

# Sinology: Making the Sovietology Mistake Again?

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As a specialist in world politics, and thus, with an interest in all regions and countries but expertise in none, the invitation to contribute to this review was attractive. On the one hand, it offered the chance to learn a great deal more in regard to the structure, culture, and behavior of the Chinese national security/foreign policy establishment in the reform period of the past two decades. At the same time, it was also a chance to ascertain the extent to which the behavioral science movement in political science might have influenced the study of China and its external relations. To make that estimate, I followed three procedures. First, I examined the Introduction by editor David Lampton in order to understand his aspirations, recruitment criteria, consultations, and funding. Nothing unconventional there, beyond the fact that this project was probably more expensive in scholarship-months and money than most of its predecessors. Second, I examined the titles of the dozen chapters for clues to that orientation: efforts to *generalize* across nations, across decades, across cases, or across issues. Some mild intimations were detected, but nothing that conveys a search for either operational measurement or systematic generalization. Third, I scanned the associations and affiliations of the contributors, and found here most of the usual suspects in the pattern of traditional and conventional organizations: departments of Political Science at such universities as George Washington, Johns Hopkins, Southern California, Maryland,

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Boston, Cincinnati, and Hong Kong—along with such think tanks as the Council on Foreign Relations, Brookings, and the Rand Corporation.

Valuable as it is in bringing the latest serious scholarship to bear on the question, this volume thus tells us little about the effort of these specialists to move beyond the classical guidelines; let me therefore suggest ways in which they might begin to do so. As I see it, there are several things the Sinologists might do to make their research more useful in both the scholarly and policy-relevant sectors. First, of course, is the matter of evidence—the ways in which rival and often equally persuasive arguments are weighted and compared. In the more traditional mode, we tend to adhere to the plausible, which is another way of saying that the conventional wisdom of our time and place will suffice. By incorporating the rules of scientific evidence in their work, regional specialists would gradually be open to ventilating and testing a more complex and differentiated set of explanations. Space limitations preclude a full discussion of the implications here, but the literature is voluminous and a few illustrations might be useful.

A good place to start, it seems to me, is the need to go for greater semantic precision: such key concepts as *adaptation*, *fragmented authoritarianism*, *bureaucratic pluralism*, *corporate pluralization*, *market conditionality*, and *tactical learning* could become more theoretically useful were they: (1) more clearly defined, and (2) in quite a few cases operationally measured. Such measurement, of course, need not be in the conventional sense of scales and magnitudes; equally useful is the process of enumeration in which we set up a typology, operationally differentiate among the sub-types, and then count the number of cases falling into each sub-type or class of cases. Speaking of measurement, we encounter frequent use of magnitude concepts: *greater*, *fewer*, *faster*, etc., but hardly any effort to devise ways of actually measuring them. As a result, we end up using yardsticks that change from case to case and thus become quite elastic.

Worth noting in connection with key concepts is the relative scarcity of those widely found in contemporary comparative politics; the index is remarkably heavy on specific concrete names, agencies, places, and events

while rather light on general classes of phenomena. The inference one might draw from this is that the several authors make few efforts to generalize across time, and perhaps also across nations. Moreover, as one reads the various chapters (as informative and evaluative as they are), this inference is supported; Lampton and his co-authors certainly come through as Sinologists, highly knowledgeable about one country during one historical period, but not particularly interested in how Chinese experience compares to that of other states in the regional or global system, or even how it might compare to earlier Chinese experiences.

The cost of such indifference is higher than most of us seem to realize. It is not only an academic cost in the sense that comparison and contrast are always illuminating and that generalization is the essence of scientific knowledge. There is also a public policy cost, exemplified all too recently in the case of the USSR, whose attributes and behavior were endlessly misunderstood in the West. Thanks to the obsessiveness of the policymakers, the ignorance of the journalists, the gullibility of the politicians, and the vulnerability of the academics and their foundation colleagues to the blandishments of the cold warriors and their financiers, we ended up with a profoundly incorrect understanding of Soviet policy from the close of World War II (if not sooner) until Mikhail Gorbachev and his followers finally told the more accurate story. I would argue that we got the Soviet story wrong over and over for the seventy-five year period from the close of World War I to the end of the "Cold War" because we treated that case as exceptional: that the USSR could not be usefully compared to other countries, and had to be understood as a unique and distinctive case, unlike any other. The costs of that foolishness were, as I suggest, not only a lack of intellectual comprehension, but also a popular and elite misapprehension that brought us alarmingly close to blowing up the Northern Hemisphere. To avoid that serious error again, it is essential that our Sinologists get out of the rut, and recognize that this country is in no way *sui generis* but rather has all too many similarities to other societies that have existed and even flourished on the face of the earth.

To be sure, the work reported here is clearly inter-disciplinary, aware of such methods as survey research, and clearly attentive to the role of

domestic politics, public opinion, and international organizations. This is the good news. I have already suggested, however, that the great potential of the social sciences (other than history and the more traditional political science) is largely neglected. The relative absence of their concepts, their findings, and—perhaps most critical—their methods just do not make much of an impact on the study of Chinese foreign policy. This is unfortunate, and not only in the scholarly sense of the word. To deal with a society of such economic, political, and military potential, one with such increasing regional and global influence, we need a far better understanding of how that society works. By denying ourselves the insight and knowledge that might be provided by attention to the behavioral sciences, we jeopardize the welfare and security of those peoples whose lives are and will be intertwined with those of China. That means a rather large fraction of the human race.