JOHN A. ARMSTRONG

Comments on Professor Dallin's "Bias and Blunders in American Studies on the USSR"

In his article on Western studies of Soviet power, purpose, and policy—which I shall call Sovietology—Alexander Dallin has brought us to a timely reconsideration of the needs and prospects of our area field. Part of the problem is whether the "remarkable catalogue of hypotheses later abandoned or disproved" really indicates that specialists in Sovietology have been relatively ineffective. Still more important are the real reasons for our failures—and surely they are numerous. In trying to examine these questions, I am obliged to indicate how much I agree, and disagree, with Dallin; but my main purpose in these comments is to pursue a somewhat divergent line of analysis which his article has stimulated.

Dallin emphasizes, quite rightly, that what is at stake is prediction of possible outcomes—of alternative futures for the Soviet system. He regards Sovietology as unnecessarily restrictive in its stipulation of the range of possible outcomes, or "plausible futures." My own impression is different: I regard the major service of Soviet specialists to be restriction or elimination of outcomes which generalists—whether scholars or practitioners—are tempted to consider likely. Let me cite a single instance. In September 1957 Allen W. Dulles, then CIA director, was quoted as believing that "Nikita S. Khrushchev is sitting on a powder keg, with military dictatorship a possibility for the Soviet Union."[1] On the face of it, this phraseology merely suggested military dictatorship as a "possibility," and one can readily grant that in this world everything is possible. The context of Dulles's remarks to the San Francisco Advertising Council, indeed the very extraordinary nature of a CIA director's making such a prognosis at all, strongly suggested that he was stipulating a plausible, if not a probable, outcome. But the fact is that at that time no Sovietologist whom I know considered military dictatorship a plausible proximate development in the Soviet system. I scarcely need to recall that just a month later Dulles's prognosis—wherever he got it—proved to be abysmally incorrect when Khrushchev, apparently with little effort and certainly with

1. "Military Regime for Soviet Seen," *New York Times*, Sept. 22, 1957.

I am grateful to the many commentators at the American Association for the Advancement of Slavic Studies symposium, New York, April 18, 1973, for their comments on Professor Dallin's stimulating paper and on my own reflections on his theme.

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the cooperation of many military leaders, ignominiously ousted Defense Minister Marshal Georgii Zhukov. In this instance, at least, the firmness of Sovietologists in *restricting* outcomes was fully justified.

Dallin's critique is not primarily concerned with Sovietologists' excessive restriction of outcomes in general, however; rather he contends that outcomes tend to be restricted in a single direction—toward "hard" or pessimistic interpretations of the Soviet future. In view of Dallin's experience in governmental as well as private analysis of Soviet affairs, which is almost unexcelled among the present generation of Sovietologists, this contention deserves very serious consideration. For what it is worth, my own conclusions are different. I see at least as many failures on the "soft" side: failure to predict the Cuban missile emplacement; failure to predict re-Stalinization after 1964; conviction that "bridge-building" could proceed without provoking counteraction like the Czechoslovak invasion. However, failing a rigorous definition of categories and laborious examination of hundreds of publications, both our conclusions can only be impressionistic.

Even if it were possible to supplement published predictions by confidential private predictions and government documents, I seriously doubt that the results of the examination would be commensurate with the effort required. No matter what proportion of Sovietological predictions turned out to be faulty, no matter how many Sovietologists were revealed as "hard liners," we should still have no standard by which to determine whether this record was better or worse than comparable social investigations. When one considers the fluctuation in demographic predictions of such well-defined and quantifiable topics as the American birth rate, the rapid succession of contradictory experiments confidently launched to broaden minority educational opportunities, or the sharp divergences among political scientists on the significance of the contemporary "youth movement," one is left with the impression that our colleagues in American studies are equally weak in predictive ability. Such an observation is no reason for complacency, much less *Schadefreude*; but it does suggest the inherent difficulty of using records of successful prediction as a way of measuring the success of our area field.

There is another way, which Dallin suggests but which other critics have pursued more emphatically: examination of the underlying assumptions of Sovietology. To such critics, excessive attachment to the totalitarian model indicates a basic weakness derived from the cold war environment which produced "hard-line" biases. To me this criticism appears weaker than the assessment of the record of predictions; but it opens a more fruitful field for exploration. Without, for the moment, questioning the undue influence of the totalitarian model, I see three kinds of evidence that this attachment was not

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primarily an artifact of the cold war. The simplest is derived from a consideration of the writers who employed the concept in the 1940s and early 1950s. Far from being the peculiar device of American Sovietologists, "totalitarianism" was almost universally employed by writers (outside the Soviet bloc) dealing with communism. Many of these writers in Western Europe were not only uncommitted to American assumptions or interests but were harshly critical of them. To cite just two examples, Isaac Deutscher wrote (in the 1949 edition of *Stalin*) that Stalin and Hitler each "built up the machine of a totalitarian state and subjected his people to its constant, relentless pressure."[2] Two years later (in the original French edition of *Political Parties*) Maurice Duverger made Soviet totalitarianism and its control of the French Communist Party a major analytic category.[3]

Consideration of the intellectual origins of the concept of totalitarianism also casts doubt on its cold war parentage. As the passage by Deutscher cited above suggests, resemblances between Hitler's and Stalin's regimes struck even observers who were optimistic about the more distant Soviet future. Enormous effort had been devoted to analysis of the Nazi regime; economization of intellectual resources alone would have led writers to apply this analytic framework to Stalin's obviously similar system.

A third reason for not accepting the cold war affiliation of the totalitarian concept is that failure of Sovietology to employ other concepts can be explained without resort to any such special thesis. General social science models were by no means readily available in the 1940s and early 1950s, when the emphasis on the totalitarian model was strongest. Political science and contemporary history had hardly been affected by the structural functionalist or related developmental theories. Communications theory was hardly significant before Karl Deutsch's *Nationalism and Social Communication* appeared.[4] These and other concepts were, to be sure, available to sociologists, as Alex Inkeles's and Barrington Moore's penetrating analyses of the early 1950s indicate.[5] As anyone involved in directing Soviet studies knows, however, the Harvard school of first-rate sociologists specializing in Sovietology could always be counted on one's fingers. The reasons for this situation are compli-

2. Isaac Deutscher, *Stalin* (London, 1949), p. 566.

3. Maurice Duverger, *Les partis politiques* (Paris, 1951), pp. 286 ff.

4. Karl W. Deutsch, *Nationalism and Social Communication* (Cambridge, Mass., 1953).

5. Alex Inkeles, *Public Opinion in Soviet Russia* (Cambridge, Mass., 1950); Barrington Moore, Jr., *Soviet Politics: The Dilemma of Power* (Cambridge, Mass., 1950); Barrington Moore, Jr., *Terror and Progress USSR* (Cambridge, Mass., 1954); Alex Inkeles and Raymond A. Bauer, *The Soviet Citizen* (Cambridge, Mass., 1959). I believe that the last work, the principal outgrowth of the Harvard Interview Project, had been conceptualized some years earlier.

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cated, but surely they relate primarily to the internal dynamics of the sociology discipline, with its increasing emphasis on intensive theoretical and methodological training. Close scrutiny of the nature of the totalitarian concept itself also makes one aware of the complex, often contradictory influences at work.

Undoubtedly a major reason for the widespread belief in the excessive influence of the totalitarian model is confusion between the very general way in which this concept has been understood by most Sovietologists and the elaborated models presented by Hannah Arendt and Carl J. Friedrich–Zbigniew Brzezinski.[6] Both models emphasized the mass terror and personal dictatorship elements of the Nazi and Stalinist regimes. More important, they contributed to the impression that these regimes were *monolithic*, as contrasted to the conflict over policy outputs in pluralist democracies.

In retrospect, it is rather difficult to understand how models which relied primarily on Nazi experience to analyze the USSR could have emphasized monolithism, for as early as 1947 Hugh Trevor-Roper's *The Last Days of Hitler* (London, 1947) had dramatically revealed the disunity and conflict which characterized the Nazi regime. In fact many—I should think a large majority—of Sovietologists in the 1950s rejected the monolithic model in favor of a conflict model of the Soviet system. This view—apparent in Barrington Moore's books and in Merle Fainsod's *How Russia Is Ruled* (Cambridge, Mass., 1953)—considers totalitarianism primarily as the *intention* of the Soviet regime, the desire to make a "new man" by total political manipulation of the social environment. The degree to which this ideological aim could be implemented was, as Moore's and Fainsod's books indicate, a very complex matter for empirical investigation. There was no general consensus among Sovietologists as to the probable outcome of efforts to carry out the totalitarian objective, although there was fairly widespread agreement that the ideology would continue to exert strong pressure in this direction. One result of this very loose use of the totalitarian concept was to allow room for broad divergences among Sovietologists which led to vigorous presentation of alternative outcomes for the Soviet system. A major example was the protracted controversy during the late 1950s between those, like Myron Rush, who believed that the structural and ideological pressures for one-man rule would lead Khrushchev to repeat Stalin's pattern, and those who considered oligarchy at least as likely an outcome.[7] Since reversion to a Stalinist personal

6. Hannah Arendt, *The Origins of Totalitarianism* (New York, 1951), esp. chap. 13; Carl J. Friedrich and Zbigniew K. Brzezinski, *Totalitarian Dictatorship and Autocracy* (Cambridge, Mass., 1956).

7. Myron Rush, *The Rise of Khrushchev* (Washington, 1958); John A. Armstrong, "Toward Personal Dictatorship or Oligarchy? Soviet Domestic Politics Since the Twentieth Congress," *Midwest Journal of Political Science*, 2 (1958): 345–56.

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dictatorship was, I suppose, the more "hard-line" prediction, the fact that some of us persisted in the opposite forecast might be taken to suggest that cold war pressures were not overwhelming. In fact, those of us on both sides of the dictatorship-versus-oligarchy controversy pursued our analyses with, as far as I am aware, complete disregard for immediate American political implications.

Current Sovietologists prefer to substitute terms like "extreme social control" for "totalitarianism." Given the variety of meanings attached to the latter term, its replacement is perhaps desirable, but the concept of a peculiar ideological drive for remaking human nature is hardly dispensable, if only as a starting point for investigating its declining force in the Soviet system.

Much more important for conceptual progress in Sovietology is the question whether totalitarianism ever constituted *the* paradigm of the field. Those observers who consider that totalitarianism did constitute a paradigm usually have referred to the Arendt-Friedrich-Brzezinski model, which, if it had indeed been generally accepted, was sufficiently elaborate to constitute a paradigmatic framework for Sovietologists. The survey I have just presented suggests that totalitarianism *in the looser, more generally accepted form,* could not constitute a paradigm in the sense that Thomas S. Kuhn uses the term in his *Structure of Scientific Revolutions.*[8] In fact, Sovietology was predominantly influenced by a different paradigm, or (to use Kuhn's later term) "disciplinary matrix." This matrix was *historicism,* in the restrictive sense in which the term was used by Friedrich Meinecke.[9] In part this matrix derives from Ranke's dictum that the historian should describe things "not as they are, but as they really were." Recently the position has been succinctly described by David A. Hollinger: "Insofar as historians have produced a body of knowledge that 'works' to the satisfaction of everyone who cares, it consists largely of the semiautonomous, name-and-date 'facts' that take up the pages of standard biographies."[10] Hollinger immediately points out that this search for the isolated fact is "only incidental to the questions historians try the hardest to answer." Undoubtedly this is true of the more analytic historians in the Russian and other areas; but it seems to me that fact-collecting, as contrasted to conscious use of conceptual frameworks, has been especially strong in Soviet historiography. To a considerable extent this concern for facts was a necessary

8. Thomas S. Kuhn, *Structure of Scientific Revolutions,* 2nd ed. (Chicago, 1970), p. 182.

9. See the discussion of variant meanings of "historicism" in Eugene F. Miller, "Positivism, Historicism, and Political Inquiry," *American Political Science Review,* 66 (1972): 797.

10. David A. Hollinger, "T. S. Kuhn's Theory of Science and Its Implications for History," *American Historical Review,* 78 (1973): 382.

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reaction to glib generalizations about the USSR current in the 1940s. All of us have profited by the severe discipline of the historicist approach. At the same time, historicism has had a strong, lasting influence on most Sovietologists' conceptions of what is important and permissible as an object of study. The implicit assumption, related to our stress on philology and language, that one can interpret a society only by immersion in its culture to the exclusion of all other concerns reinforces the assumptions of historicism. It is scarcely an accident that we have a "Slavic Studies" association and a "*Slavic Review*" but not (in the United States) a "Soviet Studies" association or a "*Soviet Review*."

Hardly less important, in the disciplinary matrix of Sovietology, has been the "quotation mongering" which Alexander Dallin rightly criticizes. The *immanent* approach to Soviet ideology, while advantageous for the mature scholar, inevitably leads the unwary student toward an isolated approach to the Soviet system in general.[11] The defect here is acceptance of Leninist doctrines as a subject of exegesis rather than as data to be incorporated in the analyst's own conceptual framework, whether that framework ultimately results in a negative or positive evaluation of the Soviet system. Like historicism, doctrinal exegesis has led many Sovietologists to regard their subject as a kind of hidden garden, not susceptible to fertilization by the methodologies and conceptualizations developed by the generalizing social sciences.

The disciplinary matrix in which Sovietology developed also produced an excessive Russocentric emphasis. Americans in Slavic studies owe an incalculable debt to eminent Russian émigrés who developed the field during the interwar period. Apart from a few economists, these men, like many of their American and West European contemporaries, were imbued with historicist assumptions. As Russians, it was quite natural for the émigré scholars to emphasize the necessity for students of Soviet affairs to absorb Russian culture in order to interpret Soviet development. For many great émigré scholars, like Michael Karpovich, the Soviet regime was an alien intrusion into the "true" course of Russian history: "The foreign policy of the Tsars was the customary policy of a national state. . . . The pre-Communist Russian intelligentsia was predominantly anti-imperialistic, and it is hard to imagine an imperialist spirit among the popular masses. This heritage quite probably has immunized the Russian people against Communist messianism."[12] Such emphasis on exclu-

11. See, for example, Herbert Marcuse's *Soviet Marxism* (New York, 1958). In his more recent semi-Freudian works Marcuse departs from this dominantly immanent approach.

12. "Russian Imperialism or Communist Aggression?" *New Leader*, June 4, 11, 1951, reprinted in Robert A. Goldwin, ed., *Readings in Russian Foreign Policy* (New York, 1959), pp. 662, 666.

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sively Russian cultural aspects tacitly depreciates the significance of the non-Russian elements in the USSR; occasionally émigré scholars lacking Karpovich's serenity vehemently opposed the beginnings of study, in the United States, of the non-Russian Soviet nationalities. Conversely, of course, the vehemence of representatives of non-Russian ethnic groups often produced reactions which precluded serious consideration of their importance. The net effect, until very recently, has been to exclude adequate consideration of alternative futures for the USSR such as the dissolution of the multinational complex envisaged by Andrei Amalrik. Even in terms of analysis of the recent past, the Russocentric standpoint has hampered consideration of the USSR as what Vernon Aspaturian calls an "arrested world state."[13]

In my view, the complex problems associated with the historicist disciplinary matrix are quite sufficient to explain why Sovietology has been so reluctant to enter the world of generalizing sociological realism.[14] Possibly, in some obscure way, the political climate of Western societies in the last three decades has influenced this reluctance; but it is at least as plausible that the general turn of the social sciences, during the 1950s and early 1960s, away from "relevant" social problems toward abstract theoretical and methodological considerations was due to fundamental social pressures. The plain fact is that the sociology of knowledge is insufficiently developed to permit one to draw firm conclusions about such underlying social pressures. We, as Sovietologists, are singularly ill-equipped to pursue the analysis.

What one can do, I think, is endeavor to see how the lag in relating generalizing social science to Sovietology can be profitably overcome. As I suggested earlier, some observers who regarded the elaborated totalitarian model as the paradigm of Sovietology (or comparative Communist studies) consider that it must be replaced by a new, single unifying paradigm. This position has been most forcefully presented by the Sinological political scientist, Chalmers Johnson: "In the absence of an informing paradigm that unites the elements of social action, writing on the subject of revolution has come closer to the genres of literature and belles lettres than to social sciences."[15] Indeed, Johnson sees a homologous relation between Kuhn's theory of knowl-

13. Andrei Amalrik, *Will the Soviet Union Survive Until 1984?* (New York, 1970); Vernon V. Aspaturian, *The Soviet Union in the World Communist System* (Stanford, 1966).

14. Following such writers as Roland Roberts, "The Sociocultural Implications of Sociology," in T. J. Rossiter et al., *Imagination and Precision in the Social Sciences: Essays in Memory of Peter Nettl* (London, 1972), p. 88, I prefer "sociological realism" to "behavioralism," which (like "totalitarianism") has unfortunately acquired an excessive polemical baggage.

15. Chalmers Johnson, *Revolutionary Change* (London, 1968), p. vii.

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edge and the general evolution of societal values: "Thomas Kuhn's 'paradigms' are an almost perfect counterpart of values in society at large."[16]

Regardless of the merits of the particular paradigm Johnson advances,[17] the notion of a single paradigm appears to me to be highly inappropriate for a field like Sovietology. It is worth examining the reasons for rejecting this notion in some detail, for they illuminate aspects of Sovietology intimately related to Alexander Dallin's critique as well as to my own remarks above. One major reason is the time element. In Kuhn's original thesis, the essential "paradigm succession" in the natural sciences required generations, if not centuries. Thus the establishment of the paradigm contained in Newtonian physics required an entire century, and the Newtonian paradigm was not succeeded by Einstein's paradigm for two more centuries.[18] As two of Kuhn's recent critics write, "The practice of science is not the constant provision of new paradigms whenever a scientist is dissatisfied with established ones, but rather a slow process of digesting, rationally evaluating and testing the ramifications of given theories, a process which is broken up by revolutions only every few centuries, and not in every issue of a learned journal."[19] In other words, it is hard to see how, even if Kuhn's analysis is generally valid, it can be relevant to a field like Sovietology, which is scarcely a quarter-century old. Attempts to apply the paradigm-succession theory to Sovietology and to comparative communism resemble the dubious efforts to predict the imminent end of ideological fervor among the Soviet elite by analogy to centuries-long processes of declining religious fervor in Islam or post-Reformation Christianity.

For quite different reasons it is highly doubtful that Kuhn would regard such a transitory, loose concept as "totalitarianism" as a paradigm requiring supersession. To Kuhn the social scientist, like the engineer, is fundamentally different from natural scientists. The latter "need not choose problems because

16. Ibid., p. 22.

17. While his approach is complex ("a synthesis of the so-called 'coercion' and 'value' theories of society," ibid., p. vi), it is apparent that Johnson basically presents a structural functionalist theory of development: "Taking a cue from the biological sciences, social science has attempted to overcome this dilemma [lack of nomological laws or historical laws] by reintroducing a modified form of teleological reasoning—namely the logic of 'functionalism.' Using the concept of function, we can talk about the purpose of a system even when we do not know, or doubt, that it has a conscious purpose" (ibid., p. 46). For my substantive critique of this type of development theory see "Communist Political Systems as Vehicles for Modernization," in Monte Palmer and Larry Stern, eds., *Political Development in Changing Societies* (New York, 1971), pp. 127–58.

18. Kuhn, *Structure of Scientific Revolutions*, pp. 33 ff.

19. David Braybrooke and Alexander Rosenberg, "Comment: Getting the War News Straight: The Actual Situation in the Philosophy of Science," *American Political Science Review*, 66 (1972): 824.

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they urgently need solution and without regard for the tools available to solve them," but primarily because the problems are challenges to the scientists' urge to solve puzzles. Social scientists and technologists, on the contrary, "defend their choice of a research problem . . . chiefly in terms of the social importance of achieving a solution."[20] As indicated earlier, a distinctive feature of the last two decades in the generalizing social sciences has been the shift from direct concern with social importance to methodological and conceptual emphases. Leaving aside the reasons for and merits of this shift, it has unquestionably been less significant in fields like Sovietology. Insofar as Sovietology has remained more directly involved with questions of social importance, the field may have been, as Dallin suggests, more susceptible to influence by the climate of the times. I do not believe, though, that this susceptibility produced excessive pressures in any particular direction, nor has concern for relevance necessarily been harmful. As I pointed out earlier we (at least as Sovietologists) simply lack the tools to determine whether continuing Sovietological concern for matters of current social importance, as contrasted to the withdrawal of generalizing social scientists to consideration of "deeper" issues, has been beneficial. Quite possibly both emphases will, in their own ways, ultimately enhance our ability to solve social problems. What is important now is to draw together the divergent paths of development of area fields like Sovietology and the generalizing social sciences. But this can be done only by recognizing that Sovietology is not only a branch of the social sciences, but to a considerable degree resembles a technology. As a technology Sovietology—as Kuhn suggests—is far from the stage where a single paradigm is useful.

Perception of Sovietology as a technology also illuminates an aspect of Alexander Dallin's critique which at first glance, I must admit, appeared confused. Who, precisely, are the Sovietologists who (in his view) have committed so many errors? At one point he implies that they are scholars, in other places he appears to extend his critique to policy practitioners. On reflection, it seems to me that such ambiguity concerning field membership is wholly justified in dealing with a partially technological field like Sovietology. That is to say, whether Sovietologists are formally scholars, full-time diplomats, policy advisers, or members of the large intermediate category exemplified by the RAND Corporation, all ought to be concerned with relatively short-term application of broader social science concepts to specific issues of social concern.

The conception of Sovietology as a technology implies that it should use

20. Kuhn, *Structure of Scientific Revolutions*, p. 164.

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not a *single* paradigm derived from the generalizing social sciences, but a variety of models relevant to its particular concerns. I think that this is what Alex Inkeles had in mind when he wrote: "There are those [models] which are more appropriate to one time or place than another. All have a piece of the truth, but it is rare that any *one* model is really adequate to the analysis of a richly concrete historical case."[21] Kuhn implies much the same thing when he points out that even natural scientists, in a crisis where no paradigm is available, will develop several alternative theories which can be easily abandoned.[22] What I am advocating, therefore, is that Sovietologists draw freely on what appear to be the best and most relevant social science conceptualizations, while keeping in mind that these frameworks are expendable *if* they are transcended by the parent social sciences *or if* they prove to be useless for specifically Soviet problems. At the same time, appropriate application of concepts to Sovietology will have an invaluable feedback to the generalizing social sciences. The importance of what Inkeles justly terms the "Soviet experiment" can hardly be exaggerated. Any social science which neglects this experience is unavoidably parochial, yet few generalizing social scientists are equipped to evaluate it. Consequently, even if Sovietology could progress without extensive application of general social science frameworks, I believe the area field would be derelict in its duty as a branch of knowledge if it failed to relate its problems to broader analytic concerns.

This position has two corollaries. Instead of advancing ad hoc theories, Sovietologists should apply, whenever feasible, conceptual frameworks derived from the generalizing social sciences. While the paucity of relevant social science frameworks in the 1940s made adherence to the totalitarian model (even in its elaborated Arendt-Friedrich-Brzezinski form) understandable, the really basic objection to this model is that it was an ad hoc conceptual framework. More recent efforts by Sovietologists to advance sweeping ad hoc theoretical frameworks are far less excusable. In a way, the tendency to invent catchy terms and concepts is the scholarly counterpart of what Bertram D. Wolfe trenchantly criticized as the pernicious journalistic search for novelty in comment on the USSR.[23]

The second corollary is that ordinarily the most fruitful application of social science models to Sovietology will take place in monographic research. This is what I had in mind fifteen years ago when I wrote, "There is no reason

21. Alex Inkeles, "Models and Issues in the Analysis of Soviet Society," *Survey*, no. 60 (1966), p. 3.

22. Kuhn, *Structure of Scientific Revolutions*, p. 87. Hollinger, in the article cited earlier, advances essentially the same argument in relation to the "proto-science" of history.

23. Bertram D. Wolfe, *Communist Totalitarianism* (Boston, 1956), pp. 25–26.

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why monographic research, if properly conceived and reported, may not both contribute to the advancement of the body of knowledge on the Soviet area and be relevant to the political science discipline. Monographs on such subjects as composition of the elite, local administration, and ties between the U.S.S.R. and foreign Communists, if properly related to broader problems in the field, should be of great interest to professional political scientists."[24]

Some researchers, like Jeremy Azrael in his remarkable analysis of *Managerial Power and Soviet Politics* (Cambridge, Mass., 1966), have the ability to combine several conceptual approaches expertly in their monographic investigation, with gratifying feedback results for the parent social science as well as for Sovietology. Others—myself included—may wish to turn to work in the generalizing social sciences after a long period of monographic research in Sovietology. We will, I hope, find our training in Sovietology a good preparation for this broader effort; but let us be aware that whatever contribution we may make to broader conceptual frameworks it is only peripherally related to Sovietology as such.

24. "Political Science," in Harold H. Fisher, ed., *American Research on Russia* (Bloomington, 1959), p. 65.

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