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My Story

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I was born on February 9, 1946, the same day that Soviet leader Joseph Stalin gave a speech which U.S. Supreme Court Justice William O. Douglas famously called a "declaration of World War III."[1] This was a bit of an exaggeration, but the Stalin speech was certainly one of the opening shots in the Cold War. And the great conflict that was about to begin would have a profound effect on my life, in more ways than one.

That effect was by no means entirely negative. The first Sputnik was launched in 1957, and, practically overnight, in the United States nerdy types like me came to be seen as a major national asset. America, it seemed, had to invest a lot more in education, especially in science and technology. So the National Defense Education Act (NDEA) was passed in 1958 and funding for the National Science Foundation (NSF) tripled from what it had been in the last pre-Sputnik year. And I profited enormously from a number of NDEA and NSF programs. The post-Sputnik climate probably had a good deal to do with where I ended up going to high school. The school I went to, which emphasized math and science, took in boys from all over New York City on the basis of a competitive exam. If anyone had asked me at that time, I'm sure I would have said that I would end up in one of the sciences, but I really wasn't thinking in such long-range terms at all.

I went to Berkeley as a freshman in 1962. It was the only place I really wanted to go. Mostly this was because of all the Nobel prizewinners there, and I was also impressed by the fact that three transuranic elements in the periodic table (Berkelium, Californium, and Lawrencium) had a Berkeley-related name. Not that any of that had anything to do with the kind of education I would get when I got there—but that point simply never occurred to me at the time.

It was during my second year in college that I really became interested in history. Even in high school I liked history a lot. The Columbia FM radio station, WKCR, used to broadcast lectures given by one of Columbia's best history professors, James P. Shenton. I loved listening to those lectures, and I very much hope WKCR has managed to preserve those tapes, because knowing how to lecture is part of the historian's craft, and one learns how to do it only by observing the masters in action. But at that time I didn't realize that history could be an intellectually challenging discipline. It was only in college that I came to see that historical work could have a real conceptual core—and it was that, in fact, that made it so fascinating.

The great turning point for me came in 1963, when I took Raymond Sontag's diplomatic history lecture course. I took it because someone in the co-op where I lived had told me he was a terrific teacher, which was certainly true. It was not just a

[1] James Forrestal Diary Entry, 17 February 1946, in Walter Millis, ed., *The Forrestal Diaries: The Inner History of the Cold War* (London: Cassell, 1952), https://archive.org/details/theforrestaldiarieswaltermillised, 141. The Forrestal Papers have been digitized by the Mudd Library at Princeton, and the original diary entry is available online: Forrestal Diaries, vol. 4 (for February 1946), *James V. Forrestal Papers*, Public Policy Papers, Department of Special Collections, Princeton University Library, Princeton, NJ, https://findingaids.princeton.edu/collections/MC051/c05081, 28.

question of style. What came through in his lectures was a certain philosophy—about how history works, and about how the past is to be understood. You could see things through the eyes of all the parties involved; you could, to a certain degree at least, even sympathize with each of them; but you could also see how those different views could interact with each other to generate a conflict, and you could analyze the way that process ran its course without engaging in any finger-wagging. For someone like me, who had been led to believe that understanding the cause of a particular war was tantamount to figuring out who was to blame for it, that approach came as something of a revelation.

It was as though the blinders had fallen off. And it was at that point that I more or less decided that I wanted to spend my life doing historical work. That decision, if that is what it was, probably had a lot to do with Sontag's personal qualities. He was very open to dealing with students one-on-one, and I spent a lot of time talking with him during his office hours. He was quoted, after he died, as having said that the art of being a good teacher consisted almost entirely of liking students. But in a conversation I had with him when I was in graduate school he put the point somewhat differently. The art of being a good teacher, he said, consisted almost entirely of getting the students to like you. I personally very much prefer the latter version. It certainly gives a better feel for what he was like.

I stayed in Berkeley for graduate school, mainly because I wanted to work with him (but also because I was rejected by the two other places I had applied to). He offered me a job as his research assistant, and I helped him with his wonderful last book, *A Broken World*, about the interwar period.[2] And I learned more about how to do historical work working with him as an R.A. than I learned in any of my courses. I remember one incident in particular. A number of books dealing with Russo-German relations in the interwar period had recently come out, and my job was to read them and figure out how the material they presented affected what he had written in his draft manuscript. I spent a number of weeks reading those books and then wrote a memo summarizing my findings. He read the memo, looked at his manuscript, thought for a minute or so, and then crossed out a word here and a phrase there and inserted one or two new words or phrases. The new text was perfect. There was a sort of elegance not just to his writing but to the way his mind worked of which I'm still in awe.

Sontag had by far the greatest influence on me of any of my teachers, but a number of others played a key role in my intellectual development. I remember, first of all, a brief conversation I had with Sheldon Wolin, another one of my teachers. I was taking Wolin's course on the history of political theory, and I was struck by the fact that the first thing he had us read was Thucydides's history of the Peloponnesian War. Since this was a political science course, I couldn't understand why he had assigned it. "Wasn't Thucydides an historian and not a political theorist?" I asked him (during his office hours), and if so why were we reading him in that course? His answer was extraordinarily gracious. "I'm sure you'd agree with me," he said, "that a certain political theory lies at the heart of every great work of history." I nodded solemnly, although the point had never occurred to me before—I had never thought that way about history at all. But a seed had been planted, and to this day I remember with great pleasure the way he had planted it.

A seminar I took with Ed Segel on the origins of the Second World War had a much greater impact on me. That class got me to see even more clearly that the best and most interesting historical writings present an *interpretation* that reflects a certain philosophy about how history works. The first book he had us read was Churchill's *The Gathering Storm*.[3] I remember his first question to the class on that book: "According to Churchill, what makes history run?" That class made me look at historical work in an entirely new way.

A third thing that made a big impression on me as an undergraduate was Hannah Arendt's *The Origins of Totalitarianism*, which Robert Paxton had assigned for his history of twentieth-century Europe course. Today I think her analysis of the

[2] Raymond Sontag, *A Broken World, 1919-1939* (New York: Harper, 1971), https://archive.org/details/brokenworld1919100sont.

[3] Winston Churchill, *The Gathering Storm* (Boston: Houghton Mifflin, 1948), https://archive.org/details/dli.ernet.242792/mode/2up.

Soviet system, at least, was dead wrong, but at the time I was simply blown away by that book. Again, a big part of the reason was that the history here was by no means purely descriptive. The goal was to tackle a major history problem, so the basic approach had to be analytical, and that book gave me a certain sense for how powerful that analytical method could be.

So that was where I was when I started graduate school in 1966. I had no plan for what I wanted to accomplish. I took courses and did other things simply to meet the requirements of the program. I was just trying to get my ticket punched in all the right places. And I took a number of really good courses—from Gerald Feldman, soon to become America's leading historian of Weimar Germany; from John Heilbron, the historian of science; and from my wonderful dissertation advisor, Richard Kuisel, whose field was French history. I also had to do some work in economics, since we were required to have an outside field. But although there was nothing systematic about this process, by the time I started to write my dissertation I had become more or less professionalized. I'm still not quite sure how exactly that process works. You learn to read books and articles in a certain way—that is, critically, with an eye to whether the work in question has an argument, and, if so, what one is to make of it in terms of its internal logic and the adequacy of the evidence supporting the author's conclusions. So you're constantly taking your measure of various historical works, and, almost automatically, you try to emulate what you like and avoid what you dislike. You write papers and do other things you're required to do and get your efforts criticized by professors and fellow students, and you try to learn from your mistakes and figure out how you could do a better job if you had to do it again. In the process you're not just learning a set of skills, you're also absorbing the values of the profession.

Did what was going on in the real world at that time have any impact on my professional development? Yes, but the effect was fairly minor. As many of the contributors to this series—at least those from my generation—have noted, the Vietnam War affected most of us in a very profound way. During my graduate student days (from 1966 to 1974), it was always there in the background, like distant thunder which you just couldn't ignore. This was particularly true for people like me with a certain interest in international politics. Like many of us, I had turned against the war in 1965. One of the things I read at that time was Robert Scheer's short book (or long pamphlet) *How the United States Got Involved in Vietnam*, which came out that same year. My feeling today is that it was not very good as history, but at the time it made a big impact on me.[4] I remember in particular what Scheer showed about how the Geneva Accords had called for Vietnam to be reunified in free elections, how Secretary of State John Foster Dulles had declared that the U.S. government "would not seek by force to overthrow the settlement," and how President Dwight D. Eisenhower had admitted in his memoirs that if elections had been held in 1954 "possibly 80% of the populace" would have voted for the Communist leader Ho Chi Minh. All of this was quite shocking to me at the time.[5] I had assumed that the U.S. government basically supported the principle of self-determination—that it believed that every people should be free to choose its own political destiny, especially through free elections—and also that the United States was a country that honored its commitments, but here was clear evidence that American leaders had been determined to prevent the Vietnamese people from deciding their own fate in free elections and, despite Dulles's statement, had actually sought to prevent the Geneva Accords from being carried out. Could it be that people like me, who had grown up during the Eisenhower period, had been sold a bill of goods? Could it be that international politics was not really a struggle between angels and devils, as I had been led to believe? But substance aside, what that experience suggested was that a lot could be learned from even a casual perusal of the historical evidence, especially if you approached it with a minimally open mind.

I had a second Vietnam-related experience at about that same time, during the academic year 1965-1966. The history majors at Berkeley during that period all had to take a seminar which focused on the writing of a research paper based on

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[4] Robert Scheer, *How the United States Got Involved in Vietnam* (Santa Barbara: Fund for the Republic, 1965), available online at the Michigan State University Vietnam Group Archive, http://vietnamproject.archives.msu.edu/fullrecord.php?kid=6-20-15FD. For the judgment that Scheer's analysis there was not very good as history, compare, for example, his account of then-Senator John F. Kennedy's speech on Indochina of 6 April 1954, on page 15, with the corresponding passage in the actual speech, now readily available online on the Kennedy Presidential Library website, https://www.jfklibrary.org/archives/other-resources/john-f-kennedy-speeches/united-states-senate-indochina-19540406.

[5] Scheer, *How the United States Got Involved in Vietnam*, 11-12, 19.

primary sources. I had spent my junior year in France and wanted to do some work with French-language material, so I took the section taught by Nancy Maginnes, then a graduate student in French history in our department (and now the wife of Henry Kissinger). One day she announced to our seminar that Kissinger, then a well-known professor at Harvard, was coming through the Bay Area the following week and that she had invited him to come to our class. When he came, he gave a talk defending U.S. policy in Vietnam, and one of his key arguments was that the people in South Vietnam basically supported the government. I remember asking him: "If everything you say is true, how come we're not winning the war?" I don't recall his answer in detail, but I do remember him telling me that I just "did not understand the oriental mind"—a term which sounded odd coming from an academic even at that time. But I was elated by that experience. If Kissinger was one of the top people in the field and he made those kinds of arguments, it shouldn't be too hard for me, I thought, to do okay in it myself.

A few years later, when I was in graduate school, the war came into play for me in a very different way. My understanding of international politics had developed as a simple by-product of doing ordinary historical work. It had not been shaped by what was going on in the world around me, nor by any exposure to what the political scientists had to say. (No one ever told me, for example, about Kenneth Waltz, even though he was in the Berkeley political science department during my last few years in Berkeley.) But a certain general understanding of how international politics works, and certain general ideas about how policy should be conducted, had taken shape in my mind in my first years in graduate school; I had developed that sort of understanding in part by looking at the way statesmen in the past had dealt with international problems, and in part by focusing on how various historians had interpreted international issues.

Around 1970 or so it occurred to me that the Vietnam War could be analyzed in that basic framework. And, when I thought about it, the main conclusion I reached was that even a complete U.S. victory in Vietnam made little sense in geopolitical terms. If America won the war, the Chinese heartland would be surrounded by an arc of American power, beginning on the mainland in South Korea, going down through Japan, Okinawa, Taiwan, and the Philippines, and reconnecting with the continent in South Vietnam. The Chinese would then have a strong incentive to mend fences with the USSR. On the other hand, a U.S. withdrawal from Vietnam and the reunification of that country under Communist auspices would probably have the opposite effect. Vietnam would naturally look to the Soviet Union as a counterweight to its great neighbor to the north; and the Soviets, given their problems with China, could hardly resist developing a certain relationship with a major country in China's rear. The Chinese would then feel more threatened by the USSR than by the United States, and that would increase their interest in improving their relations with America. But a full victory for America was probably never in the cards; as long as American forces were present in Vietnam, the guerrilla war would almost certainly continue; and the continuing war would result in a continuing drain on American military resources. It would also tend to prevent the United States from improving its relations with both China and Russia, and thus from being able to play them off against each other. So a way of thinking had taken shape in my mind which, it seemed to me, could have major real-world implications. And that way of thinking was powerful because the implications were counter-intuitive: the conclusion in this case was that a country could actually gain by losing. And all of that confirmed me in my view that if the question was whether there was a set of general principles that should guide the conduct of foreign policy, historical study might help provide the answers—which was another way of saying that historical work could have major real-world importance, and was in part worth doing for that reason.

But, as I say, none of this really played a central role in my professional development. I basically just led the normal life of a graduate student in history: doing my coursework, preparing for my Ph.D. exams (which I barely passed), and preparing to write a dissertation. I decided to work on the reparation question after the First World War. In graduate school, and even as an undergraduate, I had been very interested in the debate triggered by A.J.P. Taylor's famous book *The Origins of the Second World War.*[6] And, although I disagreed with a lot of what Taylor had to say, I did agree that historical work on the causes of that conflict had focused much too heavily on the German Führer Adolf Hitler (with a secondary focus on the appeasers in the West). I agreed with Taylor that what I would now call structural factors were of primary importance. And

[6] A. J. P. Taylor, The Origins of the Second World War (London: Hamilton, 1961), https://archive.org/details/originsofsecond000tayl/page/n7/mode/2up?q=taylor+%22origins+of+the+second+world+war%22.

this suggested that the line between the 1920s and the 1930s had been too sharply drawn—that there was more continuity between the two decades than we had been led to believe. Taylor, of course, was not the only one whose work pointed in that direction. I was also very impressed by Hans Gatzke's work on Gustav Stresemann, the German foreign minister in the late 1920s. I remember being struck, in one of Gatzke's articles, by a remark Hitler had made in 1941: "among my predecessors, Stresemann was not the worst"; "faint praise," Gatzke commented, "but still remarkable from such a source."[7] And what that implied is that the great struggle over the Treaty of Versailles, which was fought out in the early 1920s, should be a primary focus of analysis. Given that the reparation question lay at the heart of that struggle, I thought it would make sense to focus on that particular episode. That decision was also influenced by the fact that my outside field in graduate school was economics; I didn't think the more technical side of the reparation question was beyond me.

I ended up spending the entire calendar year 1971 in Paris studying that issue. I didn't quite know what I was looking for—or even that I should be looking for anything in particular. I was just immersing myself in whatever sources were then available. But I do remember being shocked that much of what I had been taught about French reparation policy—about how harsh it was, about how the French wanted to crush Germany under an impossible reparation burden, and so on—was simply untrue. I remember in particular going through the records of some key meetings during the Paris Peace Conference in the Klotz Papers at the *Bibliothèque de Documentation Internationale Contemporaine* in Nanterre. In one of those documents, Louis Loucheur, the real maker of French reparation policy at the time, was giving an analysis of the American and British proposals for a fixed sum for the reparation bill to be imposed on Germany. He could just about see, he said, how the amount the Americans had proposed ($30 billion) could be paid, but as for the British figure, which was four times as high, "we leave to the poets of the future the task of finding solutions."[8] It also turned out—and this was clear from the published notes of Paul Mantoux, the interpreter for top-level meetings at the Peace Conference—that the French were willing to accept an even lower figure than what the Americans had proposed. They were willing to accept a settlement based on a strict interpretation of Wilson's Fourteen Points, which would have meant, they said, a total bill of only $20 billion. But, astonishingly, the Americans refused to make common cause with the French. President Wilson, in fact, overruling his main advisors, agreed to include pensions and separation allowances in the bill, which raised the total amount quite substantially.[9] All of this I found upsetting, especially since some of the basic points here should have been clear from published material that had been available for years. Given all that, I found it very hard to understand why the standard accounts had been so wrong.

And it wasn't just the accounts of the peace conference that were flawed. In the post-Versailles period, the attack on the peace treaty focused on the reparation settlement. The famous economist John Maynard Keynes published an extraordinarily influential book, *The Economic Consequences of the Peace*, in which he claimed that the reparations called for in the treaty could not be paid because of what he called the "transfer problem."[10] To earn the foreign exchange with which to pay the reparations, the argument ran, the Germans would have to sell more to foreign countries than they bought, but it was impossible to see how this could be done. The problem here was that the French, in a plan they proposed at the end of 1920, were prepared to allow the Germans to pay both in kind and in paper marks, and that meant that there would be no "transfer problem." But all this was simply ignored by Keynes in his second book on the subject, *A Revision of the Treaty*

[7] Hans W. Gatzke, "The Stresemann Papers," *Journal of Modern History* 26:1 (March 1954): 49-59, here 49; https://www.jstor.org/stable/1874873.

[8] See Marc Trachtenberg, "Reparation at the Paris Peace Conference," *Journal of Modern History* 51:1 (March 1979): 24-55, here 33-34; http://www.sscnet.ucla.edu/polisci/faculty/trachtenberg/cv/rep(jmh).pdf, pp. 33-34; and Marc Trachtenberg, *Reparation in World Politics: France and European Economic Diplomacy, 1916-1923* (New York: Columbia University Press, 1980), 59-60.

[9] Trachtenberg, "Reparation at the Paris Peace Conference," 41.

[10] John Maynard Keynes, *The Economic Consequences of the Peace* (New York: Harcourt, Brace, 1920) https://archive.org/details/in.ernet.dli.2015.135642/page/n5/mode/2up.

(1922), and by most of his followers.[11] Wasn't there something terribly wrong here? I was also struck in this context by the well-known American economist Jacob Viner's review in 1947 of Etienne Mantoux's posthumously published *The Carthaginian Peace--or the Economic Consequences of Mr. Keynes*. The economists, Viner admitted, had realized at the time that Keynes's analysis was "technically defective at some crucial points, especially in its treatment of the alleged difficulties of 'transfer' of reparations." But they had kept their mouths shut because they found the Keynes argument politically congenial: "the political views which Keynes expounded with great force of exposition were those which Anglo-Saxon liberals of the 1920's, including the economists, shared almost to a man, and I suppose there then seemed little point in exposing technical flaws in an economic argument which had the virtue of leading to the desired political conclusions."[12] For me, the implications were enormous. The conventional interpretation of the early post-World War I period was simply wrong, and it was wrong because academics—both historians and economists—had not done their job as they should have.

So I was excited by what I had found. To my mind, what I had come up with was not just an interesting interpretation. I thought I could actually *prove* things—important things—on the basis of evidence I had found in the archives and elsewhere. It soon became clear, however, that few people shared my enthusiasm. But my dissertation was at least good enough to get me a job at the University of Pennsylvania in 1974, and the book that eventually emerged from it was good enough to get me tenure there six years later (although even that was touch and go for a while). But with that project completed I needed to think about what I would do as a second project. In my lecture course I had to cover the early Cold War period, and I wasn't very happy with how I dealt with it. There was Yalta, and Potsdam, and the Marshall Plan, and the Berlin Blockade, and the Korean War—just events strung together over time. I didn't have any real sense for the logic tying these things together—for why it was that events had unfolded the way they did.

And I was interested not just in the late 1940s, but rather in the whole post-1945 period. It was clear to me that the nuclear question loomed very large in this whole story. But it was equally clear that I did not know the first thing about it. So I needed to educate myself, and it was not hard to find out what those most important works dealing with the nuclear question were. And when I started to go through these writings, I was struck by how impressive a number of them were. I was quite overwhelmed by the intellectual power of Thomas Schelling's work, and I also very much liked Bernard Brodie's writings.[13] And I somehow got the idea that a good way to get some command of this material was to study how the key ideas developed. The result was an article I published in 1989 on American strategic thought in the 1950s and early 1960s.[14]

The historians, at that point, had not done much archivally-based work on nuclear issues. Some extremely important historical work was, to be sure, just beginning to come out. David Rosenberg's pathbreaking article on the "Origins of

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[11] John Maynard Keynes, *A Revision of the Treaty, Being a Sequel to The Economic Consequences of the Peace* (New York: Harcourt, Brace, 1922), https://archive.org/details/revisionoftreaty00byukeyn.

[12] Etienne Mantoux, *The Carthaginian Peace--or the Economic Consequences of Mr. Keynes* (New York: Oxford University Press, 1946), https://archive.org/details/carthaginianpeac0000mant/page/n7/mode/2up. Review by Jacob Viner, *Journal of Modern History* 19:1 (March 1947): 60-70; https://www.jstor.org/stable/1875662.

[13] I have in mind in particular Bernard Brodie, *Strategy in the Missile Age* (Princeton: Princeton University Press, 1959), https://archive.org/details/strategyinmissil0000brod; Thomas Schelling, *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960), https://archive.org/details/strategyofconfli00sche; and Thomas Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966). But I found practically everything Brodie and Schelling wrote on nuclear issues to be worth reading.

[14] Trachtenberg, "Strategic Thought in America, 1952-66," *Political Science Quarterly* 104:2 (Summer 1989): 301-334; http://www.sscnet.ucla.edu/polisci/faculty/trachtenberg/cv/stratthought(psq).pdf).

Overkill" had been published in *International Security* in 1983.[15] That article made a great impression on me—not just for what it revealed substantively, but also for what it showed about the kind of work that could be done, and about how historical work could play a key role in shaping our basic understanding of these issues. I also learned a lot from Bob Wampler about NATO strategy in the early 1950s. But on the whole the most important work on nuclear issues was done by non-historians: Schelling was an economist, Brodie was a political scientist, and Albert Wohlstetter, another leading figure in this field, had a background in mathematical logic. And I started at that time to interact with some of the political scientists working in this area. I had met David more or less by accident, thanks to an archivist in the old National Archives building in Washington, and he told me about the political scientist Stephen Van Evera, then at Princeton. Steve, in turn, put me in touch with Robert Jervis at Columbia, and I attended a workshop he ran there.

The political scientists I got to know were clearly very bright, but they simply did not see the world the same way I did. For one thing, in dealing with the question of what made for war they emphasized military factors much more than I did. But could they possibly be right? And if they were wrong, how could I prove that that was the case? I found such questions enormously stimulating intellectually. And in dealing with them my basic approach was naturally to focus on the historical side of that issue. Claims had been made, for example, about the role of military factors in bringing on war in 1914, and those claims could be studied in the light of the evidence. So I spent some time trying to get to the bottom of that issue, and I wrote an article summarizing my findings—although much to my annoyance about half of the manuscript had to be deleted from the version originally published.[16] I also thought I could get at the issue of how powerful one set of military factors was—the opening and closing of "windows of opportunity" and "windows of vulnerability" resulting from shifts in the strategic balance—by looking at the early nuclear age. And it turned out, to my amazement, that the political scientists had been right and I had been wrong—that "window logic" had in fact played a very important role in shaping the course of events during the whole period from 1945 to 1952.[17] I had argued a lot about this issue with people like Steve Van Evera, and now I was, in a sense, hoisting the white flag. But the whole experience convinced me of one thing—namely, that real insight can come from seeing that you were wrong. When you have that kind of experience, I like telling my graduate students, it's like finding gold in your hands—although I'm not quite sure how many of them believe me.

So in interacting with the political scientists I had stumbled upon what seemed to me a very powerful method for generating insight. I of course had not hit upon that method entirely on my own. Steve was named editor of *International Security* in 1984, and he asked me if I could contribute something to the journal. I had been doing research at the Kennedy Library, mostly on the Berlin Crisis and related issues, but one day when I had some extra time there I had gone through some of files relating to the Cuban Missile Crisis—mainly the files containing the official minutes of the meetings of the ExComm, the most important policy making body during the crisis. And about that same time, I had run into McGeorge Bundy, formerly President Kennedy's national security advisor, at a small conference at Columbia. Bundy told me that a transcript of the tapes of the first full day of meetings relating to that crisis, those held on October 16, 1962, had just been released by the Kennedy Library. So I read the ExComm minutes and the transcript of the October 16 meetings, mainly just out of curiosity, and I was struck by the fact that what I found in those documents was very much at odds with what I had been led to believe. All this was fresh in my mind when Steve asked me if I could write up something for the journal. So I put

[15] David A. Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960," *International Security* 7:4 (Spring 1983): 3-71; https://doi.org/10.2307/2626731 .

[16] Trachtenberg, "The Meaning of Mobilization in 1914," *International Security* 15:3 (Winter 1990-91): 120-150; https://www.jstor.org/stable/2538909. The full version of this article was, however, later published in my book *History and Strategy* (Princeton: Princeton University Press, 1991) under the title "The Coming of the First World War: A Reassessment," http://www.sscnet.ucla.edu/polisci/faculty/trachtenberg/cv/TrachtenbergWWI.pdf.

[17] Trachtenberg, "A 'Wasting Asset': American Strategy and the Shifting Nuclear Balance, 1949-54," *International Security* 13:3 (Winter 1988-89): 5-49; https://www.jstor.org/stable/2538735.

together something on the Missile Crisis based on that material. That draft, however, was not very good—much too discursive, with no real overarching argument.

So Steve put me in touch with John Mearsheimer, whom I had never met before, and John laid out for me a way of reconstructing the paper so that it could have real impact. John pointed out that in the literature on nuclear issues there were three basic arguments about how nuclear weapons could affect international politics. The first argument was that the strategic balance plays a key role in determining how political conflicts get worked out; the second argument was that nuclear weapons played no role at all—that they just "cancel each other out"—because the threat of going nuclear is simply not credible when the adversary can respond in kind; and the third argument was that the risk that matters might escalate could play a key role in determining political outcomes, and indeed that that risk could be deliberately manipulated for political purposes. Each of those three arguments would correspond to a particular interpretation of the missile crisis, and those interpretations could in turn be examined in the light of the new evidence. That approach would make the analysis systematic and therefore much more effective; it would show how historical analysis could be brought to bear on the major conceptual issues which the political scientists, and other non-historians concerned with the nuclear question, were interested in. So I rewrote the paper along those lines, and the new structure John had proposed worked beautifully—so well, in fact, that I thought he should be listed as co-author. Be he said no. That paper, he predicted, would make my reputation in the field, and he wanted me to get all the credit. That struck me as an extraordinary act of generosity. And to this day I'm amazed by how much work people put into helping other people with their work, even when they get very little credit for it—and this applies to editors as well as scholars. It's one of the things I really love about our field.[18]

But the key point is that I was coming to see that there was a method here: general, theoretical, arguments could be "translated" into particular interpretations of specific historical episodes; those interpretations could then be analyzed using standard historical methods; and, having set up the analysis in that way, the conclusions the historical analysis led to could have important theoretical implications. That method, it seemed to me, could be quite productive, and a number of political scientists, at any rate, seemed to like the work I was producing. That, of course, gave me an even higher opinion of their own intellectual abilities, and pulled me more into their world.

At the same time, I was coming to feel more and more marginalized within my own profession. Diplomatic history was increasingly viewed as old hat. Indeed, both diplomatic history and military history were coming to be seen as carriers of the wrong set of values, and some historians were beginning to wonder whether warfare was even a legitimate object of study. I remember once hearing a social historian (in Lawrence Stone's Davis Center seminar at Princeton) ask Russell Weigley, author of *The American Way of War*, "Isn't it true that military historians really like war?" And this was by no means an isolated example. The history profession as a whole was becoming increasingly politicized. I think, for example, of the American Historical Association's nuclear freeze resolution of 1982. I was particularly struck by one passage in the resolution: "as professional historians we feel compelled to warn our fellow citizens that in modern history all large scale accumulations of weapons by rival powers have invariably led to the worsening of their relations, and usually to war." "*Invariably*"? To take the most obvious counter-example, the United States and the Soviet Union built enormous nuclear arsenals in the 1950s and 1960s, but political relations were a lot better in the early 1970s than they had been in late 1950, when the two powers were on the verge of war.[19] This was a profession that was supposed to be committed to the truth above all else, but now the prevailing attitude seemed to be that if the politics were right the facts didn't matter all that much. And one often heard the argument that there was no such thing as objective truth anyway; that since everything was political in any case, one might as well not hide one's own political views; and, indeed, that it was morally proper for history to serve as a kind of bludgeon for advancing one's own (generally left-wing) political agenda. This, of course, was very much at odds with the values I had absorbed in graduate school and in which I still believed. And although I still had some good

[18] Trachtenberg, "The Influence of Nuclear Weapons in the Cuban Missile Crisis," *International Security* 10:1 (Summer, 1985): 137-163; https://www.jstor.org/stable/2538793.

[19] American Historical Association Nuclear Freeze Resolution, December 1982, from AHA *Perspectives*, February 1983, 3; http://www.sscnet.ucla.edu/polisci/faculty/trachtenberg/cv/AHAfreeze%28fin%29.pdf.

friends in the Penn history department—especially Bruce Kuklick, Alan Kors, and Tom Childers—I was becoming increasingly disaffected with the history profession as a whole.

So I was, in effect, being pushed out of my own profession and pulled toward political science. I, of course, never stopped being an historian, but I came over time to occupy a rather odd position between the two disciplines. And it was political science that gave me a home. I moved to the UCLA political science department in the year 2000. I thought of the Greek scholars who took refuge in Italy after the fall of Constantinople in 1453, or the Central European scholars who came to America during the Nazi period. My situation was of course by no means that extreme, but the political scientists had taken me in and I felt it was important to give them something in return. And the one thing I could give them was to show how the historical method could be used in their own work.

So one of my basic goals at that time was to show how historical analysis could be brought to bear on the issues the political scientists were interested in. That there was a *method* for doing so was a key theme tying together the various articles I published in *History and Strategy* in 1991, and also in my other collection of articles, *The Cold War and After*, published in 2012.[20] And I attacked that issue of method directly in my book *The Craft of International History*, which came out in 2006.[21] I had been asked to write a book of this sort by Alex George, and, given how I felt about Alex, I could hardly say no. I wanted to show how, in practice, historical work could be done—to demystify it a bit for the political scientists, but also to pass on to younger historians some of the things I had learned the hard way over the years. Most of these things were of a practical nature, but I did want to make some general points about how historical work could be done. One of the key points in the book, for example, is that historical analysis needs to be question-driven, and that the questions that drive it (in our field) are directly related to the great issues political scientists are concerned with—above all the central question of what makes for war or for a stable international system. The theory, in other words, does not provide you with the answers; it is of value mainly because it brings the questions into focus—questions you might never have been aware of if you had not developed or absorbed a certain theoretical perspective.

The thinking that served as the point of departure for my book on the Cold War is my main case in point.[22] That book, which dealt with the 1945-1963 period, was at its heart an attempt to solve a puzzle. In 1945 Europe was divided between east and west. The Soviets, to be sure, might have wanted to communize all of Europe—but not if it meant war with the United States. The Americans, on the other hand, might have wanted to see democratic regimes in Eastern Europe—but were unwilling to go to war with the USSR to bring that about. So didn't American power and Soviet power balance each other so completely that both sides were locked into the status quo of a divided Europe? And if so where was the problem? If both sides were locked into the status quo, how could there be any risk of war at all? That kind of thinking implied that the Cold War was puzzling—that it could not be explained as a simple consequence of the vast ideological differences between the two sides. But I would not have seen how puzzling it was if a certain theory of international politics—a theory that emphasized the importance of power political considerations—had not come into play.

And that kind of thinking drove the research process. It might have seemed in 1945 that the division of Europe should lead directly to a stable system—that both sides had little choice but to live with the status quo that had emerged from the war. The basic rule, in effect, was that the Soviets would call the shots on their side of the line of demarcation in Europe and the Americans and their friends would determine what happened on the western side of the line; if there had been no exceptions

[20] Trachtenberg, *The Cold War and After: History, Theory, and the Logic of International Politics* (Princeton University Press, 2012).

[21] Trachtenberg, *The Craft of International History: A Guide to Method* (Princeton University Press, 2006) http://www.sscnet.ucla.edu/polisci/faculty/trachtenberg/cv/craft.pdf.

[22] Trachtenberg, *A Constructed Peace: The Making of the European Settlement, 1945-1963* (Princeton: Princeton University Press, 1999).

to that rule, the system would have been perfectly stable. So if problems did arise, that could only be because there *were* exceptions to that rule. And, in fact, there was only one place in Europe where one of the major powers would not give the other a totally free hand, and that was western Germany. For very understandable reasons, the Soviets had to be deeply concerned with how strong western Germany would become, and indeed might conceivably be willing, in certain circumstances, to risk war over the issue. And that in turn meant that the German question—the question of German power, and especially the German nuclear question—*had* to lie at the heart of the problem. But to understand the German nuclear question, I needed to understand NATO strategy for the defense of Europe, and especially the role that nuclear weapons played in that strategy. That in turn meant I had to grapple with the basic problem of the role that nuclear weapons play in international politics in general, and in this area as well the theoretical literature was of fundamental importance. These were all for me very difficult questions, and it took me about twenty years to sort these things out—not that I had to do this work entirely on my own, since I was able in certain key areas to build on the work of other scholars like David Rosenberg and Bob Wampler. But the key point is that to do historical analysis effectively, you need a research strategy; that strategy is shaped essentially by a thought process, a process in which you're forced to grapple with theoretical issues—and to do that you need to think hard about the arguments you find in the theoretical literature, and to think about them in the light of the historical evidence.

Looking back today, I'm struck above all by how loose and undirected the whole process was. It is not as though I knew early on where I wanted to end up and had worked out even a rough strategy for getting there. And reading the essays in the "Learning the Scholar's Craft" series—many of them written by scholars whose work I've admired enormously—it's clear to me that most of us have not approached our careers in that way. The norm is rather different. You just move along, not quite knowing what your long-term goals are, interacting with people and responding to the situations that present themselves. But as you move along, you somehow get professionalized, you develop a set of skills, you become a kind of craftsman.

I was struck in particular by what Charles Maier said at the end of his essay in the series. He could not quite put his finger on what he had actually been taught in graduate school. He felt as if he had been "sherry ageing in a cask: despite the lack of outside intervention the elixir poured out after six or seven years was different from what had been poured in."[23] My feeling is much the same. You're not actually "trained" to be an historian in graduate school. You do learn certain skills, and you're helped to develop whatever talent you have. But you're not forced into a mold. The whole system, ultimately, is based on a certain respect for the young scholar as an individual. And the process does not end when you leave graduate school. You continue to feel your way forward—to move ahead not really knowing where things will take you, just doing the best you can—and that process never really ends. Which, to my mind, is probably the way it should be.

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http://www.sscnet.ucla.edu/polisci/faculty/trachtenberg/cv/cv.html. He is also the author of *The Craft of International History: A Guide to Method* (Princeton: Princeton University Press, 2006), which was designed to help younger scholars learn how to do work in this area.

[23] Charles Maier, "H-Diplo Memories," H-Diplo, 16 October 2020, https://hdiplo.org/to/E280.