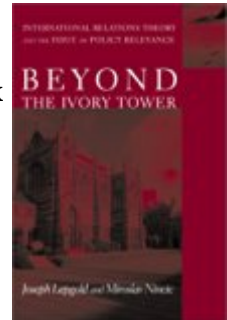


**Joseph Lepgold, Miroslav Nincic.** *Beyond the Ivory Tower: International Relations Theory and the Issue of Policy Relevance.* New York: Columbia University Press, 2001. x + 228 pp. \$81.00, cloth, ISBN 978-0-231-11658-9.



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## The Essential Irrelevance of International Relations Theory: A Challenge

Over the course of an average year, thousands of research papers are written and presented at academic conferences throughout the country on issues related to international relations. Many of these papers are later published, in one form or another as journal articles, book chapters, and even monographs. With the discipline's ever increasing number of specialized journals there are innumerable avenues in which to publish the results of our research, but how much of this "academic output" is relevant to the foreign policy community? How much of the research conducted by faculty and graduate students is useful in addressing the increasingly complex demands of the foreign policy establishment?

Over the past decade there has been a growing concern within the scholarly community that the academic discipline of international relations lacks relevance for practitioners of foreign policy. This is particularly troubling for a field of inquiry explicitly established less than a century ago to shed light on pressing policy issues (p. 28). A re-

cent article on the subject points to the lack of research conducted prior to September 11 on terror-related topics. After discussing the paucity of research related to terrorism, the author concludes that research agendas in this sub-field suffer from a "degree of marginality." [1]

It is in this context that Joseph Lepgold and Miroslav Nincic hope to contribute to the discussion on international relations scholarship and foreign policy relevance. The overall aim of their book is to revise what the authors describe as flawed assumptions related to the effects of policy relevance on the scholarship of international relations. In doing so, the authors hope to encourage the discipline to be more receptive to research that is both sound and useful (p. ix). To this end, they lay forth the thesis that international relations theory can be both more relevant and useful to exigencies of policymaking than is commonly believed, and that this goal can be attained with little or no effect on the quality of scholarship (p. 172).

Lepgold and Nincic support their view with five arguments. One of the arguments made by

the authors is that the gap between scholarship and policymakers is a result of institutional cultures, not an incompatibility in their endeavors. The authors stress the point that academics exist in a culture possessing a reward system that is not always compatible with the requirements of policymakers. Three areas of the academic incentive system are discussed. The socialization of scholars tends to focus their research on narrow issues instead of questions of a more fundamental nature. Additionally, the increasing importance of scientific inquiry has led to what the authors call "the triumph of technique" (p. 16). In an example of the proverbial tail wagging the dog, many believe that the methodological tools used to create scientific rigor have gained too great an ascendancy over, and at the price of, the substantive subject matter. Finally, the aforementioned issues are compounded by the tendency of scholars, in the words of one observer, to be "only concerned about the good opinions of about a dozen other academic specialists in their particular sub-sub-field" and not the needs of external communities. [2]

Another theme put forth by the authors to support their goal of policy-relevant international relations research addresses the type of knowledge produced by scholars and its usefulness to practitioners. Often it is believed that the only relevant information that scholars produce is instrumental, which deals with knowledge that elucidates the means to achieving policy objectives. The authors argue that scholars can contribute valuable assistance to the foreign policy process through contextual knowledge, which provides approximations of costs, consequences and context for a particular policy tool (p. 174). Additionally, the authors point out that the use of scientific methods within the scholarly community may provide a counterweight to the causal empiricism and analytical habits shaped by professional incentives of the policymaker (p. 54).

A third point made by Lepgold and Nincic is that the value of relevant knowledge is dependent upon the quality of the explanation it furnishes. Here the authors present the argument that although policy makers are capable of providing specific information needed to develop theoretically sound explanations, or what Alexander George termed "generic"[3] knowledge, scholars possess a comparative advantage in furnishing "general propositions derived from, or embedded in, some theoretical structure" (p. 175). Additionally, the peer review system in the academic community may assist in avoiding the inferential errors and perceptual biases that can occur from over dependence on what the authors term "ordinary knowledge" (pp. 41-48).

Another myth the authors go to great length to demolish relates to the methods by which information from the scholarly community reaches policymakers. Many within the academic community assume that only scholarship consciously written to be "policy relevant" is meaningful or interesting to policymakers. Scholarship produces many things, argues Lepgold and Nincic, which are "worth knowing even if no practical utility follows from that knowledge" (p. 54). In making their case, the authors position the social sciences' contribution to policy making into two rubrics: the demand-driven model and the supply-driven model.

For most observers, the demand-driven model is intuitively the most understandable when explaining how policy-relevant information moves from researchers to policy-makers. On any given policy issue there may be insufficient pre-existing information, thus leading to the commissioning of individual scholars, institutions or think tanks to fill this gap in knowledge. In this model, the government takes the initiative and information travels a direct route from producers to consumers. The authors balance this prevailing model with the supply-driven view of policy relevant information flow. The authors elaborate on several

paths in which policy information can move from the producers to the consumers without being previously requested. One example, the pure science path, focuses on knowledge that was produced with incentives unrelated to policymaking. The authors discuss the development of game-theoretic models and how they were developed with no real policy issue in mind, but were later applied successfully to such foreign policy problems as nuclear deterrence between superpowers and crisis management in the Middle East (p. 59). Another area where *a priori* non-policy-relevant research becomes useful is in uncovering future challenges. Research on the shrinking ozone layer, degradation of resource bases, etc. identify likely outcomes (p. 60) that are picked up from scholarly sources by third party interests (media, issue oriented groups, etc.) and are communicated directly and indirectly to policymakers (p. 175). It is during crises that the supply-driven needs make scholarly advice most useful. When, as Joseph Kruzel argues, some unexpected event (collapse of Soviet Union, September 11, etc.) scrambles the worldviews and policy processes, policymakers often look to the academy for new ideas on how to address a new world.[4] During these "windows of opportunities" scholars have the best chance of influencing policy in a substantial manner.

In examining their final argument, Lepgold and Nincic endeavor to refute the common academic misperception that applied scholarship equates with weak scholarship. Before addressing the crux of this argument, the authors discuss the potential dangers in research related to policymaking. It is here that, according to the authors, the goal of research (i.e., the quest for the "truth") may suffer from aspects internal to the research process. One potential problem is the danger that operationalizing concepts for use in theory construction may lead to distortion of reality through categorization. The example given by the authors demonstrates that the definition of "war" (interstate conflict involving at least 1,000 battle

deaths) is highly influential in accepting the thesis that democracies do not fight one another (p. 98). In essence, the relevance to policymaking of the Democratic Peace Thesis may be dependent upon how the concept "war" is defined.

Returning to the point of applied research equating weak scholarship, the authors argue persuasively that although there are internal threats to accuracy and relevance of scholarship, the goals of academic theory development are compatible with the requirements of policymaking. To demonstrate this, the authors assert that the quality of theory can be judged by examining its soundness and its value (p. 89). A theory is sound if it fulfills the main purpose of theory development to provide explanation and prediction. Additionally, theory is judged by the value it provides. Theory may be valuable for either the scope or the significance of the phenomena it addresses (p. 93). The authors go to great pains to demonstrate that these attributes of quality theory are the same criteria that policymakers desire in knowledge to make it relevant to their endeavors. Therefore, the authors see no reason that solidly constructed and policy-relevant theory development would compromise the intellectual integrity of researchers (p. 177).

After developing their argument in the first four chapters, Lepgold and Nincic examine the practical policy implication of two recent and influential research agendas. The goal of the two case studies, Inter-Democratic Peace and Liberal Institutionalism, is to examine the guidance that this research agenda provided practitioners (p. 108). They find mixed results. In the case of Liberal Institutionalism, the authors find a literature with a long pedigree that provides an impressive array of instrumental knowledge, but has had little effect upon policy (p. 171). Conversely, the Inter-Democratic Peace, the authors state, "represent[s] some of the best that social science has offered in recent years" and demonstrates that theory development undertaken with no practical in-

tentions has had considerable influence in the corridors of power (p. 136).

The book is not without its drawbacks. The two case studies, while valuable in addressing and supporting the authors' contentions, are insufficient by themselves and leave the reader wanting more. Specifically, two extra cases are needed. First, a chapter dedicated to what is arguably the dominant theoretical approach in international relations--realism, of one ilk or another. If, as the authors persuasively argue, a "clear understanding of cause and effect" is *sine qua non* for policy relevance, theory development and sound scholarship, then the theoretical perspective which has dominated the last fifty years should be addressed (pp. 3-4). Second, the authors select a fairly low bar to hurdle in connecting the scholarship and policy relevance of Inter-Democratic Peace and Liberal Institutionalism. A chapter on some of the newer challengers to traditional international relations theory would have increased the scope and value of the book. Specifically, a discussion of the potential policy relevance of the constructivist schools, feminist interpretations, post-modern approaches, world systems research, etc. would have provided an interesting and formidable task. Additionally, it would have engaged more than mainstream international relations scholars.

On balance, Lepgold and Nincic provide a well-written addition to the continuing debate on "Bridging the Gap" between academic scholarship and policy relevance. The authors effectively argue that the goals of international relations research are compatible with the specialized needs of policymaking. By demonstrating the relevance of international relations scholarships, the authors address head on the growing myth of irrelevancy. In doing so, Lepgold and Nincin provide both professions (academic researchers and policymakers) a valuable follow-on to their edited volume of the same theme.[5] The book is appropriate for upper-division undergraduate and gradu-

ate courses in foreign policy and international relations.

#### Notes

[1]. Bruce W. Jentleson, "The Need for Praxis: Bringing Policy Relevance Back In," *International Security* (vol. 26, no. 4, 2002), pp. 172-3.

[2]. James Kurth, "Inside the Cave: The Banality of I.R. Studies," *The National Interest* (no. 53, Fall 1998), p. 34.

[3]. Alexander L. George, *Bridging the Gap: Theory and Practice in Foreign Policy* (Washington, D.C.: U.S. Institute of Peace Press, 1993).

[4]. Joseph Kruzel, "More a Chasm Than a Gap, But Do Scholars Want to Bridge It?" *Mershon International Studies Review* (vol. 38, April 1994), pp. 179-81.

[5]. Joseph Lepgold and Miroslav Nincic, *Being Useful: Policy Relevance and International Relations Theory* (Ann Arbor: University of Michigan Press, 2000).

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