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The Social Interpretation of Foreign Policy

Marc Trachtenberg

That foreign policy is a social product, that it is in particular the outcome of a political process and an element in the national political system—these are general notions that few scholars would in principle dispute. But in practice foreign policy has been commonly portrayed in exactly the opposite way. It is viewed as something with its own life, essentially cut off from domestic politics, existing primarily in the international sphere. Scholars do of course recognize that domestic factors play a role, but the implied connection is often extremely amorphous and general, with no attempt on the whole made to specify the mode of linkage in a concrete and testable way. Thus America's return to isolation after the First World War is commonly attributed to the unwillingness of her people to bear the burden of world power; British and French foreign policy after 1924 is explained in terms of the deep-seated pacifism of the masses. The actual mechanism of linkage is not spelled out. On those occasions when a specific relation is described the very language used often betrays an unwillingness to see it as a legitimate, integral part of a normal political process. Such things as public opinion and party and interest-group politics are seen basically as exogenous forces, "intruding" or having an "impact" on the policy-making system, not as regular parts of it.

Views of this sort are the projection into the sphere of descriptive political analysis of the values of a curious mélange of two somewhat inconsistent theories: normative democratic theory and normative diplomatic theory. The first takes a somewhat idealized notion of the way things should function in a democracy and uses it to explain the way Western political systems actually work. The second, which has an elitist bent, can be summed up as a set of propositions: national interest is an objectively definable function of the international conjuncture; statesmen by virtue of their professional training and experience are best able to perceive it and prescribe policy in conformity with it; therefore, anything that interferes with their activity deforms the system and perverts its goals.

The effect of the first theory is to obviate the need for examining how a society generates foreign policy: it is simply assumed that in a democracy public attitudes are automatically transformed into policy. The second theory accepts this assumption, but gives it a negative twist: when things go wrong, it is the intrusion of the public that is to blame. The synthesis of the two satisfies everyone. The power of the people is recognized, and the failures of statecraft are excused.

The problems with the normative approach are obvious. We would like to know the extent to which social forces shape foreign policy; we would like to know what the policy-making process, viewed in the broadest perspective, looks like. It is this sort of question that the normative approach tends to short-circuit. What we would like in other words is an empirically based descriptive theory of foreign policy making.

Historians interested in such questions turn naturally to the political science literature. How much light does this field of scholarship shed on these issues? Does it provide the kind of solid descriptive theory historians need? Historians themselves, increasingly concerned with the social sources of foreign policy, have come up with a number of homegrown theories. How valid and how useful are these approaches? How in general is the whole problem of foreign policy as a social product to be studied?

These are the questions that I am concerned with here. They will be dealt with indirectly by means of a critical examination of a handful of works by political scientists and historians. The problems are difficult and criticism is easy. I hope no one takes it as a sign of disrespect. For it is not the shortcomings of individual scholars that I wish to consider, but rather the problems inherent in the different approaches to the subject. The aim is that in so doing we can get some idea of what the proper orientation of scholarship in the area should be: which approaches are productive and which are sterile.

How does a society generate a foreign policy? The very question indicates that the fundamental analytical problem is the problem of linkage—that is, the causal relation between social phenomena and policy. This is of course part of the larger problem of explaining how political systems, and in particular democratic systems, function—presumably one of the central problems of political theory. It is therefore interesting to note that in the view

of leading scholars in the field not much is known about the problem of linkage. In 1967 James Rosenau, for example, regretted the lack of attention paid to the subject: "How the various sources of foreign policy combine to produce various forms of behavior under various kinds of conditions is neither the subject of extensive research nor the focus of systematic theorizing."¹ On the basic question of the rôle of public opinion, our ignorance of the mechanism of linkage is particularly striking. Referring to "the assumption that public opinion plays a role in the policy making process," the historian Melvin Small wrote in 1970 that "no studies *prove* this traditional piece of folklore."² The same point had been made by Harwood Childs in 1965.³ And V.O. Key, in an important general work on public opinion in America, argued along essentially the same lines. He ridiculed the notion of public opinion "as a mysterious vapor that emanated from the undifferentiated citizenry and in some way or another enveloped the apparatus of government to bring it into conformity with the public will." It was clear to him that public opinion did play a role, but it was very difficult to specify precisely just how the process of linkage worked. Given the limited data available, moreover, the analyst had to resort to informed speculation: "a certain amount of surmise must substitute for hard knowledge."⁴

A curious thing about Key and Childs is that in spite of their sensitivity to the problem, they still allowed themselves to make the kind of unsupported assertions symptomatic of the failure of scholarship in this area. "As leader of the non-Communist world," Childs wrote, "the United States finds it imperative to do all that it can to win public support for its policies both at home and abroad"; and Key said: "of the capacity of mass opinion to bring party to its service in the long run there can be little doubt."⁵ This is precisely the kind of thing scholars should be trying to get away from. It is a measure of how ingrained and pervasive such un-

¹ James Rosenau, ed., *Domestic Sources of Foreign Policy* (New York, 1967), p. 3.

² Melvin Small, "Historians Look at Public Opinion," in *Public Opinion and Historians: Interdisciplinary Perspectives*, ed. M. Small (Detroit, 1970), p. 14.

³ Harwood Childs, *Public Opinion: Nature, Formation and Role* (Princeton, 1965), pp. 309-310.

⁴ V. O. Key, *Public Opinion and American Democracy* (New York, 1967), pp. 536, 409-411, 431.

⁵ Childs, p. 293, and Key, p. 494.

supported and largely untestable notions are in the literature that even those scholars with a considerable degree of sophistication in the area feel free to write in this way.

Bernard Cohen's book *The Public's Impact on Foreign Policy* is a pioneering attempt to come to grips with the problem. To Cohen, amorphous claims about the influence of public opinion are characteristic of a "general intellectual failure, a failure of political conceptualization or theoretical insight on a grand scale." A critique of the existing literature from this point of view served as the point of departure for Cohen's own work on the subject. A review of the literature enabled him to clear the ground—it showed that "we know next to nothing about the subject"—but it also implicitly generated standards of rigor for scholarly analysis. It is with regard to those standards that Cohen's own work should be judged.⁶

Cohen criticized the existing body of scholarship for its inadequate treatment of the fundamental question of linkage. The notion, for example, of officials "taking opinion into account" was too vague to be of any use.⁷ Concepts like the "limits" on policy makers, not subject "to precise observation, identification or measurement," were really "meaningless." And in general amorphous assertions about the impact of public opinion on foreign policy, which could "neither be proved nor disproved" were also "fundamentally meaningless."⁸

Given Cohen's insistence on these principles of rigor, the degree to which he ignored these precepts at the end of his book is striking. To characterize the linkage between foreign policy and public opinion, he used the word "responsiveness," which "refers to the conditions and the mechanisms whereby public preferences may get considered and possibly embodied in public policy."⁹ "Responsiveness" exists not because policy makers are embedded in a political system—electoral accountability according to Cohen has a negligible effect on policy making—but rather, at least in part, because the machinery of democratic control is so weak: officials "intuitively understand" that "there is no other way short of violent upset in which public preferences can be brought into the foreign policy

⁶ Bernard C. Cohen, *The Public's Impact on Foreign Policy* (Boston, 1973), chap. 1. Quotation on p. 19.

⁷ *Ibid.*, p. 13.

⁸ *Ibid.*, pp. 15, 18-19.

⁹ *Ibid.*, p. 187.

area." The extremely vague notion of "intuition" is thus seen as central and, as Cohen is aware, he has returned to the very notion of "taking public opinion into account" that he had criticized so sharply at the outset.¹⁰ His original assumption that impact is measured by the conscious perceptions of policy makers—an assumption which methodologically was the point of departure for the study—is thus abandoned in the end. Unconscious mechanisms, processes "operating in the recesses of the minds of officials," are seen both as crucial and as largely unknowable.¹¹

Moreover, his earlier conception of foreign policy as the outcome of a political process gets abandoned.¹² In Cohen's *Public's Impact on Foreign Policy* the assumption is that the State Department makes policy on its own; the political authority—that is, the president—has little control over the bureaucracy.¹³ The political role of Congress is admitted in passing, but this function is kept distinct analytically from Congress's role as a transmitter of public opinion.¹⁴ The idea that opinion transmitted in this way plays a direct political role by virtue of Congress's power in the policy-making process is thus circumvented; the notion that loose, essentially subjective modes of linkage are the only real connective tissue remains intact. The very title of the book is indicative of the assumption that policy making is the domain of "the foreign policy establishment" in the executive branch: the public is seen as having an "impact" on the policy making process from the outside, and not as being an integral part of the process.¹⁵ It would not occur to Cohen to speak of the State Department's "impact" on foreign policy, precisely because it is assumed on a priori grounds that the bureaucracy is an integral part of the policy-making process.

How are these problems with Cohen's work to be understood? Cohen is a scholar of considerable intelligence and perception; the problems with his book are not to be explained by any personal shortcomings on his part, but rather by certain defects in the intellectual tradition in which his work is embedded. For it is clear that Cohen's conclusions are in large measure an artifact of the entire

¹⁰ *Ibid.*, pp. 187-188.

¹¹ *Ibid.*, p. 197.

¹² Bernard C. Cohen, "The Relationship between Public Opinion and Foreign Policy Maker," in Small, p. 77.

¹³ Cohen, *Public's Impact*, p. 185.

¹⁴ *Ibid.*, p. 113.

¹⁵ *Ibid.*, p. 145.

conceptual framework, theory and method, that he starts out with—a framework not unique to him but common in contemporary political science.

Take for example his reliance on survey technique. His conclusions were drawn primarily from a set of interviews with State Department officials. To get at the question of linkage, Cohen set out to discover policy makers' perceptions of the external environment.¹⁶ He did this by asking officials how public opinion affected their behavior. It turned out (as might have been predicted) that the "most widely expressed view" was that "foreign policy officials can ignore or discount public opinion that is opposed to them."¹⁷ It is on the basis of this kind of evidence that he concludes that opinion "may not often be an important direct influence or constraint on policy."¹⁸

This mode of inference, however, is clearly vulnerable to criticism. It is true that to understand "impact," one must first understand how officials perceive opinion.¹⁹ But studying how officials, in interviews with an outsider, characterize in a general way the *impact* of opinion (rather than opinion itself), is no way of getting at the question. One simply cannot accept the claims of officials as fact; their claims that they are above party and concerned uniquely with the national interest are not the considered opinions of disinterested observers, but are rather part of the formal ideology and self-image of their profession, to be invoked in particular when questioned by an outsider in a formal interview. We know today, for example, after Watergate that Richard Nixon's ostentatious display of indifference to large antiwar demonstrations in Washington by no means reflected his real reaction. But who really believed White House professions of indifference at the time?

At points Cohen appears somewhat uncomfortable with the conclusions yielded by his method. The officials claimed that the "national interest" took priority over business interests; Cohen says, "It looks, I admit, like a one-sided and institutionally self-serving picture that argues the legitimacy of economic representations but

¹⁶ *Ibid.*, pp. 2-8, 26.

¹⁷ *Ibid.*, pp. 155-156.

¹⁸ *Ibid.*, p. 161.

¹⁹ This was in fact Cohen's original methodological orientation, explicitly borrowed from Lee Benson. See Cohen, *Public's Impact*, pp. 2-8, and Benson, "An Approach to the Scientific Study of Past Public Opinions," *Public Opinion Quarterly*, 31 (Winter 1967), 522-567.

the primacy of the public interest in responding to them.”²⁰ Nevertheless he cannot quite bring himself to abandon his basic assumption of a one-to-one correspondence between the officials’ self-image and their actual behavior; his investment in the interview method is too great. As a result, he tends to dismiss evidence that cuts in the opposite direction. One official, for example, argued that contact with the outside world through informal channels had a real effect on policy making: “I was recently at a Bar Mitzvah in — where I ended up arguing about Vietnam with everyone there. I was astounded at the unanimity of the criticism directed at our policy. All of this gets brought into our discussions here; we are not hermetically sealed.” This seems like a plausible proposition, but Cohen dismisses it by applying a criterion that he does not apply in the case of views that he agrees with: “But in the absence of information specifying policy impact, one is entitled to skepticism or at least to suspended judgment.”²¹

More fundamental perhaps is the role that political theory played in shaping Cohen’s conclusions. The weak linkage between public opinion and policy making is explained in the context of a larger body of contemporary political thought. Is a linkage between public opinion and policy making to be presumed? The traditional answer was yes, because the people through the electoral process are supposedly able to control the behavior of their leaders. But Cohen, drawing on a more recent tradition in political science, rejects this doctrine: “reality, we are learning, does not conform very closely to this description out of normative democratic theory. Democratic control of foreign policy—indeed of any particular area of public policy—by means of electoral accountability functions weakly when it even functions at all.”²²

It is I think not assuming too much to see an elitist theory of politics as logically prior to Cohen’s whole argument. Clearly the absence of a “public impact” in the sense of an objective causal link is not proved—it is essentially presumed without adequate evidence and without impartial testing, just as the idea that State Department officials make (rather than just administer) policy is taken for granted.

The adoption of this theory—in particular, the presumption about the autonomy of the “foreign policy establishment”—is in-

²⁰ Cohen, *Public’s Impact*, p. 103.

²¹ *Ibid.*, p. 154.

²² *Ibid.*, p. 185.

consistent with the notion that policy making is a broad-based political process. This latter approach involves the presumption that public opinion necessarily influences policy. The substitution of an elitist theory, by changing the operative presumption, displaces the burden of proof: unless hard evidence proves in a concrete way the importance of opinion, its role can be assumed to be minimal. Thus the underlying theory, by determining where the burden of proof lies, necessarily plays a dominant role in shaping the conclusions finally drawn.

These features of Cohen's argument are by no means idiosyncratic. To a historian, in fact, one of the most surprising things about the political science literature is the degree to which a number of leading scholars in the field avoid conceptualizing in broad-based political terms. Raymond Bauer and his associates, for example, in their important study of the politics of foreign trade, clearly avoid the notion of "power," preferring instead the concept of "communication."²³ There is an evident aversion toward the idea of an *objective* structure of linkages, implicit in the notion of power: many scholars prefer to stress subjective modes of contact. Cohen's emphasis on the concept of "responsiveness" is not uncommon. In a more nuanced way, Key (who saw in the electoral nexus the basis of an objective causal relation) also stressed the subjective mode of linkage: the "legitimization of the view that the preferences of the governed shall be accorded weight by governors constitutes the moral basis of popular government, an ethical imperative that in mature democracies is converted into consistent habits and patterns of action among those in places of authority and leadership. Linkage of opinion and government occurs as governors and governed behave in ways consistent with these ethical assumptions of popular government."²⁴

How valid is the idea that the rulers and the ruled are held together by essentially subjective, and hence relatively tenuous links? How much does the electoral nexus count for as a medium of democratic control? This question is certainly central to the

²³ Raymond Bauer, Ithiel de Sola Pool and Lewis A. Dexter, *American Business and Public Policy: The Politics of Foreign Trade* (Chicago, 1963). See esp. p. 460.

²⁴ Key, *Public Opinion and American Democracy*, p. 412. See also p. 537ff. For the similar views of a historian strongly influenced by contemporary political science, see Ernest May, "An American Tradition in Foreign Policy: The Role of Public Opinion," in *Theory and Practice in American Politics*, ed. William Nelson (Chicago, 1964), pp. 117-118.

concerns of contemporary political theory, and the analysis of "issue voting" has been the focus of much research. The results are nevertheless still not in, even for the American case; one recent study pointed out that because of methodological problems "the impact of issue voting has never been adequately measured."²⁵

Those who deny the importance of the electoral nexus rely largely on an extensive body of work done on local politics.²⁶ Even putting aside the question of whether electoral accountability can be presumed to be more powerful at the national or the local level (and thus whether conclusions from community studies can be carried over to explain the national political system), the community studies themselves have been strongly and persuasively criticized on both methodological and conceptual grounds.²⁷

It is of course possible to argue that whatever role the electoral nexus plays in general, when it comes to foreign policy its role is minimal. To support this point, it is argued that few people really care about foreign policy. Thus according to the historian Ernest May, "politically alert citizens are more apt to be interested in local, state or national affairs. Surveys of different samples of the population suggest that the proportion really caring about foreign policy is around sixteen percent."²⁸ His source for this is a passage from Key's book; Key's analysis of a set of interviews in 1956 in fact indicated that for sixteen percent of the sample, foreign issues were more salient than domestic ones. On the same page, however, Key explicitly noted that relative salience is subject to dramatic change—thirty-four percent of a sample in September 1957 said "keeping the peace" was the most important problem—and the general implication of his argument in this passage is that foreign policy issues do count in determining the way many people vote, accounting in particular for Eisenhower's impressive majorities in 1952 and 1956.²⁹ It is evident, finally, that lack of salience

²⁵ Benjamin I. Page and Richard A. Brody, "Policy Voting and the Electoral Process: The Vietnam War Issue," *American Political Science Review*, 66 (1972), 981. See also their "Comment: The Assessment of Policy Voting," *American Political Science Review*, 66 (1972), 450-459.

²⁶ Cohen, *Public's Impact*, p. 186; Ernest May, "American Imperialism: A Reinterpretation," *Perspectives in American History*, 1 (1967), esp. p. 142ff.

²⁷ Most notably in Nelson Polsby, *Community Power and Political Theory* (New Haven, 1963).

²⁸ May, "American Imperialism," p. 140.

²⁹ Key, *Public Opinion and American Democracy*, pp. 172-175. For more evidence on this point, see Kenneth Waltz, *Foreign Policy and Democratic Politics: The American and British Experiences* (Boston, 1967), pp. 65-66.

in public opinion polls might be more a measure of consensus than lack of concern.

Cohen, however, approaches the argument from the other end. He stresses not the public's lack of interest, but rather the effective autonomy of the "foreign policy establishment." But these claims also do not appear very well founded. Is the bureaucracy subject to the control of the political authority? Cohen denies it, citing testimony given by Richard Neustadt before a congressional subcommittee.³⁰ Neustadt, in his testimony, outlined the conflict of interest between the president and the bureaucracy; but he did not argue, even implicitly, that the bureaucracy in large measure prevails. Neustadt in fact stressed the increasing power of the president: he is the arbiter between competing bureaucratic factions, and because of the "irreversibility" of decision-making nowadays, he "is virtually compelled to reach for information and to seek control over details of operation deep inside executive departments."³¹

One would think, moreover, that the ability of the political authority to circumvent the established bureaucracy through the creation of alternatives is evidence of political control, but Cohen makes this observation cut in the opposite direction: "The development of a foreign policy bureaucracy in the White House in recent years is itself evidence of the intractability of the State Department bureaucracy, from the president's perspective."³² Similarly, Cohen downplays the power of Congress over the bureaucracy: the McCarthy era showed "that career officials are not beyond the reach of politics, but it turns out to be caution, more than direction, that politics contributes."³³ And finally, the one elected official who does play a certain role, the president, is largely free from public control: he "finds it very easy most of the time to ignore public preferences when he cannot mobilize or neutralize them."³⁴

The net effect of Cohen's argument is to remove the bureaucracy conceptually from the larger political system. Does the evidence support this point of view? Complaint about the power

³⁰ Cohen, *Public's Impact*, p. 185.

³¹ United States Congress, Senate, Subcommittee on National Security Staffing and Operations, *Administration of National Security*, pp. 76-77, 79.

³² Cohen, *Public's Impact*, p. 185.

³³ *Ibid.*

³⁴ *Ibid.*, p. 186.

of the bureaucracy was common during the Nixon administration; and Henry Kissinger, even before assuming office, wrote an article along these lines.³⁵ But in practice it does not seem that Nixon and Kissinger were in any significant way prevented by the bureaucracy from pursuing the kind of foreign policy they wanted. In general, although the bureaucracy, being part of the policy-making community, necessarily plays a role, it does not seem that this notion of an independently powerful bureaucracy is valid: I have never come across any example of successful bureaucratic frustration of the will of the political authority on a matter of any importance.

What can be said about the tendency to downplay the linkage with public opinion via the Congress and via the president's role in the larger political system? The power of Congress over legislation, appointments, appropriation of funds, ratification of treaties, and so on is obvious, and its actual role has been analyzed and documented by a number of political scientists, historians and journalists.³⁶ As for the argument about presidential autonomy, this also seems to lose sight of the fact that the president is embedded in a political system which imposes if not as a general rule outright institutional constraints on policy making, then certainly strong incentives and disincentives for behaving in various ways. It should be noted, finally, that one of the sources Cohen gives to support his view that most of the time the president can easily ignore public preferences is Kenneth Waltz's book, *Foreign Policy and Democratic Politics: The American and British Experience*. But Waltz's book proves nothing of the sort. Waltz in fact tends to argue from the opposite set of assumptions; he simply takes it for granted that something called democratic foreign policy exists, and he is concerned with how it should be judged.³⁷

³⁵ Henry Kissinger, "Domestic Structure and Foreign Policy," in his book *American Foreign Policy* (New York, 1969). This article first appeared in the spring 1966 issue of *Daedalus*.

³⁶ See for example Key, *Public Opinion and American Democracy*, esp. pp. 481-499; James A. Robinson, *Congress and Foreign Policy-Making* (Homewood, Ill., 1962); and for a typical historical account, Robert Divine, *The Illusion of Neutrality* (Chicago, 1962). An excellent journalistic account, focusing on Senator Henry Jackson's power over arms control policy, is Elizabeth Drew, "An Argument over Survival," *The New Yorker*, 4 April 1977, esp. pp. 108-112.

³⁷ Cohen, *Public's Impact*, p. 186 and p. 209, n. 9. Cohen also cites here some work by Richard Brody, Benjamin Page and their associates on the electoral impact of the Vietnam War issue, but their conclusions do not tend to support

What are the conclusions to be drawn from all this? If it were not for the fact that it has been so widely ignored, the basic conclusion would seem trivial: it is that foreign policy making should be conceptualized as a political process in the wide social sense, that causal relations between policy makers and the rest of society exist can be inferred from the objective structure of power in a society as well as from empirical evidence. (V.O. Key has in fact worked out an important analytical framework to characterize these linkages on the basis of such inferences.)³⁸ This is not to say that "normative democratic theory," something of a strawman in any case, should be rehabilitated, but simply that elitist modes of analysis need to be considerably broadened—that policy making must be thought of as a political, but not necessarily as a democratic process.

This is the basic point; beyond that, is there much to be said from a theoretical point of view? There are of course a number of propositions of a qualitative nature that can probably be verified. For example, it is probably true that in any society the political system for the generation of foreign policy is more centralized than the corresponding system for making domestic policy. And I think it is safe to presume that the more technical a matter is, the narrower the corresponding political process is likely to be: the public "impact" will be likely to be stronger and more broadly based in what Lee Benson called the area of general "attitudes" as opposed to the area of more highly defined "opinion" on a particular issue.³⁹ (As a point of method, this implies that students of linkage should focus more on broad issues of policy than on relatively narrow technical questions; they should concentrate more on the elaboration of policy at the high political level than on the administration of policy by the bureaucracy.)

But such qualitative propositions do not go very far beyond what we know from common sense. Will a quantitative approach

Cohen: policy voting in this case was real when the voters had a real choice, and when they did not, the convergence of candidates' views can be explained in "economic" terms as the optimal response of both candidates (from a vote-getting point of view) to a given distribution of opinion. Cohen had cited two preliminary papers; a more final and concise statement of Page's and Brody's argument is their article "Policy Voting and the Electoral Process: The Vietnam War Issue," *American Political Science Review*, 66 (1972), 979-995.

³⁸ Key, *Public Opinion and American Democracy*, Parts V and VI.

³⁹ See Benson, "Scientific Study of Past Public Opinions," pp. 524-525.

yield more far-reaching results? Such a hope is implicit in J. David Singer's statement of the problem. What scholars should do, he says, is ascertain "how much of the variance [opinions and attitudes] account for in different types of cases, the degree to which that potency has been changing over time, and the extent to which they exercise a different effect in different types of national governments."⁴⁰ It is unclear how much light such an approach will shed on the problem. The practical difficulties alone seem enormous. It is also quite possible that not much can be learned: the existing evidence in fact indicates a loose overall structure of linkage; that is, patterns of linkage seem to depend more on random factors like the personality of the leaders involved than on systemic factors.

In any case, it seems that the more fruitful area to concentrate on is not theory but method. If scholars internalize a set of rules and methodological considerations, a lot of the problems with the literature can be minimized. In addition to the usual rules of the critical method, and in particular to the need to test claims by looking for counterexamples, special attention needs to be given to problems of language—not just to the language of exposition but also, if this notion is permissible, to the language of thought. The mechanistic idiom borrowed from the natural sciences (and already something of an anachronism there) needs to be replaced by a looser, more probabilistic mode of conceptualization: social forces condition rather than dictate policy. Constraints should not be thought of as hard and fast boundaries, affecting nothing until they are reached, but becoming overwhelming as soon as they are touched; instead they should be conceived as sets of incentives and disincentives, internalized into actors' structures of preference and not superimposed on top of them.⁴¹

The question of evidence is perhaps the most crucial methodological problem. To insist on extreme rigor—to demand hard documentation of linkage—is simply unrealistic. If only because evidence is often inadequate, inference based on a rather mild set of plausible assumptions is often necessary. To avoid inference of this sort in fact results in distorted interpretation—an arbitrary

⁴⁰ In Small, pp. 34-35.

⁴¹ This can be formalized in terms of the indifference curve analysis so familiar to introductory economics courses. (The theory of consumer preference can be readily expanded to a general theory of choice.) Anything that redraws the indifference curves—for example, an "intrusion" of public opinion into decision-making in foreign policy—is a "constraint" in this sense.

exclusion of legitimate causal explanations unlikely for various reasons to be reflected directly in the documentary sources. Accommodation of interest group pressure is less likely, for example, to find its way into the written, let alone the public, record, than say relatively idealistic motives. Evidence of political motivation which does exist, moreover, is not necessarily to be taken at face value. It may be convenient for a statesman, as a way of strengthening his bargaining position, to tell a foreign colleague that he is making demands in response to domestic political pressure. Or claims about political motivation may be a means of rationalizing a policy that a statesman feels uncomfortable with, but whose real source was only marginally political.

Thus presumption and inference are both necessary and legitimate. The key point is to make sure that the assumptions used are plausible and minimal; uncontrolled speculation is clearly illegitimate. The control is effected through the continual testing of implicit models: did the United States go into the First World War because of its economic interest in the European market? if so—that is, if it is true that economic motives were so important in shaping foreign policy—why did the government prefer to lose that market after the war rather than continue giving Europe aid at that time? It is important, in other words, to avoid *ad hoc* explanations and conceptualize in terms of at least implicit models which can be tested in other circumstances.

The historian interested in the problem of the social determinants of foreign policy is not likely to get much help from political science. Nor is traditional diplomatic history, with its notoriously narrow focus, able to provide much guidance. It is thus not surprising that some historians have tried to develop for themselves the kind of conceptual framework needed to analyze foreign policy from a broad, sociopolitical perspective. Some of the most interesting new historical work, in fact, attempts to explain foreign policy primarily as a means of manipulating domestic politics. An attempt has been made, by Arno Mayer and his students, to apply this theory to the European powers in general in the period before the First World War. But its relevance to the British case has been very effectively contested by Donald Lammers, and in any case Mayer was careful not to insist too strongly on his interpretation: his emphasis on internal determinants he justifies as an attempt to redress the imbalance resulting from the traditional concentration

on purely diplomatic factors.⁴²

In any event it is with respect to imperial Germany that the best case of this sort has been made. The notion that the foreign policy of the German Empire was elaborated largely for domestic purposes has a long history, going all the way back to Lord Salisbury's 1870 article on Bismarck's seizure of Alsace-Lorraine. The annexation would be a permanent source of international tension, but, Salisbury wrote, from Bismarck's point of view an unstable international situation, and even an aggressive foreign policy, had their uses: "the unity of Germany is young and requires fostering: and war is the mother's milk of infant empires."⁴³

The real father of the school, however, is the German historian Eckart Kehr. His most important work, *Schlachtflottenbau und Parteipolitik 1894-1901*, was published in Berlin in 1930. In his time Kehr remained something of an isolated figure, but today the school has really flowered, especially in Germany.⁴⁴ The general argument is that the German system before 1914 was anomalous: the political system had not adapted itself to socioeconomic changes, and an aggressive foreign policy came into being fundamentally as a way of dealing with the tensions this generated.

At first glance, Fritz Fischer's well-known works—*Germany's*

⁴² Arno Mayer, *Dynamics of Counterrevolution in Europe* (New York, 1971), esp. chap. 6, "Internal Causes and Purposes of War in Europe, 1870-1956"—this chapter was originally published in the *Journal of Modern History*, 41 (September 1969), 291-303; Geoffrey Chapman, "The Political Main-springs of International Conflict: France, Italy and World War I" (Ph.D. diss., Princeton University, 1971); Donald Lammers, "Arno Mayer and the British Decision for War: 1914," *Journal of British Studies*, 12 (May 1973), 137-165.

⁴³ "The Terms of Peace," published anonymously in the *Quarterly Review*, October 1870. The kind of analysis Salisbury makes would now be associated with Marxist scholarship. It is thus curious to note in this context that Marx's own analysis of the probable future course of international politics ignored domestic factors of the sort Salisbury stressed and was based instead on traditional power political considerations, colored only by the racial consciousness so characteristic of pre-World War I political thought: France would be driven into the arms of Russia, and Germany would have to "make ready for another 'defensive' war, not one of those new-fangled 'localised' wars, but a war of races—a war with the combined Slavonian and Roman races" (*The Civil War in France: The Paris Commune* [New York, 1968], pp. 32-33).

⁴⁴ Some of the principal recent West German works on the subject are briefly reviewed in Raymond Poidevin, "Aspects de l'imperialisme allemand avant 1914," *Relations internationales*, no. 6 (Summer 1976), pp. 111-112. On Kehr's influence on American scholarship, see Arthur L. Skop, "The Primacy of Domestic Politics: Eckart Kehr and the Intellectual Development of Charles A. Beard," *History and Theory*, XIII, 119-131.

Aims in the First World War and its sequel *War of Illusions*—provide a prime example of this kind of analysis. The aim of Germany's policy before 1914, he writes, "was to consolidate the position of the ruling classes with a successful imperialist foreign policy; indeed it was hoped a war would resolve the growing social tensions."⁴⁵ This theme is quite common in his writings. But Fischer was hostile to the idea of clearly defined "models"—that is, to attempts "to force the historical process into the Procrustean bed of social science and politico-logical categories."⁴⁶ He therefore was unwilling to push the sociopolitical argument too hard and felt free to use whatever other arguments—cultural and economic ones, most notably—that tended to support his case. As a result of this somewhat promiscuous style of argumentation, it is hard to get a clear idea of what Fischer felt was most important—what he felt the fundamental dynamic of German foreign policy really was.

V. R. Berghahn's *Germany and the Approach of War in 1914* does have an explicit model, and is in fact more typical of the sociopolitical school. An analysis of this work thus serves our purposes better. Berghahn's central argument is that the Prusso-German system was inherently unstable, and in fact was in a state of perpetual crisis. There was a "growing rift between a quasi-absolutist monarchical system and a society which, under the impact of industrialization, was undergoing a process of rapid change. This rift could have been bridged only if the conservative elites in charge of the system had permitted its gradual transformation into a parliamentary type of government."⁴⁷ In practice the rift thus amounted to a chronic conflict between the Reichstag and the government. Was this in any real sense a "crisis"? Berghahn certainly does not prove it, and in fact government and parliament were always able to resolve their differences. It was natural, moreover, that the government should seek to control a majority in the Reichstag: but this fact Berghahn interprets in much more grandiose terms. An imperialist foreign policy, with an ambitious armament policy as its concomitant, was adopted not just as a way of assuring parliamentary majorities as a matter of convenience, but as a means of overcoming the "crisis": "with the help of the Navy, the

⁴⁵ Fritz Fischer, *War of Illusions: German Policies from 1911 to 1914* (New York, 1975), p. viii.

⁴⁶ *Ibid.*

⁴⁷ V. R. Berghahn, *Germany and the Approach of War in 1914* (New York, 1973), p. 17.

monarchy wanted to overthrow the *status quo* internationally in order to preserve it at home.”⁴⁸ The implication is that without such a strategy of “secondary integration” (Berghahn here uses Wolfgang Sauer’s phrase), the anomalous Prusso-German system would have somehow collapsed.⁴⁹

Are these notions proven by the evidence? One key document for Berghahn is a letter written by Admiral Tirpitz, perhaps the principal advocate of navalism in prewar Germany, arguing for naval expansion: “‘In my view,’ he wrote, ‘Germany will, in the coming century, rapidly drop from her position as a great power unless we begin to develop our maritime interests energetically, systematically and without delay.’ He added that an expansion had become a necessity ‘to no small degree also because the great patriotic task and the economic benefits to be derived from it will offer a strong palliative against educated and uneducated Social Democrats.’” From this Berghahn concludes: “In other words, Tirpitz’s naval policy was nothing less than an ambitious plan to stabilise the Prusso-German political system and to paralyse the pressure for change.”⁵⁰ Such a conclusion goes far beyond what the document would justify. It seems more plausible that purely international considerations—Tirpitz’s first point—were dominant, and that the domestic argument was thrown in by an advocate eager to seize on anything that might sell his policy.

Furthermore, if domestic factors were the primary consideration, one would expect the policy to have been abandoned when it turned out that navalism, because of the fiscal burden it entailed, was actually aggravating the sociopolitical problem. But nothing of the sort happened.⁵¹ Because Berghahn is not concerned with testing his theory by comparing fact with expectation, this kind of problem is not even considered. Finally, given the book’s theme, one would expect an attempt to establish a connection between the allegedly rational policy of aggression and the most important international episode in the period, the July Crisis of 1914. But Berghahn makes no such attempt—he even speaks of the “fumbling and far from cold-blooded way” in which the German government managed the crisis.⁵²

⁴⁸ *Ibid.*, p. 40.

⁴⁹ *Ibid.*, p. 13.

⁵⁰ *Ibid.*, p. 29.

⁵¹ *Ibid.*, p. 72.

⁵² *Ibid.*, p. 201.

The absence of an adequate evidentiary base once again points to the dominant role played by *a priori* assumptions. Here it is the notion that modernization in the socioeconomic sphere must be associated with the proper kind of political change, namely with "gradual transformation into a parliamentary type of government." Industrialization and democratization go hand in hand; to allow the former while suppressing the latter inevitably generated a "crisis."

Such notions about the naturalness, even the inevitability, of political development along democratic lines as societies develop economically, are so fundamental a part of the political culture of the Western democracies that it takes a real effort to stand back and ask how well founded they really are. It could be argued that these are ethnocentric notions, and that the current prevalence of liberal forms of government in advanced industrial societies is largely accidental—essentially a by-product of the outcome of the two world wars. The triumph of the democracies led to the triumph of democracy, but only because the rise of Soviet power together with the collapse of German and Japanese power created a situation where Western Europe and Japan were dependent on American friendship. To cement the alliance and guarantee American protection, it made sense to adopt American values and norms. (If there is no evidence for this, it could always be said that the calculation was "intuitive.") By creating a cultural community, the political alliance would be put on a more secure and more permanent base. To make the point another way, one can ask what would have happened if Germany had won either world war, and had imposed her system on the rest of Europe. Assuming that the regime that emerged lasted any period of time, a disinterested observer looking on such a society might have found it very easy to argue that an authoritarian political system was the natural concomitant of the inherently hierarchical form of socioeconomic organization typical of advanced industrial society. Democratic institutions and liberal ideals, it would be said, might have made sense in a society based on small-scale production, but remaining democratic societies (if any still existed) would be viewed as atavistic survivals, quite out of place in the twentieth century.

Speculative exercises of this sort should not be taken too seriously, but they do have a function. In this case, the aim is to sensitize us to the danger of imputing too tight a logic to the course of

events, and of being too quick to find sufficient explanations for what has occurred. It also serves to indicate the flaws in the theory which serves as the real basis of Berghahn's (and others') arguments. From a methodological point of view, the problem with such theories is that they preclude the possibility of disconfirmation: as Charles Maier pointed out in a somewhat similar context, the effect is to transform historical debate into a *dialogue des sourds*, into a sterile debate over values.⁵³

At first glance, Arno Mayer's important work on the Paris Peace Conference provides another example of this problem of preconception.⁵⁴ But there are certain things which distinguish his method from that, say, of Berghahn; these differences are important enough to merit extended examination. Mayer's principal argument is that foreign policy and domestic politics were interlocked. Revolution had triumphed in Russia and threatened to engulf Central Europe: clearly peacemaking could not be based on old-fashioned, purely diplomatic considerations. The specter of communism looming in the east was linked to the threat of revolution at home; together these gave rise to a counterrevolutionary movement which aimed at crushing the Left at home and abroad. The diplomacy of peacemaking, and in particular the shaping of the peace settlement with Germany, is to be understood in this context: a liberal, "Wilsonian" foreign policy was a means of accommodating the "forces of movement" at home and abroad and thereby containing the revolutionary threat, while a hard-line, jingoist foreign policy was designed to crush the Left, again both domestically and internationally. But there was another side to this coin. Not only did diplomacy have a function in the domestic political system, but foreign policy itself was to a large degree a projection of domestic politics: although statesmen sought to manipulate domestic politics via foreign policy, domestic politics also constrained and helped shape diplomacy.

That politics and diplomacy in this period were interlocked can be inferred in a general way from the logic of the situation. This point is not in dispute. The real problem in analyzing Mayer has to do with how well he establishes the concrete mechanism of

⁵³ Charles Maier, "Revisionism and the Interpretation of Cold War Origins," *Perspectives in American History*, 4 (1974), 313-347, esp. pp. 339, 345-347.

⁵⁴ Arno Mayer, *Politics and Diplomacy of Peacemaking: Containment and Counterrevolution at Versailles 1918-1919* (New York, 1967).

linkage. We thus return once again to the question of evidence. It simply is not proven, for example, that Clemenceau—to Mayer the principal advocate of a Carthaginian peace—shaped his foreign policy with an eye to crushing the Left at home. Instead Mayer darkly (and again without evidence) hints at a conspiracy theory: the “outburst of revengeful jingoism,” he implies, was not “all that spontaneous,” but rather was part of a “vast political design” which aimed at promoting the “class interests and status positions” of the “forces of order.”⁵⁵ Mayer, moreover, simply ignores even evidence which he presents which would tend to disprove his theories. Vigorous opposition to a negotiated armistice, for example, he portrays as the characteristic counterrevolutionary position. But the French—that is, the putative arch-Carthaginians—took the same line as Wilson, accepting an agreed armistice that would render Germany incapable of resuming the struggle. Mayer is unaware of any contradiction and rationalizes French policy with ad hoc explanations.⁵⁶ The British Prime Minister Lloyd George on the other hand took a hard line on the armistice question. But Mayer defines Lloyd George as “an appeaser by temperament and outlook” and again resorts to an ad hoc argument as a means of dismissing the evidence: Lloyd George was probably just testing “his colleagues’ reaction to the intransigents.”⁵⁷

That these are not isolated examples may be demonstrated by an examination of the two chapters where one would expect Mayer’s argument to be the strongest and most explicit: “Intrusion of Politics: Britain” and “Intrusion of Politics: France.” The chapter on France is essentially a summary of parliamentary and Socialist party activity, but no linkage with diplomacy is actually proven, or even explicitly suggested. Mayer does not argue for example that the government hardened its policy of German reparations to accommodate the Right or launch a “preemptive thrust” against the Left; all he shows is that the government used reparation as a means of evading public discussion of the budgetary problem.⁵⁸ In my own work on the question, I was surprised to discover how moderate French reparation policy was at this time: on the key question of figures, French and American policy more or less converged, and the French as early as April and May 1919

⁵⁵ *Ibid.*, pp. 13-14.

⁵⁶ *Ibid.*, pp. 53-54, 85.

⁵⁷ *Ibid.*, p. 64.

⁵⁸ *Ibid.*, chap. 19, esp. p. 649. The phrase “preemptive thrust” is on p. 15.

made overtures to the Germans for negotiations to work out a businesslike solution to the problem. It was clear that Clemenceau was paying little attention to political pressures in formulating policy. A close reading of this chapter of Mayer's book would lead the reader to this conclusion, in spite of the fact that it contradicts Mayer's larger argument: for Clemenceau's refusal to bow to parliamentary pressure comes out quite clearly.⁵⁹

Similarly in Mayer's chapter on Britain, there is a good deal of material on parliamentary activity and very little on the question of linkage with policy. At least this time a linkage is alleged: Lloyd George "opted" for a moderate settlement, but was held back by the political situation at home.⁶⁰ But the evidence on Lloyd George's personal inclinations is not clear-cut, and he certainly never clearly opted for a moderate reparation settlement at the peace conference: British figures during the crucial negotiations were consistently higher than corresponding French or American ones.⁶¹ Mayer discusses at great length a campaign on this question conducted by the Right in Parliament, but he does not explicitly argue that it had any direct effect on policy. (In fact it had no effect on figures and the key new British demand at this time—the inclusion of pensions—was put forward before the campaign even began.) Indeed, the conclusion to be drawn from Mayer's own evidence once again is that the government was able to get by without making any solid commitment to Parliament on foreign policy.⁶²

So far these are the same kind of problems that were examined before in the discussion of Berghahn: the priority of preconception, the failure to test assumptions against the evidence, and so on. But the basic methodological problem here is really different. It is no longer merely a question of a refusal to take into account evidence not in conformity with the original set of assumptions; there is no evidence that cannot be absorbed into Mayer's basic framework. Thus in my own work I came to the conclusion that French policy toward Germany in 1919 was quite moderate. Although Mayer takes the opposite line, if he were convinced by my work he would

⁵⁹ *Ibid.*, pp. 660-662.

⁶⁰ *Ibid.*, chap. 18, esp. pp. 624-632.

⁶¹ This conclusion is drawn largely from my own unpublished work, but the negotiations can be followed in detail in the introduction to Philip Mason Burnett, *Reparation at the Paris Peace Conference* (New York, 1940).

⁶² Mayer, *Politics and Diplomacy*, pp. 644-646.

have no trouble adjusting his argument: the French were simply more sensitive to the threat from the Left and more prone to dealing with it by means of a policy of conciliation than he had imagined. Because the theory has no predictive force—the same cause can give rise to diametrically opposed policies—it is so hospitable that anything can be explained: social turmoil can lead either to accommodation or repression, so whatever happens can be imputed to social turmoil, and there is no way of disconfirming the argument. The basic methodological problem with Mayer is thus what might be called the problem of *porosity*.

It is easy to point out the difficulties with explanations of this sort. But it is not inconceivable that Mayer is basically correct in his view that the specter of revolution lay at the root of both hard- and soft-line policies. How is this to be dealt with? I know of no satisfactory answer. But it seems clear that when theories which do not admit of disconfirmation are introduced into the analytical framework, the standards for judging the adequacy of evidence need to be tightened: more empirical proof is needed than in the case of work based on theories subject to disconfirmation.

Are general conclusions to be drawn from this examination of these historical works? As in the case of the review of the political science literature, the analysis here suggests the crucial importance of concentration on method. Above all the key thing is to keep open the possibility of disproving the argument being developed. The best recent work on the question of the domestic determinants of foreign policy is characterized by a concern for keeping an open mind, and for phrasing questions and designing research strategies so as to minimize the problem of preconception.⁶³

⁶³ Many American scholars are unfortunately unaware of the important work in this area being done in France. See especially Jean-Noël Jeanneney, *François de Wendel en République: L'Argent et le pouvoir 1914-1940* (Paris, 1976), and some of Georges Soutou's recent articles, especially "Les Mines de Silésie et la rivalité franco-allemande, 1920-1923: Arme économique ou bonne affaire?" *Relations internationales*, no. 1 (May 1974), pp. 135-154. See also an important series of works by Pierre Renouvin's students: Raymond Poidevin, *Les Relations économiques et financières entre la France et l'Allemagne de 1898 à 1914* (Paris, 1969); Pierre Guillen, *L'Allemagne et le Maroc de 1870 à 1905* (Paris, 1967); and René Girault, *Emprunts russes et investissements français en Russie, 1887-1914* (Paris, 1973). On this school, see the notes by J.-B. Duroselle in *Relations internationales*, no. 1 (May 1974), pp. 210-211, and Renouvin's own programmatic article, "Les Relations franco-allemandes de 1871 à 1914: Esquisse d'un programme de recherches," in A. O. Sarkissian, ed., *Studies in Diplomatic History and Historiography* (New York, 1961) pp. 308-321.

The most sophisticated method will not, however, answer all questions, and in this connection it is important to stress once again the importance of language. Partly because the human mind, like nature, abhors a vacuum, and partly because the historical profession rewards imagination more highly than rigor, historians commonly tend to impute a tighter logic to events than their evidence really justifies. The use of tentative language is the best way to convey a sense for the tenuous nature of the argument being developed.

All these points are really very simple. They were worth developing only because in practice they are so widely ignored. It is possible to draw a pessimistic conclusion from this: these methodological principles are so simple that the failure of scholarship to internalize them cannot be due to their inherent complexity, but must have social causes beyond the reach of intellectual endeavor. In other words, if we have so far done so poorly, we will probably never do any better. It is, however, also possible to be optimistic and say that it will only take a minimal effort to turn the situation around. The effort is certainly worth making. But it would be unwise to hold one's breath in anticipation of fundamental change in this area.