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Knowledge for Statecraft[1]
Lecture Given by the Winner of the Johan Skytte Prize in Political Science, Uppsala, October 3, 1998

Alexander L. George*

The occasion of receiving the Johan Skytte Prize in Political Science is perhaps an appropriate occasion to recall the origins of my interest in developing knowledge for statecraft. During the course of my years as a member of the RAND Corporation in Santa Monica, I became aware that much academic research on international relations was not providing the type of theory and knowledge needed for dealing with the challenges the United States was facing during the Cold War. I felt it necessary to undertake the challenging task of devising a new approach for producing the type of knowledge that would come closer to meeting the needs of policy makers who were trying to avoid getting into dangerous war-threatening crises and, when such crises nonetheless occurred, to manage and terminate them without triggering escalation to war.

Historical experience needed to be studied in ways that would provide useful knowledge of problems of statecraft such as deterrence, coercive diplomacy, escalation control, crisis management, crisis prevention, détente, security cooperation, etc. I was much impressed and inspired by a statement by the distinguished historian of the Renaissance, Jacob Burckhardt, who once remarked that the true use of history is not to make statesmen 'more clever for the next time' but to make them 'wiser for ever.' Following Burckhardt's advice in world politics, however, has proven to be particularly difficult. To be sure, it is not easy to learn from history, though almost every statesman and general has professed to have done so!

Why these difficulties in learning from history? In the first place, policy experts and scholars often disagree on the lessons of a particular historical event. Even when there is substantial agreement on the lessons of a particular case, statesmen often *misapply* these lessons to a new situation that differs

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from the past one in important respects. Clearly, attempts to draw lessons from a *single* case are dangerous. The need to avoid relying on a single historical analogy to diagnose and deal with a new case was the subject of an important book a few years ago, *Thinking in Time*, by two distinguished Harvard scholars, Richard Neustadt and Ernest May (Neustadt & May 1988). However, while these authors emphatically warned policy specialists to forego the habit of relying on a single historical analogy – such as, for example, the Munich analogy – Neustadt and May did not address the question of how, then, can one correctly formulate the lessons not of one historical case but from many cases of a given phenomenon.

For this purpose I developed a research strategy and a methodology that would produce systematic findings from the individual cases that could be cumulated within a theoretical framework. The challenge was to find a way of cumulating the 'lessons' that might be drawn from each instance of a given phenomenon – e.g. cases of deterrence – into a broader empirical theory that would identify the conditions that favored successful use of a given strategy and also other conditions that made successful use of that strategy very unlikely. I called the new methodology 'structured, focused comparison.'[2]

I have employed this approach in a research program over the past thirty years in order to develop policy-relevant theory for each of the following generic problems – deterrence, coercive diplomacy, crisis management, crisis prevention, security cooperation. I refer to them as 'generic problems' because policy makers must deal with each of them on numerous occasions.[3] I have found that a historical perspective on these problems is not only useful, but indeed it is necessary for developing policy-relevant knowledge of these activities.

The study of 'statecraft,' as historians used to call it, provides a basis for two-way interaction between historians and those political specialists who, like myself, believe it is necessary to study what goes on inside the so-called 'black box' of decision making and the 'black box' of strategic interaction between states, and not simply to make assumptions about these processes as do rational choice and game theorists. In brief, I have felt that it is necessary for political scientists to 'take the cure of history,' and it is quite encouraging to see in recent years a considerable re-emphasis on the importance of historical explanation in the fields of comparative politics and political sociology as well as in the study of international relations.[4]

Some colleagues in political science have expressed concern that an interest in developing policy-applicable knowledge will divert attention from the important task of developing a scientific theory of international relations. I emphatically disagree and have emphasized to my colleagues in political science that a policy-oriented approach to the study of foreign policy is in fact necessary to develop international relations theory more fully. Research that aims to develop policy-applicable knowledge is not at all inconsistent, as some scholars believe, with efforts to develop international relations theory. Quite the contrary: development of policy-related theory is indispensable for the further development and refinement of international relations theory.

Let me elaborate this point by commenting briefly on Kenneth Waltz's structural-realist theory, which, as you know, is the dominant theory of international relations. Structural realism is certainly necessary, but, I submit, quite insufficient by itself either for *explaining* foreign policy decisions and outcomes of interaction between states, or for *conducting* foreign policy. Indeed, Waltz himself emphasized that his theory is *not a theory of foreign policy*. Waltz went further, explicitly warning readers not to expect his theory to 'explain the particular policies of states,' and he stated that it was an error to mistake his structural-realist theory of international politics for a theory of foreign policy which, in his view, did not exist and could not be easily developed. Actually, as Waltz acknowledged, his theory 'makes assumptions about the interests and motives of states, rather than explaining them.' Waltz's theory is at most a statement of *constraints* on the conduct of foreign policy rather than a theory of foreign policy. 'What it [structural-realist theory] does,' he says, is to 'explain the constraints that confine all states' (Waltz 1979, 121–22).

We are left, then, with a large 'vacuum' in the theory of international relations that must be addressed if one is interested in developing more and better knowledge for the conduct of foreign policy. However, I do not believe it is useful for the purpose of filling the large vacuum in international relations theory to try to develop a *general* theory of foreign policy. Much more useful contributions to statecraft are made by focusing specifically on each of the many generic problems encountered in the conduct of foreign policy from time to time but in different contexts – generic problems such as deterrence, coercive diplomacy, crisis management, war termination, preventive diplomacy, crisis prevention, mediation, cooperation, etc.

I regard 'generic knowledge' and 'theory' as synonyms, but in speaking to policy specialists I have found it useful to use the phrase 'generic knowledge' rather than the word 'theory.' Thus, in interviewing policy specialists a few years ago for my book *Bridging the Gap: Theory and Practice in Foreign Policy* (George 1993), I quickly found that the eyes of policy specialists glazed as soon as I used the word 'theory.' But they nodded approvingly when I spoke of the need for 'generic knowledge!' I wondered why this was so, and the answer was both obvious and important. Policy specialists recognize that generic problems exist in the conduct of foreign policy – for example, the task of deterrence emerges repeatedly over time with different adversaries and in different contexts. Therefore, policy experts readily agree that general, or 'generic,' knowledge about the uses and limitations of a particular strategy or policy, derived from proper study of past experience, can be very helpful when they have to consider possible use of that strategy in a new situation.

Turning to another point: systematic assessment of past experience with a particular activity such as deterrence enables researchers to formulate *conditional generalizations*. These are generalizations that identify the conditions and circumstances under which a strategy is and is not likely to be successful. Such conditional generalizations are much more useful to policy makers than broad probabilistic generalizations.

Thus far in this essay I have addressed a number of related questions: (1) why should academic specialists care about making scholarly research on international relations more policy-relevant; (2) what benefit do I think this would provide for the further development of international relations theory; and (3) what methods exist for doing research that can produce generic knowledge and conditional generalizations on problems of statecraft? In the remainder of this essay I shall offer some observations on the problem of the 'gap' between theory and policy – a gap that exists because of the differences between the culture of academia and the culture of the policy-making world. (For a detailed discussion of the gap between the two cultures, see George 1993, Chapter 1).

Some years ago, I came to the conclusion that it is more appropriate to speak of 'bridging' the gap between theory and practice than of 'eliminating' it. The choice of words reflects an appreciation of the limitations as well as the uses of policy-applicable theory and generic knowledge. We have to recognize that knowledge of this kind can have only an indirect and limited impact on policy making. But since I claim that generic knowledge is nonetheless critical for sound policy, I need to explain this apparent contradiction.

The contribution that generic knowledge makes is to provide assistance to those who conduct policy analysis within the government and it helps policy makers to make judgments in dealing with specific situations. But generic knowledge cannot substitute for policy analysis within the government or for the policy maker's judgment. Even the best theoretical conceptualization of a strategy and the most highly developed generic knowledge of the strategy cannot substitute for competent policy analysis within the government in which analysts must consider whether some version of the strategy is likely to be viable in the particular situation which they confront. Similarly, generic knowledge cannot substitute for the judgment policy makers must exercise in deciding whether to employ that strategy in a particular situation, since that judgment must take into account relevant considerations not encompassed by generic knowledge of the strategy.[5]

It is in this sense that scholarly knowledge has an indirect and often limited relevance for policy. Nonetheless, as I have argued, well-developed generic knowledge of different instruments of policy is capable of making a critically important contribution to policy making.

How, then, can generic knowledge *aid* (not substitute for) policy analysis and the decision maker's judgment? First, it can assist them in making a sound diagnosis of a problem situation; then, on the basis of a good diagnosis of the problem, generic knowledge can help to determine whether an effective policy response is possible in that particular situation and what form it should take. Thus, policy-relevant theory and generic knowledge contribute to two essential tasks of policy making: the *diagnostic* task and the *prescriptive* one.

I place particular emphasis on the diagnostic contribution policy-relevant theory and generic knowledge are capable of making rather than on their ability to prescribe choice of policy. I believe it is important to emphasize the priority of the diagnostic function of theory and generic knowledge because all too often policy-relevant theory is defined solely as having a prescriptive function, and awareness of its limitations for *this* purpose often leads to negative or highly pessimistic judgments regarding the feasibility of policy theory. It should be obvious that correct diagnosis of a policy problem should precede – and as in medical practice – is usually a prerequisite for efforts to make the best choice from among treatment options. This analogy with medical practice is an apt once since the policy maker, like the physician, acts as a clinician in striving to make a correct diagnosis of the problem before determining how best to prescribe for it.

The science of microbiology and its relation to medical practice offers a highly relevant model for thinking about the relation of theory and practice in international relations. Consider the relationship of smoking cigarettes (and exposure to other carcinogens) to cancer. Statistical-correlational studies have long convinced most of us that some kind of causal relationship does exist. Microbiologists have been working for years – lately with considerable success – to identify some of *the intervening causal processes* between exposure to carcinogens and the incidence of cancer. Why is this important? Finding the causal link between smoking and cancer creates opportunities for developing new intervention techniques for halting the development of cancers. In other words, microbiologists are attempting to develop a policy-relevant theory of the relationship of smoking to cancer.

The emphasis in microbiology and medical practice on identifying the causal mechanisms that lead to a particular disease is highly relevant for understanding the task of developing policy-relevant knowledge of international relations. Knowledge of the causal mechanisms that operate in international affairs provides practitioners of foreign policy some useful leverage for influencing outcomes of interaction with other actors. Of course, the success microbiology is having in identifying causal mechanisms operative in many diseases cannot be easily duplicated in the study of international relations. Nonetheless, it is heartening that in recent years political scientists and some philosophers of science have increasingly emphasized the importance of identifying the specific causal mechanisms that help to explain, predict, and influence outcomes of interaction with other states. (See, for example, Elster 1983; 1989; 1993; Little 1990; 1998; Sayer 1992; and Dessler 1991).

What, then, are some of the implications of the preceding observations for at least some research on foreign policy? One implication is that theory and generic knowledge do not need to satisfy the high degree of verification that science attempts to achieve. Just as intelligent people are generally able to manage the many chores of everyday life reasonably well without benefit of knowledge that meets the highest scientific standards, so too can intelligent policy makers use available knowledge of the different generic problems of statecraft. In other words, when scientific knowledge has not been attained, we can at least strive to produce '*usable knowledge*.'

A second implication is that scholars should include in their research variables over which policy makers have some leverage and attempt to identify causal mechanisms which include such variables.

A third implication is that too strict a pursuit of the scientific criterion of parsimony is often inappropriate for developing useful policy-relevant theory and knowledge. The policy maker has to deal with complex situations that embrace many variables; he or she will get more help from 'rich' theories, by which I mean theories that embrace many relevant variables. Such theories must meet two criteria: their contents must be at least plausible, and they should contain indications of the particular conditions under which their propositions are likely to hold. Such theories and generic knowledge serve at the very least as a sophisticated checklist that reminds policy specialists of the numerous conditions and variables that can influence their ability to achieve desired outcomes. But when more fully developed, such 'rich' theories identify the conditions that favor – although they do not guarantee – the success of a policy option. Hence the objective of this type of policy-relevant knowledge is, as noted earlier, to produce conditional generalizations and 'usable knowledge.'

I would not want to leave the impression that development of generic knowledge of strategies and instruments of foreign policy is the only or the most important way in which scholarly research can contribute to policy. Scholars can and indeed do make other types of contributions. For example, well-informed, objective analyses of problems having to do with the impact of nationalistic, ethnic, and religious conflicts on intra-state and inter-state relations, nuclear proliferation, environmental and ecological problems, population and demographic trends, problems of food production and distribution, water scarcities, sanitation and health problems – all of these are an essential part of knowledge required for the conduct of foreign policy.

Still another type of useful scholarly contribution lies in the activity of *forecasting*, a topic that has received attention in the past but which remains in need of much additional research and reflection. Forecasting of possible future developments occupies a central role in policy making. Policy making requires forecasting, but the uneven quality of forecasts and the inadequacy of policy planning efforts in the US government over the years are well known.

What can academic scholarship provide to improve policy planning? Some years ago, Herbert Simon wrote about the importance of 'design theory' and 'design exercises' and made suggestions that need to be pursued (Simon 1969). Intelligence specialists within the government have made progress in developing '*analytical* forecasting,' again an essential refinement that should be pursued (see, for example, Nye 1994). Policy-relevant forecasting and policy planning, it should be recognized, require detailed knowledge of other countries and peoples as well as expertise in the subject matters being addressed.

These are by no means easy requirements to achieve. Given the global scope of American foreign policy interests and the very large number of countries on which specialized information and expert judgment are needed, there is a shortage of qualified area experts outside as well as within the government.

An encouraging development is the striking performance in making certain types of prediction by means of an expected utility model developed by Bruce Bueno de Mesquita and his associates. It should be noted, however, that the model is dependent on high quality inputs from area experts and, hence, is dependent on the informed sophistication of 'traditional' area experts.[6]

In addition, scholars can – and indeed do – make a variety of other types of contributions. Among these are the development of better concepts and conceptual frameworks which can assist policy makers to orient themselves to the phenomena and problems with which they must deal. Although scholars may not be in a good position to advise policy makers how best to deal with a specific instance of a general problem that requires timely action, they can often provide a useful, broader discussion of how to think about and understand that general phenomenon – such as, for example, the problem of ethnicity and nationalism.

A recent example of an important effort to mobilize scholarly knowledge and resources in order to deal with an overwhelmingly important policy problem is the Carnegie Commission's study of preventing deadly conflict. This recently concluded three-year study not only drew on available scholarly knowledge; it also stimulated important new scholarly efforts to fill the gaps in such knowledge. It is a fine example of a collaborative effort of high-level policy influentials and scholars to identify and evaluate tools for preventing or limiting violent conflicts (Carnegie Commission on Preventing Deadly Conflict 1997).

In my closing remarks, let me pose the following, somewhat provocative question: why should scholars study international relations if not to try to develop types of knowledge that can be relevant and useful in some way for the conduct of foreign affairs? Surely we should not study international relations simply to achieve a detached, Olympian view of international affairs that allows us to disavow any responsibility for the course of events in the world or to feel superior and second-guess policy makers who often stumble and bumble in conducting foreign policy! Surely our efforts to meet the criterion of abstract theory and the desiderata for scientific knowledge should not lead us to shirk the task of developing useable knowledge for statecraft.

I have emphasized that we need to recognize the limitations of structural realism, the dominant academic theory of international relations. Without ignoring its value, we need to recognize that structural realism is not a theory of foreign policy; and, hence, we are left with an important vacuum in theory and knowledge because structural realism does not come to grips with many central problems of statecraft that policy makers must deal with.

So I close with a plea that scholars devote more attention to meeting the need for developing better knowledge for statecraft, and express the hope that at least some of those present and my academic colleagues will decide to take more seriously the challenge this important task poses.

NOTES

1. This essay draws on many of the author's previous publications, particularly George (1993).
2. I will not discuss this method further in the present essay. For a detailed discussion, see George (1979).
3. Other scholars have made widespread use of the method. Two quite recent examples are the forthcoming books by Stephen R. Rock on *Appeasement in International Relations* (provisional title) and Bruce W. Jentleson, *Opportunities Missed, Opportunities Seized: Preventive Diplomacy in the Post-Cold War World* (Jentleson 1999).
4. Emphasis on the importance of historical cases has developed even among scholars who are identified with rational choice theory. Thus, Robert Bates and a number of his co-authors are publishing a book this year titled *Analytic Narratives* in which they emphasize that it is essential and fruitful to combine the rational choice approach with

historical case studies: '. . . theory with narrative is stronger than theory alone. And narrative with theory is more powerful than narrative alone' (Bates, et al. 1998, 236).

5. I have emphasized the importance of developing a better understanding of the relationship of analysis to the various types of judgments policy makers typically have to make. (See George 1993, Chapter 2). Deborah Larson and Stanley Renshon are currently collaborating in a study of the relationship between analysis and judgment.

6. For a recent description of this forecasting technique and its performance, see de Mesquita & Stokman (1994).

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