



Nigel Gould-Davies, “Rethinking the Role of Ideology in International Politics During the Cold War,” *Journal of Cold War Studies*, Volume 1, Issue 1 (Winter 1999): 90-109.

William Wohlforth, “A Certain Idea of Science,” *Journal of Cold War Studies*, Volume 1, Issue 2 (Spring 1999): 39-60.

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Brezhnev’s Elephant: Why can’t international relations theory integrate new revelations about the Cold War and the role of ideology in it?

“WHAT ELEPHANT?”

A Soviet joke of mid-1970s vintage, aimed at Leonid Brezhnev’s legendary state of denial about the condition of the USSR and his inability to accept responsibility for it, told of an elephant hunt. After a summit, Brezhnev, Gerald Ford, and other world leaders take a break and go on a safari, where they manage to catch a live elephant. They make up an elaborate schedule for guarding the beast, but when Brezhnev’s turn comes he gets drunk and falls asleep, and the elephant trots away. The next morning, when asked about their catch, Brezhnev guilelessly says, “What elephant?” Astonished, Ford then grills him: didn’t we agree to hunt an elephant? Yes, Brezhnev says, we did indeed. Didn’t we catch one? Oh yes, we did, a fine one. And didn’t we agree to guard it? Of course, we all did. “So,” Ford fumes, “where’s the elephant, damn it?” Brezhnev nods thoughtfully for a moment and asks, “What elephant?”

William Wohlforth deserves our thanks for pointing out explicitly in print what is increasingly evident to observers of international relations theory, that new evidence about the Cold War has been uniformly greeted by IR scholars with the question: What elephant? Scholars of international relations have shown a lack of interest so profound that it is in itself suspicious, and Wohlforth’s article serves not only as a call for more integration of Cold War evidence into IR theory but for a reexamination of the bias against history and facts that seems endemic to political science itself.

NEW EVIDENCE, OLD THEORIES: THE THRILL IS GONE

Wohlforth’s dissection of the reasons behind the “unstudied indifference” to new evidence about the Cold War is admirably clear, and doesn’t shy away from pointing out that much of it lies with the fact that the study of international relations has become mired in theoretical and methodological wars--what one political scientist has called “self-generated debates”--that in the end explain nothing because their aim is to establish the intellectual supremacy of one school or paradigm over another, and not actually to clarify anything about the world or the way it works.

In a particularly striking insight, Wohlforth points out that this indifference has led to a lack of “suspense” among IR scholars about what the archives might reveal, because they are--amazingly--inured to the belief that evidence can change theory. “If scholars thought that historical data about the Cold War might alter the fate of influential theories,” he writes, “surely they would look forward to new releases with some nervous or eager anticipation. This suspense, which is so typical of science, is conspicuous by its absence among scholars of international relations.” There is no suspense because the agenda has shifted from ascertaining truth to constructing theory: the thrill of research that produces a genuine discovery or new understanding has given way to mundane theoretical bickering in an empirical vacuum.

To this, we might add that it has led to a lack of urgency as well about resolving issues that have policy implications for the near future. Does what we know about the Cold War carry any implications for our relations with China? For understanding Indo-Pakistani deterrence? For the ongoing conflict with Iraq? We won’t know anytime soon, given the utter lack of interest IR scholars have shown about translating scholarship into policy. (To be fair, this is part of an ongoing disengagement from actual politics among political scientists overall, but one might think that problems of war and peace would have a particular attraction and urgency.)

Wohlforth also laments the lack of “research that marries knowledge about the international context of the Cold War with deep expertise on Soviet domestic and bureaucratic politics.” This is complemented, he correctly notes, by the fact that historians of Soviet domestic politics “seem interested in anything--culture, identity, historical memory--as long as it is not foreign affairs.”

But this is not new: it is another way of saying that traditionally, international relations and comparative politics, as subfields, do their best to stay off each others’ turf and keep their students separated, with the consequent result that area specialists do little to study the international system and IR specialists make sure that they do not descend into what they would consider the weeds of detail about specific nations. (This problem was so acute during the 1980s that the Ford Foundation actually set up a specific fellowship program to encourage “dual-competence,” i.e., to train Soviet area specialists in international security affairs and vice versa.)

Wohlforth is more than a critic, however. He then takes the lead and shows how in two important areas--power and ideology--new Cold War evidence can inform previous theoretical work on international relations. While both are good examples of places where new evidence can inform old debates, Wohlforth understands that previous schools of thought are likely either to dismiss this evidence or claim that it adds little and disturbs nothing in the previous literature.

“How to break the impasse?” he asks. “The answer is simple: recognize that IR is a historical science.” He’s quite right, of course, but if the IR subfield were capable of making that change, he would not have had to write the article he has in the first place. Wohlforth admits that his suggestion will be resisted by the gatekeepers of the subfield, but sees encouraging trends; I am less optimistic.

POLITICAL SCIENCE, HISTORY, AND “PHYSICS ENVY”

Wohlforth is part of a small group of scholars who are pointing out that Brezhnev's elephant is missing. But I suspect that his direct challenge to the international relations subfield to take historical evidence more seriously will meet with Brezhnev's response, for reasons that go beyond even those he describes.

First, the training of political scientists in general mitigates against the production of the kind of scholars (Wohlforth himself is one of the few) who have a background both in a particular area and in a body of theory. The positive hostility of many academic departments and leading members of the profession-- Robert Bates's remarkable attack on area specialists a few years back is a case in point--to scholars who specialize in deep area knowledge means that the gap between the high theorists and people who actually seek a command of history and facts is growing, not shrinking. (Any area specialist who hasn't been called a barefoot empiricist or even worse isn't trying hard enough or going to enough conferences.) Even the advent of constructivism, which at first glance might seem to counter some of the rigidly formal approaches to IR, actually contributes to the problem Wohlforth rightly calls "the scholarly aspiration to universalism" (although Wohlforth himself might disagree with me on this specific approach).

The barriers to greater integration of historical material are often quite simple, but more formidable than Wohlforth might realize. Consider, as an example, language training. This is essential to reading and assimilating new historical evidence of any kind, but most international relations specialists simply have not concentrated enough on one nation or region to master the requisite level of linguistic ability.

Indeed, many are resolute in their determination to avoid a regional focus and believe strongly in the power of theory to override the need to master historical, cultural and certainly linguistic issues. (I personally recall hearing a senior IR theorist once blandly declare that "you don't need to speak Korean to understand Korea." This will no doubt come as a shock to Korean-speaking scholars, but it is unfortunately a typical kind of comment.) From the standpoint of purely professional self-interest, of course, this makes a great deal of sense: it allows the IR theorist to hit to all fields, and move fluidly from discussing, say, China one day to Chile the next. This universalism is central to the way IR specialists are trained, and I fear that Wohlforth's call for greater attention to historical evidence might well be structurally unattainable in the subfield for the foreseeable future.

But there are other reasons as well that Wohlforth's plea will likely go unheeded. The bias against history is so strong among political scientists--precisely because they see the study of history as "unscientific"--that I have come to wonder whether anything can overcome it. This is borne of insecurity: at heart, I suspect that most political scientists do in fact understand that what we do is interpretive, rather than "scientific" in any way a chemist or biologist would understand that word.

The consequent attempt among social scientists to mimic formal science has rightly been called "physics envy," and it not only explains why political science journals have become unreadable, but it also animates a strong distaste for doing anything that could be construed as historical. Among political scientists, to say that someone works or writes like an historian is almost always

meant as an insult only slightly less dire than the most dreaded word one can use in discussing a political scientist's work: "journalistic." It is no accident that the best works about the end of the USSR, books like David Remnick's *Lenin's Tomb*, were written by journalists, because political scientists simply will not stoop to gathering and assembling facts.

In any case, asking younger scholars to declare themselves partisans of an historical approach to the study of international relations is tantamount to asking many of them to commit professional suicide, at least for now.

SEE NO EVIL, READ NO EVIL

Finally, there is the problem of politics, which Wohlforth does not (and in a scholarly article, probably should not) discuss. But it's evident--we see it here on H-DIPLO regularly--that many scholars simply do not want to know some of the things we're now finding out about the Cold War. Fred Barnes, in a recent review of books by Michael Lind and others, wonders why scholarship on Vietnam seems "frozen in time" and why there aren't more works that take into account new evidence about the war. The answer is painfully obvious: a generation of scholars are politically invested in a particular view of the war and will never revise that view, no matter what new evidence appears.

The same could be said about the Cold War. Doug Macdonald, in a posting here some months ago, wondered if perhaps there was a direct relationship between the attacks on positivist approaches and the growing amount of horrifying evidence emerging from the archives; I think this is exactly what's happening and that it will get worse before (if ever) it gets better.

Indeed, although I regret to end my comments on such an admirable article on such a pessimistic note, I will offer a prediction: the field will move in precisely the opposite direction Wohlforth hopes. The more evidence about the Cold War that emerges from Moscow and other former communist capitals, the more rigidly and obstinately will political scientists, and IR specialists in particular, retreat into yet more complex theorizing.

In sum, too many IR specialists are not capable of reading the new evidence, do not usually have the background to understand them even in translation, and in any case do not want to confront the reality they describe: thus, they will declare them irrelevant, and defend the kind of formalized abstraction that has, to my mind, crippled political science as a discipline and rendered IR theory particularly arid.

"IDEOLOGY IS BACK"

With that said, perhaps I am being overly pessimistic, and that Nigel Gould-Davies' confident assertion that "ideology is back" is a more likely view of the future of the study of foreign policy. Gould-Davies is quite right to note that there is "no clear concept of ideological agency to counterpoise to the Realist approach that discounts the role of ideas in foreign policy," and he proceeds to offer one. Like Wohlforth, he has identified a glaring weakness in the study of international politics, and he, too, calls straightforwardly for a reexamination of the whole issue.

Indeed, it can no longer be avoided, he argues, because “the new sources have largely settled the matter” of whether ideological precepts played an important role in the thinking of Soviet leaders. Both Wohlforth and Gould-Davies note Vojtech Mastny’s apt comment that “there was no double-bookkeeping” in Moscow, no private dismissals of publicly articulated ideologies. (H-DIPLO readers know that to assert any matter related to the Cold War is settled is to invite a torrent of protests that to do so is unscholarly, but Gould-Davies is simply right about this.)

Why, then, is ideology ignored? Gould-Davies finds reasons ingrained in the intellectual traditions of the subfield. He succinctly extracts from previous debates a series of fallacies that have long been used to discredit the inclusion of ideology in the study of foreign affairs, in a section that should be required reading for every student of politics. He ably demolishes them--not hard to do, given how weak these arguments were, but it is something that has long needed to be done.

Common to all of them, he points out, is that they “conflate arguments about ends with assumptions about means they mistake extremism for irrationality.” He then offers in their place quite sensible propositions about what constitutes ideological factors, how to adduce evidence about them, and what role such factors might play in foreign policy.

Perhaps most important is Gould-Davies’ depiction of “ideological states” and how they behave. As he puts it, ideological states “define security in terms of the expansion of their domestic system and threat in terms of the expansion of their adversary’s domestic system.” To a student of Soviet foreign policy, this is (or should be) self-evident. But if even this one direct and intuitive proposition could be absorbed by international relations theorists, it would be an advance in itself for the field.

THE REAL WORLD AND SIM CITY

Still, I fear that Wohlforth and Gould-Davies are unlikely to make many converts among their brethren. Gould-Davies faces a special problem: there is a cultural issue that intersects with the unwillingness of IR specialists to learn particular things about particular countries, and that is the general Western unwillingness to take ideology seriously. Americans in particular, including American academics, simply find it hard to believe that anyone in the “real world” can accept the kind of dogmas that Soviet leaders did.

Still less are they willing to believe that anyone would act on such dogmas, preferring to believe (as Wohlforth correctly notes) that deep down, people are basically alike in the sense they are either rational economic actors or, basically, realists. (I am still astounded to recall that one of my colleagues some years ago visited a senior Soviet official, found his home sumptuously decorated in antiques, and flatly told me: “There’s no way this guy is really a Communist,” as though Communists must of ideological necessity be given to poor taste in interior design.)

Indeed, with the dominance of rational choice theory in many departments, political scientists have been reduced to treating human beings the way the computer game “SimCity” treats its fictional citizens, the “Sims:” taxes or crime go up, the Sims move out, taxes go down or police stations are built, Sims move back in, and so on. These are not people, really, but little

calculating machines, responding to governmental policies (chosen in the game by the player, as mayor) in always predictable and measurable ways. Scholars of this persuasion--and there are many--no doubt would argue that understanding the influence of Marxism-Leninism in Moscow would not only require too much effort (and a lot of painful reading in Russian) but would, in any case, be pointless, since ideology cannot, in this view, overcome the rational constraints placed upon states by the structure of the international system itself.

CONCLUSION: "WHAT ELEPHANT?"

Wohlforth and Gould-Davies each have made a strong, even irresistible, case that the entire of field of international relations should simply stop in its tracks for a moment, and reassess itself in light of a mountain of new evidence. They are pointing at this evidence and in effect pleading with the field to take those materials, and their implications (particularly about ideology) seriously.

I have little confidence that international relations as a subfield can change course at this point: if the collapse of the Soviet Union did so little to reform comparativism (in my view, a more coherent and theoretically flexible subfield to begin with), I doubt that new evidence on the Cold War will reform IR. The materials on the Cold War will remain like Brezhnev's elephant: we all agree they exist, we all agree they're at least superficially interesting, and we all agree that there is a lot more of them to come. But when it comes to altering the way IR theory is studied: What elephant?

But if these two articles by Wohlforth and Gould-Davies are examples of a new kind of scholarship in IR, there may be cause for optimism. Otherwise, IR theory, to echo James Kurth's recent lament, will become increasingly banal, and political scientists will find they have removed themselves from playing any part in what will prove to be a new wave of exciting discoveries about the most important conflict of the 20th century.

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