

# History, Quantitative Analysis, and the Balance of Power

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**I**t is perhaps a paradox that history, the discipline which is closest to the raw stuff of politics, economics, and international affairs, has produced so little in agreed conclusions about such matters. Historians have managed to disagree about issues of great importance even though the data at their fingertips would seem to provide a substantial foundation for accord. Certain scholars have declared that World War I occurred because of an imbalance in international power (Hinsley, 1962); others have declared that it resulted from too great a balance (Langer, 1953). The question of responsibility for the war is still unresolved: Albertini, Fischer, and his disciples see Germany at fault; Langer, Fay, Turner, Ritter, and others believe the blame must be more evenly distributed. The divergence over World War I is magnified when one approaches World War II and the Cold War. Here "revisionist" contend with "traditional" interpretations, and there is no end to the argument in sight.

Paul Schroeder contends in his companion piece in this issue that the assumptions and methods of our work in the Situational Analysis Project are questionable and fail to meet historical requirements. We propose to show in rejoinder that the assumptions and methods we have used are fully cognizant of the difficulties he mentions, and indeed have been designed to overcome some of the

barriers that historical study currently faces. We also believe that to employ the procedures he suggests would be a retrograde and erroneous step which would not lead to a more satisfactory account of diplomatic patterns, but away from it. In what follows we divide our remarks into two sections: (1) on events, their description, abstraction, and evaluation, and (2) on power, status, and alignment—their definition and measurement. We conclude with general observations on the requirements for satisfactory quantitative work in the international relations field.

## EVENTS

### GENERAL ARGUMENT

There has been a debate at least since Collingwood over the proper way to interpret historical reality. Collingwood (1946) emphasized the motivations or intentions that lie behind historical events. In *The Idea of History*, he wrote: "For history, the object to be discovered is not the mere event, but the thought expressed in it. To discover that thought is already to understand it" (Collingwood, 1946: 214).<sup>1</sup> From this standpoint, unless one understands the intentions precipitating the action, one cannot understand the action. Essentially, it is this ideal analysis of motivations which Schroeder commends to us as a *sine qua non* of satisfactory historical research. He insists that all historical events must be seen as "part of a process" and that "only as part of this larger process and policy can these separate actions be understood" (Schroeder, 1977: 8). He declares that we can understand an action or an event "only if we know what it was intended to do." And again: "We still cannot give [an event] a valid conflict-cooperation score until we know what Bismarck was trying to do by this move" (p. 9).

Having stressed intentions as the crucial desideratum, however, Schroeder then turns around and admits that Bismarck's intentions are "still hotly controverted questions after a century of debate" (p. 9). He demands that we strive to understand Gramont's motivations and then acknowledges that knowing what Gramont was trying to do is still a "controversial question" (p. 9). Many similar examples come to mind. How seriously, for instance, should one take the view that

1. But Collingwood was not entirely consistent in his belief. Elsewhere he notes that "history gained enormously" from the Christian perspective "because the recognitions that what happens in history need not happen through anyone's deliberately wishing it to happen is an indispensable precondition of understanding any historical process" (Collingwood, 1946: 48).

Bismarck was laying a "trap" for the French in the Spanish succession crisis?<sup>2</sup> If Bismarck had been convinced that only a war with France would bring the South German states into the empire, he might have found his pretext in the Luxembourg crisis of 1867. But he did not (Taylor, 1954: 181). Historians do not resolve this dispute over objectives.

Or, to take a broader canvass, what is one to say of the basic intentions or motivations of Wilsonian or Rooseveltian policy? The interpretations of their underlying motivations are both myriad and various. According to one line of thought, the cause for Wilson's entry into war in 1917 was the violation of U.S. neutral rights (Morrissey, 1939: 194-197). But Lippmann (1941: 85) saw it as prompted by the need to maintain a balance of power in Europe. Revisionist historians have claimed, contrary to both views, that U.S. intervention was determined by the need to protect liberal capitalism on a world scale (Levin, 1968: 13-73). A generation later the objectives of Roosevelt's policy toward the Russians were equally in dispute. One line of analysis suggests that the president reacted to Soviet hostility with defensive moves, and that he had not ruled out a long-term accommodation with Moscow in the postwar period. An alternative explanation declares that U.S. demands for an economic "open door" in Eastern Europe and the use of strategic pressure on the Soviet Union reflected a basic anti-Russian orientation that led directly to the Cold War. How can one decide between such conflicting views of objectives? Historians provide no basis, and, in the nature of the case, none can be given. In short, if the illumination of events requires the prior statement of the specific governing intentions which animated them, no satisfactory account will ever be offered. Intentions and motivations are subject to endless disputation. The only evidence we have is concrete actions and communications. Such realities can be directly apprehended, but mental states can only be inferred.

Intentions and motivations are hard to grasp for another reason. In many instances statesmen have left records that are designed to give an apparent pattern of motivation that may be at variance with reality. Memoirs, and even the memoranda of the time, may be severely misleading. Bismarck's *Gedanken und Erinnerungen* is just one indication of the intractability of the problem. Leaders try to lend a greater consistency to their acts than is warranted, or they proffer intentions that are worthier than the ones actually held. But even if the "unvarnished" motives could be exposed, there are still doubts. Sometimes individuals are unaware of the basic forces which animate their actions.

2. See particularly the account in Eyck (1958: 162-174).

Suppose one might have interviewed Hitler at the time of the remilitarization of the Rhineland in 1939. Even a polygraph test might have shown that he was "sincere" in his desire for peace with the French and in offering a nonaggression pact to Paris. But the German dictator's actions revealed a pattern of territorial expansion nonetheless. Ultimately that pattern, and not conscious motivation, was the best guide to Hitler's policy.

Not all historians, of course, accept the notion that the task of history is to illuminate intentions. Kolko (1968: 8), the American diplomatic historian, notes: "It is sufficient to study [the] pattern of functional behavior, to comprehend the assumptions formulated in response to challenging situations, and to perceive a policy and pattern that in some sense makes future response predictable." And "a description of functionally defined goals and consistent actions, *whether or not someone has consciously defined and explicitly acknowledged them*, reveals the motives and consequences of any nation" (Kolko, 1968: 3; emphasis added). The pattern of national action itself becomes the best guide to operative motives. Actions out of harmony with proclaimed intentions lead one to question the motives, not the pattern of actions. Taylor, the British diplomatic historian, has carried the task even further. He uses the record of actual events to cast doubt on the express motives of leaders. Taylor (1955) elucidates Bismarck's policy by underscoring the discrepancy between the consistent and even deterministic account of purposes and policies given in the German statesman's memoirs and his actual vacillation in the cut and thrust of events. In his book on World War II, Taylor (1961) discounts the impressions conveyed by *Mein Kampf* and the Hossbach Memorandum of Hitler's purposes. The best guide is not the German dictator's speeches, writings, or pronouncements at staff conferences, but his opportunistic response to events. Taylor contends that those who seek motivations before they explain events reverse the proper sequence: to understand motivations one must first explain events.

In the behavioral sciences this truth has been acknowledged for at least two generations. Watsonian psychology embraced the notion that varying conscious mental states were not always the best guide to performance and behavior, that patterns of behavior themselves held the key to an understanding of operative motives (Watson, 1924). In a similar way, the study of international relations and international history should not seek to repeat the mistakes of historians who have wrongly convinced themselves that ultimate "intentions" must be apprehended prior to the understanding of a single atomic event. One

should not be deterred from the study of the actual record of events and actions by heady problems surrounding the “unascertainability of ultimate motives” or the “ready availability of proffered motives.” In this context, then, it is not surprising that we in the Situational Analysis Project decided eight years ago not to follow the path that Schroeder recommends, and we have seen no reason to change our judgment.

### **SPECIFIC CRITICISMS**

Let us now turn to the precise points that Schroeder raises in his discussion of our use of international events. Fundamentally they are six in number.

- (1) That diplomatic historians are not reliable in their selection of events, and, even if they were, the Situational Analysis Project has not plumbed the required sources to unearth them.
- (2) That the Situational Analysis Project has omitted events that occurred between European powers in Africa, Asia, or the Far East.
- (3) That we have erred in that “one of the most important forms of actions in international relations is precisely that of nonaction.”
- (4) That events or interactions cannot be treated as “separate occurrences” but only as part of a “broad process” (involving specification of intentions).
- (5) That in particular it is senseless to treat actions which are intended to delude the other party as cooperative.
- (6) That it is impossible to judge reliably the amount of “conflict” or “cooperation” involved in a particular action because “conflict” and “cooperation” are not polar opposites. Overtly cooperative action may actually demonstrate latent conflict.

1. **Events and Their Reporting: How Reliable are Historians?** Schroeder argues that historians offer only what he calls “ingredient events,” that are irrelevant and unimportant divorced from the argument in which they are couched (from, as Schroeder puts it, the “larger process”). Thus, one historian will amass one body of factual materials to prove one point, another will collect a different set to prove an opposing view. To try to get at the substance of the debate between them by abstracting the data on which their conclusions rest, it is contended, is vain. Events are only strung together to serve argumentative purposes; they have no existence or status aside from that.

If Schroeder were right in this assertion, there would indeed be little use in collecting or evaluating events from diplomatic historical materials. No coherent account could be based upon them. Each new historical work would add an entirely different and perhaps even con-

flicting set of data points. We have found, however, that historians do largely appeal to a common substratum of facts and events in justifying their conclusions (however different the latter may be). Not only do they appeal to the same factual bases, they describe and define facts and events in much the same way. Even historians who differ greatly on the conclusions to be drawn from events will *describe* events similarly. Thus, there is basic agreement on the facts surrounding the Kaiser's writing of the "blank check" to Austria on July 5, 1914, even though the interpretations and explanations of that event, and the praise or blame associated with it, may vary. The "Is War in Sight" crisis of 1875 is described in common terms by historians, even though there is divergence about what Bismarck "intended."

It thus becomes possible to compile a list of significant diplomatic events after culling the works of a few standard historians.<sup>3</sup> After the major sources have been surveyed, each new source adds only about 1%-2% to the total list of important diplomatic events. There is therefore no need to multiply sources simply for the sake of exhaustiveness (or exhaustion!). As to Schroeder's contention that some of the sources are out of date, one can only note that the books were chosen from the American Historical Association's *Guide to Historical Literature* (Howe et al., 1961) and are those recommended by his own professional colleagues.

2. The Inclusion of Extra-European Events. The observation that the Situational Analysis Project has sought to abstract only European interactions and not those which occurred between major powers in Asia, Africa, and the Far East in the period 1870-1890 is simply incorrect. Further, actions between major powers and extra-European states are also included. From the beginning the project explicitly included the non-European as well as European actions of the great powers, and the former comprise an important part of the master list of significant diplomatic events.

3. Schroeder has got the wrong eight. This presumably results from a failure to read our basic methods and data paper. See Goodman, Hart, and Rosecrance (1975). The sources were:

- Albertini, L., *The Origins of the War of 1914*, Vol. 1 (London, 1952);
- Fay, S. B., *The Origins of the World War* (New York, 1938);
- Hinsley, F. H. (ed.), *Material Progress and World-Wide Problems: 1870-1890*, Vol. 11 of *The New Cambridge Modern History* (Cambridge, 1962);
- Langer, W. L. (ed.), *An Encyclopedia of World History* (New York, 1952);
- Langer, W. L., *European Alliances and Alignments, 1871-1890* (New York, 1950);
- Schmitt, B., *Triple Alliance and Triple Entente* (New York, 1934);
- Sontag, R., *European Diplomatic History, 1871-1932* (New York, 1933);
- Taylor, A.J.P., *The Struggle for Mastery in Europe, 1848-1918* (Oxford, 1954).

3. Actions and Nonactions. Schroeder claims that the extraction of diplomatic events from the historical record may be particularly misleading, because nonevents are as important as events. A power's benevolent inaction may be as significant as overt intervention. This argument is similar to that heard in the analysis of community power structures, where we are told that "nondecisions" are as important as "decisions."<sup>4</sup> The problem posed by an investigation of "nondecisions" or "nonactions," however, is that while actions and decisions leave a trace and can be investigated empirically, nonactions do not do so. How would one grasp such ephemera? Are all intervals between actions to be considered nonactions? Which of these are worth recording? Presumably Schroeder would argue that if a nonaction is peculiarly in accord with the "intentions" of a diplomatist, the nonaction is significant. Thus, the discrimination between important and negligible nonactions requires one to specify intentions in advance. This cannot be done reliably and, as we have seen, certainly has not been done by historians. In short, the empirical problem of trying to record and discriminate the important nonactions soon degenerates (ascends?) into metaphysics.

This is not to say that important nonactions which are discussed with other powers (and therefore leave a trace) should not be included. Bismarck was often very clear about his abstention in colonial matters. He was quite willing to see France and Great Britain collide in imperial conflicts while Germany stood on the sidelines. In order to distract the French from the Rhine he proffered his benevolent neutrality should they wish to expand in Tunis. Such events, of course, are not "non-actions": they are important actions and are duly incorporated in our list of significant events. It is, of course, quite possible that Schroeder has assumed (wrongly) that actions are only overt movements of power or formal warnings while informal communications are not actions at all. If so, he makes an artificial distinction between "actions" and "communications" that is not found in our work. For the Situational Analysis Project *both* are events, and *both* are important.

4. Actions as Separate Occurrences.

5. Actions Designed to Mislead.

6. Conflict and Cooperation as Polar Opposites.

These three objections are best considered together, for they constitute different aspects of the same problem: whether events can be atomically separated and whether their cooperative and conflictual quality can be determined. One example given by Schroeder illustrates

4. See the argument between Bachrach and Baratz (1970) on the one hand, and Dahl (1966) on the other. Also see the Wolfinger (1971) rejoinder.

his argument under both point 4 and point 5. The hypothetical case of four ostensibly cooperative actions by thief toward his victim, followed by the theft of her purse, can be interpreted as an argument against the separability of actions as well as an illustration of the difficulty of determining the amount of cooperation involved in each event. Schroeder claims that the first four cannot be understood to be truly cooperative since they are designed to win the victim's confidence and lull her into a receptive mood. From one point of view all five events are evidences of conflict (not cooperation); from another, they are not separate actions at all. Let us consider these two possibilities.

First, suppose that there are no separate actions, but only one broad "intentional" process. What, then, is the historian or political scientist recording? Is it the mental states of diplomats or their actions? But if actions are not discrete and separable, how can they be isolated or studied? Alternatively, if one is examining mental states (intentions), how does one know when one mental state changes and gives rise to another? How long does each mental episode last? One would not wish to minimize the problems involved in inferring the intentions of diplomats from a study of their actions. The study of intentions is, as we already have seen, an intransigent reality of inquiry. But far more difficult is the opposite process: extrapolating the concrete actions and occurrences of international diplomacy from the pattern of mental states of the participants. In diplomacy, as in other aspects of human affairs, happenstance, the unforeseen, the intrusion of domestic or other forces into the policy process—all these prompt action. The record of events thus is not simply a reflection of what all would have preferred in the best of all possible worlds, but what inadequately informed statesmen have often felt forced to do under the press of internal and external circumstances. In short, there is no way in which "intentions," Schroeder's "broad process," can be studied at all. If a historical record is to be written, it must depend on actual facts and events, not on endlessly debatable mental states.

Second, let us consider the hypothesis that there are indeed separate events, but that these cannot be categorized in terms of the amount of cooperation and conflict which they involve. In the "thief episode," Schroeder argues that the first four events actually are hostile, even though they take cooperative form. A thief romances a woman and then steals her purse. But suppose the fifth event had been:

5. He reaches for the money in her purse and then, overwhelmed by his love for her, decides not to take it.

In this case, the first four events would have had a much more markedly cooperative quality, for they cause the thief to give up his plans. In



*The Music Man*, Professor Harold Hill sells the town of River City an unneeded and unusable set of instruments for a boys' band; in the end, however, he falls in love with the librarian and gives up his scheming ways. The partly cooperative nature of the first four events is even suggested by the fact that the thief uses them to disarm his victim; if they were not cooperative, she would not be disarmed (or charmed). Thus, there can be no doubt that these events have a cooperative element in them. The only question is: to what degree?

Suppose the example, however, had run as follows:

1. A *thief* takes a woman by the hand.
2. He puts his arm around her.
3. He draws her close to him.
4. He kisses her.
5. He steals money from her purse.

In this instance the cooperative quality of actions 1-4 would be considerably modified by one's advance knowledge of his past vocation. Taking account of his past actions, an observer would not take the thief's seemingly cooperative overtures very seriously. In international relations, of course, the actors are not a "man" or a "thief," but "Britain" and "Austria." It can be argued that such national designations lie somewhere between the supposed morality of the "man" and the inveterate immorality of the "thief." Those who are asked to evaluate the amounts of "cooperation" in actions of a great power will be aware of its past actions not only in general terms, but also in respect to the specific power addressed. Thus, no problem is posed by Austrian actions toward Russia in 1822 or Prussian actions toward Austria in 1863-1864. In both cases the record of previous actions would make clear the inherent rivalries in the situation. Those who were asked to evaluate the amount of cooperation would have to contrast the "form" of cooperation with its actual "substance" and reach a balanced conclusion. They would not rate the cooperative impact as high, despite the overt form of the action.

Schroeder may be tempted to object at this point that if the individual raters or scalers are given the freedom to contrast the form of cooperation with its substance, all outcomes are possible, and that the result may be various and unreliable. In theory this might seem to be true; in practice, however, it is not. In all our tests the interscaler reliability correlation was never less than .927, even where complex discriminations were involved (Goodman, Hart, and Rosecrance, 1975). In fact, once a certain number of diplomatic events had been reviewed by evaluators, they were able to hold past actions and past patterns in mind when estimating the cooperation involved in a given present act.

This approach does not involve trying to specify ultimate intentions before judging the amount of cooperation or conflict involved in a given act. Historians have failed in this task, and it is not likely that others will be more successful. But "intentions," as we have seen, can be behaviorally approximated from the pattern of past actions; indeed, "intentions" can be operationally defined in terms of the pattern of actions—for whatever a diplomatist may actually have had in the back of his mind, his actions speak louder than his words (or thoughts).

Schroeder also asserts that "conflict" and "cooperation" are not opposites; that it is possible to find high cooperation and high conflict conjoined in a single act. The thief's actions in the previous example appear to be an amalgam of the two. It is, of course, possible to regard "cooperation" and "conflict" as entirely different concepts, bearing no necessary relationship to one another. Some behavioral research has been based on the construction of two distinct scales, one for "cooperation," the other for "conflict." But to our knowledge no investigator satisfactorily has devised a procedure that permitted a given action to be classified as both "conflictual" and "cooperative."<sup>5</sup> Schroeder writes: "It is possible to entrap and undermine another power by friendship, or to bully and bludgeon it into becoming friends" (p. 12). But there is still a net assessment to be made of the interaction of cooperative and conflictual elements in a single action; ultimately they tend either toward antagonism or toward harmony.

This way of looking at reality is well sustained in historical diplomacy. Countries examine each other's actions precisely for the purpose of determining their *net* effect, to discern the direction in which relations are proceeding. Intelligence ministries do not want to have to tell their political masters that cooperation and conflict expressed by an opponent cancel each other out, leaving relations as before; they want to point to the underlying trend of events. And as events are interpreted in this way, so are they also meant. Metternich used moral suasion to convince the Tsar, but in the event Russian policy was restrained. The combination of cooperative and conflictual elements produced a net result, in Schroeder's view, a triumph of Austrian over Russian diplomacy. Thus, Schroeder himself accepts a net outcome of cooperative and conflictual factors.

## CONCLUSION

It is time now to step back and ask whether the procedures recommended by Schroeder represent an improvement on current practice or

5. See, among others, Corson (1970a: 48; 1970b: 7).

whether they are an invitation to obscurantism. In our judgment, there is no valid or reliable way of finally determining the intentions of statesmen. The very disagreement among highly trained historians over such matters is itself testimony to the difficulty, even insolubility, of this problem. If one were asked to produce a final and agreed estimate of Bismarck's or Gramont's intentions in a given diplomatic episode prior to determining the amount of cooperation involved in an event in which they were actors, the game would be given up at the start. On the other hand, it is possible (and the work of the Situational Analysis Project shows this to be true) to estimate behaviorally (and to produce reliable inferences about) operative intentions in a given action. But the information used in such estimates stems *solely* from the pattern of previous actions. It is not gleaned from a psychological or metaphysical investigation of the "real" motivations of statesmen divorced from concrete circumstances. This information is much more reliable, and it is also probably more valid than that derived from investigations of mental or psychological states. Thus, Schroeder has the cart before the horse. It is not that motivations must be understood before events can be interpreted; it is that events must be understood before motivations can be interpreted.

## THE BALANCE OF POWER

### GENERAL ARGUMENT

Historians have erred not only because they have striven to investigate questions that are intrinsically obscure and subject to disputation. They have also been confused by unclear definitions of crucial terms and by terminological usages. One of the major reasons for historical disagreement lies with different definitions of words. There has been no consensus about the proper use or even meaning of terms like "power," "cooperation," "alignment." They have not found a satisfactory definition of "responsibility" where investigations have involved fixing the "responsibility" for a particular war or pattern of tension in world politics. In the case of World War II and the Cold War, differences among historians have arisen largely from an arbitrary terminological distinction between "initiation" and "response" (Stover, 1972: 145-178). Typically, what one scholar regards as a "response" to action will be seen by another as a major "initiative" provoking conflict. The traditionalists regarded Hitler as the universal initiator (Louis, 1972). Taylor and his disciples saw him as responding disjointedly and opportunistically to the projects of Britain and France (Taylor, 1961). After 1945

the traditional explanations of the Cold War attributed all important initiatives to the Soviet Union and explained Western actions as responses (Feis, 1957). To find the West and the United States at fault, the revisionists merely reversed the sequence (Williams, 1962). In all cases, however, linguistic usage was largely responsible for the differences in point of view.

Differences over the origins of World War I also have this quality. Some see the disruption in the balance of power as responsible for the war, others the attainment of too great a balance. The first typically focus on German national power and observe its growth relative to other states (Taylor, 1954; Hinsley, 1962). The second, however, are more likely to pay attention to the power of coalitions.<sup>6</sup> From this standpoint it was the attainment of a rough balance between Triple Alliance and Triple Entente which led to war. In the 1880s, when Germany's central coalition was unchallenged, general war was out of the question. Thus, the disagreement emerges over whether the balance of power has to do with national or individual power, on the one hand, or the power of coalitions, on the other. But this difference is not empirical; it has to do entirely with what one means by "balance" in the European system.

Of all the terms in political science and history, the word "power" is probably the most Protean. Since "power" can be defined and operationalized in any number of different ways, and since it has many components, it is never possible strictly to say "A" has more power than "B." In order to make such claims, one would have to define the context, the issue over which both were contending, and the respective positions of the two. North Vietnam was surely less powerful than the United States on practically any measure one might adduce, but Hanoi won and Washington lost the war in Indochina. Schroeder is quite right in claiming that "morale," "zeal," quality of training, and quality of leadership are important elements in the calculus of power. But they cannot be measured objectively. One, then, has two choices: either to give up all attempts to test historical theories about the balance of power or to leave out subjective and inherently incommensurable elements. In our original piece we made this choice explicit:

The difficulty of measuring "power" in this comprehensive sense would be critical if the theories to be tested were formulated in such terms. If they were formulated in such terms, however, *they could not be tested at all* since "power" could not be defined or precisely approximated. We do not propose to attempt to assess such grandiose formulations: rather, we seek to find approximations to the *objective*

6. See particularly the account in Fay (1938, Vol. 1: 223-226).

power of a state, the power which is capable of objective assessment. [Rosecrance et al., 1974]

A reviewer of our work should at this point adopt one of two courses: (1) contend that an objective analysis of power is useful in its own terms, despite the exclusion of subjective elements; or (2) argue that any definition or attempt at measurement which leaves out subjective features is unsatisfactory. In fact, however, Schroeder has done neither of these two. He argues, actually, that a quantification of the balance of power is possible (see p. 19 where he writes: "Nor is this essay an argument that diplomatic history materials are inherently incapable of being quantified—that such concepts as power, status, balance, cooperation, and conflict are delicate flowers, withering under the quantifying hand of the political scientist and blooming under the qualitative treatment of the historian") and then berates us for not including intrinsically unquantifiable elements. He cannot have it both ways.

Of course, it is possible to contend that within the strictly quantifiable realm an erroneous or improperly weighted selection has been made, and it is to the specifics of his arguments on these points that we now turn.

#### **SPECIFIC CRITICISMS**

- (1) That the Power index used by the Situational Analysis Project is unsatisfactory for, among other reasons: (a) it does not sufficiently stress the military variable; (b) it fails to consider the individual situation of each country, particularly a country's size, its location, its frontiers, its insular or continental position, its national composition.
- (2) That the measure of "status" used in the work of the Situational Analysis Project is defective because the diplomatic recognition and representation are not valid measures of "status" as normally defined and understood.
- (3) That, in preference to our measures of power and status, the reports of military attachés and other officials and (within limits) "reliable" measures of such concepts are to be used.
- (4) That the Project's measure of "alignment" is defective.

1. The Unsatisfactoriness of Power. Schroeder is particularly disappointed with our conclusion that Germany was not the predominant power in Europe, 1870-1880. That we could have reached such a result must be (he thinks) because we have underestimated the military variable in our power index. If we had given greater emphasis to this element, it is thought Germany would have attained her deserved place.

In qualitative (and therefore incommensurable) terms undoubtedly Germany had certain advantages: she had excellent generalship, and her forces were well trained and led. She also could rapidly mobilize and deploy her forces. When one investigates the objective measures of military strength, however, Germany is never predominant. In numbers of men under arms she is exceeded by Russia and at least equalled by France in this period.<sup>7</sup> If population base, the reservoir of military strength, is the critical element, Russia has a great preponderance. In 1870 Russia had more than three times the population of Prussia, and almost twice that of a united Germany. If military expenditure is the correct measure, France, Russia, and Great Britain each spent twice as much on armament as Germany did in 1870, and half again as much in 1880. If army estimates are thought to be a more satisfactory standard, the result is similar: Russian expenditures were double those of Germany, with France and Britain's half again as much in 1870. By 1880 Germany improved her position, but French and Russian allocations were still, respectively, 25% and 43% higher. There is no purely military category in which Germany led.<sup>8</sup>

"That is all very well," we hear Schroeder intoning, "but Prussia-Germany won the Franco-Prussian war, and the powers that seem most 'powerful' on the index either lost or emerged greatly frightened." The Prussian victories in 1866 and 1870, however, were very near things; they are not an automatic reflex of acknowledged superiority.<sup>9</sup> Most important, they rested on the use of conscripted soldiers, a social innovation in the warfare of the time. But after 1870, all continental European powers adopted the system of military training in force in

7. Anderson (1972: 253-254) writes, "hatred of the new Germany and desire for revenge for the disaster of 1870 inspired heroic military efforts, at least by any hitherto accepted standard, in the 1870s and 1880s. The result was that by 1880 France was, in some ways at least, militarily superior to Germany. She had by then a standing army of 435,000 men to set against the 403,000 at which the German one had been fixed in 1874 by the Reichstag, and 367 field batteries against a mere 300 (armed with rather inferior guns) on the German side."

8. See the tables in Taylor (1954: xxv-xxx).

9. Much of the writing about the wars of 1866 and 1870 has an "inevitalist" tone that is not sustained by the actual military encounters. The Austrians excelled the Prussians in field pieces, but followed a classic eighteenth-century strategy. If they had attacked one of the Prussian armies before it could unite with the other, the result of the war would have been reversed. In 1870, the Prussians had an improved artillery, but their needle gun was inferior to the French *chassepot*. It was the superiority of the Prussian artillery over the French that told the tale. Howard's splendid account (1962) makes clear that Prussian strategy and Prussian infantry tactics were no better than the French. Typical is the following: "It was thus only by a series of mistakes and coincidences that the German forces, both in Alsace and on the Saar, slipped simultaneously in the first

Germany.<sup>10</sup> Thus, normal military, demographic, and resource potentials could be expected to take effect after the peace of Frankfurt. France, having suffered defeat, was resolved not to be beaten again. Her rearmament and reequipping was a major feature of the decade of the 1870s. Schroeder may well contend that “France’s primacy did not *wane* after 1870; it disappeared quickly, totally, and permanently.” But the German military was not sure of this; nor did they fail to take French precautions extremely seriously even in the late 1880s.<sup>11</sup> Taking into account subjective as well as objective factors, the Germans remained justifiably concerned about French preparations for more than 40 years.

In the developing technological and economic context of the time, military machines were fueled by the strength of a modernized nation. Economic and industrial strength and education were as relevant to the power a nation could dispose as military effectives and orders of battle. Thus, Russia could never be the premier power (at least not until her industrial potential could be mobilized);<sup>12</sup> economic and industrial

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great battles of the [Franco-Prussian] war” (Howard, 1962: 108). And, “The tragedy of Spicheren and Froeschwiller lay in the very success with which the French had fought. Their defence had been stubborn, their counter-attacks persistent, and they retired only because, at Spicheren, they had been manouevered out of position or, at Froeschwiller, overwhelmingly outnumbered. The quality of the French army was shown to be all that its admirers had claimed. The *chassepot* also had lived up to expectations . . . they were broken not by German infantry but by German guns. The German infantry did not, indeed, acquit themselves particularly well” (Howard, 1962: 118). The Germans won in Howard’s view only because their blunders were slightly less in number and significance than those of the French.

10. Fuetes (1922) makes two interesting observations. The first is that the numerical advantage which Prussia had formerly derived from her system of universal military service was now equalized to the extent that other states placed their whole population under arms in the same way. Furthermore, the art of rapid mobilization was easily copied in other states to such a point that there could never be in the first days of a war such unequal combats as took place in 1866 and 1870. In the second place, the two wars just mentioned were altogether exceptional in the fact that in both cases the party which had naval superiority had not been able to make any use of this superiority.

11. The Germans assessed the military danger from France as substantial in October 1886. Large numbers of French troops were concentrated on the eastern frontier. Work on fortification continued by day and night. Viewed from abroad, the Boulangerist measures seemed like preparations for an eventual offensive. Langer (1950: 379) writes: “The German general staff did not believe that the French were actually preparing an attack for any specific time. The whole system of fortifications and the concentration of troops on the frontier seemed to indicate an intention of awaiting the German attack. But it was agreed that if the complications should break out elsewhere, the French would seize the opportunity to take revenge.” These German fears, be it noted, were expressed at a time when, according to our power index, the Germans had well surpassed the French and had become the dominant power in Europe.

12. Testimony to Russia’s weakness was her defeat by Japan in 1904-1905.

strength would make England strong even though her army was the smallest of the great powers. But economic and technological factors also strengthened the French position: France was more militarily prepared than England, and she was far more modernized than Russia. In each of these categories, Germany was second or third. She was the industrial and demographic equal of France, 1870-1880, but she remained inferior to Britain economically. She was militarily strong in a qualitative sense; quantitatively her effort was less than Russia's and barely the equal of France's.

It is perfectly true that France was very afraid of being humbled by Germany again after 1870; her diplomacy was partly paralyzed, and she displayed few initiatives and little leadership in the ensuing decade. When crises came, she rushed to others for support, as in 1875. But this was because of the traumatic effect of 1870: her confidence was lacking, not her strength. Equally, she was not sure that Germany would not miscalculate through overconfidence. Only a crushing diplomatic rebuff would be sufficient: and this is precisely what Gorchakov and the British administered to Berlin in the "War in Sight" crisis.

Whatever the ground for Schroeder's objections, however, it is not true that greater emphasis on the objective military variables would provide (to his lights) a better intuitive power ranking of nations in the 1870s. Indeed, greater emphasis on strictly military quantities would place Russia at the top, not Germany.

The second major objection to our power index is that it is insufficiently nuanced to take into account the individual situation of each major power. For example, Schroeder contends Russia was strong defensively but weak offensively. Britain could do anything on the seas but could not project power on land. Military exactions were more easily borne by relatively rich countries like Britain; they were harder for Russia or Austria to sustain. The geographic situation of a nation also affects its power. Russia was relatively isolated, and until the twentieth century had only her Western frontier to defend. In the center of Europe, Germany and Austria faced the continual prospect of a two-front war. France had only one major enemy—Germany (though in the 1880s a new enemy emerged on the horizon—Great Britain). If a war broke out in which subject nationalities were an issue, the Austrians would be gravely weakened. France, on the other hand, was a model of national unity and strength even as compared to Berlin, which had continual worry about her subject Polish population. Financial strength operated differentially as a factor in power. Schroeder points out that Italy did not derive strength from her superior finances in 1866, though



Russia was forced to retreat diplomatically and militarily in 1878 because of "impending financial exhaustion" stemming from the Russo-Turkish War.

Undoubtedly there is some way in which such individual factors might be included in a power index. But they would have to operate the same way for each nation: superior finances could not be a help to one and indifferent or a hindrance to another. Long frontiers would constitute either strength or weakness. If one's capital was at a great distance from the border of another state, this would seem to increase defensive power. Geographic or insular barriers would appear to facilitate defensive, but limit offensive, power. The greater number of possible opponents (roughly measured by the number of national frontiers one had to defend), the less the power. When we proposed the 25-variable power index in 1974 we did so provisionally and tentatively, aware that it would have to be refined and developed. Compared to other projects which relied on population and pig iron (as the sole indicators of power) it was a signal advance. We remain committed to improving it.

It is, however, very difficult to convince oneself that the inclusion of such individual factors would produce a different ranking on the index. Russian and British strength would increase on the defensive side. Austria, with extreme nationality problems, would appear weaker, but she is already fifth on a list of five. Russia could appear either stronger or weaker in the net depending on the emphasis placed on defense versus offense. On the other hand, Germany would be unlikely to rise significantly in the rankings: her central position meant that she continually would have to face the possibility of war on two fronts. She did so with forces that were no larger than those the French deployed on a single frontier. In short, additional and helpful modifications might be made in the power index, but the net effect of the change in actual power rankings would be negligible.

There is a further problem involved in a power index which is designed to do service in a particular era. The more the temporal and geographic context is specifically included, the less generality the index can have. In our original paper we did not expect to derive a power index that could be applied, for instance, to the nuclear age. But some of the elements in the index for the 1870s clearly have relevance today (economic and financial strength, number of men under arms, military expenditure, and so on). If, on the other hand, the index is weighted with factors which appear to apply only to one area of the world and to one time period—if, in short, one really tries to take into account the uniqueness of the Russian situation, British insularity, or the Austrian

problem with subject nationalities—the testing of international theory becomes impossible. Then attempts to judge the adequacy of theories across historical and temporal contexts must in fact be given up. It does not appear to us that the alterations suggested by Schroeder, laudable in themselves, forward this objective: rather they make it more difficult to achieve.

2. The Inadequacy of Status Measures.

3. The Use of Judgments of Military Attachés and Officials as a Substitute for Measures of Power and Status. There is no truly satisfactory measure of international status. Those which have been used by investigators (number of diplomatic recognitions, size of diplomatic representation) have great deficiencies. First, they appear to be negatively correlated with each other, so that no composite measure could be devised. Second, “status” has a kind of ineffability; it is not concrete and material, like power. Since it fluctuates with the tide of events it is not likely to be captured by indices which fluctuate little from year to year. In our original paper we were very clear about its defects and urged the reader to approach it in the spirit of *caveat emptor*.

On the other hand, Schroeder’s suggestion that reports of military attachés and other officials might be used as a substitute for either power or status measures (and possibly both) strikes us as unpromising. He claims that these reports (employed with “necessary precautions”) “yield useful, reliable results” (p. 16). But how “reliable” are they? “Reliability” suggests that informed observers come to much the same conclusions about the status, power, and military strength of the parties. Indeed, if this is not what Schroeder intends, it is hard to see how such reports could be substitutes for the power or status measures used by the Situational Analysis Project. If such reports were “reliable” it would mean that military observers and officials on both sides of a dispute would within limits agree about the relative strength of the contending powers, and the probable outcome of a war. If so, their very reports should be a deterrent to war. Some historians will even go so far as to say that war only occurs when there is a difference in estimates of relative power; when there is agreement about such estimates, war does not occur (Blainey, 1973). But war *does* in fact occur; if so, officials can scarcely always agree. And there is evidence that they do not. On the eve of World War I, there were many differences between the most informed observers. The French and the Russians were generally but not uniformly optimistic. They believed that if Austria were drawn into a Balkan adventure, the Triple Entente might gain a great victory. Poincaré wrote: “the experts view the chances . . . with great optimism”

(Albertini, 1952: 373). In contrast, Moltke told his Austrian colleague in May 1914 that the sooner Russia struck, the better it would be for the central powers, for “any delay reduces our chances” (Fischer, 1967: 49). Both looked forward to a conflict in the short term arising over the Balkans. The French knew of the Schlieffen Plan and aimed to disrupt it by cutting the German salient in Belgium and taking the offensive in the Ardennes and Luxembourg. What they did not realize, however, was that Germany would throw 13 reserve corps into the opening battles. Thus, were the French or the Germans better prepared in 1914? The answer depended on how their power was used, and on this question there was universal uncertainty and great disagreement.

4. The Defects of Alignment. Schroeder also criticizes our attempt to summarize “alignment” patterns for an entire year. He is right in complaining that this compression does violence to intrayear shifts in diplomatic position. It also should be noted that our “alignments” are sometimes very weak, in that they are based on a balance of net favor or net disfavor among the parties. Thus, very mild cooperative balances among several nations put them in the same alignment category. In 1870, 1871, 1872, 1876, and 1880 the general cooperative alignment is based on such weak favorable balances. In 1878, however, Bismarck was trying not to oppose Russia, but in fact he did so, and the Russians were only too aware of this fact. Thus, the alignment measures retain meaning and significance even though they could be used more precisely.

## CONCLUSION

We trust that our rebuttal to Schroeder will not be read (for certainly it is not intended) as a general dismissal of his points. Diplomatic historians have much to say to the practitioners of international relations. Where indices can be refined and made more incisive, they unquestionably should be improved. Each quantitative investigation into international phenomena faces major problems of validity: the question whether the indices used accurately represent the concept or category in question. We do not believe that we have solved these problems; no quantitative project has. We do believe, however, that unlike many quantitative international inquiries, the Situational Analysis Project has taken the problem of validity seriously. Schroeder argues that diplomatic historians should have been in on the quest. Two diplomatic historians and two political historians were in fact involved in it, and we took their advice very seriously. At each point we asked whether our scales were realistic, our coding acceptable, our actual scaling justi-

fiable. We received support and acknowledgment on all these points.

Since Schroeder will have an opportunity to respond to our rebuttal, however, we would like to offer a challenge to him. He makes clear that his purpose is not a wrecking operation, but rather to propose useful criticisms that could facilitate desirable quantitative international work on the balance of power. This means that he should be ready to suggest means by which "international processes" might be capable of quantitative treatment. We state here our belief that the suggestions in his article are not helpful to this end. His suggestion that we abandon studying events and revert to "intentions" is vain, as proved in historical work itself. The measures of "power" which he offers are neither undisputed nor subject to quantitative interpretation. The processes which he commends would compound the well-known reliability problem. For procedures to be "reliable" they must be capable of being applied in the same way and with the same results by all competent investigators. But all of his suggestions are invitations to unreliability: to let investigators do it any way they may. If he could state in his rejoinder precisely how he would cope with both validity and reliability problems in a quantitative strategy for the investigation of the balance of power, we would be much in his debt.

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