PREDICTION, EXPLANATION, AND THE SOVIET EXIT FROM THE COLD WAR

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Given the lively and wide discussion on this topic -- largely stimulated by the thoughtful and thorough essay by John Gaddis (1992) -- many of us in the world politics research community might understandably prefer to keep a low profile, if not change the subject entirely. To walk away from the discussion would, however, be unseemly, for several reasons. First, of course, a central tenet of serious scholarship is honest confrontation of our models and methods, premises and conclusions; without for a minute endorsing adversarial proceedings as an adequate route to the truth either in legal proceedings or in scientific discourse, I nevertheless appreciate that such can be not only heuristically useful, but also a source of liveliness in a profession that often verges on the solemn if not the tedious. Second most of us who study politics as historians or political scientists fancy ourselves as relatively competent pundits on current affairs; how many of us can resist the temptation to sound off in front of the TV camera or in the columns of the *Times* or the *Post*? Third, a good many of us consciously identify with the policy science orientation especially as articulated by Lasswell and Lerner (1951), and this includes those of us who serve as government consultants, work in the think tanks inside or outside of the beltway, consort with the peace research community, and teach in public policy programs. Yet a fourth justification for joining in the discussions is the scientific one: that a major criterion of our scholarly work is its predictive power. As my title suggests, I will return to this issue later in the paper.

Failure in Prediction: Who and How Come?

A serious discussion of the winding down of the cold war might well begin with the distinction between the demise of the Soviet state on the one hand and the end of the Soviet-American armed rivalry on the other, linked together largely by the dramatic shifts in Soviet foreign and military policy; in other words, *three* related but different sets of predictors are up for discussion. Further, the sorts of predictions that were made in the months, years, and even decades before the early indications soon after Gorbachev's election by the party leadership, will rest on three different sets of understandings. One is the most obvious: the structure, culture and dynamics of the Soviet domestic system. Second -- and this should be equally obvious -- is our understanding of world politics in general. Third and least obvious

is the way in which we come to the central issue in our discipline: how we model and explain the intimate interface between domestic systems and the global system.

Before looking at some of those scholars, analysts, and practitioners who might have been expected to anticipate and alert us to the demise of the Soviet regime, their retreat from the enduring rivalry, or the end of the cold war system, we might pause for a moment to consider the "so what?" question. If a fair fraction of our colleagues yawn when these questions come up, perhaps they *are* "merely academic". In my view, the inability of most Western analysts to predict these events -- or to take more seriously the predictions and auguries coming out of the East -- was indeed costly in the extreme. First and foremost, it could have been catastrophic because it perpetuated the costly arms race for at least another half-decade, and worse yet, provided an appreciable number of additional occasions for the initiation of nuclear war. In "Missiles of October, 1988" (Singer, 1986) for example, I discussed the continuing menace of aerial probes of NATO and Warsaw Pact air space and anticipated that the downing of an intruder would lead to a major confrontation. In that scenario, I furthermore have the US launch a nuclear missile strike against the East bloc installations in Karl Marx Stadt and that the Soviets, instead of retaliating, choose rather to publicize, dramatize, and chastise the US, leading to a vote of censure in the UN Security Council, widespread revulsion at the devastation and civilian fatalities ("collateral damage" in the jargon of strategic deterrence), an overwhelming defeat of the Administration in the following month's election, and a rapid winding down of the armed rivalry. In other words, I not only lamented the continuing cold war, but also predicted its abrupt termination for the correct year, albeit via a very different sequence of events.

Second, there is the cost associated with the thought, energy, and resources that were devoted to managing the East-West contest, when they could have been allocated to more worthy tasks. Every day, week, and month that this contest was continued meant that much more in misallocated energies to the mindless and self-perpetuating activities of military preparedness and political myopia, and that much longer a delay in turning to the vital needs elsewhere in the global system. And it should be understood that the cold war not only deferred a turning to these priorities, but also that it was making matters worse as well. Vital to this understanding is the cost inflicted by the superpowers on the rest of the world: allies that had to be kept in line, and persuaded to over-allocate to the military, suppress tendencies to seditious ideas or consideration of a separate peace, and so forth. But the cost to the non-aligned societies was monumental, even though the compradors were well rewarded by the Soviets and Americans. Both superpowers interfered in the ex-colonial regions, making and breaking governments, looking for reliable allies or denying access to the other, loading them down with weapons, and in such dramatic cases as Vietnam and Afghanistan actually intervening massively and destructively in their civil wars (Singer, 1996). The tragedies unfolding in those regions today can largely be traced back not only to the original colonizers but also to the more recent Soviet-American struggle.

Yet a third consequence of our failure to anticipate the major changes in the USSR, and recognize and respond to them more promptly is the tumult and instability of that region

now. Had the Western powers and their analysts appreciated the power shift in the Kremlin, and reacted to its foreign policy implications in a timely and positive fashion, the transition might have been more gradual. Having failed to do so, we probably deprived the Gorbachev-technocrat reformers of the reinforcement they needed to move at a pragmatic pace at home and abroad (Kegley, 1994).

We could go on at considerable length as to the observed, as well as the possible, cost of perpetuation of the East-West rivalry beyond those windows of opportunity that appeared every few years after Stalin's death in 1953 until Gorbachev's initiatives from 1986 onward. And one reason that these opportunities were inadequately recognized and repeatedly ignored is that the conventional wisdom -- in both East and West -- had no room for the possibility of a Soviet retreat from the fray, and few incentives to even contemplate the possibility (Hopf, 1993).

Whom should we have expected to predict this dramatic turn of events? First of all, scholars and analysts from the USSR or its closer allies should have been the first to suspect (Rocca, 1978). To begin, there were all of the long-standing indications that the economic system was in a shambles; that its technology, even in the military sector, was largely obsolete; that its farms were unproductive; that the universities had become stultified; that the bureaucracy was dominated by a self-serving nomenklatura: and that most of the citizenry was dissatisfied, sullen, and ready for change. Another clue was that the Kruschev reform efforts, while clumsy and inadequate, might have succeeded were it not for the Cuban missile crisis and other foreign policy failures. But the clearest evidence of impending change was the rise to power of Gorbachev and his Foreign Minister Shevardnadze. Even though the American elites sat slack-jawed and cynical through the domestic and foreign policy initiatives of 1985 and 1986, some of the natives must have made correct inferences. We know, of course, that a few of them did, but were reluctant to tell what they saw; seven decades of political conformity can generate a lot of prudence. To be sure, there had been a few Russians such as Znakov (1966), Amalrik (1970) and Solzhenitsyn who went public with their jeremiads and suffered exile and worse as a result.

Turning to those of us in the West, to whom these charges are really addressed, the worst performance of all was the so-called intelligence community. Generously funded and politically lionized, these people were trained and paid to observe, analyze, and anticipate the Soviet scene. But a moment's reflection should make us less shrill in our criticism. To begin, most of the East Europe "experts" in the CIA, DIA, State, and Pentagon were trained in the most conventional manner, meaning little if any exposure to the scientific style, reminding me of a phone call several years ago from a colleague in Langley; he wanted to recruit a new Ph.D. in world politics, but emphasized that since it was a "policy position," no quantitative methods were required. Inasmuch as the significance of this will not be obvious to all, it will be addressed below. To be sure, some individuals in the Directorate of Intelligence -- if not the Directorate of Operations -- must have come close to understanding the processes under way in the Soviet Union, but the party line in the "free world" was only marginally less intimidating than that on the other side. And by the mid-

1980s, the US foreign and military policy establishment had already been well-purged of those who questioned the premises and the policies of the cold warriors, if not over China policy in the early 1950s, ballistic missile defenses and then the Vietnam intervention of the mid-1960s, and Pershing deployment in the mid-1980s; there were all sorts of grounds for wondering whether one or another of us "fully understood the nature of the Communist threat."

Another group whose members might also have done better were the journalists. While untrained in the social sciences, or perhaps even the history or culture of the regions they were assigned, media people in the West typically pride themselves on being on top of contemporary events and conditions. And, even if neophytes when they first arrive, they tool up quickly, hang around with the veterans, get to know some of the natives, and soon become "inside dopesters" (Reisman, 1950). Whether working close to home in the West or out in the field in Eastern Europe, however, they soon discern the party line, and with no need for formal indoctrination, become comfortable operators on what Lenin called the transmission belt of public opinion (Breed, 1955). To be more accurate, one might use the past tense here inasmuch as American journalists seem in the past few years to be moving to a more independent and responsible role in foreign affairs since the demise of the USSR. But such cheerleaders as Cronkite, Reston, the Kalb brothers, and Richard Burt all come to mind as people who had acquired Max Weber's "trained incapacity" to analyze and comprehend affairs in the Eastern bloc. And as Dorman and Farhang (1987) document in the context of the Iranian hostage case, US press coverage of the issue could best be described as "the journalism of deference." And in the Desert Storm assault we saw the same syndrome, with American reporters covering that sad episode as if it were an athletic event such as the Olympics, and they were there to cheer on our team. Thus, given the rewards of conformity -- and several of those mentioned above actually became members of the official foreign policy establishment -- it comes as no surprise that our media people fared little better than their governmental brethren in anticipating the dramatic changes that were soon to come in Soviet foreign and security policy.

In response to the Gaddis focus, we should now take a closer look at the academic community (Crawford and Biderman, 1976). Let me begin, however, by noting that a fair number of Western academics had been raising questions as to the viability of the Soviet system, and even going so far as to explicitly predict the collapse of the regime. For instance, invited to a 1985 conference organized by the Professors World Peace Academy in Geneva, on "Post-Soviet Russia," several of us declined on the grounds that it was sponsored by "the Moonies," and legitimized by quite a few ardent cold warriors, distrusted because they used the brutality in the Russian empire as the justification for a belligerent and provocative foreign policy against them. On reading the four-volume collection (Shtromas and Kaplan, 1988) emanating from that conference, it is evident that this turned out to be a serious and responsible enterprise, and I owe an apology to the organizers.

As the above suggests, academics in the West were by no means uniform in their outlook on the USSR, but a few impressionistic generalizations seem appropriate. Looking

first at those who were specialists in Eastern European matters and/or "Communism," it comes as no surprise that most of them accepted the conventional cold war interpretation. One might expect this convergence with the orientation of the foundations that funded many of them, the media that quoted and lionized many of them, and the government agencies, war colleges, and think tanks that legitimized and reinforced them. Worth noting, of course, is that this symbiosis became increasingly solidified thanks not only to the frequency with which the Kremlin acted in accord with these worst case analyses at home and abroad, but also thanks to the indoctrination of more than a generation of our university students, a good many of whom went on to staff the agencies, the committees of Congress, the foundations, and the media. The consensus was something to behold: the USSR was a totalitarian, atheistic monolith, imperial and expanding, and moving to the day when it would "take over the world" unless the "free nations" remained vigilant, unblinking, and armed to the teeth. As noted above, there were some exceptions, and somewhat surprisingly their work often appeared *in Problems of Communism* published by the US Information Agency. Parenthetically, as an occasional speaker traveling abroad for USIA, I was often struck by the political sophistication and diversity of its staff in Europe and Asia, in contrast to the more simplistic and homogeneous briefings given to me by Foreign Service Officers. Another contrast worth noting is that West European scholars were consistently less rigid and dogmatic about the Soviet system than their North American counterparts; they had more contact with Soviet colleagues, heard some of their dissenting voices, and thus tended toward a more differentiated and complex model of that benighted society.

Another group of academics that largely failed to appreciate the complexities and the weakness of the USSR was the military strategy / international security fraternity (and sadly, sorority). This group, while distressingly homogenous in its premises regarding the Soviet regime, their national security establishment, the dynamics of the armed rivalry, and even the "nature of" international politics, arose out of quite diverse backgrounds. Along with the usual suspects from political science, we had "defrocked" economists, physicists, biologists, mathematicians, and all sorts of engineers. From a social scientific perspective, this was a truly amateurish aggregation, but what they lacked in historical understanding, political sophistication, and epistemological standards, they more than made up in political legitimacy and bureaucratic clout inside the beltway. With, once more, the happy exceptions, these "defense intellectuals" bought into the "realist" school, worst case analysis, technological fix, and a touching faith in the theology of strategic deterrence, all of this informed by a view of the USSR remarkably similar to a provincial party apparat's view of the US. Nor could the Pentagon or the weapons labs come up with a weapon system they didn't like, from tactical nuclears to MIRV to BMD; even the Reagan "Star Wars" system still commanded an impressive measure of support in those circles as late as 1985-86.

As to this motley crew being able to consider -- no less anticipate -- any changes in the enemy's society or his foreign / military policy, the chances were poor indeed (Jensen, 1972). Even if one granted the most cherished assumptions of that community in and out of uniform, one could demonstrate the careless reasoning of their strategic doctrine. I tried in

a book (Singer, 1962) and a dozen or more scholarly or popular articles, and endless conferences including Pugwash, but to little avail. Not only was there the fear of being "soft on Communism" along with a host of other peer pressures. There were the perks of having a top secret or Q clearance, generous consulting fees, "research" contracts covering one's grad students and summer salary, the opportunity of intimating that "if you knew what I know, you wouldn't question my statement," access to marvelous " I was there" anecdotes to awe your students, and of course, close proximity to the nomenklatura! In sum, it is no surprise that these peripheral beneficiaries of America's most generous entitlement program found it easy to assume that the cold war would happily continue -- at least until they retired!

In closing this section, and before moving on to those political scientists who are singled out for some unwanted attention by Gaddis, one might want to add one group to those discussed above. Reference is to the professional historians who are not really specialists in a given region of the world or given period of time. Ranging from the old timers like Toynbee and the senior scholars of today like McNeil to those such as Paul Kennedy, Michael Howard, Peter Paret, and Gaddis, I'm not aware that any of those people did any better than the structuralists, evolutionists, and behaviorists in the world politics field to whom we will shortly turn. As a matter of fact, Kennedy (1987) was one of those historians who, on the one hand, had the courage to make some predictions a few years into the Gorbachev era, but the bad luck to predict a Soviet empire that would continue -- and remain a heavily armed super power.

The Methodological Critique: Prediction, Explanation, and Causality

Having identified some of those who might have or should have anticipated that the Soviet would in due course abandon the long, dangerous, and costly struggle with the US, if not fully embrace the policies of self-abnegation (Wolfers, 1951), there are three additional groups to whom some consideration might be appropriate. I have in mind the schools of thought in what Gaddis calls "international relations theory" even though we: a) look at global system actors other than *nations*: b) analyze not only *relations* but behavior and interaction; c) and certainly have produced no body of codified knowledge deserving to be labeled as *theory*. Leaving aside the semantic imprecisions in the world politics field (my preferred label) and certainly absolving our historian colleagues in this matter, what are these schools of thought or approaches, and what might be said in response to the Gaddis critique?

His targets are the behavioral, the structural, and the evolutionary approaches, and he singles out a few of us who best represent, in his view, these schools of thought. It should come as no surprise that this trichotomy is troubling, given that they are in no way mutually exclusive. One dimension is methodological, the next reflects biases toward levels of aggregation and classes of variables, and the third is responsive to our judgments as to the significance of the past and whether we see history as evolutionary, cyclical, or stochastic. Thus, I am far from alone in identifying with all three approaches. Nevertheless, it is the

methodological dimension to which he devotes his most interesting passages and to my writings as most representative. This is both flattering and challenging, and I will thus try to address his critique in a relaxed and non-defensive response after a few preliminary points have been addressed.

Of necessity, we ought to begin with another set of distinctions, perhaps as elusive as the above; reference is to the Freeman and Job (1979) discussion of scientific forecasting and their attempt to differentiate between a forecast and a prediction. Having now re-examined that paper and the exchanges that resulted, let me confess that I remain confused, and thus stay with the more conventional distinction, in which the so-called "point prediction" is central. By this, we mean predicting a specific event in a specific place at a fairly specific time: French troops will leave Rwanda before the next session of the UN General Assembly, or China will make a public claim to the Spratly Islands following the next naval engagement with Vietnamese ships. A more general prediction would be that at least ten states will have missile-deliverable nuclear warheads by the end of this century, or that all the Nordic states will be members of the European Union by 1999. More general still is the forecast: the frequency of intra-state war will continue to increase world wide during the next three decades, while the frequency of inter-state war will decline.

Perhaps a more useful distinction, especially from the policy science and applied science perspective, is that between the contingent and non-contingent prediction or forecast. Soothsayers, Delphic oracles, astrologers, disciples of Nostradamus, or religious fundamentalists all specialize in the non-contingent version. They typically tell us what will happen in the physical or social world, often with a firm date or a time limit. Futurologists also do so, along with serious scientists, but they seldom set down an "if, then" condition, specifying that if we do such and such, the result will be thus and so. But from the policy science perspective, the conditional or contingent prediction is appreciably more useful, indicating which particular policy actions are most likely to culminate in which outcomes. While political elites sometimes do nothing but wait for some desired or feared eventuality, more often they tend to advocate or make decisions and execute policies that will enhance the probability of some desired event, or perhaps delay the arrival of one that is less desirable. To put it simply, the name of the game in public policy is the contingent prediction, and the more accurate these predictions, the more successful and adaptive will be the policies that are chosen. An appropriate example is Kennan's "Sources of Soviet Conduct" in which he predicted that if the West were to deal with the Soviets in a firm but not provocative fashion, they would be contained and ultimately play a more conventional major power role.

An interesting and important twist on the contingent prediction is the indicator of early warning or timely assurance (Singer and Wallace, 1979; Singer and Stoll, 1984). Here, instead of seeking only to predict the *consequences of a policy act*, we also seek to predict circumstances and then to take advantage of that early warning prediction by moving to head off or ameliorate the "bad news" via preventive diplomacy, or avoid any action that might reverse or deflect the trend toward some timely assurance of a "good news" outcome.

Needless to say, the policy usefulness of any type of prediction -- irrespective of the purpose and intentions of the user -- is the accuracy and reliability of the prediction. The most solid basis for prediction is a bona fide theory, a codified body of knowledge that has been demonstrated to explain a given class of outcomes from the onset of crises to the consummation of treaties to the winding down of enduring armed rivalries. But in the absence of such bodies of codified knowledge -- and given the way in which most policy-oriented research in world politics is conducted -- that will be a long time coming. To illustrate, in 1994 the Carnegie Corporation announced formation of a "Commission on Preventing Deadly Conflict," and while the documents that have emerged seem reasonable enough, there are fewer than ten references to any of the several hundred data-based articles or books on the subject that have appeared in the past three decades; nor does one find among its research staff or advisory council members any one who might have ever conducted a piece of scientific research in the world politics field.

In the interim, there remains a fair array of adequate methods -- which when combined with the experienced and judicious intuitions of practitioners and observers -- can be helpful to prediction in the short run. These include simple extrapolation in which we merely extend any observed trend line such as the annual number of militarized disputes or growth in military expenditures in the ex-colonial world. This can, of course, be quite risky not only because history is not just one unbroken trend or another; almost all social trends follow the logistic curve pattern, meaning that they eventually approach an upper or lower asymptote and level off or reverse direction (Russett, 1965). Further, conscious intervention -- such as that of the Soviet elite in the case at hand -- can effectively interrupt or divert many trends in a significant way. The same can be said of extending cyclical patterns, especially since cycles are highly dependent upon a rather delicate mix of self-correcting and self-amplifying mechanisms, a small change in the magnitude of which can dramatically upset or constrain a long-running pattern of ups and downs.

Another is the Delphi method, by which we try to systematize conventional forecasts made by the usual mix of practitioners, think tank analysts, and academics (Helmer, 1966). This method of pooling the received wisdom of the moment may tell us a lot about the suggestibility and conformity of the respondent "experts," as in the Ash (1952) and Sherif and Hovland (1961) experiments, but is probably inferior even to the "devil's advocate" method in which one or more specialists might be assigned to challenge an agency's predictions before they are acted upon. Then there are several simulation methods, ranging from the "gaming" version in which practitioners, journalists, academics, and students are assigned roles as decision makers and then play out various scenarios. While heuristically suggestive, this is markedly inferior to the computerized simulation which, when well-done, rests on operational measures of key variables as well as explicitly stated decision rules reflecting some level of hard evidence as to which kinds of actors make specific types of decisions under a range of classifiable circumstances.

Our discussion so far has largely attended to the policy implications of theory-generated, as well as less powerful, prediction methods. Given my belief in the applicability

of social science findings and the researcher's responsibility for producing socially redeeming knowledge, this is as it should be. At the same time, though, this is not to embrace the widely accepted corollary that the 'acid test' of a good theory is its ability to predict. As already suggested, there is a good variety of bases upon which we can build fairly reliable predictions, and this includes alleged theories that are something less -- often a collection of loosely related assertions -- not to mention coherent and well-supported theories that turn out to be wrong. In the physical and biological sciences, there are some interesting examples, of which the Ptolemaic theory of astronomy with the earth as the center may be one of the more dramatic. It was empirically incorrect and produced erroneous explanations, but worked well enough for predictive proposes in the pre-Newtonian epoch. The point then is that prediction is often easier to achieve than explanation, and even if one's alleged theory does meet the test of reliable prediction, this is an insufficient standard.

Turning, then, to explanation, let me be brief and necessarily assertive, despite the reams that have been devoted to these issues. I begin with Hempel (1965) in rejecting any reliance on "causality," a metaphysical concept invented to help us believe that we comprehend certain complex phenomena. First of all, it doesn't exist in the referent world, at least in the deterministic sense of the word. From any given set of initial conditions, a variety of outcomes can ensue, and conversely, any given outcome can arise out of a variety of conditions and processes. Given this empirical reality, which of the alternative scenarios shall we identify as the causal one? Secondly, we don't even need cause in developing and evaluating our theories; explanations, despite Dessler's (1991) thoughtful insistence, will suffice, and in my judgment, *must* suffice.

Explanation here has two elements. First, it offers a coherent narrative from predictors to outcome variable, resting on a detailed description that ultimately must be sufficiently reductionist to include human perceptions, preferences, and predictions (Singer, 1968). Second, and equally controversial, the ultimate test of an explanatory theory is an inter-subjective one: does a sufficiently large sample of substantively knowledgeable and methodologically competent scholars agree that the narrative is consonant with extant data-based findings and that the procedures by which the variables are linked are sufficiently operational and appropriate? As is evident, there is no appeal here to any teleological, cosmological, or mystical doctrine; the validation of a proposed theory requires the assent/acquiescence of an ill-defined and self-selected jury of peers. From my perspective, constructing the narrative and demonstrating its general validity is a tough enough assignment, compared to which an accurate prediction is a relatively simple matter.

Summary

Let me try to summarize the possible explanation for our general failure in the West to anticipate the "end of the cold war". As I have already indicated, not everyone was taken by surprise, and during the first year of the Gorbachev-Shevardnadze regime the number of

"believers" rose appreciably. And, remarkably, not all of us came from the more dovish end of the spectrum; some saw the Soviet initiatives as a sign of weakness and Western opportunity to "tighten the screws," while others agreed that a sense of declining power on their side was partly at work and offered substantial prospects for a new detente and a reduction in the dangers and costs of maintaining the armed rivalry.

Leaving aside those few on both sides of the cold war who suspected or understood that the regime in Moscow was headed for trouble well before 1985-1986, and that larger number who came to that position around the Andropov-Chernenko-Gorbachev transition, how do we account for the fact that most did not or could not see the auguries. To begin, the Gaddis explanation is fundamentally flawed for the simple reason that the theorists in world politics -- be they behaviorists, structuralists, evolutionists or several of the above -- were far from alone. While in principle the behaviorists had the methodological advantage and thus should have been the first to identify the signs, the fact is that few of us were concentrating on the USSR and its foreign policy at the time. I suspect that a count of the articles in the more rigorous journals (*Journal of Conflict Resolution*, *International Studies Quarterly*, *Conflict Management and Peace Science*, and *International Interactions*) would show that most of that research was focused on the search for general regularities embracing many states and regions over a century or more of diplomatic and military events and conditions, rather than on this single relationship.

My point, then, is that those whose methods were more rigorous would have been the first to notice, had we been giving the question more attention. I certainly recall *thinking about* an investigation into the winding down of armed rivalries since the Congress of Vienna, and discussing it with colleagues on and off for years, but none of us ever turned to such research in a serious way. Thus, I concede here that if those whose professional interest was the Soviet Union had been studying this subject in a manner that was methodologically rigorous, historically oriented, and theoretically sensitive, some of them might well have seen what was developing in that tragic society.

This leads, in turn, to a second conclusion: the western study of the Soviets was "politically correct" beyond belief -- and I use this expression to ridicule today's conservatives and reactionaries who use it to disparage advocates of social justice and equality, a non-hysterical approach to crime, humane immigration policies, and a world-wide concern for human rights. That is, the US was in the grip of an exaggerated fear of the USSR and "communism" that was legitimized and institutionalized by the political parties, federal agencies, corporations, labor unions, the universities, the foundations, and even the entertainment business. Students were treated typically to one-sided, shrill, dishonest, and ideologically - driven teachers and texts at all levels of education, from grammar school to graduate school. For younger scholars getting a Ph.D., applying for fellowships and research grants, appearing at professional society meetings, publishing in the mainstream journals, and even publishing books encouraged a mind-numbing conformity. And coupled with this incredible conspiracy of acquiescence was the epistemological party line: rigorous methods and reproducible evidence were inappropriate to the study of "closed societies," as if the

only choice were face-to-face survey research interviews or writing pseudo-scholarly pieces in the embrace of the cold warriors' symbiotic propaganda. "Disinformation" was hardly a Soviet monopoly.

In light of the arguments developed in this paper, it is little wonder that scholars, analysts, and practitioners in the west -- especially in the US --so dismally failed to anticipate these dramatic events. Our models were corrupted, our methods too primitive, and our incentives distorted -- and little is happening now to suggest that we will do much better in the far more complicated and equally dangerous post-Soviet epoch.

More of the Same?

At the risk of over-simplifying, there is already ample evidence that the next cottage industry for western students of world affairs will be "ethnic conflict" in general, probably spilling over to Islam-bashing in particular. Furthermore, the conceptual imprecision and epistemological naiveté almost surely will equal that of the cold war period. In using so empty a phrase as ethnic conflict, we seem to suggest that the mere difference between adjacent ethnic identity groups -- whether based on language, religion, or some other cultural differentiation -- is sufficient to propel them to severe conflict or war. This is foolish as well as unfortunate; enough recent research indicates that unless such differences are effectively exploited by political elites, ethnic boundaries have little effect on the relationship between culturally divergent neighbors (Henderson,1997; Collier and Hoefler, 1996).

As to the next enemy of western civilization, there seem to be two plausible candidates -- Islam and China -- but this current preoccupation could readily turn into "the west versus the rest". Less important, however, than the identity of the new enemy is the simple mindlessness of the emerging paradigm; in place of political ideology as the fulcrum of the crusade, we will have ethnicity. For a few years, it looked as if our emerging obsession might be a logical extension of the cold war, with democracy - versus - communism converted to democracy - versus - autocracy, riding on the happy doctrine that democracies are very peaceful, that democratic dyads rarely war against one another, and that by building a new world order composed entirely or largely of democratic and free market societies, war would be a relic of the past. That particular conceit will, I suspect, go into decline and gradually merge with the broader notion of confrontation across civilizations, in which the mantle of Toynbee (1948) is taken up by Huntington (1996). It is this type of simplification, married to some sort of Hegelian doctrine of struggle and confrontation, that will becloud our vision in the coming decades.

But as I suggest earlier, a distorted and primitive world view is only part of the problem, and it can often be ameliorated by a sufficiently sensitive epistemology. The real source of our trouble in dealing with the Soviet menace after World War II (and it *was* a menace, but considerably less potent than made to appear in the west) was the relative absence of a critical and skeptical epistemology. Students of world affairs were no better

equipped than lawyers, politicians, journalists, and generals to evaluate the conventional view of the USSR and its Eurasian sphere of influence, and as a result we helped to legitimize that perspective.

This time around, let us move more cautiously to any one interpretation of the cleavages and complexities in the changing global system. To do so, I would suggest that we need to attend to two sources of confusion. One is to recognize the material, social, and psychic incentives that often propel our taste-makers toward one or another primitive world view. The other is to cultivate a more careful, rigorous, and scientific set of criteria by which we propose, evaluate, and embrace or reject contending world views.

References

Amalrik, Andrei. 1970. *Will the Soviet Union Survive Until 1984?* New York: Harper and Row.

Asch, Solomon E. 1952. *Social Psychology.* New York: Prentice-Hall.

Berry, Nicholas. 1990. *Foreign Policy and the Press: An Analysis of the New York Times Coverage of U.S. Foreign Policy.* New York: Greenwood Press.

Breed, Warren. 1955. "Social Control in the Newsroom." *Social Forces*, Vol. 33, pp. 326-25.

Carnegie Commission on Preventing Deadly Conflicts. 1997. *Final Report.* Washington, D.C: Carnegie Commission.

Collier, Paul & Anke Hoeffler. 1996. "On Economic Causes of Civil War." Oxford, United Kingdom: Center for Study of African Economics (mimeo).

Crawford, Elisabeth, and Albert D. Biderman, eds. 1976. *Social Scientists and International Affairs.* New York: Wiley & Sons.

Dessler, David. 1991. "Beyond Correlations: Toward a Causal Theory of War." *International Studies Quarterly*, Vol. 35, pp. 337-55.

Dorman, William and Mansour Farhang. 1987. *The U.S. Press and Iran: Foreign Policy and the Journalism of Deference.* Berkeley: University of California Press.

Freeman, John R. and Brian L. Job. 1979. "Scientific Forecasts in International Relations: Problems of Definition and Epistemology." *International Studies Quarterly*, Vol. 23, pp. 113-143.

Gaddis, John. 1992. "International Relations Theory and the End of the Cold War." *International Security*, Vol. 16, pp. 5-58.

Helmer, Olaf. 1966. *Social Technology.* New York: Basic Books.

Hempel, Carl. 1965. *Aspects of Scientific Explanation.* New York: Free Press.

Henderson, Errol A. 1997. "Cultures or Contiguity: Ethnic Conflict, the Simliarity of States, and the Onset of War." *Journal of Conflict Resoultion*, Vol. 41, pp. 649-668.

Hopf, Ted. 1993. "Getting the End of the Cold War Wrong." *International Security*, Vol. 17, pp. 202-208.

Huntington, Samuel P. 1996. *The Clash of Civilizations and the Remaking of World Order*, New York: Simon & Schuster.

Jensen, Lloyd. 1972. "Predicting International Events." *Peace Research Reviews*, Vol. 4, pp. 1-65.

Kegley, Charles. 1994. "How Did the Cold War Die?" *Mershon International Studies Review*, Vol. 38, pp.11-46.

Kennedy, Paul. 1987. *The Rise and Fall of the Great Powers: Economic Change and Military Conflict From 1500 to 2000.* New York : Random House.

Lasswell, Harold and Daniel Lerner. 1951. *The Policy Sciences: Recent Developments in Scope and Method.* Stanford, California: Stanford University Press.

Reisman, David. 1950. *The Lonely Crowd; A Study of the Changing American Character.* New Haven: Yale University Press.

Rocca, Gordon L. 1978. *Policy Sciences in the USSR: The Case of Soviet Social Prognostics.* Ann Arbor, Michigan: University Microfilm.

Russett, Bruce M. 1965. *Trends in World Politics.* New York: Macmillan.

Sherif, Muzafer and Carl I. Hovland. 1961. *Social Judgment.* New Haven: Yale University Press.

Shtromas, Alexander. 1981. *Political Change and Social Development: The Case of the Soviet Union.* Frankfurt: Peter Lang.

Shtromas, Alexander and Morton Kaplan, eds. 1988. *The Soviet Union and the Challenges of the Future.* New York: Paragon House.

Singer, J. David. 1962. *Deterrence, Arms Control, and Disarm*ament. Columbus, Ohio: Ohio State University Press.

Singer, J. David. 1968. "Man and World Politics: The Psycho-Cultural Interface." *Journal of Social Issues*, Vol. 24, pp. 127-56.

Singer, J. David and Michael Wallace, eds. 1979. *To Augur Well: Early Warning Indicators in World Politics.* Beverly Hills: Sage.

Singer, J. David and Richard Stoll, eds. 1984. *Quantitative Indicators in World Politics: Timely Assurance and Early Warning.* New York: Praeger.

Singer, J. David. 1986. "The Missiles of October – 1988: Resolve, Reprieve, and Reform." *Scandinavian Journal of Development Alternatives*, Vol. 5, pp. 5-13.

Singer, J. David. 1996. "Armed Conflict in the Former Colonial Regions: From Classification to Explanation" in van de Goor et al, eds. *Between Development and Destruction.* Hague: Netherlands Institute of International Relations.

Toynbee, Arnold J. 1948. *Civilization on Trial.* Oxford, United Kingdom: Oxford University Press.

Wolfers, Arnold. 1951. "The Pole of Power and Pole of Indifference." *World Politics*, Vol. 4, pp. 39-63.

Znakov, Fyodor. 1966. *Arkhiv Samizdata #374.* Radio Liberty.