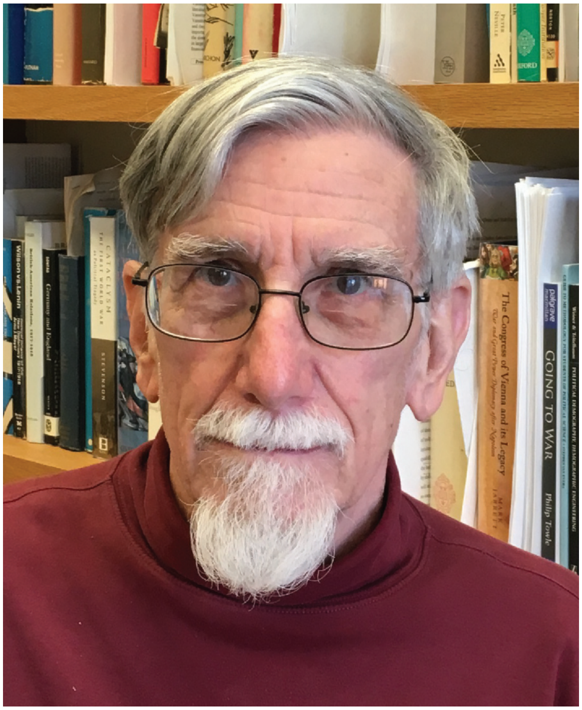

Robert Jervis

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Abstract

Throughout my life, politics and political science have been intertwined. I handed out leaflets for Adlai Stevenson at age 12, participated in protests at Oberlin and Berkeley, and, as I developed professional expertise, worked with national security agencies. Conflict has been a continuing interest, particularly whether situations are best analyzed as a security dilemma or aggression. In exploring this question, I was drawn into both political psychology and signaling, although the two are very different. I have continued to work on each and occasionally try to bring them together. My thinking about strategic interaction led to a book-length exploration of system effects, a way of thinking that I believe is still insufficiently appreciated in the discipline and among policy makers. My research continues to be stimulated by both developments in the discipline and unfolding international politics.

INTRODUCTION

I was born in 1940, so my first memories were of World War II, of my father coming homing home and saying that FDR had died, and of parades celebrating the end of the war. Of course what I remember now is influenced by what I have become, but politics, especially international politics, was a looming presence as I was growing up. The outlook of my parents and their friends was left-liberal on domestic issues, especially civil rights and civil liberties (two issues I have always engaged with, although I have not written about them), and, by the late 1940s, hardline on policy toward the Soviet Union. The intertwining of domestic and foreign policy was made most salient in the rise of Senator Joseph McCarthy, and his bullying demagogy struck home because friends of my parents were called to testify before his committee.

My environment was socially and politically homogenous. Almost all my parents' friends and the students at my school were Jewish (generally, like my parents, secular if not anti-religious) and liberal. I think there were a handful of Republicans, but they kept a low profile. The only way I stood out may have had some influence: I was a Brooklyn Dodger fan. The word "fan" hardly captures the degree of the attachment involved, and these were the heartbreaking years when the Dodgers would get to the World Series only to lose to the hated Yankees. Almost all my male classmates (in those heavily gendered times, the girls paid little attention to sports) were Yankee fans. I then gained an appreciation for the underdog, as well as a realization of the danger of treachery when Branch Rickey moved my team to Los Angeles. I also learned the sting of disappointment, and one of the last times I cried over something other than a death was when the Giants' Bobby Thompson hit the home run in the 1951 play-offs that capped the Dodgers blowing a 131/2 game lead. (More treachery: It is now believed that the Giants had stolen the sign for Ralph Branca's pitch.)

The early Cold War years were of course punctuated by crises and smaller incidents of conflict. Among the latter were cases in which American reconnaissance planes flying along the peripheries of the Soviet Union and related countries were shot down. Dismissing out of hand the Soviet claims that these aircraft were violating their territory—the United States would never do anything like that—I pestered my parents: What should the United States do in response? That question, in multiple contexts and forms, was to remain with me for the rest of my life.

I was a committed Democrat from the start and handed out leaflets for Adlai Stevenson when I was 12. (Although I now view him as flawed and have come to appreciate many of the virtues of the person who defeated him, I was particularly gratified to receive the Adlai Stevenson chair at Columbia.) In my teenage years I followed politics closely if naively. Like most of my peers, I thought President Eisenhower was not only misguided in his domestic policies (a judgment I would only slightly revise in retrospect) but also intellectually incapable of mounting a sensible foreign policy. The fact that he had successfully led the Allied armies in liberating West Europe, and perhaps even more impressively had navigated the treacherous waters of inter-Allied diplomacy, should have indicated to us that this was a man of considerable talents, but since we were teenagers, it did not. I remember being mortified for my country when Eisenhower gave a particularly incoherent answer to a press conference question about the current crises over Quemoy and Matsu, two small islands off the coast of China that were under Nationalist control. Many years later the documents were opened revealing the preconference discussion between Eisenhower and his press secretary, James Hagerty. Hagerty warned the President that he was sure to be asked the very delicate question of whether he would consider using nuclear weapons to defend the two islands. Eisenhower replied, "Don't worry, Jim, if that question comes up, I'll just confuse them" (Greenstein 1982, p. 69).

This is history, of course. As those with even a passing familiarity with my work know, although I cannot claim to have mastered any one period, let alone to have done archival research, my work relies very heavily on that of historians. I never found this work entirely satisfactory, however, and later explored what I thought were the main differences between the two fields [Jervis 2001b; also see the fascinating response by Schroeder (2001), from whom I have learned an enormous amount; Jervis 2010b]. Some of my dissatisfaction was crystallized when I audited a modern European international history course at Berkeley taught by Raymond Sontag. Since he was a great historian, it is not surprising that it was a marvelous course. The approach often seemed problematic, however. One exam question asked us to explain the rise of Prussia and the decline of Austria, and after the exams were handed back, to help the students for the next one he read out an answer to which he had given an A. My reaction was that although this was a very good account, it was descriptive rather than explanatory—to put it in today's language, it lacked an identification strategy. Later that year, I went to his office to discuss my interest in using history for theories of international politics. He could not have been personally kinder, and I was struck by the fact that he asked me about my background and my interests, and put this information on a file card for future reference. But his general stance was that political scientists could only muck up the understanding of history. Fortunately, many historians in younger generations were more welcoming, and as my career progressed I formed close personal and professional ties with many in the field. I could not do my work without them. But this gets ahead of my story.

COLLEGE AND GRADUATE SCHOOL

In the fall of 1958, I entered Oberlin College. (I was rejected by Swarthmore—something I mentioned to its president when I chaired the committee to review its political science department 30 years later.) Being somewhat shy and nerdy, I thought that a small college would be best for me and that a small town would be a good change from having spent all my life in New York. The former judgment was correct; the latter was not. I got a great education and made life-long friendships but also suffered from claustrophobia.

I knew I would major in either math or political science, and the experience of taking both a calculus and an American politics course made this an easy decision. The latter was taught by a marvelous instructor named Tom Flynn, and he started the course with a month of Supreme Court decisions. At first, I thought this was bizarre, but I found the material enthralling. Of more lasting significance, my college years coincided with the furor over the "missile gap," the widespread belief that the Soviet Union was far ahead of the United States in missile capability. The gap actually was in the Americans' favor, but those of us who lacked access to the U-2 photographs, telemetry on Soviet tests (a source at least as important as the overhead photography), and the information supplied by Oleg Penkovsky (one of the four most important spies for the United States during the Cold War), could not know this. Galvanized by a fascinating series of articles by Richard Witkin in the *New York Times* in the winter of 1959–1960, I wrote a paper on this topic for my own benefit.

In the summer of 1961, I went on a student exchange trip to the Soviet Union. Although my lack of Russian limited what I could learn, what I did see made the trip worthwhile. The first day in Moscow, a young woman in our group was approached by a friendly young man who took her around the city. At the end of the day, she gave him a phonograph record in appreciation, and immediately the police moved in and accused her of engaging in the black market. They let her go, but we all got the message. Although almost everyone we met was friendly, anything even semiofficial was filled with blatant lies. I was also frustrated because I was the only one in our group to have fairly deep political knowledge and the willingness to argue. As I have explained elsewhere, this led to my first contact with the Central Intelligence Agency (Jervis 2010a, pp. 6–7). It also led to meeting my future wife, who was one of the few in my group who put up with my constant concern with politics, my fruitless search for the *New York Times*, and my complaints about the food.

The trip was also important because, in preparation, the organizers had sent us the names of a few books we should read, among them Adam Ulam's *The Unfinished Revolution* (1960) and Joseph Berliner's *Factory and Manager in the USSR* (1957). They were simply stunning, more analytically penetrating than anything my professors had assigned. Berliner's study showed how a centralized economy inevitably led to the setting of incentives that, despite being designed to reach the appropriate goals, necessarily introduced pathologies. Although I did not cite this book much, it was always in the back of my mind when I wrote *System Effects*.

Two other books I read at Oberlin were even more important to me: Glenn Snyder's *Deterrence and Defense* (1961) and Tom Schelling's *The Strategy of Conflict* (1960). They were to be classics in the field, and the latter remains foundational for much of IR. I was enthralled by the concepts, insights, and methods of analysis. I had an instinctive sense for game theory and strategic interaction (i.e., situations in which one side tries to anticipate what the other will do, knowing that the other is behaving similarly, and in which the outcome can diverge from what either—or both—desires), but here I found these approaches laid out and developed. After that, I couldn't imagine analyzing politics without these tools.

Throughout the Cold War, the academic study of international politics was both enriched and distorted by engagement with the pressing issues of the day (Jervis 2004). Ideas about deterrence and concepts like first- and second-strike capability are abstract, but they were developed by Schelling and others at Harvard, MIT, and the RAND Corporation who were dealing with these questions on a very concrete level. In fact, Schelling (1966) wrote the first draft of much of *Arms and Influence* as a top secret government report. Later, my own interventions in the debates over nuclear strategy were similarly triggered by semiofficial engagements.

Both politics and political science drove my decision to go to the University of California at Berkeley for graduate school. Seeing a documentary about the protests that greeted the House Un-American Activities Committee when it visited San Francisco made me think that the Bay Area would be a good destination. Academically, Berkeley seemed a happy middle ground between Yale or Michigan, which looked to me too narrowly "scientific" in orientation, and Harvard, which appeared excessively traditional. In fact, I did not realize that Berkeley's IR faculty was very weak unless one wanted to study regional integration with Ernst Haas. But luck intervened. Berkeley had just hired Glenn Snyder for a two-year stint, and his IR theory course was my true introduction to the subject. My IR course at Oberlin had used Hans Morgenthau's *Politics Among Nations* as the text, and I found it (and still find it) muddy and prone to ex cathedra pronouncements unsupported by careful arguments and evidence. At that point, my interest was driven by international events, both past and present, not by scholarship. Through his lectures and assigned readings, Glenn, who later became a close friend, opened my eyes. His assigning Arnold Wolfers' *Discord and Collaboration* (1962) was particularly important. In these marvelous essays, I found what was missing in Morgenthau: concepts that were sufficiently separated from events to provide generalizable analytical tools yet not so far removed as to be unanchored, frameworks for considering alternative causal pathways (although Wolfers probably would scorn such language), and ideas that I could bring to other topics. I still return to this book for insight and inspiration.

Glenn himself provided these in full measure, and I was particularly impressed by his willingness to consider questions at length rather than coming to quick answers. In particular, I remember him saying to me, "I've been thinking about the question you raised with me several days ago and now realize that the answer I gave you was wrong, and let me tell you what I now think." Glenn was not flashy, however, and would admit when he could not really understand some of the new and trendy ideas in the discipline. In fact, the reason he could not was that many of them, such as structural functionalism in comparative politics, were badly flawed. But this kind of intellectual honesty was not valued at Berkeley, and his contract was not extended.

If Berkeley disappointed in some of my political science courses, it over-fulfilled my hopes for politics. The fall of my third year saw the Free Speech Movement, and by a quirk I became the Students for a Democratic Society's representative to its executive committee. Being one of the first to sit in front of the police car in Sproul Plaza made me think more about overcoming collective action problems. The rest of the protests throughout the semester both deepened my commitment to free speech and other civil liberties and taught some other lessons, especially that interactions were strategic, but not always in the way that the readings had suggested. Our campaign often lagged, only to be revived by foolish moves by the University. At the culminating point, I decided not to join my colleagues in the Sproul Hall sit-in but rather to sleep in my comfortable bed at home because I knew that if the administration called in the police, this would rally the faculty to our side and we would win. The phone call at 4:00 in the morning telling me that the police were hauling people out of the building drove home the point that playing with people who do not understand the rules of the game can produce odd outcomes. (In retrospect, I understand many of the pressures various factions within the university administration were under and can sympathize with the dilemmas they faced.)

I participated in some of the antiwar demonstrations, but my opposition was based, not on the view that the war was immoral or that America was particularly imperialist, but on the paper I had written in the spring of 1963 on what were then called "internal wars" that had convinced me that the United States and its South Vietnamese allies were unlikely to win. Contributing to this belief was what I had learned from bargaining theory—the importance of the balance of interest, that intensity of preference and willingness to suffer were major sources of power, and that North Vietnam's advantage on these dimensions outweighed the greater military might that the United States could bring to bear (Jervis 1972). Participating in demonstrations in the summer of 1967 at Berkeley also taught me that I really, really did not like tear gas, and I gained empirical evidence that I was in about the 90th percentile of cowardice as I always drifted to the far back of the crowd. While the war in Vietnam and the Free Speech Movement deeply radicalized many of my peers (and made conservatives out of a few of them), these experiences left my politics pretty much unchanged because they were already deeply rooted.

Back in political science, the spring of 1965 brought me another lucky break. I continued to have great interest in national security policy but had no ideas for a dissertation. I got one when Tom Schelling and Anatol Rapoport came to town for a debate on deterrence theory. The latter, although an early adopter of game theory, was a severe critic of Schelling's views. (My first book drew heavily on Schelling. Rapoport wrote a very critical review of it, missing the elements that should have been more congenial to him, but our subsequent meetings were friendly and productive.) In the spring of 1965, Vietnam was heating up and nuclear strategy was not exactly popular. So I told my colleagues they should come to hear Schelling, who I knew would impress them. He did, but not quite in the way I had expected. In talking about the basic ideas of mutual second-strike capability, he correctly argued that "overkill" was not its distinguishing feature because this had already existed. In many previous wars, such as World War II, the winners could have exterminated the losers. Using the example that later appeared in *Arms and Influence* (Schelling 1966), he said that after Japan had surrendered, the United States could have killed everyone in Japan. It would have been fine had he stopped there, but he said, "We could have killed everyone with ice picks." Those last two words undid all the proselytizing I had done.

Even though these words reinforced the Berkeley audience's impression that nuclear strategists were inhumane, the entire debate was crucial to me. As it unfolded, I came to realize that the central issue was not the general validity of deterrence or the alternative view espoused by Rapoport that I later called the spiral model [Jervis 2017d (1976), ch. 3], but rather which one (if either) applied to the Cold War, and this largely turned on competing views of the Soviet Union. The debate then gave much more sophisticated answers to the question I had asked my parents almost two decades earlier about how we should respond to the Soviets' shooting down American airplanes. While I thought the proponents of the spiral model (people who were later called "doves") were wrong about the Soviet Union, they were right that a crucial element in foreign policy was the image of the adversary and how states perceive each other. I knew I had a dissertation topic.

But as I did not have much guidance (Berkeley treated us as free-range graduate students), I both floundered and thrived. I read widely and learned a great deal, but aside from developing the point about deterrence and the spiral model, I wasn't making much progress. A few years ago I stumbled on a harsh but not inaccurate letter from Haas, who was on my dissertation committee. In an instance that drives home a Freudian truth, I had completely repressed the memory of it. Unpleasant as it was, the letter also shows one of Ernie's great strengths as a teacher. He treated us as grown-ups. Although our intellectual interests were very different, he gave me an enormous amount of his time, treated me with respect, and was never condescending. We were all treated as intellectual equals, which meant that although we did not have to adopt the ideas of the faculty members, we were expected to be able to rise to their level.

I was able to salvage something from my early explorations, especially my study of the content analyses by Robert North and his colleagues at Stanford dealing with the origins of World War I. Some of this work remains very valuable: Holsti (1965) found that in July 1914 each leader thought that the other side retained freedom of action while he himself had no choice but to act as he did, a key to understanding the interaction that has been confirmed by recent historical work. But much of the quantitative content analysis was badly flawed, I believed (and still believe), and my first publication was a critique in *International Studies Quarterly* (Jervis 1967, reprinted in a slightly different version in 1968). Soon thereafter, I received a stinging letter from North, who claimed I had failed to understand what they were doing. I mention this only because of the sequel: A few years later, North wrote a review of my first book, but instead of providing a payback, as many of us would have done, he was quite complimentary. I wish I could say that this is standard in academia.

By the middle of my fourth year, I thought I was making great progress (I was not) and was ready to reach out beyond Berkeley. I mailed a summary of my thesis to Karl Deutsch at Yale and to Tom Schelling, a somewhat presumptuous move I would hesitate before recommending to my graduate students today. Both kindly agreed to see me when I came East, and here again luck played a crucial role. Tom was spending the year in London but would be back at Harvard on a weekend that coincided with our spring break and could see me if I came by then. Tom liked what he read and saw of me and offered me a postdoc position.

HARVARD: MY FIRST JOB

It turned into a predoc as I realized that, rather than being near the end of my dissertation, I was in the middle of nowhere: I had bitten off more than I could chew. One section of the dissertation that had attracted Tom's attention was based on Erving Goffman's *Presentation of Self in Everyday Life* (1959). My point was that studying leaders' perceptions was like one hand clapping; the essential other hand was the effort of states to project images that would further their interests (what later became known as impression management), and these images might be accurate or deceptive. I asked my committee if I could expand the chapter and make it my dissertation, leaving for later the topic of perception that had been my starting point.

This became *The Logic of Images in International Relations* (Jervis 1970, reprinted in 1989 with a preface that further discusses the process). I still consider this one of my two most creative books, although it is not nearly as well-known as others, especially *Perception and Misperception in International Politics* [2017d (1976)]. Because the latter is so much better known, some scholars have cited *Logic of Images* as a work of political psychology. It only tangentially is. Rather, it is a combination of rational choice and what would later be called constructivism. It also was the hardest book to write because although Goffman and Schelling were my inspiration, there really was nothing like it. Unfortunately, I mistitled it. I should have called it either *The Logic of the Projection of Images* or, snappier, *Signaling and Deception*. The basic idea was that signalers and perceivers are in a close but not harmonious relationship. The signaler wants the perceiver to accept a desired image of it and to hold desired expectations of what it will do in the future (including how it will react to alternative moves the perceiver might make), while the perceiver needs to draw accurate inferences and realizes that the signaler may be trying to deceive it. The perceiver then needs to decide what behavior provides the best basis for its inferences, and the signaler has to try to understand this decision in order to project the desired image, whether accurate or not. A central question is what sorts of behavior provide the most valid information about the actor. I made the distinction between signals, which like ordinary language gain their meaning through implicit or explicit agreement between the parties, and indices that are meaningful because the receiver believes they are inextricably linked to the characteristics of interest. (To take a recent example, while President Trump's prepared statements are generally taken as signals, his tweets and spontaneous remarks can be seen as indices that represent his true beliefs and instincts and are diagnostic of attitudes.) I also distinguished between an actor's general reputation, which flows from perceptions of her previous behavior, and her signaling reputation, which refers to beliefs about whether she is prone to live up to her promises and threats. This distinction has developed some roots in the literature, although it is still too often ignored.

I won't provide further plot summary here (except to entice readers by noting that the book is filled with marvelous stories), but we can now see four sets of distinctions that significantly, but not entirely, overlap: signals and indices, words and deeds, tying hands and sunk costs (Fearon 1997), and cheap talk and costly signaling. The latter of course is best known, and although it is not identical to signals and indices, the overlap is not accidental: Michael Spence (1974), who originated the argument, was a student of Tom's and liked the drafts of my dissertation that Tom shared with him. I don't think any of us has gotten the relevant distinction exactly right, but I do think that many of my colleagues have adopted the categories of cheap talk and costly signals without careful contemplation and that this typology has more complexities and problems than are often recognized (for further discussion, see Jervis 2002a; a slightly revised edition appears in Jervis 2017e).

When I was struggling with these issues, I had another lucky break. Ken Waltz spent a sabbatical year in the office next to mine in the Center for International Affairs (CFIA). Actually, to be precise, our offices were in an old wooden building in back of the main CFIA building, and this mattered because it also housed the Reserve Officers' Training Corps program. The building was the target of vigorous student protests, where I learned that it is a lot less fun to be sat in on than to stage a sit-in, and failed arson attempts (the radicals had never been Boy Scouts). Ken and I talked for at least an hour a day—his wife said that when she needed to reach him, she would call my office first—and this is how I learned IR theory.

As an undergraduate and graduate student, I learned more from my peers than from my professors, good as the latter often were. The same pattern held true at Harvard, where the junior faculty formed a close-knit group that continually discussed issues of politics and political science. The group at Harvard was cohesive, partly because we enjoyed gossiping about our distinguished senior colleagues and there was no competition for who would get tenure—in the absence of regular tenured slots it was unlikely that any of us would. I ended up being considered, however, when Henry Kissinger's decision to stay in Washington created an opening. After a review that lasted almost two years, I was turned down. Given the fact that *Logic of Images* was far outside the mainstream and hard for my senior colleagues to understand and that *Perception and Misperception* was only half done, the decision certainly was reasonable. I accepted a position at the University of California at Los Angeles, and almost immediately upon receiving the offer could not recall the stress of that long, grinding review period. I knew I had felt it, but the affect had been drained; it was as though it had happened to someone else. Just like with childbirth, I have been told.

UCLA: *PERCEPTION*, THE SECURITY DILEMMA, AND WORK IN WASHINGTON

On arriving at UCLA, I set about completing my manuscript on misperception. I thought I had most of it completed but again was wrong. My hold over the psychological literature was weak, and so I spent the first year in the psychology library, reading through the past dozen years of the major journals. This was arduous but feasible because psychology journals, like political science journals, were far less numerous 40 years ago.

My children were quite young during the UCLA years, and so they (usually) welcomed parental attention. My productivity dropped a bit but did include two *World Politics* articles, one reviewing the state of our knowledge about deterrence (Jervis 1978) and the other, perhaps my best-known article, "Cooperation under the Security Dilemma" (Jervis 1979). I had discussed the security dilemma as the basis of the spiral model in *Perception and Misperception*, but I came to realize that perhaps the central question was not why states engage in conflict but how they are able to cooperate despite the danger that an increase in one side's security can make others less secure. (For excellent discussions of the security dilemma, see Booth & Wheeler 2008, Tang 2010; for my own application of the concept to the Cold War, see Jervis 2001a.) The basic idea was as old as Thucydides, and my contribution was to reconceptualize the dilemma as a variable rather than as a constant. What has received most attention is the argument that the dilemma can be mitigated if defensive weapons or policies can be distinguished from offensive ones and if the former are stronger than the latter. This theoretical argument I think holds up well, but as the extensive literature that followed demonstrates, it is far from clear how contemporary leaders and later scholars are to make this determination. Less attention has been paid to the first half of the article, which I think may be more broadly applicable. Drawing on the Prisoner's Dilemma, I argued that if we look at the cardinal rather than the ordinal payoffs, we can see that some dilemmas are harder to solve than others. Cooperation is easier if the advantages of defecting while the other side cooperates are only slightly greater than those of mutual cooperation; mutual cooperation is much more beneficial than mutual defection; and the "sucker's payoff" (i.e., what you get if you cooperate while the other defects) is only slightly worse than mutual defection. This leads both to hypotheses about when cooperation is more likely and to propositions about what states can and will do if they want to cooperate. I consider this fundamental to defensive realism, and it shows that the differences between this approach and neoliberal institutionalism are not as great as many believed (Jervis 1999).

This might be the place to briefly comment on the question I am often asked by graduate students: Am I or am I not a realist? To the annoyance of many of them, I have to say that there is no simple answer. In many cases, including mine, the label is better attached to particular books or articles than to scholars, who may write in different modes in different scholarly investigations. I am a realist in believing that states are often preoccupied by their security [although ironically the United States and other developed countries are now much more secure than they have been throughout most of history (Jervis 2016b)], that states remain the central actors, and that the inability of states to bind themselves (and the knowledge that others cannot be bound) is a central feature of international relations. But misperception, although not denied by realism, does not fit entirely well with it, and in studying American foreign policy, one would have to be blind to ignore the President's continuing need for domestic support. Furthermore, *Logic of Images*, while not directly contradicting realism, combines rational choice with constructivism, although the latter was not yet developed and the former was just emerging. Nevertheless, I see realism as a check on the propensity to see the United States as a uniquely benign or uniquely malign actor and as an encouragement to skepticism about the virtues of democracies in world politics. It is worth remembering that one of the main reasons why the thirteen colonies became united after the American Revolution was the belief that their being democracies (by the standards of the day) would not ensure their staying at peace if they remained separate states (Hendrickson 2003), and that in signing the peace treaty with Great Britain, the country broke its treaty obligations to France.

While I was at UCLA, politics again diverted me. Robert Bowie, the former Director of the CFIA, had become head of intelligence at the CIA, and he asked me to be a consultant (for the full story, see Jervis 2010a, pp. 7–14). I started working on Soviet intentions, but when martial law was declared in Iran in November 1978, Bowie wanted to know how his analysts could have so misled him when only the month before he had followed their advice and told a congressional committee that things were under control. This led to my doing a post mortem on the case (for the document and the story of how I wrote it, see Jervis 2010a, ch. 2). I learned that while perceptions were part of the problem, organizational culture and incentives loomed very large, and these were factors I had not previously focused on. I also got some limited sense of how the government functioned at the working levels. The impacts of seemingly mundane factors such as who had access to secure telephones and the role of informal networks were driven home to me.

In the course of my work on Soviet intentions, I went to see a former student in the systems analysis office of the Pentagon to ask about the rationale for what was then called the MX missile (later called the Peacekeeper). I had thought there were problems with the public explanation but assumed the experts had better reasons. He gave me a paper, and when I told him that it was totally inadequate, he grumbled a bit and dug deeper into his desk drawer and came out with a longer version. I read it and said, "This is a bit better, but not much. Give me the real paper." He replied "Bob, you have just seen an analysis so sophisticated that no one outside of this shop would read it or understand it if they did."

I admit to being shocked. While I thought that I might not be persuaded by the official analysis, I did think there would be something serious in it. So on the plane back to Los Angeles, I drew up a sketch for an article. The original title was "Why Minuteman Superiority Doesn't Matter," but as I thought about it some more, I realized the issue was a broader one: "Why Nuclear Superiority Doesn't Matter." After having been rejected by two high-ranking journals, it appeared in *Political Science Quarterly* (Jervis 1980), an outlet that lacks high status in the discipline but publishes more interesting articles than appear elsewhere, in part because of the influence of long-time editor Jim Caraley, who was willing to exercise his personal judgment.

This experience produced another lesson, which was that on the broad issues of foreign and defense policy, the government may know little of value that is denied to the general public. To take a contemporary example, if I want to know the current and projected North Korean military capabilities and the American ability to exercise various military options, I need high-level clearances. If I want to think and write about the virtues of alternative policies toward North Korea (e.g., regime change, economic sanctions, pressure on China, seeking a bargain), I probably do not.

More important for my academic career, writing "Why Nuclear Superiority Doesn't Matter" drew me back to nuclear strategy and more specifically to the broad debate between those who endorsed Mutual Assured Destruction (MAD) and those who denied that a second-strike capability was sufficient to deter the Soviet Union from launching a conventional attack on Western Europe. The latter school of thought drew on what my mentor Glenn Snyder (1965) had called the "stability–instability paradox," which pointed out that stability at the level of all-out nuclear war could enable a country to engage in provocations at lower levels of violence, secure in the knowledge that it would be suicide for its adversary to escalate. Proponents of this line of reasoning then called for the ability to defeat the adversary at every possible level of violence, or what was called "escalation dominance." This made some sense, but my other mentor, Tom Schelling (1960, ch. 8), had pointed out the potency of "the threat that leaves something to chance." That is, even without the option for controlled nuclear war-fighting, the defender could undertake actions that had some (unknown) probability of leading to nuclear war through miscalculation, emotions, misunderstandings, and failures of command-and-control systems. It could then deter without escalation dominance if it was seen as sufficiently motivated and willing to runs risks of escalation.

Although I had read all the relevant literature, I really didn't fully understand the conflicting arguments and the assumptions that underlay each position until I had to explain escalation to my students. The result was *The Illogic of American Nuclear Strategy* (Jervis 1984). This came out in the middle of heated debates during the Reagan administration and the widespread but mistaken belief that the Soviets were ahead of the United States and thought that they could fight and win a nuclear war. I was gratified when one Defense Department official told me I understood the government position better than those who espoused it, but not surprisingly this did not end the argument. My writing on the subject continued, culminating in *The Meaning of the Nuclear Revolution* (Jervis 1989). The basic claim is that what my UCLA colleague Bernard Brodie had written in the months after Hiroshima was correct: "Thus far the chief purpose of our military establishment has been to win wars. From now on its chief purpose must be to avert them. It can have almost no other useful purpose" (Brodie 1946, p. 76). Or, as Presidents Reagan and Gorbachev said at the Geneva summit meeting in 1985, "a nuclear war cannot be won and must never be fought." Building on my predecessors' exploration of these issues, I argued that this was a revolutionary change in world politics, and I drew from the argument a number of propositions about what we should expect the world to look like if this were correct. Not surprisingly, I found the description of a "nuclear revolution" to be quite accurate.

Recent scholarship has questioned whether mutual second-strike capability was as secure as I and most others believed, and whether we still live in a MAD world (Green & Long 2015, 2017; Lieber & Press 2017). I think it is now clear that the Soviets were at least as worried as American officials in the 1980s, and with better reason. It is also clear that technological innovations have now greatly increased the vulnerability of strategic systems. As far as I can tell, neither the Obama nor Trump administration has directly considered whether it should seek first-strike capability against Russia and China. Intellectually, this is inexcusable, but politically it is very understandable. The relevant studies and deliberations could not be kept secret, and the political furor that would result would have serious consequences for both domestic and foreign policy.

Although I stand behind the arguments I made in *The Illogic of American Nuclear Strategy* and *The Meaning of the Nuclear Revolution*, and believe that they represent a significant scholarly contribution, they were also interventions in a fierce political debate. I had no illusion that they would influence policy, but I was trying to persuade as well as analyze. This intention matters because it led me to deemphasize the question of why, if the nuclear balance was so stable, the policy of multiple options was dangerous (as well as unnecessary). Relatedly, I said little about what was clear from my work on perceptions: If leaders on either side believed that nuclear superiority mattered, that belief would affect their behavior. These are nasty problems, and I think that had the political stakes not been as great, I would have delved into them more deeply.

Considerations of politics also affected one section of my Iran post mortem. Because I was sure that the report would leak (it did not, in part because Director of Central Intelligence Stansfield Turner thought it was so explosive that he limited circulation to a handful of his top officials), I downplayed the fact that while CIA analysts believed that the Shah would use massive force (crackdown was the term used) if he thought his reign was endangered, Washington was urging him to continue his liberalization. The reason for the discrepancy was that the CIA was supposed to steer clear of American policy. (Indeed, when I asked the lead analyst whether he had considered the possible impact of what the United States was telling the Shah, he said he really hadn't even noticed this, even though it was clear in the reporting cables he received.) I noted this finding but did not dwell on it or the parallel failure of American policy makers to carefully consider whether their advice might have undercut both the Shah's self-confidence and one of the assumptions on which the reassuring intelligence estimates were based. As a Democrat, I did not want to give President Carter's opponents extra ammunition.

I did not expect enormous changes to be made in response to my analyses, and indeed I made no explicit recommendations. But I did think there would be some reaction. In fact, there was none—never even an acknowledgment, let alone an invitation for further discussion (Jervis 2010a, pp. 27–29).

COLUMBIA: INSTITUTIONS, SYSTEMS, AND LEARNING FROM POLITICS

It was partly thanks to my security dilemma article that in 1980 I received an offer to replace William T.R. Fox in the IR theory slot at Columbia University. The other main contender was Ken Waltz, who had studied under Bill and had just completed *Theory of International Politics*, the landmark study he was working on when we were both at CFIA ten years earlier. (As Tom Schelling told me, "It takes Ken a long time to write a book because we are going to be reading it for a long time.") I got the job not because I was a better scholar than Ken but because Columbia believed that I had the potential to come close to his level and that at this stage in my career, and with my personality, I would be more of an institution-builder. In the 1970s Columbia had suffered very badly. It never had a large endowment and was about to launch its first major fundraising campaign when the protests of 1968 erupted, giving the university an enormous black eye. The subsequent decade also saw New York's fortunes sink and the result was that the university largely stagnated. I had led UCLA's rebuilding program and also, along with Bob Art, had started the *Security Studies Series* with Cornell University Press, which has become the leading series in the field. At Columbia, I devoted serious time and energy to hiring new faculty not only in IR but in other subfields as well. This ruffled some feathers, but most of my colleagues had a nose for excellent scholarship and, with some increase in a flow of resources, we rebounded.

To finish up the story of my role in building institutions, in 2010 I founded the International Security Studies Forum (ISSF), an online venture linked to the H-Diplo listserv that hosted roundtable reviews of important books and articles, some of which sparked intense debate among subscribers. By being linked to H-Diplo, ISSF facilitated exchanges between IR scholars and diplomatic historians, a connection of great value not only to me personally but to both disciplines. I also added Forums on subjects like the role of biology in international politics and the impact of our political preferences on our research (a topic to which I will return), and, most recently, a series of contributions on President Trump and world politics.

I might mention two other informal institutions I established at Columbia, building on my experiences at Harvard and UCLA. One was a seminar on items from the literature that I selected; discussions went well because the authors were not present. The other was the lunch group. Whenever I don't have a meeting, I check in with colleagues and those of us who are available join for free-flowing discussions of politics (national and international—rarely university level) and political science. These are not only refreshing but help build informal ties across the generations, subfields, and methodologies. I think this is one reason why the divisions in my department lack the bitterness they have at many peer institutions, although of course causation runs the other way as well.

In the late 1980s and early 1990s, world politics intervened again. The Cold War ended in a way that took most of us by surprise. I could not resist venturing arguments about what this new world would be like and what the implications were for American foreign policy and IR theory [Jervis 1993b (also see the rebuttal by Huntington 1993), 2006, 2009, 2016b]. I made this the topic of my presidential address at the American Political Science Association meeting in early September 2001 and argued that the leading countries of the world now formed what Karl Deutsch called a "pluralistic security community" (Deutsch et al. 1957) within which war was unthinkable, that this was a revolutionary change in world politics, and that it posed challenges to IR theory (Jervis 2002b).

The 1990s were difficult years for me personally because I developed a (thankfully mild) case of chronic fatigue syndrome, a disease that remains mysterious and without effective treatment. While I had energy enough to go through my daily routines, I was exhausted and extremely irritable in the evenings, so my wife paid a significant share of the price.

I published a string of what I think were quite good articles in this period (Jervis 1991a, 1991–1992, 1993a), but my main project was *System Effects: Complexity in Political and Social Life* (1997). I think this may be my most important book, but because it draws on and applies to so many disciplines (it has been assigned in medical schools, for example), it has received less attention in political science than my other scholarship. To make a long and complex story short, I try in this book to show that interactions produce a variety of unintended consequences, nonlinearities, feedbacks, and second-order consequences that actors often fail to anticipate and scholars often miss. When I gave a presentation to colleagues, my friend Dick Betts said, "Well, Bob, does this make you a conservative because trying to change things often ends badly?" This hard question led me to the final chapter, "Acting in a System." For all this I drew heavily on Tom Schelling, of course, especially his *Micromotives and Macrobehavior* (1978). For those who have not read my book, I will just say that you can read the quotation from a distinguished marine biologist on page 1 and if you see the problem you don't have to read further.

I have not been able to follow out these lines of argument as much as I would like (one effort is Jervis 2012), but they did influence my thinking on "complex causation" (Jervis 2013b) in an essay arguing that many of our standard social science methods based on comparisons cannot catch everything of interest. We often focus on the factors that directly bear on an actor's decisions but neglect the forces that led the actor to be in the situation she is in and to hold the beliefs and values that she does. Here I was much influenced by historians in general and Paul Schroeder in particular, and I also remembered that when I had lectured about the Prisoner's Dilemma to a general audience, one person asked me, "Who gave the District Attorney the power to put the prisoners in that position?"

Although I had been mulling over some ideas about systems and interactions for years, I might not have started writing about them had I not been asked to contribute a chapter for Tom Schelling's festschrift (Jervis 1991b). This was not the only case in which my agenda was driven by unexpected opportunities and invitations. For example, sometime around 2004, Mel Leffler and

Arne Westad asked me to write a chapter for the *Cambridge Handbook of the Cold War*. Since this has been a continuing interest of mine, this invitation made some sense. The topic they assigned me—the role of Soviet and American identities—did not. Nevertheless, since the deadline was two years away, I accepted, only to find that when the time came I was so pressed by other obligations and so uncertain about what I would say that I nearly backed out. But then I enjoyed writing the piece (Jervis 2010c) and have been gratified by the fact that several reviewers of the collection singled it out for praise.

Serving as president of the APSA in 2000–2001 was a great honor, and the only inconvenience was that I had to buy a suit to look respectable. During my term, the association was roiled by the "Perestroika Movement," which decried what its members saw as the discipline's following economics in a methods-driven, narrow, and technocratic path that ignored normative questions and squeezed the life out of study of politics (for a collection of essays on Perestroika, including one of mine, see Monroe 2005). I thought that while some of the rhetoric was overheated, the questions being raised were not only legitimate but vital, and I regret the diminution of these conversations.

As president, I tried to engage colleagues on two professional issues, albeit with little success. First, I realized that Perestroika's ire against the *American Political Science Review* was partly generated by the fact that many departments used publication in this journal as a requirement for tenure, and I argued that judging colleagues by where they published was lazy and unprofessional. A great deal of dreck gets published in the "leading" journals; many excellent works appear in low-ranked ones. Our professional responsibility is to read the material of candidates and reach a conclusion about quality; to outsource our judgments to anonymous referees and overworked editors is simply irresponsible. Part of the reason for the trend toward this, which of course has continued despite my efforts, is that colleagues and university administrations are impressed by economics (after all, economists play a larger role in American society and get higher salaries than we do), and this discipline has a shared theoretical framework and a clear hierarchy of journals. I recently heard of a respected political science department that sought an agreed-upon ranking of journals to guide tenure and hiring decisions.

My other lost cause was to get colleagues to think about the ways in which our research was influenced by our political views. Most political scientists are liberals of one stripe or another. With the rise of rational choice, we have more libertarians and fans of market mechanisms, but these views are still a distinct minority, and the number of social conservatives is astonishingly low. While we correctly worry about our shortfalls in gender and racial diversity, few seem concerned about the lack of ideological diversity, or about the huge difference in religiosity between political scientists and those we study and teach. Of course the rebuttal is that we are doing science, and our political views do not affect the questions we ask, the propositions we entertain, or the judgments we reach. Perhaps not, but even had I not written a great deal about the theory-driven nature of perceptions, I suspect that I would have found this claim implausible. I continue to be distressed by how few colleagues seem to share my concerns (for a powerful discussion of this issue in social psychology, see Duarte et al. 2015 and the responses that follow it).

I put the text of my APSA presidential address in the mail to the office of the *American Political Science Review* on September 10, 2001. The next day changed all of our lives, of course, and also altered my research agenda. In the ensuing days I urged Jim Caraley, the editor of *Political Science Quarterly*, to put together a special issue on 9/11 and contributed an article of my own (Jervis 2002c). Like almost everyone else, I favored the invasion of Afghanistan (and, like many others, have had second thoughts), but like most IR realists I opposed the war in Iraq. At first I didn't think this was a real possibility, but in this period I was making frequent trips to Washington, and by the end of winter 2002 realized that war was not only possible but likely. I joined with other IR realists in signing an ad in the *New York Times* in the fall stating our opposition. This has proved very useful because some critics of realism saw the war as following from this approach. The center of my own argument was that even if Saddam Hussein produced nuclear weapons (and I saw no reason to doubt the intelligence reports that he was seeking them), he could still be deterred from most dangerous adventures (Jervis 2003). As the broader Bush Doctrine took shape, I tried to unpack its assumptions and point out its fallacies. I put these and other essays together in *American Foreign Policy in a New Era* (Jervis 2005), and while US policy has changed, partly as a result of the disastrous invasion of Iraq, I think that this analysis remains valid.

One of the most controversial policies of the Bush "war on terror" was the use of what the administration called enhanced interrogation techniques on suspects it had captured. In addition to a wide range of media exposés on the subject, the Senate Select Committee on Intelligence (SSCI) produced an enormous report, including a declassified 500-page summary. This itself was highly controversial, being offered by the Democratic majority with the Republicans and CIA vigorously dissenting. Gideon Rose, the editor of *Foreign Affairs*, thought I would let the chips fall where they may, and so he asked me to review it. I am glad to have done so (Jervis 2015a), although it involved a great deal of work because the report was not only detailed but badly constructed. In the end, I agreed with the SSCI that the techniques constituted torture, but I found its arguments that the CIA had misled the relevant congressional committees and even the leaders of the administration to be self-serving and implausible.

More importantly, the conclusion that torture did not yield useful information, although perhaps correct, was produced by faulty methodology, especially the excessive use of hindsight and the failure to understand the problem of separating true signals from noise that had been highlighted by Wohlstetter's (1962) classic study of Pearl Harbor. Some of the errors were caused by the fact that the staff was composed of more lawyers than trained social scientists, but many of the errors were not innocent. I do not mean to sound shocked, shocked, that elected officials play politics, but on an issue like this I was disappointed in the political divide and felt that Democrats had done a disservice to the country by dismissing the very real possibility that torture was immoral and yet effective. Much of my work on political psychology focuses on people's propensity to avoid confronting painful value trade-offs, and I think this very human way of protecting our psychological well-being significantly reduces the quality of our decision making. This is what the SSCI did by taking the easy way out. It is probably true, however, that the report made it much less likely that the US government will resort to torture in the future. As several leaders of the intelligence community have said, even if the President gave such orders, the organizations would not obey. So perhaps a dishonest report led to a good outcome, and there may have been no other way to reach this goal.

If the biggest shock to the international system and American foreign policy was the invasion of Iraq itself, the discovery that Saddam did not have active programs to develop weapons of mass destruction follows close behind and still reverberates in current arguments on whether intelligence can be trusted. My work on deception and perception of course had always given me a special interest in intelligence, and as I explained above, I had done a post mortem on why the CIA was slow to see that the Shah was in deep trouble. Then in the mid-1990s I was put on the CIA's Historical Review Panel (HRP), which I started chairing a few years later. When the SSCI issued a long, detailed, and analytically weak report on the failure, I saw an opportunity. I knew that post mortems entailed an enormous amount of digging through primary materials, but the SSCI had just done the job of a diligent but somewhat dull graduate student and provided much of the raw material out of which something significant could be made. My work at the HRP had given me high-level security clearances and close contact with top CIA officials, and so I emailed them to offer to assemble a team of experts on intelligence failures to do the job right. Much to my delight (and, I admit, surprise), they took me up on it, and because so much other material was public, I was able to publish a version of our findings, along with an evaluation of the various American and British official studies (Jervis 2006). Soon thereafter I got the CIA to declassify most of my Iran study, and I put this together with an essay on why the intelligence community and policy makers are doomed to clash in *Why Intelligence Fails* (Jervis 2010a).

Unfortunately, by the time our study was finished, leadership at the CIA had turned over, and the new director Porter Goss and his leading deputy had no interest (the latter later went to jail, not for incompetence, but for taking kick-backs). They thought that they had solved the major problems (they had not), and, in any event, were simply overwhelmed by the burdens of the job. But this work may have had at least some impact, and one of the authors of a recent important intelligence estimate told me that he and his colleagues looked at our study to make sure that they did not fall into the same traps. This work also solidified my contacts with analysts at the CIA and the National Intelligence Council, and I have continued to do studies for them and the National Security Council staff on a variety of issues. Unless I live longer than expected, I will not see those studies in print, but they gave me insights and general ideas that I can use, and they may have been of some utility to the government.

I have found almost all my interactions with people in the intelligence community to be enjoyable, and I especially value the hours I have spent with front-line analysts who almost without exception have been informed, dedicated, and highly motivated to get the right answers irrespective of the political atmosphere and implications. In most cases I could not infer their policy preferences from even long discussions with them, and more than once I have had to tell analysts that a point they were making was politically explosive and that they should word it with great care and be prepared to supply further evidence on request. In some cases, they did not see the political implications. In others, they simply did not think they were relevant.

Partly because of the constant press of commitments, in the past decade I have found that review essays are a good outlet for me. They require me to read a book carefully and to try to think through its broader implications. Taking advantage of the ISSF's flexible format, I have probed several interesting books for what they can teach us and the arguments we need to explore more deeply. For *Political Science Quarterly* I have analyzed the memoirs of Director of Central Intelligence George Tenet and Undersecretary of Defense Douglas Feith, especially for issues surrounding the war in Iraq (Jervis 2008–2009), and the memoirs of British Prime Minister Tony Blair and US Secretary of Defense Robert Gates for how decisions are made (Jervis 2010–2011, 2014a).

In recent years I have returned to political psychology, reissuing *Perception and Misperception* with a long preface on how the field and my ideas have changed since it was first published and also collecting the essays on political psychology that I have written over the years. While they do not form an entirely unified argument, I think they show how political psychology can be applied to a wide range of questions and can explain what we cannot understand if we neglect "how statesmen think" (Jervis 2017e). I am glad to see the growth of the field, including the use of experiments, which can elucidate causal mechanisms even if they may lack external validity. The role of biology, long scorned because of a knee-jerk association between it and determinism and racial and gender hierarchies, is also becoming a legitimate object of study (e.g., Hatemi & McDermott 2011). Bringing together two of my favorite subjects, scholars are also analyzing intelligence through the lens of political psychology (e.g., Yarhi-Milo 2014, Bar-Joseph & McDermott 2017).

Politics continues to intervene, of course. Like most of my colleagues and the media, I was surprised by Donald Trump's election. I was also horrified and continue to be not only deeply disturbed by his domestic policies but also worried that he could get us into war—most obviously with North Korea or Iran, although armed conflict with China or even Russia is not out of the question. It does not take a Ph.D. to reach these conclusions, so I have tried to write pieces that seek to understand rather than condemn what is happening. I wrote an essay for the ISSF treating Trump as a natural experiment for IR theory (Jervis 2017a), drawing on the framework in *Man, the State and War* where Waltz (1959) laid out what were later called three levels of analysis. I also dissected what I saw as the unusual weakness in Secretary of State Rex Tillerson's position (Jervis 2017b), being inspired by Neustadt's [1990 (1960)] classic exposition of the sources of presidential power. I continue to have discussions with friends in Washington about current foreign policy issues, especially concerning Iran and North Korea (for my defense of the nuclear agreement with the former, see Jervis 2013a, 2013c, 2015b; for some thoughts on the latter, see Carlin & Jervis 2015; Jervis 2017c, 2018), and have some projects on cyber conflict (Jervis 2016a). My arguments generally draw on both political psychology and deterrence theory, which colleagues and I brought together during the Cold War (Jervis 1982–1983, reprinted in Jervis 2017e; Jervis et al. 1985).

I also hope to return to the unfinished task of bringing signaling and perception into closer alignment. As mentioned above, they are logically two sides of the same coin. They have rarely been treated together, however, and in fact as I and others have explored them we have used very different methodologies for each (Jervis 2002a, reprinted in Jervis 2017e). I also continue to be fascinated by historical episodes, especially the Vietnam War (Jervis 2000, 2010d, 2014b) and the origins of World War I (Jervis 2017f). Perhaps the most important insight to come from these investigations and my general reading of international history is that the best analogy to international politics is not the game of poker, but the Japanese short story and movie *Rashomon*. The same tale is told from the vantage point of four participants, and what they say they saw is wildly different. This is true of many international interactions. States often live in different perceptual worlds and have little idea how others see situations or them. As a microcosm, the accounts of a conversation recorded by two diplomats often reveal significant discrepancies, and in some cases it is hard to believe they were describing the same exchange.

In closing, I should mention how much I enjoy teaching, even the introductory undergraduate class. Indeed, I probably enjoy it more than the students do. But I am able to challenge and intrigue at least some of them, and teaching this course requires me to identify the essential points that need to be conveyed. (One of my undergraduate professors required us to limit each answer to one blue-book page, an exercise I found frustrating but now appreciate.) I also enjoy teaching graduate students, and my course "Signaling and International Communication" gives me a chance to explore many of the topics I have touched on here. Because it is open to Masters students in our School of International and Public Affairs as well as Ph.D. students, there is great cross-fertilization between practical and theoretical experience and concerns. Few of our Ph.D.s have heard about the use of "nonpapers" in diplomacy, for example. But they should.

It has been a source of great pleasure to see so many students go on to contribute to scholarship and public policy. I don't want to name any because that would leave many others off the list. For all my complaints about our ideological homogeneity, I am particularly gratified that my students have served in Republican as well as Democratic administrations (although I suspect that only a few will be Trump appointees). The petty annoyances and narrow-mindedness that characterize so much of academic life are trivial compared to the pleasures of having been able to work with such a broad array of committed young people and following my own interests where they take me.

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