The Conception of Science as Deductive Formalism in the Study of International Politics: A Critique

by

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ABSTRACT

This thesis examines the conception of science as deductive formalism from two perspectives. First, it considers the philosophical foundations of two aspects of this conception: the deductive-nomological model of explanation and the hypothetico-deductive model of theory. Secondly, it considers arguments favoring the use of these models in international politics and political science and examines several deductive theories that have been put forth in international politics. It concludes that the conception of science as deductive formalism is inadequate both philosophically and practically as a model for the scientific development of international politics and recommends that scholars in this field pursue research based upon more viable alternative conceptions of science.

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It is a strange science indeed which establishes as the ultimate test of theoretical worth the rigor with which a methodological formulation is defended rather than the significance of the hypothesis advanced.

Norman Jacobson

For the basic principle of empiricism is, after all, to increase the empirical content of whatever knowledge we claim to possess.

Paul Feyerabend

There is a story of a drunkard searching under a street lamp for his house key, which he had dropped some distance away. Asked why he didn't look where he had dropped it, he replied, "It's lighter here.

Abraham Kaplan

I. INTRO UCTION

For at least the last twenty years -- perhaps we can best date it from the publication of David Easton's The Political System in 1953[1] -- political science has entertained a nearly constant 'Great Debate' concerning the methods, goals, and, occasionally, the epistemological foundations of the discipline.[2] For the most part, this debate has been conducted at the methodological level; first under the rubric of "traditionalism versus behavioralism,"[3] and more recently in terms of "behavioralism versus post-behavioralism."[4] But other fronts have from time to time been engaged, and today we can even boast, if we are so inclined, a thriving literature devoted to debate about the Great Debate.[5]

To a student of politics in the 1970s, this legacy of self-consciousness is at once challenging, perplexing, and frustrating. Within its scope have appeared some of the best thoughts available on the nature of social and political inquiry. At the same time, it has produced some of the most simple-minded, polemical, and professionally embarrassing rubbish to be found anywhere in "scholarly" writing. The challenge, of course, is to separate the wheat from the chaff, and thereby to gain some insight into the character of both political reality and the tools available for probing it. This thesis is meant to illustrate one possible way of carrying out this task.

The Great Methodological Debate has, with only a few important exceptions, exhibited a crucial flaw; it is superficial. This superficiality can be held responsible for its characteristic lack of focus, its redundancy, its confusion, and its failure to resolve itself in any fundamental way. I believe these failings are a result of the paucity of truly radical --

in the original sense of the term -- analyses of the foundations of the different methodological positions advocated in the debate. This type of analysis has only recently begun to appear, most often in the 'debate about the debate' referred to above, and I, at least, am of the opinion that we need much more of it.[6] Such analysis is by definition, philosophical. And if the subject we are concerned with is the science of politics, then the area within philosophy to which we should turn is philosophy of science. Thus, in this thesis I will examine a particular approach to the study of international politics in terms of its philosophical underpinnings.

The term "science" is problematic; we can correctly speak of different "conceptions" of science. These conceptions, I hold, are at the roots of the different methodological approaches to the scientific study of politics that are the usual terms of debate. If we can understand these conceptions and appreciate their strengths and weaknesses, I believe we can say more about the potential success of the respective approaches than we can if we merely examine the approaches themselves. The conception of science often referred to as deductive formalism constitutes an important school in philosophy of science and, from our point of view, is perhaps most prominently associated with the idea that the form and methods of the physical sciences are the proper models for emulation by the social sciences. The general principles of this conception are, with varying degrees of specificity, adhered to by many political and social scientists. The question of its adequacy as a model for inquiry is therefore an important one. This is the question I will examine in the following pages.

I have focussed my analysis specifically on only one sub-field of political science, the study of international politics. This focus does not imply any special characteristics of international phenomena, but for the most part merely reflects my substantive interests in this particular area and my desire to limit the scope of examples and reference to relevant literature. Another factor is the recent appearance among the theoretical work in international politics of a few eloquent pleas for a deductive formalist approach to the field that deserve careful examination. This focus will not, of course, prevent us from considering relevant studies that are addressed to political science generally, or even, in a few cases, to all of social science. Given the relative homogeneity of the problems confronted in the social sciences, the delimitation of focus should not be critical to the overall efficacy of the study. For one of the advantages of a radical analysis is the extensive range of applicability it can attain. Thus, this critique of deductive formalism in the study of international politics should, in its broad implications, be relevant to political science as a whole and to all of social science as well.

A. The Role of Philosophy of Science in Understanding Social and Political Political Science

1. Different Approaches to Philosophy of Science

As a field, philosophy of science subsumes quite an array of approaches and attitudes to the study of science. Or, to put it another way, there are many philosophies of science within philosophy of science. The present discussion is meant to familiarize the reader with the most important of these approaches, and to highlight the ways in which they interact, and ultimately conflict, with each other. This will hopefully clarify some of the more fundamental issues, which often remain implicit, underlying much of the debate about deductive formalism we will be concerned with further on.

At the most general level, we can denote the scope or domain of philosophy of science, and observe how it contrasts to the domain of science itself:

> The domain of any science is the material of that part of the world with which it is concerned. The language of science consists of propositions about the world. The domain of the philosophy of science is the activity of science. The language of philosophy of science consists of propositions about the activity of science.[7]

In an article devoted to a taxonomic overview of the field, Frnan McMullin notes that there are essentially two types of propositions made by philosophers of science when they address themselves to the question that is our primary concern with reference to international politics: How does one (an individual or a discipline) go about being scientific? The first type is grounded upon considerations McMullin describes as "of the phenomelogical 'don't you see that it must...' variety," whereas the second is grounded upon "a chronicle of the strategies that 'successful' scientists have followed."[8] If we were to attempt to draw a single demarcation line for the purpose of dividing philosophy of science into two camps, a line between these two statements would probably be most successful. For they lead to two quite distinct senses of science. In the first sense, science becomes "a collection of propositions, ranging from reports of observations to the most abstract theories accounting for these observations." It is, basically, "the end product of science." In the second sense, science is "the ensemble of activities of the scientist in the pursuit of his goal of scientific observation and understanding." This includes science in the first sense, but is far broader and vaguer. In fact, as McMullin points out, it would be impossible to convey this sense of science fully. "The interest in [science in the second sense] is only this, that in a very definite sense, it serves to explain how [science in the first sense] came to be formulated in the first place."[9]

From these two senses of science, the two primary approaches within philosophy of science follow. The best way to differentiate these approaches is to make explicit the warrants or justifications that each enlists in its defense. McMullin bases his taxonomy on warrants of two types; external to the practice of science, and internal. External warrants are also of two types, metaphysical and logical.[10]

A metaphysically-warranted philosophy of science takes as its starting point a general theory of knowing and being that is prior to any analysis of the actual procedures followed in science. It examines science in light of this theory. Such an approach is taken, for example, by Plato, Aristotle, and Descartes in their philosophies of science. It is clear that this type of procedure leads to propositions of the 'don't you see it must...' variety mentioned above. Although obviously important, especially in a historical sense, we will not have the opportunity to examine metaphysical philosophies of science in this study.

A logically-warranted philosophy of science also takes its justification from outside the practice of science, but in a different way. The logician, after an examination of the practice of science, reconstructs what he sees as the idealized formal version of the logic intrinsic in science. This reconstruction is sometimes normative, but most often purely rational. Its starting point is the set of rules that constitutes formal logic. The purpose of science, once the reconstruction has been achieved, is to attempt to attain as close an approximation of its intrinsic logic as possible. The propositions of philosophy are thus primarily prescriptive. This is essentially the approach to science taken by deductive formalism.

An internally-warranted philosophy of science focusses on the ongoing practice of science itself, but is more than a mere history of science. Although it relies on a description of how scientists work, this description is not produced as an end in itself. It is used to make philosophical judgements about the adequacy of scientific research, in light of the historical record. As McMullin puts it, internally warranted philosophy of science

> ...often involves a careful reading of the history of science as a <u>warrant</u> for the philophical claim made. Such work accomplishes both a historical and a philosophical goal. The writer tries to illuminate the historical instance with all the relevant philosophical analysis he can produce so that, despite its singularity, he can understand it as best he can. He also uses the documented historical instance to make a further philosophical point: it serves not merely as illustration, but as evidence for this point.[11]

This approach, involving an interaction of history and philosophy, can be seen as prescriptive as well as descriptive. Its prescriptions cannot be construed in the sweeping sense of the above two approaches, however, as its warrant is merely the activities of 'successful' scientists, not a metaphysical or logical principle. This type of approach, usually referred to as historical or contextual, is followed by many of the critics of deductive formalism to be discussed below. This is not surprising, since the most vulnerable point in the logician's programme is his conceptualization of the 'logic intrinsic in science'. In contrast, whatever is 'intrinsic' in science should be most easily discovered through a meticulous examination of its historical development.[12]

These sketches of the different approaches to philosophy of science, it seems worth stressing, are very substantial simplifications. I have presented them as such here only for the sake of illustration and easy differentiation. In fact, each approach is far from monolithic and contains many 'sub-approaches', some of which contradict each other in fundamental ways. In addition, there are even a few areas in which the approaches are quite compatible with each other in their prescriptions or descriptions. These subtleties will become more apparent when we come to a more detailed examination of deductive formalism in section II. For our present purposes -- exposing the _casus belli_ as it were -- these caricatures will suffice.

2. How Can Philosophy of Science Help Facilitate Better Social and Political Research?

Up to this point, I have said very little about the goals or underlying assumptions of this thesis. I would now like to discuss these issues more fully, and at the same time note some more general considerations concerning the relevance of philosophy of science to political inquiry. In the above discussion of the Great Methodological Debate, I posited that philosophy of science could help lead to a better understanding of the nature of political phenomena by establishing a level of analysis prior to the usual methodological one. This conclusion is shared by John Gunnell, who states that "the development of complex analytical schema and quantitative techniques is no substitute for philosophically specifying the character of social and political phenomena and the nature of explanation."[13] But merely making the claim, of course, is hardly enough. The question before us now must be: How can this better understanding be achieved? In the process of answering this question, I hope to be able to explain more precisely what this thesis is meant to accomplish.

I can identify at least three ways in which philosophical speculation benefits political research: (1) as a guard against personal biases, (2) as a heuristic device, and (3) as a means of identifying and evaluating the explicit and implicit philosophical positions of other social scientists. The last of these is essentially how I intend to employ philosophical analysis in the present study. We can consider each in turn.

(1) The 'value-free' social scientists has finally died a slow and somewhat painful death, to the relief of most. In his place we now have the 'value-conscious' social scientist, striving to make his value assumptions as explicit as possible. The point of the matter is that some values are less valuable than others, especially when they inhibit

effective research. The same holds for personal opinions. A philosophical perspective can help a social scientist ascertain the justifications for his values and opinions and evaluate them accordingly. In Eugene Miller's words:

> ...it is very difficult today for a political scientist to establish a direct relationship to the phenomena of political life and see political things as they are. From his undergraduate years, the political scientist is taught to see politics in terms of certain methodologies and conceptual frameworks. These become filters or screens that restrict and distort his vision of political reality. Before he can see political things as they are and understand them, the political scientist must identify his inherited opinions about politics and science and subject them to critical scrutiny. These inherited opinions are but residues and abbreviations of comprehensive philosophical statements that originated in the past. In order to clarify his opinions, therefore, the political scientist is forced to give attention to the history of philosophy, where speculation about politics is inseparable from speculation about the character of human knowledge. By working his way back to the source of his opinions about political things and how to study them, he is able to understand these opinions more clearly and to assess them more accurately.[14]

Although it is perhaps questionable that all opinions are inherited from comprehensive philosophical positions, it seems certain that at least some are, and that these are susceptible to careful analysis. Thus, a consideration of philosophical issues can act as a guard against personal biases. It should be noted, of course, that such consideration must be made in the spirit of self-criticism, rather than self-justification, if it is to avoid merely hardening already held opinions and values.

(2) When reading through the literature in philosophy of science, one cannot help but be struck by certain parallels between the types of problems dealt with these and those dealt with in social science. The philosophy of science is often concerned with studying the scientific community or individual scientist in terms of the social milieu within which they function. Consider the following passage from Abraham Kaplan:

> Every scientific community is a society in the small, so to speak,with its own agencies of social control. Officers of the professional associations, honored elders, editors of journals, reviewers, faculties, committees on grants, fellowships, and prizes -- all exert a steady pressure for conformity to professional standards, as their counterparts in the larger society provide sanctions for the more general norms. In certain respects scientific training functions to produce not only competence but also a kind of respectability, essential to membership in the professional community. Doctoral examinations, most candidates agree, have much in common with the tortures of initiation rites -- with the added tribulation of fear of failure: no one has ever had to repeat his Bar Mitzvah.[15]

In this context, the philosopher must face such problems, for instance, as the relative importance of 'causes' and 'reasons' in the explanation of scientific change, or the relative effects of 'internal' or 'external-environmental' factors in determining the structure of a scientific community at a given time.[16] Similar problems crop up continually in social research.

The manner in which these problems are approached in philosophy of science, therefore, can be seen as informative to the social scientist. As one example, we can consider how Stephen Toulmin relates some of his conclusions concerning the structure of science to the structure of society:

> In the case of science...the different concepts of a scientific discipline are related more loosely than philosophers have assumed. Instead of being introduced at one and the same time, and all of a piece, as a single logical system with a single scientific purpose, different concepts and theories are introduced into a science independently, at different times and for different purposes. ...This means recognizing that an entire science comprises an 'historical population' of logically independent concepts and theories, each with its separate history, structure, and implications.
>
> In sociology, likewise, the different institutions of a society are related more loosely than has recently been assumed. Instead of being intelligible only if considered all of a piece, as a single integrated 'social system', they need to be thought of in more historical terms, since they were originally established at different times and with different ends in view. ...This, too, means moving beyond the systematic theory of social structure and the revolutionary theory of social change which is its antithesis, and allowing that entire societies comprise 'historical populations' of institutions, each with its own history and internal structure.[17]

Toulmin is more forthright than most philosophers of science in noting the links between his field and social science. To a sociologist dissatisfied with the adequacy of systematic sociology, or to his political science counterpart who maintains reservations about the usefulness of the 'political system' as a focus of analysis, a comparison such as Toulmin's can prove of heuristic value in ordering his thinking about possible alternatives. Many more examples could be cited. The point is that the emphasis and techniques employed in philosophy of science are often refreshingly divergent from those in social science. As a result, new perspectives on old problems can become available, and new directions in research can be initiated.

(3) It seems safe to agree with Miller's observation that "philosophy of science is one major source of the prevailing opinions in political science about methodology."[18] Indeed, this is one important assumption underlying the present study. As a result, irrespective of one's personal views concerning either science or philosophy, at least a minimal understanding of the different positions within the field can be extremely valuable in understanding the arguments of other political and social scientists who bother to be explicit about the foundations of their methodological or theoretical approaches. In some cases, a cursory observation of which philosophers of science are cited in the footnotes of an article can reveal more about its implicit suppositions than will a careful reading of the text itself.

I cannot go so far as Miller, however, when he claims that philosophy of science "has exercised a tremendous influence over the minds of political scientists, who have little choice but to keep up with developments in the philosophy of science and choose sides in its disputes."[19] In my opinion, the penetration has not yet been nearly as complete as he envisions, and this leads to another assumption that underlies this thesis.

Gunnell has pointed out that reference to philosophers of science need not necessarily imply a firm acceptance or even understanding of their positions. "Although in the literature of contemporary social science there are frequent references to certain works in the philosophy of science and to philosophical issues related to methodology, these are more often in the context of broad pronouncements and shibboleths relating to the nature of science, its goals, and the character of its reasoning."[20] My own experience leads me to conclude that this complaint is essentially correct; many political scientists, at least, appear quick to mouth formula-like platitudes about science apparently with little consideration of their philosophical foundations. One purpose of this study, accordingly, will be to put this perception to a bit more rigorous test, and attempt to discern how prevalent this practice actually is among political scientists who claim adherence to the formal deductive conception of science.

Gunnell also makes a further indictment, a consideration of which will occupy the bulk of this thesis. He asserts that social scientists' uncritical acceptance of these simplistic pronouncements about science results from two unsubstantiated assumptions; first, that the physical sciences are the proper model for the development of the social seiences, and second, that the conception of science as deductive formalism is an adequate representation of the structure and functioning of physical science. He charges, however, that upon closer examination, both these assumptions are incorrect; that social scientists

> ...have dogmatically and superficially embraced a particular, and indeed controversial, model of science and a position on the unity of empirical inquiry advocated by certain philosophers of science without considering alternative views or the applicability of such a model to the problems of explanation with which they are confronted. In other words, the so-called "behavioral" approach in political science may be criticized from at least two perspectives: first, the adequacy of both its understanding and application of the logic of natural science, and, second, the relevance of the naturalistic approach, even if properly understood, for the explanation of social phenomena.[21]

It is clear, to return to our first point, that in order to effectively evaluate Gunnell's assertion as to the "dogmatic and superficial" character of some social scientists' use of the deductive approach to philosophy of science, we must ourselves be conversant in that field. Moreover, the outcome of such an evaluation could have a profound effect on the course of research, if not throughout political science, at least on the part of certain individuals. Thus, by identifying and appraising the philosophical positions held by other social scientists, philosophy of science can help facilitate better research.

Essentially, the present study can be seen as an attempt to carry out an examination of the two accusations made by Gunnell. This will be done in the following way: First, in section II, we will closely scrutinize the deductive formalist conception of science in order to ascertain its "relevance...for the explanation of social phenomena," especially the phenomena of international politics, as well as its relevance to the task of explanation in general. The underlying question that will guide this inquiry will be: How can the deductive model of science help students of international politics become better scientists and conduct their discipline in terms of effective and efficient scientific procedures? Next, in sections III and IV, we will turn to a consideration of political scientists' "understanding and application" of the deductive model. Section III will be concerned with the question of understanding. We will examine several prominent arguments put forth by political scientists in favor of a deductive approach either to the discipline as a whole or to selected parts of it, and attempt to ascertain the accuracy with which these arguments reflect the philosophical position they are advocating. We will be primarily interested in <u>why</u> these scholars think deductive formalism is relevant to political science, and <u>how</u> they propose the model can best be applied to specific problem areas. In section IV, we will deal directly with applications of the deductive approach to international politics. The goal here will be to discover how adequately deductive models perform in terms of making intelligible the empirical phenomena they examine. This will constitute a practical test of deductive formalism that will supplement the philosophical test in section II. It will also serve the further function of comparing the methods used by deductively-oriented political scientists to those presented in the philosophical model.

A few words are in order about why this examination will be presented in terms of a critique, as opposed to a mere appraisal or discussion. In extolling the virtues of radical analysis above, I failed to mention a difficult problem to which it is susceptible. We might refer to this as the problem of a "justificatory regress." Each level of analysis seems to imply a more fundamental level, which must be examined in order to justify conclusions reached at the first level. This second level then requires justification at a third level, and so on. Eventually, however, although the exact point is always somewhat arbitrary, we must stop justifying and accept some assumptions as self-evident, at least in our own eyes. Otherwise, we are liable to lose contact with the substantive problem that prompted the inquiry in the first place. The reason this thesis has been characterized as a critique is because, at this most fundamental level, the assumptions about science I hold as self-evident are, as far as I can tell, contradictory to the most basic assumptions of deductive formalism. So, in order to understand the thrust of this critique, these assumptions should be made explicit.

I hold as self-evident the assumption that a science, first and foremost, must be dedicated to the explication, to as great a degree as possible, of the subject matter with which it is concerned. This is the point underlying the quotes from Norman Jacobson and Paul Feyerabend which precede this introduction. It is the <u>content</u> of international politics -- no matter how elusive, analytically obstinate, or ambiguous it may be -- which must be our first priority concern. Thus, I also hold as self-evident the assumption that it is fundamentally bad science to attempt to truncate or distort a subject of inquiry in order to mold it to a preconceived notion of methodological necessity. Conversely, it follows that it is good science to continually search for new methodological techniques that better illuminate and adapt to the subject under consideration.

These assumptions, I think, also underlie the work of Abraham Kaplan, and, for this reason, the reader will note his being referred to approvingly at various points throughout this thesis. In particular, Kaplan's conceptions of scientific autonomy, logic-in-use, and reconstructed logic are relevant to the points under discussion. The autonomy of inquiry must be a fundamental principle of successful science. Kaplan defines it as follows:

> ...the various sciences, taken together, are not colonies subject to governance of logic, methodology, philosophy of science, or any other discipline whatever, but are, and of right ought to be, free and independent. Following John Dewey, I shall refer to this declaration of scientific independence as the principle of autonomy of inquiry. It is the principle that the pursuit of truth is accountable to nothing and to no one not a part of that pursuit itself.[22]

The distinction between logic-in-use and reconstructed logic is equally fundamental:

> ...scientists and philosophers use a logic -- they have a cognitive style which is more or less logical -- and some of them formulate it explicitly. I call the former the logic-in-use, and the latter the reconstructed logic. We can no more take them to be identical or even assume an exact correspondence between them, than we can in the case of the decline of Rome and Gibbon's account of it, a patient's fever and his physician's explanation of it.[23]

The important thing to remember about logic-in-use is that it is a variable concept; there are many different logics-in-use and the differences between them are not always susceptible to normative analysis. In Kaplan's words:

> That the world of ideas has no barriers, within or without, does not call for one true "logic" to govern it. The conviction that there is such a logic -- as it happens, ours -- is a parochialism like those of which comparative ethnology made us painfully aware in the course of the last century. The myth of a "natural logic", defining a universal rationality, has been penetratingly analyzed by Benjamin Lee Whorf and his successors among linguists and anthropologists. Not only language and culture affect the logic-in-use, but also the state of knowledge, the stage of inquiry, and the special conditions of the particular problem.[24]

Likewise, there are many reconstructed logics. These, however, are more susceptible to normative judgement, at least among themselves.

Their ultimate test, however, must be the success with which they reflect the logic-in-use of the activity with which they are concerned. Thus, "a reconstructed logic is itself, in effect, a hypothesis."

> As with other hypotheses, as time goes on it may become more and more awkward to "fit" the hypothesis to the facts -- here, the facts constituted by the logic-in-use. It is not a question of whether the facts _can_ be so construed, but rather whether it is still worthwhile to do so, whether the reconstruction in question continues to throw light on the sound operations actually being used.[25]

The assumptions put forth above can be restated concisely in terms of this distinction: logic-in-use must be analytically prior to reconstructed logic. I feel that if this rule is not always kept in mind, then the distinction becomes muddled, and the result can be seriously detrimental to scientific progress and the autonomy of inquiry. Again, this feeling is shared by Kaplan:

> The great danger in confusing the logic-in-use with a particular reconstructed logic, and especially a highly idealized one, is that thereby the autonomy of science is subtly subverted. The normative force of the logic has the effect, not necessarily of improving the logic-in-use, but only of bringing it in closer conformity with the imposed reconstruction. It is often said that behavioral science should stop trying to imitate physics. I believe that this recommendation is a mistake: the presumption is certainly in favor of those operations of the understanding which have already shown themselves to be so preeminently successful in the pursuit of truth. What _is_ important, I believe, is that behavioral science should stop trying to imitate only what a particular reconstruction claims physics to be.[26]

This brings us, then, to the point made in the third quote which precedes this introduction. This study was prompted by an opinion that the use of deductive formalism to pursue a science of international politics is akin to the drunkard's use of the street lamp to find his key. The light may be better for searching, but the prospects of success, which must be our ultimate concern, are better enhanced elsewhere.

Merely to say all this, of course, is only a first step to producing a convincing argument. All we have seen so far is why this thesis constitutes a critique.[27] Following a few more preliminary remarks concerning the framework within which the analysis will be made, I will attempt to develop such an argument.

B. The General Characteristics of Science: A Framework for Analysis in Terms of Form, Content, and Context

Science, I have suggested, is a problematic notion, amenable to various interpretations. This is not meant to imply, however, that these different interpretations have nothing in common. In fact, there are some characteristics of science that are accepted as legitimate by nearly all philosophers of science and scientists alike. These characteristics, it should be emphasized, cannot in any way be construed as the 'core' or 'essence' of science simply because of their ubiquity. More correctly, they can be seen as the boundaries or framework within which the different interpretations become meaningful. They are, in themselves, fairly useless in any role except as organizing devices.

The two characteristics of science I will use to organize this examination of deductive formalism are explanation and theory. This choice relies on what appears to be a general consensus that (1) explanation consists of making empirical phenomena comprehensible and is a primary goal of science, and (2) theories make explanations comprehensible, and thus contribute to this goal of science.[28] Of course, different approaches, while agreeing as to these goals, are in sharp disagreement as to how they can be attained. We can now attempt to outline a framework within which these views can be compared.

1. <u>Explanation</u>[29]

Explanation is certainly not the only goal of science; prediction and control are two others that immediately come to mind.[30] These can be considered in light of their relationship to explanation, however, since the three of them are highly interrelated. Prediction, especially, will occupy our attention further on. For now, we want to look at explanation in as broad a manner as possible in order to ascertain its general characteristics. These characteristics, then, can be employed as categories in our examination of deductive formalism. In addition, they should be capable of acting as a framework within which different approaches to explanation can be compared and evaluated.

At the most general level, we can say that a scientific explanation is a group of statements. These statements must stand in some sort of relation to each other; that is, the explanation must have a <u>form</u>. This form can range anywhere from a tight, purely logical relationship to a loose, connotative structure. Secondly, a scientific explanation must have a substantive <u>content</u>. This simply means that it must explain 'something', and this something must have a referent in the empirical world. Thirdly, the explanation must have a <u>context</u>. There seem to be two types of contexts that influence an explanation. The first might be called the <u>context of application</u>, and is the context within which the explanation makes sense. It includes the totality of 'already understood' things which give prior meaning to the terms and scope of the explanation. The second might be called the context of evaluation, and is the context within which the explanation is judged. It includes the types of rules or norms that are applicable to the explanation in various circumstances. These three characteristics -- form, content, and context -- appear to be common to all explanations,[31] and will constitute the framework within which we will examine the deductive approach to explanation.

The main problem we will be interested in is that of explanatory completeness. What, in the deductive view, qualifies as a complete explanation? How is this characterization relevant to international politics? Under what conditions are complete deductive explanations available in international politics and what research benefits can be accrued from them?

2. Theory[32]

The framework we will use to examine the deductive conception of theory is essentially the same as that used to examine explanation. Thus, the analysis will be focussed of the form, content, context of application, and context of evaluation of deductive theories. Theory, however, being a bit more complicated concept than explanation, encompasses a greater variety of problems. Therefore, in addition to the question of what constitute a complete theory, we will be concerned with the questions of how a complete theory can be built, and how it can be tested once it is built. These questions are important to the development of a science of international politics. A proper consideration of them will require an extension of the above framework in terms of a more careful differentiation of the context of evaluation of a theory.

Philosophers of science say notoriously little about the building of theories, primarily because it is often an extremely subjective process and therefore difficult to analyze systematically. The most common technique used to avoid discussion of theory building is to note that it is an aspect of the "context of discovery" as opposed to the "context of justification." These are the two primary categories within the context of evaluation. They were introduced by Hans Reichenbach in 1938 and have often proven quite valuable as analytic tools. The distinction is based on the "well-known differences between a thinker's way of finding this theorem and his way of presenting it before the public."[33] The latter is seen as susceptible to logical analysis, while the former is not. Thus, "the act of discovery escapes logical analysis; there are no logical rules in terms of which a 'discovery machine' could be constructed."[34] The delineation of the scope of the context of discovery -- that is, the decision concerning which parts of science are consigned to it and which parts are not -- varies from one approach to science to another. N.R. Hanson, for example, is particularly concerned with opening up to analysis as much of the context of discovery as possible.[35] In the examination to follow, we shall want to consider how this problem of the scope of the context of discovery is handled by the deductive formalist approach to science, especially in relation to what it can tell us about theory building, which, although analytically stubborn, is not completely immune to analysis, and is an extremely important part of successful scientific practice.

The testing of theories, in contrast, is always subsumed under the context of justification and has been subjected to several varied types of systematic analysis. The general characteristics of theory testing involve specifying an interpretation of the theory in terms of empirical referents, and then comparing this interpretation to observation or experimentation. Ideally, this comparison should result in either the confirmation or falsification of the theory. Our analysis of the means employed in testing deductive theories will, accordingly, concentrate on these two processes of confirmation and falsification.

In summary, then, I have here attempted to sketch out the common framework within which the formal deductive conception of science can be evaluated and, as the opportunity arises, compared to other conceptions of science. Its purpose is to simplify and organize the discussion that follows. Thus, fully aware of the fact that I am greatly simplifying matters, the remainder of this thesis will focus on science in terms of explanation and theory. These will be analyzed in terms of form, content, context of application, and context of evaluation. In addition, the context of evaluation of deductive theories will be analyzed in terms of the contexts of discovery and justification.

II. The Philosophical Source: Formal Deductive Models of Explanation and Theory

We can now turn to a critical examination of the conception of science as deductive formalism that is put forth in the literature of philosophy of science. As mentioned above, the discussion will be oriented around the question: How can this conception of science help facilitate a science of international politics? In general, we will find that the limitations of the approach are well understood by its proponents, and that our critique will only occasionally have to take note of the various critics within the field such as the historically-oriented philosophers of science. We will first consider the general characteristics of the approach, then focus on the specific deductive models of explanation and theory.

A. The General Characteristics of Deductive Formalism

1. Deduction

Kaplan observes that "A great deal hinges on whether science is viewed as a body of propositions or as an enterprise in which they are generated, as product or process."[36] There seems to be no better starting point for our examination of deductive formalism than to note that it is the preeminent example of the study of science as product.[37] In particular, it is concerned with the form of this product. It holds that an adequate explanation or theory must be structured hierarchically with each statement strictly deducible from those above it. This requirement is the sine qua non of deductive formalism. For it has the effect of turning what would otherwise be an unrelated system of statements into a tautology in which true premises must imply true consequences, irrespective of the content or context of either.

The deductive link is achieved through the logical ooperation of modus ponens, which Carl Hempel defines as "the 'rule of detachment', of deductive logic, which, given the information that 'D' and also 'if D then P', are true statements, authorizes us to detach the consequent 'P' in the conditional premise and to assert it as a self-contained statement which must then be true as well."[38] Thus, with reference to explanation, May Brodbeck notes that:

> If the generalizations and individual statements of fact serving as premises are accepted as true, then, because of the tautological connection, the conclusion must be true. This and this alone is the virtue of deductive explanation. Once such terms as 'must', 'guarantees', and 'logically implies' are clarified, then it is clear why deduction, and deduction alone, 'justifies' the conclusion. At the same time, it is also clear why any other kind of explanation of individual facts cannot possess this conclusiveness. Either the explanation is deductive, or it does not justify what it is said to explain.[39]

An analogous statement could be made with reference to theories; only a tautological connection between assumptions and empirically relevant theorems can justify the confirmation or falsification of a deductive theory.

2. Abstraction

The viability of the deductive link depends upon an acceptance of the fact that it constitutes an abstraction from scientific practice. This is to say, referring back to our discussion of the logical approach in section I, that the deductive explication of scientific explanations and theories is not meant to be a description of how scientists actually formulate explanations and theories, but is instead an abstract reconstruction of the logical form seen by deductivists as intrinsic in that formulation. In Hempel's words:

> ...these models are not meant to describe how working scientists actually formulate their explanatory accounts. Their purpose is rather to indicate in reasonably precise terms the logical structure and rationale of various ways in which empirical science answers explanation-seeking why-questions. The construction of our models therefore involves some measure of abstraction and of logical schematization.
>
> In these respects our concepts of explanation resemble the concept, or concepts, of mathematical proof (within a given mathematical theory) as construed in metamathematics.[40]

Abstraction is seen as necessary if generality is to be attained. Generality, in turn, allows the model to be applicable to a maximum number of contexts. Thus, for example, Ernest Nagel declares:

> It is well to bear in mind that the unusually abstract character of scientific notions, as well as their alleged "remoteness" from the traits of things found in customary experience, are inevitable concomitants of the quest for systematic and comprehensive explanations. Such explanations can be construed only if the familiar qualities and relations of things, in terms of which individual objects and events are usually identified and differentiated, can be shown to depend for their occurrence on the presence of certain other pervasive relational or structural properties that characterize in various ways an extensive class of objects and processes. Accordingly, to achieve generality of explanation for qualitatively diverse things, those structural properties must be formulated without reference to, and in abstraction from, the individualizing qualities and relations of familiar experience.[41]

The abstractness of the deductive conception of science must be of particular concern to those interested in the development of a science of international politics. For it implies that the deductive model is more an ideal 'measuring rod' for judging already existing scientific explanations and theories than it is a 'blueprint' for producing such explanations or theories in the first place. This is not necessarily grounds for rejecting the model, however, since the value of ideal-types for guiding actions is well known; at least as long as the ideal is, in fact, a true ideal for the activity which it is meant to guide. Whether the deductive model is such an ideal will require further analysis.

3. Historical Note

The philosophical roots of deductive formalism are to be found in the epistemological movement usually referred to as logical positivism or logical empiricism. Under the influence of Gottlob Frege, Bertrand Russell, Ludwig Wittgenstein and others, the movement began in Vienna in the 1920s as a reaction to the metaphysical German philosophies of the nineteenth century. This new approach advocated the logical analysis of scientific concepts, statements, and explanations as a means of achieving an objective understanding of the empirical world. Michael Scriven offers a typical brief description of the movement:

> Impressionistically speaking -- and in this area of the history of thought I do not believe we can be very precise -- one thinks of the logical positivists as attacking nineteenth-century German metaphysics and what they called psychologism in the sciences (which sometimes included Gestalt theory and always included Verstehen theory), and as upholding the analytic-synthetic distinction, the distinction between the context of discovery and that of verification, the facts-value distinction, operationalism, verificationism, phenomenalism, conventionalism, and formalism (especially in the philosophy of mathematics and in the reconstruction of scientific theories in terms of an uninterpreted calculus and correspondence rules).[42]

Unfortunately, it is beyond the scope of this thesis to trace the links between contemporary deductive formalism and the evolution of logical positivist thought, even though such a discussion would shed considerable light on the historical aspects of the issues with which we are concerned. Instead, I will merely note the lineage here, refer to some relevant literature in the footnotes, and point out particularly significant relationships as they become pertinent to topics at hand.[43]

B. Deductive-Nomological Explanation: The Covering Law Model

In this section, we want to consider the deductivist response to the question: What constitutes a complete scientific explanation? We will attempt to classify this response in terms of our four categories of form, content, context of evaluation, and context of application.

Deductive-nomological (D-N) explanation was first given explicit formulation as a reconstructed model of scientific explanation by Carl Hempel and Paul Oppenheim in 1948,[44] Since then, the model has undergone some revision,[45] as we shall note where appropriate, but as a whole is still accepted largely in its original form.[46] In their article, Hempel and Oppenheim propose four requirements for adequate scientific explanation. The first three are called "Logical Conditions of Adequacy" (R1-R3) and the fourth is termed an "Empirical Condition of Adequacy" (R4). The best way to approach the D-N model of explanation is to consider each of these requirements in turn.

1. Form

The first requirement is that deductive relationships must hold between statements in the explanation:

> (R1) The explanandum [that which is being explained; the consequent] must be a logical consequence of the explanans [the premises]; in other words, the explanandum must be logically deducible from the information contained in the explanans; for otherwise, the explanans would not constitute adequate grounds for the explanandum.[47]

This requirement merely points out the priority of logical form which we discussed above.

The second requirement is also concerned with form, but in this case it is the form of the individual statements within the explanans:

> (R2) The explanans must contain general laws, and these must actually be required for the derivation of the explanandum. We shall not make it a necessary condition for a sound explanation, however, that the explanans must contain at least one statement which is not a law....[48]

The requirement of general, or covering, laws immediately leads to the difficult question of specifying exactly what laws are. This issue has generated much controversy, not only between deductivists and critics, but among deductivists themselves. Since the nature of laws will be an important element in discussing the applicability of D-N explanation to international politics, we will take some time to consider it closely here.

Hempel and Oppenheim's conception of general law is not completely clear. They insist that a law must be true (this is taken up in R4) and universal in form. Beyond this, a law must be infinite in scope if it is "fundamental," but can be finite in scope if it is logically derived from a fundamental law. Finally, a law may contain no terms in its predicate whose meaning requires reference to any particular object or spatio-temporal location.[49] This formulation leaves much room for confusion, as the resulting debate quickly made clear. As a result, further attempts at clarification by Hempel and others have now led to a more differentiated conception of laws. Specifically, we can differentiate among perfect laws, imperfect laws, accidental universals, empirical generalizations, and tautologies.

Broadbeck has noted a distinction between perfect and imperfect laws, both of which are viewed as types of general laws:

> Any law, whether it is about physical objects, persons, or societies, is 'imperfect' if it does not permit us to compute (predict or postdict) the state of the system, either an individual or a group, at <u>any</u> moment from its state at <u>one</u> moment. ...In general, imperfect laws are indefinite with respect to time, or hedged in by qualification, or they are statistical.[50]

Obviously, perfect laws are hard to come by.[51] Imperfect laws, however, are also considered permissible in deductive explanations. According to Brodbeck, "the deductive model by no means requires that premises be the deterministic process laws of perfect knowledge. Once this is grasped, the admitted difficulty in formulating so-called universal laws...no longer appears insuperable."[52] But this still leaves the matter somewhat vague. How are <u>either</u> of these types of laws to be distinguished from other law-like linguistic entities?

The distinction between a law and an accidental universal is an important but imprecise one. It is important because, on Hempel's account, a law can serve as the basis for an explanation, whereas an accidental universal cannot.[53] It is imprecise because, in spite of this important difference, no definite means for identifying an accidental universal has yet been formulated. As a result, the distinction is usually described in terms of what accidental universals are <u>not</u>, or in terms of very simplistic examples.

Hempel notes a "telling and suggestive" difference: "a law can, whereas an accidental universal cannot, serve to support <u>counterfactual conditionals</u>, i.e., statements of the form 'If A were (had been) the case, then B would be (would have been) the case', where in fact A is not (has not been) the case."[54] To illustrate this, he notes the following examples:

> Thus, the assertion 'If this paraffin candle had been put into a kettle of boiling water, it would have melted' could be supported by adducing the law that paraffin is liquid above 60 degrees centigrade (and the fact that the boiling point of water is 100 degrees centigrade). But the statement 'All rocks in this box contain iron' could not be used similarly to support the counterfactual statement 'If this pebble had been put in this box, it would contain iron'.[55]

This distinction is not completely unambiguous, however, since the accidental universal can support <u>some</u> counterfactuals, if not this specific one. For example, the statement 'If this pebble had been <u>pulled out</u> of this box, it would contain iron' is supported by the universal.

A similar difference, according to Hempel, is that "a law, in contrast to an accidental universal, can support <u>subjunctive conditionals</u>, i.e., sentences of the type 'If A should come to pass, then so would B', where it is left open whether or not A will in fact come to pass."[56] This is represented by the statement 'If this paraffin candle should be put into boiling water, then it would melt'. But the same problem described above also applies here. In spite of these difficulties and ambiguities, however, most explications of the difference between accidental universals and laws revolve around these two arguments.[57]

Quite simply, these explications are unsatisfactory; they seem to add little to what appears to be an intuitively obvious difference between the two specific statements. The more important question is: Why does one counterfactual appear so obviously acceptable, while the other appears unacceptable? The answer, I think, involves the nature of the 'background knowledge' or context that is pertinent to each case. This can be illustrated by making some modifications to the 'rocks in the box' example. Suppose we are given the following universal statement: 'All iron rocks in this box are magnetic'. Is this a law or is it an accidental universal? Let us consider the counterfactual: 'If this iron rock had been put in this box, it would be magnetic'. The question of whether this statement is supported by the original universal is not obvious as in the previous examples. The reason is because there is a gap in our background knowledge. This gap prevents us from knowing whether the box is simply a cardboard box sitting on a table somewhere -- in which case the counterfactual is not supported by the original statement, which then must be an accidental universal -- or if the box is situated, say, within the field of a huge magnet -- in which case, because the rock would become magnetized when placed in the box, the counterfactual would be supported by the original statement, which then would be judged a legitimate law. Until the gap is our background knowledge is filled, until we know more about the location of the box, we cannot make a judgement about whether or not the universal is a law. Thus, the distinction between laws and accidental universals is highly dependent on the question of context. A law is a law because, given the context of the way we view the world, it seems to fit in with other phenomena and processes we observe. In contrast, an accidental universal seems to contradict our background knowledge. This distinction will become more clear when we come to examine further on the role played by laws in theories.

The distinction among laws, empirical generalizations, and tautologies can be explicated by a similar argument. Hanson defines the three types of statements in terms of the three dimensions of logical space: syntax, semantics, and epistemological status. Although not himself a deductivist, Hanson's argument is quite amenable to the present deductive position. Its particular value stems from the clarity with which it illustrates the problems involved in recognizing and classifying laws. Before considering Hanson's definitions, however, we must briefly note the rudimentary characteristics of the three logical dimensions.

In terms of syntax, a statement is either synthetic or analytic; that is, it is either logically consistent with its negative, or it is not. not.[58] Perhaps this is best illustrated by examples. The statement 'All Swedes have blond hair' is logically consistent with its negative 'Not all Swedes have blond hair'; therefore it is synthetic. On the other hand, the statement 'All bachelors are unmarried males' is analytic because its negative, 'Not all bachelors are unmarried males', makes no sense in light of the accepted definitions of 'bachelor' and 'unmarried male'. Thus, syntax involves the question of consistency. In terms of semantics, a statement is either contingent or non-contingent; that is, its claim to truth is either vulnerable, e.g., on the way the world is, or on the rules of the game, or on the conditions of inquiry within a given context, or it is not. The statement 'All Swedes have blond hair' is contingent on the observation of whether or not they all in fact do have blond hair; whereas the statement 'All bachelors are unmarried males' is non-contingent, its claim to truth is non-falsifiable with reference to anything outside itself. Thus, semantics involves the question of meaning or truth-content. In terms of epistemological status, a statement is either justified _a priori_ or _a posteriori_; that is, its truth is either evident by reflection alone, or it needs to be corroborated by experience. The first of our examples is obviously justified _a posteriori_ whereas the second justifies itself and is therefore _a priori_. Thus, epistemological status involves the question of justification.

According to Hanson,[59] empirical generalizations are synthetic, contingent, and _a posteriori_. 'All Swedes have blond hair', then, is an empirical generalization. Tautologies, in contrast, are analytic, non-contingent, and _a priori_. 'All bachelors are unmarried males' is a tautology. (Note again that a D-N explanation is a tautology, and therefore exhibits all these tautological characteristics.) But what about laws? The problem seems to be that laws exhibit characteristics of both empirical generalizations and tautologies. Indeed, the history of debate concerning the nature of laws has most often consisted of attempts to show that they are 'in fact' either one or the other. The present position taken by deductivists (and, for that matter, the great majority of all philosophers of science) comes down somewhere in the middle. It holds that laws are synthetic and a posteriori, like empirical generalizations. But they are also to a certain extent non-contingent, like tautologies. This non-contingency, however, is relative (unlike tautologies) and stems from the fact that a law is usually imbedded in a theoretical structure. As a result, the negation of a law would have the effect of negating the whole theoretical structure as well. So a law is usually expected to hold true, thus its relative non-contingency and its differentiation from empirical generalizations. As Scriven puts it:

> Deduction from a 'mere empirical generalization' is very rarely explanatory, and it is only because laws usually involve more than this (as well as less) that they carry explanatory force. (The fact that they commonly reflect some underlying processes, albeit imprecisely, accounts for much of the inductive reliability we ascribe to them, and hence for much of our willingness to allow contrary-to-fact inferences from them.)[60]

Like the distinction between laws and accidental universals, this distinction between laws and empirical generalizations will be clarified by our discussion of deductive theories below.

The reason it is important to be explicit about the differences between laws on the one hand and accidental universals and empirical generalizations on the other,[61] is because the charge has been made on several occasions that the so-called laws developed in social science are actually accidental universals or empirical generalizations. For example, Paul Meehl notes that many problems of social scieitific explanation result from the "incompleteness of the social scientists' nomological network."

> Underlying (derivationally and causally) the known laws of social science are the unknown ones -- the "true reasons why" the known laws are the way they are. Furthermore, very odd but true, some of the laws are, from a philosopher's viewpoint, not nomologicals but accidental universals. This is because many "laws" of biological and social science are structure-dependent and history-dependent in a special way, so that while their logical form (taken singly) is that of laws of nature, they are not derivable from the fundamental nomologicals (laws of physics).[62]

However, he also notes the same difficulty involved in recognizing accidental universals which we have been grappling with here.

> Unfortunately, it is not always easy to ascertain when a biological or social-science generalization (taken as true and well evidenced) is really akin to to "All silver melts at 960.5° C." -- a nomological -- and when it is akin to "All the coins in my pocket are silver," an accidental universal. .... [The nomological] generalization is a theorem within a formalized physical theory (and, note carefully, would be non-trivially <u>true</u> for all worlds on our world family even if no silver existed on some of them). In biology, the statement "A mammal dies if deprived of oxygen" is of this sort, since its structure dependence can analogously be represented in an adequate theoretical (anatomical + physiological) definition of 'mammal.' By contrast, the taxonomic generalization "All mammals have paired gill-slits at some stage in their development" is an accidental universal, as is "If a species of animal has a heart, it has kidneys." These taxonomic property correlations are..."historical accidents," reflecting the course of evolution which could have been different given the same fundamental nomologicals but differing initial conditions of the earth.[63]

Meehl's conception of a law bears a greater resemblance to Hempel and Oppenheim's fundamental and derived laws or Brodbeck's perfect laws than it does to Hanson's conception, which merely requires that a law be imbedded in some theoretical structure, not necessarily the theoretical structure of fundamental physics. In either case, however, the problem of accidental universals and empirical generalizations posing as laws in social science is an important one. Basically, there appears to be two ways of handling this problem, both of which are somewhat damaging to the deductive position.

On the one hand, we can accept Meehl's conception of laws. In this case, it is clear that there are no laws in social science. Moreover, it would not be implausible to conclude that, in light of this conception, laws of social science are unavailable in principle. However, we can see that it is equally clear that there are many explanations in social science (some, admittedly, far less reliable than others). Does this mean that social science is inadequate in terms of deductive explanation, or does it mean that the deductive model is inadequate in terms of explaining social phenomena? The former argument is usually put forth, but we must regard it as unacceptable because it is incompatible with the basic assumptions we outlined in the introduction to this thesis; it places reconstructed logic in a position analytically prior to logic-in-use. Thus, if indeed social explanation is presently inadequate, it must be inadequate in terms of the understanding it sheds on social reality, not in terms of its compatibility with the D-N model of explanation. As a result, we must, if we are in a position to make a choice concerning future strategies for improving social explanation, reject the strategy of trying to mold explanation to the D-N model and, rather, follow a strategy of trying to mold explanation to the contingencies of social reality. If this means developing modes of explanation that do not demand recourse to laws, so be it. This is the approach advocated by Scriven, who notes, for example, the role of truisms as opposed to laws in warranting historical explanations:

> [Truisms] are trivial, but they are not empty; and can only look like shoddy pre-scientific laws, whereas historical explanations are neither shoddy nor pre-scientific. The paradox is resolved by seeing that the sense in which good historical explanations are based on such truisms is simply that the explanations can only be denied by someone who is prepared to deny such an obviously true statement. The truism tells us nothing _new_ at all, but it says _something_ and it says something _true_, even if vague and dull. It ill fits into a deductive proof; but it has no need to do so, since the justification of an explanation is a a context-dependent _inductive_ procedure.[64]

It is not necessary to go into the details of Scriven's approach in order to stress our main point, that an acceptance of Meehl's conception of laws requires the abandonment of deductive explanation of social phenomena and the development of at least some alternative that is more relevant to the problems and characteristics of the social world.

On the other hand, an acceptance of Hanson's less stringent conception of laws still confronts us with the problem of differentiating among laws, empirical generalizations, and accidental universals. In this case, the differentiation is even more difficult, since the distinctions among the three become quite obscure, as we have already seen. Thus, the problem becomes more a practical than a theoretical one. But once we begin defining laws, and hence deductive explanations, in terms of practical considerations, we are weakening the logical foundation of the deductive approach in favor of a pragmatic one. This is a dangerous tactic because one goal of deductive formalism must be to define laws in _logical_ terms. Otherwise, there can be no guarantee that the laws are true, which means no guarantee that the deduction is justified, and therefore no guarantee that the explanation is complete. Thus, the difficulty into which this conception of laws leads us is one of logical adequacy, as opposed to the difficulty of practical adequacy which characterized the previous conception.

2. Context

The third requirement of D-N explanation involves the content of the explanans:

> (R3) The explanans must have empirical content; i.e., it must be capable, at least in principle, of test by experiment or observation. This condition is implicit in (R4); for since the explanandum is assumed to describe some empirical phenomenon, it follows from (R1) that the explanans entails at least one consequence of empirical character, and this fact confers upon it testability and empirical content.[65]

How the explanation is to be tested is a matter that is not taken up in much detail by Hempel and Oppenheim. This is because they are primarily concerned that the statements in the explanans be testable, not actually tested. The purpose of an explanation is to explain, not to be tested, and this is why (R3) is implicit in (R4). As a result, the testability condition refers to testability 'in principle', since, by the Empirical Requirement of Adequacy (R4), the statements in the explanans are required to be true in the first place:

> (R4) The sentences constituting the explanans must be true.[66]

This requirement has also led to much confusion and difficulty, as is evidenced by Hempel and Oppenheim's own discussion of it:

> That in a sound explanation, the elements constituting the explanans have to satisfy some condition of factual correctness is obvious. But it might seem more appropriate to stipulate that the explanans has to be highly confirmed by all the relevant evidence available rather than it should be true. This stipulation, however, leads to awkward consequences. Suppose that a certain phenomenon was explained at an earlier stage of science, by means of an explanans which was well supported by the evidence then at hand, but which has been highly disconfirmed by more recent empirical findings. In such a case, we would have to say that originally the explanatory account was a correct explanation, but that it ceased to be one later, when unfavorable evidence was discovered. This does not appear to accord with sound common usage.[67]

An alternative way of dealing with this problem has also been proposed by Hempel. He notes that "R4 characterizes what might be called a correct or <u>true explanation</u>. In an analysis of the logical structure of explanatory arguments, therefore, that requirement may be disregarded.[68] If we accept this modification, then the totality of D-N explanations becomes divided up into two groups somewhat as follows:

> ...in order to have obtained <u>The Scientific Explanation</u> of a phenomenon one must have arrived at the true laws of nature and the true and complete picture of the antecedent or initial conditions; whereas to have <u>A Scientific Explanation</u> is to have an account satisfying R1-R3...but not necessarily R4. At any stage in the history of science, we can properly describe mankind as having <u>A Scientific Explanation</u> of a particular phenomenon, but rarely -- if ever -- can it be claimed that <u>The Scientific Explanation</u> has been arrived at.[69]

The problem is that this dichotomization of D-N explanation simply does not work. The reason is because what has been referred to as <u>A Scientific Explanation</u> is actually no explanation at all, since it is false. As we noted above, the purpose of scientific explanation is not to be tested, but to explain. In the deductive formulation of explanation, therefore, this means that the premises must be true (as emphasized by Hempel above with reference to modus ponens). The failure to realize this leads to errors like the one involved in the following passage from Brodbeck:

> Far from requiring "exact truth" for its premises, all that the deductive model requires is exact statement of a hypothesis about this truth. The hypothesis is then tested by the "exact deduction" ....[70]

In an explanation, the premises are not tested by the deduction; the consequent is justified by the conjunction of the true premises and the deduction. To call what Brodbeck has here described an "explanation" is to completely obliterate the meaning of the term.

Thus, we are brought back to the original formulation, that adequate D-N explanation requires true premises. This requirement, Hempel and Oppenheim's reasons for advancing it notwithstanding, has been subjected to the criticism that it is simply too stringent to be applicable to any real-life scientific explanation. This is a serious criticism for, as we have seen, a logical reconstruction must take its warrant from the logic intrinsic in science, not from some a priori, metaphysical principle. If deduction from true premises is not a fair extrapolation from the practice of science, then the model's adequacy is put in serious doubt.

One variant of this basic criticism involves, once again, the nature of laws. It holds that a fundamental property of laws is that they are never literally true. Nothing in our discussion of laws above seems to definitely contradict this position, and it is interesting to note that no deductivist has ever directly challenged it. However, if accepted, it is irretrievably damaging to the D-N conception of explanation -- which demands both true premises and laws as premises -- since it renders these demands incompatible. In putting forth this specific criticism, Scrivan makes the following remarks about the nature of physical laws:

> The examples of physical laws with which we are all familiar are distinguished by one feature of particular interest for the traditional [deductive] analyses -- they are virtually all known to be in error. Nor is the error trifling, nor is an amended law available which corrects for all the error. The important feature of laws cannot be their literal truth, since this rarely exists. It is not their closeness to the truth which replaces this, since far better approximations are readily constructed. Their virtue lies in a compound out of the qualities of generality, formal simplicity, approximation to the truth, and theoretical tractability.[71]

It is worthwhile to stress that this particular criticism refers to the deductive model's role in physical explanation, not merely its role in the problematic social context. Thus, the points made here are even more general than those made with reference to laws above. In fact, they strike at the very heart of the area where the deductive approach is usually regarded as most secure; the domain of physics. We will be able to pursue this argument in greater depth in relation to the discussion of deductive theories further on.

These, then, are the four requirements of D-N explanation. The form of this type of explanation is represented in the following schema:[72]

Logical deduction	$C_1, C_2, \ldots C_k$	Statements of Antecedent Conditions	Explanans
	$L_1, L_2, \ldots L_k$	General Laws	
	E	Description of the Empirical Phenomenon to be Explained	Explanandum

An explanation that fulfills all four requirements is, on the deductive view, a 'complete' explanation. This means it includes all of the necessary and sufficient conditions of adequacy and therefore fully accounts for the phenomenon under consideration.[73]

To these criteria for complete explanation, Hempel and Oppenheim add a further proviso, which has probably had more controversy attached to it than the original four requirements combined. This addition is that "an explanation of a particular event is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the event in question. Consequently, whatever will be said...concerning the logical characteristics of explanation or prediction will be applicable to either."[74]

The initial reaction to this proviso was to point out that it must obviously be false, especially in terms of the claim that predictions could warrant adequate explanations. Elementary examples, such as the well-known difference between correlations, which can generate predictions, and causal relationships, which are the foundations of explanations, were put forth.[75] As a result, Brodbeck was prompted to attempt a clear definition of prediction, in order to distinguish it from prophecy, or non-explanatory forecasting:

> If by 'prediction' we mean any prophesy, or simply 'a claim that at a certain time a certain event will occur', then we may certainly predict without being able to understand. On the other hand, if by a prediction we mean one for which reasons can be given, then after the event we should be able to explain it.[76]

Brodbeck's definition is hardly adequate, however. A sudden drop in barometric pressure is certainly a good reason for predicting a storm, but who would be willing to say it explains the storm? Further attempts at justification fared no better, and, as a consequence, Hempel finally modified the original position:

> ...the thesis of structural identity amounts to the conjunction of two sub-theses, namely (i) that every adequate explanation is potentially a prediction...(ii) that conversely every adequate prediction is potentially an explanation. ...I will argue that the first sub-thesis is sound, whereas the second one is indeed open to question.[77]

With this modification, the thesis of a logical symmetry between explanation and prediction (but not the thesis that complete explanation can imply prediction) is set aside.

Although getting ahead of ourselves a bit, it seems propitious at this time to make a point with reference to the question of the accuracy of political scientists' understanding of the D-N model of explanation. It seems that many political scientists, including some of the deductively-oriented scholars we will consider in section III, have failed to note this important modification in the deductive position concerning the relationship of explanation and prediction. For example, in an article we will examine further on (written in 1972; eight years after Hempel's retraction), Oran Young rather glibly informs us that "many investigators pursue the development of theories from an interest in explanation rather prediction, even though the logical status of explanation and prediction is treated as identical by most philosophers of science."[78]

Likewise, Davis Bobrow, in another article in the same volume, remarks:

> Explanation and prediction have the same structure but different starting points. When we explain something, we start from an observed outcome and work backwards to explain it. ...When we predict something, we start with a set of principles and work out the consequences they imply.[79]

These formulations are neat and simple, but incorrect. As such, they are reminiscent of the "broad pronouncements and shibboleths" referred to above by Gunnell (p. 12) in that they fail to reflect an adequate understanding of the model they claim to represent.[80] It seems a fair guess that many political scientists focus on predictive, rather than explanatory, criteria for their research designs not only because of the practical consideration that predictive hypotheses are easier to verify, but also because of this erroneous assumption that prediction, if successful, will suffice as explanation. For now, we can only say that this assumption is certainly not warranted by the D-N model of explanation as it is presently formulated.

The aspect of the explanatory-predictive thesis that is still defended by the deductive approach -- that every adequate explanation is potentially a prediction -- has also come under attack. At its most superficial level, this criticism notes such obvious points as the fact that the explanation of laws, which presumably hold at all times, can have no meaningful counterpart in terms of a prediction.[81] At a more fundamental level, it maintains that scientists sometimes employ completely adequate explanatory arguments that have no capacity, in principle, for being converted into predictions. The debate surrounding this claim has often focussed on the question of whether the explanatory account of organic change put forth in Darwin's theory of evolution is legitimately scientific, since this account is almost universally understood to be non-predictive. Toulmin, for example, observes that "No scientist has ever used this theory to foretell the coming-into-existence of creatures of a novel species, still less verified his forecast. Yet many competent scientists have accepted Darwin's theory as having great explanatory power."[82] Similarly, Scriven concludes:

> The most important lesson to be learned from evolutionary theory today is a negative one: the theory shows us what scientific explanations need not do. In particular it shows us that one cannot regard explanations as unsatisfactory when they...are not such as to enable the event in question to have been predicted.[83]

The deductive response to this critique is to contend that Darwinian explanation is not a 'complete' scientific explanation. Thus, Hempel writes:

> The story of evolution might tell us, for example, that at a certain stage in the process dinosaurs made their appearance and that, so much later, they died out. Such a narrative account does not, of course, explain why the various kinds of dinosaurs with their distinctive characteristics came into existence, nor does it explain why they became extinct. Indeed, even the associated theory of mutation and natural selection does not answer the first of these questions, though it might be held to shed some light on the latter. Yet, even to account for the extinction of the dinosaurs, we need a vast array of additional hypotheses about their physical and biological environment and about the species with which they had to compete for survival. ...The undeniably great persuasiveness of Toulmin's argument would seem to derive from two main sources, a widespread tendency

> to regard the basically descriptive story of evolution as explaining the various states of the process, and a similarly widespread tendency to overestimate the extent to which even the theory of mutation and natural selection can account for the details of the evolutionary sequence.[84]

We are, of course, in no position to evaluate Hempel's charge as to the logical inadequacy of the Darwinian explanatory model. This is an issue which must be left to biologists and paleontologists, not political scientists. There is ample evidence, however, that Darwinian explanation is accepted as a legitimate, or even preeminent, form of explanation by many scientists who are familiar with it.[85] Thus, we are left, once again, with the question of deciding which holds priority in cases of divergence, the reconstructed model of the "logic intrinsic in science" or the explanatory procedures employed by practicing scientists. In coming down on the side of procedures rather than formal logic, we take a position similar to Toulmin's, who responds to Hempel's analysis of evolutionary explanation as follows:

> The Darwinian account of the origin of species... could be matched against his formal models only by the unrealizable requirement that we know vastly more about the prehistoric course of events than we actually do know. Does this mean so much the worse for the Hempelian models? No, it means so much the worse for the Darwinian theory. ...Note that by refusing the term "explanation" to the theory of mutation and natural selection Hempel has in mind no shortcomings of a biological nature. All he objects to is the failure of paleontological reasoning to conform to his own a priori patterns.[86]

3. <u>Context of Evaluation</u>

We have already noted several times that the problem of evaluation or testing is of only minor importance to the D-N model of explanation.

This is because a complete D-N explanation forms a syllogistic tautology (assuming all four of its requirements are met) and, as we have seen, the truth-content of a tautology is non-contingently determined. There is, however, a distinction made by deductivists with reference to explanations that bears a resemblance to the discovery-justification distinction made with reference to theories. This is the distinction between the logical and pragmatic contexts of an explanation. As Hempel describes it:

> ...scientific research seeks to account for empirical phenomena by means of laws and theories which are objective in the sense that their empirical implications and their evidential support are independent of what particular individuals happen to test or apply them; and the explanations are meant to be objective in an analogous sense. This ideal intent suggests the problem of constructing a non-pragmatic concept of scientific explanation -- a concept which is abstracted, as it were, from the pragmatic one, and which does not require relativization with respect to questioning individuals any more than does the concept of mathematical proof.[87]

The pragmatic elements of an explanation are essentially its psychological and contextual elements. They involve such expressions as 'realm of understanding' and 'comprehensible', which cannot be incorporated into a logical analysis because they vary from one individual to another.[88] Thus, these aspects are separated from those which can be logically analyzed. As we have seen, however, to the extent that laws cannot be defined logically, pragmatics still influence D-N explanations. On the other hand, to the extent that the analysis remains logical, this distinction allows us to account for some of the abstract character of the model.

4. Context of Application

All four of the requirements of D-N explanation are confined to the areas of form and content. By now it should be clear that the primary purpose of the deductive approach is to nullify the effect of context on its model of scientific explanation in order to make it equally applicable at all times and in all places. This goal, or the acceptance of its feasibility, is the assumption underlying the deductivist view of the symmetry of physical and social science. The difference between the two is in terms of the context within which they operate. If these contexts can be rendered irrelevant, then physical and social science become logically indistinguishable. As Brodbeck makes clear, "Virtually all those who accept the deductive model hold that it applies not only to physical but also to human phenomena, whether individual or social, whether in the past or in the present."[89]

The goal of context-invariance was inherited by deductive formalism from its progenitor logical positivism. Toulmin notes the importance of this goal in the work of both Frege and Russell:

> For the early Russell, as for Frege, concepts and propositions remained ideal, timeless entities which were captured at best incompletely by the colloquial words and sentences employed at one or another moment in history. The true character of the timeless entities could be displayed only in logical terms, as a system of necessary relations; this meant that philosophers must develop a logical symbolism and calculus, by which to extend Frege and Peano's treatment of arithmetical concepts, first to mathematics as a whole, and then to the remaining concepts of natural science and practical life. In this way, one might finally separate off the philosophical analysis of concepts proper, which is aimed at a formal system of

> necessary relations, both from the historical study of changes in our collective conceptions and word-meanings, and from the psychological study of intellectual development in the individual. In this way alone we would be sure of escaping the twin heresies of 'psychologism' and the 'genetic fallacy'.[90]

This project envisioned by logical positivism was, as Toulmin goes on to show, doomed from the start. Its fundamental flaw lay in its inability to justify its own claim to universal authority:

> Universal authority may be claimed for an abstract, timeless system of 'rational standards', only if it has first been shown on what foundation the universal and unqualified authority rests; but no formal schema can, by itself, prove its own applicability. Until the problem of authority is dealt with, our capacity to construct alternative logical systems is limited only by our formal ingenuity. Given these alternatives, we must then face the additional question, 'Why are we to accept this formal analysis, rather than that?' -- a question which is, evidently enough, concerned less with the internal consistency than with their power to throw light on the merits of substantive arguments.[91]

Thus, present-day deductivists, in contrast to their forebears, are careful to emphasize the practical benefits of the approach. Hempel, for example, notes that the D-N model "is not, of course, susceptible to strict "proof"; its soundness has to be judged by the light it can shed on the rationale and force of explanatory accounts offered in different branches of empirical science."[92] Further, Hempel holds that although D-N explanation itself is meant to be context-invariant, the decision as to whether or not a particular phenomenon can be explained deductively is context-dependent, and therefore subject to pragmatic judgement.[93] We can conclude, accordingly, that the area in which invariance is felt to be achieved is still quite small. We can also note, however, that much of the abstractness that characterizes the deductive approach can be seen as a by-product of the effort to obtain even this small degree of invariance.

It would be a mistake to assume that these indicators of the cautiousness of modern deductivists imply that the goal of context-invariance is no longer pursued. On the contrary, the quest for scientific unity under the deductive banner is still very much alive. The carefully composed formulas of the most influential and knowledgeable adherents to the approach contrast strikingly with the brash slogans of their more zealous followers, some of whom we will meet in our discussion of deductively-oriented political scientists in section III.

C. Hypothetico-Deductive Theory

Unlike D-N explanation, hypothetico-deductive (H-D) theory is not the product of a singular, generally recognized formulation. As Alan Ryan points out, "In the case of elucidating the nature of scientific theories, there is no general consensus to be found, in spite of an enormous literature seeking to unravel the distinction between theories and such close relations as models, maps, metaphors, and analogies."[94] In order to minimize the effects of this lack of consensus, I will rely heavily on the formulations of the more influential deductivists; primarily Hempel, and secondarily Braithwaite, Nagel, and Popper. Of course, there are also differences of opinion here which we will consider, but they are minor in comparison to the overall area of agreement. With these caveats in mind, we can turn to an examination of H-D theory in terms of the relative roles of form, content, context of application, and context of evaluation.

1. Form

A good starting point for this discussion is a consideration of the link between H-D theory and D-N explanation. Willard Humphreys notes that hypothetico-deductive theory "is a natural and plausible extension of the Deductive-Nomological pattern of explanation if we assume that theories are comprehensive systems of scientific explanations."[95] This assumption is fundamental to the deductive view of theories. Hempel, for example, describes theory in a general sense as follows:

> Theories are usually introduced when previous study of a class of phenomena has revealed a system of uniformities that can be expressed in the form of empirical laws. Theories then seek to afford a deeper and more accurate understanding of the phenomena in question. To this end, a theory construes those phenomena as manifestations of entities and processes that lie behind or beneath them, as it were. These are assumed to be governed by characteristic theoretical laws, or theoretical principles, by means of which the theory then explains the empirical uniformities that have been previously discovered, and usually also predicts 'new' regularities of similar kinds.[96]

In this sense, the purpose of theories is often summarized by the familiar formulation, "Statements of individual fact are explained by laws; laws are explained by theories."[97] This explanation is achieved, in part, by the deductive subsumption of the laws under the overarching principles of the theory. Thus, H-D theories constitute, in Kaplan's terminology, a hierarchical conception of theory:

> A hierarchical theory is one whose component laws are presented as deductions from a small set of basic principles. A law is explained

> by the demonstration that it is a logical consequence of these principles, and a fact is explained when it is shown to follow from these together with certain initial conditions. The hierarchy is a deductive pyramid, in which we rise to fewer and more general laws as we move from conclusions to the premises which entail them.[98]

Or, in Braithwaite's words, "a scientific system [theory] consists of a set of hypotheses which form a deductive system; that is, which is arranged in such a way that from some of the hypotheses as premises all the other hypotheses follow."[99]

This aspect of H-D theory -- the formal aspect -- is closely analogous to the corresponding formalism of D-N explanation. Thus, in the present discussion we will have the opportunity to note similarities between the two models. However, as we mentioned above, a theory is a bit more complicated than an explanation and, as a result, in our discussions of content and context we will see how theories differ from explanations in fundamental ways.

The formal deductive properties of H-D theories allow us to extend our examination of the nature of laws to the theoretical context. We can see now more clearly what was meant by the statement that laws, in contrast to empirical generalizations and accidental universals, exhibit a relative non-contingency with respect to the theory they help constitute. This non-contingency results from their being 'locked into' the broader deductive network of the theory. As Hempel puts it:

> ...A statement of universal form, whether empirically confirmed or as yet untested, will qualify as a law if it is implied by an accepted theory (statements of this kind are often referred to as theoretical laws); but even if it is empirically

> well confirmed and presumably true in fact, it will not qualify as a law if it rules out certain hypothetical occurrences...which an accepted theory qualifies as possible.[100]

Thus, the difference between laws and other law-like statements cannot be discerned by an examination of the syntactical elements of the statements alone. It requires the examination of the theoretical context within which the statement stands. 'Background knowledge', then, in the deductive understanding of the term, must refer to the formal deductive theory that underlies a given law. In this way, the contextual elements of an explanation can still be held to be in the domain of logical, as opposed to psychological or historical, analysis.

In the traditional deductive view of H-D theories, the formal structure of the theory -- usually referred to as the calculus -- is held to be analytically prior to the empirical content of the theory. As Hanson describes it:

> ...a scientific theory is (ideally) a deductive structure, an inferential reticulum, an algorithm, a physical interpretation of which is explicitly brought about by coupling terms and formal properties of the algorithm to objects and processes within a subject matter. Interpreting a theory is something done to formally finished frameworks. It is something clamped onto a theory; or perhaps it is "hooking" of theory to subject matter somewhat like the hooking of wire mesh over the frame of what will become a modeled statue.[101]

This formulation, however, has recently come under attack from deductivists and non-deductivists alike. For example, Hempel points out that overemphasis on axiomatization (the process of formalizing a calculus) leads to a disregard of the substantive import of the theory.

"Generally speaking, the formalization of the internal principles as a calculus sheds no light on...interpretation. It sheds light at best on part of the scientific theory in question."[102] Further, in response to the claim that axiomatization makes explicit the foundation assumptions of a scientific theory, Hempel notes that:

> ...axiomatization is basically an expository device, determining a set of sentences and exhibiting their logical relationships, but not their epistemic grounds and connections. A scientific theory admits of many different axiomatizations, and the postulates chosen in a particular one need not, therefore, correspond to what in some more substantial sense might count as the basic assumptions of the theory; nor need the terms chosen as primitive in a given axiommatization represent what on epistemological or other grounds might qualify as the basic concepts of the theory; nor need the formal definitions of other theoretical terms by means of the chosen primitives correspond to statements which in science would be regarded as definitionally true and thus analytic.[103]

A similar, but perhaps more far-reaching, criticism is made by Hanson, who also holds that formalism cannot be analytically prior to interpretation:

> It is not a question of meaning being pumped up (or "seeped up" or "zipped up") through an already designed algebraic formalism. Rather, the facts of scientific life require us to attend to formalisms being pumped up...from algebraic expressions already rich with meanings, charged with structural representations of phenomena, and informative as to what matters most within our observational encounters in...science.[104]

Hanson goes one step further, however, and declares that the formal and interpretive aspects of a theory are, in fact, indissoluble, and that any attempt to analytically separate them must perforce fail to achieve a full understanding of the theory:

> To restrict one's philosophical attention, focusing now only on syntactical structure, and later on the host of semantic issues involving interpretation and meaning -- this is to have failed to recognize that "physical theory" is an <u>indissolubly</u> complex concept to begin with. ...But complexity is not confusion. When analysis results in destroying complexity in the name of clearing up confusions, to that extent it destroys the concept in question. It slices it out of existence. To talk of the formalism within a physical theory is not to be talking about the <u>physical</u> theory itself. To discuss only the "interpretation" of the theory is also not to be discussing the theory itself... . To chop theories apart into <u>formalism</u> and <u>interpretation</u> -- and then to identify only the formalism with the "theory" -- <u>is</u> the simple mistake of misplaced discreteness.[105]

The reason Hanson can take this further critical step is because he is discussing scientific theories as produced by practicing scientists. Hempel, on the other hand, is discussing a certain reconstruction of scientific theories which, in his opinion, is problematic <u>as</u> a reconstruction, not as a theory. He does not specifically challenge the formalism-interpretation distinction here; he merely questions the <u>emphasis</u> on formalism. We will see further on, however, that other aspects of Hempel's latest position could be construed as rejecting this traditional deductivist distinction.

These two criticisms are particularly valuable to our present concerns in that they illustrate quite well the tenuous relationship between scientific theories and axiomatized reconstructions of those theories. This tenuousness is worth stressing here, since it is important not to make the mistake of confusing the two when considering the question of theoretical structure in international politics. Hempel, who is always careful to avoid confusion, is helpful on this point. He emphasizes, for example, that reconstruction "was never claimed to provide a descriptive account of the actual formulation and use of theories by scientists in the ongoing process of scientific inquiry; it was intended, rather, as a schematic explication that would clearly exhibit certain logical and epistemological characteristics of scientific theories."[106] Others have not been so careful, however, and as a result there have been several cases of attempts to 'put the cart before the horse' and develop axiomatizations of sciences for the purpose of making prescriptive recommendations concerning the future conduct of inquiry within those sciences. One such case is J.H. Woodger's attempts to axiomatize biology.[107] Toulmin concludes that at least the part of this axiomatization which was meant to apply to genetics "could have been -- if his professional colleagues had taken serious notice of it -- a major obstacle to progress in the science."

> Like a good logical empiricist, he interpreted theoretical statements in genetics as universal or statistical correlations between observable macroscopic characters in animal populations. Next, he set about redefining the term "gene" as an "intervening variable" in the system of formal theorems linking such observable characters. In this way he played down almost entirely the cytological and biochemical theories from which genetics had so much to gain. Even heuristically axiomatization was in this case a handicap rather than a help.[108]

In summary, then, it appears that when H-D theories have been oriented primarily toward formal criteria of adequacy, the empirical elements of the theories have tended to atrophy. Hanson has put it well:

> Indeed, there may be an inverse relationship between the degree of formal elegance built into a...theory and the possibility of applying it to really intricate phenomena at the frontiers of research. If the investigator's eye is fixed upon perplexities within the subject matter, his theory is sure to be syntactically inelegant. But, if his concern is with formal elegance, attention will not long remain on the intricacies within the data themselves.[109]

2. <u>Content</u>

In discussing how H-D theories handle the problem of content, we will take as our standard Hempel's latest ideas on the subject. These ideas involve, as we have just seen, a shift in emphasis from the formal to the substantive aspects of theories. As such, they differ in important respects from his, and most other deductivists', earlier formulations. Since this shift is basically in favor of the priorities advocated in the introduction to this thesis, we will be particularly interested in ascertaining why, and to what degree, Hempel has changed his mind.

Theories, on Hempel's account, contain two kinds of principles; internal principles and bridge principles. Internal principles "serve to characterize the theoretical setting or the 'theoretical scenario': they specify the basic entities and processes posited by the theory, as well as the theoretical laws that are assumed to govern them." Bridge principles, on the other hand, "indicate the ways in which the scenario is linked to the previously examined phenomena which the theory is intended to explain."[110] These two types of principles are expressed in terms of two types of vocabularies:

> The formulation of the internal principles will typically make use of a <u>theoretical vocabulary</u> V_T, i.e,, a set of terms not employed in the earlier

> descriptions of, and generalizations about, the empirical phenomena which T [the theory] is to explain, but rather introduced specifically to characterize the theoretical scenario and its laws. The bridge principles will evidently contain both the terms of V_T and those of the vocabulary used in formulating the original descriptions of, and generalizations about, the phenomena for which the theory is to account. This vocabulary will thus be available and understood before the introduction of the theory, and its use will be governed by principles which, at least initially, are independent of the theory. Let us refer to it as the <u>pre-theoretical, or antecedent, vocabulary</u>, V_A, relative to the theory in question.[111]

Internal principles, although primarily employing theoretical terms, also make use of the pre-theoretical vocabulary. Thus, they involve a measure of empirical content that is unavailable to the axiomatized calculus discussed above. Hempel distinguishes between the two concepts as follows:

> The assumption, in the standard construal, of an axiomatized uninterpreted calculus as a constituent of a theory seems to me...to obscure certain important characteristics shared by many scientific theories. For that assumption suggests that the basic principles of the theory -- those corresponding to the calculus -- are formulated exclusively by means of a "new" theoretical vocabulary, whose terms would be replaced by variables or by dummy constants in the axiomatized calculus....Actually, however, the internal principles of most scientific theories employ not only "new" theoretical concepts but also "old," or pre-theoretical, ones that are characterized in terms of the antecedent vocabulary. For the theoretical scenario is normally described in part by means of terms that have a use, and are understood, prior to, and independently of, the introduction of the theory.[112]

Hempel's modification of the calculus concept, then, has the effect of anchoring the empirical import of the theory directly to its most 'theoretical' principles. This can be seen as an important step in the direction of recognizing the indissolubility of form and interpretation advocated by Hanson above. Moreover, it is an important step in lessening the gap between a theory and its logical reconstruction.

Still, internal principles alone are not adequate to constitute a theory. To the extent that they are theoretical (abstract or defined in terms of nonobservables), they must be explicitly linked to observable empirical phenomena. This is the role of bridge principles. "Without bridge principles...a theory would have no explanatory power...it would also be incapable of test."[113] In illustrating how bridge principles achieve these purposes, Hempel resorts to examples from physical science. We can consider two here:

> In the classical kinetic theory of gases, the internal principles are assumptions about gas molecules; they concern their size, their mass, their large number; and they include also various laws, partly taken over from classical mechanics, partly statistical in nature, pertaining to the motions and collisions of the molecules, and to the resulting changes in their momenta and energies. The bridge principles include statements such as that the temperature of a gas is proportional to the mean kinetic energy of its molecules, and that the rates at which different gases diffuse through the walls of a container are proportional to the numbers of molecules of the gases in question and to their average speeds. By means of such bridge principles, certain micro-characteristics of a gas, which belong to the scenario of the kinetic theory, are linked to macroscopic features such as temperature, pressure, and diffusion rate; these can be described, and generalizations concerning them can be formulated, in terms of an antecedently available vocabulary, namely, that of classical thermodynamics. And some of the features in question might well be regarded as rather directly observable or measurable.[114]

Bridge principles, however, do not always link nonobservables and observables. Specifically, they link the "new" vocabulary of the theory in question to an antecedently available vocabulary. This antecedently available vocabulary need not describe observable phenomena; it may just as well describe other nonobservables which are held to be understood because of their position within another, different, well accepted theory. For example, when discussing Bohr's early theory of the hydrogen atom, Hempel notes that the bridge principles of the theory connect theoretical entities ('atom', 'electron') to the wavelengths of certain lines in the emission spectrum of hydrogen. "These wavelengths are not observables in the ordinary sense of the word, and they cannot be simply of directly measured as, say, the length and width of a picture frame of the weight of a bag of potatoes."

> Their measurement is a highly indirect procedure that rests on a great many assumptions, including those of the wave theory of light. But in the context we are considering, those assumptions are taken for granted; they are presupposed even in just stating the uniformity for which a theoretical explanation is sought. Thus, the phenomena to which bridge principles link the basic entities and processes assumed by a theory need not be "directly" observable or measurable: they may well be characterized in terms of previously established theories, and their observation or measurement may presuppose the principles of those theories.[115]

Just as internal principles are introduced because of the difficulties inherent in the axiomatized calculus, so bridge principles are introduced as an alternative to the other main constituent of the traditional H-D conception; the correspondence rules. Hempel's critique of correspondence rules is especially pertinent to the concerns of this thesis, since it highlights some very important limitations of the standard deductive approach.[116]

Correspondence rules have usually been conceived as "a class of sentences that assign empirical content to the expressions of the calculus; and their designation as operational definitions, coordinative definitions, or rules of correspondence conveys the suggestion that they have the status of metalinguistic principles which render certain sentences true by terminological convention or legislation."

> The sentences thus declared true -- let us call them interpretive sentences -- would belong to an object language containing both the calculus and the pre-theoretical terms employed in the interpretation. The theoretical terms in the calculus are then best thought of as "new" constants that are being introduced into the object language by means of the correspondence rules for the purpose of formulating the theory. The interpretive sentences might have the form of explicit definition sentences (biconditionals or identities) for theoretical terms, or they might be of a more general type....But at any rate, they would be sentences whose truth is guaranteed by the correspondence rules.[117]

The problem with this formulation, in Hempel's view, is that once the truth of a statement is guaranteed by definition, the statement becomes impervious to empirical test and subsequent revision or rejection. The presence of such statements in the deductive reconstruction of theories does not match well with the situation within actual theories, where "scientific statements that are initially introduced by 'operational definitions'...usually change their status in response to new empirical findings and theoretical developments."[118] Here,then, truth by convention is inappropriate and Hempel approvingly quotes Quine, "conventionality is a passing trait, significant at the moving front of science, but useless in classifying the sentences behind the lines."[119] Another way to interpret this criticism is to say that Hempel is rejecting the analytic-synthetic distinction by maintaining that analytic (true by definition) statements are seldom found in real-life theories. "For, with the possible exception of the truths of logic and mathematics, no statement enjoys this kind of absolute immunity."[120] Thus, once again, Hempel is shifting the emphasis away from the formal (analytic) aspects of the deductive reconstruction to its empirical (synthetic) aspects. Bridge principles, as opposed to the interpretive sentences generated by correspondence rules, are empirically contingent and susceptible to test.

Before proceding to a consideration of some of the implications of Hempel's reformulation as a whole, one point should be mentioned here which will be developed later when we come to examine the context of justification of H-D theories. This is the question of the truth-content of laws and the relationship of H D theory and D-N explanation. From this discussion of correspondence rules and Hempel's critique of the feasibility of truth by convention, it follows that no law can ever be proclaimed true by convention. Moreover, it follows that the truth of a law cannot be guaranteed by the fact that it is contained within the deductive reticulum of a theory. This may be a necessary condition, but it is not a sufficient one. A further requirement, of course, is that the law undergo empirical verification (which is the topic we will take up with reference to justification). For now, we can note that this dual process of theoretical and empirical substantiation must be capable of proving a law to be true. Otherwise, a potential contradiction is created between the H-D and D-N models, since the latter demands true laws in order to adequately explain phenomena.

With this point in mind, we can now turn to the critical implications of Hempel's reformulation of the content of H-D theories.

First, it appears that the formalism-interpretation distinction has been decisively rejected. As we have seen, both internal principles and bridge principles contain theoretical as well as antecedently available terms. As a result, Hempel's new construal is better suited to the task of accurately reflecting the formal and interpretive aspects of real-life theories than is its deductive predecessor.This accuracy, however, has been purchased at a high logical price. As Hempel makes clear, the new distinction is not precise:

> ...it should be explicitly acknowledged...that no precise criterion has been provided for distinguishing internal principles from bridge principles. In particular, the dividing line cannot be characterized syntactically, by reference to the constituent terms... .Nor is the difference one of epistemic status, such as truth by convention versus empirical truth. The distinction is, thus, admittedly vague.[121]

In order to introduce realism into the deductive model, Hempel has had to sacrifice analytic rigor. This does not speak well for the adequacy of the former model's conceptualization of the "logic intrinsic in science." In fact, to the extent that it eschews logical distinctions, Hempel's reformulation does not speak well for the adequacy of the logical approach in general.[122]

A second implication of Hempel's model follows from his inclusion of antecedently available terms in the vocabulary of the internal principles of a theory. This inclusion seems to imply that the unit of meaningful analysis of the deductive approach should be shifted from the individual theory to the family of theories which contribute meaning to the terms employed in that individual theory. For once it is admitted that at least some of the terms which are central to the meaning of a given theory are defined in relation to other theories, then it follows that this theory cannot be fully understood, and therefore logically evaluated, except in conjunction with an equivalent understanding and evaluation of those other theories. Thus, no theory can be understood in isolation.

This implication leads to some difficult problems for the application of deductive analysis to social science in general or international politics in particular. As we have emphasized repeatedly, the deductive model is a tool for evaluating the logical adequacy of existing theories, not a guide for producing future theories. As such it may be applicable to certain isolated deductive theories in international politics (Indeed, we will be pursuing this notion in section IV.) However, in none of these theories can it be said that all the terms involved in their internal principles are antecedently defined in other, equally formal, theories. On the contrary, the semantic sources of most terms employed in these theories are explicitly 'common-sense' at best, and hopelessly obscure at worst. For such isolated theories, on Hempel's new account, an adequately full deductive evaluation is impossible. Further, it appears there will be a long wait before the required theoretical interrelatedness is attained in any social science outside of economics. The question is: Do we wait until the prerequisites for deductive analysis are met or do we begin looking for other evaluative tools more closely attuned to the problems of social science theories?

3. Context of Application

Whereas in our analysis of the D-N model of explanation we emphasized the context of application over the context of evaluation, just the opposite approach is required in the present analysis. Thus, here we have very little to say that has not already been said with reference to explanation. The goal of context-invariance is equally applicable to the H-D formulation. Moreover, the purpose of this goal is the same; to guarantee the formal indistinguishability of physical and social theories and thereby to unify the logical structure of all empirical science. And, as we have seen, the goal still appears to be far from attained.

4. Context of Evaluation

The importance of the context of evaluation to the H-D model of theory reflects a fundamental difference between this model and the D-N model of explanation. Quite simply, the difference is that hypothetico-deductive theories are hypothetical. They are subject to test, and to subsequent revision or rejection if necessary. D-N explanations, on the other hand, must be testable in principle, but, as we have noted, are not expected to fail such a test since their premises are required to be true in the first place. There is no analogous requirement concerning the truth of the premises of a theory. As a result, the only way a theory can be verified is through empirical testing. This is why the context of evaluation plays such a prominent role in the analysis of H-D theories.

a. Discovery

As we observed in the introduction, the scope allotted to the context of discovery by an approach to philosophy of science is an important variable in evaluating how helpful that conception would be to a young science in need of procedural guidelines. It appears that, in view of its goals of context-invariance and logical tractability, the deductive approach demands an extremely large context of discovery, including all aspects of science that could be construed as psychological or historical in nature. Wesley Salmon outlines the general deductivist view:

> If one accepts the distinction between discovery and justification as viable, there is a strong temptation to maintain that this distinction marks the boundaries between history of science and philosophy of science. History is concerned with the <u>facts</u> surrounding the growth and development of science; philosophy is concerned with the <u>logical structure</u> of science, especially with the evidential relations between data and hypotheses or theories. ...The items in the context of discovery are <u>psychologically relevant</u> to the scientific conclusion; those in the context of justification are <u>logically relevant</u> to it. Since the philosopher of science is concerned with logical relations, not psychological ones, he is concerned with the rationally reconstructed theory, not by the actual process by which it a came into being.[123]

Probably the most significant consequences of this view is that the deductive approach offers no suggestions for theory construction. In a formal sense, this is because there can be no logical <u>rules</u> for such a procedure. Thus, Hempel disparages the quest for "rules of induction" in the study of scientific methodology:

> Generally speaking, such rules would enable us to infer, from a given set of data, that hypothesis or generalization which accounts best for all the particular data in the given set. But this construal of the problem involves a misconception: While the process of invention by which scientific discoveries are made is as a rule <u>psychologically</u> guided and stimulated by antecedent knowledge of

> specific facts, its results are not logically determined by them; the way in which scientific hypotheses or theories are discovered cannot be mirrored in a set of general rules of inductive inference.[124]

We can conclude, therefore, that deductivists are not interested in the problems of theory building or methodology and, beyond noting that such problems are a product of creative imagination and impervious to logical analysis, have nothing to say about them. Their emphasis, rather, is on testing. As McMullin notes:

> Science [in the logical sense] does not have a history, strictly speaking; the tentative groping that precedes the formulation of concept or axiom is in no way reflected in the final product, and the only specifiable methodology for science is clearly that of logical demonstration. ...
>
> The only fruitful methodological issues, therefore, concern the way in which different propositions in the system are related to one another, the types of inference used to validate one proposition on the basis of others. What is sought is a logical theory of confirmation which will allow one to justify a "scientific" proposition by applying a set of logical rules...to the propositions constituting the evidence.[125]

This strict dichotomization has been subjected to various sorts of of criticism, most of which cluster around either of two conclusions. On the one hand, there are critics who hold that the distinction, while legitimate, is overstated. Thus, Salmon notes that "There is no reason at all why one and the same item cannot be both psychologically and logically relevant to some given hypothesis."[126] Similarly, Hanson maintains that sharp differentiation discourages critical philosophical analysis of the concepts (as opposed to their origins) usually relegated to the context of discovery:

> The slogan contrast between "the context of justification" and "the context of discovery" is often advanced to stifle queries that are fundamentally conceptual in character. Too many explorations into the concept of discovery have been dismissed by contemporary analysts as turning on issues of psychology and history, when it is our very ideas of discovery, of creativity, and of innovation which are at issue in such inquiries.[127]

On the other hand, there are critics who claim that no legitimate differentiation can be made between the two contexts. This view often rests on the conclusion that the context of justification is a pseudo-context, purportedly free of psychological influences, but in reality permeated by them. As Israel Scheffler puts it:

> It has been suggested that the justificatory processes of science themselves fail to objectivity, that personal factors in actuality permeate not only the genesis of theory but also its evaluation, and that psychology is therefore crucially relevant to the explanation of both. The fundamental Reichenbachian distinction...has accordingly been rejected, together with his correlative distinction between epistemology and psychology, neither distinction being capable of saving objectivity as an actual feature of the processes of science.[128]

A similar sentiment is apparent in Feyerabend's statement that "the theory which is suggested by a scientist will also depend, apart from the facts at his disposal, on the tradition in which he participates, on his preferences, on his aesthetic judgements, on the suggestions of his friends, and on other elements which are rooted, not in facts, but in the mind of the theoretician and which are therefore subjective."[129]

Feyerabend goes further, however, and posits that a firm adherence to the discovery-justification distinction does not merely ignore methodological issues, but actually subverts effective research. Interestingly enough, he bases this argument on a view of observation languages as products of antecedently available theories that is quite similar to that adopted by Hempel. Unlike Hempel, however, who accepts the antecedently available notions as already tested and verified, Feyerabend sees their use as implying that they should be subjected to test in light of the notions of the new theory, and not just the other way around. In this way, older assumptions are constantly brought up for reappraisal, and conceptual stagnation is thereby combatted:

> Research at its best is an interaction between new theories which are stated in an explicit manner and older views which have crept into the observation language. It is not a one-sided action of the one upon the other. Reasoning within the context of justification, however, presupposes that one side of this pair, viz. observation, has frozen, and that the principles which constitute the observation concepts are preferred to the principles of a newly invented point of view. ...[we] should rather demand that our methodology treat explicit and implicit assertions, doubtful and intuitively evident theories, known and unconsciously held principles, in exactly the same way, and that it provide means for the discovery and the criticism of the latter...[130]

Thus, Feyerabend concludes that the justification-discovery distinction is detrimental to effective research. This view is shared by others.[131] We can take it as one more point worthy of notice in our evaluation of the adequacy of deductive formalism as a model of science for international politics.

b. Justification

The testing of H-D theories must be carried out from the bottom up. This is because the highest level hypotheses of the theory are usually so general as to have no direct experiential or observational referent. As Braithwaite puts it:

> As the hierarchy of hypotheses of increasing generality rises, the concepts with which the hypotheses are concerned cease to be properties of things which are directly observable, and instead become 'theoretical' concepts...which are connected to observable facts by complicated logical relationships.[132]

In order to test these highest level hypotheses, then, it is necessary to make use of the deductive interconnections between hypotheses and absorb the test implications of the lowest level hypotheses upwards into the formal structure of the theory:

> The empirical testing of the deductive system is effected by testing the lowest-level-hypotheses in the system. The confirmation or refutation of these is the criterion by which the truth of all the hypotheses in the system is tested. The establishment of a system as a set of true propositions depends upon the establishment of its lowest-level hypotheses.[133]

Thus, to use W.V.O. Quine's apt metaphor, a theory meets experience only at its periphery.[134]

There are two possible outcomes when a hypothesis is tested against observations; either the observations will be in agreement with empirical predictions made by the hypothesis, or they will not. Each of these outcomes implies different consequences, as well as different problems, for the verification procedure. We will consider falsification first, since it is logically less problematic, then will turn to a consideration of confirmation.

Karl Popper, for one, has asserted that falsification is the only legitimate test that can be put to theories by experience. He writes:

> ...I shall certainly admit a system as empirical or scientific only if it is capable of being tested by experience. These considerations suggest not that the verifiability but the falsifiability of a system is to be taken as a criterion of demarcation. In other words, I shall not require of a scientific system that it shall be capable of being singled out once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical test, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience.[135]

To the extent that Popper is here discussing scientific systems and not single hypotheses, we can only conclude that his assertion that falsification, which must take place at the periphery of a theory, can refute the theory as a whole is at best a simplification. This can be shown by examining the logic of falsification a bit more precisely. Suppose that hypotheses H_2, H_2, ... , H_n are employed in deducing the observational consequence H. If H should turn out to be falsified by experience, then it is clear that at least one of the hypotheses H_i must be regarded as false. Likewise, if that H_i is, in turn, logically deduced from higher level hypotheses H'_1, H'_2, ... H'_m, then at least one of these must also be false. Thus, the implications of the observational falsification 'seeps up' into the theoretical system.[136] However, and this is the point we are mainly interested in, the assertion that at least one higher level hypothesis must be false in no way implies the falsification of all the hypotheses involved, as Popper's account seems to claim. In fact, there is even a serious question concerning whether <u>any specific</u> implicated higher level hypothesis can be decisively refuted in such a case. This is emphasized by Braithwaite:

> ...in the case of almost all scientific hypotheses, except the straightforward generalizations of observable facts which serve as the lowest-level hypotheses in the deductive system, complete refutation is no more possible than complete proof. What experience can tell us is that there is something wrong somewhere in the system; but we can make our choice as to which part of the system we consider to be at fault. In almost every system it is possible to maintain any one hypothesis in the face of apparently contrary evidence at the expense of modifying the others. ...But at some time a point is reached at which the modifications in a system required to save a hypothesis become more implausible than the rejection of the hypothesis; and then the hypothesis is rejected.[137]

Thus, the logic of falsification is much more complicated and ambiguous than Popper's formulation would lead us to believe. If for a moment we move beyond the logic to the pragmatics of falsification, we can note further that even the incontrovertible refutation of a complete theory does not necessarily imply its rejection. In the absence of an adequate alternative, scientists may be forced to retain the falsified theory for some time. The classic example of this is the numerous problems that became evident with the Newtonian theory of gravitation throughout the 1800s. Until Einstein's alternative emerged in 1905, however, there was no question of a simple discarding of Newton's formulation. Although falsified in many contexts, it was, quite simply, the best alternative available.[138]

The logic of falsification, although problematic, can at least be justified as valid in terms of deductive inference. No doubt this is why

Popper finds it so appealing. Such is not the case with the logic of confirmation, however, which is based on inductive inference and is therefore subject to extreme difficulties in terms of purely logical criteria. This can readily be seen by reconsidering the hypothetical example outlined above. Suppose again that hypotheses H_1, H_2. ... H_n are employed in deducing the observational consequence H. Suppose this time, however, that the outcome predicted by H is observed to occur. What, then, can we infer about the higher level hypothesis H_i? The answer, if our justification is limited to the rules of logical inference, is that we cannot infer anything about the truth of any of the hypotheses from the one 'confirming' instance. In fact, even if we had a great number of confirming instances, the inference would still be unjustified. There is simply no logical justification for inductively leaping from a singular statement to a generalization of universal form.[139] Thus as Salmon puts it, "The main shortcomings of the H-D method are strongly suggested by the fact that, given <u>any</u> finite body of observational evidence, there are infinitely many hypotheses which are confirmed by it in exactly the same manner."[140]

This logical truism stands in obvious contrast to the fact that such 'unjustified' inferences are made every day, and for good reasons, in the actual practice of science. This becomes understandable when we realize that these reasons are pragmatic and not logical. With reference to the question of confirmation, such pragmatic considerations usually involve assessments of either the evidential or theoretical support of a given hypothesis. Hempel notes several factors which tend to increase the level of confirmation in a given hypothesis. For instance, in terms of evidence, a hypothesis can be regarded as more strongly confirmed the more times it has been favorably tested, the greater the variety and diversity of tests it has passed, and the greater the precision in terms of measurement tolerances of tests it has passed. It is also more highly confirmed when it implies new tests for itself and then passes these favorably. In terms of theoretical support, as we have already seen in our discussion of laws, a hypothesis is more highly confirmed when it is logically deducible from more inclusive hypotheses or theories that have independent evidential support.[141]

These norms of confirmation can never be fully satisfactory from a deductivist point of view, however. This is because they simply allow too many pragmatic, psychological, and logically unjustified considerations into the context of justification. One attempt to circumvent this difficulty has been suggested by Salmon, who advocates the use of Bayes Theorem in order to inject what he calls "plausibility arguments" into the evidential foundations of a hypothesis. A plausibility argument is, essentially, "an assessment of...the plausibility of a hypothesis, prior to, or apart from, the results directly testing the hypothesis."[142] On Salmon's view, these arguments

> ...are considerations relating to the acceptance or rejection of scientific hypotheses which, on the H-D account, must be judged evidentially irrelevant to the truth or falsity of the hypothesis, but which are, nevertheless, used by scientists in making decisions about such acceptance or rejection. These same items, on the Bayesian account, become evidentially relevant. Hence, the judgement of whether

> scientists are making decisions on the basis of evidence, or on the basis of various psychological or social factors that are evidentially irrelevant hinges crucially upon the question of whether the H-D or the Bayesian account of scientific inference is more nearly correct.[143]

Salmon's suggested reformulation appears to be more a definitional sleight-of-hand than an actual solution, however. It merely calls psychological factors 'evidential' without coming to grips with either their arbitrariness or variability among individuals. As a result, this approach, like others similar to it,[144] does not contribute to extricating the deductive position from its dilemma.[145]

To date, the most highly developed potential solution that seems compatible with the underlying assumptions of deductivism is Rudolf Carnap's logical confirmation function.[146] Carnap's goal is to create a function which will state for any given hypothesis and body of evidence a quantitative, probabilistic coefficient of confirmation for that hypothesis on the basis of that body of evidence. Should he succeed, the degree of confirmation of a hypothesis will be ascertainable as a purely logical consequence of the hypothesis and evidence in question, and therefore will satisfy the twin deductive requirements of logicality and context-invariance. At present, Carnap is only able to produce such a function with reference to rigorously formalized model languages whose structure is far simpler than that required for the purposes of science. Whether or not he will be able to generalize this function is an open question. Some critics claim that the task is impossible in principle, others assert that it is impossible in practice.[147] The arguments for and against are far too complicated to go into here.

Suffice it to say that the obstacles are formidable, and a complete solution to this crucial problem will certainly not appear in the near future.

In light of this discussion of logical confirmation, we can conclude, along with Hempel, that:

> The gradual elimination of some among the conceivable alternative hypotheses or theories can never, it is true, narrow the field of competitors to the point where only one of them is left; hence, we can never establish with certainty that a given theory is true, that the entities it posits are real. But to say this is not to disclose a peculiar flaw in our claims about theoretical entities, but to note a pervasive characteristic of all empirical knowledge.[148]

This conclusion leads to one more criticism of the deductive approach perhaps the most telling of all.

Up to this point, we have recognized two primary defenses of the conception of science as deductive formalism. The first -- represented,for example in the quote from Hempel on page 50 above, is that the deductive account should be accepted because it sheds light on the "rationale and force of explanatory accounts offered in different branches of empirical science." In our examination, however, we have noted several points where the deductive reconstruction diverges considerably from actual scientific practice. Thus, this defense cannot be taken as decisive.

The second defense is by far the more fundamental. As represented in the quote from Brodbeck on page 25 above, it is based on the fact that the deductive models, and only the deductive models, guarantee a conclusive bond between premises and consequents. This is taken to imply that they alone can offer the hope of achieving truly reliable, objective and cumulative knowledge of the empirical world. As Thomas Greene stresses, the "posited linkages between events or data must be deductive in nature." Any other kind of linkage will not satisfy those who prefer logical precision to what they might describe as a casual disregard for the strictures of formal methodology."[149] Given this view, it is not difficult to understand why deductivists may come to regard a gap between the deductive ideal and practicing science as a problem for science, not for deductivism.

The present discussion of logical confirmation, however, throws this whole line of argument into serious doubt. It appears that, to the extent that the confirmation of hypotheses is pragmatic, the "strictures of formal methodology" are no more precise or certain than those of any other approach. At this critical juncture, then, the deductive claim to certainty and objectivity breaks down totally. We can note only one important implication of this breakdown.

It follows from the unavailability of complete logical confirmation that the empirical requirement of true laws in the D-N model of explanation cannot be fulfilled in terms of the H-D model of theories. Because no laws can ever be absolutely confirmed, none can ever be accepted as unquestionably true. Moreover, as Hempel and Oppenheim have pointed out, a true law cannot be replaced by a highly confirmed one if the tautological qualities of the model are to be maintained. As a result, we are led to the conclusion that the D-N model is actually not a model of true explanation at all, but instead a contextually bound, contingent schema that is logically indistinguishable from any other approach. Thus, it must be judged in terms of the first defense of deductivism alone, which, as we have seen, is a questionable defense at best.

In conclusion, and before turning to the role played by deductive formalism in political science and international politics, we can note the following passage from Paul Feyerabend:

> ...the enterprise [or reconstructionism] soon got entangled with itself...so that the main issue is now its own survival and not the

> structure of science. That this struggle for survival is interesting to watch I am the last to deny. What I do deny is that physics, or biology, or psychology can profit from participating in it. ...
>
> This can be shown both theoretically, by an analysis of some rather general features of the present state of the program of reconstruction, and practically, by exhibiting the sorry shape of the subjects (sociology; political science) which have made a vulgarized version of the program their chief methodological guide.[150]

III. Arguments Favoring Deductive Formalism in International Politics and Political Science

The best procedure to follow in discussing the role of deductive formalism in international politics and political science, it seems to me, is to consider separately the claims that have been made in its behalf and the attempts that have been made to implement it. Accordingly, this section will examine arguments concerning why and how a deductive program should be developed in political science, while the next section will take up the problem of implementation. In considering these defenses of deductive formalism, we will be particularly concerned with noting how well political scientists understand the philosophical model they are advocating.

A. Oran Young

In "The Perils of Odysseus: On Constructing Theories in International Relations," Oran Young observes that "Confusion and misconceptions about the state and nature of 'theory' have reached monumental proportions in the field of international relations."[151] In order to 'cut through some of this confusion and to set the problems of constructing theories about international phenomena in perspective,"[152] he proposes what he considers to be a proper definition of theory:

> A theory is a set of general statements such that (1) some of the statements (the assumptions or premises) logically imply the others (the theorems), and (2) the theorems can be cast in the form of falsifiable predictive statements about the real world.[153]

This conception of theory is similar in form to the H-D model discussed above, but is more simplistic in terms of content in two respects. First, its goal is prediction, not explanation. As we have seen, an explanatory theory can often be employed to make predictions, but the converse is much less often true. Young's failure to recognize this has already been discussed. Second, Young has not differentiated between internal and bridge principles, or between theoretical and pre-theoretical terms. The H-D conception of theory achieves its scope and breadth only as a result of the abstract or theoretical nature of its highest level hypotheses. Without these theoretical elements, a theory cannot be distinguished from an explanation or even a non-explanatory set of deductively related empirical statements. Thus, these terms should be an explicit part of any definition of a deductive theory.

Turning from the problem of what constitutes a theory to the problem of how theories can be constructed, Young notes what he describes as the "current dilemma" facing the field:

> It is not difficult to construct logically workable models that have some bearing on international phenomena, but no one has yet constructed models of this type that yield predictive results which are at all impressive. On the other hand, one can begin by working out richer descriptive frameworks or by

> searching for empirical regularities about international phenomena on an inductive basis. However, so far efforts along these lines have failed entirely to lead toward the predictive and (sometimes) manipulative capabilities associated with the development of viable theories.[154]

The "dilemma," then, involves the familiar tension between the demands of logical formality on the one hand and empirical relevance on the other that we encountered in the previous section. We can consider here whether Hanson's observation concerning an inverse relationship between the two (p. 58) seems to hold with reference to the study of international phenomena.

In deciding which of these three available strategies should be employed in developing theories of international relations, Young apparently holds to the view that the structure of a theory should be reflected in the method by which it is constructed. Accordingly, he concludes that "there is a powerful case for emphasizing the construction of simplified logical models _if_ one's principle objective is the search for viable theories of international relations."[155] He lists seven reasons for this choice; the first five dealing with the positive advantages of simplified logical models, the last two noting what he considers the fatal disadvantages of the two alternative methods.[156]

> First, the ultimate achievement of logical closure is always a necessary condition for the development of viable theories... .
>
> Second, simplified logical models sometimes play an important role in revealing the fundamental structure of a set of complex relationships even when the models are not sufficiently realistic to yield good predictions.

> Third, ...a workable logical model['s]...correspondence with the real world can be improved by a disciplined process of relaxing certain assumptions, introducing supplementary assumptions, and so forth.
>
> Fourth, ...the very process of creating and manipulating simple logical models generates a creative dynamic that gradually leads to the development of more sophisticated and realistic models.
>
> Fifth, ...the explanation of some empirical regularities may involve only a small number of factors and...simple logical models are more powerful than complex ones because they are more parsimonious.
>
> Six, ...projects that start out to maximize descriptive accuracy will seldom lead to the development of viable theory. They will ordinarily produce lists of potentially relevant factors which are logically unmanageable. This is the taxonomic fallacy, and it is alarmingly common among those who regard themselves as theorists of international relations.
>
> Seventh, ...heavily inductive work...becomes an enemy of theory...when practitioners become too wrapped up in the search for empirical regularities... . They then forget that these activities can only serve the cause of developing theories when they are carefully related to the processes of creating logical models and examining the accuracy of the predictions derived from such models.[157]

These reasons seem to depend on two basic assumptions, both of which hover just above the surface of the argument. The first, of course, is that the deductive conception of theory is unquestionably correct. The second is that the descriptive and inductive methods themselves are not necessarily antithetical to theory construction, but that the theorists who employ them are in some way incapable of employing them correctly.[158] Thus, the justification for the first assumption is a priori, the justification for the second is ad hominem. These two assumptions control the course of the argument.

Consider the following reconstruction: When Young first decided to address himself to the question of the relationship between theory and empirical reality, he had two paths open to him. He could either accept the complexity of reality as given and consider different conceptions of theory that might be compatible with it (essentially the method advocated in the introduction of this paper), or he could do what he did, which was to accept one conception of theory as given and consider different ways of making reality compatible with it. Upon choosing the second course, he discovered, in agreement with Hanson's observation, that reality could be molded to theory only at the loss of considerable empirical content. This did not cause him to reject his conception of theory, however, since he apparently could not (or was unwilling to) conceive of any viable alternatives. As a result, he accepted the necessity of sacrificing the content.

Three of his seven reasons for making this sacrifice (3, 4, and 5) are merely vague promises to retrieve it at some time in the future. These promises, however, stand in sharp contrast to the statement he makes twice that descriptive richness and viable theory are essentially incompatible.[159] Of his remaining reasons, one (1) is a reaffirmation of his first assumption, two (6 and 7) are reaffirmations of his second assumption, and the remaining one (2) posits a weak ("sometimes") link between undefined "fundamental structures" and logical models. We can thus conclude that his reasons only become plausible if his two basic assumptions are accepted first.

The importance of Young's argument, besides its logical weakness, is its failure to recognize a fundamental tenet of deductive formalism, namely, that the model is indifferent to the problems of theory building. In terms of the deductive approach, any method -- building logical models, developing descriptive taxonomies, inductively inferring empirical generalizations -- is equally justifiable, since there are no rules to differentiate various methodological procedures. To the extent that Young's argument is held to follow from his deductive definition of theory, therefore, his preferred method is purely ad hoc. And, as we have seen, the other reasons he gives for choosing this method are also highly questionable.

In one sense, however, it is understandable how Young's preferred method could be seen as somehow implied in the deductive model. The emphasis on the form of a theory and the subsequent view that empirical content is "hooked onto" the formal calculus, could be easily construed as indicating that, in the conduct of inquiry, a formal model should first be developed, and only later be given an empirical referent. But this view involves confusing the logical priorities of the reconstructed model, in which the distinction is analytical, with the temporal priorities of methodology, in which the distinction is pragmatic.[160] Once again, it is important to keep in mind that H-D theory is a reconstructed model,of science as product, not process.

B. Morton Kaplan

Since the mid 1950s, Morton Kaplan has been a proponent of the principle of deduction while maintaining a skeptical attitude toward its attainment in international politics. In System and Process in International Politics, he observes that "international politics, and social science generally, is so poorly developed that the construction of a precise deductive system would be more constructive and misleading than enlightening, [and] at this stage of development, some ambiguity is a good thing."[161] However, in his introduction to the paperback edition of System and Process in 1967, he acknowledges that progress is being made:

> I then [1957] did not know how to construct a strictly deductive theory which would cover the variables essential to a meaningful and useful theory of international relations. Since that time, we have been working with the materials of the theory and we are reaching the point where at least one portion of the theory -- that concerned with the "balance-of-power" international system -- is being developed with greater rigor.[162]

Kaplan's view that deductive theory is the goal of scientific inquiry stems from his study of the physical sciences, where it is more commonly achieved. In fact, he often begins his articles with a comparison of the state of theory in physical and social science.[163] These comparisons usually lead, however, to somewhat pessimistic conclusions concerning the chances for deduction in social science; his above statement notwithstanding. Thus, in 1969, we find Kaplan concluding that "Deductive elegance, of the type obtained in physical theory, is rarely if ever attained in the social sciences."[164] It appears, then, that in Kaplan's view deductive theory is the 'ideal'; seldom attained, but always pursued. This somewhat pessimistic outlook stands in sharp contrast to the more optimistic advocates of deductivism.

However, as the examination in the last section makes clear, even the quest for deductive rigor as an ideal can sometimes have stultifying effects on the course of research.

C. Davis Bobrow

Another less than completely enthusiastic supporter of deductive theory in international politics is Davis Bobrow. In an article appearing in the same volume as Young's, Bobrow adopts a similar definition of theory. "By theory," he states, "we refer to a system of internally consistent statements which allow us to explain or predict deductively."[165] Like Young and Kaplan, he notes that this ideal is seldom attained. He is not, however, particularly concerned with this discrepancy, mainly because of his view that other products of international relations research are as valuable, or perhaps more valuable, than theory. "A catholic stance may be warranted if we regard theory construction as of a higher status than work directed toward other sorts of products. I do not so regard it and accordingly suggest a narrow and demanding formulation."[166] Thus, Bobrow's conception of theory is essentially opportunistic. It results more from a desire to make definite distinctions among different research products than it does from a careful consideration of the nature of either theory or deduction.

D. David Easton

No overview of pleas for deductive formalism in political science could ignore the extremely influential writings of David Easton. In *The Political System*, Easton explicitly draws his conception of theory from physical science. Theory, "in its sophisticated state as found in physics or economics, ...is deductive,"

> It begins with a few postulates of empirical reference and from these deduces a series of narrower generalizations. From these in turn stem singular generalizations capable of empirical proof. This is a theoretical system which serves as an analytical model of the concrete political system. It is conceivable that someday in the social sciences, such a framework might reach the stage of maturity associated with theory in physics, for example.[167]

The only specific method he advocates for achieving this stage involves systematizing the 'basic assumptions' of the discipline. "[O]ne cannot deny that behind all empirical research there are those basic assumptions with regard to the major variables in the field and their relations and that one way of promoting the maturation of a discipline is to raise these assumptions to the point of consciousness for purposes of careful examination."[168] Unfortunately, no prescriptions are given concerning how this examination will be able to differentiate between equally basic yet contradicting assumptions.

Easton's conception of theory differs explicitly from the H-D model in the same way that Young's conception differs implicitly; it holds that the highest level hypotheses of a theory are essentially empirical generalizations. But, as we have seen repeatedly, the most general postulates of a theory need not have "empirical reference," as Easton assumes. Indeed, it is preferable that they do not, as Hempel makes clear when he remarks that "if science were...to limit itself to the study of observable phenomena, it would hardly be able to formulate any precise and general explanatory laws at all...."[169] Thus, Easton's conception of theory exhibits an important misunderstanding of its philosophical source.

However, by 1965, Easton could refer to a "revolution in political theory," apparently aiming in the direction of just this model of general theory:

> ...given the very short time that the behavioral approach has been persuasive in political research, it may come as a pleasant surprise to discover that there are a respectable number of alternative conceptual approaches for the study of political life or some of its major segments. Not that these conceptual structures are fully developed or close to any ideal form. They do, however, constitute a beginning and a promise for the future.[170]

This revolution, then, appears to be more one of intent than actual accomplishment. It also seems to be more problematic for the development of a general deductive theory than Easton appears to recognize. Once again, inconsistencies between the different approaches are underplayed, and the problems involved in synthesizing them into a general framework are not confronted.[171]

E. William Riker

Like Easton, William Riker bases his advocacy of a deductive approach on the successes of physical science:

> What social scientists have so greatly admired about the physical sciences is the fact that these latter actually measure up to our notion of what science should be. That is, they consist of a body of related and verified generalizations which describe occurrences accurately enough to be used for prediction. Generalizations within each science are related because they are deduced from one set of axioms, which, though revised from time to time, are nevertheless a coherent theoretical model of motion.[172]

He concludes that a science of politics must strive for the same structure.

By 1973, we find him claiming that "Political science...is beginning to proliferate new theories, new bases of empirical investigation, and new forms of organization of knowledge. It is even beginning to be deductive in method."[173]

Riker's view of deductive theory as outlined in these passages exhibits two important errors. First, like Young and Bobrow, he considers science to be a predictive tather than explanatory enterprise. Second, he mistakenly assumes that the individual physical sciences each follow from <u>one</u> set of axioms. This assumption is simply incorrect. Each physical science is in reality a conglomeration of a great variety of theories, many of which are founded on completely different, even contradictory assumptions. To assume a non-existent general theory in physical science, therefore, is to falsely justify the quest for general theory in political science.[174]

We will be considering Riker's work, and especially his formulation of a deductive theory of political coalitions, more fully in the next section.

F. <u>Robert Holt et al</u>.

The field of comparative politics claims a strong contingent of deductively-oriented scholars. Robert Holt and his associates take a position of the form and construction of theories that is nearly identical to Young's. Thus, Holt and John Richardson, Jr. define theory as "a deductively connected set of propositions, which are, depending on their logical position with respect to one another, either axioms or theorems."[175] Similarly, Holt and John Turner posit a link between theory and research that also resembles that put forth by Young. With reference to the source of hypotheses, they remark that "Ideally, scientific research in its simplest form involves, first, the deduction of a hypothesis from a set of theoretical propositions, and second, investigations to determine whether the facts of relationships predicted by the hypothesis manifest themselves empirically."[176] They are quick to point out, however, that "Little research in political science...follows this ideal."

> The major reason is obvious... .The theoretical structure of political science is not deductively powerful, and hence the rigorous deduction of hypotheses is, with few exceptions, impossible. Most hypotheses that are tested by political scientists are either the loose implications of a rather amorphous theory or are the researchers' hunches about a reasonable outcome of empirical research.[177]

It is perplexing that Holt and Turner should demand such strict requirements for the formulation, as opposed to the testing, of hypotheses. Certainly no position in philosophy of science, deductivism included, advocates this 'ideal' of scientific research. We can only conclude that they have somehow managed to confuse, like Young, the <u>rules</u> of H-D theory testing with the <u>processes</u> of hypothesis creation. The sources of hypotheses, contrary to Holt and Turner, are irrelevant to testing.

With reference to the construction of theories, Holt and Turner would probably approve of Young's preferred method of building simple logical models. Holt and Richardson's preferred method also involves starting from scratch in order to achieve adequate deductive theory:

> The grand paradigms of Almond, Deutsch, Easton (and for that matter, Holt and Turner) are little more than heuristic schema. They present an interesting way of looking at political phenomena, but do little more. What is needed is clear. First, a small group of theoretical primitives must be established. Second, additional concepts must be defined using only these theoretical primitives and some specifically identified logical (or mathematical) operations. Third, a set of axioms must be developed using only the concepts and operations defined. Fourth, a set of propositions must be deduced from these axioms for empirical testing. Fifth, criteria of admissibility and rules of interpretation must be developed.[178]

Here again, an analytical distinction is mistakenly assumed to imply a temporal methodological prescription. On the other hand, Holt and Richardson have at least noticed that highest level hypotheses must be theoretical in nature. In this respect, their reading of the deductive approach seems to be sound.

Holt and Richardson go further than Young in considering the kinds of prerequisites that would be necessary if comparative politics, or political science, were to develop as described here. In particular, they note the need for more rigorous training in mathematics:

> None of these steps except the first can be taken satisfactorily without the use of some body of rules that establish the principles for deduction. Our suggestion is that political scientists must turn to mathematics for these rules of logic and that until this is done, the grand schemata will remain essentially heuristic. ...[W]e can see no other way to introduce the necessary deductive power into a paradigm.[179].

This appeal to mathematics does not address itself directly to the question of the relationship of logical rigor and the empirical complexity of social phenomena, however. On this point, Holt and Richardson do mention that new breakthroughs in mathematics may be necessary in order to accommodate the needs of deductive social sciences.[180] But their primary means of dealing with this issue seems to be to drastically de-limit what counts as a 'problem' in political science:

> A science that is heavily committed to dealing with socially and morally relevant problems finds little use for this kind of paradigm or for the commitment to mathematics that it requires. For political science to advance, it must shed this professional commitment to solving social and moral problems.[181]

This controversial solution certainly would limit the number of political problems; and those that remain might even prove to be more amenable to deductive procedures. It is difficult to imagine, however, why anyone would be interested in studying them.[182] The particular value of Holt and Richardson's example is that it shows more explicitly than most the kinds of sacrifices that might be necessary in order to bring together deductive theory and empirical reality.

G. A. James Gregor

One advocate of deductive formalism whose knowledge of its philosophical foundations seems more thorough than most is A.J. Gregor. He describes political science as a "partially formalized science." Its proper goal is to become "fully formalized." One attribute of a formalized science is formal theory, which Gregor defines more carefully than any of the above scholars:

> A theory, in a substantially formalized system, includes as constituents (1) an uninterpreted or formal calculus which provides for syntactical invariance in the system, (2) a set of semantic rules of interpretation which assign some determinate empirical meanings to the formal calculus thereby relating it to an evidential or empirical

> base, and (3) a model of the uninterpreted calculus, in terms of more or less familiar conceptual or visualizable materials... .[183]

We can recognize this conception of theory as an almost verbatim account of that given in Nagel's The Structure of Science. Since Gregor's article was written in 1968, we cannot expect him to have been aware of Hempel's critique of this conception, which did not appear until 1970. Thus, so far, Gregor's view of theory is only susceptible to the criticisms put forth in the previous section, but not to the added criticism that it fails to grasp the full deductive model of theory.

Another benefit of Gregor's analysis is that he is explicit about why formal theory is necessary:

> ...formalization seeks to satisfy the minimal requirements of any serious knowledge enterprise: to provide for syntactical and semantic invariance without which reliable knowledge is simply not conceivable. ...Semantic and syntactical invariance afford the minimal necessary conditions for drawing out the testable implications of any set of empirical propositions, for identifying the locus in which false propositions are located when a hypothesis fails to meet empirical test, for coordinating research in order that separate findings support each other, for isolating the most strategic propositions for testing, and in order to provide the most parsimonious summary of actual or anticipated research efforts.[184]

Thus, in Gregor's view, "the requisite semantic and syntactic precision... are the minimal responsibilities implied in David Easton's injunction that the science of politics, in its effort at theory construction, attempt to meet the methodological requirements characteristic of the natural sciences."[185]

The problems with this formulation are two: First, there are no set "methodological requirements characteristic of the natural sciences." As Paul Feyerabend puts it:

> The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science gets into considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible and however firmly grounded in epistemology, that is not violated at some time or other.[186]

Second, even if there were such requirements, syntactical and semantic invariance would not be among the properties that would identify them. Both, as we have seen above, go out the window with the formalism-interpretation distinction. Syntactical invariance rests on the untenable analytic-synthetic dichotomy, while semantic invariance breaks down in the face of the failure to achieve absolute confirmation.

Gregor's approach appears to be more akin to Holt and Richardson's than to any of the other scholars discussed here. Like them, he holds that most present attempts to be 'scientific' are abortive. He is especially hostile to functional and systems approaches, which he feels are "all too frequently parasitic upon suggestive analogy and metaphor, trafficking on our familiarity with goal directed systems."[187] In light of these failings, he advocates that political science never borrow concepts from other partially formalized sciences. To do so, he warns, "involves considerable risk."

> The disposition on the part of some political scientists to be uncritically accommodating to such borrowing threatens to burden political science as a knowledge enterprise with an inventory of vague and ambiguous concepts little calculated to further the effort to explain and predict.[188]

On the other hand, also like Holt and Richardson, he concludes that political science must turn toward physical science and mathematics for guidance. "The adoption of concepts from a formalized empirical or non-empirical discipline entails minimum hazard since the implications of such assimilation are maximally specified."[189] But he has nothing to say about how this assimilation is to be achieved, nor does he reveal what concepts should be borrowed or why. As a result, we are informed as to why we should want to become formalized, but are left in the dark concerning how.

This list of examples could be lengthened without much difficulty, but I think the above authors are representative of most of the approaches that have been taken. An important area we have not touched upon, but which deserves careful attention, is the conception of science portrayed in the various "scope and methods" textbooks in political science. Many, such as George Graham's Methodological Foundations for Political Analysis,[190] take an almost purely deductive approach with little mention of alternative possibilities. Others, like Meehan's The Theory and Method of Political Analysis, point out the difficulties involved in attaining deductive theories and explanations, but interpret this as an indication of the inferiority of political science.[191] None that I have come across critically examine either the relationship of physical science and social science or the criticisms of the deductive approach -- even as an ideal of science -- that are the subject of this thesis. Given that many graduate students are introduced to these issues only through such textbooks, this narrowness of focus does not speak well for the education of methodologically well-rounded flexible political scientists.[192] In conclusion, we can note that Gunnell's charge concerning political scientists' poor grasp of the philosophical model of deductive formalism seems to be upheld, at least with reference to the small sample we have discussed here. Most of these scholars seem to emphasize only those aspects of the model which they find compatible with their own interests, and discard or ignore the rest. Thus, for example, most of these arguments are characterized by a great concern with form at the expense of content. Only Holt and Richardson and Gregor mention the problem of interpretation, and their handling of it is inadequate. Perhaps more disturbing, however, is the nonchalance with which all these political scientists approach the possibility of drastically cutting back what they are willing to define as "political" in order to satisfy the formal requirements of their preconceived notions of theory. We shall see in the next section that this tendency is even more pronounced when attempts are made to actually implement deductive theories of international phenomena.

IV. Applications of Deductive Formalism in International Politics

Several attempts have been made in the study of international politics to develop deductive theories along the lines sketched out in Section II. None has been considered fully satisfactory either by their creators or their critics, but all have been praised at one time or another as important "first steps" in the direction of viable theory. Interestingly enough, the same cannot be said for the development of deductive explanations. To my knowledge there have not been any attempts to produce strictly deductive explanations in the study of international politics or political science that resemble those advocated by Hempel and Oppenhein et al. Some reasons for this will be considered further on, but since these reasons are somewhat dependent upon some of the characteristics of the deductive theories that have been put forth, we should turn to these first.

A. Deductive Theories

1. Reaction Equations and Arms Race Models

One area of international politics that has proven conducive to the construction of formal theories is the study of arms races. Building upon the foundations laid down by Lewis F. Richardson, scholars such as Smoker, Caspery, Wolfson, Milstein and Mitchell, and others have attempted to develop mathematical models that effectively mirror the dynamics of competitive military spending.[193] These models are not strictly deductive in that the variables they deal with are related to each other through mathematical functions rather than deductive syllogisms. But such theories, which we might call systematic in order to distinguish them from pure deductive theories, can easily be converted to deductive form.

In general, these arms race models bear a strong resemblance to the simplified logical models described above by Young. They involve a high degree of abstraction, usually being based upon simple stimulus-response or "reaction-process" assumptions. Thus, for example, they pay little or no attention to internal decision-making processes or other internal characteristics of nations. As a result they fare rather poorly in attempting to bring together logically tight theoretical structures and empirical relevance. In a recent examination and critique of the arms race literature, Peter Bush makes the following observations:

> First, quantitative models of arms races cannot be tested with anything approaching desirable rigor. Secondly, this analytical approach is not now in a position to offer accurate, quantitative predictions of military spending.[194]

Busch cites two basic reasons for these deficiencies; poor data and inadequate theory. The data problem is a double one. First the data is often unreliable. There are good reasons to believe, for example, that the Soviets and Chinese have often manipulated their reported military expenditures for various reasons over the last 30 years. In addition, even if the figures were dependable, the small quantity of available data points greatly limits the use of statistical measures as well as the efficacy of the models themselves. Models are only adequate as long as the dynamics they describe remain operative. System breakdowns or reorganizations, such as those which followed the last two world wars, set new dynamics, often involving new actors, into motion. Thus, new models become necessary. In the recent past, the longest period of relative stability was between 1870 and 1914. Not surprisingly, many arms race and conflict escalation studies focus on this period.[195] Since that time, the interwar and post-WWII periods have only lasted 22 and 29 years respectively (so far). This is probably the major obstacle to effective testing of arms race models.

In terms of theoretical difficulties, Busch's conclusion is succint.

"It is not only the case that the important variables cannot be measured -- we do not really know which variables are important!"[196] Consequently, different models emphasize different types of variables. But the paucity of data precludes a definitive judgement as to which models are more adequate, and thus brings to a standstill the process of cumulative correction of errors and theoretical improvement.[197]

What then is the use of mathematical models of arms race? In Busch's view, the empirical and theoretical failings of the approach do not completely negate its value. He holds that it also serves an important heuristic function:

> The contribution to be expected -- indeed demanded -- from such models it that they organize and clarify the verbal concepts and theories which have been used to describe arms races and that they explicate the often unsuspected implications of our commonly used theories. The fulfillment of these criteria requires, that every term in the various equations have a meaningful political definition. Equally important, every specified functional relationship must be politically plausible. Finally, the mathematical implications of the equations must correspond with reality.[198]

In light of these criteria, it appears that arms race models in their present form are more akin to taxonomies than full-fledged H-D theories, in spite of the fact that they posit strong deductive interconnections between variables. As we have seen, such interconnections are a necessary, but not sufficient condition for a complete H-D theory. To translate Busch's critique into the terminology introduced in section II, arms race models are inadequate in terms of bridge principles and empirical reference, and are untestable through either verification or falsification. It also follows from this that the 'laws' of arms race models -- e.g., Richardson's reaction equations -- are not theoretically substantiated. Thus, the models fail to meet at least three minimal requirements of H-D theory.

2. Game Theory and Deterrence Studies

A second area of international politics to which deductive models have been applied is the study of deterrence strategy. Here game theory has been the chief means of formalization. In general, game theoretic models deal with the problems of rational choice in situations involving interdependent decision-making, or strategic interactions among purposive actors.[199] Thus, their applicability to problems of deterrence seems apparent. However, their performance in this capacity has been somewhat problematic.[200]

Richard Smoke notes that abstract, deductive models were originally used to study problems involving the deterrence of thermonuclear war; a situation which, fortunately, has never had an empirical referent. Thus, such models were particularly valuable. But they have since been applied to the study of less catastrophic, but more complicated cases such as deterrence of limited wars and crises, for which much empirical data is available, and for which the game theoretic models have exhibited less relevance. In these cases, game theory, much like reaction equations in the study of arms races, has focussed attention on only selected aspects of the problem, mainly "commitment" and "signalling," and has left other important considerations unexamined. Smoke's complaint is not that game theory is inapplicable to deterrence problems, but only that it highlights some aspects at the expense of others which might be equally important.[201] In other words, the scope of game theory is inadequate for dealing with the complex totality of a deterrence situation.

A similar point, which also has important implications for the deductive status of game theory, is made by Thomas Schelling. Schelling notes that unique deductive solutions to strategy problems are only available in "zero-sum" two-person games. Such games may adequately reflect the "all-or-nothing" nature of nuclear deterrence, but are much less applicable to the more subtle interactions that characterize crisis-type situations. For these cases, the so-called "mixed-motive" games, in which the players have interests in common as well as in conflict, become more applicable. But as Schelling points out, mixed-motive games do not have a unique deductive solution. "The principles relevant to successful play, the strategic principles, the propositions of a normative theory, cannot be derived by purely analytical means from _a priori_ considerations."[202] Thus, he concludes, "the mathematical structure... should not be permitted to dominate the analysis [since] some _essential_ part of the study of mixed-motive games is necessarily empirical."[203] This seems to imply that with respect to those aspects of deterrence situations to which it is most applicable, game-theory is often not completely deductive.

Like arms race models, however, game theory is not put forward to stand or fall on its merits as a deductive or predictive theory, but is held by its supporters also to be important as a heuristic tool for organizing and conceptualizing. Schelling, in particular, holds that the rudiments of game theory can act as a framework for analysis within which social scientists can pursue their substantive interests:

> ...what may be most important to a social scientist is these rudiments. The rudiments can help him to make his own theory, and make it in relation to the particular problems that interest him. One of the first things that strike a social scientist when he begins to experiment with illustrative matrices is how rich in variety the relationships can be even between two individuals and how many different meanings there are for such simple notions as "threat," "agreement," and "conflict."...Even the simplest of situations, involving two individuals with two alternatives a piece to choose from, cannot be exhaustively analyzed and catalogued. Their possibilities are almost limitless. For this reason, game theory is more than a "theory," more than a set of theorems and solutions; it is a framework for analysis.[204]

This passage illustrates well the inability of formal theory to subsume the various manifestation of empirical social phenomena. We might be inclined to disagree with Schelling here only with reference to his claim that game theory, in its role as a framework for analysis, is more than a theory. By the requirements of H-D theory, it is certainly far less than a theory. Its deficiencies are similar to those of arms race models; it is empirically inadequate and therefore predictively poor, it is untestable even when applicable to empirical cases because of the basically subjective element involved in assigning utility values to different outcomes, and it contains no universal laws (read 'solutions') except in the most trivial instances.

3. Economic and Rationalistic Models of International Processes

Game theory, at a more general level, is a product of economics. It was first formulated in John von Neumann and Oscar Morgenstern's classic Theory of Games and Economic Behavior.[205] Thus, it contains one important assumption which we have not yet discussed -- the assumption of rationality. This assumption is primarily responsible for the gains that have been made in developing deductive, axiomatic theories in the field of economics.[206] In von Neumann and Morgenstern's theory, it is operationalized in terms of the obvious goal of monetary gain in economic behavior:

> We shall assume that the aim of all participants in the economic system...is money, or equivalently, a single monetary commodity. This is supposed to be... identical, even in the quantitative sense, with whatever 'satisfaction' or 'utility' is desired by each participant. ...The individual who attempts to attain these respective maxima is also said to act 'rationally'.[207]

In addition to its use in game theory, there have been several other attempts by both political scientists and economists to transfer this concept of rationality from the economic to the political sphere. Since there is no quantifiable measurement of rational utility readily available in political contexts that is analogous to money, such a transfer is problematic. But solutions have been put forth and, as a result, economic models have become the most prevalent source of deductive theories in international politics, as well as political science as a whole. These theories, sometimes referred to as "rationalistic" models of political behavior, often become quite sophisticated in their deductive structure. We can briefly mention some of the more important ones here and then examine one -- Riker's Theory of Political Coalitions -- in more detail in order to ascertain how it handles the concept of political rationality and how it matches up to the requirements of H-D theory.

A. Collective Goods, Oligopolies, Bureaucracy, and General Theories of Politics

Some aspects of international politics have been analyzed in terms of problems associated with the supply of collective goods. A collective good can be generally defined as any good that cannot be withheld from any member of a specified group once it is supplied to some member of the group. In political terms, national defense would be considered an example of a collective good. Norman Frolich and Joe A. Oppenheimer, for example, develop a theory of "entrepreneurial politics" in which political leaders are conceptualized as "entrepreneurs" dispensing collective goods in order to accrue political gains. They use this scheme to examine different international situations such as defense alliances, foreign aid, and military intervention.[208] Other uses of the collective goods concept include studies of alliances, leadership, and political participation.[209]

Several social scientists have noted a similarity between the international system and some of the characteristics of oligopolistic markets. Young observes that the international system resembles such market situations on several count:

> ...it involves competitive-cooperative interactions among a limited number of purposive actors who have some capacity to communicate with each other. Each of the actors is a relatively complex collective entity whose external behavior is often hard to capture in simple models. The number of actors in the system is virtually always greater than one.

> But the number of actors is small enough so that every actor can interact with the others as differentiated entities, rather than having to interact with all the others together as an aggregated and undifferentiated environment.[210]

The only major application of this idea to international politics is Kenneth Bounding's general theory of conflict, which is structurally based on the theory of oligopolies in combination with the reaction models and game theoretic concepts discussed above.[211] Beyond this, not much explicit work has been done tying the international system and oligopoistic market contexts together.

A third area in which rationalistic models have been applied is the study of bureaucracies. This can be seen as at least indirectly applicable to the study of international politics, especially in terms of helping to develop viable models of national actors that will shed light on some of the internal causes of international behavior. The major rationalistic contributions to this literature are Anthony Down's _Inside Bureaucracy_ and Gordon Tullock's _The Politics of Bureaucracy_.[212] These axiomatic, deductive theories stand in interesting contrast to the other prominent approach to bureaucratic studies exemplified by the work of Richard Neustadt and Graham Allison.[213]

Finally, a few attempts have been made to develop general theories of politics based upon rationalistic assumptions and economic models. Among these have been theories of democracy,[214] and of political exchange.[215] Others, such as the general theory put forth in Riker and Ordeshook's _An Introduction to Positive Political Theory_, are even more extensive in scope. Riker's theory of political coalitions, with its broad range of applications, can be seen as a general theory of this type.

The centrality of the concept of rationality to these theories is difficult to overemphasize. In a recent work, Riker likens its role in social science to that of "mechanism" in physical science:

> ...it is clear that the assumption of rationality and the assumption of mechanism play comparable roles in the explanation of the social and physical worlds. The mechanical assumptions assert that there is something about things that assures us they will (usually) move regularly, and the rationality assumption asserts that there is something about people that makes them behave (usually) in a regular way. In each case the function of science is to generalize about the regularities.[216]

Further, he sees it as the only adequate base for deductive theory-building in the social sciences. In this regard, he contrasts the "postulated regularity" imposed by the rationality principle with the "observed regularity" associated with empirical-inductive methods; holding like a good deductivist, that the former is intrinsically superior to the latter:

> ...the notion of rationality...is one of the ways by which we arrive at the regularity necessary for generalization. Whether or not it is better than simple observation is currently the subject of some discussion in political science... .As is apparent, we side with deductive methods and postulated regularity, largely because we believe them more efficient than their alternative. By the method of postulated regularity one can at least hope to avoid erroneous generalizations based on accident... although one cannot avoid errors of observation that occur with either method. At the same time, the method of postulated regularity is positively more efficient, because it permits the easy generation of hypotheses and offers a single and parsimonious explanation of behavior. As against

> this efficiency, the method of observed regularity is _ad hoc_. No hypothesis can be derived without a set of prior observations, and every hypothesis is another kind of explanation. This often results in a myriad of noncontradictory hypotheses that need theory to bring them together. Even if catalogued into hypotheses, behavior appears extraordinarily complex, when, with a simplifying and coordinating theory, much of the complexity disappears. On the practical grounds of efficiency, therefore, we prefer postulated regularity. Hence, the notion of rationality must play an extremely important role in our theory of politics.[217]

This passage is particularly interesting in its explicit espousal of the requirements of form over those of content. To Riker's criterion of efficiency, it seems prudent to add the proviso: efficiency for _what_? Certainly he must be concerned primarily with understanding politics _as practiced_, not with building 'efficient' theories for their own sake. It should be emphasized that simplicity _per se_ is not an ineluctable component of good theories. As Kaplan makes clear, blind allegiance to simplicity is unnecessary as well as antithetical to healthy empiricism:

> Why should the simpler theory be the better one?... What is beyond dispute is the physicist Fresnel's remark that "Nature doesn't care about mathematical difficulties." Indeed, the argument can sometimes be made _against_ a theory (for instance, a theory of human motivation) that the trouble with it is that it is too simple; nature sometimes seems to prefer complexity. More realistically, both sorts of cases must be acknowledged. "If we study the history of science," says Poincaré, "we see happen two inverse phenomena, so to speak. Sometimes simplicity hides under complex appearances; sometimes it is the simplicity which is apparent and which disguises extremely complicated realities." The progress in science is not always in the direction of simpler theory.
>
> All things considered, perhaps the best methodological counsel as to the norm of simplicity is

Whitehead's: "Seek simplicity and distrust it."[218]

b. Riker's Theory of Political Coalitions

With the importance of the notion of rationality in mind, we can now turn to a consideration of how Riker employs it in his Theory of Political Coalitions. As we mentioned above, one of the major difficulties encountered in applications of rationalistic models to political decision-making involves the development of rules of interpretation for the concept "rational utility." Quite evidently, the isomorphism between utility scales and monetary units assumed by economics has no obvious counterpart in political science. As a result, two alternative approaches have at various times been proposed. One, put forth by R. Duncan Luce and Howard Raiffa, holds that any decision is rational as long as it represents the preferred outcome of the person who makes it.[219] Riker objects to this formulation, noting that although it solves the problem of irrational behavior, which is defined out of existence, it results in the rationality condition becoming "no more than a condition for the existence of participants who behave in a social situation."[220] Thus it makes the development of a subjective utility scale impossible. The second approach is to accept a more limited definition of rationality. This, however, also presents a problem. As Riker puts it, "How can the rationality condition be stated in such a way that it is more than a tautology but not subject to the criticism implied by those experiments which show that the scale of individual utility is not the same as a scale of money?"[221]

Riker's solution is to define rationality in terms of the concept of "winning." "Politically rational man is the man who would rather win than lose, regardless of the stakes."[222] He restates this definition in the following form, which he holds to be both defensible and nontautological:

> Given social situations within certain kinds of decision-making institutions (of which parlor games, the market, elections, and warfare are notable examples) and in which exist two alternative courses of action with differing outcomes in money or power or success, some participants will choose the alternative leading to the larger payoff. Such choice is rational behavior and it will be accepted as definitive while the behavior of participants who do not so choose will not necessarily be so accepted.[223]

There are, of course, still problems with this formulation. First of all, in order to avoid being a tautology, the definition demands the condition of total knowledge. That is, "Every possible course of action open to the participants and its rewards to them must be known to them ..."[224] Although Riker mentions this as a condition for two-person, zero-sum games, which he feels are too restrictive as a model of politics, he fails to explicitly include it as a condition for his preferred model, which is based on n-person, zero-sum games. However, it is clear that such a condition is necessary for a rational decision to be made in terms of the above definition. Otherwise all participants would merely choose the outcome they felt would lead to the highest payoff, and this would revert the definition to one of all decisions being essentially rational. Secondly, the total knowledge condition implies that irrational behavior can indeed occur, but is not covered by the theory. Thus, all behavior that does not conform to the rational pursuit of "winning," either in terms of money, power, or success is by definition excluded from consideration as "political behavior." Viewing this restriction as a bridging sentence connecting the theory to empirical reality, we can see that the universe of cases it allows as acceptable content for the model is correspondingly decreased. For example, Lyndon Johnson's decision not to run for another term as president in 1968 would not be admissible as political behavior in terms of Riker's formulation of the condition of rationality. Thirdly, there is a question as to whether Riker's definition really avoids the problem of equating utility scales and money scales. By lumping money, power, and success together, it could be argued that he is assuming an isomorphic utility scale among them. Thus, if a participant has a choice between x amount of money and y amount of power, he will require a means of measurement common to them both in order to choose the rational response. How this approach avoids the problem of equating monetary and utility scales is not explicated by Riker. In spite of these problems, however, he emphasizes that "this revised form of the rationality condition can be verified in only one way; that is by showing that a model using it permits the deduction of non-obvious hypotheses which can themselves be verified by experiment, observation, and prediction.[225] Before we turn to the problem of verification, we should examine the other assumptions upon which the theory is based, and the non-obvious hypotheses that are deduced from these assumptions.

The second major assumption of Riker's theory is the zero-sum condition. "In application to society," he notes, "the zero-sum condition is the requirement that social situations be abstracted for study in such a way that only the direct conflicts among participants are included, and common advantages are ignored."[226] He is well aware that this is not a particularly common state of affairs in politics, that some element of mutual gain is usually evident. For example, he observes that:

> ...one very interesting thing about political societies is that people consent to remain in them, even when they are on the losing side in particular decisions. This fact, which has impressed political philosophers at least since the time of Plato and which is the observational basis for the innumerable and drearily repetitious theories of social contract, cannot be expressed in terms of a zero-sum game.[227]

However, he maintains that "still we do frequently perceive what we imagine to be pure conflict situations."[228] Among such situations, he includes elections and wars. He concludes, therefore, that:

> ...whether or not one should use the zero-sum model depends entirely on the way one's subject is commonly perceived. In discussing bargains, which are perceived as mutual gain, of course, a non-zero-sum model is probably best. On the other hand, in discussing elections and wars, which are perceived as requiring indivisible victory, the zero-sum model is probably best and I shall use it here when I wish to talk about these and other essentially political decisions.[229]

It is clear that this assumption also contains bridging sentences which restrict the content of the theory. In this case, all political situations that involve mutual gain become inadmissible for study under the theory. Thus, it is difficult to imagine many "essentially political decisions" other than elections and all-out warfare that satisfy Riker's initial conditions.

The third basic element of Riker's theory of political coalitions is the n-person theory of games. In terms of the H-D formulation, this element can be seen as contributing to the internal principles of the theory. It provides a sort of formal calculus, and contains substantive implications such as definitions and scope conditions that serve to underlie the above assumptions.

Together, these three assumptions combine to form the axiomatic foundation of the theory. They are taken as "given," subject to verification in terms of lower level empirical hypotheses that are deducible from them. Riker derives three such hypotheses:

> (1) The size principle: "In n-person, zero-sum games, where side payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur."[230]
>
> (2) The strategic principle: "...in systems or bodies in which the size principle is operative, participants in the final stages of coalition formation should and do move toward a minimum winning coalition."[231]
>
> (3) The disequilibrium principle: "...in systems or bodies where the size and strategic principles are operative, the systems or bodies are themselves unstable. That is, they contain forces leading toward decision and hence toward the elimination of participants."[232]

We can now turn to the question of how Riker attempts to verify these hypotheses. Since proposition (1) is logically prior to propositions (2) and (3), we can focus our examination on it. This examination should in turn provide us with an indication of some of the strengths and weaknesses of the theory as a whole, as well as of the rationalistic approach in general.

In order to verify the size principle, Riker first formulates an "analogous statement about the real world:"

> In social situations similar to n-person, zero-sum games with side payments, participants create coalitions just as large as they believe will ensure winning and no larger.[233]

He then proposes to verify this principle in the following way:

> I have devised two classes of situations in which coalitions of the whole have been formed by reason of some accidental circumstance. Then I have shown that in <u>every</u> instance in these two classes of events, the size of the coalition of the whole, which according to the theory, has no value, has been reduced to a smaller size that has some value. Thus, the validity of the size principle has been <u>proved</u> for these two classes and, to the degree that these classes are representatively drawn from zero-sum situations, it is strongly implied that the size principle holds in all other classes of events likely to occur in the situations from which these classes are drawn.[234]

The two classes of situations invoked by Riker involve overwhelming majorities in American politics and world politics. Since we are mainly interested in the implications of Riker's theory for international phenomena, we will only consider his argument with reference to the second class here. The overwhelming majorities in this case result from the outcomes of total wars.

The first step of verification involves the specifying of links between the empirical situation and the internal principles of the theory. Thus, Riker notes:

> The development in the sixteenth century of the system of European nation-states and the fairly recent extension of this system to the whole world created a pattern of international

> politics very much like an n-person game. The players are the nations... .
>
> [O]ccasionally international politics turns into a zero-sum game as when total war has occurred.... If it does become an analogue of a zero-sum game, the experience of international politics also becomes relevant evidence about the model.[235]

If we add the assumption that the players act rationally, then it appears that Riker's three underlying assumptions are satisfied in cases of total war.

Next, he gives an (empirical) interpretation of the (theoretical) size principle:

> If one side actually wins, that is, if one side is exhausted before the other, then victory, by removing the losers, transforms a (probably minimal) winning coalition into a grand coalition....Assuming, as I shall, that winners in total war retain for some time the zero-sum habits of thought engendered by their very participation in it, then they will reject a coalition of the whole and begin to squabble among themselves. Presumably they will seek to substitute for it something that approaches a minimal winning coalition. If, in fact, they actually do so, their action constitutes...verification of the size principle.[236]

With his theory thus hooked to the subject matter at hand, Riker is able to perform a test. He examines the "three instances of total war in the modern state system: the Napoleonic wars and the first and second world wars."[237] In each instance, he observes, the grand coalition that emerged victorious at the end of the conflict quickly divided into opposing factions. The Concert of Europe divided over the territorial ambitions of Prussia and Russia, World War I resulted in the formation but eventual collapse of the League of Nations, and World War II quickly led to the Cold War, in which the U.S. and the Soviet Union attempted to marshall the world into opposing coalitions. From this examination of empirical data, which includes all cases of total war in the Western state system, Riker reaches the following conclusion:

> ...the winning coalitions of total war do not long survive victory. Both in the model and in actuality they have become valueless. They die because victory renders them nugatory. To win something of value in the next phrase following total war, the size of the winning coalition must be reduced. From the evidence of total war..., the size principle is thus additionally verified.[238]

Does this test verify the size principle? The answer, if given in terms of the requirements of H-D theory, must be no. What Riker has shown here is not that post-war coalitions tend to become definitely <u>minimal</u>, as the size principle demands, but only that they tend to become less than maximal. In his explication of the test, Riker notes that the size principle will be proven if all cases of grand coalitions following total wars eventually break up. This is an incorrect inference. In the first place, as we have seen, verification can never be absolute; thus, the hypothesis may well be disconfirmed in a future case. Secondly, the hypothesis that Riker's test <u>has</u> lended credence to is not the size principle, but this: after total wars, grand coalitions tend to break up into smaller coalitions. No inferences concerning the specific sizes of the resultant coalitions can be drawn from this particular test.

In terms of the requirements of H-D theory, then, it is clear that Riker's theory of political coalitions is far from adequate. However, it should be noted that it is much more acceptable than, for instance, the arms race and deterrence models discussed above. The major virtue of Riker's theory is its explicitness. It is explicit about its basic assumptions, its axioms, and its hypotheses. Further, by differentiating between these three levels, it fulfills Braithwaite's requirement concerning the levels of hypotheses. It is also explicit about its interpretation, noting precisely the type of empirical behavior it is meant to be applicable to. Admittedly, this greatly limits the professed empirical scope of the theory, but at least the boundaries are distinct. Finally it is explicit about its verification procedures. Somewhat ironically, this is why it is relatively easy to see how those procedures fail. On these counts, therefore, Riker's theory scores high in terms of H-D requirements.

On the other hand, however, the theory is lacking in several fundamental respects, First it contains far too many implicit assumptions, the implications of which are not adequately spelled out. Thus, in Hempel's terminology, the theory is "elliptical." This is easily corrected, of course, simply by making the implicit assumptions explicit. At a more fundamental level, the theory is also "partial;" that is, it leads to the inference of a class of empirical cases rather than to a unique case. As we have seen, the exact size of the minimum winning coalition cannot be deduced from the hypotheses of the theory. Instead, only a class of sub-maximal coalitions can be so deduced. As a result, the concept of minimum winning coalition comes dangerously close to being a tautology (the minimum winning coalition is that coalition which in fact turns out to be minimum). It follows from the partiality of the theory that it is not truly deductive. Thus, Riker is forced to employ extra-logical evidence in order to justify his supposedly deductive inferences. In this regard, his theory, like game theory, gains its empirical plausibility and value in just those contexts where its deductive characteristics are weakest.

In conclusion, the example of Riker's Theory of Political Coalitions allows us to make some general remarks about the rationality principle and the ap roach based upon it. The main difficulty faced by this approach is what we have already observed Young alluding to with reference to logical models in general; namely, their applicability to political behavior is prohibitively narrow. Or, conversely, what is defined as "political" in terms of the rationality principle is only a small fraction of the myriad phenomena which are of interest to political scientists. This narrowness plagues all the theories we have discussed in this section. Although we cannot here evaluate the deductive elegance of each of these theories, we can say with some assurance that whatever deductive characteristics they exhibit have been purchased at the price of empirical relevance. This is particularly dismaying when a theory attempts to claim a broader range of applicability than it can justifiably account for. For example, Riker's theory of coalitions is introduced in a chapter entitled "The Prospect of a Science of Politics." Thus, the implication is that this theory is such a prospect and that it can constitute the science of politics as a whole. Such an implication, as we have seen, is in no way justifiable.

B. Deductive Explanation

As a result of this discussion of deductive theories in international politics, we are now in a position to see why there have been no deductive explanations put forth in the discipline. The reasons are actually fairly simple. We saw above that, on the deductive view, theories explain laws and laws explain facts. The way theories explain laws is by theoretically substantiating them from above and empirically substantiating them from below. Accordingly, a law is regarded as being more highly confirmed the greater its substantiation from these two sources. One of the requirements necessary for a law to adequately explain a single phenomenon is that it be true. For the moment, let us avoid the fundamental problem concerning the truth of laws discussed in section II, and take this to mean that the law's level of confirmation is unquestionably high. If the law is very highly confirmed, then, we can take a syllogism for which it acts as a premise to be a viable deductive nomological explanation.

From our examination of deductive theories available in international politics, we can conclude that no highly confirmed laws have been generated. This is primarily because the theories themselves are often not strictly deductive; in which case it is impossible to test inferred laws in the required fashion. In the cases where the theories are deductive, such as the arms race models, we have seen that they are predictively weak, or, to put it in the stricter terminology of H-D theories, they are empirically false. Thus, the laws that are deduced from them are also false and hence unfit as premises for D-N explanations.

Therefore, the reasons why there are no adequate D-N explanations of international phenomena are: (1) there are very few laws available in international politics and (2) those which are available are not strictly true, or even highly confirmed.

V. CONCLUSION

In the preceding pages, I have attempted to answer most, if not all, of the questions put forth in the introduction. Since these conclusions have been explicitly stated throughout the paper, it seems unnecessary to repeat them all, especially the more specific ones, here. We have already summarized many complex arguments to their limit, I think, and to further separate conclusions from the intricate arguments that engendered them appears to me to be counterproductive. Accordingly, the argument of this thesis stands as its own conclusion and this final section will merely attempt a brief recapitulation of the more general and important points that follow from it.

(1) Most importantly, I believe this study has shown that philosophy of science is a valuable tool for examining the underlying assumptions of different approaches to political science and international politics. By getting to the roots of deductive formalism, we have been able to evaluate it prior to its application to political phenomena, and, as a result, have been able to examine its fundamental strengths and weaknesses separately from the very different types of problems associated with its application to a specific subject matter. This analytical distinction has combatted conceptual confusion and greatly simplified the difficulties inherent in a study which proceeds

simultaneously on several levels of analysis.

A subsidiary aspect of this point is that the form-content-context framework appears to be helpful in differentiating the various approaches to philosophy of science. In a more ambitious project, it would be interesting to employ this framework in an explicit comparison of different approaches in relation to the needs of political science.

(2) I hope this analysis has succeeded, at the very minimum, in making clear that science is a problematic notion susceptible to many interpretations, and that the correct definition of science or the scientific method simply do not exist. To the extent that these myths have been exposed, this thesis stands as a contribution toward discouraging the unfortunate, and all too prevalent, tendency among political scientists to look for quick and simple solutions to complex methodological or epistemological problems within the pages of one or two well-regarded philosophy of science texts. The world, I hope I have shown, is more complicated than that.

On the other hand, I hope this study will not be interperted as implying that philosophy of science can solve all our problems in political science. On the contrary, I have tried to make it clear from the beginning that it is the empirical content of the political world which must dictate methodological innovation. Philosophy of science is only of value to the degree that it illuminates this process.

(3) Turning to somewhat less general issues, we have noted considerable evidence in section II that the conception of science as deductive formalism is beset by many more difficulties than one would be

led to believe by examining the deductive literature in social or political science. This conclusion lends credence to Gunnell's first assertion that the deductive models are indeed "controversial" and subject to serious objections within philosophy of science. Although there is no need to rehash the specific objections here, I would like to note that the criticism concerning the contradictory views of laws in H-D theories and D-N explanations, unlike all the other criticisms, is not brought out in any of the critiques of deductivisn I have consulted in writing this paper. As a result, I regard it as more tenuous than the others, and feel it would profit from further careful analysis at some future time.

(4) The most telling argument against the use of deductive formalism as a model for a science of international politics, I think, is not a product of the many criticisms that can be put against it. Rather, it results from a property intrinsic to the approach and well accepted by its philosophical adherents. This is that the deductive models are not meant to reflect how scientists actually formulate or implement their theoretical and explanatory accounts. As has been emphasized time and again by Hempel and others, the models are unconcerned with methodological or procedural considerations. As a result, their use as prescriptive schemata for international politics must be nugatory. When this fact has been ignored, as by Holt and Richardson, it has led to particularly unpalatable conclusions.

(5) The discussion in section III seems to attest to the accuracy of Gunnell's second assertion that political scientists do not fully

understand the deductive models and their philosophical implications. In light of the study as a whole, we can conclude from this that it would probably not be of much value to concentrate on applying a more accurate view of the models to international politics or political science. A more profitable course would probably involve intensifying the search for viable alternative conceptions of science that reflect more closely the epistemological concerns of the discipline.

(6) The main point trought out by the analysis in section IV is that the attempt to implement deductive theories of international politics has suffered primarily from a lack of empirical relevance. As a result, these theories have been defended in terms of their heuristic rather than their formal value. This pragmatic failing of the deductive approach is in many ways complementary to its philosophical failings discussed in section II. Once again, the study as a whole has shown that the best response to this is not to expend more energy in attempting to combine logical rigor and empirical breadth. All the evidence we have examined seems to support Hanson claim that the two are inversely related. Thus, in political research we should strive not to achieve both maximum rigor and maximum breadth, but to balance them as skillfully as possible in light of whatever problem is under consideration.

VI. FOOTNOTES

[1]New York, Alfred A. Knopf, 1953.

[2]In international politics, this is the second controversy to be dubbed a 'Great Debate'. The first, which reached a peak in the late 1940s, involved the relative merits of realism and idealism as criteria for guiding and evaluating foreign policy. See especially Hans.J. Morgenthau, "Another 'Great Debate': The National Interest of the United States," American Political Science Review, vol. 57, no. 4 (December 1952), pp. 961-88. The degree to which this earlier debate was concerned with actual policy formulation can be contrasted to the much less "policy relevant" nature of present one throughout the 1950s and 1960s. The relationship between theory and practice is now once again becoming prominent, however, and the imaginative observer can note the wedding of the two debates in such volumes as Raymond Tanter and Richard Ullman, Eds., Theory and Policy In International Relations, Princeton, Princeton University Press, 1972.

[3]See, e.g., Easton, op. cit.; David Truman, "The Impact on Political Science of the Revolution in the Behavioral Sciences," in Research Frontiers in Politics and Government, Eds. S Bailey et al., Washington, The Brookings Institute, 1955; Herbert Storing, Ed., Essays on the Scientific Study of Politics, New York, Holt, Rinehart, and Winston, 1962. With special reference to international politics, see Klaus Knorr and James Rosenau, Eds., Contending Approaches to International Politics, Princeton, Princeton University Press, 1969, especially articles by Hedley Bull, "International Theory: The Case for the Classical Approach," and Morton A. Kaplan, "The New Great Debate: Tradition vs. Science in International Relations."

[4]See, e.g., David Easton, "The New Revolution in Political Science," American Political Science Review, vol. 63, no. 4 (December 1969), pp.1051-61; Charles A. McCoy and John Playford, Eds., Apolitical Politics: A Critique of Behavioralism, New York, Thomas Y. Crowell Co., 1967; Sheldon Wolin, "Political Theory as a Vocation," American Political Science Review, vol. 63, no. 4 (December 1969), pp. 1062-82. A 'post-behavioral' literature aimed directly at international politics has not appeared as yet. See, however Marshall Windmiller, "The New American Mandarins," The Dissenting Academy, Ed. Theodore Roszak, New York, Random House, 1967, for possible directions of attack.

[5]This literature is a major source for some of the ideas to be developed in this thesis. See, especially, M.W. Jackson, "The Application of Method in the Construction of Political Science Theory," Canadian Journal of Political Science, vol. 5, no. 3 (September 1972), pp. 402-17; Paul F. Kress, "On Method in Science: A Reply to Jackson," and Jackson's rejoinder, "Words and the World: Kress and Some Dogmas of Pragmatism," Canadian Journal of Political Science, vol. 7, no. 1 (March 1972), pp. 143-54; John Gunnell, "Deduction, Explanation, and Social Scientific Inquiry," American

Political Science Review, vol. 63, no. 4 (December 1969), pp. 1233-46; responses by Arthur S. Goldberg, A. James Gregor, and Gunnell's rejoinder, ibid., pp. 1247-62; Eugene F. Miller, "Positivism, Historicism, and Political Inquiry," American Political Science Review, vol. 66, no. 3 (September 1972), pp. 796-817; responses by David Braybrooke and Alexander Rosenberg, Richard Rudner, Martin Landau, and Miller's rejoinder, ibid., pp. 818-73.

[6]Radical, or, to use another neologism, meta-political analysis has certainly not been met with indifference by the various combatants. The reader has merely to note the vituperative edge evident in the above two symposia in order to get a feeling for the reaction. Such a response usually means that at least some sensitive nerves have been touched. To note some examples of radical analysis: four books which I feel have been important, if not completely successful illustrations are Floyd W. Matson, The Broken Image, New York, George Braziller, 1964; A.R. Louch, Explanation and Human Action, Berkeley and Los Angeles, University of California Press, 1966; Henry Kariel, Open Systems, Itasca, Ill., Peacock, 1969; and Thomas L. Thorson, Biopolitics, New York, Holt, Rinehart and Winston, 1970.

[7]M.W. Jackson, "Towards a Science of Politics," paper presented to the 43rd Annual Meeting of the Canadian Political Science Association, St. Johns, 1971, p. 75.

[8]Ernan McMullin, "The History and Philosophy of Science: A Taxonomy," in Minnesota Studies in the Philosophy of Science: Vol. 5, Historical and Philosophical Perspectives on Science, Ed. R. Stuewer, Minneapolis, University of Minnesota Press, 1970, p. 31.

[9]Ibid., pp. 15-16.

[10]The following three paragraphs lean heavily on ibid., pp. 23-29.

[11]Ibid., p. 60, emphasis in original.

[12]An examination of the epistemological difficulties to which this approach is susceptible would make an interesting thesis in itself. Unfortunately, such an analysis is outside the scope of this essay. This may lead the reader to the erroneous assumption that the historical approach is the "hero" of this analysis, while the logical approach is the "villain." Although the latter part is probably a fair interpretation, the former is more questionable. A few brief points can be made here: Whereas the "tragic flaw" of logical analysis is a priorism, that of historical analysis is relativism. The first gives us rules of conduct, but why should we believe them? The second is extremely valuable for falsifying logical prescriptions, by showing their inapplicability to science as it is practiced, but how can it verify its own prescriptions about "successful" scientific practice? How can different historical episodes be compared without resorting to trans-historical, a priori,

principles of success and failure. The answers are not simple. Thus, in this thesis the "white hat" will be assigned to historical analysis only in its critical role, not in its prescriptive role. For a good summary of these issues, see Miller, op. cit., esp. pp. 806-17; and Martin Landau, "Comment: On Objectivity," American Political Science Review, vol. 66, no. 3 (September 1972), pp. 846-56, esp. pp. 848-49.
For a solution, see Stephen Toulmin, Human Understanding, Princeton, Princeton University Press, 1972.

[13]John G. Gunnell, "Social Science and Political Reality: The Problem of Explanation," Social Research, vol. 35, no. 1 (Spring 1968), p. 163.

[14]Miller, op. cit., pp. 861-62.

[15]Abraham Kaplan, The Conduct of Inquiry, San Francisco, Chandler Publishing Co., 1964, p. 4.

[16]Stephen Toulmin has most thoroughly and explicitly dealt with these two specific problems, especially in Human Understanding. They are of course, highly interrelated. On "reasons," see pp. 222-42; on "causes," pp. 242-60; and on the relative roles of internal and external factors, pp. 200-22. These subjects are also considered, if less thoroughly by other philosophers of science of various persuasions. See especially Thomas Kuhn, The Structure of Scientific Revolutions, 2nd ed., enlarged, Chicago, University of Chicago Press, 1970, Chs. V-IX; Carl G. Hempel, Aspects of Scientific Explanation, New York, The Free Press, 1965, pp. 425-32, 463-86; Paul Feyerabend, "Consolations for the Specialist," in Criticism and the Growth of Knowledge, Eds. Imre Lakatos and Alan Musgrave, Cambridge, Cambridge University Press, 1970.

[17]Toulmin, op. cit., p. 130.

[18]Miller, op. cit., p. 862.

[19]Ibid.

[20]Gunnell, "Deduction, Explanation, and Social Scientific Inquiry," p. 1233.

[21]Gunnell, "Social Science and Political Reality," p. 167.

[22]Kaplan, op. cit., p. 3.

[23]Ibid., p. 8.

[24]Ibid., pp. 8-9.

[25]Ibid., p. 10, emphasis in original.

[26]*Ibid.*, p. 11.

[27]Although essentially critical, this study will hopefully be at least implicitly constructive. When we point out that the deductive formalist conception of science fails to come to grips with certain characteristics of international politics, we will in effect be stating a requirement that an adequate conception of science will have to fulfill. Thus, behind every criticism is an implicit recommendation. The positive statement of these recommendations must await a future study, however.

[28]At the risk of pressing what might already appear an obvious point, I would like to emphasize the peripheral relationship of these two characteristics to the practice of science. Kaplan, in discussing a similar issue, notes an analogy to baseball. The goal of baseball is winning, and the goal is pursued by scoring runs when you are batting and preventing them when you are in the field. To say this is to say nothing about the different ways of pitching, hitting, or running bases; ways of fielding; managerial strategies for pinch runners and hitters; ways of signalling, coaching, or maintaining team spirit. It merely delimits the boundaries within which these activities and strategies go on. Thus, calling explanation the "essence" of science is as empty as calling winning the "essence" of baseball. Kaplan, *op. cit.*, p. 27.

[29]On explanation as a goal of science, see, e.g., Ernest Nagel, "The Nature and Aim of Science," in *Philosophy of Science Today*, Ed. Sidney Morgenbesser, New York, Basic Books, Inc., 1967, p. 5; Eugene Meehan, *The Theory and Method of Political Analysis*, Homewood, Ill., Dorsey Press, 1965, p. 88; Toulmin, *op. cit.*, p. 364; Hempel, *Aspects of Scientific Explanation*, pp. 333-34.

[30]See, e.g., Nagel, *op. cit.*, pp. 3-4; Hempel, *op. cit.*, p. 333.

[31]These categories bear a family resemblance to the familiar three dimensions of signs or logical space; syntax or sign design, semantics or meaning, and epistemological status or justification. The similarity is more than a coincidence, but less than an isomorphism. One part of the form of an explanation is the analytic or synthetic form of each of its statements. Likewise, one part of the content is the contingent or non-contingent status of the truth of its statements; and one part of the context concerns whether the statements are justified *a priori* or *a posteriori*. But each category involves many other considerations as well, as will become clear as the analysis develops. On "signs," see C.W. Morris, *Signs, Language and Behavior*, New York, Prentice-Hall, Inc., 1946; on logical space, see N.R.Hanson, *Observation and Explanation: A Guide to Philosophy of Science*, New York, Harper and Row, 1971, pp. 49-52.

[32]On the role of theory in making explanations comprehensible, see, e.g., Willard C. Humphreys, *Anomalies and Scientific Theories*, San Francisco, Freeman, Copper, and Co., 1968, p. 106; Meehan, *op. cit.*, pp. 128-29; Kaplan, *op. cit.*, p. 302; N.R. Hanson, *Patterns of Discovery*, Cambridge, Cambridge University Press, 1961, pp. 70-71.

[33]Hans Reichenbach, Experience and Prediction, Chicago, University of Chicago Press, 1938, p. 6.

[34]Reichenbach, The Rise of Scientific Philosophy, Los Angeles and Berkeley, University of California Press, 1966, p. 231.

[35]Hanson, Patterns of Discovery. See also Hanson, "Logical Positivism and the Interpretation of Scientific Theories," in The Legacy of Logical Positivism, Ed. Peter Achinstein and Stephen F. Barker, Baltimore, The Johns Hopkins Press, 1969, pp. 57-84.

[36]Kaplan, op. cit., p. 7.

[37]See, e.g., the discussion in Richard Rudner, Philosophy of Social Science, Englewood Cliffs, N.J., Prentice-Hall, Inc., 1966, pp. 7-8.

[38]Hempel, Aspects of Scientific Explanation, p. 384. A second logical operation, called modus tollens, also achieves the deductive link. In this case, we are given the information 'if D then P' and 'not D'. From this we can logically deduce 'not P'. Thus, whereas modus ponens reasons from a positive conclusion, modus tollens reasons from a negative one. For a comprehensive source on deductive argumentation, see Irving M. Copi, Symbolic Logic, 2nd ed., New York, Macmillan Co., 1965.

[39]May Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," in Minnesota Studies in the Philosophy of Science: Vol. 3, Scientific Explanation, Space and Time, Eds. Herbert Feigl and Grover Maxwell, Minneapolis, University of Minnesota Press, 1962, p. 239, emphasis in original. See also, e.g., Hempel, "Inductive Inconsistencies," in Aspects of Scientific Explanation, pp. 60-61.

[40]Hempel, Aspects of Scientific Explanation, p. 412.

[41]Ernest Nagel, The Structure of Science, New York, Harcourt, Brace, and World, 1961, p. 11.

[42]Michael Scriven, "Logical Positivism and the Behavioral Sciences," in Achinstein and Barker, Eds., op. cit., p. 197, emphasis in original.

[43]Several excellent introductions to the logical positivist movement are available. See, e.g., Achinstein and Barker, Eds., op. cit., especially the article by Herbert Feigl, "The Origin and Spirit of Logical Positivism;" A.J. Ayer, Ed., Logical Positivism, Glencoe, Ill., The Free Press, 1959, especially Ayer's "Introduction;" J.O. Ormson, Philosophical Analysis, Oxford, Clarendon Press, 1956; Paul A. Schilpp, Ed., The Philosophy of Rudolf Carnap, La Salle, Ill., Open Court, 1963; Victor Kraft, The Vienna Circle, trans. Arthur Pap, New York, Philosophical Library, 1953; and Joergen Joergensen, The Development of Logical Empiricism, Chicago, University of Chicago Press, 1951.

[44] Carl G. Hempel and Paul Oppenheim, "The Logic of Explanation," Philosophy of Science, vol. 15 (1948), pp. 135-75; reprinted in Aspects of Scientific Explanation as "Studies in the Logic of Explanation," pp. 245-90.

[45] The main revision has been the addition of a logically distinct class of complete scientific explanations -- inductive and deductive statistical explanations -- in the early 1960s. This addition is meant to fill the obvious gap in the model concerning statistical explanation, which is used quite extensively in all the sciences, physical as well as social. It constitutes in one respect a concession on Hempel's part in that it retracts his earlier position that all explanation in science must be deductive. On the other hand, the new statistical model is intimately tied to the original model in that, although not deductive, it is still explanation under subsumption of covering laws. The difference is that these laws are of the form "the probability that A is B, is p," rather than the nomological form "all A's are B's." This difference has important implications, but, unfortunately, I will be able to say little about them in this thesis. Some aspects of the critique of D-N explanation will also apply to statistical explanation, such as those dealing with the adequacy of covering laws in guaranteeing complete explanations. But many others will be inapplicable and it appears that a satisfactory account of statistical explanation would, quite simply, require another paper. For Hempel's treatment of statistical explanation, see "Deductive-Nomological vs. Statistical Explanation," Minnesota Studies in the Philosophy of Science: Vol. 3, pp. 98-169; and Aspects of Scientific Explanation, pp. 376-412.

[46] It might be worthwhile to note here that D-N explanation is not synonymous with causal explanation, as many proponents of a deductive approach to social science seem to believe. Hempel points out that:

> Causal explanation in its various degrees of explicitness and precision is not...the only mode of explanation on which the D-N model has a bearing. For example, the explanation of a general law by deductive subsumption under theoretical principles is clearly not an explanation by causes. But even when used to account for individual events, D-N explanations are not always causal.

Aspects of Scientific Explanation, p. 352.

[47] Ibid., p. 247; also p. 337.

[48] Ibid., p. 248; also pp. 424-25.

[49] Ibid., pp. 264-69.

[50] Brodbeck, op. cit., pp. 245-46, emphasis in original.

[51] Whether or not they are impossible to come by is another, perhaps more exciting, issue. See, e.g., Matson's discussion of the implications of quantum physics in The Broken Image, pp. 129-155.

[52] Brodbeck, op. cit., p. 247.

[53] Hempel, Philosophy of Natural Science, Englewood Cliffs, N.J., Prentice-Hall, Inc., 1966, p. 56.

[54] Ibid., emphasis in original. Hempel credits the formulation of this difference to Nelson Goodman, Fact, Fiction, and Forecast, 2nd ed., Indianapolis, Bobbs-Merrill Co., 1965, ch. 1, "The Problem of Counterfactual Conditionals."

[55] Ibid.

[56] Ibid., emphasis in original.

[57] See, e.g., Nagel, op. cit., pp. 49-52.

[58] The analytic-synthetic distinction has recently come under strong attack in the philosophy of science. This attack focusses on the boundary line between the two, which has proven to be less definite than first thought. However, these considerations need not concern us here, since we are dealing with the distinction only in its broadest sense. The ambitious reader is referred to W.V.O. Quine, From a Logical Point of View, Cambridge, Mass., Harvard University Press, 1953, 2nd ed., 1961, pp. 20-37.

[59] Observation and Explanation, p. 58. See also Alan Ryan, The Philosophy of the Social Sciences, London, MacMillan and Co., 1970, pp. 55-61; and Hempel, Aspects of Scientific Explanation, pp. 315-47.

[60] Michael Scriven, "Explanations, Predictions, and Laws," in Minnesota Studies in the Philosophy of Science, Vol. 3, p. 212.

[61] One important distinction we have not considered is the difference between empirical generalizations and accidental universals. If indeed there is such a distinction, I think it can be described as follows: an empirical generalization must be equivalent to a finite conjunction of single observations, whereas an accidental universal can be infinite in scope. To return to the rocks, we can thus say that 'all rocks in this box contain iron' is independent of the number of rocks in the box and of any particular individual rock. It is an accidental universal. On the other hand, if we say 'all 43 rocks in the box contain iron', our generalization is based on the conjunction of a finite number of individual observations and is an empirical generalization. It appears that accidental universals are more easily confused with laws than are empirical generalizations. But it also appears that social scientists are more likely to attempt deductions from empirical generalizations than from accidental universals.

[62]Paul E. Meehl, "Nuisance Variables and the Ex Post Facto Design," in Minnesota Studies in the Philosophy of Science: Vol. 4, Analysis of Theories and Methods of Physics and Psychology, Eds. Michael Radner and Stephen Winokur, Minneapolis, University of Minnesota Press, 1970, p. 390, emphasis in original.

[63]Ibid., pp. 390-91, emphasis in original.

[64]Michael Scriven, "Truisms as the Grounds for Historical Explanations," in Theories of History, Ed. Patrick Gardiner, New York, The Free Press, 1959, p. 458, emphasis in original.

[65]Aspects of Scientific Explanation, p. 248.

[66]Ibid.

[67]Ibid.

[68]Ibid., fn. added in 1964, emphasis in original.

[69]Willard Humphreys, op. cit., pp. 69-70, emphasis in original.

[70]Brodbeck, op. cit., p. 143, emphasis in original.

[71]Brodbeck,"Explanation, Predictions, and Laws," p. 312. See also Scriven, "The Key Property of Physical Laws -- Inaccuracy," in Current Issues in the Philosophy of Science, Eds. Herbert Feigl and Grover Maxwell, New York, Holt, Rinehart, and Winston, 1961.

[72]Aspects of Scientific Explanation, p. 249.

[73]We can briefly mention here three kinds of 'incomplete' explanations and how they fall short of the D-N ideal. (1) Elliptical explanation is incomplete only in the sense that the covering laws involved are not specified explicitly, usually because of their obviousness. It is easily made complete by performing the required specification. (2) Partial explanation involves premises that logically lead to a class of conclusions rather that the unique conclusion the explanation is meant to explain. It differs from a statistical explanation in that the latter also leads to a class of phenomena, but is not meant to be any more specific. Note, incidently, that a statistical explanation, although not deductive, is considered by Hempel to be complete by its own standards. (3) An explanation sketch is the least complete type of explanation. It is less specific and explicit than a partial explanation, and often contains vague hypotheses and problematic empirical references. For more details see Hempel, ibid., pp. 415-24.

[74]Ibid., p. 249.

[75]See, e.g., N.R. Hanson, "On the Symmetry Between Explanation and Prediction," The Philosophical Review, vol. 68 (1959), pp. 349-58; Stephen Toulmin, Foresight and Understanding, Indianapolis, Indiana University Press, 1961, ch. 2; Scriven, "Truisms as the Grounds for Historical Explanation," and "Explanations, Predictions, and Laws."

[76]Brodbeck, op. cit., p. 253, emphasis in original.

[77]Aspects of Scientific Explanation, p. 367, emphasis in original.

[78]Oran Young, "The Perils of Odysseus: On Constructing Theories in International Relations," Tanter and Ullman, Eds., op. cit., p. 183.

[79]Davis Bobrow, "The Relevance Potential of Different Products," ibid., p. 210.

[80]Another example can be found in Graham Allison, Essence of Decision: Explaining the Cuban Missile Crisis, Boston, Little, Brown, and Co., 1971, p. 279. This is a particularly interesting example of the use of philosophy of science positions for purely honorific purposes. Allison, on the page noted, declares that he will employ Hempel's concept of explanation in his study and that this implies, among other things, that "prediction is essentially the converse of explanation." But the overall argument of the book is actually a very profound challenge to Hempel's position, since it holds that logically incomplete explanations can adequately explain the outcomes of complex events even when the premises of these explanations contradict each other. Thus, it stands as an important vindication of the view that understanding may be better served by the development of incomplete explanations within carefully selected contexts than by an attempt to develop a single, logically-rigorous explanatory framework.

[81]See, e.g., Scriven, "Explanations, Predictions, and Laws," p. 179.

[82]Toulmin, Foresight and Understanding, pp. 24-25.

[83]Michael Scriven, "Explanation and Prediction in Evolutionary Theory," Science, vol. 30, no. 3374 (August 28, 1959), p. 477. See also, e.g., Kuhn, op. cit., p. 172; Kaplan, op. cit., p. 347; George Gaylord Simpson, The Meaning of Evolution, New Haven, Yale University Press, 1949, pp. 261-62.

[84]Aspects of Scientific Explanation, p. 370.

[85]See, e.g., Michael T. Ghiselin, The Triumph of the Darwinian Method, Berkeley, University of California Press, 1969; David Hull, Darwin and his Critics, Cambridge, Mass., Harvard University Press, 1973; Donald T. Campbell, "Variation and Selective Retention in Socio-Cultural Evolution," General Systems Yearbook, vol. 14 (1969); Toulmin, Human Understanding, Ch. 5.

[86]Stephen Toulmin, review of Aspects of Scientific Explanation, Scientific American, vol. 214, no. 2 (February 1966), p. 130.

[87]Aspects of Scientific Explanations, p. 426.

[88]Ibid., p. 413.

[89]Brodbeck, op. cit., p. 231.

[90]Toulmin, Human Understanding, p. 58.

[91]Ibid., p. 63.

[92]Aspects of Scientific Explanation, p. 425.

[93]

> ...quite broadly speaking, an opinion as to what laws hold in nature and what phenomena can be explained surely cannot be formed on analytic grounds alone, but must be based on the results or empirical research.

Ibid.

[94]Ryan, op. cit., p. 76.

[95]Humphreys, op. cit. 106.

[96]Hempel, Philosophy of Natural Science, p. 70.

[97]Gustav Bergmann, Philosophy of Science, Madison, University of Wisconsin Press, 1957, p. 78. See also Meehan, op. cit., p. 129. This view of the explanatory purpose of theories is not universally shared, of course. For instance, Wilfred Sellers notes:

> Theories about observable things do not explain empirical laws, they explain why observable things obey, to the extent that they do, these empirical laws; that is, they explain why individual objects of various circumstances of the observation framework behave in those ways in which it has been inductively established that they do behave.

"The Language of Theories," in Feigl and Maxwell, Eds., op. cit., p. 71, emphasis in original.

[98]Kaplan, op. cit., p. 298, emphasis in original.

[99]R.B. Braithwaite, Scientific Explanation, Cambridge, Cambridge University Press, 1953, p. 12.

[100]Philosophy of Natural Science, p. 58.

[101]Hanson, "Logical Positivism and the Interpretation of Scientific Theories," p. 58, emphasis in original. The conception of theory described here is most often associated with the early logical positivists. See, e.g., N.R. Campbell, Physics: The Elements, Cambridge, Cambridge University Press, 1920, reprinted as Foundations of Science, New York, Dover, 1957; R.P. Ramsey, The Foundations of Mathematics, London, Routledge and Kegan Paul, 1931; Rudolf Carnap, Foundations of Logic and Mathematics, Chicago, University of Chicago Press, 1939. It still attracts many adherents, however. See, e.g., Braithwaite, op. cit.; H.E. Kyburg, Philosophy of Science: A Formal Approach, New York, Macmillan, 1968; Patrick Suppes, "The Desirability of Formalization in Science," Journal of Philosophy, vol. 65 (1968), pp. 651-64.

[102]Carl G. Hempel, "On the 'Standard Conception' of Scientific Theories," in Minnesota Studies in the Philosophy of Science: Vol. 4, p.152.

[103]Ibid.

[104]Hanson, "Logical Positivism and the Interpretation of Scientific Theories," p. 84, emphasis in original.

[105]Ibid., pp. 76-77, emphasis in original.

[106]Hempel, "On the 'Standard Conception' of Scientific Theories, p. 148.

[107]J.H. Woodger, The Axiomatic Method in Biology, Cambridge, Cambridge University Press, 1937; The Technique of Theory Construction, Chicago, University of Chicago Press, 1939.

[108]Toulmin, review of Aspects of Scientific Explanation, p. 133.

[109]Hanson, "Logical Positivism and the Interpretation of Scientific Theories," p. 76.

[110]Hempel, "On the 'Standard Conception' of Scientific Theories, p. 142.

[111]Ibid., p. 143, emphasis in original.

[112]Ibid., p. 153.

[113]Hempel, Philosophy of Natural Science, p. 74.

[114]"On the 'Standard Conception' of Scientific Theories, p. 144.

[115]Philosophy of Natural Science, p. 74.

[116]Probably the most influential presentation of the 'standard approach' criticized here is Ernest Nagel, The Structure of Science. With reference to theories, Nagel states (p. 90):

> For the purposes of analysis, it will be useful to distinguish three components in a theory: (1) an abstract calculus that is the logical skeleton of the explanatory system, and that "implicitly defines" the basic notions of the system; (2) a set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment; and (3) an interpretation or model for the abstract calculus, which supplies some flesh for the skeletal structure in more or less familiar conceptual or visualizable materials.

We will not be able to discuss the model component here, since it is a heuristic rather than logical aspect of a theory. See Nagel's analysis of models, ibid., pp. 95-97, 107-17; also Kaplan, op. cit., pp. 258-93. Other important sources of the standard view include Braithwaite, op. cit.; Rudolf Carnap, "The Methodological Character of Theoretical Concepts," in Minnesota Studies in the Philosophy of Science: Vol. 1, The Foundations of Science and the Concepts of Psychology and Psychoanalysis, Eds. Herbert Feigl and Michael Scriven, Minneapolis, University of Minnesota Press, 1956, pp. 38-76; Carnap, Philosophical Foundations of Physics, Ed., M. Gardner, New York, Basic Books, 1966. Hempel's own contribution to the construal he now criticizes has also been substantial. See his Fundamentals of Concept Formation in the Empirical Sciences, Vol. 2, no. 7 of The International Encyclopedia of Unified Science, Chicago, University of Chicago Press, 1952; and "The Theoretician's Dilemma: A Study in the Logic of Theory Construction," reprinted in Aspects of Scientific Explanation, pp. 173-226.

[117]"On the 'Standard Conception' of Scientific Theories," pp. 158-59, emphasis in original.

[118]Ibid., p. 159.

[119]Ibid., p. 160; quote from W.V.O. Quine, "Carnap and Logical Truth," in The Ways of Paradox and Other Essays, New York, Random House, 1966, p. 112.

[120]"On the 'Standard Conception' of Scientific Theories," p. 161.

[121]Ibid.

[122]It should be stressed, however, that in no way can Hempel's reformulation be seen as a throwing over of the study of reconstructed theories for the study of actual theories. On this he is quite explicit:

> My misgivings do not concern the obvious fact that theories as actually stated and used by scientists are almost never formulated in accordance with the standard schema; nor do they stem from the thought that a standard formulation could at best represent a theory quick-frozen, as it were, at a momentary stage of what is in fact a continually developing system of ideas. These observations represent no telling criticisms, I think....

Ibid., p. 148.

[123]Wesley Salmon, "Bayes Theorem and the History of Science," in Minnesota Studies in the Philosophy of Science: Vol. 5, pp. 68-69, emphasis in original.

[124]Carl Hempel, "Studies in the Logic of Confirmation," reprinted in Aspects of Scientific Explanation, p. 5, emphasis in original.

[125]McMullin, op. cit., pp. 12-13, emphasis in original.

[126]Salmon, op. cit., p. 72.

[127]Hanson, "Logical Positivism and the Interpretation of Scientific Theories," p. 74, emphasis in original.

[128]Israel Scheffler, Science and Subjectivity, Indianapolis, Bobbs-Merrill, 1967, p. 73. It should be noted that Scheffler's book is an attempt to refute this notion.

[129]Paul Feyerabend, "Explanation, Reduction, and Empiricism," in Minnesota Studies in the Philosophy of Science: Vol. 3, p. 49, emphasis in original.

[130]Paul Feyerabend, "Against Method: Outline of an Anarchistic Theory of Knowledge," Minnesota Studies in the Philosophy of Science: Vol. 4, p. 70, emphasis in original.

[131]See, e.g., Kuhn, op. cit., pp. 8-9; Toulmin, Human Understanding, p. 313; Gunnell, "Social Science and Political Reality," p. 170; "Deduction, Explanation and Social Scientific Inquiry," pp. 1238-39.

[132]Braithwaite, op. cit., p. vii.

[133]Ibid., p. 13.

[134]

> ...total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements.

Re-evaluation of some statements entails re-evaluation of others, because of their logical interconnections...."
W V.O. Quine, From a Logical Point of View, p. 42; quoted in Humphreys, op. cit., p. 109.

[135]Karl Popper, Logic of Scientific Discovery, New York, Science Editions, 1961, pp. 40-41, emphasis in original

[136]Humphreys, op. cit., p. 110.

[137]Braithwaite, op. cit., pp. 19-20.

[138]See the discussion in Kuhn, op. cit., pp. 72-74.

[139]For an excellent consideration of the problem of induction, see Max Black, "The Justification of Induction," in Morgenbesser, Ed., op.cit., pp. 190-200.

[140]Salmon, op. cit., p. 77, emphasis in original.

[141]Hempel, Philosophy of Natural Science, pp. 33-45.

[142]Salmon, op. cit., p. 80, emphasis in original

[143]Ibid., p. 81, emphasis in original.

[144]See, e.g., the concept of "subjective probability" in L.J. Savage, The Foundations of Statistics, New York, Wiley, 1954; and "entrenchment" in Goodman, op. cit. It should be emphasized that these solutions are only inadequate in their ability to save the deductivist point of view. As independent contributions leading toward a post-deductivist approach to confirmation, they are exciting and promising.

[145]Another, tangential, problem relating to Salmon's approach is this: Why should only plausible arguments be allowed as evidential support for new hypotheses? As Paul Feyerabend contends, implausible arguments are also often responsible for creating fruitful new ways of conceptualizing and solving problems. Thus, he observes:

> ...I suggest introducing, elaborating, and propagating hypotheses which are inconsistent either with well-established theories or with well-established facts. Or, as I shall express myself: I suggest proceeding counterinductively in addition to proceeding inductively.

"Against Method," p. 26, emphasis in original.

[146]Carnap's work is summarized in The Continuum of Inductive Methods, Chicago, University of Chicago Press, 1952; and The Logical Foundations of

Probability, Chicago, University of Chicago Press, 2nd ed., 1962. For a more elementary account of his basic ideas, see Carnap, "Statistical and Inductive Probability," in The Structure of Scientific Thought, Ed. E.H. Madden, Boston, Houghton-Mifflin Co., 1960, pp. 269-79; and "The Aim of Inductive Logic," in Logic, Methodology, and Philosophy of Science," Proceedings of the 1960 International Congress, Eds. Ernest Nagel, Patrick Suppes, and Alfred Tarski, Stanford, Stanford University Press, 1962, pp. 303-18.

[147]For a sample of the critique of Carnap's work, see P.A. Schilpp, Ed., The Philosophy of Rudolf Carnap, op. cit., especially articles by Arthur Banks, John Kemeny, Ernest Nagel, and Hillary Putnam; Popper, op. cit.; and Wesley Salmon, The Foundations of Scientific Inference, Pittsburgh, University of Pittsburgh Press, 1968.

[148]Hempel, Philosophy of Natural Science, pp. 80-81, emphasis in original.

[149]Thomas Greene, "Values and the Methodology of Political Science," Canadian Journal of Political Science, vol. 3, no. 2 (June 1970), pp. 281-282.

[150]Paul Feyerabend, "Philosophy of Science: A Subject With a Great Past," Minnesota Studies in the Philosophy of Science: Vol. 5, p. 181, emphasis in original.

[151]Young, op. cit., p. 179.

[152]Ibid.

[153]Ibid., p. 180. It is worthwhile to note that Young cites as references for this definition of theory Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," and Hempel, Philosophy of Natural Science

[154]Ibid., p. 195.

[155]Ibid., p. 197, emphasis in original.

[156]Young's position concerning the relative merits of deductive and inductive theory building procedures has hardened considerably in the last few years. In 1968, he was able to make the following remarks about the complementarity of deductive and inductive methods:

> The conceptual distinction between deductive and inductive procedures...tends to become hazy in the context of many specific projects. Deductive statements are frequently formulated so sloppily that their intellectual status is unclear. Inductive analyses are generally based on at least hunches concerning causal connections. ...And, in

> addition, there is frequently a back and forth relationship between deductive and inductive operations. That is, the inspection of empirical material on an inductive basis is sometimes heuristic in identifying variables and generating insights for deductive formulations. And the resultant deductive formulations are apt to serve as guidelines in directing inductive foraging operations.

Oran Young, "A Systemic Approach to International Politics," Research Monograph No. 33, Center for International Studies, Princeton University, June 30, 1968, pp. 54-55.

[157]Young, "The Perils of Odysseus," pp. 197-99.

[158]This aspect of Young's critique of descriptive and inductive methods -- that the fault lies with the practitioners, not the practice -- is clearly evident in his no-holds-barred attack on Bruce Russett, "Professor Russett: Industrious Tailor to a Naked Emperor," World Politics, vol. 21, no. 2 (April 1969), pp. 486-511. For Russett's reply, see Bruce Russett, "The Young Science of International Poltics," World Politics, vol. 21, no. 4 (October 1969), pp. 87-94.

[159]Young,"The Perils of Odysseus," pp. 196,198.

[160]As Nagel makes clear with reference to the model outlined above (fn. 116):

> We will develop these distinctions in the order just mentioned. However, they are rarely given explicit formulation in actual scientific practice, nor do they correspond to actual stages in the construction of theoretical explanations. The order of exposition here adopted must therefore not be assumed to reflect the temporal order in which theories are generated in the minds of individual scientists.

Nagel, The Structure of Science, p. 90.

[161]Morton A. Kaplan, System and Process in International Politics, New York, John Wiley and Sons, 2nd printing, 1967, pp. 245-46.

[162]Ibid., "Preface to the Paperback Edition."

[163]See, e.g., "Problems of Theory Building and Theory Confirmation in International Politics," in The International System: Theoretical Essays, Eds. Klauss Knorr and Sidney Verba, Princeton, Princeton University Press, 1961, pp. 6-24; "The New Great Debate," in Knorr and Rosenau, op. cit., pp. 39-61; "Glimpses into a Philosophy of Politics," in Macropolitics, Chicago, Aldine Publishing Co., 1969, pp. 3-48.

[164]M.A. Kaplan, Macropolitics, p. 16.

[165] Bobrow, _op. cit._, p. 210.

[166] _Ibid._, p. 209.

[167] Easton, _The Political System_, p. 58.

[168] _Ibid._, pp. 58-59.

[169] Hempel, _Philosophy of Natural Science_, p. 81.

[170] David Easton, _A Framework for Political Analysis_, Englewood Cliffs, N.H., Prentice-Hall, Inc., 1965, p. 19.

[171] For a subtle evaluation of these difficulties, see Karl Deutsch, _The Nerves of Government_, New York, The Free Press, 1963, Chs. I-IV. For a critique of the plausibility of a _general_ theory of politics, see Thorson, _op. cit._, ch. 3.

[172] William H. Riker, _The Theory of Political Coalitions_, New Haven and London, Yale University Press, 1962, p. 3, emphasis in original.

[173] William H. Riker and Peter Ordeshook, _An Introduction to Positive Political Theory_, Englewood Cliffs, N.J., Prentice-Hall, Ins., 1973, p. xi.

[174] Toulmin maintains that the goal of general theory in social science may actually be a sign of immaturity, if we take the physical sciences as an example:

> Later science has demonstrated its maturity not least, in the fact that scientists have given up this prematurely general ambition. Instead, physicists and physiologists now believe that... we shall do better, in these fields, by working our way towards more general concepts progressively, rather than insisting on complete generality from the outset. And if behavioural scientists eventually reach a similar conclusion -- deciding that 'human behavior in general'...represents too broad a domain to be encompassed within a single body of theory -- that could again be a sign of maturity rather than defeatism.

Stephen Toulmin, _Human Understanding_, p. 387.

[175] Robert Holt and John Richardson, Jr., "Competing Paradigms in Comparative Research," in _The Methodology of Comparative Research_, Eds. Robert Holt and John Turner, New York, The Free Press, 1970, p. 24.

[176] Robert Holt and John Turner, "The Methodology of Comparative Research," in _ibid._, p. 6.

[177] _Ibid._, pp. 6-7.

[178] Holt and Richardson, op. cit., p. 70, emphasis in original.

[179] Ibid., p. 70. They also add, significantly, that:

> In making an appeal for more mathematics, we are not talking about statistics. ...[S]tatistics provides a science with a basis for rigorous induction. Our critique suggests that the crying need in comparative politics is for more rigorous deduction and this is where mathematics, not statistics, is relevant.

Ibid., p. 71, emphasis in original.

[180] Ibid., p. 70.

[181] Ibid., pp. 70-71, see also pp. 26, 59.

[182] Holt and Richardson's inhospitable attitude toward socially relevant problems may be a by-product of their uncritical acceptance of Kuhn's early formulations. For example, he states in The Structure of Scientific Revolutions that:

> ...the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. In this respect, also, the contrast between natural scientists and many social scientists proves instructive. The latter often tend, as the former almost never do, to defend their choice of a research problem -- e.g., the effects of racial discrimination or the causes of the business cycle -- chiefly in terms of the social importance of achieving a solution. Which group would one expect to solve problems at a more rapid rate?

Kuhn, op. cit., p. 164. Holt and Richardson's attempt to make social scientists more like physical scientists by stripping away all that is distinctive about their subject matter is a classic, if somewhat horrendous, example of putting the priorities of form above those of context. To the extent that their views result from a reading of Kuhn, it is interesting to note that his ideas have recently come under intense attack within philosophy of science. See, e.g., Lakatos and Musgrave, Eds., op. cit.; and Toulmin, Human Understanding, pp. 96-130. For a more specific critique of applications of Kuhn's ideas to political science, including Holt and Richardson's, see Jerone Stephens, "The Kuhnian Paradigm and Political Inquiry: An Appraisal," American Journal of Political Science, vol. 17, no. 3 (August 1973).

[183] A. James Gregor, "Political Science and the Uses of Functional Analysis," American Political Science Review, vol. 62, no. 2 (June 1968), p. 425.

[184] Ibid.

[185] Ibid., p. 426.

[186] Paul Feyerabend, "Against Method," pp. 21-22.

[187] Gregor, op. cit., p. 439.

[188] Ibid.

[189] Ibid.

[190] Waltham, Mass., Xerox College Publishing, 1971. See also, e.g., A.Isaac, Scope and Method of Political Science, Homewood, Ill., Dorsey Press, 1969.

[191] Meehan's approach in this volume is at odds with that taken in his later Explanation in Social Science, Homewood, Ill., Dorsey Press, 1968, where he develops a strong case against the adequacy of the deductive model of explanation in social science. But this second book, not being addressed specifically to political science, is technically not of the class of textbooks I am discussing here. Neither is Kaplan's The Conduct of Inquiry. Both these books, however, are often employed in introductory methodology courses in political science, thus adding some much needed balance. For further comments on the role of textbooks, see Michael W. Jackson, "Textbooks in Social Scientific Education: A View from Political Science," The New Scholar, vol. 3, no. 2 (1973), pp. 211-20.

[192] This, of course, is a main point of Sheldon Wolin's critique of graduate education in political science. See "Political Theory as a Vocation," op. cit.

[193] See, e.g., Lewis F. Richardson, Arms and Insecurity: A Mathematical Study of the Causes and Origins of War, Pittsburgh and Chicago, Boxwood and Quadrangle, 1960; Paul Smoker, "Fear in the Arms Race: A Mathematical Study," Journal of Peace Research, vol. 1 (1964), pp. 55-63, "The Arms Race: A Wave Model," Peace Research Society (International Papers, no. 4 (1966), pp. 151-92, "The Arms Race as an Open and Closed System," Peace Research Society (International) Papers, no. 7 (1967), pp. 41-62; William R. Caspary, "Richardson's Model of Arms Races: Description, Critique, and an Alternative Model," International Studies Quarterly, vol. 11, no. 1 (March 1967), pp. 63-90; Murray Wolfson, " Mathematical Model of the Cold War," Peace Research Society (International) Papers, no. 9 (1968), pp. 107-23; Jeffrey S. Milstein and William C. Mitchell, "Computer Simulation of International Processes: The Vietnam War and the Pre-World

War I Naval Race," Peace Research Society Papers, no. 12 (1970).

[194] Peter A. Busch, "Appendix: Mathematical Models of Arms Races," What Price Vigilance? The Burdens of National Defense, Bruce M. Russett, New Haven and London, Yale University Press, 1970, p. 194.

[195] Milstein and Mitchell, op. cit. See also the various quantitative studies of the pre-World War I period by Robert North and Nazli Choucri, e.g., "Dynamics of International Conflict: Some Policy Implications of Population, Resources, and Technology," in Tanter and Ullman, op. cit., pp. 80-122.

[196] Bush, op. cit., p. 232.

[197] For further critical discussion of reaction-process models, see Caspary, op. cit.; Martin C. McGuire, Secrecy and the Arms Race, Cambridge, Mass., Harvard University Press, 1965; Anatol Rapoport, Fights, Games, and Debates, Ann Arbor, University of Michigan Press, 1960; Thomas L. Saaty, Mathematical Models of Arms Control and Disarmament, New York, Wiley, 1968; John C. Harsanyi, "Mathematical Models for the Genesis of War," World Politics, vol. 14, no. 4 (July 1962), pp. 689-99.

[198] Busch, op. cit., pp. 194-95.

[199] The best overall introduction to game theory is R. Duncan Luce and Howard Raiffa, Games and Decisions, New York, Wiley, 1957. See also Martin Shubik, Ed., Game Theory and Related Approaches to Social Behavior, New York, Wiley, 1964; Anatol Rapoport, Two-Person Game Theory, Ann Arbor, University of Michigan Press, 1966, N-Person Game Theory, Ann Arbor, University of Michigan Press, 1970. Specific applications to international politics are offered by Morton Kaplan, System and Process in International Politics, Chs. 9-11; Thomas Schelling, The Strategy of Conflict, Cambridge, Mass., Harvard University Press, 1960, Arms and Influence, New Haven, Yale University Press, 1966.

[200] For critiques of the use of game theory in deterrence analyses, see, e.g., Philip Green, Deadly Logic, New York, Schocken, 1968; Alexander George and Richard Smoke, Deterrence Theory and Practice, New York, Columbia University Press, 1974; Karl Deutsch, The Nerves of Government, Ch. 4, The Analysis of International Relations, Englewood Cliffs, N.J., Prentice-Hall, 1968, esp. pp, 112-32; Thomas Milburn, "The Concept of Deterrence: Some Logical and Psychological Considerations, Journal of Social Issues, vol. 17, no. 3 (1961); Anatol Rapoport, Strategy and Conscience, New York, Harper, 1964.

[201] Richard Smoke, "The Normative-Prescriptive Model of Deterrence," mimeo., Harvard University, 1972. This is scheduled to appear as Ch. 3 of George and Smoke, op. cit.

[202] Schelling, The Strategy of Conflict, p. 163.

[203] *Ibid.*, p. 162, emphasis in original.

[204] Thomas Schelling, "What is Game Theory?" in *Comtemporary Political Analysis*, Ed. James C. Charlesworth, New York, The Free Press, 1967, pp. 219-20.

[205] Princeton, Princeton University Press, 1944.

[206] For an incisive critique of this view of the central role of the rationality principle to economics, see Maurice Godelier, *Rationality and Irrationality in Economics*, London, New Left Books, 1972.

[207] von Neumann and Morgenstern, *op. cit.*, pp. 89.

[208] Norman Frolich and Joe A. Oppenheimer, *An Entrepreneurial Theory of Politics*, unpublished Ph. D. diss., Princeton University, 1971, "Entrepreneurial Politics and Foreign Policy," in Tanter and Ullman, *op. cit.*, pp. 151-78.

[209] On alliances, see Mancur Olson, Jr. and Richard Zeckhauser, "An Economic Theory of Alliances," in *Economic Theories of International Relations*, Ed. Bruce M. Russett, Chicago, Markham, 1968, pp. 25-45, "Collective Goods, Comparative Advantage, and Alliance Efficiency," in *Issues in Defense Economics*, Ed. Roland N. McKean, New York, Columbia University Press, 1967, pp. 25-63. Leadership is examined in Norman Frolich, Joe A. Oppenheimer, and Oran Young, *Political Leadership and Collective Goods*, Princeton, Princeton University Press, 1971. Participation studies include Mancur Olson, Jr., *The Logic of Collective Action*, Cambridge, Mass., Harvard University Press, 1965; Albert and Raymond Breton, "An Economic Theory of Social Movements," *American Economic Review*, vol. 59 (May 1969), pp. 196-205; Robert H. Salisbury, "An Exchange Theory of Interest Groups," *Midwest Journal of Political Science*, vol. 13 (February 1969), pp. 1-32.

[210] Young, "The Perils of Odysseus," p. 191. On the attributes of oligopolistic markets, see Paul A. Samuelson, *Economics*, 5th ed., New York, McGraw-Hill, 1961, Ch. 25; and William J. Fellner, *Competition Among the Few*, New York, Alfred A. Knopf, 1949.

[211] Kenneth Boulding, *Conflict and Defense, A General Theory*, New York, Harper Torchbooks, 1962, see p. viii.

[212] Anthony Downs, *Inside Bureaucracy*, Boston, Little, Brown, and Co., 1967; Gordon Tullock, *The Politics of Bureaucracy*, Washington, D.C., Public Affairs Press, 1965.

[213] See, e.g., Richard Neustadt, *Alliance Politics*, New York, Columbia University Press, 1970; Graham Allison and Morton Halperin, "Bureaucratic Politics: A Paradigm and Some Policy Implications," in Tanter and Ullman, *op. cit.*, pp. 40-79.

[214] Anthony Downs, An Economic Theory of Democracy, New York, Harper and Row, 1957; Gordon Tullock and James M. Buchanan, The Calculus of Consent: Logical Foundations of Constitutional Democracy, Ann Arbor, University of Michigan Press, 1965.

[215] R.L. Curry and L.L.Wade, A Theory of Political Exchange: Economic Reasoning in Political Analysis, Englewood Cliffs, N.J., Prentice-Hall, 1968.

[216] Riker and Ordeshook, op. cit., p. 11.

[217] Ibid., pp. 11-12.

[218] Kaplan, op. cit., pp. 317-18, emphasis in original.

[219] Luce and Raiffa, op. cit. Riker paraphrases this assumption as follows:

> Given a social situation in which exist two alternative courses of action leading to different outcomes and assuming that participants can order these outcomes on a subjective scale of preference, each participant will choose the alternative leading to the most preferred outcome.

Riker, op. cit., pp. 18-19.

[220] Ibid., p. 19.

[221] Ibid., p. 20.

[222] Ibid., p. 22.

[223] Ibid., p. 23.

[224] Ibid., p. 15.

[225] Ibid., p. 23.

[226] Ibid., p. 27.

[227] Ibid., p. 30.

[228] Ibid.

[229] Ibid., p. 31.

[230] Ibid.

[231] Ibid., p. 211.

[232] Ibid.

[233] *Ibid.*, p. 47.

[234] Ibid., pp. 54-55, emphasis in original.

[235] *Ibid.*, pp. 66-67.

[236] *Ibid.*, p. 67-68.

[237] *Ibid.*, p. 68.

[238] Ibid., p. 71.

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