Marc Trachtenberg

THE COLD WAR AND AFTER

History, Theory, and the Logic of International Politics

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Preface

I FIRST BECAME interested in international politics almost half a century ago, during my freshman year in college at Berkeley in 1963. From the start I knew that this was the field I wanted to go into, and in fact I have spent practically my whole life working in this area; my particular focus has been the history of great power politics in the twentieth century. One of my main goals at this point in my life is to pass on what I have learned over the years about how historical work in this area can be done, especially to people just starting out in this field. To that end I recently published a book on historical method called *The Craft of International History*. But although I tried there to be as concrete as I could, it seemed to me that I could do more to show how in practice an historian interested in this area of scholarship could proceed.

If I had to sum up in a single sentence what I have learned over the years about how historical work on international politics should be done, it would be this: the key to doing meaningful work in this area is to find some way to get conceptual and empirical issues to link up with each other. I don't think it makes sense to approach the core issues that this field is concerned with—above all, the great problem of war and peace—on a purely abstract level. That sort of theorizing, to my mind, cannot in itself take you very far. On the other hand, a purely empirical approach is also fairly sterile. There is not much point to simply accumulating a lot of facts. You need some way to figure out what they mean, and to do that you need to bring a kind of conceptual framework to bear—if only to generate the questions the empirical evidence can help answer. But general points of this sort are in themselves rather anemic. Their meaning sinks in only when you see how historical work that takes those principles as its point of departure can actually be done.

The basic aim of this book is therefore to show through example how to go about doing that kind of work. Only one of the articles included here (chapter 2) is directly concerned with issues of method, but all the articles, in one way or another, show how that fundamental approach works in practice. The first chapter, for example, on the question of realism, shows how the sort of understanding that takes shape in your mind as you grapple with historical problems can be brought to bear on core theoretical issues—on fundamental questions about what makes for war or for a stable international system. It was written as a kind of reaction to what I had found in the international relations literature. It

seemed to me that the claim I found there about how the international system worked—about how the competition for power was the fundamental source of conflict—was simply wrong. Over the years, thinking about particular historical episodes, I had reached very different conclusions about power politics and the causes of war. Those conclusions could be made explicit; there was a certain value, I thought, in showing directly how the historical analysis related to the specific claims that the theorists made.

But it is not just that historical study can have a real impact on your understanding of how international politics works. The connection works the other way as well: grappling with conceptual problems can have an enormous impact on how you do historical work. The goal of historical analysis, after all, is not simply to recount what happened. The aim is to go beneath the surface and try to bring out the logic underlying the course of events. In working out that logic, you have to draw on your whole understanding of why states behave the way they do and why they sometimes go to war with each other. This is not because theory in itself can give you the answers. A theoretical framework can never replace detailed historical analysis. But bringing a theoretical perspective to bear helps you see what is puzzling in the episode you are studying and thus gives you some sense for the specific questions you need to focus on.

The third chapter, on American policy toward eastern Europe in 1945, is a good case in point. This chapter might look like straight history and you might think that theory plays no role there at all. And yet the original article would not have been written if I had not found the coming of the Cold War so puzzling. American power and Soviet power, it seemed, balanced each other so completely that both sides were locked into the status quo; but if that were the case, where was the problem? Why wasn't the status quo of a divided Europe perfectly stable from the very start? That whole way of looking at things was obviously grounded in a certain sense for how international politics works—for how power realities shape policy. That conceptual framework in itself, of course, could not explain the coming of the Cold War. If anything, it "explained" something that did not happen in the immediate postwar period: a U.S.-Soviet accommodation based on a joint acceptance of the status quo.

But this sort of thinking helps you see what the questions are. Were both sides, you wonder, simply incapable of thinking in power political terms? Was it out of the question that they could agree to an arrangement based on a common acceptance of a divided Europe? Were the Americans, in particular, never willing to accept a Soviet-dominated eastern Europe, no matter what the Soviets were willing to give in re-

turn? Approaching the problem in this way allows you to see things you might otherwise be unable to see—the importance, for example, of Secretary of State Byrnes's references to the Polish precedent at the September 1945 Council of Foreign Ministers meeting, and of the way eastern Europe and Japan were tied together in the U.S.-Soviet negotiations at the end of the year.

And this kind of approach makes sense not just when you are trying to deal with a relatively narrow historical problem like U.S. policy toward eastern Europe in 1945. It is of even greater value, I think, when you are tackling a much broader subject. In the paper on the 1963–75 period, for example—chapter 6 in this book—my goal was to focus on fundamentals. What did each side want? What were their core interests? What, if anything, were they willing to go to war over? Certain assumptions of a theoretical nature, about what states really do care about, play a key role when you are trying to get a handle on this kind of problem. The western powers, you assume, had to be concerned with the growth of Soviet military power; they had to be concerned with how the USSR could be kept at bay when the American nuclear deterrent was becoming increasingly hollow, and when the most exposed European ally, West Germany, was unable to build a nuclear deterrent of her own. That in turn brings a whole series of questions into focus: how, in the military sphere, did the western powers propose to deal with this problem? How was that situation related to what was going on in the political sphere? Was there a real risk, for example, that western Europe in general, and West Germany in particular, would become "Finlandized"? Is the German Ostpolitik of the Willy Brandt period to be understood essentially in that context—that is, as an example of "Finlandization" in action? And what are we to make of Soviet policy in this period? How is the Soviet military buildup to be explained if the USSR's basic political goal was the stabilization of the status quo in Europe?

The paper itself does not try to answer questions of this sort in any definitive way. It is essentially a rough cut, an attempt not only to sketch out the structure of great power politics during the middle period of the Cold War, but also to give some sense for the structure of the historical problem here—the problem of making sense of what was going on during that period. My aim when I wrote it was to show how a major problem of historical interpretation—whether there was a real risk of war during this period, or whether great power politics was fundamentally stable—could be broken down into its component parts. The way those big issues are dealt with will then depend on how relatively narrow and relatively concrete questions are answered; as you deal with those specific historical problems in this general context, you automatically

deepen your understanding of what was happening in the period as a whole.

So a general analysis, like the one included here in chapter 6, can take you only so far. At some point you have to switch gears and do more detailed historical work—never entirely losing sight, of course, of the general historical problem you are concerned with. In the case of the article on the 1963–75 period, the whole question of relations between the United States and its European allies looms large, and you can get some insight into that basic issue by looking at one important bilateral relationship. That is the connection between chapter 6 and chapter 7, the paper on the Franco-American relationship during the Nixon-Pompidou period. A detailed study of this sort can give you a feel for the texture of U.S.-European relations during that period; but it is ultimately just a brushstroke—just one of a whole series of studies you need to do to get to the bottom of this general historical problem.

The main issues I was concerned with in those two papers—U.S.-European relations and military issues, especially issues relating to nuclear weapons—have long been of interest to me, and a number of the other articles in the book deal with those questions: the paper I did with Christopher Gehrz on the German rearmament question in 1950 (chapter 4), the piece on MC 48 (chapter 5), the article on "Preventive War and U.S. Foreign Policy" (chapter 8), and the paper on the Iraq Crisis of 2003 (chapter 9). One of the great questions that anyone interested in the post–1945 period has to be concerned with has to do with how international politics works in a nuclear world, and in particular with how seriously the risk of war in such a world is to be taken, and historical analysis can certainly shed some light on those issues. This implies that the conclusions you reach in studying particular historical episodes can have a certain bearing on how you think about key policy issues today, most notably nuclear proliferation.

History, in fact, can serve as a kind of workshop within which basic views about core policy issues get hammered out. When you are trying to deal with those issues, it helps if you can think about them in concrete historical contexts. It is much easier to wrap your mind around a specific problem than to think about the general issue on a purely abstract level. And indeed when you are doing history, and you are trying to figure out why events took the course they did, you have to put yourself in the shoes of the people you are studying and ask yourself what choices were available to them. When you do that, it is hard not to ask yourself what you would have done if you had to make those choices. As I say in chapter 2 here, there is not much of a jump from thinking about what *could* have been done to what *should* have been done, and that sort of thinking almost automatically produces various

general conclusions about how policy ought to be conducted. And conversely, thinking about policy issues helps you see which historical episodes you might want to study and which questions you might want to put at the heart of that historical analysis. Thinking about policy issues, in other words, can help bring historical work into focus—it can help you draw meaning from the work you do.

What all this means is that the three parts of the field—history, policy, and theory—are deeply interconnected, more tightly bound up with each other than I would have been prepared to admit when I was first starting out in this business in the 1960s. If your goal is a peaceful world, you have to concern yourself with fundamental conceptual problems, and above all with the problem of what makes for a stable international order. If your goal is to avoid war, you have to concern yourself with the basic theoretical issue of what makes for war. And there is no way to get at that kind of issue without looking at specific wars, albeit with a mind that has been prepared by grappling with the core conceptual problems that define this field of inquiry.

Years ago I thought that history had to be understood exclusively on its own terms—that the goal of historical research was simply to see the past, to use Ranke's famous phrase, as it really was. At that time, international relations theory was of little interest to me. I could not see how it gave you any insight that you could not get directly from doing ordinary historical work. I thought that an interest in policy issues, even when those concerns were just in the back of your mind and did not show up directly in the historical work you produced, was vaguely improper—that it was somehow illegitimate to make history speak to those issues, that a concern with policy tainted the scholarly enterprise, and that a real scholar should not pay attention to questions of that sort. Today my views on those fundamental questions of method, of how work on international politics ought to be done, are very different. I think that history, policy, and theory can be made to relate to each other in an intellectually respectable way, and indeed the basic rationale for publishing this collection of articles as a book is to show how I think this can be done.

Contents

PART I

Theory

CHAPTER ONE

The Question of Realism: An Historian's View

DIFFERENT COUNTRIES WANT different things; sometimes those desires conflict; how then do those conflicts get worked out? The basic insight that lies at the heart of the realist approach to international politics is that the way those conflicts run their course is heavily conditioned by power realities. In a world where war cannot be ruled out if conflicts are not settled peacefully, rational states are bound to be concerned with the structure of power in the sense not just of the distribution of military capabilities both actual and potential, but also of the whole web of relationships that would affect what would happen if war actually broke out. But rational states not only *adjust* their policies to such power realities. If the structure of power is of such fundamental importance, it stands to reason that states might well try to *alter* it to their advantage. That striving for power political advantage in turn might well come to dominate the system. The fact that states live in an anarchic system—that is, a system not governed by supranational authority—can therefore have a profound impact on state behavior, and some of the most central problems of international relations theory thus have to do with the importance of such "systemic" or "structural" effects in international political life.

It is commonly assumed that this concern for power, and especially this striving for power political advantage, puts states at odds with each other—that the struggle for power is a major source of conflict in and of itself. Such arguments are quite familiar. Opponents of realism have always assumed that power politics leads to conflict. Woodrow Wilson's whole approach to international politics was rooted in assumptions of that sort, and even today such attitudes are by no means dead. One leading contemporary theorist, Alexander Wendt, thus takes it for granted that a world in which states behave in accordance with the dictates of Realpolitik is a violence-prone, kill-or-be-killed, Hobbesian world.[1] It is perhaps more surprising to find realists themselves ar-

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[1] Alexander Wendt, *Social Theory of International Politics* (Cambridge, England, and New York: Cambridge University Press, 1999), pp. 262–66. Hobbes's original argument,

guing along these lines. The prevailing assumption among realists as a whole is that "mutual fear drives the great powers apart," that "international anarchy fosters competition and conflict," and that the "anarchic nature of international politics" encourages "cut-throat behavior among states."[2]

The argument is developed in its purest form by "offensive realists" like John Mearsheimer. "The structure of the international system," Mearsheimer writes, "forces states which seek only to be secure nonetheless to act aggressively toward each other." "Great powers," he says, "that have no reason to fight each other—that are merely concerned with their own survival—nevertheless have little choice but to pursue power and to seek to dominate the other states in the system." They have little choice because they fear other states and they know that they "have to seek more power if they want to maximize their odds of survival." But if states want to "maximize relative power," Mearsheimer argues, they have to "think offensively toward other states, even though their ultimate motive is simply to survive."[3]

The basic argument, however, is by no means limited to people like Mearsheimer. Even the "defensive realists," those scholars of a realist bent who take a relatively moderate position on this whole set of issues, fundamentally agree that a dynamic of this sort plays a central role in international politics. To be sure, their analyses are more guarded, more hedged, more inclined to emphasize the importance of second-order or unit-level considerations—the offense/defense balance, most notably—which in their view determine how strong in practice that dy-

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an argument that dealt specifically with international politics, was laid out in the *Leviathan*, part 1, chap. 13.

[2] Stephen Van Evera, "The Hard Realities of International Politics," *Boston Review*, 17 (November–December 1992), p. 19; Joseph Grieco, "Anarchy and the Limits of Cooperation: A Realist Critique of the Newest Liberal Institutionalism," *International Organization* 42, no. 3 (Summer 1988): 485; John Mearsheimer, review of Roger Spegele, *Political Realism in International Theory*, in the *International History Review* 20, no. 3 (September 1998): 776. Note also John Mearsheimer, "The False Promise of International Institutions," *International Security* 19, no. 3 (Winter 1994/95): 9; John Mearsheimer, *The Tragedy of Great Power Politics* (New York: Norton, 2001), chap. 2; and Kenneth Waltz, "The Origins of War in Neorealist Theory," in Robert Rotberg and Theodore Rabb, eds., *The Origin and Prevention of Major Wars* (Cambridge, England, and New York: Cambridge University Press, 1989), p. 43. See also the sources cited in n. 7 of this chapter. I say "prevailing assumption" because there are exceptions. See, for example, Charles Glaser, "Realists as Optimists: Cooperation as Self-Help," *International Security* 19, no. 3 (Winter 1994/95); Randall Schweller, "Neorealism's Status-Quo Bias: What Security Dilemma?" *Security Studies*, 5, no. 3 (Spring 1996), reprinted in Benjamin Frankel, ed., *Realism: Restatements and Renewal* (London: Cass, 1996); and Andrew Kydd, "Sheep in Sheep's Clothing: Why Security Seekers Do Not Fight Each Other," *Security Studies* 7, no. 1 (Autumn 1997).

[3] Mearsheimer, *Tragedy of Great Power Politics*, pp. 3, 21, 34.

namic is.[4] And they sometimes write in a way that suggests that security competition need not be a major source of international instability—that states will normally be satisfied with an "appropriate" amount of security, and will show little interest in reaching for more.[5] But the comparatively mild way in which they frame their arguments should not obscure the fact that, whatever qualifications they make, even leading defensive realists believe that in an anarchic system the major powers are pushed into conflict with each other—that anarchy is more than just a permissive cause of war.

Kenneth Waltz, for example, clearly believes that anarchy breeds conflict. Waltz, the most important theorist in the defensive realist camp, developed his argument most explicitly in an important 1988 article called "The Origins of War in Neorealist Theory." "Competition and conflict among states," Waltz wrote, "stem directly from the twin facts of life under conditions of anarchy: States in an anarchic order must provide for their own security, and threats or seeming threats to their security abound. Preoccupation with identifying dangers and counteracting them become a way of life." The measures states take to deal with these problems and make themselves more secure necessarily threaten other powers, who react in kind. "Some states," he says, "may hunger for power for power's sake." But "neorealist theory"—and that means Waltz's own theory—"shows that it is not necessary to assume an innate lust for power in order to account for the sometimes fierce competition that marks the international arena. In an anarchic domain, a state of war exists if all parties lust for power. But so too will a state of war exist if all states seek only to ensure their own safety." This logic does not, of course, explain the origins of particular wars, but it does, he says, "explain war's dismal recurrence through the millennia." The "recurrence of war" is to be understood in structural terms: "The origins of hot wars lie in cold wars, and the origins of cold wars are found in the anarchic ordering of the international arena."[6] Other defensive realists share that basic view. Indeed, as one leading scholar points out,

[4] Indeed, the defensive realists have been criticized for placing increasing emphasis on such nonsystemic factors. See especially Jeffrey Legro and Andrew Moravcsik, "Is Anybody Still a Realist?" *International Security* 24, no. 2 (Fall 1999).

[5] Waltz, "Origins of War in Neorealist Theory," p. 40. In context, however, the assumption here was still that rational states would seek to maximize relative power. "Excessive strength" was to be avoided because it might lead "other states to increase their arms and pool their efforts against the dominant state"—that is, because it might actually weaken a state's position in the system. Other defensive realists, however, take a clearer position and say explicitly that states "satisfice"—that they are not necessarily power maximizers but seek only the level of power sufficient for their purposes. See Barry Posen, "The Best Defense," *The National Interest*, no. 67 (Spring 2002): 119.

[6] Waltz, "Origins of War in Neorealist Theory," pp. 43–44.

in the international relations literature more generally nowadays, the anarchic structure of international politics is "routinely cited as a root cause of or explanation for the recurrence of war."[7]

Some traditional realists, however—not every major writer, but people like George Kennan, for example—took a very different view. They took it for granted that stability depended on the ability of states to pursue a policy framed in "realistic" power political terms. Over and over again, they stressed the point that to ignore the importance of power—to allow emotion and ideology and "impractical idealism" to dictate policy—was to court disaster.[8] Implicit in that whole line of argument was the assumption that "realist" foreign policies—that is, policies attuned to power realities—were not the problem. But today most realists seem to assume that they *are* the problem, and that a system of states acting rationally in power political terms—a system of states pursuing "realist" policies, the sorts of policies the system tends to encourage—is a violent, brutal, war-prone system.

For me, this issue was particularly salient because, like those traditional realists, I had come to believe that "power politics" was not the problem—that is, I had come to believe that serious trouble developed only when states *failed* to act in a way that made sense in power political terms. My basic thinking in this area had taken shape as a simple by-product of ordinary historical work; I had never tried to think these issues through on a more theoretical level; and I was puzzled when it

[7] James Fearon, "Rationalist Explanations for War," *International Organization* 49, no. 3 (Summer 1995): 384. It is in fact taken for granted in the scholarly literature that this view is held by neorealists of all stripes. Dale Copeland, for example, refers in passing to the "core neorealist premise that anarchy forces states into recurrent security competitions"; the assumption here is that this view is by no means limited to the offensive realists. Stephen Walt says that "the central conclusion of all realist theories—what might be termed the 'realist *problematique*'—is that *the existence of several states in anarchy renders the security of each one problematic and encourages them to compete with each other for power or security.*" Andrew Kydd says that "structural realists"—he has both offensive and defensive realists in mind—"argue that international anarchy renders states insecure, and that the search for security is the main task of states, and the main cause of conflict." And Robert Kaufman notes that "realists of all persuasions agree that the quest for power and the rivalries it engenders offer the most basic explanation for the origins of war." Dale Copeland, "The Constructivist Challenge to Structural Realism," *International Security* 25, no. 2 (Fall 2000): 188; Stephen Walt, "The Enduring Relevance of the Realist Tradition," in *Political Science: The State of the Discipline,* Ira Katznelson and Helen Milner, eds. (New York: Norton, 2002), p. 200 (emphasis in original text); Kydd, "Sheep in Sheep's Clothing," p. 114; and Robert Kaufman, "On the Uses and Abuses of History in International Relations Theory: Dale Copeland's *The Origins of Major War,*" *Security Studies* 10, no. 4 (Summer 2001): 180.

[8] See especially George Kennan, *American Diplomacy, 1900–1950* (Chicago: University of Chicago Press, 1951), chap. 4; the phrase quoted is on p. 69.