4 Scholarship and Relevance: Is There a Tradeoff?

Even if scholarship can help guide the conduct of international affairs, it does not necessarily follow that it should be used for that purpose. From a scholarly perspective the costs may be too great. Prima facie, it surely is better to be useful than not to, *unless*, perhaps, the costs in terms of international relations scholarship are too great. Academics have a responsibility to their own calling as well as to national policy goals—if the claims of the two should collide, it is not obvious that the latter's should prevail. The issue, then, is whether the production of knowledge with concrete bearing on practical problems may undermine the intellectual foundations on which that knowledge rests. Two broad and occasionally intersecting categories of concerns have been expressed in this regard: the first involves the proper relation of scholar to the society of which he or she is a part; the second concerns the consequences of a quest for policy relevance on the quality of scholarship—in particular, development of good theory. We will assess the two concerns in turn.

Relevance, Scholars and Society

The particular nature of the social scientist's position flows from the requirement of objectivity, which assumes that the scholar must examine his or her society from the position of a detached observer. Such a stance requires aloofness from societal values and interests that might interfere with an objective approach to analysis and data-gathering.[1] In turn, this requirement is said to (a) constrain the scholar's professional ability to engage in controversies about the fundamental values that should drive policy, and, (b) require a commitment to preserving the epistemological integrity of his work from standards of evaluation external to the academic community. It is sometimes feared that the constraints may be violated and the commitment abandoned where knowledge seeks to guide the conduct of policy.

Ends, means, and the Problem of Value-Neutrality

The worry is that policy-relevant work may violate an ideal of education and scholarship implied by the liberal, value-neutral, conception of the democratic state. As philosopher Michael Root has argued, the preference for a value-neutral state, committed to procedures of democratic decisionmaking but not to the promotion of one view of the public good over another, is a core component of the liberal creed.[2] Often, within this creed, commitment to value-neutrality is extended to the pursuit of knowledge. Thus, it is felt that scholarship should encourage epistemologically proper methods of inquiry, without, at the same time, seeking to promote a particular set of social values via this inquiry.[3] The latter are grounded in moral feelings and corporate interests, with no scientific truth-value, whereas only a concern with statements possessing truth-value is the business of the social sciences. "The liberal state is forbidden to use the law; the liberal schools are forbidden to use the classroom or curriculum; and the liberal social sciences are forbidden to use teaching or research to endorse one conception of the good over another.[4]"

An implication sometimes drawn from the liberal ethos is that, while political science may concern itself with the analysis of means, any discussion of ends lies outside its province. In George Herbert Mead's words, "Science does not attempt to formulate the end which social and moral conduct should pursue."[5] Decisions about ends flow from democratic procedures of aggregating societal preferences, not from the arguments of scholars. The business of the academic enterprise involves Questions of Path, not Questions of Covenant.[6]

If Mead's dictum were accepted, there might be reason to worry that policy-relevance could jeopardize scholarly independence. Because science should be concerned only with means, those who advise on the pursuit of ends would not be value-neutral. By proposing explicit courses of action, they cannot avoid urging certain ends over others and thus certain values over others. Such partisanship might then undermine their intellectual independence. The solution is sometimes thought to lie in an explicitly restricted definition of the analyst's role, in injunctions to the effect that such a person must act as an agent, not as a principal. In this vein, Alexander George and Richard Smoke have maintained that:

> Policy science, as we would define it, is itself value free, although in a different sense from the value-freedom of empirical theory. The policy theorist, acting as such, accepts the values of the constitutionally authorized decision-makers of his nation and offers contingent advice: "if you want to accomplish x, do y in your policy."[7]

This solution to the ends-means problem may seem simple and workable; actually it is elusive. It assumes that a meaningful distinction can be drawn between the ends and means of political action—that science can help society select the latter without affecting its positions toward the former. This can rarely be done. The distinction between ends and means is tenable only with regard to "pure" ends: those that can be defined as objectives in and of themselves, not as a means for attaining any other, more general or more elevated, goal. Although a set of "pure" ends surely exists, it remains that: (a) this set has very few members, and, (b) these are axiomatically desirable. Objectives of this sort may include "justice," "welfare," "felicity," i.e., goals that virtually everyone would embrace and whose meaning is definable only in the most abstract terms. Similarly, the ultimate goals of U.S. foreign policy—involving peace, prosperity, and the promotion of democracy—are never seriously questioned.[8] Lacking a firm empirical content, they are of very limited analytical use. By the same token, social scientists have no incentive to either urge or discourage their pursuit, and, in the abstract sense given to such objectives, no one would argue that we need to understand how they should be attained.

But once one moves even slightly away from what amounts to trivial statements of "pure" ends, almost every goal is, at some remove, subordinate to them by an assumed instrumental relationship, either direct or indirect. In other words, virtually every nontrivial societal goal is ultimately an instrumental goal, so that virtually all political arguments and policy debates, even within a relatively broad common frame of political values, really involve means. Democrats and Republicans, liberals and conservatives, hardly ever argue about pure, or "ultimate," ends; instead they typically argue only about the proper methods of proceeding toward them. For example, no one disputes the need to reduce poverty, but the desirability of doing so via government or market forces may be sharply debated. There is rarely disagreement on the need to improve American schools, but not everyone concurs on the sort of knowledge that should be conveyed to the nation's youth, or whether this best done via private or public schools, or perhaps by a system of school vouchers. Everyone agrees that peace is desirable, but not everyone agrees on the proper mix of force and diplomacy that its pursuit requires.

In this sense, it is emphatically not the case that ends are value-laden and sharply debated while means are value-free and uncontroversial. As a rule, it is precisely the other way around: virtually everyone seems to subscribe to the same restricted set of noninstrumental aims. There are, however, many possible ways by which one might try to achieve these ends, and much value judgment and consequent debate surrounds these means. Because quarrels almost always involve ways of attaining ends, the injunction that policy-relevant work must be value-free is quite inconsistent with a recommendation that it concern itself with means alone. It is also vacuous, because no one wishes to debate ultimate ends, but short of these the distinction between ends and instruments is, most of the time, meaningless.

To illustrate the argument, note that George and Smoke find it necessary to qualify their notion of value-free policy research:

> When necessary, the policy analyst should indeed urge that the objectives of current and contemplated policy be redefined to make them more consistent with what he perceives to be the final goals of the policymaker. However, he does not assert his own "final" goals or values (except perhaps negatively by declining to assist in implementing certain policies).[9]

Quite apart from the admission that scholars may, after all, seek to influence "final" goals negatively (logically no different a matter from trying to do so "positively"), the authors recognize that policy scientists can engage in goal manipulation—it all depends on the level of the goal.

All of this might lead one to suggest that academics should engage in *no* policy-relevant work, but this is not tenable: barring scholars from a discussion of both ends and means places restrictions on the academic enterprise that are disturbingly broad, and that bear no correspondence with actual academic practice—past, present or, in all likelihood, future. If social scientists could not comment on links between ends and means, most of their work would be vacuous. The sensible approach is to place no constraints whatsoever on what the social sciences can examine. This does not eliminate the possibility that work with a policy-orientation may fall short of ideals of scientific inquiry. Consciously or not, scholars in this area may be found to interpret facts in light of values, or to twist analyses to encourage acceptance of particular objectives. How much of a problem this in fact is, and whether such problems are likely to be especially common in policy-relevant work or are shared by scholarship of a less applied sort, is further discussed elsewhere in this volume.

The Issue of Peer Evaluation

Another argument against policy-relevant work is that it may vest power to evaluate scholarship in hands other than those of academic peers, threatening the intellectual integrity of the social sciences. The natural sciences have staunchly resisted non-peer evaluation, a steadfastness deemed crucial to their record of achievement. If this principle were relaxed even slightly, some people fear, scholars might respond to the concern of either the lay public, or policymakers, or both, implicitly agreeing to allow those segments of society to judge the quality of their contributions. Were this to happen, the growth of innovative and empirically verifiable knowledge would likely be impeded.[10] Still, the issue is not clear-cut: it all depends on which aspect of the scientific product is externally evaluated.

Certainly, it is unacceptable to place the authority to evaluate scholarship's *epistemological* merit in any hands other than those of scientific peers, since such merit can be judged only according to the canons of scientific inquiry, and only by those who have accepted and mastered these canons. But this does not exhaust the issue, because the purpose of scientific inquiry cannot merely be to demonstrate epistemological virtue. Its ultimate mission is to answer meaningful questions about the natural or social world, and any judgment on the value of a scholarly product must, in addition to epistemological considerations, be concerned with the significance of the questions it addresses (as this chapter's second section will further argue). This significance may be grounded in the applications to which the new knowledge can be put, but it may also have nothing to do with practical considerations. According to sociologist Scott Greer, three sorts of problems, or questions, typically engage the attention of social scientists,[11] and the appropriate judges of the importance of the issues addressed may depend on the category of question involved.

A first category involves "policy" issues, i.e., social problems to which some practical urgency attaches. Practical in this context means that a problem is, in principle, amenable to solution. Urgency suggests that some segments of society want it to be resolved, sooner rather than later. The second category of problems are those of "general social philosophy," originating from a need to conceive of social existence in terms of a meaningful system of institutions and relations, and to harmonize that belief with actual experience. Here, scholarly problems usually stem from a clash between accepted world views and apparent evidence. The purpose of the inquiry, then, is to resolve the discrepancy, either by integrating the new evidence or new ideas within an existing frame of reference or by creating one that is more satisfactory. Problems of the third type are those "intrinsic to developing scientific disciplines." These concern the internal consistency of scientific theories, as well as their match with observable evidence; the problems to be resolved under this heading come from challenges to the validity of existing theories or to their empirical accuracy. These three categories are not mutually exclusive, and any one of them can lend significance to scholarly inquiry; but the appropriate judges of the importance of the questions addressed by scholarship may vary according to the category of question involved.

The third class of issues—problems intrinsic to developing scientific disciplines—are rarely recognized outside the scholarly community. Even if they were more broadly recognized, neither their scientific importance nor their methodological integrity can easily be judged by those without the theoretical background and training needed to interpret the implications of relevant evidence. Consequently, the importance of this sort of scholarship typically must be judged internally, by the professional peers of those conducting the inquiries. But the same conclusion does not apply to the other two classes of problems.

To begin with, scholars cannot be considered the *only* proper judges of the importance of issues of general social philosophy they choose to address. Gaps between social and political world-views and actual practice may be recognized at various levels of society and by those directly concerned; such awareness can be as meaningfully rooted in the daily experience of ordinary citizens as in the academically sanctioned writings of professional social scientists. Thus, for example, the importance of economic globalization may legitimately be evaluated by those who experience its effect. Similarly, many though not all segments of the lay public can evaluate intellectual efforts at bridging gaps between ideals and reality. This is not to say that anyone could provide a satisfactory solution to such gaps, but that many people other than the peer reference groups of professional scholars can form a reasonable opinion of the value of scholarly efforts in this area. Consequently, excluding non-peers from judging the substantial value of social scientific attempts to deal with problems of general social philosophy is hard to justify, even if those outside the scholarly community cannot say much about the epistemological merits of scientific work.

This conclusion also applies to the first of Greer's three categories of problems addressed by social scientists—those relevant to *policy*. It is hard to believe that scholars are necessarily the best judges of the practical urgency of policy issues. It seems that those who stand to be affected are in as good a position to estimate the urgency of problems addressed. For this reason, segments of the lay public may legitimately judge the practical value of the scholarly effort, assuming it is epistemologically sound. It could further be argued that those who are charged with implementing a policy—i.e., the society's policymakers—are apt to have a pretty good idea of the feasibility and implications of possible solutions: at least as good an idea as many professional academics. Engineering, rather than basic science, may be the more appropriate model here. Under the circumstances, it is reasonable to open evaluation of the value of policy-related inquiries by social scientists to members of non-peer groups.

Because many objections to non-peer evaluation invoke the example of the natural sciences, it is necessary to observe that, in fact, the natural sciences do not provide a satisfactory parallel with regard to the role of non-peer groups. The closest analogy to policy-related questions within the natural sciences are those involving the technological implementation of knowledge produced by basic science. In most cases, only engineers and technicians are in a position to estimate the feasibility of developments that lead from scientific principles to practical applications.

Analogies between the social and natural sciences break down completely where other categories of issues are concerned. If it is possible within the social sciences to speak separately of "general problems of social philosophy" and of the substantive problems "intrinsic to developing scientific disciplines," this is because there is assumed to be a class of theoretical questions validly addressed by both scholars and non-scholars, and another falling within the former's exclusive purview. While this may be an appropriate view, it is hard to find a parallel in the natural sciences, where virtually all theoretical matters, because of the highly specialized conceptual foundation and intellectual tools involved, are within the domain of none but those who have mastered them professionally. Consequently, it is understandable that the scope for non-peer evaluation should be much more restricted in the natural than in the social sciences, a conclusion that applies to the *importance*[12] as well as the soundness of scholarship.

We conclude that a concern for policy relevance will neither impair SIR scholars' links to their society nor damage the integrity of the standards by which their work is judged. A lingering worry, however, is that their ability to produce good scholarship might be damaged by more basic incompatibilities between knowledge designed for practical application and that pursued as an end in itself.[13] Accordingly, the next section asks whether the quality of international relations scholarship, especially its theoretical foundation, is likely to suffer from efforts to make it useful.

The Consequences for Theoretical Development

Like the previous section, this one has two parts: the first describes what we estimate to be the principal attributes of "good" theory; the second inquires whether these qualities might be impaired by an emphasis on policy relevant knowledge.

Theory is a set of general propositions about the same subject, connected by relations of conjunction and implication, that, by embedding knowledge in a meaningful structure, allows relevant properties of that subject to be explained and predicted.[14] There are many conceptions of the items that should be placed on a scorecard of theoretical worth, but no consensus defines a common set, particularly not in the social sciences.[15] Nevertheless, we will not stray too far from most perspectives by suggesting that the desirability of theory can be judged at two levels: their *soundness* and their *value*.

Sound Theories

The main functions of theory are to explain and predict, and a sound theory is one that competently discharges both functions. There is little controversy about the meaning of prediction: it is the business of anticipating a future condition, by establishing a link between it and an antecedent condition.[16] The antecedent condition may be as simple as a prior value of the property that is being predicted, or it may involve a complex pattern of multivariate causation; but the meaning of prediction is relatively uncontroversial.[17]

The relation between explanation and prediction is more complex, as is their respective place in empirical theory. The logical structure may be quite similar in the two cases, but it need not be, since prediction is possible without prior explanation. For example, the ability of ancient astronomers to anticipate the movement of celestial bodies far outstripped their ability to explain why these movements occurred as they did. With regard to everyday experience, most people can predict that speaking on one end of an open telephone line will result in their words being reproduced at the other end, even if they lack a grasp of the processes involved. Of the two, therefore, explanation is the more demanding task and, by extension, the more ambitious scientific achievement. It is also apparent how a prediction differs from an explanation. Aside from the fact that prediction (unlike explanation) involves some reference to the time of the assertions contained in the premises, explanation rests on at least one theoretical generalization linking an antecedent and consequent condition, while prediction requires, in principle, no more than the observation of some empirical regularity. (For example, one could predict that Britain and France will not fight each other because, since the Napoleonic wars, they have never done so. Similarly, my voice will be reproduced at the other end of the telephone line because this has always been the case, for me and everybody I know).

While an ability to predict says little about a capacity to explain, the obverse rarely applies: in the vast majority of cases, we are well placed to predict that which we are in a position to explain. To take an example discussed in chapter 5, if one can explain why war is very uncommon, if not unheard of, among mature democracies, one would be in a good position to predict the consequences for war as political liberalization proceeds. The bottom line is that, while even prediction alone may be very useful, a theory which allows no more than this is a comparatively modest accomplishment (no matter how sophisticated the analytical tools marshaled for the purpose), while the production of generalizations capable of explaining is a more ambitious and valuable attainment.[18] Because of this, and while predictive power alone may characterize an adequate theory, the measure of a superior theory is its ability to *explain* classes of phenomena that we have some reason to care about. (Accordingly, we are adopting a conception of theoretical purpose that is closer to the "realist"[19] than to the "instrumentalist" position in the philosophy of science.[20])

Two key attributes stand behind a theory's explanatory ability: (a) the truth of its premises (are they empirically correct?), (b) its completeness (in the sense that no propositions crucial to the task of explanation are missing).

True Generalizations According to the dominant "correspondence theory"[21] of truth, truth is an objective property of statements (e.g., C, I, or G), determined by the correspondence between that statement and observable data. In other words, a statement is true to the extent that it corresponds with reality.

To some extent, the implications of a requirement that the premises of theoretical arguments should be true depend on whether the logical structure of the argument behind the theory is deductive or inductive. Although an inductive argument is sometimes thought of as one that moves from the specific to the general, while a deductive argument starts from general premises, this is not a strictly accurate basis for distinguishing between the two (except, for example, in the case of induction by enumeration). From a strict epistemological point of view, the distinction is this: a deductive argument is one whose premises fully support its conclusion, while the premises of an inductive argument support the conclusion, but less than fully.[22] A correct mathematical derivation, for example, embodies a deductive argument; most reasoning by analogy, as well as most statistical arguments, represent inductive arguments. While the basis for the conclusion of a deductive argument is always logical, that of an inductive argument is always empirical. In the former case, conclusions follow from necessity; in the latter case they rest on probability. Obviously we need both. Inductive arguments add to our store of knowledge new empirical truths. Deductive arguments allow us to maintain consistency among our propositions. Still, they involve truth in different ways.

If the premises of a deductive argument are true, then, as long as the argument is logically valid (e.g., a valid syllogism), the conclusion is implied by logical necessity. In other words, in a valid deductive argument, true premises necessarily imply a true conclusion. But the converse does not apply, for a deductive argument containing one or more false premises may, nevertheless, produce a true conclusion. For example: all chairs have two legs, George W. Bush is a chair, therefore George W. Bush has two legs. From a deductive point of view, this argument is perfectly valid, and the conclusion is undeniably true; nevertheless, both of its premises are false. The problem is that nothing in the process of deduction *itself* can tell us whether a true conclusion was produced by false premises—a situation that is obviously perilous to explanatory endeavors. More obviously, false premises may produce a false (though perfectly valid) conclusion: if validity were to be confused with truth, the consequences for knowledge-creation would be obvious.

Closely related to this point, in discussing the respective merits of prediction and explanation, we should consider the argument occasionally made by social scientists of a deductive bent, one made in Milton Friedman's much-quoted article on "The Methodology of Positive Economics."[23] In this piece, Friedman claims that the value of a theory depends on how useful it is at predicting certain outcomes, whether or not the assumptions behind the successful predictions are correct. Since, as we have seen, it is logically entirely possible for false assumptions to yield accurate predictions, the statement is not completely indefensible. Nevertheless, two caveats are necessary. The first is that it is much more likely that an accurate prediction would be produced by a valid argument based on true premises than on false premises, since the prediction's accuracy (its truth value) could be merely coincidental in the latter case. Because of this, attentiveness to the truth of one's assumptions is likely to enhance the quality of one's predictions. The second observation is that, even though false premises may coincidentally yield correct predictions, they certainly provide no basis for *explaining* the outcomes they seek to predict; in fact, they may lead us *away* from correct explanation. Accordingly, arguments yielding correct predictions from erroneous premises must be judged a less ambitious achievement than arguments that, proceeding from true premises, provide both an explanation *and* a prediction of some relevant outcome.

The premises of an inductive argument typically involve statements that are, explicitly or implicitly, conditional or probabilistic. Accordingly, and unlike the deductive case, true premises need not produce true conclusions in an inductive argument—all that can be said here is that true premises are more likely to yield true conclusions, and vice-versa. (Although rigorous statistical tests may, when appropriate, give us a reasonably good idea of the probability that our conclusions are in fact true.) Thus, while we cannot be certain that true premises lead to true conclusions in the inductive case, false premises less often lead us to true conclusions here than in the deductive case. Our ability to both explain and predict, on the basis of generalizations inductively produced, becomes a matter of degree; and this ability is enhanced to the extent that our premises are true.

Clearly, then, whether primarily deductive or inductive, the explanatory value of a theory benefits from true premises: in the deductive case, it ensures a true conclusion; in the inductive case, it makes it much more likely. Under the circumstances, the empirical correctness of the premises of a theoretical argument is an important condition of the theory's ability to do an adequate explanatory job.

Theoretical Completeness A complete theory is one that omits no general propositions needed to explain the phenomenon it addresses. This sounds straightfoward, but the pursuit of completeness requires careful judgment. In international relations, as elsewhere in the social sciences, consequences rarely possess a single cause or lack secondary effects; and in a world of intricate causal patterns and multiple layers of implication, boundaries tracing perfectly complete theories can rarely be drawn. Moreover, as parsimony is equally a measure of good theory, the typical strategy is to stay well within these hypothetical boundaries.

These caveats notwithstanding, it is desirable that theories should have no debilitating gaps: full explanations are obviously preferable to partial explanations, and the predictions they yield are correspondingly intellectually satisfying as well as more accurate. Explanatory incompleteness can assume two forms. The omitted influence may have a bearing on the phenomenon to be explained while, at the same time, being unrelated to other influences with such bearing (e.g., in statistical analysis, the case of orthogonality). If so, the explanation would be impoverished by ignoring this influence, but the estimated impact of the causal factor(s) encompassed by the theory would not necessarily be biased empirically—it is just that the picture would be incomplete. If, however, the causal factor(s) included were related to those that were not, even this partial picture could be distorted, for what is attributed to the first may actually be reflecting the operation of the latter. In other words, even the partial explanation is misleading. Consequently, and while an elegant theory is preferable to one that is cumbersome, aesthetic appeal cannot be weighed equally with completeness. A sound theory, one that does a good job of explanation and prediction, is likely to be based on true premises and encompass most pertinent causal propositions.

Valuable Theories

Because a theory can be both commendably sound and disappointingly banal, it must be evaluated not only in terms of its epistemological virtues but also by the added intellectual value it provides. Value, in turn, has both a qualitative and a quantitative dimension, since a theory may be valuable because of: (a) the *scope* of the phenomena it accounts for, and, (b) the *significance* of the phenomena it addresses.

Theoretical Scope A complete theory is one that neglects no significant component of the explanation behind an outcome. By contrast, a theory of great scope is one from whose premises many *implications* may be drawn. Theories that correctly account for many phenomena that had previously been poorly understood, or that suggest new paths to explanation, are obviously better than those which illuminate a very narrow range of questions or questions to which we already had satisfactory answers. Accordingly, a theory's scope is a first measure of its value.

This criterion is consistent with Imre Lakatos' dictum that bodies of related theory ("research programs") should be evaluated in terms of how "progressive" or "regressive" they prove to be.[24] In Lakatos' view, theories are rarely rejected just because some of their premises appear untenable. Rather, most have a "hard core" of assumptions and hypotheses considered irrefutable, in the sense that they cannot be questioned without opting out of the research program. The hard core is shielded by two sorts of rules. The first (the "negative heuristic") defines this core by specifying which assumptions and hypotheses are unassailable. The second (the "positive heuristic") indicates how the research program may expand and develop, consistent with the hard core's assumptions.

Thus, the research program establishes a "protective belt" of generalizations and assumptions, subject to a range of permissible modifications in light of the evidence. If observational evidence is at odds with the hard core, the explanation and the remedies are to be found in the protective belt. By adjusting the protective belt, a theory can be modified to yield a product that continues to resemble itself, but without some of the problems and inconsistencies of the original theory—permitting it to remain part of the research program, with a family resemblance ensured by the negative heuristic shielding the hard core. For example, Marxism may be considered a research program, one which in many eyes entered a degenerative phase some time ago. When its initial predictions of a rising rate of surplus value extracted from labor in response to declining rates of profit, and of a correspondingly intensifying class conflict, failed to be vindicated in the early part of the twentieth century, Lenin's *Imperialism*[25] sought to demonstrate how profit rates could be maintained without increasing the rate of surplus value extraction: by means of imperialist expansion. With the aid of additional assumptions, Lenin attempted to rescue the essence of Marxist political economy from falsification by events. Similarly, political realism can be considered a research program, whose neorealist variant, as devised mainly by Kenneth Waltz,[26] was intended to develop an additional layer of assumption (an expanded protective belt) meant to cope with flaws in the classical realism of authors such as Hans J. Morgenthau.[27]

But how far can one go in adapting the protective belt? In other words, when does a research program become too burdened with ad hoc assumptions and exceptional conditions to justify further fidelity to its basic premises? According to Lakatos, research programs remain progressive as the new assumptions *expand* the range of phenomena that the theory can explain. A research program that continues successfully to account for novel phenomena satisfies this condition and should be kept alive. One that does not is a "degenerating" program and it deserves to be abandoned. Similarly, when two competing research programs within the same field of inquiry are compared, the one that is more progressive—i.e., the one that explains what the other does and then some—is to be preferred. In our case too, and in a related vein, a theory with a wider explanatory reach is to be considered preferable to one with a narrower reach, other attributes being equal.

Significant Theory In addition to conditions of scope, a theory's importance is also defined by what may loosely be termed its significance: the knowledge it provides must be knowledge worth having. For this to be the case, and as we are dealing with empirical theory, its concepts must refer to world states that exist or that could exist, and for which an acceptable operational definition is provided; in other words, the concepts must be empirically meaningful.[28] The "righteousness" of policy is not an empirically meaningful concept, but the "the cost" of a policy is. Moreover, a concept may be empirically meaningful in one area of the social sciences, but not in another—simply because the empirical content may not carry over from one to another. The issue is pertinent to much political science, where concepts developed in other social science disciplines sometimes are uncritically imported, yielding categories bereft of much empirical meaning upon transplantation.[29] Beyond this, judgments about significance are rooted in values and expectations rather than in the canons of scientific inquiry. Because the knowledge produced within the field of international relations, especially that cast in quantitative and formal terms, is sometimes charged with triviality, we must begin with an overview of the foundations on which such charges rest.

Triviality can assume at least two forms. If a theoretical argument involves a question whose answer we have no reason to care about, we are in the presence of a pure form of triviality. The reasons for our indifference may be pragmatic. Since we tend to care about knowledge that affects our well-being, a judgment of triviality could result from a perception that the question involved does not concern our well-being in any discernible fashion. But, as Cardinal Newman pointed out in his celebrated work, "There is a knowledge worth possessing for what it is, and not merely for what it does."[30] Justifications for the humanities rarely rest on pragmatic grounds, and even scientific results need not provide implications for action to be considered important. For example, most people would regard as meaningful theories that contribute to our understanding of human evolution, although it is unlikely that many practical implications would be drawn from even the best evolutionary theories. Similarly, geological theories on continental drift would scarcely be judged trivial, even though they contain few guides to action. Thus, even relatively "useless" knowledge may be considered important if it addresses questions that culture, human experience, or natural curiosity lead us to seek to answer. If we simply do not care, the knowledge is intrinsically trivial, no matter how sound the theory behind it, or how broad the scope of meaningless phenomena it addresses. For reasons discussed in chapter 1, in the traditional SIR era—for example, the work of Morgenthau, Wolfers, Bull, or Fox—the scholarship could hardly ever have been charged with triviality.

In addition to intrinsic importance, the significance of conclusions is sometimes measured by how surprising they are in terms of initial expectations. Generally, the interest that a statement of theoretical relation, like a statement of fact, generates is inversely proportional to its antecedent plausibility, since it is usually deemed more important to demonstrate the unexpected than to confirm the self-evident.

It may be objected that, while it could seem trivial to confirm what most people would have in any case expected, it is often a good idea to do so—simply because the contrary finding, though unlikely, would be extremely interesting.[31] For example, although most people would yawn at the finding that U.S. presidents dislike Communism, it would be highly intriguing to discover that, contrary to expectations, some do not necessarily feel this way. This argument makes a useful point, but it assumes that the question to which the anticipated answer is provided is of considerable *intrinsic* importance; otherwise, it could not be considered important, even if its antecedent plausibility had been very low. The degree of triviality, therefore, *jointly* depends on the intrinsic significance of the question and on the antecedent plausibility of the answer: it is inversely proportional to the former and directly proportional to the latter.

Having defined desirable theoretical knowledge as that which is both sound and valuable, each being further defined by two properties, we may ask how these qualities could be undermined by adding, as an additional requirement, that the theory be policy-relevant. This implies an overview of the forms that relevance can take, since its implications for the growth and quality of theory may depend on the type we have in mind.

Theory and Relevance: Is There a Tradeoff?

Would a quest for policy relevance impair the quality of theory? Since "disinterested" scholarship provides the standard of comparison, the question is whether policy relevant theory is likely to do less well than its disinterested counterpart in terms of soundness and/or value. There are two reasons to think that this might be the case. The first is *epistemological*: is there something intrinsic to the logic of inquiry of the two sorts of theory that favors the soundness and/or value of the disinterested variant? The second concern is essentially *sociological*: it springs from the possibility that the professional incentives by which the two sorts of scholars—relevant and disinterested—are driven may favor the soundness and/or value of the latter's work. Figure

4 catalogues the possibilities for theoretical impairment, and an answer to the question posed in this section requires a comparison in these terms.

A first observation is that there are no purely *epistemological* reasons for thinking that either type of theory should fare better with regard to *soundness*. In either case, the irreducible function of theory is to explain, an ability logically independent of whether the phenomenon to be explained involves a policy outcome or not. As Philip Melanson has pointed out, "relevance is a perspective applicable to the focus of research and not to the epistemic quality of inquiry."[32] But there may be something about the patterns of inquiry associated with relevant or disinterested theorizing that could affect the *value* of their respective contributions. It is also conceivable that the professional incentives and culture proper to the two types of scholarly work could affect differently their soundness and/or value.

Truth and Relevance If the substance of a conclusion matters more to a scholar than its empirical correctness, the argument's premises may be

Source of Impairment	**Truth**	**Completeness**	**Significance**	**Scope**
Internal Logic	Relevant / Basic	Relevant / Basic	Relevant / Basic	Relevant / Basic
Professional Incentives	Relevant / Basic	Relevant / Basic	Relevant / Basic	Relevant / Basic

FIGURE 4 Comparison of Relevant and Basic Theory

distorted, consciously or not, to justify the desired inference. The incentive to distort could spring from partisan objectives or ideological blinders. Inducements to delude could infect most forms of relevance, but one suspects that they are most to be feared in the instrumental case, for it is there that desired outcomes are involved most directly. Incentives to mislead could also be found in estimates of direct or secondary costs, if a scholar's commitment to certain policies should cause their costs to be minimized. The possibility of tendentious distortions in policy-relevant theorizing cannot be dismissed, but a search for concrete instances turns up very little,[33] suggesting that the concern may be overstated. In any case, this form of corruption cannot be considered the monopoly of relevant theory, since ideological values and political predilections may color *any* scholarship that engages political values. A statement about the viability of socialism or, say, about the peacefulness of Islam might reflect a scholar's political inclinations, whether or not that statement had any obvious bearing on policy choices.

An incentive to deceive could also be linked to the narrow professional interests of scholars, rather than to their political beliefs. For example, the problem may be rooted in a desire to create maximum scholarly impact, either by challenging a broadly held assumption, by seeming to fill a widely lamented gap in knowledge, or by scoring points in a visible academic debate. While it cannot be denied that such objectives weigh heavily on many scholarly minds, or that, in exceptional cases, truth may be twisted accordingly, there is again no reason to suppose that such incentives are more likely to infect policy-relevant work than scholarship with no concern for policy.

It is important to remember here that truth may also suffer from problems that have no basis in an incentive to distort. In inductive work, the problem may stem from a necessarily arbitrary operational definition of theoretical terms. Thus, whether or not it is true that democracies don't fight each other may depend on whether or not one accepts the convention that defines war as an interstate conflict involving at least 1,000 battle deaths.[34] Similarly, findings about international inequality that are true when national wealth is measured in conventional (GNP per capita) terms may have to be modified if a broader measure of quality of life is substituted.[35] The risk here is not that truth is misrepresented, but that sometimes it is difficult to agree on its exact boundaries. Similar problems may be produced by imperfect measuring instruments and procedures, which, if the measurement error should be systematic rather than random, may bias the generalizations produced by the research. But there is no reason to think that systematic, though wholly unintended, measurement errors are more apt to impair the truth of theories that try to be relevant than of those that do not.

Truth may also be undermined by simplifications designed to foster theoretical parsimony and elegance, a problem more often encountered in deductive than in inductive work.[36] Simplifications that strip away redundant layers of meaning or that ignore idiosyncratic deviations from common tendencies are integral to theory-building. But when simplifications play havoc with the truth of the premises employed, the implications for explanatory generalizations are debilitating, even where interesting conclusions follow as valid deductions from dubious premises. An illustration is provided by realpolitik's claim that the pursuit of power is a dominant aspiration of states, one that pervades their conduct of international affairs. In the "classic" realism of Hans Morgenthau, the power drive is rooted in one of those "elemental bio-social drives by which in turn society is created."[37] In the neorealism of Kenneth Waltz, a concern about relative power flows from the anarchic structure of the international system, and the pervasive security dilemma this creates.[38] Certain predictions, especially about the way in which nations acquire and manage power, follow naturally from this premise, and gratifyingly elegant models of international politics have been derived from this foundation. But the truth of their assumptions is tenuous; as an empirical matter, one can identify many nations that, linked to other members of the international system by objective conflicts of interest, are keenly attentive to power considerations. But it is equally easy to compile long lists of countries that are not troubled by security fears, and whose policies indicate no great concern with power. Yet so influential is political realism, so convenient are its assumptions for theory, and so many professional careers have been built on these assumptions, that there has been little incentive to test their empirical correctness. This is unfortunate. Given the ultimate implausibility of many of these premises, one may doubt their truth.

Significantly, contemporary realism's assumptions are unrelated to attempts at relevance. While Morgenthau was concerned with the practical applications of his precepts, Waltz's contribution to neorealism displays no great interest in policy implications; his objective is a pure theory of international relations.[39] It seems that misleading simplifications, with their corresponding impairment of explanatory ability, are likely to occur wherever parsimony is more valued than truth. It is likely that deductive research compares unfavorably with inductive work on this basis, but it is not likely that the problem weighs more heavily on relevant theory than on disinterested academic efforts.

To some extent, truth may also fall victim to the imperative of theoretical novelty that is a weighty element in the academic reward structure. This novelty may be substantive, but too often in recent years it has been displayed mainly in the forum of new research techniques. As we argued in chapter 1, trudging over well-charted paths does little to enhance scholarly reputations or the growth of knowledge. As one observer noted:

> There exists the possibility that in some fields of science, where many basic truths are fully known, the emphasis on novelty will detach itself from social utility and come to constitute its own reward. . . . A considerable gap between truth and novelty seems to have materialized in the field of political studies.[40]

Even though the exact impact of a quest for novelty on the truth of explanatory propositions is hard to determine, it would seem less likely to affect policy-relevant than disinterested work. With the former, pragmatic purpose is likely to outweigh pure novelty as a measure by which scholarship is judged. This is clearly the case with demand-driven theory; in the supply-driven model, the only instance where this may not be so is with basic theory that becomes relevant only in light of some subsequent problem (and where theory is not developed in response to a practical challenge). In either case, it is hard to have a significant and sustained influence on policy with conclusions or premises that are wrong.

The Goal of Completeness An emphasis on policy usefulness could conceivably limit the comprehensiveness of theoretical explanations, especially if there was some demand-driven urgency for a particular study. Even so, the magnitude of the problem depends on how broad a conception of relevance is adopted, as well as on the pattern of incentives behind the analytic endeavor. For example, the policy tools included in the instrumental relations outlined by the scholar could be determined by an estimate of how effectively they could be acted upon, neglecting those which, while causally significant, are less malleable. Similarly, although recourse to certain policy instruments might also be precluded by domestic politics, or cultural constraints, or might not be affordable in terms of their direct costs and secondary consequences, this does not make them less necessary to a theory accounting for the desired outcome. Scholars might also be led to emphasize policy instruments that promise to have the greatest causal impact on the desired outcome, disregarding those whose influence is less weighty, with similar costs to theoretical completeness. Accordingly, if the analyst is guided by a narrow notion of short-term practicality rather than a broad conception of what policy-relevant knowledge should encompass, the explanatory structure behind the policy recommendations may be weakened by significant gaps.

Afflictions of this sort are possible, but the problem is not rooted in the intrinsic *nature* of policy-relevant work. Because soundness is a necessary condition for meaningful theoretical relevance, and since theory must be reasonably complete in order to be sound, a proper view of the analyst's job precludes omissions of this sort. Thus, in a satisfactory program of policy-relevant theorizing, the instrumental relations would encompass a comprehensive statement of links between policies and outcomes, an adequate survey of pertinent contextual considerations, as well as a discussion of direct and secondary consequences. Influences that in a vision bounded by narrow practicality might be neglected become variables whose values are explicitly accounted for.

On grounds of both truth and completeness, risks to soundness cannot be entirely neglected, but they appear modest and, where they cannot be dismissed, their source is more likely to lie in misguided professional incentives than in the logic of inquiry inherent in relevant theory. By the same token, there is nothing to suggest a bias toward incompleteness on the part of disinterested theory, particularly with general propositions empirically derived on the basis of an examination of observational data, as in statistical models whose success is partly measured in terms of variance explained. Some grounds for concern can be found in deductive work, where elegance and parsimony are often purchased at the cost of theoretical completeness. But the problem, if there is one, is not grounded in the logic of deduction per se, but in incentives that are more aesthetic than genuinely epistemic.

Thus, there is little grounds for believing that relevant theories are likely to be less sound, qua theories, than those rooted in a disinterested agenda. But, since this does not exhaust the standards of merit for theory, we must ask whether relevant theory is apt to be less valuable than is pure theory.

Relevance and Theoretical Significance The first issue is whether policy-relevant theorizing asks questions that are intrinsically at least as meaningful and interesting as those addressed by scholars with a commitment to disinterested theory. We believe that it does. A question that claims the attention of a policy-relevant theorist typically involves the pursuit of some desirable objective; while it is logically possible that objectives of trifling importance would be addressed, plainly this is unlikely. Lacking much incentive in the form of professional rewards to be relevant, scholars usually embark on such work because of the importance attached to the issues involved, an importance typically measured by the consequences of failing to attain the policy objective.

However, because intrinsic importance need not be measured exclusively by pragmatic criteria, issues addressed by disinterested theory may be even more significant. In fact, it is sometimes assumed that the really important scientific questions can only be tackled with a wholly disinterested frame of mind, unfettered by practical concerns. Abraham Flexner expressed a widely held view when he maintained, decades ago and in a subsequently much-quoted article, that:

> Throughout the whole history of science most of the really great discoveries which had ultimately proved to be beneficial to mankind had been made by men and women who were driven not by the desire to be useful but merely by the desire to satisfy their curiosity.[41]

We believe that this view rests on a romanticized view of scholarly curiosity and of the manner in which questions are selected for scientific examination in the academic community. As we discussed in chapter 1, in most of the social sciences, where professional recognition depends largely on the apparent sophistication of the research tools and conceptual categories employed, questions are often selected according to the methods and concepts that can plausibly be employed in their analysis. These are not criteria that have much bearing on the intrinsic importance of the issues addressed, and some loss may be expected here. Often too, issues are selected because, for whatever reason, a critical mass of colleagues has already decided to deal with them. In part, this view stems from an understandable conviction that a question addressed by a large number of colleagues must be worth addressing. It also springs from the fact that an intensely studied issue implies the easy availability of empirical information (say, a useful data set) upon which research can readily be conducted. Partly too, it is because incorporation into a vigorous stream of scholarship stands to promote the visibility of associated work by embedding it in a lively pattern of mutual citation, promoting the professional visibility of participating scholars. These are circumstances better explained by the sociology of knowledge than by the canons of scientific method, and experience indicates that either significant or trivial issues may occupy scholarship in the process.

In principle, issues that are not currently very meaningful may become so subsequently, as new information becomes available or new needs or interests emerge. However, to be at all plausible, the justification requires that the currently trivial does, in fact, often prove significant with time. Whether or not this is so in the natural sciences may be debated; it would be a debate complicated by the difficulty of disentangling the various strands of scientific work, both pure and applied, that precede most useful discoveries in these areas. In the social sciences, instances of trivial work for which value has later come to be found do not readily come to mind—what was trivial decades ago is likely to be every bit as trivial today.

In the field of international relations, charges of triviality are also directed at conclusions that appear self-evident. Here, the claim of banality can be refuted if the importance of a correct answer is great enough to make *any* uncertainty intolerable. The claim can further be undermined in inverse proportion to the conclusion's antecedent plausibility (not all expectations are equally firmly held). Controlling for intrinsic importance, the task is to decide whether policy-relevant thinking is more or less prone to confirming the expected than is basic-theoretical work.

Such concerns are at least plausible. For example, it might be feared that political rewards—in forms ranging from public acclaim to access to power—may sometimes be proffered to scholars who prove the obvious, because even that which is apparent may be hotly denied in political debates. But actual instances of such intellectual corruptions do not suggest themselves, and it is hard to be impressed by an abstract possibility supported by so few concrete examples. As far as disinterested theory is concerned, there are no reasons intrinsic to its logic to lead us to expect the banality of high antecedent plausibility. Nevertheless, this form of triviality could be (and sometimes is) the byproduct of academic agendas wherein the significance of conclusions matters less than the appeal of the analytical methods employed. Where this is the case, there is no reason to expect that the substance of the work would be especially interesting (except, perhaps, on purely methodological grounds).

Relevance and Theoretical Scope Since the scope of what scholars seek to explain is restricted by the questions asked, we might wonder whether disinterested scientific curiosity yields a greater range of questions than does policy-relevant work. Again, such concerns are at least plausible. Except in some supply-driven instances, questions generated by policy-relevant theory tend to be limited by the objectives sought by policymakers. In turn, policy objectives are guided by current interests and values, and these may be bounded by shifting and parochial considerations, in addition to ideological agendas.[42] Nevertheless, the gravity of the problem depends on the category of policy-relevant knowledge involved.

It could be argued that concerns loom largest with cases of demand-driven relevance where theories focus on instrumental relations alone, and that, if the enterprise were broadened to encompass contextual relations, as well as direct and secondary consequences, the resulting theoretical edifice might be very encompassing. A counterargument might be that in policy-relevant work, the starting point is always given by the policy objective, and that the theory's scope cannot extend very much beyond its confines. Assuming that this is so, the criticism does not distinguish theory that seeks to be useful from that which does not, since there is no reason to think that the latter's explanatory structures are less firmly rooted in the outcomes to be explained than the former's. The only issue that really matters here is whether either type of theory concerns itself with a wider range of outcomes than the other, a question whose answer would require careful examination of the two bodies of scholarly literature. Again, and we have no a priori basis for predicting what conclusions would be reached.

It must be observed that degenerative research programs—those that keep expanding their protective belt of ad-hoc assumptions and auxiliary hypotheses with no corresponding increase in the range of phenomena explained—are unlikely to be compatible with policy-relevant work. Here, pragmatic incentives make an understanding of policy outcomes (both the causes and implications) the principal justification for theoretical development, and the measure of its success is whether, with regard to these outcomes. These research programs explain the range of the possible and the implications of choices. Where observational data indicate that a theory cannot explain these matters, it will probably be modified or abandoned. Here, then, usefulness provides a direct incentive to create and maintain well-performing theory. Concerns extraneous to its objectives—for example, the faddishness or apparent sophistication of the analytical tools used—are unlikely to sustain an unsuccessful theory. Therefore, considerations that sometimes ensure the longevity of degenerative theory are less likely to afflict relevant than disinterested work, and the former's explanatory scope may benefit accordingly.

Conclusions

The impact of a quest for relevance on the quality of international relations theory will continue to be debated,[43] but certain claims must be challenged. There is little reason why relevance should distort the proper relation between scholar and society, either in terms of unwelcome academic intrusion in discussions about the society's ends or by placing the evaluation of scholarship in nonprofessional hands. The more closely these claims are examined, the less conviction they carry. Most important, there is little reason to assume that policy-relevant scholarship must fare less well than its disinterested counterpart in terms of either soundness or value. The possibility that relevance would corrupt knowledge by twisting it to conform with ideological biases, as at one time the natural sciences were hobbled by being tethered to theological agendas, may reasonably be set aside; it is highly implausible so long as professional scholarly peers act as watchdogs on issues of theoretical soundness. And while it is of course possible that weak arguments or empirical evidence might be used to justify certain policies, few cases can be found of SIR that is corrupted in such a fashion over any significant period of time. In a liberal society the competitive marketplace of ideas makes it likely that such ideas will be detected when they are used in this way.

Other threats may be entangled with a quest for relevance, but they are hardly unmanageable; in any case, similar problems afflict disinterested theory. Truth, completeness, and explanatory scope could, in principle, suffer from the professional incentives of relevant theorists, if these incentives were to lead them to generalizations that favor simple, direct, and immediately workable guides to action, of the sort that may be most appealing and comprehensible to policymakers. But in the nature of things, this is a problem more plausibly encountered in policy *advocacy* than in policy-relevant *scholarship*. These are very different types of work. Moreover, the structure of professional rewards facing the scholar unburdened by a concern with relevance seem at least as often to be based on criteria external to the quality of the explanations offered. Whatever qualms about relevance one may entertain, they tend to be rooted in assumed professional incentives, not in the nature of the explanatory enterprise. Moreover, these incentives are often compared to an overly idealized conception of the drives behind the development of disinterested theory.

If it is not likely that theoretical development would be harmed by relevance, it may actually benefit from a concern with practical implications. In other words, the very *opposite* of what is sometimes maintained may be true.

In an observation as apposite to the field of international relations as to other behavioral sciences, Abraham Kaplan observed that inquiry related to practice

> has the advantages of providing anchorage for our abstractions, and data and tests for our hypotheses. For behavioral science the advantages are especially great, counteracting the tendency to empty verbalizations characteristic of some sociologies, or the self-contained formalism of certain economic theories.[44]

In a similar vein, Joseph Ben David, a distinguished sociologist of science, has observed that an intellectual grounding in the world of practice may lead to considerably more innovative and interesting work than scholarship shaped exclusively by the ethos of ivory towers:

> Practice . . . is an invaluable guide in locating relevant problems—rather than finding illusory ones, which happened not infrequently in the history of academic thinking. . . . The problems of practice are always real, and it usually possesses a tradition which is the result of a long collective process of trial and error and which may suggest the way toward new theory and new methods.[45]

Nurtured both by a comprehensive view of usefulness and an insistence on high standards of scholarship, relevant scholarship may produce premises and thus conclusions that are more likely to be empirically true than those yielded by disinterested theory. It is less likely to be rooted in those conceptual issues that have little connection to a meaningful empirical reality, even though they provide vehicles for the advancement of academic careers. It may also stand a better chance of being valuable: by definition, it deals with matters of practical significance; matters that are no less likely to be intrinsically important, from a theoretical standpoint, than those addressed by theoretical efforts unconcerned with usefulness.[46] Accordingly, both soundness and value may benefit.

We must, finally, remain open to the possibility that the pursuit of useful knowledge actually may produce theory of a *better* quality, because it would be empirically more meaningful and more focused on the truth of its premises, than a program of knowledge-creation dominated by the reward structure of disinterested theory.