. ER4 and ER5, the rules of open and of full disclosure, are, likewise, specific applications of more general norms we have already considered in Chapter Six, "be honest" and "be forthcoming."

ER6 and ER7 pertain to replication; they are norms which propose that economics should replicate empirical findings to ensure quality, and these too we have already considered in the theoretical discussion of Chapter Six. ER8 and ER9 can be seen as meta-norms. They argue that economics should be structured (e.g., should be intellectually competitive) so as to create incentives that promote the observance of norms such as those just noted, i.e., economics should attempt to align individual and collective interests.

ER1 is also a general intellectual norm; it says that economists should prefer more reliable data to less, (with the corollaries that reliability is promoted when survey respondents have an incentive to be truthful and when they are not asked to speculate). ER1 is, in part, specific to social science, as it concerns the reliability of data that is self-reported. Unlike in the natural sciences, self-interest impinges at two levels in economics; the economist must worry not only about the reliability of her fellow scientists, but also that of her subjects. ER1, then, combines the general intellectual goal of reliable data production with the economist's view of likely respondent behavior.

The sociologist surely wants accurate data as much as the economist, hence the norm of reliable data production applies in sociology as well. But the sociologist may worry less about the incentives facing respondents or about the hazards of speculation. Hence, economists and sociologists may share the scientific goal of accurate data production, but their different views of human action can result in somewhat different rules for survey work. These small variations notwithstanding, note that all of these norms are instances of what I argue are more general intellectual norms operating in the specific setting of economics.

ER10 (economists should undertake NEs) can likewise be seen as pertaining to the special empirical circumstances in a social science. ER10 is not itself a norm, so much as a discipline-specific strategy for meeting the broader scientific goal of developing reliable empirical results. ER3 (economists should invest more in data development as against data manipulation) can been seen in a similar light, that is, as a goal-promoting strategy specific to the current situation in economics. ER3 is a economics-specific strategy that attempts to address a divergence from the scientific goal of producing reliable data — a divergence due the perceived historical under investment in data development by economists.

Again, the point is that these norms are instances in economics of more general intellectual norms (or, in the case of ER3 and ER10, strategies for norm-promotion). It is reasonable to suppose that these norms will persist in economics, as they will in other sciences, at least as long as there remains a gap between individual and collective interests.

The theoretical rules identified, are, in contrast to most of the empirical rules, more discipline-specific and more conventional in nature. The Law of Demand (TR11) is clearly discipline-specific, and the rule which says that it should be regarded as true is an important convention in contemporary economics. TR13, TR14 and TR15 may also be regarded as conventions, conventions which pertain to theory appraisal, with varying degrees of disciplinary-specificity.

TR13 (when evidence contradicts theory, reject data not the theory) names the convention that economists generally observe when facing inevitable Duhem-Quine problems. As Schotter 1996 suggests in his example of psychology, other disciplines employ a different convention in situations where data contradict theory. In psychology, Schotter suggests, the convention is the opposite: when the data contradict the theory, reject the theory. A conventional response to uncooperative data helps to coordinate research strategies within a discipline, but, like all conventions, it need not be optimal, as it is an evolved response that reflects historical choices in Duhem-Quine situations.

TR14 (do not reject theories absent viable alternatives) is a related convention. It reads like common sense — why abandon even a creaky, unreliable old car for no car at all — and can be construed as counsel against naive falsification. Yet, as phrased, TR14 begs the question of what is to count as "viable." And, if interpreted strongly enough, it can work to immunize current theory from potential competitors, given that theoretical alternatives do not, as a practical matter, emerge as full-blown, complete rivals to the prevailing theory. To the extent that (see Reder 1982 on the Chicago

School, above) prevailing theory is regarded as nearly inviolable, theoretical innovation will likely be thwarted.

TR15 (more econometric evidence for a theory is evidentially preferable to less) is also a convention in economics which pertains to theory appraisal. Given the conventional preference for theory over evidence in economics, TR15 can be seen as a kind of subsidiary institution. It gives (econometric) evidence some credence (in preferring more to less) in theory appraisal, without permitting the evidence, by itself, to be decisive when making theoretical choices. In effect, econometric evidence is admitted asymmetrically — it can never decisively refute a theory, but it may be used to support a theory.[183]

The policymaking rules are also norms of conduct, but these norms pertain to economists in their role as policy makers (or advisors) rather than as scientists. The policymaking rules derive from a normative view of what constitutes good social policy — promoting social welfare through greater efficiency — and from a positive view of the political process in the United States — policy is biased toward inefficient choices (and toward obscuring the inefficiency). This combination of views leads to PR16 and PR17 norms: economists should ferret out all costs and benefits of policies, and should be partisans of efficiency, particularly since no one else will.

PR16 and PR17 are norms because they ask economists, in effect, to serve (what they take to be) the national interest, irrespective of their individual self interest. Intriguingly, some policy economists do indeed see their role as partisans of efficiency; clearly such a role can be characterized seen as part of the economist's *ethos*. (Cordes, Klamer and Leonard 1993). But, unless we are to exempt economists from their own theory, PR16 and PR17 are best seen as norms.

[183] Several readers have suggested that the overthrow of the Philips Curve constitutes a kind of decisive refutation of theory by the data. While I agree that the current macroeconomic consensus views the (long-run) Philips Curve as vertical, the original Philips Curve was less a theory (on a par with, say, the Law of Demand) than a putative empirical regularity, invoked, in part, to rationalize aggregate demand management.

7.10 Conclusion: what have we learned?

First, it is important to issue a caveat regarding the scientific rules inferred from the minimum-wage case. I believe that the theoretical, empirical and policy making rules are correct, i.e., that they constitute a plausible representation of what most economists actually believe. They remain, however, conjectures. Clearly, more research will be required to ascertain the extent to which the conjectures are accurate, and the extent to which to they apply in economics (and elsewhere) more generally, outside of labor economics proper. But the task is begun.

What have we gained in the attempt to empirically identify scientific rules in economics? There are, I suggest, three useful outcomes: illustration, evidence, and the beginnings of what I hope is a larger effort to empirically confront some hypotheses in the theory of science (economics). The first outcome is straightforward: we have produced concrete illustrations of the more abstract categories of rules developed in Chapters Five and Six. It is one thing to propose that norms operate in science, and it is another to show an actual working instance. The minimum-wage case study has suggested what I take to be plausible norms and conventions, scientific rules (and corollaries) that are real-world examples of the more abstract theoretical categories developed in the preceding discussion.

Second, the rules identified from the minimum-wage case offer some confirming evidence. The rules are not, of course, empirically conclusive, but, to the extent they are correct, they provide some support for the Smithian economic view of scientific rules developed in Chapters Five and Six. They provide evidence, for example, for the existence of the different categories of rules. Among the rules identified are both norms of intellectual conduct, which tend to apply across many disciplines, and also methodological conventions, which tend be more discipline-specific.

The rules also reveal some of the difficulties which all rules are likely to entail. Recall from Chapter One the discussion of standards of theory appraisal, which noted difficulties of consensus, ambiguity in interpretation, and conflict among standards. There is some evidence from minimum-wage case of conflict among rules, and, perhaps, of difficulty in interpretation. (We obviously do not assay consensus among economists on rules, though the discussion on beliefs in Section 7.3.2 gets to theoretical consensus).

The rule that economists should develop more data, for example, may sometimes be in conflict with an implication of the "be careful" norm, namely, don't use your own data to test your hypotheses. The risk, as noted, is that one will be tempted to produce friendly data, which perhaps creates a rationale (as Welch 1995 suggests) for independent data production by, say, government statistical agencies. On the other hand, it is exclusive reliance on independent data sources that has encouraged, in part, the relatively greater investment in data manipulation over data production.

There are also, among the rules identified, some which lend themselves to different interpretations. With TR14 (do not reject theory absent viable alternatives), for example, much depends upon how "viable" is construed. Interpreted weakly, it is a merely a caution against excessively naive falsification; interpreted strongly, it is an immunizing strategy that can work to protect prevailing theory from any and all criticism. Different economists may well have different interpretations of how to interpret TR14, depending upon their respective readings of "viable."

As will have been to clear to most readers, many of the identified rules are not met in the actual practice of economics. The open disclosure rule (ER4) and the replication rules (ES6 and ES7) are conspicuous examples that have been discussed earlier. Methodological discourse in economics routinely invokes rules, especially norms, that are not observed in practice.

There are also results from the empirical work in this chapter that bear on meta-theoretical issues more directly. It is important, I believe, to recognize that different types of scientific rules — norms of conduct, conventions regarding methods and rewards, standards of theory appraisal — perform different functions. Since some scientific functions are likely to be common to different disciplines, so too are the rules which perform them. Though empirical research on other fields will be required, it seems likely that some basic scientific rules — especially norms of intellectual conduct — will indeed apply widely. This is because norms generally arise in incentive-incompatibility settings common to most disciplines, and most scholars who refer to norms in science generally do conceive of them as having general application.

If norms do apply more generally, this is a counter-factual to the new methodologists' claim that rules **must** be relative to a given community, and cannot travel beyond the local borders. (See Chapter Four). It seems clear that some rules can indeed travel. More substantiation is needed, but I think that this preliminary counterfactual result is interesting, both for its own sake, and also because it suggests an expanded role for empirical work in the theory of science (economics), a point taken up below.

By the same token, however, it is also apparent that some scientific rules are indeed discipline-specific. The conventions which indicate economists' preference for theory over data in Duhem-Quine situations are likely in this category. Other fields, such as psychology, appear to have rather different conventions when the data disagree with the theory. This result is a counterfactual to the received-view idea that rules are universal across disciplines. Some rules travel and others don't, and this supports a meta-theoretical position that mediates between the extreme claims of the received view and of the new economic methodology.

An additional result of this chapter's empirical work pertains to the new methodological claim that scientific rules are difficult to articulate, because they are tacit, or complex, or idiosyncratic, or routinely in flux, some combination of these things. Hence, "the reasonable rhetorician cannot write down his rules." (McCloskey 1985: 52).

I find this demurral puzzling, for it is at odds with other laudably empirical projects of rhetorical economics. Rhetorical economics has had an impact beyond most methodological work in large part because it has made empirical claims about economics: that practice departs from methodological preaching, and that official discourse is quite different that revealed in more informal settings (Klamer 1983), for instance. In any case, I take the rules identified from the minimum-wage case to constitute a counterfactual to the new methodological position: the rules **can** be written down. Without claiming that my list is necessarily correct (or exhaustive), I do believe that it demonstrates that the rules can be written down, and with some specificity.

This takes us to some final thoughts on the prospect for empirical research on rules in science.

7.10.1 Empirical research on rules in science studies

In the introduction to this dissertation I noted a central puzzle that motivated my inquiry: how to reconcile a realistic view of scientific motivation (scientists are self-interested and fallible) and of scientific knowledge (certain knowledge is unobtainable) with the view that science is successful in producing reliable knowledge. Either its success is a miracle of happenstance, or there is something noteworthy in the way that science is organized. I choose the latter explanation, and, in proposing a (Smithian) economic view of science, have emphasized the importance of rules in the organization of science.

For those who opt for explanation by miracle or who would deny the premise (that science is successful), the foregoing seven chapters will have been entirely beside the point. Some constructivists deny that any scientific rules are operational, meaning that individual scientists' choices are wholly subjective, and that, therefore, science is irrational. This explanation, however, cannot be reconciled with the high degree of consensus that obtains in many scientific fields.

More sophisticated constructivists, such as the new economic methodologists, grant that there are rules in science (see Chapter Three), and that the rules are influential. But they argue that science is not successful, and thereby arrive at a pessimistic conception of the nature of scientific rules. The new methodologists argue that scientists proceed as they do **not** because there are objective reasons for doing so. Rather, we call those procedures reasonable because a certain group sanctions them.

(Casti 1989: 26). In other words, scientific rules exist, but they do no epistemological work. They are to be seen as *post hoc* rationalizations of currently prevailing practices. Hence: "[A]ny method is arrogant and presumptuous." (McCloskey 1983: 490).

The problem with this position is that it attempts to deny any normative role for the theory of science, ostensibly reducing science studies to pure description. I don't think this can be done, as I argued in Section 3.2. No matter how distasteful we find the pronouncements of an earlier time, no matter how presumptuous their proponents might have been, the normative role of methodology is unavoidable. Working scientists (economists) make theoretical (and other) choices with reference to standards. Unless we are to be completely indifferent to those choices, then the rules matter. And if the rules matter, then it is worth knowing what they are — as well as something of their nature, source and function.

The principal difficulty is that the evidence is hard to come by. Though science studies has greatly increased its output of empirical research in recent times, there has been relatively little work on scientific rules.[184] Scientific rules in economics exist, and they can be powerful motivators, but they are hard to observe directly. The rules of economics, we have seen, are revealed only fleetingly — in the choices economists make, and, to some extent, in their methodological arguments.

But difficulty is not sufficient grounds for abandoning the empirical task. Ultimately, in fact, there is no other option. Theorists of science (economics) can benefit from empirical work when evaluating competing meta-scientific views. We need to test our theories, no less than the scientists we study.[185] If our theories are not brought into contact with actual phenomena, then we are

[184] In addition, as noted, the most active empirical researchers in science studies are constructivists who deny any epistemic significance to scientific rules, concentrating their efforts elsewhere. This problem is quietly being remedied, as scholars who are not constructivists become more empirically oriented.

[185] More empirical work also has the virtue of making methodology more accessible to practicing economists, who are put off by methodological discussion that, too often, comes enshrouded in

unnecessarily impoverishing our research. Dan Hausman makes the case as follows:

> The philosophy of science is itself an empirical science. All conclusions about the scientific enterprise that the philosopher of science draws are, or should be, scientific conclusions . . . . When the philosopher of science makes pronouncements about the goals of science or the basis or bases upon which scientists accept various theories or about any other feature of science, we should regard these pronouncements as scientific claims and assess them as we would assess the various assertions the sciences make. (Hausman 1992b: 221).

This naturalistic view of the theory of science, "stated trenchantly and simplistically," is: "the philosophy of science is itself an empirical science." (ibid).

Empirical work is not about to crowd out theory in the theory of science. Problems of knowledge are very complex theoretically, and it is no accident that the bulk of this dissertation is itself theoretical. Many issues in the theory of science simply do not lend themselves to testing. But others do, particular when meta-scientific claims take the form of what Popper called bold conjectures, employing terms like "never," or "always" or "known to all." In the end, science itself must be studied if we are to have an empirical basis for choosing among rival claims in the theory of science, and this position is what motivates my very preliminary attempt to identify some of the institutions which operate in contemporary economics. Meta-scientific "pronouncements" regarding scientific rules should be, at least in some instances, testable hypotheses.

As an example, take the received-view and the new methodological positions on standards of theory appraisal. The received view says that standards are universal, hence standards apply in all communities and are invariant over time. It also claims that standards are known to all inquirers, which implies that there is a consensus on standards, that there is no ambiguity in interpretation, nor conflict in application. Hence, standards of theory appraisal can be codefied, and together constitute an algorithm for theory choice (and theoretical disagreements are something of a puzzle).

The new methodologists, in contrast, argue that standards are relative to specific communities;

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metaphysical vapors.

they cannot apply more broadly, and will vary over time. Further, there is disagreement on what the standards are; they are ambiguous in application and may conflict. Hence, standards are difficult to explicitly articulate, never mind represent as an algorithm (and theoretical agreements are something of a puzzle).

My point is that meta-scientific claims such as these (and others like them) are testable. But to test them, we need some data. More empirical work on scientific rules needs to be done, and I hope to have shown, by example and by argument, that such work is worth undertaking. There are intellectual profits to be had in using economics to think about science, and also in using science to think about economics and the rest of science.

Appendix 7A What does the public think about minimum wages

Minimum wages tend to be popular politically. CK cite 1987 Gallup Poll data which indicate that three-quarters of the U.S. population favored an increase in the minimum wage. Another 1987 poll, more targeted, finds that the public was evenly divided on whether minimum wage increases reduce employment — 24 percent "agreed a lot"; whereas 23 percent "disagreed a lot" with the statement. (1995: 7).

Freeman 1996 has some interesting results from a 1989 ABC/Washington Post poll. The poll asked the following questions, to which the public responded:

> "Would your salary or anyone in your immediate family's salary go up if the government increases the federal minimum wage to $3.35 an hour?"

chief wage earner's salary would go up	08%
someone else in family's salary would go up	12%
salary of no one in family would go up	79%

> "Would you still favor raising the minimum wage if businesses passed the increased salary costs to the consumer in the form of higher prices for some goods and services?"

Yes	82%
No	16%

Freeman finds it "striking" that the "vast majority of consumers in a country predisposed against government interventions in wage-setting and not particularly favorable to income redistribution were willing to pay higher prices for an increased minimum. . . ." (1996: 641).

It is interesting to note, however, that the popular enthusiasm for a minimum wage increase again wanes when the possibility of employment losses is added to the survey. The same poll asked this question:

> "[Would you support a minimum wage increase] even if it causes some businesses to fire workers or stop hiring new workers rather than pay the increased salary?"

Yes	52%
No	42%

Another three percent of respondents volunteered that they not no firm would ever do such a thing. (Freeman 1995: 643). The evidence is sketchy, of course, but minimum wages are generally politically popular, if less so when job losses are considered. Most citizens probably believe that, as individuals, they are unlikely to incur any costs.

Fuller et al. (1995) surveyed 2500 Democratic and Republican national conventional delegates, asking the same questions put to economists in Alston et al. 1992, plus nine other propositions. The sample was split evenly between the two parties. Response rates, as well as mean and variance of the parties' respective overall degrees of consensus were not statistically different (at 0.1 level), though their respective responses to individual propositions varied in 33 of 39 cases. In those 33 cases, 19 involved opposing responses, not merely similar responses with different degrees (ϵ) of consensus.

In response to the minimum wage proposition — minimum wages increase unemployment among young and unskilled workers — Republicans, like economists, agreed, though with even more consensus than economists. The Republican ϵ = 0.68, versus 0.74 for economists. Democrats disagreed with the proposition, and with even more consensus in opposition. The Democratic ϵ = 0.58. The responses to the minimum wage proposition are distributed as follows: (Republicans: A=64.5, N=13.1, D=20.7; Democrats: A=14.6, N=10.7, D=73.2).[186]

I derive a few tentative conclusions from this limited data. First, the public generally sees minimum wage legislation as no-cost, at least to them. From the voter's perspective, this is preferable to anti-poverty programs which do impose costs (like taxes) on them. Second, voters tend to see economic issues in terms of "jobs," not in terms of prices or efficiency. Policies which are seen to

[186] The survey is slightly different in format from Alston et al. 1992 (and the predecessors)and must be treated with caution. A is "mainly agree"; N is "neither agree or disagree" (which corresponds to "agree with provisos" in the earlier surveys); D is "mainly disagree." (Fuller 1995: 232).

promote (or to save) U.S. jobs are good, period. Third, not surprisingly, consensus tends to be high among political party activists. Fourth, minimum wages are a kind of political litmus test issue for party activists, and their positions are based on issues somewhat different from those economists' consider. Democrats see the issue in "fairness" terms (income inequality being the principal concern); Republicans see the issue in "freedom" or "anti-intervention" terms (free contracting and less regulation being the principal concern).

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