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AN EXAMINATION OF THE SCIENCE OF POLITICS IDEA

by

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CHAPTER I

INTRODUCTION

The science of politics idea is a product of the eighteenth century. It was inspired by the successes of Newtonian physics, and given form by the British empiricists' explanation of those successes. On that view, social sciences seemed both possible and desirable: the method could be applied to any factual subject, and it seemed to be the only means for gathering such reliable knowledge as might be had. The results would be limited to an instrumental mapping of factual patterns, but a political science so constrained was preferred to one consisting of ungrounded speculation. With only minor modifications, this argument was repeated in the works of many influential writers, including Comte and John Stuart Mill. It reached American political science largely by that route.

The science of politics idea has taken two rather different forms in American departments. The first is a straightforward extension of the eighteenth century argument. This instrumental view (which structured the "Science of Politics" movement of the 1920s) was joined after the Second World War by a modified version, in which science is assumed to offer a realistic understanding of political events. Both forms continue to guide research, but the second is easily the more influential.

A broad range of critical attacks have been directed against these research programs. For present purposes, this diversity may be divided into two categories--one consisting of doubts about the availability of political events to scientific treatment, and the other of suspicions concerning the meaning of such theory as might be produced. In the first group, one finds arguments to the effect that the relevant concepts are inherently qualitative, that the patterns of influence are too complex to be convincingly mapped, and too difficult to isolate, given the rare opportunities for experimental control, that social events are unique, hence unavailable to generalization, and that, in any case, free will makes nonsense of the idea of behavioral laws. In the second group one finds concern that the technical challenges, (and the violence done to events by supposing that facts and values can be separated) would at best limit theory to trivia, and at worst produce something seriously misleading. In addition, it has been held that "facts" are necessarily biased by social interest, or by other factors--biases that would inevitably infect the associated generalizations--and that explanations which ignore verstehen cannot conceivably provide a realistic understanding of human behavior.

This paper is an attempt to clarify the prospects for these science of politics programs. Most importantly, this means clarifying the prospects for a realistic science of politics, as this is the version for which political scientists have shown real enthusiasm. Fortunately, persuasive external evidence (from the history of science) can be brought to bear upon this form of the argument. It seems that the physical sciences--those disciplines which inspired and have subsequently served to justify the science of politics idea--do not themselves achieve realistic theory. It is unlikely, then, that a political science modeled upon them will do so.

The instrumental version of the argument is unaffected by this evidence, of course, and by the internal criticism concerning the status of positive theory. If realism is not attempted, then failure to achieve it is not much of an obstacle. One is left, then, with arguments concerning the degree to which political events may be available to scientific treatment. Such arguments cannot be decided at present. It seems clear that the critics' "impossibility" arguments are too strong--there are no obvious and final limits to the application of the proposed techniques--but there do seem to be significant practical obstacles to theory construction. This inevitably directs attention to practical performance.

In sum, then, it is argued that a science of politics is probably limited to instrumental forms, that even this may be quite difficult, given the practical research problems, and that nothing short of instrumental *performance* can establish the legitimacy of the idea or the value of research to that end.

CHAPTER II

AN AMERICAN SCIENCE OF POLITICS

The modern effort to create rigorous, empirical social sciences on the model of physics may be traced to the "Newtonian revolution". The overwhelming successes achieved in the sciences concerned with physical events raised hopes that Newton's approach could be extended to other subjects, including psychology, moral philosophy and politics. There was disagreement concerning the relative significance of his procedures and his substantive conceptual apparatus--broadly, of empiricism and mechanics--but there was a widely shared sense that social inquiry ought to be structured by the example of physics.

For the proponents of the mechanical view, this meant that individual and social events, as much as planetary motion or terrestrial physics, were to be reduced to deterministic patterns of matter and motion. These ideas had received considerable attention in the previous century, and developed at least as much along lines laid down by Descartes as along those taken from Newton. But

Newton's association with the mechanical ideas added stature to existing efforts, and encouraged further development.

It is by no means clear that Newton would have supported these efforts. He repeatedly argued that science is limited to the provisional description of empirical patterns--that it is incapable of going beyond these patterns to a realistic explanation of events, mechanical or otherwise.[1] Moreover, he doubted that a purely mechanical view could be made consistent with the evidence, even in physics. On the contrary, he argued that both the large scale structure of the universe and the stability of its parts required the intervention of nonmaterial agents.[2]

But these skeptical attitudes toward metaphysical goals and the prevailing conceptual framework were not consistently held. His private correspondence and his published works were filled with explanations, and these were often mechanical. Gravity, to take a prominent example, was attributed first to the (mechanical) effects of an ethereal medium,[3] later to the actions of nonmaterial spirits,[4] then again to a somewhat more exotic ether.[5] He adopted the notion of primary and secondary qualities, arguing explicitly for a colorless, odorless world of atoms in motion.[6] He introduced notions of space, time and mass that could not conceivably be regarded as products of empiricism.[7] And he repeatedly raised general metaphysical issues in his scientific papers.[8] So, while he claimed to be an empiricist, seeking only descriptions of observed events, his work demonstrated implicit metaphysical commitments and explicit metaphysical interests.

He sometimes distinguished his speculative hypotheses from his scientific results, but he did not (and probably could not) fully separate the mechanical constituents of his outlook from the remainder. This inherent ambiguity was exacerbated by popularizers like Fontenelle, who placed particular emphasis upon the mechanical aspects of Newton's work, and reached far larger audiences than the great physicist himself did.[9] It is not surprising, then, that many of the enthusiasts for a "science of man" expected that science to be grounded in mechanics.

The more significant and lasting form of the eighteenth century effort to create social sciences, however, was built upon the empiricist interpretation of Newton's accomplishments. This interpretation was heavily influenced by the British empiricists, being largely structured in the first place by Locke's ideas, and subsequently responding to the arguments of Berkeley and Hume.[10] By midcentury, then, the interpretation had taken a severe form, which limited attention to empirical sources, but denied that much could be found there. Science seemed to be no more capable than other approaches of producing an understanding of events. Nor, of course, could it decide value questions. On the contrary, the only form of reliable knowledge seemed to be a tentative and approximate mapping of such regularities as might be found among phenomenal fragments. These maps would be limited to instrumental purposes, but other approaches seemed to offer even less. Thus political and social philosophers were encouraged to ask a new set of questions. Those traditional concerns which could not be addressed in the "Newtonian" manner simply could not be the objects of serious study.

This argument was repeated, with only minor modifications, in the influential works of D'Alembert, Saint-Simon, Comte, John Stuart Mill, Durkheim and others.[11] Thus, when serious social science programs were added to American universities in the last quarter of the nineteenth century, the empiricist interpretation was a well represented and prominently argued component of the intellectual environment.

In the beginning, American political science was only indirectly affected by these views. The first graduate programs were organized in accord with the Hegelian orientations of the very influential German departments, with some attention to British idealism.[12] The focus of attention was history, which was regarded as the progressive realization of the ideals of spirit in a set of objective cultural forms. Political science was concerned with the pattern of political ideas and institutions that had emerged from this process, or that could plausibly be expected in the (near) future.[13]

Even this approach to the study of social events had been affected by the intellectual standing of the natural sciences. The evolution of forms was said to consist of lawlike sequences of ideas and events that could be expressed as "scientific" generalizations.[14] The ultimate cause of these changes was thought to be spirit driving toward self-development, but proximate causes, like economic relations provided details about the instruments and procedures by which that development was actualized.[15] A factual base of "statistics" (state data summaries) and documents was studied comparatively to isolate the lawlike structures in the historical flow.[16] These analyses were not expected to yield final truths; rather tentative interpretations, "one-sided, colored, incomplete".[17] (In other words, the familiar "provisional generalizations", with a moderately increased sense of approximation and misdirection, to accord with the Hegelian emphasis upon ideational reaction.) This "empirical" and "inductive" research was sharply distinguished from prescientific forms of political analysis, in particular the deductive natural rights theories.[18]

The period between the establishment of the American departments and World War One witnessed two fundamental changes in this original disciplinary viewpoint. There was an emphasis upon more "realistic" data sources, and there was a drift away from the Hegelian explanatory framework. As early as 1885, Woodrow Wilson (under the influence of James Bryce and M. Ostrogorsky)[19] questioned the value of formal documents as sources of "empirical" information. He accepted the remainder of the prevailing comparative-historical approach, but insisted that real events and processes were almost always unlike those described in the constitutions and other formal documents that provided the factual base for political research. The proper starting point had to be real events rather than written materials--political scientists would have to do their research in the field.[20]

In 1888, James Bryce published The American Commonwealth--the first important research within this "realist" framework.[21] It was based upon interviews and observations, from which Bryce had constructed a variety of small-scaled generalizations and suggestive cross-national comparisons. The book received universal acclaim, and remained a realist exemplar for decades.

Others quickly added their support; indeed, there seems to have been no serious debate about this redefinition of properly relevant data. The turn of the century literature is filled with positive references, like this one from W. F. Willoughby:

> The period when political science concerned itself almost exclusively with questions relating to the political character and constitution of public bodies, happily, has passed. Intense activity is now being manifested in all matters having to do with the *actual* administration of government affairs.(22)

At the same time, references to more traditional German approaches were increasingly negative:

> Unless the study of the strictly legal side of constitutional law be informed and enlivened by constant reference to the actual working of institutions and their transformations under the influence of historical forces, it is certain to eventually degenerate into the veriest scholastic emptiness. . . . There can be no doubt that a Bryce's *American Commonwealth* for Germany would be a great boon.(23)

> Dr. Buchi's statements of fact seem to be exact, and his inferences cautious, but there is a mechanical quality about the work--a failure to penetrate beyond the official record.(24)

The declining regard for German texts is also indicated by the changing pattern of books reviewed in the journals. In 1886 about thirty-five percent were of European origin (the bulk of them German), but by 1920 this had declined to less than five percent.[25]

This was only partly due to dissatisfaction with the traditional data sources. It reflected, in addition, a waning interest in the Hegelian organizing framework. Those factors which had previously been assigned an intermediate, indicative role, as descriptions of the way spirit worked, were given a more final character. Economic explanations had been prominent in the Hegelian framework, and continued to receive attention. But the increasingly dominant explanatory concepts were psychological, as attention shifted from the interaction of institutional forms to the activities of individual actors.[26]

The most influential (or, at any rate, the most frequently cited) proponent of this shift was Graham Wallas. His _Human Nature in Politics_ emphasized the instinctive and irrational aspects of human behavior as major causes of political events, a position which was repeatedly praised, and said to be widely held.[27]

There was very little published resistance to these changes. It was overwhelmingly agreed that "realistic" data ought to be studied comparatively to produce general explanations of political events, with particular attention to individual motivation. But actual political science tended to be quite different.[28] Scholars continued to rely heavily on statutes, ordinances, charters and written constitutions, almost always accepting them at face value. Comparison was employed infrequently, and typically for illustrative rather than analytic purposes. The notion of "realism" degenerated into a concern for detail, as official structures and procedures were treated to ever more precise description. And there was virtually no attention to generalization. Publications were dominated by these legalistic descriptions, and by current events. Despite this, political scientists generally regarded themselves as scientific, and continued to distinguish modern political analysis from the deductive, rationalistic style of the natural rights theorists.[29]

A few political scientists seem to have had a more sophisticated grasp of the scientific process, and at least two raised the traditional methodological case explicitly. Both A. Lawrence Lowell and Arthur F. Bentley wanted political science to be the search for descriptive laws of political behavior.[30] Lowell had some hope that these laws might be quantitative, and conducted the discipline's first statistical analysis. As he saw it, the primary distinction between the natural and social sciences was the reliance upon experimental rather than ex post facto research designs.[31] Bentley made a similar argument, but conceived and stated it in an unusually severe form.[32] The analysis was to be strictly limited to observable "activities". The psychological explanations of the realists, as much as the Hegelian concepts that preceded them, he disallowed as intangible "soul stuff". But these views were combined with a mechanistic metaphysics of "forces" and "pressures" among interest groups that did not begin to conform to his methodological requirements. Charles A. Beard wrote a positive review of The Process of Government for the American Political Science Review, but Bentley does not seem to have been widely read or discussed.[33] Lowell was read and discussed (indeed, he was elected President of the American Political Science Association), but he was not imitated.

Only after 1920 was there widespread interest in a science of politics. The increased attention seems to have been very largely due to the efforts of Charles Merriam. In an influential article that appeared in the American Political Science Review of 1921, he urged his colleagues to trade their characteristic approach for one modeled on the natural sciences.[34] There, and in many subsequent publications, he made the familiar arguments. Science had vastly increased man's control of the forces of nature, releasing human energies from manual toil, improving health, and revolutionizing the nature of human intercourse by transforming the instruments of communication and transportation.[35] But the study of politics had produced very little useful knowledge. Physical and biological sciences had progressed and solved problems, their own field had mainly generated disagreements.[36]

If social scientists would embrace the mode of inquiry perfected by natural scientists, they could reasonably expect a drastic reduction in social conflict:

"Probably war can be prevented, revolutions reduced to remote possibilities and maladjustments vastly reduced in number and intensity."[37] In addition, they could expect the elimination of the standard political abuses (graft, spoils, exploitation and neglect), an increase in productivity and good feeling, and the release of human potential through the conscious control of human evolution.[38]

These were not presented as mere possibilities. World conditions made it imperative that "intelligence" be substituted for "hatred, prejudice and passion".[39] Science had to be applied to social subjects to balance the power that had been released in other realms:

> Have we not reached the time when it is necessary to adjust and adapt more intelligently, to apply the categories of science to the vastly important forces of social and political control? . . . at any time out of the depths of ignorance and hatred may emerge world war, anarchy, industrial and political revolution, recurring discontent and distress. What advantage shall we reap if science conquers the whole world except the world's government, and then turns its titanic forces over to a government of ignorance and prejudice, with laboratory science in the hands of jungle governors?(40)

In all of this, Merriam reads very much like Comte. The shift to positive science was expected to bring a world of peace, prosperity, efficiency and freedom.

He had very little to say about the nature of these powerful scientific procedures, and it seems clear that he had no serious grasp of the issues. He expected the comparative-historical approach to be the source of hypotheses for more rigorous analysis, which he was sure would be quantitative and empirical. But he gave no details, and conducted no research from which such ideas might be abstracted. There are occasional references to Karl Pearson, but these were not taken up for serious analysis. He really only directed his energies toward methodological issues when confronted with specific critical arguments, in which cases he almost always agreed that there was a problem, then reasserted his optimism. It is not surprising that contemporaries like Robert T. Crane and William F. Ogburn accused him of encouraging a position he did not understand.[41]

One must turn to other advocates of a science of politics, then, to find coherent treatments of the required methods. The most comprehensive (and most influential) of these were written by George E. G. Catlin and Stuart A. Rice, both of whom relied heavily upon Karl Pearson, a positivist with close ties to Hume and Mach.[42] Accordingly, he held that scientific theory is purely instrumental--a series of symbolic summaries of phenomenal patterns, which employ (and, indeed, require) freely invented abstractions. (Pearson called them "useful fictions".) Theory constituents like "matter" and "force", and organizing assumptions like uniformity and determinism, were employed because they supported prediction and manipulation, not because they mirrored reality. The goal is utility, and this was to be had from an economical set of symbolic summaries.

Although he was not, strictly speaking, a political scientist, Stuart Rice was probably the most sophisticated and convincing proponent of a science of politics. He produced a diverse body of writings that ranged from general philosophical statements to practical research. Indeed, he was the only major advocate who seems to have understood the research process in any very detailed manner. Following Pearson, he embraced the fact-value split, holding that value questions cannot be decided scientifically, and that this had been a fundamental source of trouble in the past.[43] He agreed that the key to science was method, and believed that essentially the same method could be applied to all factual subjects, including politics.[44] He emphasized that the goal was a set of instrumental summaries (based upon fictional abstractions and organizing concepts, including the notion of lawlike determinism) that could be used to ameliorate social distress.[45] And he added the movement's most articulate consideration of the role of quantification. He did not think that all science had to be quantitative (indeed, he feared that important subjects would be neglected if the demand were pressed), but he emphasized the great value of numerical expression.

Most importantly, he thought, quantitative forms would minimize bias in the collection and analysis of data.[46]

George Catlin made a similar argument (rather less well), in which he proposed a specific set of useful fictions for political science. He was particularly enthusiastic about the notion of "political man", an analog of "economic man", which was defined entirely in terms of the will to dominate.[47] Employing this and other working fictions (including the determinism and uniformity that had worked so well in physics), the political scientist was to construct simplified summaries of political events, which could be tested by resort to quantitative, empirical data. Those hypotheses that maintained predictive usefulness across many tests would eventually be accepted as laws of political behavior.

The diversity of viewpoints (and the inconsistency within viewpoints) make it impossible to reduce the science of politics idea to a coherent philosophical position. The general outlines, however, are fairly clear. The goal was utility--the capacity to ameliorate, or solve, pressing social problems. The implied formal goal was a set of predictive laws of political behavior and events. The methods expected to yield these laws consisted of (preferably) quantitative, and sometimes "experimental", tests of hypothesized generalizations, conducted in a value free manner. (Only factual hypotheses were to be considered; value preferences could not be justified scientifically.) Those who followed Pearson wanted to build useful, though fictional, accounts, employing some of the organizing concepts that had proved valuable in the physical sciences. Others (for example, William B. Munro and Merriam) probably expected the resulting theory to be realistic. Within this framework, a considerable variety of explanatory notions were entertained, with the greatest prominence given to psychology.[48]

The critics responded by asking whether social events are available to these methods. The controversy ranged across many superficially different questions, but there were only five distinct epistemological issues: (1) Are social events available to quantification? (2) Are social events too unique to support interesting generalizations? (3) Can social experiments be conducted? (4) Are social events too complex to be comprehended as natural events are? And (5) is the notion of social law compatible with free will?

Isolated comments on one (or occasionally more) of these points are sprinkled through the literature of the period.[49] But easily the most comprehensive analysis was undertaken by William Yandell Elliott.[50] He addressed all five of the critical concerns, in ways that just about exhaust the negative views expressed by his contemporaries. The following examination of his comments and the responses of major proponents may therefore be taken as a reasonably accurate view of the controversy.

The issue of measurement was not thoroughly examined. The critics approached this (and virtually every other) problem as though the impossibility of accomplishment was fairly obvious. In this case, Elliott simply noted that the interesting social concepts are not quantitative. The proponents seemed to realize that existing concepts might be more or less successfully translated into other concepts that are quantitative (i.e. "operationalized"), or that new concepts of a quantitative form might be invented. Merriam was quite willing to agree that this presented difficulties, and was unsure of the outcome. He also warned of moving too easily and uncritically into particular quantitative forms.[51] Rice apparently took the possibility of useful quantitative forms for granted, but emphasized that considerable time and effort would be required. In any case, he joined Merriam in warning that the fashionable insistence upon the value of quantitative methods could easily lead to poorly informed and inadequate attempts. He was also concerned that significant issues would be ignored if they could not be explored in the preferred quantitative framework.[52] In short, the critics assumed that few, if any, interesting problems could be approached quantitatively, and the proponents assumed that they could, sooner or later. No prominent spokesman, save Merriam, was willing to say that it was really an empirical matter--that one would have to wait and see whether convincing measures could be found.

The problem of the uniqueness of social events also seemed fairly clear to the critics: generalizations would be impossible, if they were not trivial, because wars, organizations or elections have much less in common than electrons do. Natural science events were thought to fall more easily and convincingly into categories and types. Rice responded to this point as follows:

> In any final and absolute sense of the term there are no such things as repetition or identity in the perceptual world.
>
> When the chemist two times in succession performs the experiment . . . he is not repeating the same event, but rather bringing about two similar events.
>
> It must, of course, be admitted that in degree of exactitude, the advantages are usually in favor of the "natural" sciences.
>
> . . . things and events which are sufficiently alike for the pragmatic purpose are classified together. The ability of science to find "repetitions" of events, then, depends upon the "scientific fictions" that we have been discussing. It depends in particular upon suitable classification, a wholly pragmatic process.(53)

On this view, one may generalize if one manages to find or invent categories that contain enough cases. It is only the use of narrow categories that leaves one with a sense of "unique" events. This position, taken directly from Karl Pearson, was apparently not addressed by the critics, who wrote as though the categories employed for social analysis were given.

The issue of experiments probably received more attention than any other. Critics like Corwin and Elliott simply asserted that experiments are impossible, relying heavily on the idea of unique events to make their case.[54] Merriam granted that this was probably true.[55] Rice seemed to disagree, but the conflict was due entirely to his decision to include ex post facto research designs with statistical controls in his definition of "experiment". He was well aware of the problems here, noting that political scientists would probably have to work with uncontrolled, or inadequately controlled, correlations.[56] In short, he did not think that experiments of the manipulative sort would be possible. Catlin also seemed to disagree. Indeed, he called himself a "political experimental scientist" and argued that "there is no more inherent impossibility in experimenting with men than with pigs".[57] But, like Rice, he employed a rather broad definition of "experiment":

> . . . if I observe the amount of intoxication in ten overcrowded areas and in ten adequately housed areas of the same general type, observe the changes in the statistics of intoxication after clearing a congested area (perhaps cleared at my suggestion),

> and the change when an area . . . becomes more crowded, I have conditions which are experimental in the same sense as a piece of chemical research may be called experimental.(58)

The real issue was the control of confounding variables, so that the hypothesized relationship could be brought into relief. Among the prominent writers, only Rice seems to have understood this. Unfortunately, he called ex post facto research designs with statistical controls "experiments". Catlin added an "experimental" procedure that shared only the superficial structure of real experiments, while abandoning their most central feature. And the critics attacked the textbook definition of experiments, rather than the kinds actually being suggested. In short, energies were wasted on a series of semantic confusions; the issue of controls got almost no attention.

In contrast, the argument about complexity was straightforward. Elliott was content to refer to the "hopeless complexity" of social events, implying that rigorous treatment would be equally hopeless. Merriam agreed that social events are much more complex than physical ones, but expressed confidence that painstaking research would nonetheless bring the patterns into relief.[59] Those who followed Pearson simply pointed out that a realistic picture of events was not the goal of scientific research. They were "entitled to simplify", as Catlin put it, so long as the resulting laws had utility.[60]

Again, regarding the compatibility of laws and free will, the critics were satisfied to identify the conflict. Catlin and Rice responded that scientists could properly use any "fictions" that proved useful. They did not deny conscious purpose; they simply thought deterministic theory ought to be tried, in view of its great success in other scientific fields.[61] William B. Munro, speaking for those more inclined to realism, emphasized the consistency of human behavior, despite free will:

> . . . human nature, after all, is a relatively stabilized and dependable thing. Were it not so, our social order would be without permanence. There is an underlying consistency in human conduct, as every psychologist knows, and the evidences of this consistency are to be found in the animals of all nations, recurring age after age. Men in the mass everywhere respond to the same passions and desires in much the same way. "They are stirred by the same motives" says Lord Bryce, "and think upon similar lines."(62)

Yet another position was taken by Harry Elmore Barnes:

> We now recognize that every human thought or act is strictly determined by a long process of antecedents, including our physicochemical nature, our biological heredity, our endocrinal and metabolic processes, and our personal experiences in human association from the time of parturition to the moment of the particular act or thought. There is not the slightest iota of choice allowed to any individual in any act or thought from birth to the grave. If better and saner types of conduct are to be achieved, this must be brought about by giving the individual a better set of experiences through heredity, education and association. What these new guiding criteria for conduct shall be can only be determined by the most earnest and prolonged collaboration of

> natural and social scientists, each a specialist, and all dominated by the aim of social betterment. (63)

It is not clear how a fully determined being would be able to define "saner and better", and give new direction to these processes. In any case, this does not seem to have been a widely held view.

An examination of these five epistemological issues suggests two fairly clear conclusions. First, the critics' impossibility arguments are unconvincing. They demonstrated no very serious grasp of the philosophical dimensions of these problems, nor of natural science history or procedure. It was undoubtedly very easy for proponents to ignore their protests. But, if their arguments settled nothing, they did raise interesting and important issues. Measurement was problematic in practice, even if one allowed the proponents' positions. Similarly, it could have been granted that uniqueness is a product of concept choice, without agreeing that broad scale concepts would be found. The emphasis upon experiments should have been recognized as misleading from the example of astronomy, but the related issue of controls was central and very problematic in political studies. Although simplification of the apparent complexity of social events may be a legitimate strategy (depending upon one's goals), it was not clear that effective simplifications would be discovered. And it was far from evident that deterministic patterns or interesting statistical tendencies would actually be found in human behavior. In short, it was not at all obvious whether a science of politics would be possible, or interesting. Positions depended overwhelmingly upon a priori assumptions and beliefs rather than upon evidence.[64]

American political science was profoundly affected by the science of politics movement. Shortly after Merriam's first paper, the Executive Council of the American Political Science Association appointed him chairman of a new Committee on Political Research.[65] Its purpose was to review the scope and method of research in the field and to recommend reforms. In 1923, the committee urged the establishment of a Social Science Research Council to aid in the development of research techniques, promote the required new teaching skills, direct research efforts in accord with social needs, and secure funding. (This was established with Rockefeller Foundation support later in 1923.) They also recommended a permanent Committee on Political Research, and changes in association meetings to reflect methodological concerns.[66] There followed three National Conferences on the Science of Politics (1923, 1924 and 1925), which emphasized measurement techniques, data sources, methods of analysis, and applications to particular social problems.[67]

Starting in 1925, a series of American Political Science Association roundtables were also devoted to methods.[68]

Studies influenced by the new style started to appear in 1924. Early examples included A. N. Holcombe's _Political Parties Today_[69], Stuart Rice's _Farmers and Fieldworkers in American Politics_[70], and Merriam's joint effort with H. G. Gosnell, _Nonvoting: Causes and Methods of Control._[71] Holcombe's review of this last volume indicates something of the optimism and sense of purpose which early proponents of the scientific approach shared (as well as a rather weak grasp of the epistemological problems):

> Too long they have contented themselves with "academic" arguments founded on conjecture and hearsay . . . .
>
> Now come Professor Merriam and Dr. Gosnell with their study . . . . They give us the facts.
>
> They really know what reasons weighed most heavily in the minds of nonvoters at that particular election.
>
> We need to check the results of this investigation by similar studies in other places under different other conditions, but on the basis of this first experiment at Chicago, it ought to be possible to make a series of investigations by means of which a political scientist could pronounce a final judgement on such expedients as compulsory voting with all the assurance of a chemist proving the quality of a new paint remover, or a biologist testing a germicide.(72)

In 1925, the first "scientific" articles appeared in the _American Political Science Review._[73]

The subsequent flow of books and articles included a considerable methodological literature.[74] Measurement was the most frequently addressed issue (indeed, it comprised the great bulk of the total methodological output), with particular attention given to public opinion polls. Statistical procedures also attracted some attention. But the remainder of the research process was neglected.[75]

As the 1930s passed, interest in a science of politics seems to have thinned out. World conditions and the unnerving irrationality and political ignorance that had been encountered in survey work have been credited with diverting attention to other topics (in particular to economic problems and a defence of democracy).[76] It is difficult to say exactly how many political scientists were influenced by the movement, or how much. It is worth noting, however, that Merriam's University of Chicago department produced more than ten percent of the discipline's PhDs during the 1930s, and a very large proportion of the influentials-to-be.[77] (Lasswell, Gosnell, Key, Wright, White, Woody, Beyle, Mott, Overacker, Almond, Pritchett, Simon, Leiserson and Truman were all associated with the Chicago department.)[78] One analyst notes a shift, even in the "nonscientific" research of the period, from institutional analysis to policy and process, with a much greater emphasis upon observation.[79] Still, as the 1930s passed, explicit interest in a science of politics on the model of physics seems to have declined.

After 1940, however, the arguments began to appear again. A series of articles charged that political studies had been seriously biased by implicit value commitments.[80] These charges sparked a broad debate concerning the proper role of values, which quickly boiled down to a confrontation between the adherents of "traditional" and "scientific" viewpoints. A series of short papers written by William F. Whyte and John H. Hallowell contains the essential features of the conflict.[81] Whyte complained that political scientists were preoccupied with political philosophy, public administration, and international law "whose connections with practical politics are more or less remote."[82] Not only was the research misdirected, it was permeated with undefended, and often hidden, value biases. Political scientists wasted too much time complaining about "corruption"--the failure of institutions to conform to a priori theory. Instead, they should concentrate on finding out how the institutions actually work. John H. Hallowell replied that the value neutrality of positivist political science was responsible for the moral disasters of the 1930s; one did not need research methods, he argued, so much as convictions.[83] Others added that the supposedly "scientific" studies of the Chicago school had been conducted in a naive manner, without benefit of standard measures or unifying theory.[84]

These ideas surfaced during a period of considerable discontent in the discipline. Political scientists had been unable to account for the rise of fascism or communism. Then, during the war, they had learned that their skills were not highly valued in government. Those who had found positions in the public service had been forced to admit that this was probably justified. And after the war, their contact with nonwestern, nonindustrial systems (through foreign assistance programs) had made the gap between the discipline's generally accepted knowledge and political realities even more apparent. Increasingly, the fear was expressed that political science had fallen behind the other social sciences.[85]

Other developments also encouraged political scientists to reconsider the discipline's direction. The migration of European social scientists to the United States brought new viewpoints, including the work of Max Weber and the logical positivists.[86] And research funds were increasingly controlled by organizations with "scientific" preferences. These included the Social Science Research Council's Committee on Political Behavior, the Ford Foundation's Behavioral Science Program, and the National Science Foundation.[87]

It is not surprising, then, that a number of works from the "scientific" mold were produced in the years immediately following the war, including Simon, Administrative Behavior(1947); Pritchett, The Roosevelt Court (1948); Key, Southern Politics(1949); Lasswell and Kaplan, Power and Society(1950); Simon, Smithberg and Thompson, Public Administration(1950); and Truman, The Governmental Process(1951).[88] It is also indicative of the times that Arthur F. Bentley's The Process of Government, which had been ignored in 1908, and had not been reviewed by any political science journal upon its reissue in 1935, was welcomed as one of the most important books on government ever written in America.[89]

An amorphous "behavioralist" movement gathered momentum very quickly. Merriam, with characteristic optimism, wrote that the scientific viewpoint was finally sweeping the field.[90] But this does not seem to have been the case. A survey of political science departments in the spring of 1950 found that very little research met even the minimal requirements for a science of politics program.[91] There was the usual gap between the views of the most visible political scientists and the mass of practitioners. But, even among the influentials, much of the interest in a behavioral political science seems to have taken less rigorous forms. A later survey found a stronger, but still far from overwhelming, trend.[92] Indeed, as late as the mid 1960s, Somit and

Tanenhaus found a discipline about equally divided between behavioral and alternative outlooks, with considerable diversity among the behavioral advocates.[93] In short, behavioral political science was (and is) a complex set of only partly overlapping viewpoints. The various efforts to analyze its central commitments have invariably (and correctly) been rejected by large numbers of adherents as irrelevant to their work.

At the same time, it is clear that a subset of behavioralists did consciously aim to return to the science of politics idea. The programmatic statements of David Truman, Robert Dahl, David Easton, and Heinz Eulau were much like those of Rice and Catlin. They embraced the fact-value distinction, made rigorous empirical methods the key to scientific success, emphasized reliance upon quantitative forms whenever possible, and sought general predictive theories of political events. Logical positivism displaced Karl Pearson's views, and there was less emphasis upon the rapid development of theory with which to attack social problems, but the basic structure of commitments and the rhetorical forms used to express and analyze them, were very similar.[94]

The one striking change concerned the status of the resulting theory. The emphasis upon instrumental theory which had played a central role in the programmatic statements of the 1920s gave way to a widespread sense that a science of politics was a superior means for building a realistic understanding of political events.

Some practitioners continued to argue for instrumental theory. George Lundberg was probably the most prominent proponent of the early behavioralist period, insisting across many books and articles that scientific theory would not tell social scientists how things are, rather how it is scientifically profitable to regard them. The point was utility, not realism.[95] And Eugene Meehan has been a prominent representative of the instrumental view more recently: ". . . it is now clear that science is not and has never been concerned with the nature of physical reality. Science says nothing about 'reality' . . . ."[96] The point was to construct a systematic knowledge of phenomenal patterns that would serve practical ends.[97]

But this has not been the prevailing view. Instead one finds a fairly consistent combination of positivist rhetoric and realist goals.[98] This has only rarely been explicitly defended. It seems that realism has been taken for granted, and that certain components of a less than fully grasped positivism have been grafted on. There is an unmistakable sense that "institutional" analysis and normative bias are being replaced by a tough-minded examination of the way things actually work. These views are close to the surface in a broad range of political science subjects and approaches, and they are associated with each of the influential programmatic statements which have been cited.[99]

Perhaps the most striking example of these views is found in the works of the positive political theorists. Their central theoretical commitment is to rational choice models. As this idea is shared with price theory, it is not surprising that many of the leading proponents have been economists, including Arrow, Black, Buchanan, Downs, Niskanen, Olson, Plott, Sen and Tullock.[100] The basic idea is that individuals rationally seek to maximize their utility. Formal procedures typical of economic analysis are employed to reconstruct political situations from this standpoint. The plan is to verify these theoretical structures by deducing nonobvious consequences that can be tested empirically. As the predictions are confirmed (and they plainly are expected to be), confidence in the predictive and explanatory powers of the apparatus are said to be justified. In short, they reconstruct political processes in accord with what is admitted to be an unrealistic model of human decision making, then check to see whether implications of these reconstructions match up with empirical events. No orientation within political science seems more available to an instrumental interpretation of theory. Nonetheless, prominent representatives have insisted upon a form of realism.

In the first application of this framework to political events, Anthony Downs held that theoretical models should be tested primarily by the accuracy of their predictions rather than by the reality of their assumptions.[101] But he insisted that a model which performs well predictively is more than an instrument: the conformity of observed behavior with expectations derived from the model should be taken to indicate that the model accurately describes the involved behavior--that men _are_ rational in the sense employed in the model.[102]

W. Hayward Rogers challenged him to explain how a set of admittedly unrealistic assumptions (concerning "rational man") could conceivably be the basis for a realistic theory of behavior.[103] Downs' response was dominated (as his original argument had been) by other concerns.[104] He emphasized the _form_ that scientific theory ought to have, and the priority of theory in the scientific process much more than the _status_ of a confirmed theoretical structure. These issues were being debated in the literature of the period, and his attention to them is, in that sense, understandable. But the emphasis placed upon them at the expense of the point at issue seems rather odd. The most reasonable interpretation seems to be that Downs took realism as much for granted as Rogers did. He was sure that science provided a realistic understanding of events; the real issue was whether a science of politics was possible and what form it ought to have. Because the process he had followed seemed to him to be a close analog of the process that had yielded such remarkable results in physics, he was prepared to declare his reconstruction a "tentative truth"--a "correct" model that lent support to the idea that man actually _is_ rational. At worst, his reconstruction was incomplete.[105]

Other political scientists operating within the rational choice framework have held similar views. Thus Riker and Zavoina have argued that the study of formal games yields a realistic knowledge of the way political decisions are made, and the degree to which they actually are based upon rational utility maximization.[106] And Davis, Hinich and Ordeshook distinguished prediction devices from realistic, if incomplete, models, arguing that their mathematical model of the electoral process ought to be understood as an example of the latter.[107] The uppermost concern has continued to be form. Riker and Ordeshook wrote an entire text on rational choice models, with fairly detailed attention to the many possibilities for application, without giving any significant attention to the status of the results.[108] And Riker wrote a more recent article on the future of the science of politics idea that overwhelmingly emphasizes form: science means well verified, deductive explanatory structures. These have, contrary to certain critical suspicions, actually been constructed for some areas of human behavior (price theory), and political science is pregnant with similar opportunities. One ought to do away with "angels and demons" (most notably Hegelian or Marxist historicism and teleological functionalism) in favor of a rigorous application of the conceptual apparatus that has demonstrated its value for scientific social analysis (rational choice theory). He notes briefly that the proposed system of axioms is incomplete, and that axiom sets that satisfy formal requirements may later be abandoned, but insists that such theories, nonetheless, contribute to understanding. Having thus opened the door to a discussion of the status of theory and begged a question or two concerning just how, or in what sense "understanding" follows from such theory, he returns to his discussion of form.[109] Again, the formal modelers (including the rational choice theorists) are the political scientists that seem most likely to understand and embrace instrumental interpretations of scientific theory. They recognize the a priori nature of theory, and emphasize that only a few (and frequently rather distant) implications of the theoretical system are verified empirically. Still, even these political scientists have tended to hold (if only implicitly in many cases) realistic views of theory.[110]

The critics of the science of politics idea have expanded their list of objections in accord with this emphasis upon realism. The traditional issues--those concerned with the availability of political events to scientific treatment--have been joined by questions concerning the status of scientific results. It has been held that such theory as might be constructed--in the face of the technical challenges and in ignorance of the close and subtle relations between facts and values--would amount to "pretentious collections of trivia".[111] There has been concern about the degree to which facts could be expected to carry bias, hence the degree to which theory based upon them could be expected to have that bias. The most influential form of this complaint followed Mannheim, who argued that perception and thought (hence theorizing and the process of confirmation) are colored by social interest.[112] The only escape from these biases (and it is no more than a partial escape) is through the comparative study of different points of view. This argument was joined with fears that a political science orthodoxy had been established, in which supposedly neutral research concealed conservative political preferences.[113] A related argument emphasized broad epistemological, rather than purely political or social sources of bias. Drawing from a variety of fields, including analyses of language (particularly as addressed by Nietzsche, Whorf and

Wittgenstein) and nonpositivist reconstructions of science (Hanson, Toulmin, Scriven and, most prominently, Kuhn) these critics emphasized the degree to which the positivist roots of the science of politics idea needed rethinking.[114] Another form of this argument construed Kuhn to mean that scientific theory amounts to an arbitrary product of social agreement--established by fiat and maintained by political pressure and sanctions.[115] Needless to say, this form of the epistemological objection was often found in the company of the argument concerning the role of social interest.

Yet another critical attack has centered about the notion of verstehen. This radically different approach to social explanation first appeared in the works of Wilhelm Dilthey. Writing in explicit opposition to the methodological preoccupations of Auguste Comte and John Stuart Mill, Dilthey argued that the external mode of explanation applied to physical events is incompatible with the study of human action. As the latter depends upon ideas and viewpoints--the meanings of activities to the actors--it cannot be understood without recourse to those meanings. The subjective status of actors thus became the focus of social research, to be discovered through a process of empathic identification.[116] Dilthey's ideas have been subjected to a great many interpretations, including efforts to bridge the gap between the "subjective" and "positive" approaches. These include the influential works of Max Weber, Charles Horton Cooley, George Herbert Mead, Robert M. MacIver, Theodore Abel, Alfred Schutz, A. R. Louch, Peter Berger, Thomas Luckmann and Harold Garfinkel.[117] The very considerable attention given to this topic in the sociological literature has not been equalled in political science, but objections to a purely external approach have occasionally been raised.[118]

It has been held, then, that political events cannot be studied the way physics can, and that insofar as they are, the results will be trivial or misleading. These arguments have not had much effect upon proponents of the science of politics program. It is generally held that the technical problems are manageable, that scientific method filters off the effects of personal bias, and that the form of explanation that works in physics <u>is</u> appropriate for political science.[119]

If critics assert that measures cannot be made convincing, proponents assert that they can be. If it is held that social explanation requires verstehen, it is countered that judging positivist efforts from the standpoint of verstehen is no more legitimate than judging verstehen from the standpoint of positivism, or that positivism can point to physics, but verstehen has no successful reference to pin its hopes on. Proponents and critics each have interesting things to say, but neither has offered evidence that the other finds persuasive. There does seem to be one source of evidence that can help, however, and that is the subject of the section to come.

Notes to Chapter II

[1]See Edwin A. Burtt, The Metaphysical Foundations of Modern Physical Science (revised edition; Garden City, New York: Doubleday Anchor Books, 1932), pp. 212, 217, 226. See also Alexandre Koyre, Newtonian Studies (Cambridge: Harvard University Press, 1945), p. 177.

[2]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 258-261; Alexandre Koyre, From the Closed World to the Infinite Universe (Baltimore: Johns Hopkins Press, 1957), pp. 206-210.

[3]A letter to Robert Boyle, 1679, cited in Koyre, Newtonian Studies, p. 149.

[4]A Letter to Richard Bentley, 1692, cited in Koyre, From the Closed World to the Infinite Universe, p. 179.

[5]Koyre, Newtonian Studies, p. 161.

[6]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 233-234, 239.

[7]Ibid., pp. 246-255.

[8]Koyre, From the Closed World to the Infinite Universe, pp. 206-210.

[9]See Ernst Cassirer, The Philosophy of the Enlightenment, trans, by Fritz C. A. Koelln and James P. Pettegrove (Boston: Beacon Press, 1951), p. 65; Herbert Butterfield, The Origins of Modern Science 1300-1800 (revised edition; New York: Free Press, 1957), pp. 177-185.

[10]John Locke, An Essay Concerning Human Under standing; George Berkeley, Principles of Human Knowledge; David Hume, An Enquiry Concerning the Human Understanding.

[11]D'Alembert, Elements of Philosophy; Saint-Simon, Essay on the Science of Man; Comte, Course of Positive Philosophy; John Stuart Mill, A System of Logic; Durkheim, The Rules of Sociological Method.

[12]Walter James Shepard, "Political Science" in Harry Elmore Barnes, ed., The History and Prospects of the Social Sciences (New York: Alfred A. Knopf, 1925), p. 423; Raymond Gettell, History of American Political

Thought (New York: The Century Company, 1928), p. 616. The first department was established by John W. Burgess at Columbia University in 1880. Six others (Michigan, Cornell, Pennsylvania, Yale, Harvard and Princeton) had been added by 1885. See Albert Somit and Joseph Tanenhaus, The Development of American Political Science: From Burgess to Behavioralism (Boston: Allyn and Bacon, 1967), p. 17; and Richard J. Storr, The Beginnings of Graduate Education in America (New York: American Press, 1953).

[13]John W. Burgess, "Political Science and History," Annual Report of the American Historical Association for the Year 1896 (Washington, D. C., 1897), pp. 203-211.

[14]Somit and Tanenhaus, The Development of American Political Science, pp. 19-21; see also Munro Smith, "Introduction: The Domain of Political Science," Political Science Quarterly, I (1886), pp. 1-8.

[15]Shepard, "Political Science" in Barnes, ed., The History and Prospects of the Social Sciences, pp. 422-425; Smith, "Introduction: The Domain of Political Science," p. 7.

[16]Smith, "Introduction: The Domain of Political Science," p. 4; Gettell, History of American Political Thought, p. 616; Somit and Tanenhaus, The Development of American Political Science, p. 8.

[17]Burgess, "Political Science and History".

[18]Somit and Tanenhaus, The Development of American Political Science, pp. 19-21.

[19]Gettell, History of American Political Thought, p. 610. Martin Landau argues that he was also under the influence of Walter Bagehot. See "On the Use of Metaphor in Political Analysis" in Landau, Political Theory and Political Science (New York: Macmillan Company, 1972), p. 92.

[20]Woodrow Wilson, Congressional Government (New York: World Publishing Company, 1885).

[21]James Bryce, The American Commonwealth (New York: Macmillan Company, 1893).

[22]W. F. Willoughby, Review of Henry James Ford, The Cost of Our National Government 1910, American Poli-

tical Science Review, V (1911), p. 143.

[23]Walter James Shepard, Review of Ludwig Dambitsch Amtsrichter, Die Verfassung des Deutschen Reichs Mit Erlauterungen, American Political Science Review, VI (1912), p. 122.

[24]Jesse S. Reeves, Review of Robert Buchi, Die Geschichte Der Pan-Ameribanishem Bewegungmit Besonderer Berucksichtigung Ihrer Volker Rechtlichen Bedeutung, American Political Science Review, IX (1915), p. 791.

[25]Somit and Tanenhaus, The Development of American Political Science, p. 62.

[26]Gettell, History of American Political Thought, pp. 610, 618.

[27]Graham Wallas, Human Nature in Politics (Lincoln, Nebraska: University of Nebraska Press, 1962).

[28]An(accidentally) ironic use of the realists' favorite word.

[29]Morton White has commented upon a group of the period's leading intellectuals (including Charles A. Beard) as follows: "So far as I can see, they have never said anything about the logic of scientific procedure which has not been either elementary or obscure. They cannot be taken seriously in their observations about the methods of physics or mathematics, and when they come to talk about the nature of science in general they are reduced to vagueness or dependence on dubious second hand information. . . . In spite of this vagueness, however, they respected the method of science, admired its results and wanted their own disciplines to be closely associated with it." (Morton White, Social Thought in America: The Revolt Against Formalism (Boston: Beacon Press, 1947), pp. 239-240.)

[30]A. Lawrence Lowell, "Physiology of Politics," American Political Science Review, IV (1910), pp. 6-7.

[31]Lowell, "Oscillations in Politics," Annals of The American Academy of Political and Social Science, XII (1898), pp. 69-97.

[32]Arthur F. Bentley, The Process of Government: A Study in Political Pressures (Cambridge: Belknap Press, 1967).

[33]*American Political Science Review*, III (1909), pp. 739-741.

[34]Charles Merriam, "The Present State of the Study of Politics," *American Political Science Review*, XV (1921), pp. 173-185.

[35]Charles Merriam, *New Aspects of Politics* (third edition; Chicago: University of Chicago Press, 1970), p. 91.

[36]Merriam, "The Present State of the Study of Politics," p. 177.

[37]Merriam, *New Aspects of Politics*, p. 53.

[38]*Ibid.*, p. 54.

[39]Merriam, "Political Research," *American Political Science Review*, XVI (1922), pp. 315-321; see also *New Aspects of Politics*, p. 101.

[40]Merriam, *New Aspects of Politics*, p. 55; see Harry Elmore Barnes, ed., *History and Prospects of the Social Sciences*, p. xvi for similar views.

[41]See Barry D. Karl, Forward to Merriam, *New Aspects of Politics*.

[42]Karl Pearson, *The Grammar of Science*.

[43]Stuart A. Rice, *Quantitative Methods in Politics* (New York: Alfred A. Knopf, 1928), pp. 14-19.

[44]*Ibid.*, pp. 21-22.

[45]*Ibid.*, p. 24.

[46]*Ibid.*, pp. 3-8, 19.

[47]George E. G. Catlin, *The Science and Method of Politics* (Hamden, Conn.: Archon Books, 1964).

[48]This latter commitment posed problems, as psychology was increasingly divided into conflicting schools (physiological, behaviorist, psychoanalytic, etc.). The philosophical realism at issue here (and henceforth) is to be distinguished from the concern for "realistic" data sources in turn of the century political science.

[49]See, for example, Harold Laski, *The Dangers of*

Obedience (New York: Johnson Reprint Corp., 1968); Walter J. Shepard, "Political Science" in Harry Elmore Barnes, The History and Prospects of the Social Sciences; James Hart, "Political Science and Rural Government," American Political Science Review, XIX (1925); H. Mark Jacobsen, "Evaluating State Administrative Structure--The Fallacy of the Statistical Approach," American Political Science Review, XXII (1928); Charles A. Beard, "Time, Technology and the Creative Spirit in Political Science, " American Political Science Review, XXI (1927).

50"The Possibility of a Science of Politics: With Special Attention to the Methods Suggested by William B. Munro and George E. G. Catlin" in Stuart A. Rice, ed., Methods in Social Science: A Case Book, the work of the Committee on Scientific Method in the Social Sciences of the Social Sciences Research Council (Chicago: University of Chicago Press, 1931), pp. 70-92. See also Elliott, The Pragmatic Revolt in Politics (New York: H. Fertig, 1968).

51Merriam, New Aspects of Politics, chapter 3 and pp. 201-207; Merriam, "Recent Advances in Political Methods," American Political Science Review, XVII (1923), pp. 274-312; Merriam, "The Present State of the Study of Politics," pp. 179-180.

52Rice, Quantitative Methods in Politics, pp. 3-4.

53Ibid., pp. 24-28, 44-45.

54Edwin S. Corwin, "The Democratic Dogma and the Future of Political Science," American Political Science Review, XXIII (1929), p. 588.

55Merriam, New Aspects of Politics, chapter 3.

56Rice, Quantitative Methods in Politics, pp. 30-33.

57Quoted in Somit and Tanenhaus, The Development of American Political Science, p. 114.

58Catlin, The Science and Method of Politics, p. 114.

59Merriam, New Aspects of Politics, pp. 207-208.

60Catlin, The Science and Method of Politics, p. 125.

[61]*Ibid.*, p. 109.

[62]William B. Munro, *The Invisible Government* (New York: Macmillan Company, 1928), p. 36.

[63]Barnes, ed., *The History and Prospects of the Social Sciences*, p. xv.

[64]A number of critics expressed the fear that policy analysis and political philosophy would be dismissed as unscientific, hence unworthy of attention. This argument reached mature form only somewhat later. It will be taken up in connection with behavioralism. See, for example, Corwin, "The Democratic Dogma and the Future of Political Science"; Elliott, "The Possibility of a Science of Politics"; Charles A. Beard, "Time, Technology and the Creative Spirit in Political Science"; and Pitman B. Potter, "Political Science in the International Field," *American Political Science Review*, XVII (1923), pp. 381-391. Merriam and Rice also expressed this concern, as noted above.

[65]Merriam, "The Present State of the Study of Politics".

[66]Merriam, Robert T. Crane, John A. Fairlie, and Clyde L. King, "The Progress Report of the Committee on Political Research," *American Political Science Review*, XVII (1923), pp. 274-312.

[67]"Reports of the National Conference on the Science of Politics," *American Political Science Review*, XVIII (1924), pp. 119-166; "Reports of the Second National Conference on the Science of Politics," *American Political Science Review*, XIX (1925), pp. 104-162; "Report of the Third National Conference on the Science of Politics," *American Political Science Review*, XX (1926), pp. 124-170.

[68]The 1925 meetings included a discussion of public opinion research, with an emphasis upon election results. See "Reports of Roundtable Conferences," *American Political Science Review*, XX (1926), pp. 396-412. This was followed in 1926 by roundtables devoted to "Scientific Methods in the Study of Electoral Problems" and "The Problems of a Scientific Survey of Criminal Justice". See "Reports of Roundtable Conferences," *American Political Science Review*, XXI (1927), pp. 389-409.

[69]A. N. Holcombe, *Political Parties Today*.

70 Stuart A. Rice, Farmers and Fieldworkers in American Politics (New York: Columbia University Press, 1924).

71 Charles Merriam and Harold F. Gosnell, Nonvoting: Causes and Methods of Control (Chicago: University of Chicago Press, 1924).

72 A. N. Holcombe, Review of Merriam and Gosnell, Nonvoting: Causes and Methods of Control, American Political Science Review, XIX (1925), pp. 202-203. Similarly, Floyd Allport greeted Catlin's Science and Method of Politics with this enthusiastic announcement: "It will soon be possible for political scientists to cease considering their field one of formal description and legalistic philosophy and regard it as a natural science." (Quoted in Somit and Tanenhaus, The Development of American Political Science, p. 126.) Other notable works included Gosnell, Getting Out the Vote, which claimed to be an "experiment" in stimulated voting, and Rice's Quantitative Methods in Politics, which applied a number of techniques.

73 Hanna Grace Roach, "Sectionalism in Congress (1870-1890)," and John D. Barnhart, "Rainfall and the Populist Party in Nebraska," American Political Science Review, XIX (1925), pp. 500-526, 527-540.

74 On statistical analysis, see Stuart Rice, "Some Applications of Statistical Methods to Political Research," American Political Science Review, XX (1926), pp. 313-329. On scaling and measurement, see Floyd Allport and D. A. Hartman, "The Measurement and Motivation of a Typical Opinion in a Certain Group," American Political Science Review, XIX (1925), pp. 735-760; Stuart Rice, "The Identification of Blocs in Small Political Bodies," American Political Science Review, XXI (1927), pp. 619-627, and Quantitative Methods in Politics; W. F. Ogburn and Nell Snow Talbot, "A Measurement of the Factors in the Presidential Election of 1928," Social Forces, VIII (1929), pp. 175-183; Leonard D. White, The Prestige Value of Public Employment in Chicago (Chicago: University of Chicago Press, 1929); Harold D. Lasswell, "The Measurement of Public Opinion," American Political Science Review, XXV (1931), pp. 311-326; Louis L. Thurstone and E. J. Chave, The Measurement of Attitude (Chicago: University of Chicago Press, 1929); Peter Odegard, The American Public Mind (New York: Columbia University Press, 1930). American Political Science Review editor Frederic A. Ogg noted in the 1930 Review that half a dozen methods books had appeared in the recent past, with a heavy

emphasis on data gathering. See Ogg, Review of George A. Lundberg, Social Research and Wilson Gee, ed., Research in the Social Sciences, American Political Science Review, XXIV (1930), p. 197.

[75]See, for example, Louis L. Thurstone, "Multiple Factor Analysis," Psychological Review, XXXVIII (1931), pp. 406-427; C. H. Titus, "Voting in California Cities 1900-1925," Southwest Political Science Quarterly, VIII (1928), with followup articles in 1929 and 1930; and W. F. Ogburn and Nell Snow Talbot, "A Measurement of Factors in the Presidential Election of 1928".

[76]Somit and Tanenhaus, The Development of American Political Science, pp. 103ff.

[77]Ibid., pp. 103-108.

[78]Ibid., p. 113.

[79]Ibid., pp. 132-133. They cite Peter H. Odegard, Pressure Politics: The Story of the Anti Saloon League (New York: Octagon Books, 1966); E. Pendleton Herring, Group Representation Before Congress (New York: Russell and Russell, 1967); Frederick L. Schuman, International Politics; Roy V. Peel, The Political Clubs of New York City (New York: I. J. Friedman, 1968); E. E. Schattschneider, Politics, Pressures and the Tariff (Hamden, Conn.: Archon Books, 1963); Walter R. Sharp, The Government of the French Republic (New York: D. Van Nostrand and Co., 1941); Dayton D. McKean, Pressures on the Legislature of New Jersey (New York: Russell and Russell, 1967); Avery Leiserson, Administrative Regulation (Chicago: University of Chicago Press, 1942).

[80]B. Lippincott, "The Bias of American Political Science," Journal of Politics, III (1940) seems to have been the first of these, although Robert S. Lynd in Knowledge for What? (Princeton, New Jersey: Princeton University Press, 1939) had previously called attention to unsuspected value biases.

[81]William F. Whyte, "A Challenge to Political Scientists," American Political Science Review, XXXVII (1943); John H. Hallowell, "Politics and Ethics," American Political Science Review, XXXVIII (1944); Whyte, "Politics and Ethics: A Reply to John H. Hallowell," American Political Science Review, XL (1946); Hallowell, "Rejoinder," in the same issue.

[82]Whyte, "A Challenge to Political Scientists,"

p. 692.

[83]Hallowell, "Rejoinder," p. 312. This view was expressed often. Consider, for example, Arnold Brecht: "There can be little doubt that totalitarianism has greatly profited from that value-emptiness which has been the result of positivism and relativism in the social sciences." See Brecht, Review of Jacques Maritan, _Scholasticism and Politics_, _American Political Science Review_, XXXV (1941), pp. 545-546.

[84]See, for example, Robert D. Leigh, "The Educational Function of Social Scientists," _American Political Science Review_, XXXVIII (1944).

[85]Somit and Tanenhaus, _The Development of American Political Science_, pp.147, 184.

[86]_Ibid._, p.184.

[87]_Ibid._

[88]Herbert Simon, Administrative Behavior (New York: Macmillan Co., 1947); C. Herman Pritchett, _The Roosevelt Court_ (New York: Octagon Books, 1963); V. O. Key, Jr., _Southern Politics_ (New York: Alfred A. Knopf, 1949); Harold D. Lasswell and Abraham Kaplan, _Power and Community: A Framework for Political Inquiry_ (New Haven: Yale University Press, 1950); Herbert Simon, D. W. Smithberg and V. A. Thompson, _Public Administration_ (New York: Alfred A. Knopf, 1961); David B. Truman, _The Governmental Process_ (New York: Alfred A. Knopf, 1951).

[89]Bertram M. Gross, Review of Arthur F. Bentley, _The Process of Government_, _American Political Science Review_, XLIV (1950), pp.742-748.

[90]Merriam, "Political Science in the United States," in _Contemporary Political Science_, UNESCO publication 426 (Paris, 1950).

[91]Claude E. Hawley and Louis Dexter, "Recent Political Science Research in American Universities," _American Political Science Review_, XLVI (1952), pp. 470-485.

[92]C. B. MacPherson, "World Trends in Political Science Research," _American Political Science Review_, XLVIII (1954), pp. 429-449.

[93]Somit and Tanenhaus, _American Political Sci-_

ence: Profile of a Discipline (New York: Atherton Press, 1964).

[94]See, for example, David Easton, The Political System (New York: Alfred A. Knopf, 1953), and "Traditional and Behavioral Research in American Political Science," Administrative Science Quarterly, II (1957), pp. 110-115; David B. Truman, "The Impact on Political Science of the Revolution in the Behavioral Sciences," in Stephen K. Bailey et al, eds., Research Frontiers in Politics and Government (Washington, D. C.: The Brookings Institution, 1955), pp. 202-231; Robert Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," American Political Science Review, LV (1961), pp. 763-772; and Heinz Eulau, The Behavioral Persuasion in Politics (New York: Random House, 1963). The list in the text closely follows Easton's summary in "The Current Meaning of Behavioralism" in James C. Charlesworth, ed., Contemporary Political Analysis (New York: The Free Press, 1967), pp. 11-31. For another essentially identical summary, see Somit and Tanenhaus, The Development of American Political Science, pp. 177-179.

[95]See, for example, his pointed reply to critic Charner Perry: "Discussion," American Political Science Review, XLIV (1950), pp. 414-422.

[96]Eugene J. Meehan, Contemporary Political Thought: A Critical Study (Homewood, Illinois: The Dorsey Press, 1967), p. 55.

[97]See also Meehan, The Theory and Method of Political Analysis (Homewood, Illinois: The Dorsey Press, 1965), and The Foundations of Political Analysis: Empirical and Normative (Homewood, Illinois: The Dorsey Press, 1971).

[98]This was not entirely new, of course. Merriam and William B. Munro can be suspected of realist expectations; and the confrontation between Whyte and Hallowell (examined above) was much more a clash of traditional and realist goals, than of traditional and positivist, instrumental hopes. Whyte's point was that political scientists ought to concentrate on the way things actually operate, rather than upon value judgements.

[99]For examples of these tendencies in the authors of the programmatic statements, see David Easton, The Political System: An Inquiry into the State of Political

Science (New York: Alfred A. Knopf, 1953), p.57; Robert Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," American Political Science Review, LV (1961), pp. 765, 769; Samuel J. Eldersveld, Alexander Heard, Samuel P. Huntington, Morris Janowitz, Avery Leiserson, Dayton D. McKean, David B. Truman, "Research in Political Behavior," American Political Science Review, XLVI (1952), pp. 1003, 1005; Heinz Eulau, "Perceptions of Class in Party Voting Behavior: 1952," American Political Science Review, XLIX (1955), pp. 364-384; Eulau, The Behavioral Persuasion in Politics (New York: Random House, 1963), pp. 3, 9, 14-15, 24, 38-39.

An example of a substantive area in which commitments to realism are clear is the voting analysis conducted by the University of Michigan Survey Research Center. They objected to the aggregate orientation of the sociological analysts that had preceded them on the grounds that their models were poor predictors, but also on the grounds that individual voting could only be understood from the standpoint of individual motivation (Warren E. Miller, "Party Preference and Attitudes on Political Issues: 1948-1951," American Political Science Review, XLVII (1953), pp. 45-60), emphasized that they sought the meaning of patterns--that they were not satisfied simply to map them (Donald E. Stokes, Angus Campbell and Warren E. Miller, "Components of Electoral Decision," American Political Science Review, LII (1958), pp. 367-387; Philip E. Converse, Aage R. Clausen and Warren E. Miller, "Stability and Change in 1960: A Reinstating Election," American Political Science Review, LV (1961), pp. 269-280)--they interpreted results realistically (see, for example, Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," American Political Science Review, LV (1961), pp. 45-56; Philip E. Converse, Aage R. Clausen and Warren E. Miller, "Electoral Myth and Reality: the 1964 Election," American Political Science Review, LIX (1965), p. 321), and insisted that other political scientists modify their work when it seemed unrealistic (Donald E. Stokes, "Spatial Models of Party Competition," American Political Science Review, LVII (1963), pp. 368-377).

[100]Joined by a number of political scientists, including Sloss, Cohen, Wilson, Riker, Ordeshook, Gibbard, Satterwaite, McKelvey, Schofield, Coleman, Kadane and Oppenheimer.

[101]Anthony Downs, An Economic Theory of Democracy (New York: Harper, 1957).

[102]Ibid., p. 21.

[103]W. Hayward Rogers, "Some Methodological Difficulties in Anthony Downs's _An Economic Theory of Democracy_," _American Political Science Review_, LIII (1959), pp. 483-485.

[104]Downs, "Dr. Rogers' Methodological Difficulties--A Reply to His Critical Note," _American Political Science Review_, LIII (1959), pp. 1094-1097.

[105]Downs, _An Economic Theory of Democracy_, p. 34.

[106]William H. Riker and William James Zavoina, "Rational Behavior in Politics: Evidence from a Three-Person Game," _American Political Science Review_, LXIV (1970), pp. 48-60. Riker had also made this point previously, claiming that a more profound understanding of bargaining, coalition formation and strategy choice can be expected to follow from a study of games. See "Bargaining in a Three-Person Game," _American Political Science Review_, LXI (1967), pp. 642-656.

[107]Otto A. Davis, Melvin J. Hinich and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," _American Political Science Review_, LXIV (1970), pp. 426-448, see particularly p. 444. Again, in the same issue, Hinich and Ordeshook were anxious to make Downs' model more realistic. See "Plurality Maximization Versus Vote Maximization: A Spatial Analysis with Variable Participation," pp. 772-791.

[108]Riker and Ordeshook, _An Introduction to Positive Political Theory_ (Englewood Cliffs, New Jersey: Prentice-Hall, 1973).

[109]Riker, "The Future of a Science of Politics," _American Behavioral Scientist_, XXI (1977), pp. 11-38.

[110]Another early example of the effort to copy the formal aspects of physical theory is instructive. Fred Kort ("Predicting Supreme Court Decisions Mathematically: A Quantitative Analysis of the 'Right to Counsel' Cases," _American Political Science Review_, LI (1957), pp. 1-12) constructed a nonlinear prediction device which had no remotely realistic interpretation. Franklin M. Fischer ("The Mathematical Analysis of Supreme Court Decisions: The Use and Abuse of Quantitative Methods," _American Political Science Review_, LII (1958), pp. 321-338) protested that Kort had made a number of technical errors, but that, in any case, prediction was not enough. The point was to produce a realistic understanding of events.

Kort's reply reflects the confusion that followed the effort to combine what seemed to be the formal requirements of scientific theory and the generally assumed goal of a realistic understanding of events. He was not, he said, offering a "perfect, or unique" formula. He was not attempting to close the issue, rather only to make "a small contribution to understanding human behavior" in a limited area. He was trying to show that a pattern existed; the explanation for the pattern could come later. As in the confrontations between positive political theorists and their critics, the effort to join realism and positivism tended to yield clumsy and unconvincing results. Again, in response to an attack upon roll-call analyses as an unrealistic technique that "explained" votes by resort to external variables, David R. Derge ("On the Use of Roll-Call Analysis: A Reply to R. T. Frost," American Political Science Review, LIII (1959), pp. 1097-1099) cited a list of famous names that had been associated with the technique, listed a number of impressive technical tools that could be brought to bear, and asserted that roll-calls were an excellent source of data, none of which addressed the point at issue.

To paraphrase Charles Lindblom: The idea of a realistic understanding of events fits oddly into the formal scientific approach. Indeed, it does not fit.

[111]Hans Morgenthau, "Reflections on the State of Political Science," Review of Politics, XVII (1955), with comments about the futility of quantification. See, for example, Charles A. McCoy and John Playford, eds., Apolitical Politics: A Critique of Behavioralism (New York: Thomas Y. Crowell, 1967).

[112]Karl Mannheim, Ideology and Utopia (London: Routledge and Keegan Paul, 1936).

[113]The idea that political research was built upon an implicit conservatism (or that it tended to avoid real issues) was strongly affected by the period of Viet Nam involvement. Many members of the profession apparently concluded that a political science which was aloof to war and discrimination had lost touch with reality. Their frustration, which brought about the formation of the Caucus for a New Political Science, and fueled a short period of intense critical activity, was focused upon those characteristics of the science of politics program that seemed responsible for the official lack of interest. See Marvin Surkin and Alan Wolfe, eds., An End to Political Science: The Caucus Papers (New York: Vintage, 1969); David Easton, "The New Revolution in Political Science," American Political Science Review,

LXIII (1969), pp. 1051-1061; George J. Graham, Jr., and George W. Carey, The Post-Behavioral Era: Perspectives on Political Science (New York: David McKay and Co., 1972). Christian Bay, "Politics and Pseudopolitics: A Critical Evaluation of Some Behavioralist Literature," American Political Science Review, LIX (1965), pp. 39-51.

[114]See, for example, Eugene F. Miller, "Positivism, Historicism, and Political Inquiry," American Political Science Review, LXVI (1972), pp. 796-817.

[115]This view depends upon a series of misinterpretations. The central problem is that Kuhn has not made scientific theory arbitrary, or theory change political. On the contrary, he has argued strenuously against both. For Kuhn's argument, see The Structure of Scientific Revolutions (revised second edition; Chicago: The University of Chicago Press, 1970). For examples of the various degrees of misuse, see Marvin Surkin and Alan Wolfe, eds., An End to Political Science: The Caucus Papers (New York: Basic Books, 1970), pp. 6-7; Sanford Levinson, "On Teaching Political Science" in Green and Levinson, eds., Power and Community (New York: Vintage Books, 1969); J. Peter Euben, "Political Science and Political Science" also in Green and Levinson; and Sheldon S. Wolin, "Paradigms and Political Theories," in Preston King and B. C. Parekh, eds., Politics and Experience (Cambridge: Cambridge University Press, 1968).

[116]For Dilthey's writings in English, see H. P. Rickman, ed., Meaning in History: Wilhelm Dilthey's Thoughts on History (London: George Allen and Unwin, 1961); H. A. Hodges, The Philosophy of Wilhelm Dilthey (New York: Humanities Press, 1952), and Hodges, Wilhelm Dilthey: An Introduction (New York: Oxford University Press, 1944).

[117]Max Weber, The Methodology of the Social Sciences, trans. and ed. by E. A. Shils and H. A. Finch (New York: The Free Press, 1949); Charles H. Cooley, Sociological Theory and Social Research (New York: Holt, Rinehart and Winston, 1930); George H. Mead, Mind, Self and Society (Chicago: University of Chicago Press, 1934); and The Philosophy of the Act (Chicago: University of Chicago Press, 1938); Alfred Schutz, Collected Papers I: The Problem of Social Reality, edited and with an introduction by Maurice Natanson (The Hague: Martinus Nijhoff, 1962); Peter L. Berger and Thomas Luckmann, The Social Construction of Reality (Garden City, New York: Doubleday Anchor Books, 1967); A. R. Louch, Explanation

and Human Action (Berkeley: University of California Press, 1966); Harold Garfinkel, Studies in Ethnomethodology (Englewood Cliffs, New Jersey: Prentice-Hall, 1967); Robert M. MacIver, Social Causation (Boston: Ginn and Co., 1942); Theodore Abel, The Foundation of Sociological Theory (New York: Random House, 1970).

[118]See, for example, Charner Perry, "The Semantics of Political Science," American Political Science Review, XLIV (1950), pp. 394-406; Henry S. Kariel, "A Comment on Methods," American Political Science Review, LIV (1960), pp. 200-201; Walter Berns, "The Behavioral Sciences and the Study of Political Things: The Case of Christian Bay's "The Structure of Freedom," American Political Science Review, LV (1961), pp. 550-559.

[119]Other objections are regarded as even less convincing. For example, Eric Voegelin and Leo Strauss have argued that the fact-value distinction is undermined by more than the limitations noted concerning facticity. In addition, they have held that values are not arbitrary. Voegelin, whose orientation is largely taken from classical philosophy, and especially from Plato, argued that one's values and outlook depend upon one's personal development. The mass may contain a welter of conflicting values, but the "best man" measures things according to ultimate, transcendental criteria--he is in touch with real values. It is precisely this objective value orientation, according to Voegelin, that gives facts their meaning. See Eric Voegelin, The New Science of Politics (Chicago: The University of Chicago Press, 1952). Strauss took a similar position. He did not claim a transcendental source for proper value, but did emphasize that (at least some) values are not arbitrary. He added that the pretense of value freedom had allowed undefended commitments to dogmatic atheism and permissive egalitarianism to prevail. ("An Epilogue" in Herbert J. Storing, ed., Essays on the Scientific Study of Politics (New York: Holt, Rinehart and Winston, 1962); see also the critical comments of John Schaar and Sheldon Wolin in "Essays on the Scientific Study of Politics: A Critique," American Political Science Review, LVII (1963), and the subsequent responses, pp. 125-160.) Unfortunately, these arguments have not been accompanied by convincing criteria for distinguishing correct values from the rest. Voegelin and/or Strauss may be correct, but their arguments are regarded as efforts to elevate certain values by mere assertion. They are left, in short, in a situation where their claims are opposed by other claims, and evidence seems unlikely to change minds on either side.

CHAPTER III

ON THE PROSPECTS FOR A SCIENCE OF POLITICS

It will be the argument of this paper that the history of the physical sciences offers the most direct and useful evidence concerning the prospects for a *realistic* science of politics. That evidence is the change in physical theory over time. This argument is not based upon *interpretations* of theory change, like Thomas Kuhn's *The Structure of Scientific Revolutions*. It is based, instead, upon a straightforward examination of the theories themselves.

The point may be stated simply: if the theoretical structures of the hardest and most convincing sciences--the disciplines that inspired and have subsequently served to justify the science of politics idea--change so fundamentally over time that theory regarded as satisfying and realistic at one point in time is later thought to be inaccurate, misleading, or even ridiculous, then it is difficult to see how a realistic interpretation of theory can be justified. And if the most rigorous and successful sciences have yielded a shifting pattern of theoretical commitments, for which no straightforward realist interpretation seems to be possible, then a political science structured by their example seems unlikely to produce a realistic understanding of political events. Chapter IV, then, is a survey of the conceptual structures which have served physical theorists over time. The point is simply to make clear how fundamentally these ideas about physical events have changed.

There remains, of course, the possibility of an instrumental science of politics. Questions about the prospects for a science of this kind take two forms: whether it is technically possible, and whether it would answer interesting questions. The latter issue will not be addressed in this paper save to point out that very few of the questions political scientists have been anxious to ask seem to be available to instrumental treatment. Very little research was conducted during the period when instrumental views dominated thinking about a science of politics, and when research was undertaken on a large scale, instrumental views became scarce. However this is interpreted, instrumental theory will presumably retain some appeal. It is therefore important to consider the severity of the technical problems that have been cited. Chapter V is an examination of those issues.

CHAPTER IV

THE HISTORICAL EVIDENCE

The last peak of classical scientific activity (in the second century) was followed by a long slow decline. Interest in science had virtually disappeared from the Christian world by the seventh century, at which time even the documents containing the ancient learning were lost. For five hundred years scientific activity was confined to the Arab world. Then, with the Moslem withdrawal from Toledo in 1085, large numbers of classical works (accompanied by a wealth of Arab commentaries and additions) were again available to Christian scholars. The recovery of Greek science, and in particular of Aristotle's works, had an overwhelming impact, entering cultures with no remotely comparable intellectual traditions. Eventually, critical and independent thought emerged, but in the short term, the emphasis was upon absorbing the classical knowledge, and adapting the Christian system to it.[1] The history of the modern sciences properly begins with this period of recovery, as subsequent developments were structured by (even when they were directed against) the Greek ideas.

From early in the period of recovery, "physical" science was dominated by Aristotelian ideas.[2] On that view, the universe consisted of a set of concentric spheres, with the earth at the center.[3] The heavenly bodies (in order the moon, Mercury, Venus, the sun, Mars, Jupiter, Saturn, and the fixed stars) were carried around the stationary earth by the combined motions of these spheres. The outermost, the Primum Mobile, imparted motion to the entire system, driving the fixed stars from east to west one full revolution in twenty-four hours. The movements of each of the lower spheres were an interactive product of motions transferred from the adjacent outer spheres and independently driven local spheres. This complex system was required by the seemingly irregular motions of the planets. Although the sun and the moon seemed to circle the earth in a straightforward manner, the other planets revolved at varying speeds, and periodically undertook retrograde motions relative to the backdrop of the stars. Plato's pupil Eudoxus, and his successor, Callipus, had invented a system of "homocentric" spheres to account for these events as a set of the perfect circular motions Plato insisted upon for the celestial realm. They reconstructed the movements of each planet as a set of spheres with different axes of rotation. Thus four spheres were employed for Saturn, four for Jupiter, and five for each of the other planets. In an effort to make the model more convincing, Aristotle added additional spheres to transfer movement between planetary sections, bringing the total to some fifty-five. And he added spiritual movers to drive the supplementary spheres across and/or against the prevailing motion of the system.[4] Later Christian commentators employed a variety of simplified versions, which reduced the system to as few as eleven spheres.

The Eudoxian theory that Aristotle embraced was not the last word in Greek astronomy. Although it fit the observed evidence better than the models it replaced, there remained several prominent deficiencies. For example, the implication that the heavenly bodies should always remain the same distance from the earth did not conform to the observed variation in the brightness of planets like Venus and Mars. Similarly, eclipses of the sun had been observed to differ in degrees of completeness, indicating that the relative distances of the sun and moon from the earth must vary. A variety of alternative explanations were offered to account for these anomalies. Even before Aristotle adopted the homocentric system, Heraclides of Pontus had suggested that Mercury and Venus, which never wandered far from the sun, actually orbited that body, rather than the earth. He also revived the view that the earth rotated on its axis, substituting that motion for the daily revolution of the entire celestial realm. But he had few supporters. Later, Aristarchus of Samos suggested that the earth rotated on its axis and circled the fixed sun once a year. He thought the other planets also circled the sun. This view does not seem to have generated much enthusiasm either. Still later, Apollonius of Perga suggested that the planetary motions be reconstructed as a system of epicycles and deferents, which would allow for variations in distance and other inconsistencies without requiring a moving earth. It was this approach that dominated astronomy in the late classical period. Subsequent practitioners refined it by allowing the heavenly bodies to move in eccentric circles (the centers of the orbits not being coincident with the center of the earth), and allowing them to have "equant" motion (uniform <u>angular</u> motion with respect to a point other than the geometric center, in which case the velocity of planetary movement was not uniform). Ptolemy, the last of the influential Greek astronomers, employed more than eighty circles in a complex pattern of major and minor epicycles, eccentrics and equants to account for the motions of the seven planets.

For four centuries after Aristotle's death, then, Greek astronomers continued to observe the heavens and modify their ideas. These developments were not, however, incorporated into comprehensive philosophical systems. Nor were they made compatible with the Aristotelian system. So, when the Greek science was recovered by Christian Europe, there was a considerable gap between the astronomical works and the Aristotelian framework. The gap was closed by making astronomy a set of purely abstract geometric reductions--adequate for predictive purposes, but not to be regarded as real. They settled, in short, for a roughly accurate cosmology, and a more accurate, but unrealistic, astronomy.[5] This made good sense to the Aristotelians, who regarded mathematical abstraction as inherently much less real than arguments of a more empirical and qualitative sort.[6]

The system of spheres was divided into two fundamentally different realms. The celestial part above the moon's orbit consisted of incorruptable, unchanging heavenly bodies, revolving about the earth in a set of etherial (rigid, yet transparent and weightless) crystalline spheres. The natural motions for this realm were those uniform circular movements that Plato had identified as perfect. The sublunary region, by contrast, amounted to an imperfect hierarchy of the four elements. The ponderous material core of earth was surrounded by a layer of water, then a layer of air, and finally one of fire. The closer one got to the center of the universe, the baser and more sluggish its constitution.

Everything had a "natural place" in this hierarchy. If an element were forced away from its appropriate layer, it would strive to return there. Thus if a quantity of earth were lifted into the air, it would automatically return to its central location. And if air were forced down into the realm of water, or earth, it would automatically return to its proper place. The elements could only be put in an unnatural order by some external force. These "violent motions" would be resisted, and when the offending force was removed, a restorative natural motion would reestablish the balance. This vertical preference for place was accompanied by a horozontal preference for rest. Violent motion was required to generate movement, which movement would stop when the force was removed. The natural condition of this realm, then, was rest, and the natural motions were those rectilinear changes that would restore the system to hierarchical balance.

Under ideal circumstances, this realm would consist of four fully separated strata of decreasing materiality. These conditions did not prevail, however, as the movement of the surrounding spheres caused a constant jostling and mixing of the four layers. Some of the resulting mixtures were relatively stable, providing the varied substances of the observed world.

This system of ideas yielded several closely related "physical" sciences. Within the terrestrial region, motions could be examined as complexes of violent and natural movements (and the associated forces), the various materials of the world could be examined as compounds of the four elements (and the associated primary qualities), meteorology could be studied as the large scale interaction of these elements, and there were reasonably well developed treatments of optics and acoustics (though these owed little to Aristotle). In addition, the celestial realm could be studied mathematically to produce unrealistic, but predictively useful models of the natural uniformities.

Early opposition to the framework issued primarily from the church. In 1210 Paris authorities forbade the teaching of Aristotle's natural science works, and, a few years later, they banned the metaphysics as well. These prohibitions were renewed several times during the thirteenth century;[7] as late as 1277, attempts to reconcile Aristotelian and Christian viewpoints were condemned at Paris and Oxford.[8] The tension was severe until Thomas Aquinas produced an acceptable compromise.

A second wave of criticism, which overlapped with the first, was driven by an enthusiasm for alternative classical viewpoints. Thus Duns Scotus and (much more forcefully) William of Occam attacked the Thomist synthesis in the name of skepticism.[9] In the works of Occam's pupil Nicholas of Autrecourt, this skepticism was combined with atomism, a view that was also prominent in the works of Giles of Rome.[10] The most significant of the philosophical alternatives, however, was the Neoplatonic outlook that the Aristotelian system was in process of displacing. The earliest Aristotelians strove to combine the two perspectives, and many subsequent thinkers hoped to retain the Neoplatonic commitment to abstraction and quantity.[11] Although these efforts failed in the short run, the Neoplatonic ideas were never fully eclipsed. Indeed, it is not too much to say that a reemergent Neoplatonism was the dominant force behind the "scientific revolution" of the sixteenth and seventeenth centuries.

In the short run, however, the philosophical alternatives faded from view. The source of controversy and change shifted to internal analyses of the Aristotelian framework itself. As the system was absorbed and clarified, several pockets of difficulty were located--events which could not be explained in a straightforward manner. The most prominent of these concerned projectile motion and falling bodies. On the Aristotelian argument, an arrow should have fallen to the ground as soon as it lost contact with the bowstring. The propulsive force having been removed, the arrow was expected to seek horizontal rest, then its appropriate level in the hierarchy of elements (and mixtures). The actual behavior of arrows and other projectiles was said to be due to the commotion which the initial movement had produced in the air. As the arrow moved, the air had to rush around behind it to prevent a vacuum from occuring, This was thought to result in an additional thrust. As the thrust was at any moment less than that required to sustain the original speed, the velocity slowly died away.[12] The critics wondered why, if the continued flight was due to the rush of air, a stone would be carried so much farther than a ball of feathers.[13] There was also widespread doubt that the air could simultaneously be a source of resistance and of propulsion. A similar problem was associated with falling bodies, which seemed to accelerate, rather than move at the expected constant velocity. Some attempted to adapt the argument for the rush of air. Others thought that as the body approached earth, the increasing proportion of the atmosphere above resulted in greater downward pressure, while the shorter column below would offer a diminishing resistance.[14] During the thirteenth century the problem of falling bodies generated a broad interest in the efficient cause of vertical motion, and a number of experiments were conducted. Many of the arguments which assumed prominence in the seventeenth century (including those concerned with action at a distance) received attention.[15]

During the fourteenth century, these problems led some Aristotelians to propose a major modification in the system of dynamics.[16] Jean Buridan, noting that the rush of air could not account for the continued motion of a disc like a grindstone, offered instead the idea that the mover impresses upon the moved body a certain "impetus"--a motive propensity which only slowly dies away. In the case of falling bodies, the force of gravity was said to add constantly to the reservoir of impetus, thus causing the observed acceleration.[17] Buridan measured impetus as the product of the quantity of matter and velocity.[18] This allowed him to account for the different behaviors of the stone and the ball of feathers.

Buridan and others (including Albert of Saxony, Marsilius of Inghen and Nicole Oresme) noted that this made possible an extension of the dynamic principles of the terrestrial realm to celestial events.[19] But this truly revolutionary possibility was not considered very seriously. It seems that the "impetus" idea was intended entirely as an adjustment within the Aristotelian framework. Indeed, the proponents went to great lengths to make it conformable with the Aristotelian classification system.[20] In that spirit, it was widely accepted in France, England, Germany and Italy.[21]

The most significant critical development having to do with the celestial realm concerned the possibility that the earth might rotate upon its axis. Oresme's _Livre du ciel et du monde_ (1377) is regarded as the most detailed and acute analysis of the idea to be written prior to the works of Copernicus.[22] He pointed out that the apparent rotation of the heavens could very easily be due to a motion of the earth--all of the observations were purely relative. If the earth moved, however, a number of critical questions had to be answered. There should, the Aristotelians argued, be a strong wind blowing from the east, as the earth turned within the atmosphere. And, if an arrow were fired straight into the air, it ought to be left behind as the earth turned under it. This is a perfect instance of the case in which a crucial experiment is impossible, as "the facts" depend upon the underlying metaphysical framework. In the Aristotelian system, there was no concept of inertia, or any other idea that would justify a moving atmosphere. The natural state in the terrestrial realm was thought to be rest. So, if the earth turned, it would have to _be_ turned by a constant force. There was no reason to think the atmosphere would turn with it, though it might be churned up. For the same reason, an arrow would not be expected to turn with the earth; as soon as it left the surface it would be left behind. The facts being otherwise, Oresme's argument was rejected. He suggested, however, that the air might share the earth's rotation, and the arrow might have two motions--one due to the bowstring, and one imparted by the earth's rotation. This latter position made no sense to the Aristotelians, who believed a body could have only one motion at a time. Nor were they impressed with Oresme's (Neoplatonic) claim that his simpler system was more realistic because it was more "perfect" than their views.[23] In this, as in many subsequent scientific controversies, the "facts" depended upon a complex system of metaphysical assumptions.

This very fertile period of philosophical and scientific activity ran down at the end of the fourteenth century. Alternative viewpoints were increasingly relegated to a few scattered enclaves, as proponents of the Aristotelian mainstream achieved positions of dominance in all scientific fields. One of those enclaves was the Platonic academy at Florence, where Marsilius Ficinus, John Pico della Mirandola, and others subjected the Aristotelian system to fundamental criticism, and continued to propose a thoroughgoing mathematical approach to natural events.[24] The work of the Florence Platonists penetrated eventually to every important center of thought south of the Alps, including the University of

Bologna, where their most important representative was Dominicus Maria di Novara, professor of mathematics and astronomy. He opposed the Ptolemaic astronomy as well as the Aristotelian framework. Partly this was due to certain empirical deficiencies, but more critically it was because he regarded it as clumsy. His Neoplatonic commitments led him to see the universe as a harmonious mathematical order--an order totally unlike the complex and cumbrous Ptolemaic model.[25]

For six years, Novara was Nicholas Copernicus' friend and teacher. Copernicus had been attracted to the Neoplatonic movement before he went to Italy. But after 1496 he became a firm believer. Under Novarra, he learned Greek, studied alternative classical viewpoints, and developed a strong distaste for the Ptolemaic system.[26]

Philosophical preferences aside, the need for astronomical reform of some kind was generally accepted. The calendar was out of joint, tables for the cycles of the moon and tides, upon which seamen relied, were unreliable, and in other respects the prevailing system had been only very approximately accurate.[27] Many thought that the works of Ptolemy had once been more sophisticated, having been corrupted by translators and analysts. But the recovery of ancient documents in the fifteenth century made it clear that astronomy had not declined.[28] It was expected, then, that some reformulation of the Ptolemaic position would eventually be attempted.

The solution Copernicus offered did not constitute a revolution on behalf of critical empiricism. He made very few observations and even fewer measurements, all in a short period around 1515.[29] His data was that collected by the Greek astronomers (Timocharis, Hipparchus and Ptolemy) and their Arab successors (Arzachel, al-Battani and Thabit)--the same data upon which the Aristotelian-Ptolemaic dominance had been built. His method amounted to fitting existing alternatives to existing data in a mathemetically rigorous manner. He was familiar with classical arguments for a sun-centered universe and for the rotation of the earth upon its axis, and he was aware of at least some of the fourteenth century arguments concerning the latter possibility.[30] He simply checked the conformity of these ideas with the existing observations. His motivation was not a more realistic astronomy in the modern empirical sense, nor was he primarily concerned with instrumental limitations. He was committed to the Neoplatonic outlook, which required that empirical complexities be reduced to a harmonious and simple underlying order. Like his mentor, he objected less to the Ptolemaic astronomy's practical shortcomings than to its complexity and clumsiness.[31] He was particularly anxious that celestial events be understood as patterns of "perfect" (uniform, circular) motions.[32] His goal, then, was to make astronomy conformable with the Neoplatonic metaphysics.

From a practical standpoint, the results were not revolutionary. Astronomical tables calculated on the new theory proved to be only marginally superior to the existing alternatives.[33] And the system remained very complex; according to Kepler, the actual gain in economy was no more than one circle in seven.[34] In short, the Copernican system, as much as the Ptolemaic, was cumbersome and imprecise.

Moreover, the empirical evidence seemed to be entirely against it. There were four major empirical arguments--three concerning the daily rotation of the earth upon its axis, and the other concerning the annual motion of the earth around the sun. Two from the first group are familiar from the fourteenth century debate: The motion of the earth would mean that clouds and birds, or an arrow shot in the air, would be left behind. Moreover, there would be a stiff wind from the East. In addition, it was argued that terrestrial rotation would generate an enormous centrifugal force that would cause the earth to fly to pieces. And, finally, it was held that the annual motion would result in a stellar parallax (the apparent position of the stars changing as the earth moved around the sun).[35] As in the fourteenth century, the evidence seemed to be quite plain: the earth showed no propensity to fly apart, things in the atmosphere were not left behind, there was no prevailing wind, and there was no stellar parallax.

The heavy, inert earth was somehow to be regarded as violating the natural tendency toward the center of the universe, taking instead (and in the absence of any known driving force) a complex orbital path around a point in space near the sun. An entire worldview, intimately bound up with all existing science and with Christianity was to be abandoned to satisfy a Neoplatonic urge to reconstruct astronomical events a little more simply, and in accord with uniform circular patterns. There was no real improvement in accuracy, and every empirical test was against it. As Edwin A. Burtt has it, "Contemporary empiricists, had they lived in the sixteenth century, would have been the first to scoff out of court the new philosophy of the universe."[36]

Copernicus attempted to answer the critics by modifying the Aristotelian framework. Having removed the earth from the center of the universe, he needed an alternative explanation for gravity. He also needed a driving force to account for the earth's movement. He endeavored to keep as much of the rest of the Aristotelian framework as he could. Thus he retained the traditional sublunar structure, the distinction between natural and violent motions, and the celestial spheres (with a new center).[37] To supply gravity, he revived the ancient doctrine of Empedocles: gravity amounted to a certain natural appetite by which things strove to become wholes, and, as wholes, to become spherical. This property was said to apply to all matter, including the heavenly bodies.[38] Further, Copernicus argued that all things which achieved the perfect (spherical) shape naturally assumed the perfect (uniform, circular) motion. This is why he insisted that his system be constructed of uniform circles; as the resulting movements were natural, they required no external driving force.[39]

This alternative metaphysics allowed Copernicus to answer the critics fairly effectively (if one grants his assumptions). As the earth is a sphere (a point he expended great energy proving), it naturally revolves on its axis. And as the motion is natural, rather than violent, it follows that the sphere will not fly apart. (It makes very little sense to assume that perfect motion is self-destructive).[40] As for the earth's rotation on its axis creating a wind and causing things to be left behind, Copernicus agreed with Oresme (or, rather, with Oresme's argument): the atmosphere and the things in it share the rotation. He gives a new reason, however, claiming that the element earth mixed in the air causes it to rotate in sympathy with the main body of the earth.[41] The issue of stellar parallax he solved by making the distance to the stars much greater than had previously been thought--at least 400,000 times greater than the prevailing models. This meant that parallax was immeasurably small, rather than nonexistent.[42] It also meant, as the critics were quick to point out, that the stars would have to be of absolutely shocking dimensions, to appear so large at so great a distance.[43]

Not surprisingly, Copernicus' argument was unconvincing to the great bulk of his contemporaries. Unless one shared his enthusiasm for the Neoplatonic criteria, there was no remotely convincing reason to take him seriously. Only half a dozen people are known to have supported these views in the years immediately following his death, and only a few more during the remainder of the century. Virtually all of them were professional mathematicians who shared his commitment to the Neoplatonic metaphysics.[44]

Mainstream science faced serious difficulties in the sixteenth century, but these had nothing to do with the Copernican argument. They were, instead, a product of observations made by conservative astronomers. In 1572, a new star appeared, brighter than any celestial object, save the sun, moon and Venus. Then, in 1574, it disappeared.[45] This was an absolutely fundamental blow to the idea that the heavens are unchanging and perfect. Efforts were made to show that it was a sublunary event, but there was widespread doubt.[46] Then, in 1577, a comet (which all agreed could not be sublunar) cut straight across the crystal spheres on a path neither circular, nor centered upon the earth. This single event caused many astronomers, including the great Tycho Brahe, to declare their disbelief in the celestial orbs.[47] Tycho was familiar with the Copernican arguments, but refused to abandon the entire Aristotelian framework. He suggested a compromise system, in which the sun revolved around the earth, but the remaining planets circled the sun. This relativistic equivalent of the Copernican system (it was mathematically indistinguishable from it) solved a number of problems. As the earth remained stationary, the empirical arguments against the alternative astronomy lost their force. For the most part, the scientific and religious traditions could be saved, without rejecting the possible advantages of the Copernican viewpoint. Tycho's system remained an influential alternative through most of the seventeenth century.[48]

But his greatest contribution was the generation of better data. He designed new measuring devices of great ingenuity, which reduced errors to within four minutes of arc--one-half of the error of the best existing data, and about one-fourth the error of the best classical observations. He devoted thirty years to the improvement of astronomical maps, and, though his efforts were quickly overshadowed with the development of telescopic observation, his data played an important role in the late sixteenth and early seventeenth centuries.[49] It was his observation of the new celestial body in 1572 (and his observations of comets in 1577, 1580, 1585, 1590, 1593 and 1596) which convinced so many that the celestial realm was subject to change, and that the notion of crystalline spheres would have to be abandoned.[50] More importantly, for the long term, it was his data that made Johann Kepler's work possible.[51]

Kepler was also a committed Neoplatonist. Like Copernicus, he believed that God had created the world as a system of mathematical harmonies. Knowledge of events as they are immediately presented to the mind through the senses he regarded as obscure, confused, contradictory and untrustworthy. The "real" world consisted of the underlying mathematical patterns.[52] Kepler sought those patterns with an energy unmatched by any other scientist of the period. As he retained Copernicus' finite universe enclosed by the celestial vault, his search was limited to such patterns as might be found in the behavior of the planets. But his interests went beyond simple orbital forms to systemic relations. He sought, for example, harmonies in the relative sizes of the planetary orbits. He examined simple numerical patterns (in the orbital distances, and in the differences between them), trigonometric relations, and, finally, geometric patterns. He tried arranging geometric figures according to the number of angles, and eventually decided that a nested system of the five regular solids explained the structure of the universe. If the hypothetical spheres of the planets were inscribed in, and circumscribed about these forms, the distances between the orbits of the six known planets bore a rough resemblance to the distances that seemed to prevail.[53] The fit was off by as much as ten percent, but his a priorism was so strong that he blamed the existing data. He went on to argue that this pattern is the reason why there <u>are</u> six planets.[54] Later, he sought these large scale relations in musical harmonies, consisting of notes determined by the angular velocities of the planets, as they followed their elliptical orbits.[55] The three laws associated with Kepler in current texts were buried amongst arguments of this kind in his own works.

This search for structural uniformities was not the extent of Kepler's contribution. He was the first (and before Descartes the only) scientist to seek a physical explanation for planetary motion. Copernicus had attempted to save Aristotle's division of terrestrial and celestial regions, with their separate natures and laws.

Tycho, despite having eliminated the possibility of celestial spheres, had not addressed the issue of motive force. It was Kepler who took the next step, attempting to unite terrestrial and celestial dynamics under a single set of concepts and principles.[56]

Kepler's first professional task had been to analyze the orbital motion of Mars, using Tycho's new observations. This was a fortunate choice, as the orbit of Mars is the only one with an eccentricity large enough to be apparent from pre-telescopic observations.[57] Kepler tried many structures, striving always to reduce the apparent complexity to the simple patterns he was sure he would find. Finally, he combined two Copernican ideas--that planets move faster the closer they are to the sun, and that the velocity of revolution is inversely proportional to its distance from the body about which it revolves (which Copernicus had applied only to the entire system of planets)--with alternative orbital shapes to produce a convincing model of orbital dynamics.[58] Having decided that the orbit was elliptical and depended upon motive forces that attenuated with distance, and having assumed that rest is the natural state (so that motion requires a cause), he sought forces capable of moving the planets along the required path. He retained the Aristotelian notion that circular motion is natural to the celestial region, but introduced physical forces to propel the planets, and account for the deviations from perfect circular orbits. He argued that the sun reaches out to the planets and moves them with radial arms of force that lie in the plane of the planetary system, and attenuate with distance.[59] This set of propulsive forces moved the planets along their natural circular paths without attracting or repelling them. Additional motive forces were required to bend these circles into ellipses. Kepler solved this problem by incorporating Gilbert's recent argument that the earth is a giant magnet. If the other planets were also magnets, as he believed, then one pole might draw each toward the sun while the other repelled. Further, if the axis of each planet were tilted relative to the plane of orbital motions (as the earth's was known to be), then the required oscillation (making circles into ellipses as an epicycle would) could be expected.[60] In a later work, Kepler gave the sun responsibility for all of these motions with the addition of a quasimagnetic force that attracted or repelled the planets as their "friendly" or "hostile" side was directed toward the sun.[61] Both arguments accounted for the observed behavior very well.

Kepler's contribution, then, was considerable. He retained the services of souls and spirits (most notably to turn the sun and the planets on their axes).[62] And he kept the finite universe of the Copernican argument, as well as the Aristotelian biases in favor of circular motion and absolute rest. But he was the first to attempt to unify terrestrial and planetary dynamics, and he introduced a very modern kind of empiricism, in which hypotheses were subjected to rigorous observational tests. He also offered a briefly influential and (more significantly) suggestive view of the forces behind orbital dynamics.

But all of this was buried in a confusing set of mystical arguments and odd metaphors that most enlightened people of the period found unconvincing.[63] His books were extremely difficult, even without the overlay of Pythagorean symbolism, and they were published far from the main centers of scientific activity. It is not too surprising, then, that he had no immediate followers. Descartes and Galileo (with whom he corresponded) seem to have been completely unaffected by his ideas. His influence was, therefore, delayed until Borelli (somewhat ineptly) revived his unified dynamics and his orbital calculations.[64]

In the meantime, the most significant developments concerned terrestrial physics. One of the great problems with the new astronomy was that it undermined the integrity of the other Aristotelian sciences. If the earth was not stationary at the center of the universe, instead rotating on its axis while following an annual orbit, then it was hard to see how the traditional ideas of natural and violent motions, or the ordered hierarchy of the four sublunary elements and the celestial fifth matter, could be retained. Every one of the physical sciences was affected, at least indirectly, and some were reduced to a state of chaos. It was simply essential that a new physics be added to the new astronomy, making possible once again a consistent set of physical science concepts and models.

Galileo was the primary architect of that new physics. He shared the Neoplatonic conviction that God had created a mathematical world--that nature is, at bottom, a simple, orderly system of regular and necessary relations. The world of sense, on the other hand, he regarded as subjective and unreliable--a riddle that could only be understood when it was resolved into its underlying mathematical components and reconstructed as a set of exact mathematical demonstrations. Empiricism was to play only the most peripheral role in this process. The scientist began with sense, but quickly left it for consideration of the <u>real</u> constituents of the world, which could not be observed. The deductions from these were believed to be true, whether they could be empirically confirmed or not, providing a scientific understanding that was objective and certain. Experiments were conducted only to convince those who did not understand, or share confidence in, the method.[65]

This notion of primary and secondary qualities, in which the absolute, objective, immutable and mathematical primary qualities are contrasted with the subjective and unreliable world that sense makes available, had been closely associated with atomism in classical thought. Kepler managed to employ a form of the argument without considering atomism, as the mathematical harmonies he was concerned to discover consisted of large scale geometrical relations between simple celestial bodies. But Galileo was primarily interested in terrestrial events, which inclined him toward the atomistic arguments.[66]

In any case, Galileo's real contribution was to transfer the Neoplatonic outlook from celestial to terrestrial events. He attempted to create a physics in which bodies with simple mathematical properties moved in abstract geometric space under the influence of forces. Optical writers had always done this, treating light rays (thus explaining reflection and refraction) purely geometrically. And Archimedes (whose work had been recovered in 1543, and who had a powerful effect upon Galileo) had treated physical statics in this manner.[67] But no one had extended the method to the motions of terrestrial bodies, nor had anyone been bold enough to argue that this was the only valid approach to any physical problem whatever.

The basic properties into which Galileo resolved sensory events consisted of bodies (with size, shape and weight) located and moving in an abstract and homogeneous Euclidean space, under the influence of forces (the most prominent of which was gravity). An important implication of this reduction was the idea of inertia. In a vacuous and homogeneous geometrical space, there could be no privileged places, and no natural or violent motions. A body in motion would continue to move with the same velocity and direction until acted upon by other forces. This inertial viewpoint made it possible for bodies to be simultaneously affected by multiple, independent forces. This was in marked contrast to previous systems of dynamics, which required one motion to be completed before another could achieve significant influence.[68] When forces acted, the effect was the same whether the body was at rest or in motion. Only <u>changes</u> required explanation.[69]

These explanations were entirely in terms of efficient causal patterns. All senses of external cause (including final causality) were banished; causality was lodged entirely within the system of bodies itself--all events being attributed to the lawlike interaction of the bodies and a set of simple forces. Scientific knowledge amounted to an awareness of these patterns.[70]

Although this system of terrestrial dynamics was

Galileo's major contribution, he was much better known for his participation in the astronomical debate. That participation consisted primarily of observations. In 1604 he found yet another celestial newcomer without parallax, which put it in the realm of the fixed stars. (Tycho's similar demonstration that the nova of 1572 was a celestial event reached a wide audience only in 1603, so Galileo's observation received considerable attention.)[71] Then, in 1609, he constructed a telescope, with which he made a number of discoveries. The surface of the moon appeared to have mountains and valleys, just like the earth. Indeed, Galileo thought he saw rivers, lakes and seas.[72] He found an immense number of hitherto unobserved fixed stars. He substantiated the view of other observers that the sun had dark spots, and interpreted their changes as evidence that the sun rotated on its axis. Jupiter was found to have satellites. And Venus was observed to have phases, as Copernicus had predicted.[73] In short, celestial phenomena were much more complex than any extant astronomical system allowed. The stars did not seem to be arrayed in a single shell, rather to be distributed through space. The appearance of the planets changed over time, and the sun itself was stained by spots. The moon seemed to be much like the earth, and it was suspected this might be true of the other planets as well. The heavens Galileo saw could hardly be called incorruptable or unchanging. This evidence made it much more plausible to attempt to unify the sciences of the celestial and terrestrial realms.[74]

Despite his extensive observational activity, Galileo did not make a significant contribution to astronomical theory. He made the familiar arguments in favor of the earth's rotation, save that he employed the new principle of inertia to explain why things were not left behind by the earth's rotation on its axis.[75] Then he presented a simplified version of the Copernican model, with plain circular orbits centered on the sun, and argued that orbital dynamics ought to be understood as a case of <u>circular</u> inertial motion. He was unaware of the more complex orbital forms Kepler was exploring, and unable to escape the Aristotelian idea that circular motion was natural for the celestial realm. It was not until Descartes produced a comprehensive system on the new basis that the mechanical outlook fully displaced these Aristotelian structures.[76]

Descartes was also a committed Neoplatonist. He agreed that the real world was a system of mathematical relations between simple entities. By contrast, the world of sense, upon which traditional philosophy had been erected, he regarded as subjective and untrustworthy. The appropriate method, then, was to resolve events into their simple natures (extension, figure and motion), which were grasped intuitively, then deduce the actual macrostructure of events from these basic quantitative elements.

But Descartes disagreed with some of the metaphysical assumptions that his predecessors in the Neoplatonic movement had made. He insisted, to begin with, that the universe is a plenum--that Galileo's vacuous space is impossible, and that matter is continuous, rather than atomistic. He was also anxious to eliminate the idea of unexplained "forces" or "attractions" (what is more the strange powers Kepler had called upon). Events were to be understood entirely in terms of bodies and <u>directly communicated</u> motions. He also insisted that Galileo's positivism be replaced with a commitment to full intelligibility. He regarded Galileo as a naive phenomenalist, who had been successful in isolated feats of mathematical description, but had completely failed to understand the <u>reasons</u> for the patterns he found.[78]

Descartes believed that this simple system had been given a fixed quantity of original motion, and that all subsequent events amounted to the lawlike transmission of this motion through the system of bodies. The effort to extend this simple structure to astronomical events and to gravity (those subjects which had inspired his predecessors to resort to "forces") led Descartes to the notion of an ethereal "first matter".

This all pervasive medium was said to have fallen into a series of whirlpools, or "vortices", which carried the visible astronomical bodies around, and impelled them toward the axes of rotation--a propensity that was just offset by the rectilinear inertia of the orbiting bodies.[79]

As Descartes was anxious to construct a complete philosophical system, he had to find a place for those events that had been left out of his mechanistic universe--those peculiarly human experiences of awareness, thinking, willing and feeling. The result was his famous dualism.[80] Alongside the mathematically regular world of matter, God had created minds full of these other things. But also, fortunately, minds capable of grasping the nature of the physical world. This understanding was blocked by the confusion inherent in the world of sense, but available to minds focused clearly upon the fundamental natures.

Descartes created the first complete picture of nature on the new mechanical basis. If this attempt to replace the comprehensive Aristotelian system was not ultimately successful, it was nonetheless very influential. Scientists of the mid seventeenth century found much more in the Cartesian philosophy than in Galileo's physics, or the undeveloped empirical approaches. It inspired the bulk of the natural scientific thinking on the continent for the remainder of the century (and much of the next).[81] Although the idea of vortices was ultimately ridiculed, some very great minds (including Huygens and Leibniz) accepted it. Even Newton did not reject it outright, but subjected it to a careful and serious analysis.[82] Furthermore, many of his ideas survived the demise of the Cartesian system. He was the first to suggest a balance of centrifugal and centripetal tendencies in orbital dynamics. He invented the measure of "motion" (i.e. mv) that Newton employed. He gave inertia its modern form, placing motion and rest fully on the same ontological level. And he was the first to recognize the need to reconcile the Neoplatonic outlook with the characteristically human experiences that were increasingly being shut out of the "real" world.[83]

In at least one other respect, Descartes pushed physics toward its modern form: he regarded the universe as limitless. Although Copernicus had expanded the Aristotelian universe, his system remained finite, being enclosed by the celestial vault. Other Astronomers were equally satisfied with a bounded universe, as their attention was focused upon the nearby system of planets. The idea of an unlimited space was fully compatible with the Neoplatonic argument, but it seems to have originated elsewhere. in arguments concerning the principle of plenitude. It was widely held that God's infinite creative power would manifest itself without limit, so long as the addition of new elements did not result in a more than offsetting detriment to those already created.[84] This staple of medieval phisosophy had been applied freely to issues like the number and kinds of living beings. But it had been constrained in the astronomical and cosmological realms by the prevailing Aristotelian system. When arguments were raised against the classical structure, this constraint was weakened. A few of the fourteenth century critics speculated along these lines, and subsequently the argument became widespread. By the early sixteenth century (before Copernicus published), theories of the plurality of planetary systems and of inhabited planets, of the infinity of stars, and of the infinite extent of the universe were common topics of discussion.[85] Later in the century, the Copernican astronomer Thomas Digges added the notion of an infinite universe to the Copernican structure, breaking the enclosing sphere of the fixed stars. But the idea received its most intense advocacy from Giordano Bruno.[86]

These arguments had rather little immediate effect upon the scientists. Tycho Brahe rejected the idea out of hand.[87] Kepler regarded it as scientifically meaningless, as no conceivable observation or test could determine its accuracy; nor would any physical theory be affected by it. Perhaps more importantly, it violated his sense that the finite universe represented an order and harmony that could not be found in an infinite, and thus formless, universe.[88] Galileo took no part in the debate, though he rejected the concept of a local center for the universe, and suspected that the fixed stars were so many suns. Shortly after, in the work of Descartes, the idea of a limitless universe entered the physical sciences explicitly.[89] Henceforth, the universe would have this character.

Not all of European physical science was conducted in the Neoplatonic framework. An alternative approach had emerged from the craft tradition in the sixteenth century--a qualitative and empirical investigation of physical events. The outstanding early representative was William Gilbert, who designed instruments and conducted experiments, with particular emphasis upon magnetic and frictional electrical phenomena. For the most part, his procedure consisted of aimless observation and experiment, followed by a speculative search for theory that would save the phenomena. He rarely returned to the subject for further experimental clarification. Still, he made some interesting discoveries, and revived a number of others from the experimental writings of the thirteenth century. As noted above, his belief that the earth is a huge magnet, and his theory of magnetic attractions affected Kepler.[90]

Shortly after, Francis Bacon endeavored to bring about a "marriage of the empirical and rational faculties," to overcome the weaknesses that each approach exhibited in isolation. He did not trust mathematical arguments, and was particularly averse to Galileo's method, in which events were reduced to those few structural properties that could be treated quantitatively. As he did not share the enthusiasm for Neoplatonism, he did not believe this constituted a clarification of confused sensory information. On the contrary, it seemed to be a disastrous retreat from realism. This learned tradition had simply lost contact with experience. The practical craft orientation had a better grasp of the value of empiricism, but was equally ineffective scientifically, as it was unsystematic and inadequately concerned with generalization. If these two approaches could be combined, Bacon believed that the technological progress of the craft disciplines would be speeded up and extended to new fields, with very positive practical consequences.[91]

He had a hopelessly naive sense of the magnitude of the scientific task. He thought all natural phenomena could be explained in an encyclopedia about six times the size of Pliny's <u>Natural History</u>. And, as he believed the number of relevant facts were few, simple inductive sifting seemed entirely adequate to produce accurate explanations. The key to method was to be systematic in examining the very limited set of facts, while avoiding prejudice and reverence for authority.[92] No one employed his method, but Bacon inspired many scientists to attempt the proposed "marriage", including the founders of the Royal Society.[93]

Robert Boyle was probably the most interesting of the empiricists before Newton, as he was the first to attempt a practical combination of the several metaphysical currents of the mid seventeenth century. He was not a profound mathematician, and did nothing to further the development of quantitative knowledge. But he accepted the core of the Neoplatonic argument, and the mechanical view of nature with which it was associated. Method, he agreed, rightly centered upon the reasoned analysis of sensible facts, in which events were resolved into their simple constituent parts, then recomposed as sets of formal deductions. He was concerned, however, to give experiment a more significant role. He feared (with Descartes very much in mind) that science would be misdirected by impatient efforts to build whole systems. He wanted to stay closer to the empirical evidence, subjecting each particular implication to experimental test. He was particularly distressed by the propensity to make human realities peripheral and "secondary". He argued that man with his senses is as much a part of the universe as bodies in motion, thus the primary and secondary qualities ought to be regarded as equally real. He also doubted that the mechanical system constituted a fully independent machine. He argued that God intervened regularly on behalf of an order that would otherwise (i.e. under purely mechanical conditions) quickly dissipate. And in an associated argument, he asserted the value of final causality. He did not suggest it as a replacement for efficienct causality, which rightly got the primary attention of the scientists; rather as a supplement, a reminder of the dependence of this natural system upon God's will and ongoing concern. In many of these arguments, Boyle predates Newton, who ultimately provided the more successful version of Bacon's "marriage".[94]

On the eve of the Newtonian revolution, then, European physical science was divided into two main currents. The neoplatonic belief that the real world consists of a set of simple mathematical harmonies had given rise to an abstract, deductive physics. Its practitioners were anxious to resolve the subjective and unreliable world of sense into its simple, underlying components, which amounted to bodies moving inertially in an unbounded and homogeneous Euclidean space. These objective primary qualities were to be examined mathematically to determine the efficient causal patterns behind the secondary effects that could be observed.[95] This mathematical physics was paralleled by an empirical approach that put emphasis upon experimental tests. The resolutive-compositive procedure was accepted as the necessary core of an effective physical science, but it was feared that the process did not necessarily generate reliable results. The empirical method was therefore offered as a supplement. The implications of the deductive argument were to be subjected to experimental test.

Newton was very much a proponent of the empirical viewpoint. For him, there was absolutely no a priori certainty that the world had a simple mathematical character (or, insofar as it did have this form, that it would be available to existing mathematical techniques). In the Universal Arithmetic he argued that some problems would probably never be reduced to mathematical forms. So, while he followed the resolutive-compositive procedure, and employed mathematical reasoning whenever possible, he insisted upon the purely abstract and speculative character of the results, until they received physical verification. His works are filled with denunciations of speculative hypotheses which had not been, or could not be, tested empirically.[96]

The basic constituents of Newton's theories are familiar from the mechanical views of his predecessors: bodies moving under the influence of forces in infinite space. The bodies he made atomistic--composed of absolutely hard, indestructable particles which embodied the traditional primary qualities. All change in nature was reconstructed as separations, associations and motions of these permanent atoms. Bodies were characterized (for the first time) by their mass, rather than their weight. The motions were entirely inertial, in the sense that Galileo had suggested, and Descartes had clarified. The forces were quantified in terms of the new concept of mass (in combination with acceleration). And the space was at various times filled with quasi-Cartesian ethereal media, though he gave them rather less to do than Descartes had.[97] The goal of analysis was a set of lawlike descriptions. Newton, like Galileo (and in clear contrast to the Cartesians), was satisfied to find patterns in the interactions of masses and forces.[98]

In Principia, Newton was able to show that the whole intricate movement of the solar system could be deduced from a single assumption: that each particle of matter behaved as though it attracted every other particle with a force proportional to the product of the masses, and inversely proportional to the square of the distance between them. This notion of the role of gravitation had been developing for some time. In 1643, Roberval stated the idea in a rough (and purely qualitative) form.[99] In 1659, Huygens provided a mathematical analysis of centrifugal action.[100] In 1666, Robert Hooke read a paper before the Royal Society arguing that rectilinear motion could be bent into an orbit by an attractive property of the central body. In 1674 he decided that the attractive property could be gravity, and by 1678 he was arguing for gravity as the universal principle that binds all of the bodies of the solar system together, accounting for coherence as well as orbital behavior. Later he decided that the inverse square law was the appropriate form for the attractive relationship, if circular orbits could be assumed. But he was no mathematician, and his mastery of the ideas was by no means complete.[101] By 1684, Hooke, Halley, Huygens and Wren all seem to have shown that gravity operating according to the inverse square principle would explain simple orbits. But Borelli had revived Kepler's work, drawing attention to the complex orbital forms that Galileo and Descartes had ignored (and to Kepler's quantitative laws of orbital motion).[102] Thus several fellows of the Royal Society wondered whether a planet moved by the proposed forces could describe an ellipse. Halley, on a visit to Newton, found that he had solved the problem almost twenty years earlier, for the case in which the mass of a planet could be treated as though it were concentrated at a point in the center. As he had been unable to prove that assumption, he had set the work aside. Under Halley's prodding, he took it up again, proving this time that spheres of matter could be so treated. The result was Principia.[103]

In it he demonstrated not only that Kepler's elliptical orbits (and his quantitative laws of orbital dynamics) would follow from the law, but also that the terrestrial dynamics of Galileo and Huygens could be understood as products of this same gravitational attraction. At last the new astronomy and the new physics had been firmly united.[104] He also showed that the Cartesian vortices would lack stability, that they were incompatible with Kepler's laws of orbital motion and the principle of conservation of motion, and that they would require a nonmechanical "active principle" (which the Cartesians would not accept).[105]

Later, Newton made optics a branch of physics, by demonstrating that his corpuscular theory of light and the theory of matter were cognate and complimentary. Previously, the theory of light had been almost entirely geometrical. Descartes had offered the first physical hypothesis concerning light, making it a sensation caused by pressure of the ether upon the optic nerve. Hooke subsequently made light a sensation due to vibrations in the transmitting medium, a position which Huygens also developed.[106] The chief difficulty for this undulatory view was the existence of sharp shadows; vibratory light would be expected to spread out as it progresses. This convinced Newton to construct a corpuscular theory, though he was not without reservations.[107]

It was absolutely central to Newton that this system of relations between bodies and forces not be regarded as independent and self-contained. He did not believe that a purely mechanical universe would be stable; on the contrary, he was sure that it would rapidly lose its harmonious structure.[108] God not only created the universe, he continued to actively participate in it. Indeed, God was Himself extended throughout the infinite space. Operating through an intermediating "spirit of nature", He provided the purposive structure that a purely mechanical system could neither achieve nor maintain. Thus he provided cohesion, gravity, magnetism and other "forces" that had been observed to operate. Moreover, He periodically reformed the solar system's inherently unstable orbital relations, and prevented the fixed stars from collapsing together at a point in space. This notion, which is familiar from its appearance in Robert Boyle's works, seems to have originated with their friend and colleague Henry More.[109] More had been concerned to reintroduce God into the new science, from which He had increasingly been removed. He accepted the basic physico-mathematical worldview, but believed that

God was present within the world of bodies and motions--that the scientists' space was literally divine. He pointed to the purposive events in nature that did not seem reducible to blind material relations, suggesting that God's intervention was the only reasonable explanation for them.[110] Newton employed this idea to account for the many mysteries surrounding his system, from the persistence of structural patterns to the many otherwise unexplained "forces". It was also responsible for his outwardly strange belief in absolute space and time. He knew very well that these were without empirical meaning, save in the case of circular motion. He insisted upon them for purely theological reasons: as space is divine, things were moving in God. He, at least, would know whether motion was absolute or relative. It was the divine consciousness that provided the structure of reference.[111]

This Newtonian synthesis, which proved so powerful in its capacity to account for structural patterns, was rapidly accepted in England. But on the continent, where the Cartesian ideas were at a peak of influence, it was less successful. The problem was straightforward: it failed the Cartesian test of full intelligibility. For those accustomed to Descartes' criterion, the Newtonian science seemed strangely incomplete, settling, as Galileo had, for phenomenal descriptions of unexplained patterns.[112]

In fact, Newton agreed that his philosophy was incomplete. He had proposed several efficient causes for gravity over the years, and settled for a descriptive science only because he was unable to make these explanations convincing.[113] With the single exception of Colin MacLaurin, however, the very first generation of his pupils (including Cotes, Keill and Pemberton) made gravity a real physical force, accepting action at a distance as a property of matter. It was this doctrine that was pitted against the Cartesians in the later stages of the conflict, and which aroused the strongest opposition.[114]

For fully fifty years after the publication of _Principia_, there were efforts to reconcile the Newtonian laws with the Cartesian theory of celestial vortices--efforts to make the laws intelligible as products of simple mechanical interactions. Finally, after 1740, the opposition died down. The idea of attraction had slowly lost its strangeness; scientists had become accustomed to the idea of action at a distance, and abandoned efforts to provide an external mechanical explanation. Instead, the great physicists and mathematicians of Europe (most notably Maupertuis, Clairaut, D'Alembert, the Bernoullis, Euler, Lagrange and Laplace) began the work of perfecting the Newtonian apparatus.[115]

A series of general mechanical principles were found to cover whole classes of previously independent problems: the principles of Conservation of Force, Virtual Velocities and Least Action, D'Alambert's Principle and Euler's Equations. Then, late in the century, Lagrange moulded the whole of theoretical mechanics into a unified system, with which it was possible to describe the motions of any set of bodies. Where Newton had determined results from laborious geometric arguments, Lagrange provided a systematic, streamlined and fully abstracted presentation.[116] In this, and the equally comprehensive works of Laplace, the Newtonian framework was brought to a peak of perfection that seemed to defy significant improvement.

Along the way, of course, the irregularities that had caused Newton to think the system was fundamentally unstable were eliminated, or shown to be periodic and self-correcting. Newton's cherished theology was thus rapidly peeled off in favor of a tough minded system of purely mechanical interactions.[117]

These fundamental changes in the Newtonian argument were not regarded as detracting from Newton's stature. On the contrary, Laplace assigned Principia pre-eminence among all the productions of the human mind, and Lagrange complained that nothing of equal significance remained to be done. On a basis of Newton's arguments, the astronomical and cosmological issues that had troubled the world for more than two centuries seemed to have been solved for good.[118]

In the next period, the aim of physics became the extension of the mechanical framework to other subjects: sound, heat, light, electricity and magnetism. Newton had already brought light within the boundaries of the system, making it a corpuscular substance that, falling upon the eyes, creates the sensation of light. The old geometric approaches to optical phenomena were easily translated into this framework. The emitted particles travelled in straight lines and were reflected from certain surfaces in a mechanical fashion. The problem of refraction had been more difficult, but Newton solved it by making the corpuscles subject to local forces associated with the matter of the refracting body. These forces changed the direction of movement as it passed through. The problem of color Newton solved by giving each tone its own kind of corpuscle. These in turn were given different properties, so that the refracting forces would affect them differently.[119] At the beginning of the nineteenth century, this substance theory of light was generally accepted.[120]

In the study of sound, there was also general agreement upon a mechanical theory. It was regarded as a set of longitudinal waves proceeding from source to receiver through physical media.[121] This theory represented the second explanatory alternative available to the mechanists: if events could not be reduced to substances (i.e. forms of matter), then they were to be regarded as forms of motion.

Like light, heat was thought to be a substance. This "caloric" was conceived as a kind of all-pervading, highly elastic fluid, the particles of which were attracted by ordinary matter, but repelled by one another. When two bodies of different temperatures came together, it was supposed that caloric flowed from the hotter to the colder body until equilibrium was established. When expansion resulted from heating, the expansion was attributed to the mutual repulsion of the caloric particles which entered the bodies. The development of frictional heat was explained by one of two alternatives. Either the particles of a body abraded by friction were said to lose some of their capacity for caloric, which was thus liberated, or friction and pressure were thought to squeeze out some of the caloric content. These ideas supported a considerable body of quantitative scientific work, including researches into specific heats, latent heat and thermal expansion.[122]

As caloric was a substance, it was naturally supposed that it had some mass. Accordingly, various attempts were made to determine relations between the temperatures of bodies and their weight. As in all experimentation, the results were ambiguous, being complicated in this case by problems which could not be clarified through the prevailing conceptual frameworks. Stated briefly, no consistent relationship between temperature and mass was found. This did not generate much skepticism concerning the existence of the supposed substance, however. Instead, it came to be regarded as weightless.[123]

Electricity and magnetism received similar treatment. Electricity was thought to be composed of two fluids--one positive and one negative--which were in balance in neutral bodies, but out of balance in charged ones. The observed attractions and repulsions of charged bodies were explained as due to the propensity of each fluid to attract its opposite, but to repel its own kind. There was a brief conflict between this two fluid theory and Benjamin Franklin's single fluid theory, in which an excess above, or shortfall below, "normal" levels accounted for charge. This model was inconsistent with subsequently observed phenomena, however, and was abandoned. Priestley, Robinson and Cavendish suspected, and Coulomb finally demonstrated, that these attractions and repulsions conformed to an inverse square law, as gravity did.[124] After a period in which it was explained in terms of Cartesian vortices, magnetism was conceived very similarly, and Coulomb was able to show that its effects followed the same law. As with heat, the failure to find an association between these phenomena and weight was thought to indicate that the substances were weightless.[125]

This original set of mechanical theories was fairly stable during the early nineteenth century. The first exception was light. Newton had embraced the corpuscular theory primarily because the wave theory implied that light would bend around obstacles, rather than producing sharp shadows. Almost alone among eighteenth century physicists, Leonhard Euler had argued for a wave theory. Then, in the work of Young and Fresnel, it was demonstrated that light does bend, but only very slightly due to the small wavelength and high speed. On this theory light amounted to longitudinal waves in an all pervasive ether. Color was attributed to wavelength. And refraction to the differing speeds of wave transmission through different materials. This view won universal acceptance when Leon Foucault demonstrated that light indeed travels faster in air than in water.[126]

There was also some interest in making heat a form of motion, rather than an independent substance. But the most important events for mechanistic physics concerned the transformation of these forms into one another. When, in 1800, Volta created the first continuous flows of electrical current, it was noted that the wire carrying the current changed temperature.[127] In about 1820, Oersted discovered a relationship between electricity and magnetism.[128] Then, in 1831, Faraday discovered that both of these could be linked with mechanical motion.[129] The argument concerning heat was understood by some to mean that mechanical energy was being transformed into heat.[130] Joule confirmed experimentally that this was the case, and undertook to determine the rate of exchange.[131] Increasingly, physicists applied themselves to the task of understanding these relationships. During the 1830s, the idea of an underlying "energy" that could take different forms started to appear. Friedrich Mohr called it "force", and noted that it could appear as motion, electricity, magnetism, light, heat or cohesion. It seemed that the transformations from one form to another were lawlike--thus it seemed that this "energy" must be conserved. Helmholtz published the first paper to this effect in 1847, then in 1851 the law was offered as a refined general principle by Clausius and Kelvin.[132] This idea seemed to require a kinetic view of these energy forms, so that transformations could be easily conceived.[133] (It is a great deal easier to suppose that one form of motion becomes another, than to suppose that substances are so transformed.) In any case, the Newtonian system seemed to have reached another peak of concentration and power. A variety of phenomena had been linked very closely by application of the mechanical concepts, and it had been shown that energy (motion) as well as mass is conserved. The notion that the physical world consists entirely of matter, motion and simple forces depending only on distance seemed more plausible than ever.

There were, nonetheless, a few problems. The first of these concerned the wave theory of light. It was discovered that the waves must be transverse, rather then longitudinal, and, indeed, that there must be a complete absence of longitudinal motion. Fresnel pointed out in 1821 that these transverse vibrations could only take place in a medium that possessed the characteristics of a solid (unlike longitudinal waves, which could be propagated through a fluid). Then Poisson demonstrated, in 1828, that such a quasi-solid ether would necessarily add the proscribed longitudinal motions. Moreover, any such medium would retard the motions of the planets. The situation was complicated further by the need to make the ether conformable with gravitational, magnetic and electrical phenomena. The result was a series of fantastic proposals, and, eventually, a suspicion that the mechanical ideas required some fundamental modification. All of this caused Einstein to identify the emergence of the wave theory of light as the first serious problem for the Newtonian physics (though, of course, it was not recognized at the time).[134]

Strange things were also happening in research concerning electricity and magnetism. When Oersted placed a magnetic needle in the plane of a circular wire carrying electrical current, the needle was deflected into a position perpindicular to that plane. This meant that the force between the magnetic pole and the wire could not lie along lines connecting the two, but must be perpendicular to them. This was the first appearance of a force which could not be reduced to the linear push-pull form.[135] Then, in 1831, Faraday discovered that moving a magnet near a closed circuit caused a current to flow through the circuit. This, in combination with Oersted's discovery (which indicated that current flowing through a circuit creates magnetic effects) led to the notion of the "field".[136] Considerable energy was expended in attempts to understand these fields and bring them within the mechanical framework. Faraday insisted that they had a material existence, perhaps as chains of polarized particles.[137] At first, Clerk Maxwell, who brought the field ideas to full mathematical maturity, also thought the field might be interpreted mechanically, with the help of the ether. But repeated efforts came to nothing. In the end, he produced a fully abstract electromagnetic theory that described the dynamic structure of the fields, without attempting to provide a set of realistic material images.[138]

For several decades, it was expected that a mechanical superstructure would be found for the theory, but this did not occur.[139] Meanwhile, Maxwell's equations demonstrated increasing power and importance. An implication of his work was that electromagnetic action should travel through space in transverse waves, like those of light, and at the same velocity. Heinrich Hertz proved the existence of the expected waves and demonstrated that they possessed many of the properties of light. This similarity was pursued, with the result that light phenomena were brought within the theory's boundaries.[140] Ordinarily, this simplification would have been welcomed, but there was widespread concern about the status of the theory itself.

There followed a controversy of major proportions. Indeed, it was the most heated debate in physics since the confrontation between Newtonians and Cartesians over the status of action at a distance. The conservatives insisted that the mechanical ideas amounted to a set of empirical discoveries--that they were realistic, and therefore formed the necessary basis for physical understanding. Others argued for a more symbolic view of physical science. Ernst Mach, Karl Pearson, Hertz, Henri Poincare, and Pierre Duhem argued prominently for instrumental views, in which theory would be regarded as a set of symbolic summaries or prediction devices, employing freely invented abstractions. Duhem went further, providing many examples in which the observations by which theory had been tested were shown to be influenced by the theories at issue (and by other theoretical commitments).[141]

Meanwhile, the ether theories, which carried so much of the burden of the mechanical views, had come under renewed attack. They had never been entirely convincing, but a series of experiments conducted late in the century made the ether's constitution more problematic than ever. Various experiments attempting to measure the earth's motion through the ether, including the famous Morley-Michelson efforts, seemed to indicate that the ether moved with the earth. But the contrary position also had experimental support.[142] Then, Einstein argued that all of the apparent options--that the ether is carried along, that it is partly carried, or that it is stationary--must be incorrect, as the speed of light is constant in all coordinate systems. In short, no imaginable form of the ether hypothesis could be made conformable with the evidence. As the mechanical viewpoint had become overwhelmingly dependent upon the hypothesis, it was jeopardized as well. It seemed increasingly possible, as the instrumentalists had suggested, that the mechanical position amounted to a set of abstract conceptual models, for which no realistic status could justifiably be claimed--that mechanics was a useful way of thinking about natural processes rather than a realistic description of them.

Einstein went on to construct a "relativistic" physics in which many formerly unsuspected relations had to be taken into consideration. Thus length, mass and time, all traditionally regarded as fixed, were found to be related to the velocity of events relative to observers. It followed that two events which were simultaneous for one observer need not be for a second observer moving relative to the first. Nor did two lengths or two masses have to have comparable proportions for the two observers. Moreover, as this velocity approached the speed of light, the length of bodies and the speed of clocks both approached zero, while the mass of bodies approached infinity. The absolute distinction between mass and energy also collapsed. The two were found to be convertible, so a single set of conservation laws was applied to the unified concept of mass-energy. Another classical concept also took on new properties: the simple Euclidean space of the Newtonian physics was traded for more flexible views, in which space itself could be regarded as bending under the influences of masses and velocities. Even the Newtonian law of gravity was modified.[143]

This assault upon the traditional concepts was intensified with the development of quantum ideas. In 1900, Max Planck suggested that a problem concerning black body radiation be solved by assuming that energy is given up discontinuously in quanta. This concept found its most important application in the investigation of the structure of the atom. A series of discoveries early in the century led Rutherford to suggest a model of the atom in which electrons orbited a nucleus. On the classical view, this model was unstable, as charges moving in an electromagnetic field would continuously emit radiation. The electrons would therefore gradually spiral into the nucleus. Niels Bohr pointed out that the model could be saved if it were assumed that radiation could be emitted only in definite quanta. This idea was wielded into a complex structure of stable states, separated by quantum jumps. This was a most disturbing idea, as it violated the absolutely basic mechanistic commitment to continuity. In time, it was necessary to suppose that the world is made up of "things" that can change location without occupying, even momemtarily, the positions between those locations. Then, after 1920, an additional complication rose to prominence. Both Bohr and Werner Heisenberg began to argue that models of atomic behavior were inherently abstract and symbolic anyway. Information about atomic behavior could only be gotten by rather indirect methods--methods that gained one piece of information by changing others. This seemed to these scientists to pose a fundamental barrier, beyond which realistic human knowledge could not be had. Heisenberg preferred to construct fully abstract theory on the model of Maxwell's equations. Bohr was satisfied to use models, but only for utilitarian purposes. Hence, he was willing to let the wave and particle theories of light coexist, despite the fact that they were contradictory. Better, he thought, to have two models that between them cover the phenomena, than to decide upon one, and lose the additional capabilities of the other. As neither was realistic, it was pointless to insist upon clarifying the issue.[144] These two were soon joined by others who had abandoned hope for (and increasingly lost interest in) realistic theory for the small-scaled events that would play a central role in twentieth century theory.

As late as 1920, some physicists hoped to bring their discipline back to its former course:

> . . . a physicist is bound in the long run to return to his right mind; he must cease to be influenced unduly by superficial appearances, impracticable measurements, geometrical devices, and wierdly ingenious modes of expression; and must remember that his real aim and object is absolute truth, however difficult of attainment that may be; that his function is to discover rather than to create; and that beneath and above and around all appearances there exists a universe of full-bodied, concrete, absolute reality. (145)

But events have not been conformable with these hopes.

On the contrary, continuing developments within the physical sciences and the increasing volume of historical research have suggested ever more strongly that science has not been in process of discovering the nature of things. Rather it has been constructing symbolic structures which support prediction and intervention, structures that are not unique in their capacity to save the phenomena at issue. Over time, physical theorists have given these structures a considerable variety of incommensurate forms, so that ideas which seemed convincing and realistic in one period came to be regarded as defective, or even preposterous in a later one. The history of science is heavy with examples: The Copernican view that gravity is a natural appetite by which things strove to become spherical wholes, that the earth turns on its axis because spherical things naturally do so, and that the atmosphere and its contents share the rotation because earth mixed in the air causes it to be carried along in sympathy. Kepler's use of souls and spirits to turn the sun and planets on their axes, his belief that there were six planets with the observed distances between them because a nested system of the five regular solids conforms to that structure, and his explanation of elliptical orbits as a compound product of naturally circular movement powered by arms of force extending radially from the sun and supplemental magnetic forces that attracted or repelled the planets as their "friendly" or "hostile" sides were directed toward the central body. Descartes' rejection of atomism, his insistence that all physical events had to be explained by resort to directly communicated motions, without reference to occult forces like gravity, and his reliance upon an ethereal "first matter" that had fallen into a series of "vortices" that carried the visible astronomical bodies around and impelled them toward the axes of vortical motion. Newton's view that atoms consist of absolutely hard, indestructable particles, his interest in a set of exotic ethereal media, the corpuscular theory of light, in which each color had its own kind of corpuscle, and refraction depended upon very local forces in the refracting body which affected the colors differently, and his belief that a purely mechanical universe was incomplete and unstable, requiring the intervention of a physically extended God, operating through a "spirit of nature". The many forms of heat, light, electricity and magnetism that depended upon highly elastic and weightless fluids, made up of particles that variously attracted normal matter, or one another, but repelled their own kind. Then the transformation of most of these into forms of motion. The idea of light as longitudinal waves, then transverse waves, with refraction due to differing speeds of wave transmission in different media, then back to a substantive form as "photons". The proliferation of ethers to transmit various of the new "energy" forms, which finally collapsed in a mass of contradictions. The various material forms of electromagnetic fields, and the proliferation of ideas concerning the structure of atoms. It does not seem reasonable to argue that the science which relied upon these (and many other long forgotten) conceptual structures is in process of providing a realistic understanding of events. Science yields a justifiably valued power, but it does not seem to be a means for discovering the nature of things.

And if the physical sciences fail to achieve a realistic understanding of events, then it seems unlikely that a science of politics modeled upon those physical sciences will do so. There remains the possibility of an instrumental science of political events, if the practical obstacles to research can be overcome. The next task, then, is an examination of those issues.

Notes to Chapter IV

[1]Stephen F. Mason, A History of the Sciences (revised edition; New York: Collier Books, 1962), pp. 112-113; William Dampier, A History of Science, and its Relations with Philosophy and Religion (third edition; London: Cambridge University Press, 1944), p. 91.

[2]The brief history which follows is limited to those subjects which are encompassed by the boundaries of modern physics. Chemistry is excluded, despite its recent close ties with physics, as it was only very distantly related for the bulk of the period under consideration. To include it would require a significantly expanded paper, which would probably not make the point more effectively. Astronomy is included, because its association with physics over most of this period was very close. The consideration of astronomy ends at the close of the eighteenth century, however, as it played a much less vital role in the subsequent development of physical science ideas.

[3]The following description of Aristotelian "physical" science is condensed from Thomas S. Kuhn, The Copernican Revolution: Planetary Astronomy in the Development of Western Thought (Cambridge: Harvard University Press, 1957), pp. 1-80; A. Rupert Hall, The Scientific Revolution 1500-1800: The Formation of the Modern Scientific Attitude (second edition; Boston: Beacon Press, 1962), pp. 11-26; Herbert Butterfield, The Origins of Modern Science 1300-1800 (revised edition; New York: Free Press, 1957), pp. 15-28; Mason, A History of the Sciences, pp. 40-55; and Alexandre Koyre, "Galileo and Plato," in Metaphysics and Measurement: Essays in the Scientific Revolution (Cambridge: Harvard University Press, 1968), pp. 22-29.

[4]Aristotle's predecessors (and, indeed, the bulk of his successors) were concerned with the construction of mathematical models that would save the phenomena. Aristotle seems to have been the first to regard these models as physical explanations rather than abstract, geometric reductions. It is not surprising, then, that he added spheres and sources of movement to make the system physically believable.

[5]This position had been taken by most of the Greek astronomers themselves. Ptolemy explicitly rejected the idea that his system could be realistic, as his calculations required that the apparent size of the lunar

disc change in the ratio of one to four over a full cycle--an event which plainly did not occur. (See Mason, _A History of the Sciences_, p. 55)

[6]The Neoplatonists, on the other hand, were inclined to take the mathematical reductions much more seriously--indeed, to make them more real than observables. This has had the most fundamental consequences for scientific development.

[7]W. T. Jones, _A History of Western Philosophy_ (second edition, four volumes; New York: Harcourt, Brace and World, 1969), II, p. 211.

[8]Butterfield, _The Origins of Modern Science 1300-1800_, p. 21.

[9]A. C. Crombie, _Medieval and Early Modern Science_ (revised second edition, two volumes; Cambridge: Harvard University Press, 1961), II, pp. 28-34.

[10]_Ibid._, p. 39.

[11]_Ibid._, pp. 90-95. Prominent among the first group were Robert Grosseteste and Roger Bacon. A striking example of the latter was Nicole Oresme.

[12]Butterfield, _The Origins of Modern Science 1300-1800_, pp. 17-18.

[13]_Ibid._, p. 24.

[14]_Ibid._, p. 18. It was generally held that a constant force should result in a constant velocity.

[15]Crombie, _Medieval and Early Modern Science_, II, pp. 43-47. This concern with efficient causality was just as real as the (frequently emphasized) reliance upon final causes in explanations of natural events.

[16]This argument had been made by Philoponus in the sixth century, but his writings do not seem to have been available during this period, so the idea was probably arrived at independently. See Crombie, _Medieval and Early Modern Science_, II, pp. 51-52; and Mason, _A History of the Sciences_, p. 69. Actually, this was only one of many alternatives under discussion, though it was easily the most influential. See Crombie, pp. 60-67 for some of the others.

[17]Crombie, _Medieval and Early Modern Science_,

p. 67.

[18] *Ibid.*, p. 71.

[19] *Ibid.*

[20] Alexandre Koyre, "Galileo and Plato," p. 30.

[21] Crombie, *Medieval and Early Modern Science*, p. 73.

[22] *Ibid.*, pp. 76-84.

[23] In any case, Oresme treated the arguments as matters of possibility only, concluding finally that, while the evidence could be made to conform to a moving earth, there was actually no movement.

[24] Frederick Copleston, *A History of Philosophy* (nine volumes; Garden City, New York: Image Books, 1950-1974), III, 14-21.

[25] E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science* (revised edition; Garden City, New York: Doubleday Anchor Books, 1932), p. 54.

[26] Alexandre Koyre, *The Astronomical Revolution: Copernicus, Kepler, Borelli*, trans. by R. E. W. Madison (Ithaca, New York: Cornell University Press, 1961), p. 21; Burtt, *The Metaphysical Foundations of Modern Physical Science*, p. 55.

[27] Hall, *The Scientific Revolution 1500-1800*, p. 53.

[28] Kuhn, *The Copernican Revolution*, p. 125.

[29] Hall, *The Scientific Revolution 1500-1800*, p. 61.

[30] Koyre, *The Astronomical Revolution*, pp. 38-39.

[31] His alternative did not, as it turned out, offer significant advantages in this regard. See Hall, *The Scientific Revolution 1500-1800*, p. 63.

[32] Koyre, *The Astronomical Revolution*, p. 26.

[33] Kuhn, *The Copernican Revolution*, p. 168 and Hall, *The Scientific Revolution 1500-1800*, p. 63.

[34]Koyre, The Astronomical Revolution, pp. 43, 49. Copernicus claimed more, but it seems that he exaggerated both the complexity of Ptolemy's system and the simplicity of his own.

[35]Koyre, The Astronomical Revolution, p. 57; Burtt, The Metaphysical Foundations of Modern Physical Science, p. 37; Butterfield, The Origins of Modern Science 1300-1800, pp. 44-45, 70-71.

[36]Burtt, The Metaphysical Foundations of Modern Physical Science, p. 38.

[37]Kuhn, The Copernican Revolution, p. 154; Hall, The Scientific Revolution 1500-1800, p. 63.

[38]Hall, The Scientific Revolution 1500-1800, p. 66; Butterfield, The Origins of Modern Science 1300-1800, pp. 43, 153; Koyre, The Astronomical Revolution, p. 56; Kuhn, The Copernican Revolution, p. 153.

[39]Koyre, The Astronomical Revolution, pp. 40, 58-59.

[40]Crombie, Medieval and Early Modern Science, p. 84; Koyre, The Astronomical Revolution, p. 58; Hall, The Scientific Revolution 1500-1800, p. 66; Butterfield, The Origins of Modern Science 1300-1800, p. 44.

[41]Butterfield, The Origins of Modern Science 1300-1800, p. 45.

[42]Kuhn, The Copernican Revolution, pp. 156-159. In fact, the parallax was not measured until 1838.

[43]Butterfield, The Origins of Modern Science 1300-1800, pp. 70-71; Hall, The Scientific Revolution 1500-1800, p. 66.

[44]Burtt, The Metaphysical Foundations of Modern Physical Science, p. 51; Hall, The Scientific Revolution 1500-1800, p. 55. Copernicus was condemned in 1616, some 73 years after his death.

[45]This was Nova Cassiopeiae. See Arthur Lovejoy, The Great Chain of Being: A Study of the History of an Idea (Cambridge: Harvard University Press, 1964), p. 104.

[46]Butterfield, The Origins of Modern Science 1300-1800, p. 72.

[47]The Aristotelian view had been that comets are exhalations of the earth, ignited in the sphere of fire.

[48]Kuhn, The Copernican Revolution, pp. 206-208.

[49]Hall, The Scientific Revolution 1500-1800, pp. 118-119.

[50]Kuhn, The Copernican Revolution, pp. 201-205; Hall, The Scientific Revolution 1500-1800, pp. 65-66.

[51]In 1599, Kepler visited Brahe in Prague. He was kept four months, then asked to join the staff permanently. In 1601, he officially became Brahe's assistant, and when the latter died later in the year, Kepler became the new imperial mathematician. He thus had access to Brahe's data and his technical apparatus. See Koyre, The Astronomical Revolution, pp. 160-163.

[52]The Neoplatonic rejection of empirical knowledge was reinforced in Kepler's thought by the re-emergence of the distinction between primary and secondary qualities. The ancient atomistic and skeptical arguments to this effect had recently been revived by Vives, Sanchez, Montaigne, Campanella and others. See Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 56-71.

[53]The five regular solids (figures with equal sides and equal angles) were to be arrayed as follows: A cube was to be inscribed between the spheres of Saturn and Jupiter, a tetrahedron between Jupiter and Mars, a dodecahedron between Mars and earth, an icosohedron between earth and Venus, and an Octahedron between Venus and Mercury. See Burtt, The Metaphysical Foundations of Modern Physical Science, p. 62; Koyre, The Astronomical Revolution, pp. 138-139.

[54]Koyre, The Astronomical Revolution, pp. 147-149.

[55]Ibid., pp. 334-336.

[56]Copernicus' system was not sun-centered; rather it was centered on the middle of the earth's orbit. The sun was somewhat off center, and played no role in system dynamics. See Koyre, The Astronomical Revolution, pp. 154-155.

[57]Indeed, that is why Brahe, who was still committed to circular motions, regarded it as needing expert

mathematical attention. See Koyre, The Astronomical Revolution, p. 165.

[58]Koyre, The Astronomical Revolution, pp. 173-259.

[59]Butterfield, The Origins of Modern Science 1300-1800, p. 158; Koyre, The Astronomical Revolution, pp. 197-213.

[60]Koyre, The Astronomical Revolution, p. 215.

[61]This was done to keep the sun from being moved, as a result of mutual attraction, from its immobile position--a requirement of his trinitarian symbol system. The planets retained their magnetic properties, but the extent of their influence was too limited to affect the sun. See Koyre, The Astronomical Revolution, pp. 323-325.

[62]Ibid., p. 290.

[63]Burtt, The Metaphysical Foundations of Modern Physical Science, p. 71.

[64]Koyre, The Astronomical Revolution, pp. 363-364.

[65]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 72-83; Hall, The Scientific Revolution 1500-1800, pp. 168-171.

[66]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 83-89. Still it is arguable that atomism is not central to his physics.

[67]Butterfield, The Origins of Modern Science 1300-1800, p. 25; Hall, The Scientific Revolution 1500-1800, pp. 168-171.

[68]A. Wolf, A History of Science, Technology and Philosophy in the Sixteenth and Seventeenth Centuries (second edition, prepared by Douglas McKie, two volumes; London: George Allen and Unwin, 1962), I, 44-45.

[69]Galileo's version of inertia was not entirely clear; the first satisfactory definition was only given somewhat later, by Descartes. See Hall, The Scientific Revolution 1500-1800, p. 87.

[70]Burtt, The Metaphysical Foundations of Modern

Physical Science, pp. 92-103.

[71]Charles Singer, A Short History of Scientific Ideas to 1900 (London: Oxford University Press, 1964), p. 242.

[72]Wolf, A History of Science, Technology and Philosophy in the Sixteenth and Seventeenth Centuries, I, p. 30; Singer, A Short History of Scientific Ideas to 1900, p. 243.

[73]Kuhn, The Copernican Revolution, pp. 220-225; Wolf, A History of Science, Technology and Philosophy in the Sixteenth and Seventeenth Centuries, I, 30-31; Singer, A Short History of Scientific Ideas to 1900, pp. 243-245.

[74]Wolf, A History of Science, Technology and Philosophy in the Sixteenth and Seventeenth Centuries, I, pp. 30-32; Singer, A Short History of Scientific Ideas to 1900, pp. 242-246.

[75]Hall, The Scientific Revolution 1500-1800, pp. 109-112.

[76]Wolf, A History of Science, Technology and Philosophy in the Sixteenth and Seventeenth Centuries, I, pp. 32-35; Hall, The Scientific Revolution 1500-1800, pp. 109-117.

[77]Burtt, The Metaphysical Foundations of Modern Physical Science, p. 107.

[78]Hall, The Scientific Revolution 1500-1800, pp. 181, 94-95.

[79]Ibid., p. 98.

[80]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 118-119.

[81]Newton's influence was practically restricted to England, and even there it did not dominate Cartesianism. See Koyre, Newtonian Studies (Cambridge: Harvard University Press, 1945), p. 54; Burtt, The Metaphysical Foundations of Modern Physical Science, p. 125; Hall, The Scientific Revolution 1500-1800, p. 182.

[82]He may even have accepted it when young. See Koyre, Newtonian Studies, p. 63.

[83]*Ibid.*, pp. 64-65, 69.

[84]Lovejoy, *The Great Chain of Being*, p. 111.

[85]*Ibid.*, pp. 112-115.

[86]*Ibid.*, pp. 116-117. For his trouble, Bruno was burned at the stake in 1600.

[87]Hall, *The Scientific Revolution 1500-1800*, p. 10.

[88]Koyre, *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins Press, 1957), pp. 58-76.

[89]Descartes refused to accept the label "infinite" for a variety of reasons, but he embraced an endless, "indefinite" universe without a center. See Koyre, *From the Closed World to the Infinite Universe*, pp. 99-104.

[90]Mason, *A History of the Sciences*, pp. 138-140.

[91]Butterfield, *The Origins of Modern Science 1300-1800*, p. 118.

[92]Mason, *A History of the Sciences*, pp. 141-147.

[93]Singer, *A Short History of Scientific Ideas to 1900*, p. 267.

[94]Burtt, *The Metaphysical Foundations of Modern Physical Science*, pp. 168-202.

[95]In some cases, of course, forces were added as fundamental constituents, and the distinction between primary and secondary qualities led many to embrace atomism. This was particularly true in the second half of the century, after the Epicurean philosophy received an enthusiastic treatment in the works of Pierre Gassendi. See Koyre, "Gassendi and the Science of his Time," in *Metaphysics and Measurement*, pp. 118-130.

[96]Burtt, *The Metaphysical Foundations of Modern Physical Science*, pp. 212-217, 220. Newton engaged in speculation himself, but strove to keep these possibilities separate from his experimentally confirmed results.

[97]Koyre, *Newtonian Studies*, pp. 66-67, 88; Burtt, *The Metaphysical Foundations of Modern Physical Science*, pp. 231, 241, 264-269. The ethers Newton regarded as

as unverified hypotheses.

[98]Burtt, The Metaphysical Foundations of Modern Physical Science, p. 226.

[99]Butterfield, The Origins of Modern Science 1300-1800, p. 161.

[100]Ibid., p. 162. But he did not publish until 1673, and even then did not apply the analysis to orbits.

[101]Hall, The Scientific Revolution 1500-1800, pp. 265-267; Kuhn, The Copernican Revolution, pp. 249-255.

[102]Koyre, The Astronomical Revolution, pp. 467-501.

[103]Dampier, A History of Science, and its Relations with Philosophy and Religion, pp. 167-170; Butterfield, The Origins of Modern Science 1300-1800, pp. 164-167; Hall, The Scientific Revolution 1500-1800, pp. 267-269.

[104]Singer, A Short History of Science to 1900, p. 253; Hall, The Scientific Revolution 1500-1800, p. 245. He also described a method of determining orbits from observations of position, established the broad theory of the tides, determined the degree of asphericity of the earth caused by its rotation, and offered an approximate solution of the problem (essential to the dynamical theory of the moon's orbit) of determining the motions of three mutually gravitating bodies.

[105]Koyre, Newtonian Studies, pp. 99, 101.

[106]Hall, The Scientific Revolution 1500-1800, pp. 250-252.

[107]Dampier, A History of Science, and its Relations with Philosophy and Religion, pp. 178-179; Hall, The Scientific Revolution 1500-1800, p. 253.

[108]Koyre, From the Closed World to the Infinite Universe, pp. 189, 210.

[109]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 283-295; Koyre, From the Closed World to the Infinite Universe, pp. 159, 179, 218; Koyre, Newtonian Studies, p. 91.

[110]Koyre, From the Closed World to the Infinite

Universe, pp. 114-147; Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 135-148, 166-167.

[111]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 247-261; Koyre, From the Closed World to the Infinite Universe, pp. 166-167.

[112]Butterfield, The Origins of Modern Science 1300-1800, pp. 169-170; Hall, The Scientific Revolution 1500-1800, pp. 275-276.

[113]See Chapter II

[114]Koyre, Newtonian Studies, pp. 16, 149-163.

[115]Hall, The Scientific Revolution 1500-1800, pp. 275-276, 342; Koyre, From the Closed World to the Infinite Universe, pp. 274-276; Koyre, Newtonian Studies, pp. 17-18, 163.

[116]Wolf, A History of Science, Technology and Philcsophy in the Eighteenth Century (second edition, two volumes; London: George Allen and Unwin, 1962), I, 63-69.

[117]Burtt, The Metaphysical Foundations of Modern Physical Science, pp. 298-299; Koyre, Newtonian Studies, p. 21; Koyre, From the Closed World to the Infinite Universe, pp. 274-276.

[118]Koyre, Newtonian Studies, p. 18; Hall, The Scientific Revolution 1500-1800, p. 245.

[119]Albert Einstein and Leopold Infeld, The Evolution of Physics: From Early Concepts to Relativity and Quanta (New York: Touchstone by Simon and Schuster, 1938), pp. 93-100.

[120]The only major advocate of an alternative theory seems to have been Leonhard Euler, who revived Huygen's wave model. See Wolf, A History of Science, Technology and Philosophy in the Eighteenth Century, I, pp. 161-166.

[121]Singer, A Short History of Scientific Ideas to 1900, p. 423.

[122]This work was done by Black, Lavoisier, Laplace, Taylor, Ellicott, Sweaton and Ramsden. See Wolf, A History of Science, Technology and Philosophy in the Eighteenth Century, I, pp. 177-193; Einstein and Infeld,

The Evolution of Physics, pp. 37-42.

[123]The work of Boerhaave, Buffon, Roebuck, Whitehurst and Black was outstanding on this issue. See Wolf, *A History of Science, Technology and Philosophy in the Eighteenth Century*, I, 193-194. Again, an alternative theory made heat a form of motion. This view had been held by Galileo, Boyle, Hooke and Huygens in the seventeenth century, but was replaced in the eighteenth by the "caloric" idea. It was revived by Benjamin Thompson (Count Rumford) in 1798, with very little impact. The kinetic view only achieved currency after 1850. See Hall, *The Scientific Revolution 1500-1800*, p. 349; Dampier, *A History of Science, and its Relations with Philosophy and Religion*, pp. 222, 245-246; Mason, *A History of the Sciences*, pp. 486-487.

[124]Mason, *A History of the Sciences*, pp. 474-476; Dampier, *A History of Science, and its Relations with Philosophy and Religion*, pp. 222-223; Hall, *The Scientific Revolution 1500-1800*, pp. 350-357; Singer, *A Short History of Scientific Ideas to 1900*, p. 354; Wolf, *A History of Science, Technology and Philosophy in the Eighteenth Century*, I, 227-228, 242-249; Einstein and Infeld, *The Evolution of Physics*, pp. 69-76.

[125]Wolf, *A History of Science, Technology and Philosophy in the Eighteenth Century*, I, pp. 260-269; Dampier, *A History of Science, and its Relations with Philosophy and Religion*, p. 223; Einstein and Infeld, *The Evolution of Physics*, pp. 82-83.

[126]Singer, *A Short History of Scientific Ideas to 1900*, pp. 367-374; Einstein and Infeld, *The Evolution of Physics*, pp. 105-116.

[127]Einstein and Infeld, *The Evolution of Physics*, p. 86.

[128]*Ibid.*, p. 88.

[129]Mason, *A History of the Sciences*, p. 479.

[130]Hall, *The Scientific Revolution 1500-1800*, p. 349.

[131]Einstein and Infeld, *The Evolution of Physics*, pp. 47-50.

[132]Mason, *A History of the Sciences*, pp. 490-495; Singer, *A Short History of Scientific Ideas to 1900*, p.

377; Dampier, A History of Science, and its Relations with Philosophy and Religion, p. 246.

133 Mason, A History of the Sciences, p. 495; Einstein and Infeld, The Evolution of Physics, p. 55.

134 Mason, A History of the Sciences, pp. 468-473; Einstein and Infeld, The Evolution of Physics, pp. 118-119; Dampier, A History of Science, and its Relations with Philosophy and Religion, p. 219; Singer, A Short History of Scientific Ideas to 1900, pp. 427-429.

135 Einstein and Infeld, The Evolution of Physics, p. 88; Mason, A History of the Sciences, p. 477; Singer, A Short History of Scientific Ideas to 1900, p. 359.

136 Einstein and Infeld, The Evolution of Physics, pp. 125-142.

137 Dampier, A History of Science, and its Relations with Philosophy and Religion, pp. 242-243; Singer, A Short History of Scientific Ideas to 1900, pp. 363-366.

138 Einstein and Infeld, The Evolution of Physics, pp. 152-153; Singer, A Short History of Scientific Ideas to 1900, p. 367.

139 Einstein and Infeld, The Evolution of Physics, pp. 143-146.

140 Dampier, A History of Science, and its Relations with Philosophy and Religion, pp. 261-262; Einstein and Infeld, The Evolution of Physics, pp. 150-151.

141 Cassirer, The Problem of Knowledge: Philosophy, Science and History Since Hegel, trans. by William H. Woglom and Charles W. Hendel (New Haven: Yale University Press, 1950), pp. 97-114; Henri Poincare, Science and Hypothesis, trans, by W. J. Greenstreet (New York: Dover Publications, 1952), orig. 1902; The Value of Science, trans. by G. B. Halsted (London: Dover Publications, 1907), orig. 1905; Science and Method, trans. by F. Maitland (London: Dover Publications, 1914), orig. 1908; Pierre Duhem, The Aim and Structure of Physical Theory, trans. by P. P. Weiner (second edition; Princeton: Princeton University Press, 1954), orig. 1914. Duhem wrote extensively in the history of science, including Le Systeme du Monde: Histoire des Doctrines Cosmologiques de Platon a Copernic (8 volumes; Paris, 1913-1958).

[142] Mason, _A History of the Sciences_, p. 542; Singer, _A Short History of Scientific Ideas to 1900_, p. 431.

[143] Einstein and Infeld, _The Evolution of Physics_, pp. 153-245.

[144] Holton, "The Roots of Complimentarity" in _Thematic Origins of Scientific Thought: Kepler to Einstein_ (Cambridge: Harvard University Press, 1973), pp. 116-120; Einstein and Infeld, _The Evolution of Physics_, pp. 251-296; Mason, _A History of the Sciences_, pp. 550-563.

[145] Sir Oliver Lodge, quoted in Holton, _Thematic Origins of Scientific Thought_, p. 33.

CHAPTER V

PRACTICAL PROBLEMS

Critical arguments concerning the availability of social and political events to scientific treatment have taken five basic forms. It has been held that the relevant concepts are qualitative, that social events are unique, hence unavailable to generalization, that the patterns of influence among social variables are too complex to be adequately modeled, that social experiments are rarely possible, and that free will makes nonsense of the idea of behavioral laws.

Typically, these have been offered as simple impossibility arguments, in which form they have not been entirely effective. The history of science is heavy with cases in which qualitative concepts have been translated into quantitative forms, or in which new quantitative conceptual structures have replaced qualitative frameworks, without sacrificing content. Similarly, conceptual transformations have made formerly unique events available to convincing aggregation. Both of these arguments rest upon an unjustified reification of particular, familiar concepts. The idea that free will makes behavioral laws an inappropriate goal is also fairly easy to dismiss. The point is that behavioral patterns exist, not that they are inevitable. The remaining points (complexity and experimentation) pose more serious obstacles, however. It is possible to focus upon a relative few variables, while holding other factors constant, but this is most convincingly done by resort to experiments, and opportunities for these are correctly held to be rare. Alternative methods for disentangling the effects of variables are available, but they are comparatively limited. (It is worth noting, perhaps, that this difficulty is not confined to quantitative, empirical forms of political analysis. Traditional approaches are even less able to separate influences, as a direct result of the reliance upon qualitative forms.) Complexity and the limited opportunities for experiment do not make a science of politics impossible, then (there are techniques for dealing with them under at least some circumstances), but they do make it difficult.

If these arguments do not put an end to the science of politics project (least of all in the simple form in which they have characteristically appeared), they do raise serious issues. The first of these concerns the translation of the research interest into satisfactory quantitative, empirical forms. In the long run, it may be possible to formulate an entirely new framework for the analysis of political events, in which the gap between general statements and operational forms is closed somewhat. But, for the present, it is almost always necessary to impose researchable (i.e. quantitative, empirical) forms upon questions generated by, and defined in terms of, preexisting qualitative frameworks. This is rarely a simple or straightforward matter. It is essential to choose operational forms that are equivalent to (or, at worst, very much like) the concepts one would like to examine. It is essential to measure _as_ "education" that which one would like to _mean_ by it. If the point is the degree of intellectual development, or of social perspective possessed by those being studied, then one cannot settle for the number of years spent in classrooms, or the highest level of education completed (measured as primary, secondary, some college, and so forth). Similarly, if the research issue is the role of social communication, or information flow, in political development, it is misleading to consult available data sources for vaguely related items like telephones per capita. If by "communication" one refers to the quantity and quality of information that is passed between parties, with attention to large scale flow patterns in the society, then one's measure(s) of communication must give attention to precisely those things. To substitute more convenient notions is to change the meaning of the results, and. quite possibly, to drain it of import. Further, it is pointless to attempt to aggregate research outcomes that follow from different operational forms of a common issue. Research that measures different things is about different things. It is possible that three studies addressed to problems of "education" can be related to one another, but it is hardly inevitable. One cannot properly satisfy the requirements of the research process, then, unless the issue is stated clearly, and the translation to researchable forms is exhaustive and precise. This is not routinely achieved. So, while it is not correct to say that social events are unavailable in principle to quantitative treatment, it is nonetheless true that operationalization presents very considerable problems in practice.

Once the research has been put into a manageable form, it is necessary to choose a research design that will isolate the relationship at issue from confounding influences. If one ignores the (frequently considerable) potential for experimental effects, the best way to achieve this is by resort to a randomized experimental design. In practice, many complex variations have been employed, but the basic idea is quite simple. One eliminates (or at least reduces) the effects of all variables other than the one to be manipulated by making the research groups very similar. This is accomplished by making each group an adequately large random sample of the total population. Thus each group can be expected to contain very similar structures of all other variables. As a result, they can be expected to differ primarily in the degree to which they are exposed to experimental treatment. This procedure has an overwhelming advantage in social research: it reduces the impact of all potentially confounding variables, whether the researcher is aware of them or not. Unfortunately, it is rare that political research can be conducted in an experimental framework.

There are alternative means for controlling variables, but no other technique is remotely equal in its capacity to isolate the research interest. The most powerful of the alternatives controls extraneous factors by including them explicitly in a statistical model that (under at least some circumstances) has the capacity to separate effects. Technical problems may make it difficult to pull the variables fully apart, and variables are held constant in a particular sense, which is not necessarily equivalent to experimental control. But, these problems aside, it is possible to lift the research relations above the context of confounding factors. So long as the sample size is large enough (i.e. so long as the loss of degrees of freedom is not too serious), one can introduce any number of additional variables in this manner, finding the net relationships between the dependent variable and each of the independent variables with the others controlled. This requires, however, that every confounding variable be known to the researcher--a considerable problem. And the implications of errors are serious. The exclusion of even one related variable can change the outcome drastically. So there is a considerable difference between randomized experimental designs, in which variables need not be recognized to be controlled, and ex post facto designs, in which the variables must be identified, convincingly measured and explicitly included in the analysis (and in which the structure of variables must meet certain technical requirements). Again, if the traditional in principle argument is too strong, there are nonetheless very serious practical obstacles to be confronted.

Once the research issue has been translated into an operational form and a plan for the control of confounding variables has been completed, one may proceed with data collection. The first requirement is that the analysis be done with representative samples of the population (or, occasionally, with the entire population). The only way to achieve representative samples is by resort to randomization, as every other technique requires unrealistic amounts of information about relevant characteristics. The best way to proceed is by random selection from an exhaustive list of population members. Unfortunately, such lists are often unavailable. Indirect methods of random selection can be substituted, but only if samples are large enough to offset the increased probability of serious deviation from the population structure. Any technique which allows significant numbers of drop outs or nonresponses results in unrepresentative samples. So techniques like return mail, or phone, surveys are very rarely acceptable. Studies which suffer violations of the necessary random patterns pose serious interpretive obstacles.

Random samples are only representative if they are big enough. The idea is to recreate, in the sample, the relevant structure of the population. How large a sample must be to achieve this depends upon the complexity of the structure at issue. If the point is to recreate the population distribution for a three valued variable to the nearest percent (as in the case of a simple election poll) then the sample required may be quite small. One simply needs to find three proportions to two (or three) significant figures. If, however, the point is to examine the reasons for the decline in SAT scores, then an extremely complex structure maybe involved. If there were six relevant variables (plainly, there could be many more), each of which could take ten values (or ten values different enough to be consequential), then there would be 10^6 combinations of variable values. Even if many of these did not occur in the population, a very large number could remain, and recreating proportions for each of them would require a huge sample. If the goal were to reduce these structures to simple patterns (as in descriptive statistics), then a much less accurate structural "picture" of the population may be fully adequate. Still, the sample size requirements for real research problems may be much greater than is generally recognized. Rules of thumb are not an adequate substitute for informed analysis.

It is also necessary to hold down measurement error. A convincing operational form of the research interest can easily be undermined by careless use. But, again, the accurate and precise application of the measurement scales poses problems. In (roughly) descending order of concreteness, one may apply them to observed behavior, to survey information, or to documents. Rather little political behavior seems to be available to direct observation. Further, many observable situations are readily altered by the process of observation. Surveys are not necessarily more promising. The fundamental difficulty (which is shared with documents) is that one must deal with <u>reports</u> of behavior or conditions, rather than with these things directly. If in-person interviews are conducted, then it is necessary to extract reliable information from a complex interpersonal process, which is overwhelmingly likely to involve strangers. Respondents may feel that their privacy has been invaded, may be suspicious of the interviewer's motives, may be too preoccupied to give serious attention to the interview, may fear appearing stupid, may just give the visitor what they think is wanted, and so forth. Communication problems due to cultural and subcultural differences, inappropriate vocabularies, or unfamiliar topics present additional difficulties. Moreover, the information collected is a product of the very creative process of recalling the past and characterizing the present. If mail (or other impersonal) questionnaires are used, the more immediate interpersonal reactions can be avoided, but it is even more difficult to detect (or remedy the effects of) fears, suspicions, and lack of interest. And there are almost certain to be sampling problems. Documents may be better or worse, depending upon their original function (that is, their actual function, as opposed to their official role), the care with which they are kept, and whether the record keepers know they will be used. Official records of programs which are regularly evaluated on a basis of those records, for example, would be an unpromising source of "objective" data.

In sum, then, the data collection process must employ truly random samples that are big enough to be representative of the population, to meet the requirements of the inferential process, and to support the explicit introduction of the full range of relevant variables. Further, measurement error must be controlled, despite the near inevitable recourse to rather indirect measurement procedures. The translation of research interests into a set of related "indicators", which are then measured by indirect means, has very serious consequences for the precision with which relationships can be described, or even recognized.

The next step in the research process is to rearrange the data, or information implicit in the data, to bring interesting features into relief. There are no ultimate or inevitable structures; rather only the particular structures that have been embodied in particular statistical models. Linear regression analysis will not reveal relationships _per se_, rather only the kind that the model is designed to measure. If there is something going on that a flat, multidimensional "plane" fit by the least squares criterion cannot summarize effeciently, then it will be missed or distorted. So, one must know what the models can do. Further, one must understand the outcomes in substantive, rather than merely statistical, terms. It is not adequate to rely upon standard thresholds of significance (substantive or statistical) defined apart from the context of an actual case. And particularly unsatisfactory when the standard thresholds have been chosen in a circular manner. (As when a correlation of 0.3 is labelled "interesting" on the grounds that better results are rarely achieved.)

It should not be necessary to add, at this late date, that hypothesis tests are not, by themselves, very significant. Statistical inference may pose the most interesting problems for statisticians, but it provides only the most preliminary information for political scientists. Failure to achieve statistical significance means that the result could, quite plausibly, be attributed to sampling error, in which case it is pointless to analyze the sample at hand further. The rejection of the null hypothesis (i.e. the achievement of significance) simply means that the deviation from the hypothesized value is greater than one would expect from sampling error alone. If that error has been well controlled by the use of large samples, then the deviation from the hypothesized value that is required for statistical significance may be trivially small by any substantive cirterion. If one achieves significance, then, it simply means that structural analysis, conducted in accord with substantive standards may be justified. Even here, caution is required, for it is easy to imagine a situation in which a substantively interesting coefficient has a very broad confidence interval which, nonetheless, does not include the null condition. The coefficient is statistically significant and substantively strong, but there is so little certainty regarding its magnitude in the population that the outcome cannot be meaningfully interpreted. It must be added that the assumptions upon which the inferential apparatus depends are commonly violated. Under these conditions, the interpretation of test results is complicated, sometimes severely.

In short, the intelligent use of statistical techniques requires a considerable knowledge. One must understand the models structurally (knowing what the potential blind spots and weaknesses are, how to test for them, and what to do if they appear), and know how to interpret the results substantively. One must also understand the inferential process and know how to overcome the many practical obstacles associated with it. This does not seem to pose any overwhelming barriers to good research. At the same time, actual research has often been deficient.

The advantages of rigorous, quantitative analysis follow from the power of quantitative logic: precise measures of degrees of occurance can be processed through very long chains of inference with no loss of information. This means that very nonobvious structural properties can be brought into relief. For these purposes, qualitative logic is very limited, as efforts to pursue the implications of relations characterized by qualitative degrees very quickly lose their shape. Thus, "some" of "most" of "the great bulk" of "part" carries very little meaning compared with (.42)(.88)(.97)(.20). By resort to quantitative forms, then, one can reach conclusions that are simply unavailable to qualitative approaches. These include precise predictions that provide for powerful tests, and for practical utility.

This is a very considerable capability. But political scientists can take advantage of it only if the translation of the research interest into quantitative, empirical forms is convincing (including the variables to be controlled), the control of extraneous variables is successful (meaning that all relevant variables have been identified and included explicitly in the model, and there are no technical problems blocking the separation of effects), the sample is truly random, and large enough for all of the relevant structural properties to be recreated reliably in the sample, measurement error is well controlled, statistical models are employed that can convincingly summarize the structures at issue, there are good substantive grounds for choosing thresholds of interest, and the assumptions upon which the statistical model is based have been adequately satisfied.

In practice, of course, these criteria are rarely, if ever, met. It is not unusual to find loose operationalizations, inclusion of, at most, a handful of variables, small samples relative to the apparent complexity of the population, significant deviations from the required randomness, conceivably very severe measurement error, and questionnable interpretations. Even the most meticulous work is plagued by the worst of these problems, those associated with measurement and the control of confounding variables.

CHAPTER VI

CONCLUSION

The science of politics idea has taken two rather different forms in American departments. The first was a product of nineteenth century positivisms (most notably, the views of Comte and Pearson). It was expected to yield an instrumental knowledge of political events that would support a problem-solving (or, at least, a problem-ameliorating) social engineering. The second cannot be assigned unequivocal intellectual roots. It draws upon positivist rhetoric, but combines this with the expectation that a realistic understanding of political events can be produced. This latter version has been the more influential since 1950, appearing in most of the programmatic statements, and apparently being assumed in the great bulk of the research for which close ties with the science of politics ideas are claimed.

It has been the argument of this paper that the second interpretation is not, in any straightforward manner, conformable with evidence from the history of science. It appears that scientists build plausible explanations for observed events, which explanations almost always employ an abstract and symbolic conceptual apparatus that is unavailable to direct verification.[1] The tests upon which scientific research is based do not concern the realism of these conceptual structures, rather only the conformity of events to logical (and frequently rather distant) implications of the theoretical framework. Success in this regard confirms performance as a bounded predictive device, but not as a realistic portrayal of actual processes. One may believe that science converges on the truth eventually, by eliminating alternatives along the way, but this makes no practical difference, as one cannot determine the degree to which theory construction is so constrained at any point in the process. The most reasonable interpretation of scientific theory, even in the "hardest" sciences, therefore, seems to be that it does not provide a realistic understanding of events.

The instrumental version of the science of politics idea escapes this criticism, of course. It makes no claims concerning the realism of its theoretical reconstructions, rather only concerning their utility for predicting and manipulating outcomes. Most political scientists have apparently been anxious to have a better understanding of what is going on, rather than predictive and manipulative capabilities irrespective of what is going on, but some enthusiasm for this form of the argument can be expected.

Strenuous critical effort notwithstanding, no evidence concerning the prospects for an instrumental science of politics clearly puts it out of reach. Certain of the practical problems examined in Chapter V do pose significant obstacles--most notably those associated with measurement and the control of confounding variables--but they do not constitute definite barriers to effective research. The most reasonable response to this situation seems to be to wait and see whether instrumental theory is, in fact, constructed. In this regard, it is surely relevant, if hardly conclusive, to note that very little in the way of instrumental theory has been constructed to date.

It is probably also worth noting that the indicator of success in this regard must be practical performance. Good intentions and "right method" guarantee nothing; one must achieve actual predictive success. Quantification and rigor are not independent virtues that guarantee superior results. They are valuable because they provide for precise predictions and powerful tests. It is performance on the tests that matters, not conformity to methodological guidelines.

The idea that quantitative data is more "objective" seems to be based upon the idea of counting. It would, for example, seem to be more objective to say that John Smith has 374 horses, than to say that he has "quite a few" or, worse, "too many". But when numbers are assigned in accord with nonobvious rules, this advantage is lost. Quantitative measurement is just as subjective as qualitative characterization if one takes account of the choice of rules, as well as the application to cases. One can objectively decide who has the most horses by counting, but one cannot objectively decide which of the owners is more intelligent by measuring, unless the measures are obvious. One can objectively apply a measurement rule, but this guarantees nothing if the rule is questionable. Simple counting is a degenerate and misleading example, as the rules of assignment are trivial.

Nor is research necessarily meaningful or valuable just because it is conducted in a rigorous manner--by resort to careful sampling, sophisticated research design, powerful statistical tools and so forth. Given the practical limitations, results are almost certain to reflect the assumptions, compromises and errors of the research process as much as any objective circumstances. It is particularly misleading to suppose that research can adequately show the <u>non</u>existence of an expected pattern, as the most common result of poorly conceived or executed research is the attenuation of coeffecient values.[2]

It does not appear to be possible, then, to assure the reliability of one's results by sticking to the facts and being rigorous in all phases of research. Even the most carefully designed and executed studies, applied to the most tractable materials, may suffer severe bias from problems like excluded variables. And typical research is subject to many other difficulties. Again, the only legitimate means for generating confidence in the results is through practical performance--the prediction of outcomes. Anything less than predictive capability simply cannot be distinguished from speculation, as the severity of the practical problems is necessarily unknown.

In sum, then, it seems that a science of politics yielding a realistic understanding of political events cannot be expected. Nor is it reasonable to think that studies are interesting, useful, or valuable in some preliminary way simply because they employ scientific procedures (or because the theoretical apparatus has certain formal properties which are thought to prevail in physical theory). The only form in which the science of politics idea can be defended is as a means to build instrumental theory, which theory can only be established by actual predictive performance. It is not clear that even this will be possible, given the practical research problems, nor is it clear that political scientists will want it, or be able to justify it. Insofar as the science of politics idea remains at all defensible, however, it seems to be limited to this form.

Notes to Chapter VI

[1]Physical scientists have at times offered rather minimal structures of empirical regularity, as with Newton's description of gravitational effects, or Maxwell's equations. These descriptions are by no means free of conceptual bias, but they seem less indebted to abstract explanatory structures than more typical scientific theories. It is instructive that scientists (including Newton and Maxwell) have almost invariably expressed exasperation at their inability to go beyond these basic phenomenal structures to provide a conceptual explanation for what is observed. There seems to have been a fairly general assumption that scientific theory ought to allow one to think about events realistically, or as if real.

[2]Consider, for example, the organizational literature which is thought to have tested Maslow's hierarchy of needs. In a review article, M. A. Wahba and L. A. Bridwell conclude that "Some of Maslow's propositions are totally rejected, while others receive mixed and questionable support at best." ("Maslow Reconsidered: A Review of Research on the Need Hierarchy Theory," in Richard M. Steers and Lyman W. Porter, Motivation and Work Behavior, second edition (New York: McGraw-Hill, 1979), p. 52.) Some weaknesses in the research were noted, but this article seemed to be designed to show that Maslow's views could be pretty safely ruled out. An examination of some fourteen studies shows that, on the contrary, Maslow's hierarchy has not in any remotely adequate sense been tested. (The studies cited, and examined, include C. P. Alderfer, "An Empirical Test of a New Theory of Human Needs," Organizational Behavior and Human Performance (1969), pp. 142-175; Alderfer, Existence, Relatedness, Growth (New York: McGraw-Hill, 1972); M. Beer, Leadership, Employee Needs and Motivation (Columbus, Ohio: Ohio State University Press, 1966); H. P. Dachler and C. L. Hulin, "A Reconsideration of the Relationship Between Satisfaction and Judged Importance of Environmental and Job Characteristics," Organizational Behavior and Human Performance (1969), pp. 252-266; F. Frielander, "Underlying Sources of Job Satisfaction," Journal of Applied Psychology (1963), pp. 246-250; D. T. Hall and K. E. Nougaim, "An Examination of Maslow's Need Hierarchy in an Organizational Setting," Organizational Behavior and Human Performance (1968), pp. 12-35; G. Huizinger, Maslow's Need Hierarchy in the Work Situation (The Hague, 1970); E. E. Lawler and J. L. Suttle, "A Causal Correlation Test of the Need Hierarchy Con-

cept," _Organizational Behavior and Human Performance_ (1972), pp. 265-287; R. Payne, "Factor Analysis of a Maslow-Type Need Satisfaction Questionnaire," _Personnel Psychology_ (1970), pp. 251-268; L. W. Porter, "Job Attitudes in Management I," _Journal of Applied Psychology_ (1962), pp. 375-384; Porter, "Job Attitudes in Management II," _Journal of Applied Psychology_ (1963), pp. 141-148; K. H. Roberts, G. A. Walter and R. E. Miles, "A Factor Analytic Study of Job Satisfaction Items Designed to Measure Maslow Need Categories," _Proceedings of the 78th Annual Convention of the American Psychological Association_ (1970), pp. 591-592; R. H. Schaffer, "Job Satisfaction as Related to Need Satisfaction in Work," _Psychological Monographs_ (1953), Whole no. 364, p. 18; J. C. Wofford, "The Motivational Basis of Job Satisfaction and Job Performance," _Personnel Psychology_ (1971), pp. 501-518.) No study cited even comes close to operationalizing the conceptual framework at issue. In addition, no study, save Alderfer's, makes any effort to control other variables (and Alderfer's research design is completely inadequate). Samples represent rather odd populations, when they represent anything at all. And the data consists entirely of survey responses with their attendant measurement problems. (In addition, there are very serious intercoder reliability problems on the two studies that report measures.) Under these conditions, the failure to find structure in the expected places is much less a failure of Maslow's ideas than of the researchers' methods. The view that "Some of Maslow's propositions are totally rejected . . . ." seems to be correct in a rather odd sense only.

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