# The State of International History: Where We've Been, Where We Are, and Where We're Going

Note: This talk was given on August 23, 2010, at a conference on "International Organizations and Institutions: Past and Future Prospects," held at the Norwegian Nobel Institute in Oslo.

There are two things I'd like to talk with you about today. First, I want to talk about how our field—"diplomatic history," as it used to be called, or the history of international relations, or even just plain "international history"—has changed over the last thirty or forty years—that is, even since I did my own graduate work in the late 1960s and early 1970s. And then I want to move on to what many of you might find a more interesting, although not unrelated, topic, and that's the question of how this field *should* develop in the years to come.

But let me start by talking neither about the past nor about the future of the discipline, but about the present. Where do we stand now? What are we to make of the kind of work that is being done today? This is not an easy question to answer in any simple way. I don't know how many of you are familiar with the sentence that Charles Dickens used at the start of *A Tale of Two Cities*—"It was the best of times, it was the worst of times"—but I couldn't get that sentence out of my mind when I was trying to decide what to say here today. In some ways, we're living in a golden age: the best works today—I think, for example, of Adam Tooze's history of the "making and breaking of the Nazi economy," his book *The Wages of Destruction*—are quite extraordinary, better than anything that had been produced in the past. Just compare that book with another book on the same topic that came out in 1959, *Germany's Economic Preparations for War* by Burton Klein. Tooze's analysis is just much deeper and more penetrating: what is impressive is not simply the mass of evidence that he presents, although that's extraordinary in itself, but the way that evidence is marshaled into an argument—and even more the way that argument forces you to rethink, in a very fundamental way, your whole understanding of the origins of the Second World War.

Or compare Paul Kennedy's *Rise of the Anglo-German Antagonism* (which came out in 1980) with Raymond Sontag's *Germany and England: Background of Conflict* (published in 1938). The Sontag book was a superb work of history—exceptionally perceptive, beautifully crafted—but Kennedy was able to go into the issue in much greater depth. His book, after all, was based on a massive amount of primary source material, found in British, German, and Austrian archives—not just the official material, but also over 233 collections of private papers, found in 54 different repositories. And I haven't even mentioned the very large number of historical works that he cited in the book—he spent 32 pages just listing them all.

Or let me refer to a less massive work, Mary Sarotte's new book *1989: The Struggle to Create Post-Cold War Europe*, a study (which, as she points out) was based "on research in archives, private papers, and sound and video recordings" from "Moscow, Warsaw, Dresden, Berlin, Leipzig, Hamburg, Koblenz, Bonn, Paris, London, Cambridge, Princeton, Washington, College Station and Simi Valley"—again, not to mention all the published material she was able to draw on. And that's just one of a number of books that have come out just within the past year that are really very impressive, not just in terms of source material, but also in analytical terms. I'm thinking here, for example, of Jochen Laufer's *Pax Sovietica* and Fraser Harbutt's new book *Yalta 1945: Europe and America at the Crossroads*. In fact, I've read a number of works recently that I've really liked—Frédéric Bozo's book on Mitterrand and the end of the Cold War; Benedikt Schoenborn's book on de Gaulle and the Germans; and Stefan Schmidt's study of French foreign policy in the July Crisis of 1914, to give just a few examples—and when I think of works like that, I come away feeling that the standard is remarkably high—that in intellectual terms, the field is remarkably healthy.

But let me pause here briefly to consider a very simple question: why is it that that sort of work can be done *now*? Couldn't that kind of work have been done in the past? I know this is something that people might disagree with, but I don't think that scholars are able to do that kind of work today essentially because our *perspective* has changed—that in the past people had a very narrow view of what diplomatic history was, that they tended to think of it as simply the study (to use a famous phrase) of what "one clerk said to another," but that now we take a much broader view of what the field is about. That's a view that many people have, but I just don't think it's accurate. There certainly *was* a lot of utterly boring, utterly mindless, historical work of the "what one clerk said to another genre" that was published decades ago, but the best work—and this field, like any other, should be judged mainly by the *best* work it produces—was never cut from that cloth. Think, for example, of the Sontag book I referred to before, or of R.W. Seton-Watson's *Disraeli, Gladstone, and the Eastern Question* (which came out in 1935)—or for that matter think of Thucydides.

The change, to my mind at least, has a more mundane taproot. It has to do with the material conditions within which work can be done—the vast increase in the amount of primary source material that is available to researchers today, and also—and this is of increasing importance when we think about the future of the field—the remarkable technological changes that have taken places in recent decades. When I came over to Paris in 1971 to do the research for my dissertation, I couldn't even get access to the foreign ministry materials for the immediate post-World War I—that is, to material that had been produced half a century earlier. And as for the material I was able to get access to, I had to either summarize or paraphrase or transcribe by hand whatever I found in a document that seemed to me worth noting. Photocopying was much too expensive to use freely. Cheap xeroxing came on the scene in the 1980's, and today the use of digital cameras makes copying even easier and less expensive.

And today—and this is in many ways a very recent development—it's possible to do archival-type work without even having to go to archives, and this will become increasingly true as archives proceed to "digitize" their collections. This process is proceeding quite rapidly—in part, in the United States, as the by-product of new computer-based methods that are being put in place to increase the efficiency of the declassification process, in themselves spurred by the need to review absolutely massive amounts of material—but even what has happened so far is really remarkable. Today I can sit in my office at UCLA and download and print out, very cheaply and easily, a great mass of material—and by that I mean essentially archive-type material—that I have access to over the internet: material from the Declassified Documents Reference Service, from the Digital National Security Archive, British Cabinet records, German cabinet minutes, the U.S. State Department electronic telegrams collection, the electronic supplements to the State Department's *Foreign Relations of the United States* series, the "electronic reading rooms" containing documents released under the Freedom of Information Act by various U.S. government agencies, and so on. I can then mark up those printouts, and when it's time to write something up, I can work from those marked up documents. I can go through material maybe ten times as quickly as I was able to forty years ago, and still get as much out of them. And since it's hard to travel, especially when you have to teach or if you have a family, I can spend a lot more time doing archival-type research than I would have been able to if things had not changed so dramatically. Putting those two things together, what all this means is that in the course of a lifetime one can do a lot more in terms of primary source analysis than was possible a generation ago. And it's not just a question of learning a lot of facts—the real payoff is that it is now possible to develop a much deeper understanding of the general subject—deeper insight, that is, into the question of what makes for war and what makes for a stable international system.

So you might be tempted to look at all this and say that things are going pretty well—that the field is in pretty good shape. And yet when you talk with people who do this kind of work, especially in the United States, you sense a certain malaise. There's a certain sense that the field is not what it should be—that the field, perhaps, does not have a clear sense for what it's about, for what it should be trying to accomplish. The work that's being produced, especially in recent years, is all over the map: the field seems fractured, Balkanized—there doesn't seem to be any overarching sense of purpose.

In America especially, the field, especially in its more or less traditional form, is very much under attack. (In Europe, you see some signs of the sort of thing I'll be talking about, but the problems I'll be outlining are much milder over here.) But in America the situation is quite serious. Many historians doubt whether our field has much legitimacy at all. Military history is in even worse shape. I remember attending a seminar at the Davis Center at Princeton University at which Russell Weigley, one of America's leading military historians, had given a talk. During the question period, a social historian asked Weigley a question: "Isn't it true," he said, "that military historians really like war?" That kind of attitude is very common, and it's in fact very hard to get a decent academic job if you do military history of any sort. Diplomatic history, on the other hand, has not been entirely drummed out of the profession, but it has been pushed to the margins. People in other fields have very little respect for it as an intellectual enterprise. At my own university, the history department is enormous—something like 80 standing faculty members—but not one who does international politics or foreign policy or anything like that as his or her main field. The jobs go instead to people who work in fields that are considered more "politically correct"—that is, who work on topics having to do with the "holy trinity" of Race, Class and Gender, especially Gender. Diplomatic history is considered to be the carrier of a set of distasteful social values, and is also opposed because it is considered too old-fashioned in methodological terms—people in this field are viewed, with some justification, as methodologically conservative, and in particular as opposed to the idea that historical work can serve as a bludgeon for advancing a "progressive" political agenda.

Some diplomatic historians have attempted to adapt to those new trends and have tried, for example, to study U.S. foreign policy from a “gendered” or “postmodern” point of view. But those attempts have not been entirely successful, even in terms of academic politics: they have not enabled the field to reestablish a foothold in departments that have turned away from it. In the meantime, the marginalization of mainstream diplomatic history has had far-reaching effects on the field as a whole. The best graduate students see which way the wind is blowing; they tend to shy away from doing anything that can be thought of as traditional diplomatic history.

This is not to say one cannot get a decent job if one is interested in the international scene. But your chances of getting a job are much greater if you can market yourself as a national historian—as an Americanist, for example, it would be acceptable to teach a course on the history of American foreign relations. But that sort of thing simply perpetuates the parochialism of a lot of the work that’s done in this area: most people working in this field in the United States are historians of American foreign policy, not historians of international politics. Even the one professional organization in the United States in this area calls itself the “Society for Historians of *American* Foreign Relations.”

The national approach, however—and this is a point that applies to many countries, and not just the United States, although the American example is particularly exteme—the national approach is at odds with what the field should be about. The key to understanding international politics is to see it as an *interactive* process—to understand that what one state does is very heavily influenced by what other states do, that foreign policy is not something that just wells up from deep within a society, but rather is to be understood in terms of the environment in which a country finds itself. And that means also that in trying to understand how international political life runs its course, you have to try to see things through the eyes of the different actors involved in the story. None of this is possible if you're focusing just on the foreign policy of a particular country. And yet that's the prevailing approach—and it's prevalent, to a certain extent, because of the way academia is structured.

Let me make just one last point about the place this field has in American academic life. Because it's been marginalized within the U.S. historical profession, a number of people who were trained in this area have migrated to—or maybe I should say taken refuge in—political science departments, schools of government, and the like. Mary Sarotte, who I mentioned before, is in an international relations department at the University of Southern California, even though that school has a history department. Frank Gavin, who also does very impressive work—and I think some of you may know him, since he spent some time here—is at the LBJ School at the University of Texas. And I, of course, after teaching for 26 years in the history department at the University of Pennsylvania, am now in the Political Science Department at UCLA.

Now, this kind of migration away from history departments is bound to have somewhat mixed effects. On the one hand, the kind of training graduate students get in political science departments, for example, is very different from the kind of training people like me got when we were in graduate school. The great emphasis on what political scientists call "method"—and by that they mean statistical method (above all regression analysis) and game theory—means that graduate students in that discipline really don't have the time to do things that most of us consider essential, like learn foreign languages. How one can presume to study international politics without knowing at least one foreign language is utterly beyond me—it's not simply a question of access to the sources, what's more important is that learning a foreign language is the only way to break out of the provincialism of your own culture, and American culture especially is in many ways quite provincial.

On the other hand, the sort of migration I've been talking about is not an altogether unhealthy thing. First of all, it might have a certain impact on the way history departments conduct their business. If people like me teach courses outside history departments that attract a lot of students, that might put a certain pressure on those departments to take this field more seriously. But the more important effect, it seems to me, is intellective in nature—that is, it might actually have a positive effect on the kind of work that is done. I know, for example, that interacting with political scientists has had a very positive effect on the kind of work I do.

I'll give just a couple of examples here, and both of them have to do with the "preventive war" issue. The dynamic here—the idea that states worry about shifts in the structure of power, and often feel a certain pressure to act before the balance shifts against them—is taken quite seriously by political scientists, whereas historians traditionally have paid little attention to this kind of thing. When the George W. Bush administration, for example, put forward a doctrine of this sort, saying how the United States had to nip problems in the bud before they became completely unmanageable, historians like Arthur Schlesinger took the line that this marked a total break with American tradition—that earlier U.S. governments had never pursued policies based on that kind of thinking. More generally, historians tended to associate the preventive war philosophy with the lunatic fringe.

It turns out, however, that this kind of thinking *did* play an important role in shaping U.S. policy at key points in the twentieth century—it certainly played a more important role than most historians have been prepared to admit. Take the case, for example, of U.S. policy in 1941, in the months right before Pearl Harbor. There's one key document from that period, the so-called "Victory Program" of September 11, 1941, signed by Army Chief of Staff Marshall and Admiral Stark, the Chief of Naval Operations. The basic point of that widely-cited document, if you read the standard historical accounts, was that *if* the United States entered the Second World War, it should give top priority to the war against Germany; the war with Japan would be of secondary importance. But if you read the document itself, the real point was that if the United States waited too long to get into the war, the problem with Germany would become unmanageable: if the Germans had a free hand to consolidate their position in Europe—to bring order out of chaos in the areas they occupied in Russia—there would be no way to defeat them, certainly not at acceptable cost. So America needed to enter the war as soon as she could, even before she herself was attacked. This was thus not a *military* argument about how the war should be fought if the United States entered it; it was a *political* argument about why the United States had to go to war with Germany before it was too late.

Now this I think is very important, and the basic point here has major implications about how the events of 1941 are to be understood. But why was it that scholars were unable to see what the military authorities were saying? And why was I able to see it when other scholars, who in fact were specialists in this area, were just blind to it? It's not that they're stupid or anything like that. What this example shows is how important it is to have a prepared mind. You pick things up when you kind of know what to look for—when your radar screen is activated, when your antennae are turned on. The evidence becomes salient only when you're asking the right questions. And that's what exposure to "theory"—that is, to the sort of thing that international relations theorists talk about—gives you. Not *answers*, but a sense for what the right *questions* are.

The second example relates to another important document from this period, the Atlantic Charter signed by Franklin Roosevelt and Winston Churchill in August 1941. This document is commonly treated as just a standard statement of standard liberal principles; it certainly is not treated as a charter for a permanent strategy of preventive war. And yet Point Eight of the Charter called for the disarmament of countries which might threaten aggression—not just when they actually committed acts of aggression, but when the "policemen" who dominated the system saw a *threat* on the horizon. And Roosevelt took that provision quite seriously. The "whole point" of that provision, he said, "was to make clear what the objective would be if the war was won."[1] As he saw it, only four states—America, Britain, Russia and China, the "Four Policemen," as he called them—would remain armed; everyone else, including even countries like France, would be disarmed, by force if necessary. "The four major nations," he told the Soviets a year later, "would maintain sufficient armed forces to impose peace." "If any nation menaced the peace," he said, "it could be blockaded and then if still recalcitrant, bombed."[2] Or as

[1] Welles memo, August 11, 1941, FRUS 1941, vol. 1, p. 366; see also Theodore Wilson, *The First Summit*, pp. 173-175, 192, 197-199,

[2] See Roosevelt-Molotov meetings, 29 May and 1 June 1942, FRUS 1942, 3: 568–69, 573, 580; Roosevelt-Stalin meeting, November 29, 1943, FRUS: Conferences at Cairo and Teheran, pp. 530-531.

Vice President Wallace put the point the following year: the way to maintain peace and security in the postwar world was to "bomb the aggressor nations mercilessly"[3]—and I think it's important to understand that we're talking here about the bombing of civilian populations, the sort of bombing campaign that was conducted against Germany and Japan in World War II, in part with an eye to influencing their behavior in the post-war period. And yet all of this has been simply filtered out of our historical consciousness: it doesn't register because our minds are not be prepared to pick it up. But if you interact with political scientists, if you try to see the world through their eyes, if you grapple with the ideas and arguments they're concerned with, you're much more likely to see these things.

All of which brings me to the second issue I wanted to talk about, the whole question of "where do we go from here"—the question of how the field ought to develop in the years to come. Let me begin by trying to summarize what I've been saying about the problems with the field as I see them; the question of how to proceed will naturally take that diagnosis as its point of departure. So to reiterate: the field, as I see it, has two basic problems. There is first the *intellective* problem, the *substantive* problem—the absence of any sense of overarching purpose, the fact that the field does not quite know what it is about, the fact that people go off in all kinds of different directions, and that there is not much that pulls them together and gives them the sense that they're involved in a common intellectual enterprise. And, second, there's the *political* problem, which is particularly acute in the United States, although some aspects of it can be detected in Europe as well—and this is the problem of the marginalization of the field within the

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[3] Robert Hilderbrand, *Dumbarton Oaks: The Origins of the United Nations and the Search for Postwar Security* (Chapel Hill: University of North Carolina Press, 1990), p. 142.

historical profession, and the shriveling up of an adequate institutional base for the field. And of course the two problems are related in all kinds of ways: if, for example, the intellective problem could be dealt with, that might have a certain effect on how the field fared in the academic world.

So how *can* the intellective problem—and above all the fracturing, of the field—be dealt with? The first step here, it seems to me, is that we need to think hard, and think collectively, about what it is exactly that we're trying to do—that we need to develop a kind of philosophy, a sort of theory, of the field. And it seems to me that this is the kind of thing that we're going to have to do, if not collectively perhaps then at least as individuals, simply in order to get a handle on the great mass of evidentiary material that we now have access to, and (for the reasons I talked about before) can go through much more efficiently than was the case in the past. We can't approach our material any more the way I, for one, was taught to in graduate school. It's not enough to say that you need to just "look at the sources." You have to figure out which sources to look at, and in more or less what order. You have to figure out how to tackle that huge mass of material, and that means you need a *strategy*—that is, you have to think hard about the structure of the problem you're dealing with, how the different questions you're concerned with relate to each other, how big issues turn on, or can be made to turn on, more specific, and therefore more studiable questions. But that means you first have to have some sense for what the big issues are, and for the sorts of debates that revolve around them.

And here I think a lot of the work that's been done by international relations theorists, especially in recent years, might be of real interest to us. I'm thinking especially of James Fearon's extremely influential 1995 article, "Rationalist Explanations for War." What was important about that article, I think, was not so much the particular argument he put forth about how war could be rationally explained, but the core aspiration that his whole approach was based on. This was the idea that to get at the issue of war and peace, it didn't make sense to just come up with a long laundry list of factors that seemed to have some explanatory power. Instead, what you needed to do was (in Fearon's words) to "take apart and reassemble those diverse arguments into a *coherent theory* fit for guiding empirical research."[4] When explaining war, you had to frame the question in such a way that you could see why the answer *had* to do, *necessarily*, with a certain type of factor. His argument was that war was never the optimal way of resolving conflicts; there was always a solution that was better for both parties than armed conflict; the central question, the whole focus of analysis, therefore *had to be*: what was it exactly that prevented a negotiated solution from being reached? One can quarrel with the way he answered that question, but that whole approach was enormously productive because it gave focus to the analysis. In principle, it enabled people who normally simply talked past each other to engage each other intellectually—to engage, that is, in productive debate.

Can we draw on that kind of theory when we're trying to figure out how we'd like work in our own field to be structured? To a certain extent, I think the answer is yes—but only to a certain extent. The international relations theorists are concerned ultimately with the same kinds of issues as we are, and if our goal is to make our own field more coherent intellectually, we can profit by paying attention to what they're trying to do. But our goals are somewhat different from theirs. We're certainly not interested in theory-building as a kind of end in itself. And although political science ideas and

[4] James Fearon, "Rationalist Theories of War," *International Organization* (Summer 1995), p. 382.

arguments can often help us decide which empirical questions we want to focus on—and this, I should say in passing, is certainly true of my own work over the last twenty-five years—I think we should have enough intellectual self-confidence to set our own scholarly agendas.

Should we then try to set an agenda in a rather direct and self-conscious way? We tend to be rather individualistic in terms of how we do our work—how we choose the particular projects we work on. Is this perhaps a mistake? Maybe we should think a bit more in terms of developing collective scholarly programs. This sort of thing, after all, is not unheard of in our world. Pierre Renouvin, the dean of French diplomatic historians, had very strong views about how work in this field should be done; I remember being struck in particular by a programmatic article that he published in 1961.[5] Of course, this sort of thing cannot be pushed too far—no one would want to be straitjacketed by programs established by other people—but I do think it makes sense to give some thought to what exactly we should be trying to do, and how as a community we could achieve those goals. I do think it would make sense to talk about those issues explicitly, and not just say "let a hundred flowers bloom" and hope for the best. Even if we never arrive at a consensus, the whole process of grappling with questions of that sort might be of value in its own right.

And I think we could begin that process by looking at the kind of work we ourselves are doing, and the others around us are doing, and just asking, as honestly as

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[5] Pierre Renouvin, "Les relations franco-allemandes de 1871 à 1914: Esquisse d'un programme de recherches," in A.O. Sarkissian, ed., *Studies in diplomatic history and historiography in honour of G. P. Gooch* (London: Longmans, 1961). See also Pierre Renouvin and Jean-Baptiste Duroselle, *Introduction à l'histoire des relations internationales* (Paris: Colin, 1964); Jean-Baptiste Duroselle, "De 'l'histoire diplomatique' à 'l'histoire des relations internationales,'" in *Mélanges Pierre Renouvin: Études 'histoire des relations internationales* (Paris: PUF, 1966); and René Girault, "Souvenirs et pratiques," in René Girault, *Être historien des relations internationales* (Paris: Publications de la Sorbonne, 1998), pp. 15-23.

we can, what the point of it is. "Does this work really matter? Does anyone really care about what we come up with?" I don't mean to suggest that the answers are bound to be negative. It's just that this is the way to start thinking about the kinds of goals we should set for ourselves as a community.

I have to admit that when I think about these issues, I take what I'm sure will strike many of you as a fairly narrow view. For me, the fundamental question, the question that practically defines the field, is the question of war and peace. What makes for war? Or to look at the same question from the other side what makes for a stable international system? And related to that, there are fundamental questions about policy—about whether there is anything that can be said, of a general nature, about the principles that should guide foreign policy. And this, to be sure, means that questions relating to the origins of specific wars, and especially of conflicts like the two world wars, are of particular interest. But those, of course, are not the only questions that are of interest. War is the product of a political process, and all kinds of things affect the way that process runs its course. The whole question of the reform of the international monetary system, for example, especially in the 1970s, had a certain political importance in this connection—the monetary policies the major industrial countries adopted helped determine the sort of political relationship they had, and that relationship in turn had a certain effect on east-west relations—that is, on the stability of the larger international political system. So all sorts of things are relevant. But the point is that from this perspective—from the perspective that takes the question of war and peace as paramount—those things are of interest only to the extent that they bear on that fundamental question. What we want to focus on—if you accept that way of looking at things—is how they fit into the larger scheme of things, how they bear on the question of war and peace.

And this, I should note, is not simply a way of limiting the sorts of things we're interested in. It's a way of defining questions so that we can dig deeper into the issues we're concerned with. We might be tempted, for example, to simply assume that the unification of Europe is a source of stability. French president Mitterrand, for example, certainly took it for granted that by "building Europe" the international system could be made more stable. What was the logic here? The basic idea (as Bozo showed in his book on Mitterrand and the end of the Cold War) was that Germany remained a problem; the assumption was that German power could be "contained" within strengthened European structures.[6] The idea that those structures would have that kind of effect is accepted, more or less uncritically, especially in Europe. It's taken, that is, more or less as an article of faith. But when you approach the problem with that one basic question in mind—the question about what makes for war or for a stable international system—you can hardly help asking whether it's in fact true that those European structures would work as advertized. When you approach it from that point of view, you have to ask how exactly would those European structures contain German power? If the unified Germany basically accepted the new status quo, that Germany would have no problem working with her neighbors, no matter what European structures were in place. But if that Germany wanted to pursue any sort of revisionist policy, it is hard to see how those European structures would prevent her from doing so. The point here is simply that approaching the issue in this "narrower" and more focused way leads you to ask certain questions you might not

[6] Frédéric Bozo, *Mitterrand, the End of the Cold War and German Unification* (New York: Berghahn Books, 2009), p. 99; see also pp. 65-66 and 22.

otherwise ask. You are less likely to just take the conventional wisdom at face value; you're more likely to try to go into the issue more deeply.

Now, that's *my* view of what the fundamental issues are, the issues that should define what the field is about, and I'm sure that other people—and indeed perhaps most of you here in this room—would take a very different view. And I'd never say that you should change your mind and agree with me just because I'm making that argument. What I am saying is that if you *do* disagree, you should try to figure out for yourself how you would answer those basic questions about what the point of what we're doing is—about what the thinking is that lies at the bottom of the whole scholarly effort we're engaged in. This, to my mind, is the kind of issue that should be discussed and debated, and who knows what will come out of those discussions? At the very least, we'll all get a clearer sense for what other people are trying to do, and indeed for how it relates to what we're trying to do.

And why do I think it's important to ask these kinds of questions? I'd break the answer down into three parts. First, as *individuals*, we only go around once in life, and we want our life to have a certain meaning. I think, in fact, that most of us go into the academic world, even though we could probably make a better living on the outside, because we feel the kind of work we do is *meaningful*. We therefore owe it to ourselves, as individuals, to ask ourselves, as we do our academic work, what the point of it is. We owe it to ourselves, as individuals, to design our research projects in a way that guarantees that they will be *meaningful*—that we can answer the "who cares?" question, at least to our own satisfaction.

And second, we want to do work that is not just satisfying to us as individuals. We don't want to live in a totally atomized scholarly world. We don't want to live in a world where the whole is less than the sum of its parts. We want to live instead in a scholarly *community*, where people actually listen to each other, where they argue with each other, where those arguments are conducted in a way that enables people to see beyond their own preconceptions. We want to live in a community where people engage each other intellectually, where a body of thought which we can all contribute to develops as issues are argued out seriously, and with reference to the evidence. That's one of the main reasons we need to stand up for the norm that questions should be framed in such a way that the answers turn on what the evidence shows. And by thinking hard about what the point of what we're doing is, about what the field as a whole should be trying to accomplish, we might be able to develop a common frame of reference—we might be able to pull the field together and make it less fractured, and more of a community.

And third by asking these questions we're implicitly assuming that the field should have a *purpose*—and in large part I mean that the field should have a *social* purpose. I'm not a very religious person, and this is a little hard for me to say, but I think there's something almost sacred about what we do. You study these issues and you really do develop certain general conclusions—about what makes for war, what makes for a stable international system, and, related to that, conclusions also about how, in a very general way, policy should be conducted. Those conclusions matter, because they're at variance with common assumptions that you draw from the general culture as you grow up; and historical work is the laboratory for hammering out answers to those fundamental questions—in the nuclear age quite obviously the most important questions that the human race has to deal with.

What I’m saying is that in social terms we have a very important role to play, and we play it by doing our job the way it should be done—and that means making sure that we think hard about what it is exactly that we should be trying to do—how we can do historical work that in some sense matters, work that can pass the “who cares?” test. And we owe it to ourselves, we owe it to our profession, and we owe it to the world as a whole, to do work of that sort.

Thanks for listening to me, and I’m really looking forward to getting your reactions.