

## Reflections about Systems "Theorists" in Search of International Politics

### Introduction

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What has been called the "theory about theory" literature in international relations has loomed especially large in that field relative to other sub-disciplines of political science since the wide acceptance of behavioral premises concerning the conduct of political science inquiry. This metatheoretical literature has alternatively been cast in one of two forms: (1) general encompassing analytical schemes which attempt to prescribe future research programs for the discipline together with their accompanying methodologies; and (2) criticisms of these programs largely, but not exclusively, emanating from "traditional" quarters.

The dominant imagery invoked to describe the macrophenomena of international politics has been the "international system". Credit for this "contribution" has generally been claimed by behavioralists, although in fact the system concept has been part of the stock in trade of students of the subject since as early as 1700. The present study, however, scrutinizes the behavioral purposes that "system" has served; and the analysis, at least initially, is undertaken within behavioral parameters. As it proceeds, however, it is found that under scrutiny, even within modern behavioral usage, the term fragments into four distinct senses which, moreover, lay bare significant epistemological conflicts, far more serious than the tired, overstated traditional-behavioral debate itself.

The first of the four major system types that the author identifies is the "theoretical system". Insofar as behavioral systems writers thought they were contributing something new and important to the study of world politics, they developed a fixation, as it were, for an ideal but exceptionally demanding form of theoretical scientific explanation that has rarely proven successful even in the "hard" natural sciences. Indeed, a major thrust of the present argument is that systems writers in international politics first decided on an entirely *a priori* basis, what kind of knowledge of their subject matter was most worth having and then set about to discover how the subject matter could be fitted to it. The difficulty with this line of attack is that the uncovering of theoretical systems proved possible only in cases where matters of non-scientific interest (i.e.,

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matters of intrinsic human interest) were subordinated to criteria of pure science. Although interesting insights into the nature of the subject matter resulted from this approach, the attempt, even on the admission of systems writers, has by and large failed.

The next major system type identified, "directively organized" ones, resumed, in a sense, where theoretical systems left off. Their proponents continued to pursue the logical objective of theoretical systems while organizing their systems around matters they conceded had intrinsic human interest, e.g., systems employing organic analogies. In the present study it has been argued that these attempts, as well as the "logics-in-use" they represent, have also failed to produce results remotely resembling theoretical systems.

The other two system types, together with the characteristic logics that they represent, round out the present classification of system types and provide, in the process, two important ways of supplementing, if not partially displacing, the previous two types. What have been identified as "historical systems" tread a narrow path between history and social science. While historical system writers undertake to explain units of analysis rather than individual events, their critics tend to find their general explanations diaphanously "metaphysical" and their more focused explanations too dependent on the logic of historical explanation to be taken seriously by behavioral scholars. In fact, while using the systems terminology, historical systems writers have been exceedingly wary of behavioral methodological strictures, viewing them as blunt, procrustean and insensible with respect to the field's phenomena. Raymond Aron together with the school of "historical sociology" provide an example of this type.

"Technological systems" form a part of the technical decision-making apparatus. Technological systems writers, although traditionally considered outside the academic literature while increasingly impinging upon it (under the guise of policy-relevance concerns) shed a great deal of light on the epistemological problems of the discipline. This is the case because the academic discipline of international politics itself is, in an important sense, concerned with overwhelmingly practical technological problems rather than the "pure scientific" problems of the most successful natural sciences in which theoretical systems, to the extent that

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they were found anywhere, were discovered. Moreover, paradoxically, there is some reason to speculate that whatever "basic" science there may be in international politics may emerge out of a relationship of close interaction with logics epitomised by technological systems. This would reverse, to some extent, the experience of the most rigorous natural sciences where sure technological applications presuppose the existence of an underlying basic science. However, to the extent to which international politics qua discipline can profitably be viewed as a kind of grand technology it becomes, by definition, non-scientific.

On the assumption that fields of inquiry advance to the extent to which their specific field-dependent logic(s) are uncovered together with their characteristic ways of drawing inferences, the analysis concludes: (1) that theoretical and "directively organized" systems, together with the logical assumptions they represent, will be of marginal usefulness in the development of the field of international relations judging from past experience and considerations of logical plausibility, (2) that the logic implicit in historical system writing (e.g., emphasis on the value of description and historical explanatory forms) is entitled to greater recognition in the cultivation of the field, and (3) the logic of technological systems is ripe with suggestive insights, not only for the solution of specific practical problems, but also with respect to how the discipline of world politics can acquire whatever qualified knowledge it may.

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## System Types As They Have Been Applied

Some might argue that international politics is in the throes of a scientific revolution in the Kuhnian sense. The more conservative would argue that we are in what Kuhn has called a "pre-paradigm" stage of development, for we have briskly swept through traditionalism and behavioralism. Some even think we are on the brink of giving ourselves over to post-behavioralism (the most obvious international relations counterpart being "peace research"), leaving in our wake countless unexplored epistemological assumptions and without having sufficiently articulated these would-be paradigms so as to identify specific, palpable contributions or anomalies.

There are at least two reasons why scholars might be inclined to pursue the natural science model, however. One is idealistic and the other self-serving. The idealistic one, of course, is the excellence of mature scientific knowledge. If the development of such knowledge of our subject matter were possible, there would be little else, intellectually, that we could wish for. Theoretical systems particularly represent the highest form of scientific achievement (where they have been identified) and one can readily appreciate their appeal. The second, perhaps less noble, reason why we might wish to emulate the natural sciences is that without claim to scientific status many international relations scholars believe they would plummet to the bottom of the academic status hierarchy. As Edward Shils, Jerome Ravetz and others have suggested, international politics, like other social sciences, would lack prestige, credibility, and perhaps access to scarce research funds.

Regardless of how one characterizes the field's philosophical differences, all of the problems the debate poses are as big – or bigger – than the field itself. A few of these issues can be scrutinized by penetrating beyond the apparently monolithic facade of "systems analysis". Here it is possible to see a number of important presuppositions in combat in the microcosm of the field that "system" provides. For systems writers of every stripe have said that they find the system concept suggestive as an ordering device. What interests the writer is the different ra-

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tionales for the order of the field's phenomena that they expect to find. It is generally understood by the writers who entertain such questions that the *raison d'être* of international politics (in contrast, say, to history) is the cultivation of systematic, highly qualified knowledge about classes of phenomena. It follows, almost ineluctably, that this knowledge is to be organized in some theoretical form or other. It can be argued, however, that each of the four system types represents a different hunch about where theoretical coherence (however defined) will most probably turn up. Another way to put this is the way Hugh Stretton has in his **The Political Sciences: General Principles of Selection in Social Science and History**. Stretton, in a knowledgeable and critical sweep of the social sciences, tries to discover, as the title of his book suggests, what "principles of selection" of problems, data, evidence, and forms of reasoning operate in the various social science disciplines and how viable they are.<sup>1</sup>

With respect to assumptions concerning respective theoretical rationales, historical and technological systems can be ranged together, for many purposes against theoretical and "directively organized" ones. For one thing, as Johan Galtung has observed, "No science of man seems to be as much torn between the idiographic and the nomothetic traditions in science as the science of international relations",<sup>2</sup> While neither historical nor technological systems can do without theory or, at a minimum, theoretical laws, their relationships with it are much more attenuated. Whether this is the case in principle or in practice, theory, in both cases, is a second thought, as it were, because intellectual coherence of the system type is believed to lie elsewhere. Thus viewed from the point of view of a behavioralist, theory for the historical systems writer is a speculative, impressionistic overlay, not too harmful if kept at arm's length, but pernicious if taken too seriously. It is a source of suggestion and insight but, in one sense, the order it imposes is academic. For example Aron writes, that only a historical sociologist can adequately advise a prince. On the other hand, he has also written on another occasion:

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If we expect a theory of international relations to provide the equivalent of what a knowledge of construction materials provides the builder of bridges, then there is no theory and never will be.<sup>3</sup>

Thus, while historical systems are sufficiently practical for the scholar to proffer advice to a statesman, this knowledge must be almost ineffable and hardly amenable to Arnold Brecht's "intersubjective transmissibility." The order that exists arises out of complex and qualitative patterns of moral, legal, prudential, and political actions. For Aron, it is essential to visualize these patterns as systems; but

systems and social events are undefined in the epistemological sense of the term: as they are experienced by their subjects and observed by historians or sociologists, they neither parcel themselves out into neat and definite subsystems, nor can they be reduced to a small number of variables that could be organized into a body of interconnected propositions.<sup>4</sup>

Although the "theory" of historical systems writers, a compound of legal, moral, strategic, sociological, historical, and political issues, is elusive to the behavioralist<sup>5</sup>, it is the only kind of knowledge worth achieving from the point of view of the historical systems writer. Given the *modus operandi* of the historical systems writer, however, his knowledge is better equipped to explain than predict.<sup>6</sup> As R.N. Rosecrance and J.E. Mueller write:

The truly relevant information for international relations – the exact knowledge of governmental policy and planning decisions – is scarcely ever available (except long after the event); "experts" have to rely on sources once or twice removed: reports of political debates and discussion, statements of public spokesmen, economic statistics, and actual changes of policy which occurred previously.<sup>7</sup>

But it is only given historical events and trends that are worthy and even amenable to explanation.

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Similarly, while successful technological systems must deal with theory (in fact, logically presuppose it), they can develop it as they go. But often, in practice, as Quade and Boucher point out, they never develop it at all. This, as we have seen, often leads to highly **ad hoc** solutions to practical problems. This limitation, however, is not of life – and – death importance to technological systems analysts. If their systems could explain as well as contrive, that would be an unexpected and happy byproduct of their work. Their constructs are designed to achieve something quite specific and, given the social science “theories” available, they often find their less elegant resources more helpful, e.g., expert guesswork, trends, **ad hoc** mathematical models, and all conceivable details that may settle the outcome of a particular course of events whether or not social science has anything systematic to say about them.

While historical systems writers spend most of their time explaining the past and technological systems analysts are concerned with forecasting and even often creating the future, this difference does not identify an important break with respect to logical assumptions. While at first glance, no system type practitioners could seem more disparate than the erudite, philosophically inclined, historical systems scholar and the stereotype of the philosophically illiterate, brainlessly computer-addicted technological systems analyst, this is very far from the fact. We have already seen that theory, in practice, is ancillary to their work in each case. For both types the coherence of their work stems from their degree of success in identifying the necessary and sufficient conditions for the occurrence of some more or less specific phenomenon. The more of these conditions that theory can usefully illuminate, the better. But both kinds of systems writers generally find that for their purposes usable theories are lacking – because they are unoperationalisable, or because, as Morton Kaplan has frequently stated, macro-level theories (like his systems analysis) cannot be used to explain micro phenomena like particular policy decisions.<sup>8</sup>

Without overdramatizing their situations, there is a sense in which the premises and ways of working of both systems types are quite existential. On the one hand, both are thoroughly steeped in matters of intrinsic interest. In the case of technological

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systems it is very easy to see this. A practitioner cannot proceed unless what he sets out to achieve is articulated as comprehensively and precisely as possible. The intrinsic interests of historical systems writers can also be ferretted out. Apart from their close affinities with humanistic tradition, they are generally characterized by an unabashed normative orientation toward their subject matter<sup>9</sup>. Indeed, Stanley Hoffmann has recently come to advocate the investigation of "relevant utopias", blurring the distinction between the two to an even greater extent. The more pronounced technological emphasis is also underscored in George A. Kelly and Linda B. Miller's "Internal War and International Systems: Perspectives on Method," one of the relatively few "historical systems" studies clearly stamped with Aron and Hoffmann's "historical sociology."<sup>10</sup> Describing their particular choice of methodology Kelly and Miller write: "It is not just a technique for putting flesh on paradigmatic bones or looking at the temporal context of occurrences. It is also a therapeutic acquisition of historical sense."<sup>11</sup> This remark combines the normative practical facet of historical systems scholarship with its elusive, almost transcendental quality. It also intimates skepticism about a thorough-going once-and-for-all systematization of international politics. Moreover, the concern, shared by both systems types, for phenomena possessing intrinsic interest means that representatives of both system types resort to a minimum "decomposition" and reconceptualizing of their subject matters, a practice they would find a counter-productive detour. (In the case of technological systems this general rule varies to some extent. Insofar as such systems are used for heuristic purposes (e.g., Harold Guetzkow's Northwestern Simulated International Processes Project) there would be some tendency to look for new theoretical formulations and appropriate corresponding concepts; on the other hand, these "technological systems" become increasingly similar to "directively organized" ones and correspondingly of less immediate value to the decision-maker. But to the extent to which technological systems are designed for immediate or proximate policy-making purposes, they tend less to do this). While technological systems sometimes use methods of data-processing that are so sophisticated that they have acquired the status of separate disciplines, and while they

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are obliged to translate phenomenologically perceived components into esoteric mathematical symbols, the translation is temporary and does not affect the inherent character of the final product. To reiterate, it is something other than theory, as behavioralists view it, that predominates in both cases. In the case of the historical systems writers, it may be an ultimately inexpressible, partly subjective ordering of phenomena, which cannot be vindicated by the usual testing procedures of science.<sup>12</sup> In the case of technological systems, there is also much reliance on inarticulate "intrinsic expertise". But, in the last analysis the logic is supplied by the purely pragmatic problem of what we need to do to get from here to there.

As far as theoretical and "directively organized" systems are concerned, however, since they have foresworn immediate intrinsic interests (although, in the case of "directively organized systems" this is a matter of degree), they are obliged to use different "principles of selection" to go about their work. They assume that their subject matter has pure, inherent theoretical coherence which it will yield sooner or later to persevering, resourceful research. However, yet another presumably authoritative (given the prominence of the author and the journal) verdict has been handed down that international relations still lacks theories in the "proper" (behavioral) sense of the word.<sup>13</sup> These discouraging assessments, however, do not suggest to behavioralists that they are pursuing a dubious philosophical course. The typical response from that quarter is that the discipline is a young one. Behavioralists in international politics vary in their willingness to make the demurrer that international relations is more difficult than other subject matters. John Herz, however, has pointed out that for a number of reasons the relatively short life-span of international politics as an autonomous and scientifically self-conscious field is a misleading explanation. For the numbers of scholars in the field and the character of the computer technology at our disposal should increase exponentially our capacity to learn. Yet, thus far our research efforts have not accumulated in any encouraging way but have led to an increasing proliferation of studies and conclusions, sometimes contradictory, about the same phenomena.<sup>14</sup>

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Each of the three major strategies used for developing theoretical systems has encountered its own distinctive obstacles. The construction of deductive models taken from other fields (e.g., "field theory") simply has not led to the development of the kind of theory that could satisfy the criteria of theoretical systems. They have either remained in the rigorous model stage in which they continue to justify their existence by their "heuristic promise" or have begun to test their insights by the exploration of statistical correlations, many of which have been tautologous, or nearly so, and none of which has been really enlightening.

Another strategy used by proponents of theoretical systems — that of constructing "analytic frameworks" for the discipline — has also reached a dead-end. Rosenau's linkage framework illustrates this. In his own edited volume containing studies exploring the fruitfulness of his scheme he notes that the writers included found it difficult to confine themselves to his framework.<sup>15</sup> Small wonder that this was the case. The framework is a painfully obvious one, so gross that it does not lend itself to particular empirical studies but to investigations which invariably find that their concerns immediately lead them into more profound conceptual problems. The point is that one cannot conceptually order a discipline *a priori* and in the abstract. This is particularly true where the discipline so far lacks apparent stable, objective ontological bases. Any subject matter can accommodate a virtually unlimited number of classification schemes. The crucial requirement is to find the one that parallels the significant features of the phenomena to be explained. It is not difficult to see that unless the discipline has reached a certain level of theoretical development, such a program is bound to fail. In the social sciences, one partial exception to this general difficulty is Talcott Parsons' explication of the social system. To the extent that this conceptualization of the subject matter of sociology has proven durable, it is a reflection of Parsons' attempt to distill from the empirical work and metascientific assumptions of some of the most brilliant minds in sociology (i.e., Weber, Durkheim, Pareto, and the economist Alfred Marshall) a common perspective.<sup>16</sup> Nonetheless, Parsons' own claims to the contrary notwithstanding, this is not theory by any significant connotation of the word.

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The third strategy sometimes pursued by those in search of theoretical systems is the manipulation of statistical correlations. Despite the enormous amount of resources of all kinds invested in this effort, the results have been disheartening. Students of the field who have devoted their energies to this strategy have generally banked on the existence of significant explanatory, synchronic regularities in their data. Recently, scholars have tried to add the diachronic dimension to their search for laws. So far, nonetheless, interesting laws (or even very many interesting statistical generalizations) have not been discovered. Yet this would only be the necessary first step to the construction of a theoretical system.

As if in implicit acknowledgement of these frustrations and recognition that not every aspect of the field's phenomena is as important to explain as every other, the field has oriented itself more and more toward "directively organized systems," without, however, relinquishing most of the methodological premises underlying theoretical systems. This new perspective has led the discipline into a variety of different directions. For example, general equilibria as the focal point for the construction of theoretical systems were abandoned in favour of homeostatic models, which, after all, were more appropriate analogues to human behavior because they were, like people and societies, self-adapting. Although theoretical systems were achieved in the exact sciences only under highly circumscribed conditions, the optimistic, open-minded attitude natural scientists find it useful to maintain is doggedly held to in international politics. But it is under all the circumstances somewhat misunderstood and even, in a sense, exploited. For instance, what has, for some time, been an uneasy and disturbing impression about the reasons for holding to the theoretical systems model (many of whose assumptions, as we have argued, also hold in the case of "directively organized" systems) has recently been conceded by Orna R. Young in his "The Perils of Odysseus." Young explicitly states that one major reason so much time is devoted to the exploration of deductive models, largely borrowed from other disciplines (in Young's case particularly from economics) is not that we find them enticingly plausible and isomorphic analogues of the phenomena of our field but (1) because we know from the history of science what excellent theoretical systems are

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like; and (2) these models possess many of these features, i.e., closure, elegance, and deductive relationships. This, for Young, justifies their manipulation in order to determine how the findings of our field may be brought to relate to them.

Another intriguing illustration of this thinking can be found, unsurprisingly, in the writing of Karl Deutsch, J. David Singer, and Keith Smith. In two papers, one written by Deutsch and another co-authored by Deutsch, Singer and Smith,<sup>17</sup> there is the attempt to set out the features desirable for theories in international politics and elsewhere. Deutsch, for example, argues that theories, conceptual schemes etc., are alike in that they are all "subsets of the general class of codes for storing and transmitting information." Among the criteria Deutsch specifies for evaluating competing theories are "realism, generality, comprehensiveness, prior improbability, the capacity for self-transcendence (i.e., capacity to lead to further scientific discoveries), efficiency ("quantitative ratio of some measure of effectiveness to some measure of cost") and elegance." In "The Organizing Efficiency of Theories: The N/V Ratio as a Crude Rank Order Measure," Deutsch and Singer argue that theories can be assessed in terms of their "organizing efficiency" expressed in terms of a relatively large number of cases related and explained by a relatively small number of variables. The larger the former, the smaller the latter, *ceteris paribus*, the more "efficient the organizing power" of the theory, model, or whatever, and the more valuable the accomplishment.

Now starting from the ideals of "social science naturalism" the views presented in these two papers are unexceptionable. But even, for the moment, granting the appositeness of these premises, there is something jarringly incongruous about how these articles could have been written in the first place. Only if the discipline were being inundated with solid theoretical candidates, containing a respectable number of these features clamoring for recognition, would it seem in order to lay down such stringent *a priori* criteria for theories or analytic frameworks to meet. On the other hand, even in the natural sciences, where scientists could, if they wished, lay down highly demanding standards of excellence for theories to meet (because they have had much more opportunity to see what kind of theo-

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ries their phenomena seem to accommodate) scientists have learned not to look gift horses in the mouth.<sup>18</sup> Instances of Gustav Bergmann's perfect knowledge are few and far between. Natural scientists, assuming certain minimal criteria are met, may in practice be less demanding than Deutsch and Singer appear to be, but with more reason to be so.

Most fundamentally, however, until we have laws and theories of some sort or other, Deutsch, Singer, Smith, and Young seem engaged in a kind of pragmatic pleading that is apt to advance the discipline nowhere, if not backward. To say we will address ourselves to certain kinds of intellectual activities because this is the kind of theory we would like to have (while apparently repressing the knowledge that we had only modest initial expectations and that subsequent experience has placed these cherished wishes further in doubt) is an especially perverse instance of placing the cart before the horse.

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## **Progress in a Field Viewed as the Uncovering of "Field-Dependent" Logic(s)**

It is becoming increasingly appreciated that fields differ with respect to the logics they employ as much as with respect to their subject matters. This general condition has also been found to apply **within** fields of science. Abraham Kaplan has written of the existence of a wide variety of "logics in use" in the sciences at large and in the behavioral sciences to an at least comparable degree.<sup>19</sup> Don K. Price in **The Scientific Estate** writes that the methods of the natural sciences have proliferated in many directions and that natural scientists, as a group share only common denominators that are external to science, e.g., their social status or political impact.<sup>20</sup>

The most systemic analysis of this phenomenon is to be found in Stephen Toulmin's **The Uses of Argument**. Toulmin asserts that many more features of mature fields of knowledge are "field-dependent" than "field-invariant." The logics used to establish conclusions in various disciplines are not, by and large, different renditions of a single "Scientific Method", or a single classic and necessary logical form, but are substantially different, owing to the fact that they are expressions of different empirical and historical circumstances.

Logic conceived in this manner may have to become less an **a priori** subject than it has recently been; so blurring the distinction between logic itself and the subjects whose arguments the logician scrutinises .....

But not only will logic have to become more empirical; it will inevitably tend to be more historical. To think up new and better methods of arguing in any field is to make a major advance, not just in logic, but in the substantive field itself: great logical innovations are part and parcel of great scientific, moral, political or legal innovations. In the natural sciences, for instance, men such as Kepler, Newton, Lavoisier, Darwin and Freud have transformed not only our beliefs, but also our ways of arguing and our

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standards of relevance and proof: they have accordingly enriched the logic as well as the content of natural science. **We must study the ways of arguing which have established themselves in any sphere, accepting them as historical facts; knowing that they may be superseded, but only as the result of a revolutionary advance in our methods of thought. In some cases these methods will not be further justifiable – at any rate by argument: the fact that they have established themselves in practice may have to be enough for us.**<sup>21</sup> [Emphasis supplied].

Consistent with his insistence that methods of reasoning in various disciplines are apt to be **sui generis**, Toulmin cautions that the particular conditions of any field, or its subdivisions, must be seen through "the eye of the naturalist, without preconceptions or prejudices imported from outside."<sup>22</sup> In this sense, international politics could certainly benefit in its time of epistemological turmoil from a more "naturalist" scrutiny than has been evident in the "theory about theory" literature.

In a preliminary and partial way, the writer has tried to contribute to a more "naturalist" scrutiny of the logics of international politics. To this end, I have examined four major system types in the literature, around which have polarized, as a matter of historical fact, a great part of contemporary research in the field. From two different points of view I am now in the position to suggest that in reality there are only three basic system types (or contending logics), because the "directively organized system type" should be assimilated either to the "theoretical systems type" or to the "technological systems type", as I am inclined to think. In either case, one is left with three fundamental intellectual emphases that virtually exhaust the logics in use in the field, whether cast in a systems or non-systems format.

In his stimulating new book, **Scientific Knowledge and Its Social Problems**, Jerome R. Ravetz argues with a slightly different but not incompatible emphasis that any given discipline consists of a specific compound of "history", "philosophy", and after an embarrassed hesitation on the part of the discipline, "art". For Ravetz' analysis, history

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meant description, not merely of past times, but of any class of objects, and "natural history" still survives in the title of some long-established museums. "Philosophy," on the other hand, meant reflection and explanation, as applied to any problem. Thus Dalton's atomic theory was announced in his "New System of Chemical Philosophy," and what we now call "physics" was known as "natural philosophy" in England throughout the nineteenth century. Each type of inquiry had its own criteria of adequacy, the one emphasising faithful and comprehensive accounts, and the other coherence of argument.<sup>23</sup>

Although "art" has occasionally been omitted from the repertoire of possible logics, an artistic component has often been crucial in determining the success of a discipline. According to Ravetz,

[t]he traditional term was not restricted to subliterate handicrafts; for Aristotle, it extended to the set of principles defining the methods of any class of tasks. Arts could be "liberal" as well as "mechanical;" and could involve a sophisticated (and genuine) scientific component, as the Renaissance "art of navigation". Now, a literate art would naturally be based partly on the "history" of its objects, and be informed to some extent by a "philosophy" of its principles. On occasion, a rising art would develop a "philosophy," with the function of enhancing its prestige; such was the case with architecture during the Italian Renaissance. But these related inquiries would be ancillary and incidental to its real work, whose strength and success was independent of theirs.<sup>24</sup>

Interpreting Ravetz' insights for the present argument, we would say that the artistic component relates most clearly to our notion of technology, in its broadest sense, in international politics, and has its counterparts in the "crafts" of the field, i.e., diplomacy, decision-making, planning, and problem-solving in general, all of which "muddle through," to use Charles Lindblom's expression, without much sustenance from basic science.<sup>25</sup>

Ravetz' views of "history" and "philosophy" are obviously interlocked in many complex ways and together they produce

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what theory there may be in international politics. The end result, we think, however, approximates Toulmin's concept of "natural history" much more than "theoretical science". Ironically, we are beginning to see definitions of theory and explanation cropping up in recent literature that are extremely difficult to disentangle from notions of historical explanation, a distinction jealously guarded by international politics, even in the face of generalizing historians.<sup>26</sup> M.B. Nicholson, for example, in "Mathematical Models in the Study of International Relations", has written:

Theory is understood to be the selection of various characteristics of a situation coupled with the assertions, whether formally or informally expressed, that these attributes are in some way causally connected. ... The assertion that one situation is the cause of another is a theory in this terminology. This sort of statement is found continuously in historical writing and history would be very dull without them. They are, however, theoretical statements, and for this reason it is asserted that the study of history is theory-laden. It does no good to assert that theories are not there simply because they are not described as such.<sup>27</sup>

Similarly, in a purported attempt to establish a **rapprochement** between theory and policy, Graham T. Allison and Morton H. Halperin assert that international politics, in order to be "policy-relevant" must concern itself with specific explanations (e.g., Why, [d]uring the Tet holiday of 1968, did North Vietnamese troops launch massive attacks on a large number of South Vietnamese cities?")<sup>28</sup>, predictions, and plans. However, Allison and Halperin go on to propose yet another typology of policy and decision-making, in this case called "A Bureaucratic Politics Model".

The "artistic" component of the discipline is generally denied, although many political scientists are still willing to concede that the **practice** of politics is an art. The artistic element in the cultivation of the **discipline** is much less commonly conceded, perhaps, because it is, in a sense, somewhat threatening. What it would seem to imply is that not all of us who have been properly and rigorously trained can expect to have "connoisseurship" or our "intellectual passions" as Polanyi would have it, sufficiently

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delicately attuned.<sup>29</sup> However, there is a more pronounced "artistic" element in the problem-oriented work of a J. Barrington Moore, Jr. for instance.<sup>30</sup>

Shifting the focus somewhat, another set of distinctions Ravetz advances bears significantly on the argument we are making. Ravetz suggests that the true character of science together with its interface with society can best be understood by separating for analysis the special concerns of scientific, technical, and practical problems. Ravetz defines science as "problem solving on artificial or intellectually constructed objects." (Of course, commonsense language is "artificially and intellectually constructed" too. What Ravetz seems to have in mind is a quite substantial component of what Toulmin would call "theoretical" as opposed to "natural" science). Moreover, for Ravetz, scientific problems presuppose

a matrix of technical materials, existing information with the intellectual objects it describes, tools, and a body of methods including criteria of adequacy and value. **For in the absence of such a matrix of technical materials, a genuine problem could never come into existence, and the decisions on investigating it, and on shaping it during the work, would have no foundation.**<sup>31</sup> [Emphasis supplied].

Technical problems, on the other hand, are closely related to science because they presuppose the existence of "mature and effective" scientific fields. In every case, the "goal is defined by the desired performance of a pre-assigned function."<sup>32</sup> Practical problems are, however, to an important extent, on their own. While practitioners and technicians alike share the frustrations arising from being thrust into contact with a reality that is not ordered by intellectually constructed categories and must cope somehow or other with the interstices between theory and practice, the plight of the practitioner is more pronounced. (Parenthetically, it is interesting to note how frequently the world "practitioner" is used as a synonym for "scholar", "writer" or "student of international politics.") Ravetz views practical problems as virtually cut adrift from science. In this sense, practical and technological problems, on the one hand, and those of applied science and technical ones, on the other, largely coincide. Thus, according to Ravetz,

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we may define a practical problem as a statement of a purpose to be achieved, whose means are to be established as the conclusion of an argument, with a plan for its accomplishment. In some respects, such practical problems are the most difficult to solve, as well as including some of the most important tasks facing our society, these coming under the category of "welfare". **It is to these that the "social sciences" are directed, as well as the embryonic sciences of "the environment".....** Ultimate purposes, which are remote and diffuse in science become a part of the criteria for the controlling judgments in technology, and here they determine the goal itself. Outside those limited fields where purposes are capable of being handled in terms of accepted explicit intellectual objects, the framing of a new practical problem is an essentially creative act. Whereas the setting of a problem in science involves the partial and tentative specification of a conclusion about artificial objects in a self-contained universe, and a technical problem involves imagining a device to perform a pre-assigned function, here the specification is of a state of affairs in human society which does not yet exist. Each of the controlling judgments of feasibility, cost, and value involves a multiplicity of factors, few of them reducible to quantitative or routine assessment.<sup>33</sup> ([Emphasis supplied].

In this way, Ravetz adds his concurrence to the view that social science, whatever particular logic it may display, remains in the broadest sense, practical or technological and, therefore, essentially non-scientific.

Returning to our classification of four system types which the author has collapsed into three, she now argues that they represent the three major "meta" logics in use in the discipline. The writer now also argues that in all likelihood it will be impossible to develop theoretical systems in international politics as a whole. This does not necessarily mean that all components of the discipline labor under identical liabilities for international politics is a Comtean-like, but rearranged hierarchy of disciplines. It is, as Quincy Wright understood, a highly complex and synthetic conglomeration of many disciplines. Alternatively, although I am skeptical of how far such attempts may succeed, theory in

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international relations could conceivably be an off-shoot of some other discipline of which it might be considered a special case. For example, Parsons has argued that politics is a subsystem of the larger social system and lacks disciplinary coherence and autonomy of its own. Thus, if one views the international sphere as a "subsystem" of a larger international "social system," the progress of international politics would be closely dependent upon the progress of an "international sociology." John Galtung, too, argues that although international relations is *sui generis*, sociology is its closest analogue and should be an oblique source of hypotheses about international politics. Chadwick Alger and J. David Singer are persuaded that in some respects international relations' explanations will be special cases of psychological ones. Oran R. Young, for his part, has come to believe that theory in international politics will derive from the general progress of strategic thought, which intersects international relations and other disciplines. Accordingly, he pursues economic analogies as a first step in that general direction. Finally, the philosopher, W.G. Runciman has argued in "Sociology in its Place" that sociology (and political science *a fortiori*?) is "parasitic" on history and psychology, having little structure of its own that is not supplied by these other fields.<sup>34</sup>

At the same time the writer realizes that while we can register our skepticism about the development of true theoretical systems in international politics and detailed reasons, it is a conclusion that, in the nature of things, cannot ever be established definitively once and for all. For one is keenly aware that the history of science contains numerous examples for someone in the process of saying something couldn't be done, being stopped in his tracks by someone doing it.

On the other hand, it is irresponsible not to defend our multifarious research efforts in terms of plausibility viewed in the first and last analysis with the "naturalist's eye". It is also irresponsible to fail to scrutinize our respective records of achievement from time to time, after having imported rigid standards of excellence from other disciplines, and then maintaining that it is too soon to judge the result of our work because, after all, there are no "in principle" limitations on what "scientific method" can achieve.<sup>35</sup> It is also indefensible to act as though the heuristic

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“promissory note” need not be made good in some proximate future or to agree not to disagree by refusing to criticise the work of other scholars.<sup>36</sup> On the contrary, the writer thinks, like the analysis of Abraham Kaplan, Stephen Toulmin and others, that since disciplines develop their own “field-dependent” logics, serious, but of course, sympathetic criticism is required all the more.

Although the author takes a dim view of the prospects of theoretical systems, she does not necessarily think a less inflated and dogmatic form of theory impossible. But one’s guess is that it will be theory that is more limited in scope, in time and place. The kind of thinking that underlies historical and technological logics seems a useful antidote and supplement to theoretical efforts. Both tend to contract the synchronic dimension and extend the diachronic, while spreading our interest more generously in the idiographic realm at the expense of some attention to nomothetic possibilities.<sup>37</sup> Moreover, both tend to operate at a “natural history” level of perception and explanation, pre-occupied largely with how we came to do what we did or what we ought to do and how. It is not that the concerns of a social science should not transcend the immediately practical; but that the more immediately practical may provide a useful focal point around which the theory one may find may accrue. These system types also do not adopt the (in our opinion), absurd stance that order in international politics is to be wondered at in the same way that it is in the physical world. Without adopting a philosophical phenomenalism or a self-defeating Winchian view of our subject matter,<sup>38</sup> one can appreciate the enormous role that reasons, plans, expectations, and compulsion play in our political behavior and ought to play in our explanation of it. “Regularities in nature” are often too gross and of very little intrinsic or scientific interest, for that matter, in comparison. As Hugh Stretton puts it in the context of the explanation of individual behavior:

My choices seem neither perfectly predictable nor perfectly inscrutable. They are among the causes of action; they are unlike most causes in nature. Behavior is full of predictable similarities and repetitions. They are reliable enough to support some science and a lot of experience; but not, apparently, to replace much of the experience by a science of, **merely**, regularities ... Most [behaviora-

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lists] ... do admit that some scientific rules need adjustment to social facts. Strict behaviorism has few champions left. Perhaps the quest for uniformities is the next to need adjustment. Rules which define the variations as random are merely obscurantist, since many of the variations are understandable and some (for example, by understanding intentions) are predictable. But the variations are so various that, where regularity cannot work as selector, selection may usefully be guided by political as well as scientific evaluation.<sup>39</sup>

"Directively organized" systems of interest to academics rather than policy consultants are, as we have suggested, predicated on the conviction that classes of means connected to classes of ends can be made the subject of rigorous scientific theory. Very many of our scientific and intrinsic interests in our subject matter are staked on this turning out to be true. This may be true, but so far the record of "directively organized" systems spinning off theoretical ones is singularly unimpressive. Moreover, policy-makers have tended to find "scientific" international politics, which is increasingly cast in this form, more alien to their concerns than the traditional variety.<sup>40</sup> Yet this is a matter of considerable moment. Policy-making has always been the most practical or technological aspect of international politics; it has recently become increasingly considered the "theoretical" focal point in the academic literature, too. However, this is to take a big intellectual gamble, for particular decisional or policy problems would have to become "technical" ones firmly grounded in a basic science. But, thus far, game theory, cybernetics, policy and decision-making models, simulations and other forms of "directively organized" systems have not provided the knowledge that could convert these problems from technological to technical status.

If we are persuaded by Polanyi that there is a necessary trade-off between the scientific quality of knowledge we are able to obtain in the phenomena we study for their own sake (i.e., because they permit themselves to be known) and those we study because we must for our sake;<sup>41</sup> and if we accept that intrinsic interest, although it plays a role in science, is itself something that falls outside science, one does not have to be a "traditionalist" to see that the situation of international relations is

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especially precarious scientifically. This is so, of course, at a time when the practical or technological demands placed upon it have probably never been more critical. All over the world, there is the sense that events are overtaking us and many of the problems facing us, whether their sources are domestic or international, must find international solutions (e.g., population explosion, ecological disaster, the "tide of rising expectations," racial and ethnic tensions, and the "balance of terror".)

To establish the present argument, it isn't necessary to deny the insights provided by "latent functions," nor the existence of compelling subconscious motivation of a Woodrow Wilson, nor clusters of distinctive characteristics, if so far obvious, that surround crisis decision-making. It is enough to be persuaded as strategic thinkers are, that deliberate contriving is as central to international politics as any other phenomena is, to see that the logic of the discipline will continue to be overwhelmingly practical, artificial (in Herbert Simon's sense), or technological probably as far into the future as we can see.<sup>42</sup>

## FOOTNOTES

1. Hugh Stretton, *The Political Sciences : General Principles of Selection in Social Science and History*. London : Routledge and Kegan Paul, 1969. *passim*.
2. Johan Galtung, "The Small Group and the Theory of International Relations," *New Approaches to International Relations*, edited by Morton A. Kaplan, p. 270.
3. Raymond Aron, "What Is a Theory of International Relations ?" *Journal of International Relations*, XXI (Summer, 1967), p. 204. At the beginning of this paper Aron explicitly contrasts two senses of "theory" :
  - (1) contemplative knowledge, drawn from ideas or from the basic order of the world, can be the equivalent of philosophy. (Aron distinguishes this form of theory "from action and from knowledge animated by the will to predict and act." The practical and the theoretical are diametrically opposed.
  - (2) a hypothetical, deductive system consisting of a group of hypotheses whose terms are strictly defined and whose relationships between terms (or variables) are most often given a mathematical form (p. 187).

It is quite apparent that Aron identifies his view of theory with the first definition.

4. *Ibid.*, p. 195.
5. Oran R. Young, "Aron and the Wha'le : A Jonah in Theory," *Contending Approaches to International Politics*, pp. 129-143.

6. The standard philosophy of science view on the matter of prediction and explanation is that the prerequisites that enable one to predict also enable one to explain, and *vice versa*. However, we are more sympathetic to Helmer and Rescher's argument spelled out in *On the Epistemology of the Inexact Sciences*. In that monograph they argue that, as a matter of fact, in the inexact sciences prediction and explanation are asymmetrical, *i.e.*, that the power to explain (which belongs to basic sciences) does not always or even usually lead to the power to predict (which, of course, is essential to technology). Their recommendation is that the social sciences devote much more attention to the problems of systematic prediction (which we have generally treated as a part of our technological systems type). If Oran R. Young is typical of the thinking in the behavioral literature on this question, a very subtle and significant shift in emphasis and interpretation is occurring that ought not to go unnoticed. In his most recent "The Perils of Odysseus," he seems to be hewing to a rather classical view of theory (which Bobrow's contribution to this special edition of *World Politics* seems to share) except, in Young's case with one extremely significant difference : he repeatedly speaks of theory and prediction together, and makes virtually no reference to explanation. We think this is an oddly but revealingly inconsistent stance to adopt. Theory has generally been equated with basic science and explanation, and with prediction only when supported by a highly developed scientific theoretical structure. Now it is possible to say (as post-behavioralists tend to) that it is the predictive cap-

ability of theories that is most important, and that theories ought to be ranked and cultivated on the basis of this criterion. But one should also be aware that this position is not easily reconcilable with all the other features of scientific theory which, if one must choose between prediction and explanation, must throw in its lot with the latter.

Cf. Raymond Tanter, "Explanation, Prediction and Forecasting in International Politics," in Rosenau, Davis, East, *op. cit.*, pp. 41-53. Tanter's analysis, too, somewhat distorts the standard philosophy of science treatment of prediction. Coming to the realization that prediction, in contrast to theoretical explanation, has been somewhat neglected heretofore, he exaggerates the importance that Hempel and Oppenheim assign to prediction. Hempel and Oppenheim feel free to emphasize the importance of prediction only because in the hypothetico-deductive model of explanation that they depict, prediction and explanation almost invariably advance together. Predictions which are not substantially underwritten by scientific explanations (more properly called forecasts) have only very problematic status.

See Mario Bunge, "Technology as Applied Science."

At the same time, to say that social science is in a better position to explain than predict, as we think Aron would say and as Rosecrance and Mueller seem to be saying in the quotation immediately below, is also to say something significant about its scientific status, but also, more profoundly, about its epistemological nature. For if fairly specific social science prediction were, in principle, impossible on the basis of social science explanations, this conclusion could have repercussions for other aspects of social science epistemology. For a technological systems practitioner's view of prediction (as opposed to forecasting) see Olaf Helmer's, "Political Analysis of the Future," 3.

7. R. N. Rosecrance and J. E. Mueller, "Decision-Making and the Quantitative Analysis of International Relations," *The Yearbook of World Affairs*, 21 (1967), pp. 1-19, p. 3.

Cf. the rather analogous situation in psychoanalysis which Freud describes : So long as we trace the development from its final outcome backwards, the chain of events appears continuous, and we feel we have gained an insight which is completely satisfactory or even exhaustive. But if we proceed to reverse the way, if we start from the premises inferred from the analysis and try to follow these up to the final results, then we no longer get the impression of an inevitable sequence of events which could not have been otherwise determined. We notice at once that there might have been another results, and that we might have been just as well to understand and explain the latter. The synthesis is thus not so satisfactory as the analysis ; in other words, from a knowledge of the premises we could not have foretold the nature of the result.

It is very easy to account for this disturbing state of affairs. Even supposing that we have a complete knowledge of the aetiological factors that decide a given result, nevertheless what we know about them is only their quality, and not their relative strength. Some of them are suppressed by others because they are too weak, and they therefore do not affect the final result. But we never know beforehand which of the determining factors will prove

the weaker or the stronger. We only say at the end that those which succeeded must have been the stronger. Hence the chain of causation can always be recognized with certainty if we follow the line of analysis, whereas to predict it along the line of synthesis is impossible.

S. Freud, *Psychogenesis of a Case of Female Homosexuality* (1920). *Standard Edition*, Vol. XVIII, p. 167 f. Hogarth Press, 1955. Cited by Robert Waelder, *Progress and Revolution: A Study of the Issues of Our Age*. N.Y.: International Universities Press, Inc., 1967, p. 126.

8. Morton A. Kaplan, *Macropolitics*, p. 8.

9. E.g., Hedley Bull, "Order vs. Justice in International Society," *Political Studies*, XIX (September 1971), pp. 269-283; J. Siotis, "Social Science and the Study of International Relations: A Perspective from Geneva," *The Yearbook of World Affairs*, 24 (1970), pp. 1-24, p. 2.

10. George A. Kelly and Linda B. Miller, "Internal War and International Systems: Perspectives on Method," *Struggles in the State: Sources and Patterns of World Revolution*, edited by George A. Kelly and Clifford W. Brown, Jr (New York, 1970), pp. 226-260.

11. *Ibid.*, p. 230. The affinity with technological systems is made even plainer in the "Editors' Comment" preceding the article.

It is important to grasp the range and limitations of this approach in political analysis. No claim is made to what the modern philosopher of science calls *prediction and explanation*. Through a generally inductive pattern of historical examination one seeks to establish the most durable concepts and "rules" by which the observer may reasonably expect to order and understand the chaos of events that we nevertheless feel free to call a "civilization" or a "system." This is more an exercise of clarification and ratiocination than of prophecy, and it necessarily implies an intellectual tension between political typologies and the ultimately inscrutable movement of history. *To use Kant's language, the generalizations that emerge from "historical sociology" are used only regulatively; they are not "laws" that could be constitutive of the political universe.*

Hence, it follows that politics still depends on a philosophy that is established by methods beyond political research and analysis. (p. 224).

12. See quote in footnote 11, above.

13. Oran R. Young, "The Perils of Odysseus: On Constructing Theories of International Relations," *World Politics*, XXIV (Spring 1972 Supplement), p. 179.

14. R. N. Rosecrance and J. E. Mueller, "Decision-Making and the Quantitative Analysis of International Relations," pp. 5-6 and *passim*; Robert Jervis, "The Costs of the Quantitative Study of International Relations," Knorr and Rosenau, *op. cit.*, pp. 177-217; Michael Nicholson, "Measured Approaches to Peace: Thinking by Numbers (##9)," *Times Literary Supplement* (November 5, 1971), 1397-8.

15. James N. Rosenau, ed., *Linkage Politics : Essays on the Convergence of National and International Systems* (New York, 1969), " Introduction : Political Science in a Shrinking World." p. 15.

Yet it cannot be said that our research strategy was entirely successful. It is a measure of the conceptual challenge posed by linkage phenomena that these essays, taken together, do not achieve the degree of comparability to which we aspired at the outset. Despite the shared commitment to examine certain basic linkages in the same analytic framework, the essays are marked as much by variability as by similarity. The original linkage framework presented in Chapter 3 does infuse the chapters that follow with common dimensions, but comparisons across these nine empirical chapters are difficult to make. They vary in the extent to which they adhere to the original assignment and consider the links between leadership attributes and international system variables.

16. Talcott Parsons, *The Structure of Social Action* (New York : 1937).

17. Karl W. Deutsch, " On Theories, Taxonomies and Models as Communication Codes for Organizing Information," *Behavioral Science*, 11 (January, 1966), pp. 1-17. Karl W. Deutsch, J. David Singer, and Keith Smith, " The Organizing Efficiency of Theories : The N/V Ratio as a Crude Rank Order Measure," *American Behavioral Scientist*, IX (1965), pp. 30-33.

Cf. Peter Nettl, " The Concept of System in Political Science," *Political Studies*, 14 (October 1966), esp. 323-338.

18. See, for example, Mario Bunge, " The Weight of Simplicity in the Construction and Assaying of Scientific Theories," *Philosophy of Science*, 28 (1961), pp. 120-149.

19. Abraham Kaplan, *The Conduct of Inquiry : Methodology for Behavioral Science* (San Francisco : Chandler Publishing Co., 1964), pp. 3-11.

20. Don K. Price, *The Scientific Estate*, pp. 111-12.

The scientist at work in his laboratory has not, during the twentieth century, been pursuing the ideal of a single scientific method that is gradually giving a single comprehensive view of reality. He has been developing the particular method of a particular discipline, the one that works well and produces results with respect to a special aspect of reality. And when he undertakes to join with his fellow scientists to advance his discipline, he finds it most useful to associate not with a continuously broadening group, but a narrower and more specialized one . . . .

Even when new scientific concepts and techniques began to integrate the methods of certain sciences, the result was not to unite their organizations but to split them further. As the techniques of chemistry seem to be more useful in biology, the result was not to unify the associations of chemists and biologists, but to add a new American Society of Biological Chemists. The scholarly and professional motives of the scientist led toward ever-increasing specialization ; the 1960 cata-

logue of scientific and technical societies in the United States and Canada listed 1,836 organizations. Scientists have indeed managed to maintain comprehensive organizations. But they have done so only when they have been interested in some purpose beyond science itself. For example, they established the Federation of American Scientists, and in recent years built up the American Association for the Advancement of Science, out of an interest not only in science but also in its social and political implications.

21. Stephen Toulmin, *The Uses of Argument*, p. 257.

22. *Ibid.*, p. 258.

23. Jerome R. Ravetz, *Scientific Knowledge and its Social Problems*, p. 372.

24. *Ibid.*, p. 373.

25. Charles Lindblom, "The Science of 'Muddling Through,'" *Public Administration Review*, XIX (Spring 1959), pp. 79-88.

26. See, for example, Louis Gottschalk, ed., *Generalization in the Writing of History*. A Report of the Committee on Historical Analysis of the Social Science Research Council. (Chicago: University of Chicago Press, 1963); *Generalizations in Historical Writing*, edited and with an introduction by Alexander V. Riasanovsky and Barnes Riznik. (Philadelphia: University of Pennsylvania Press, 1963); Frederick J. Teggart, *Theory and Processes of History*. (Berkeley: University of California Press, 1941).

27. M. B. Nicholson, "Mathematical Models in the Study of International Relations," *The Yearbook of World Affairs*, 22 (1968), pp. 47-68.

28. Graham T. Allison and Morton H. Halperin, "Bureaucratic Politics: A Paradigm and Some Policy Implications," *World Politics*, XXIV (Supplement to Spring, 1972), pp. 40-79, p. 40.

Cf. Morton A. Kaplan, *Macropolitics*, Part III, "On Historical Explanations," and A. L. Burns, "Ascertainment, Probability, and Evidence in History," *Historical Studies: Australia and New Zealand*, IV (May 1951), in which Burns discusses historical explanation at length and proposes "consilience as a criterion for assessing historical explanations." ("The strengthening effect of one historical hypothesis upon another when it is noticed that both hypotheses would be explicable by means of a third historical hypothesis.") (p. 337) But compare Morton A. Kaplan's differences with Michael Scriven's arguments in "Truisms as the Grounds for Historical Explanation," *Theories of History*, edited by Patrick Gardiner (Glencoe: The Free Press, 1959), taken up in *On Historical and Political Knowing*.

29. See Morton A. Kaplan, *On Historical and Political Knowing*, for a discussion about the entirely non-mechanical problem of "identification," pp. 10-12.

30. J. Barrington Moore, Jr., "The New Scholasticism and the Study of Politics," *World Politics*, VI (October 1953), pp. 122-138.

31. Jerome R. Ravetz, *op. cit.*, p. 136.

32. *Ibid.*, p. 5.

33. *Ibid.*, pp. 339-340.

34. Talcott Parsons, *The Social System* (Glencoe, Ill., 1951), p. 126 ; Chadwick F. Alger, "Trends in International Relations Research," *A Design for International Relations Research : Scope, Theory, Methods, and Relevance*, edited by Norman D. Palmer ; J. David Singer, "Knowledge, Practice and the Social Sciences," *ibid.* ; Oran R. Young, "The Perils of Odysseus," p. 195, p. 187 footnote 36 ; W. G. Runciman, "Sociology in its Place," *Sociology in its Place and Other Essays* (Cambridge, 1970), p. 2 and *passim*.

35. Heinz Eulau, "Segments of Political Science Most Susceptible to Behavioristic Treatment," *The Limits of Behavioralism in Political Science*, edited by James C. Charlesworth (Philadelphia, 1962).

36. Michael Haas, "International Relations Theory," p. 449.

A secondary purpose of the overview will be to point out some of the shortcomings that beg for analytical work to render each methatheory more useful. In this second task the essay may appear to break with a tacit gentleman's agreement among international relations theorists not to quibble too much with one another's problems in the pioneering stages of conceptualization, theory construction, and operationalization. Yet after more than a decade of such pioneering, with a danger of theory diverging completely from research in prospect, it seems urgent to attempt such a constructive critique.

37. Johan Galtung, "The Social Sciences : An Essay on Polarization and Integration," Knorr and Rosenau (eds.), *op. cit.*

38. See, for example, Alfred Schutz, "Commonsense and Scientific Interpretation of Human Action," and "Concept and Theory Formation in the Social Sciences," both contained in *Philosophy of the Social Sciences : A Reader*, edited by Maurice Natanson (N.Y., 1963) ; Peter Winch, *The Idea of a Social Science* (New York, 1958).

39. Hugh Stretton, *op. cit.*, pp. 164-165.

40. See generally sections devoted to "Relevance in *A Design for International Relations Research : Scope, Theory, Methods, and Relevance*, especially, Joseph E. Johnson, "Policy Implications and Applications of International Relations Research for Foreign Policy and Diplomacy," pp. 229-239.

41. Don K. Price, *op. cit.*, p. 129.

42. Cf. George Modelski, *Principles of World Politics*, p. 11.

For any given society, politics is that set of activities that is concerned with the achievement (or "production") of order and justice. World politics is the set of activities that is concerned with the achievement of order and justice for world society.

On the basis of all the foregoing arguments, it is very difficult to maintain a tidy and stable distinction between a social scientist and a member of a profession. As Don K. Price puts it,

[t]he professions (for example, engineering and medicine) make tremendous use of the findings of the sciences, but they add something more : a purpose. Science has advanced by getting rid of purpose.

except the abstract purpose of advancing truth and knowledge. But the professions put it back in again ; basic science could not cure a patient or build a bridge or an airplane, but the medical and engineering professions are organized to do so. *Op. cit.*, p. 133.

In his " The Perils of Odysseus " Oran Young maintains that theory-building will continue to be the single most important intellectual activity in the discipline, but that he now concedes that other kinds of intellectual exercises may legitimately occupy scholars, *i.e.*, " sensitization, conceptualization, factual assessment, simple generalizations, correction, and extrapolation." (pp. 187-190).

## تأملات في كتابات أصحاب نظرية النسق الخاصة بدراسة السياسة الدولية

كارول بريجه

كانت صورة « النسق الدولي » هي المسيطرة في المحاولات المتعددة لوصف الظاهرة الكلية الخاصة بالسياسة الدولية . وقد عزى الفضل في رسم معالم تلك الصورة الى العلماء السلوكيين .

وتحاول هذه الدراسة التدقيق في الأغراض السلوكية التي خدمها مفهوم « النسق » أما المقاييس المعتمدة في هذا التحليل فهي ، على الأقل في المراحل الأولى من الدراسة ، مقاييس سلوكية . غير أنه مع الاستطراد في التحليل ، يضع البحث يده على أربعة أنماط من « النسق » . وتنصب المعالجة ، في مجملها ، على تفحص جدوى كل واحد من تلك الأنماط في دراسة السياسة الدولية ، وعلى قيمته وفائدته في المستقبل .