

DIPLOMATIC HISTORY AND INTERNATIONAL RELATIONS THEORY

(Chapter Two of Marc Trachtenberg, *The Craft of International History*)

An historical interpretation has to have a conceptual core. The facts never really just “speak for themselves.” The historian thus has to make them “speak” by drawing on a kind of theory—by drawing, that is, on a certain sense for how things work. But what does this mean in practice? What role does theory in that broad sense play in actual historical work? And how does a particular conceptual framework take shape in the mind of the historian in the first place? How *should* historians go about developing the sort of theoretical framework they need to make the past intelligible? Should they try to grapple with basic conceptual issues in a relatively direct way, by studying international relations theory and coming to terms with the arguments they find in that literature? Or are less direct, less formal, methods good enough for their purposes?

As for the theorists, what, if anything, can they hope to get by studying history? How can historical analysis be brought to bear on the study of theoretical issues? What in general can the theorists get from the historical literature? If historical interpretation has a conceptual core, doesn't that imply that that literature should not be viewed just as a great storehouse of factual material that can be drawn on for the purposes of theory-testing? Doesn't that imply that the theorists might be able to get a lot more from it if they approach it the right way?

The Historian and International Relations Theory

This chapter will be concerned with questions of this sort—questions about the relationship between history and theory—and I want to begin by looking at this set of issues from the historian's point of view. How, first of all, does the historian actually use theory? In very broad terms, the answer is simple: theory is above all an instrument of analysis, and, depending on what that analysis reveals, it can also serve as the basis for interpretation. But that point is very general, so let me explain what I mean by

giving a specific example. It relates to a passage in an article written over seventy-five years ago by the French historian Elie Halévy, perhaps the finest historian of his generation.

Halévy in that article—it was actually one of the Rhodes lectures he gave at Oxford in 1929—summed up the origins of the First World War in a single but quite remarkable paragraph. By 1914, he wrote, Austria’s leaders had come to believe that the problem of Slav nationalism could be dealt with only if Serbia were crushed militarily. “But everyone knew, who chose to know, that, whenever Austria declared war upon Serbia, Pan-Slavist sentiment would become too strong for any Russian government to resist its pressure,” and “everyone knew, who chose to know, that whenever Russia gave so much as a sign of declaring war upon Austria, Pan-German feelings would compel the German government to enter the lists in its turn.” “It was likewise common knowledge,” he went on to point out, “that Germany, whenever she declared war upon Russia, was resolved not to tolerate the existence in the west of an army that was after all the second best army in Europe; that she would first march upon Paris and annihilate France as a military power, before rushing back to the east, and settling matters with Russia.” It was also clear that in order to implement that plan, the German army felt it would have to march through Belgium. But “everybody understood that if ever the Belgian coast and the northern coast of France were to fall under the domination of Germany, Great Britain, feeling her prestige and her security in danger, would enter the war on the side of Belgium and France.” What all this meant was that by 1914 war had become virtually inevitable: “everyone knew, who wished to know, not only that a European war was imminent, but what the general shape of the war would be.”¹

Halévy was a truly great historian, and it is amazing how much he was able to pack into that one paragraph. A mere decade after the fighting had ended, he was able to analyze the coming of the war with Olympian detachment. He had a sense of tragedy. Events unfolded in accordance with a certain inexorable logic, and it was the historian’s job not to blame one side or the other for the war, but simply to show what that logic was. But as impressive as this was, you still have to wonder about some of the points he made. Russia was bound to come to the aid of Serbia *no matter what*, even if such a policy

meant war with both Germany and Austria? Wouldn't Russia's decision depend on whether she had a good chance of winning such a war, and wouldn't that depend in large measure on whether she could count on the active military support of Britain and France? And wouldn't the western powers, for their part, have a certain interest in holding Russia back? Wouldn't they want to avoid a war if they could, because of the risks they would run and the price they might have to pay? And part of that price might be political: even total victory in such a war might not be an unalloyed blessing. Would it really be to their interest to destroy the German counterweight to Russian power in Europe? Wouldn't some kind of balance of power in Europe be much better than war? Perhaps those sorts of considerations came into play, and if so maybe things might have developed in all kinds of different ways. And that point makes you wonder. Wasn't there much more of a story here than Halévy had made out?

For me, having worked in this field for over forty years now, such questions come to mind quickly. Implicit in those questions is a certain view of international politics. I read the Halévy passage and think to myself: *this just can't be*. I find it hard to believe that the Russians would have gone to war for the sake of Serbia no matter what. My strong suspicion, without looking at a single document, is that their policy on this issue *had to* depend very much on their sense for what France and Britain would do. My assumption, in other words, is that power factors of that sort were *bound* to be of fundamental political importance—that European leaders *could not* simply ignore such considerations and allow their policies to be shaped essentially by Pan-Slav or Pan-German sentiment.

Those italicized terms are a tip-off. They show that an element of *necessity* has been brought into play, and thus (as we saw in the previous chapter) that a causal theory has been brought to bear on the problem. Of course when I say these things I am using my terms rather loosely. There is no physical or logical impossibility here. It is not absolutely out of the question that Halévy was essentially right. So when I say to myself that power factors *had to be* more important than Halévy thought, all I really mean is that I find it very hard to believe that they did not come into play in a major way. But even bearing

these caveats in mind, it is clear that in reacting to Halévy that way, I am drawing on a kind of theory—on a rough sense for “what had to be,” rooted in a general sense for how international politics works.

But note the role that that theory, if you can call it that, actually plays. It does not provide any ready-made answers. Instead, it serves to generate a series of specific questions you can only answer by doing empirical research. What, for example, did the Russians actually think France and Britain would do if they went to war over Serbia, and were those calculations in their minds when they decided on a course of action in July 1914? The theory, in other words (if it is used correctly), is not a substitute for empirical analysis. It is an *engine* of analysis. It helps you see which specific questions to focus on. It helps you see how big issues (like the origins of the First World War) turn on relatively narrow problems (like what Russia calculated about Britain and France, and how that affected her behavior in the crisis). It thus helps you develop a sense for the “architecture” of the historical problem you are concerned with, and helps you see how you can go about dealing with it. It thus plays a crucial role in the development of an effective research strategy.

To see the point, just compare this approach with its alternative—an approach to research that is not rooted in any particular conceptual framework. If you adopt that sort of method, how do you go about doing your work? Do you just plunge into the sources and begin looking around in a totally mindless way, not having the slightest idea of what you are looking for, hoping that some interpretation will more or less automatically take shape when you have absorbed enough information? After all, in history as in science, it does not make sense to simply gather up a mass of facts “like pebbles on the beach.” In history as in science, a conceptual framework does not emerge in a purely mechanical way from a simple piecing together of empirical observations. But what that means is that a lot of thought has to go into the research effort—that it has to be question-driven. You therefore need to develop some sense for what the questions are—that is, you need something that can help generate those questions. And it is in this area that some theory—some sense, that is, for how international politics works—is really indispensable.

But there are dangers here. Theory can of course be misused. If you rely on a certain theory, you run the risk of seeing only what that theory says is important, or of trying to force the evidence into some preconceived theoretical structure. You might fall in love with a certain way of looking at things and interpret the past accordingly. But problems of this sort are not unmanageable. The important thing here is to realize that theory, in itself, does not provide answers, and that its main function is to bring questions into focus.

How does this process work in practice? As you deal with a particular historical problem, you are constantly trying to see how things fit together. You never want to interpret history as just a bunch of events strung together over time. Your goal instead is to understand the *logic* that underlies the course of events. And it's in that context that theoretical notions come into play.

Suppose, for example, you want to understand the origins of the First World War. You know that Russian policy in the Balkans is an important part of the story. Russia, of course, went to war in 1914 to protect Serbia. But what exactly *was* Russian policy in that area? How did it take shape, and why? It turns out, when you study that issue, that Russia did not pursue a purely defensive, status quo-oriented policy in that area in the years before World War I. The Russians, for example, helped set up the Balkan League in 1912. But as French prime minister Poincaré pointed out at the time, the treaty establishing the Balkan League “contained the seeds not only of a war against Turkey, but of a war against Austria as well.”² Given that Germany was Austria's ally, the Russians were obviously playing with fire. How then is that Russian policy to be understood?

In dealing with that question, you need to draw on certain assumptions of a theoretical nature. Russia was too weak to take on Germany by herself. Such a course of action would have been suicidal, and you assume that power realities *had to* be taken into account. But Russia was not alone: she could count on the support of France, and perhaps of Britain as well. The attitude of the western powers was crucial, but why would they be willing to go to war for the sake of Russia's Balkan policy? The French, it turns out, had been far more cautious in that area during the early days of the Franco-Russian alliance in

the 1890s. Why the shift? Didn't the answer have to do with Germany, or more precisely with the deterioration of Franco-German (and Anglo-German) relations in the decade before World War I? It stands to reason—and this is another essentially theoretical assumption—that as relations between Germany and the western powers deteriorated, Britain and France would become more dependent on Russia. They would have to worry about what Russia would do if war broke out in the west, and even in peacetime they would have to worry about the possibility of Russia mending fences with Germany if they did not give her more or less unconditional support. For similar reasons, Germany also had a certain interest in trying to wean Russia away from the western powers. And all this, the theory would suggest, would tend to put Russia in the driver's seat. It would tend to increase her freedom of action and make it possible for her to pursue a forward policy in the Balkans.

So using your theory, you generate a series of hypotheses—about French policy, German policy, Russian policy and British policy. Those hypotheses tell you what to look for when you start studying the sources. Did the French really feel that they had to support Russia no matter what? If so, was this a result of the way their relations with Germany had developed? Did the Russians feel that both sides were courting them, and did this have any effect on what they thought they could get away with in the Balkans? The theory, once again, does not provide you with the answers, but it does give you some sense for what the questions are—that is, for which questions should lie at the heart of the analysis.

But suppose you are able to answer those questions. Suppose, in fact, that the hypotheses that you had started out with ring true in terms of the evidence. You would then have an interpretation of what was going on. You would have been able to pull together a whole series of different things—the events leading to the deterioration of relations between Germany and the western powers, and increased Russian assertiveness in the Balkans. And that interpretation would draw on a theory—that is, on the very theory that was used to generate those hypotheses in the first place.

In other words, in developing that interpretation—in explaining why things took the course they did before 1914—you would be drawing on certain principles of a theoretical nature. The basic principle

in this case is this: when relations between powers, or blocs of powers, deteriorate, the position of third powers necessarily improves, and in each case those shifting power relations have an important effect on policy. This is not just an empirical regularity à la Hempel which you just happen to observe. With a little reflection, you can see why things more or less *have* to work that way.

And you can use this basic theoretical principle in historical work in all kinds of different contexts. Suppose, for example, you were interested in great power politics in the late 1960s and early 1970s. You know, of course, that Sino-Soviet relations deteriorated dramatically just prior to that period. And using that theory, you can hypothesize that the course of Sino-Soviet relations had a good deal to do with the coming of détente in Europe and with the improvement in America's relations with both Russia and China by around 1970. Without looking at a single document, but just by drawing on very general knowledge and on assumptions of a theoretical nature, you can generate an hypothesis which, depending on what the sources show, you might be able to develop into an historical interpretation.

But your basic understanding of how things work is by no means set in concrete. It is not just “there,” fixed for all time, just waiting for you to draw on it. It takes shape—it evolves—as you grapple with fundamental conceptual issues in specific historical contexts. It develops, in particular, as you react to what other people have to say, and especially as you react to historical arguments with a certain theoretical resonance.

How, for example, is the war of 1939 to be understood? For many years it was taken for granted that this question had a very simple answer: Hitler was to blame for that conflict, and not much more needed to be said on the subject. But in 1961 A.J.P. Taylor mounted an attack on that piece of conventional wisdom in his famous book *The Origins of the Second World War*. According to Taylor, the problem after 1919 “was not German aggressiveness or militarism, or the wickedness of her rulers.”³ The real problem in his view was that Germany was basically so much stronger than any other power in continental Europe. Fascist war-lust was not a crucial factor; indeed, it *could not be* the crucial factor: “Even the Fascist dictators would not have gone to war unless they had seen a chance of winning.”⁴

Power realities therefore *had to be* of fundamental importance: “the essential problem” in interwar Europe, Taylor wrote (in what was probably the most important sentence in the book), “was political, not moral”—that is, it had to do with power realities and not with aggressive intent.⁵

Taylor obviously took the argument too far (and I will show why in some detail in the next chapter), but as you grapple with that argument it becomes clear that his basic claim that it did not make sense to think of Hitler as pushing ahead without regard to the political and military conjuncture *had to be* correct. The structure of power as a whole, both what it was at any particular point and the way it was changing over time, *was bound* to come into play. To understand the origins of the war, the story of international politics in the 1930s therefore had to be reconstructed, and this was a story whose logic had a good deal to do with power realities. What gives this point a certain bite is the fact that it is at odds with what was at least at one time the conventional wisdom—at odds with a view of the origins of the war which indeed focused heavily on moral considerations and did not adequately take power factors into account.

That last point is in fact very important for our purposes. You develop your understanding of how things work by seeing things that you had not seen before. You are convinced by certain *ideas*—by certain lines of argument—about the way things work. But you also develop your thinking by reaching certain conclusions of a purely factual nature. Thomas Kuhn at one point talked about why philosophers of science of his generation had become interested in the history of science: “we were already dissatisfied with the prevailing tradition and were seeking behavioral *clues* with which to reform it.” The history could serve as a kind of springboard for grappling with the philosophical issues. Indeed, it eventually became clear to Kuhn that “many of the most central conclusions we drew from the historical record [could] be derived instead from first principles.”⁶ Historical conclusions are often “clues” in that sense. They are the clues you use to work your way back to a more basic understanding of international politics. They force you, as Hanson would put it, to reach for the fundamental principles from which those findings would follow “as a matter of course.”

And those findings can have a quite extraordinary impact on your thinking when they are dramatic and unexpected, and especially when they differ sharply from what you yourself had believed. When this happens—when you reach the conclusion that you were wrong on some important issue—it's like finding gold in your hands. It forces you to think about the source of the error and to rethink your basic assumptions; it thus enables you to deepen your understanding of how international politics works.

So your basic thinking evolves as you do historical work. You react to the arguments historians make. What is to be made of the fundamental assumptions that lie at the heart of particular interpretations? You try to think the issues through for yourself. And as you do your own empirical work, you are in effect testing out various general ideas. What rings true in terms of the evidence? Which ideas do not seem to count for much, in terms of their ability to explain what was going on in the real world? Sometimes your assumptions turn out to be wrong. Why exactly were you mistaken? Was there something important you had missed? As you go through this process, you automatically develop a certain sense for how things work.

So in that way a conceptual framework takes shape in your mind. But should the historian also try to approach the issue in a more direct way? Political scientists and other theorists have had a lot to say about the big issues historians need to be concerned with. Does it make sense for historians to study what they have written and to try to come to terms with their arguments, or is the homegrown theory of the sort I have been alluding to so far all the historian really needs?

Many historians have a low regard for the sort of work the theorists do, just as many theorists tend to look down on historians as mere fact-mongers.⁷ I don't think that those attitudes are warranted in either case, but for now let me just say that diplomatic historians have a lot to gain by taking international relations theory seriously. There are a number of reasons why this is so. First, and most obviously, there are certain basic issues that the historian needs to be able to sort out, and theoretical writings can provide important guidance. Suppose, for example, that you are studying international politics in the nuclear age. You realize you need to learn something about nuclear weapons and about the impact they have on

international political life. Do they simply “cancel each other out”? Are they a force for peace or a source of instability? In coming to terms with such problems, the works of theorists like Thomas Schelling and Bernard Brodie are of absolutely fundamental importance.

So certain theoretical works can help the historian deal with particular issues. But that’s not the only reason the historian should study the international relations literature. I talked a few paragraphs back about “dramatic” findings, and in fact philosophers of science often emphasize the importance of findings of this sort.⁸ But what makes a finding “dramatic” is not just that it differs, even radically, from what people had previously believed. If the issue were trivial, findings of that sort wouldn’t count for much. The finding has to be *important*, and what makes a finding important is the way it bears on fundamental beliefs—in this case, beliefs about how international politics works.

The importance of a particular finding will therefore register in your mind with particular force when you’ve grappled directly with the fundamental conceptual issues that lie at the heart of this area of inquiry. Only then will you really see why a certain finding is surprising and therefore important. Only then will your antennae be turned on; only then will your radar screen be activated; only then will you be able to hear the alarm bells sound and understand why certain findings matter. If surprise is important, then theory has to be important. The two go hand in hand. As Robert Jervis says: “Without a theory, you can’t be surprised by anything—i.e., events are surprising because they do not fit our expectations, and these can only come from implicit or explicit theories. People sometimes think that not being surprised is evidence for a great deal of knowledge; in fact, it is the reverse. People who know nothing cannot be surprised by anything.”⁹

Let me be clear about what I’m arguing here. When I say that historians should grapple with basic theoretical issues, I’m not implying that they should simply buy into the worldview of the theorists. Most of them would not be able to do that in any case, for the simple reason that the theorists just do not look at the world the same way the historians do.¹⁰ But it’s for that very reason that you can profit enormously, if you’re an historian, by going into the intellectual world of the theorist and trying to come to terms with

the ideas found there. Even if you ultimately reach the conclusion that the political scientists are basically wrong on some issue, the process that led to that conclusion can be quite rewarding. And when it turns out that you yourself had been wrong, as it sometimes does, the payoff can be quite extraordinary.

History and the Theorist

Now let's look at the issue from the other side. Why exactly should the theorist study history? It's not hard to come up with some fairly obvious answers. History can provide the theorist with examples that can serve to illustrate particular theoretical points. Those examples will help make the theorist's meaning clear, and might also provide a degree of empirical support for the particular point the theorist is making. Empirical examples, whether cited by the theorist or not, can also help someone who is grappling with important conceptual issues see the power of a theoretical argument. Thus Kenneth Waltz in his *Theory of International Politics* made an important argument, very much in the spirit of his basic structural approach to international politics, about the role of competitive pressure in shaping political behavior.¹¹ Suppose you are familiar with this argument and then come across the case of a top Prussian official (Prince Hardenberg) arguing in 1807 that his country had been unable to stand up militarily to France because the Revolution had "brought the French people a wholly new vigor" and that Prussia, if it hoped to survive, could not simply cling to the old order. The force of the new revolutionary principles was such, according to Hardenberg, "that the state which refuses to acknowledge them will be condemned to submit or to perish."¹² That way of thinking, it turns out, had a lot to do with the very important reforms Prussia adopted at that time. You note these things, and you're struck by the way they relate to Waltz's argument. His theoretical point takes on a certain reality quality. The historical example suggests that Waltz had not just come up with an interesting intellectual construct, but rather that the argument he develops can actually help you understand how things work in the real world.

And studying history can help theorists see things they might not otherwise see. In international relations theory, there is an important body of thought that "emphasizes the dangers that arise when the

offense is strong relative to the defense.”¹³ You might find that basic idea quite plausible. But if you study the Anglo-Russian relationship in the 19th century, a defense-dominant relationship if there ever was one, you might be struck by how bellicose key British leaders at various points—for example, during the Near Eastern Crisis in 1877.¹⁴ In that case, a rather different idea might take shape in your mind. Could it be that the danger in this case had to do with the fact that this was a *defense*-dominant relationship—that it was precisely *because* the risks for both sides were limited that statesmen could approach war in a relatively cavalier, and indeed frivolous, way? Of course, even if that were true in this case, that in itself would not mean that the general argument about offense-dominance being destabilizing was necessarily wrong. But the example could nonetheless provide some food for thought. It might suggest that the theoretical issue was more complex (and perhaps even more interesting) than you had originally thought.

It’s of course perfectly legitimate for theorists to use history in such ways. Historical examples can illustrate theoretical points, and the study of historical cases can serve as a spur to theoretical analysis. But if, from the theorists’ point of view, that was essentially all that history was good for, it would scarcely make sense for them to go into historical issues in any great depth. Historical analysis, if that were the case, could scarcely play a fundamental role in theoretical work. So if historical work *is* important, it has to be because it gives the theorists something more than what I have been talking about so far. It has to give them something fundamental—something that relates to their core intellectual aspirations.

What then do the political scientists who study international relations really want to do? They would like, by and large, to do more than just say intelligent things about how international politics works. Their goal, by and large, is to move beyond the “essay tradition.” They would like their field to be a kind of science. Their aim is to develop not just an intellectually respectable body of thought but a body of *theory*.¹⁵ And their approach to history is often rooted in such aspirations. On the whole, they tend to be positivists at heart. They generally take it for granted that theories are tested by looking at the facts,

and they often approach historical work from that point of view. Their assumption is that history can serve up the facts that are needed to test theories.

But one of the key insights to be drawn from the philosophy of science literature is that the very notion of “theory testing” is far more problematic than you might think. The problem derives from the fact that theories are not *supposed* to give as accurate a picture of reality as possible. The goal instead is to cut to the core—to simplify, to focus on what is driving things, to bring out what was really important in what is being studied. Theories therefore have to provide a kind of model—that is, a somewhat stylized view of reality. And that model has to differ from the chunk of reality it is supposed to help you understand. “Completely eliminating *all* differences between the model and the original state of affairs,” as Hanson says, would destroy “the very thing the model was meant to achieve—namely, the provision of an ‘awareness of structure’ absent from the original confrontation with a complex of phenomena.”¹⁶

This is as true in the study of international politics as it is in science as a whole. Explanatory power, as Waltz points out—and part of his eminence has to do with the fact that his approach to theory is rooted in an exceptionally sophisticated understanding of these philosophy of science issues—“is gained by moving away from ‘reality,’ not by staying close to it,” so it is a mistake to think that the best model “is the one that reflects reality most accurately.”¹⁷ Is the theory of gravitation defective because it fails to explain “the wayward path of a falling leaf”? Is classical economic theory to be faulted because it is based on a theoretic construct, “the famous ‘economic man,’” which, as every sensible economist knows, “does not exist”?¹⁸ To say that theory should try to replicate reality, “to say that a ‘theory should be just as complicated as all our evidence suggests,’” Waltz notes, “amounts to a renunciation of science from Galileo onward.”¹⁹

What then are we to make of the simple idea that theories should be tested by looking at the empirical evidence? A test consists of a comparison between what the theory implies and what observations show. If a theory is supposed to offer only a stylized picture of reality, a gap between the two is to be expected. How then can a discrepancy, even in principle, be said to falsify the theory? Such

gaps, moreover, are generally not hard to deal with. As philosophers of science have noted for over a hundred years, ad hoc explanations can easily be developed to save theories from falsification.²⁰ The well-known philosopher of science Imre Lakatos tells a story (imaginary, but based on a number of real historical episodes) to illustrate the point. The path of a newly-discovered planet is calculated using Newton's laws, but the actual path of the planet is different. The Newtonian astronomer conjectures that the deviation is due to the existence nearby of another hitherto unknown planet, but new and more powerful telescopes fail to disclose its existence. The astronomer then "suggests that a cloud of cosmic dust" accounts for the fact that the telescopes were unable to detect it. A satellite is sent up to look for this "conjectural cloud," but the result is another failure leading to another ad hoc conjecture. The process, he says, can go on indefinitely. Success at any point would be treated as a great victory for the Newtonian theory, but failure can always be explained away. At no point does failure mean that the basic theory has been refuted.²¹

Lakatos's point is that testing in science is not nearly as straightforward a concept as one might suppose. People think that a sharp distinction can be drawn between the theoretician and the experimenter, that "the theoretician proposes" and that "the experimenter—in the name of nature—disposes." "Man proposes a system of hypotheses," as one writer put it. "Nature disposes of its truth or falsity. Man invents a scientific system, and then discovers whether or not it accords with observed fact."²² But, as Lakatos argues, things are just not that simple. Ad hoc explanations can always be put forward: "the prime target remains hopelessly elusive."²³ "Nature may shout *no*," but human ingenuity "may always be able to shout louder."²⁴ And it is for that reason, he says, that in science "falsifications are somehow irrelevant."²⁵ What really matters, according to Lakatos, are "dramatic" results, predicted by the theory, otherwise unexpected, and confirmed by observation. And he gives the example here of the 1919 experiment that showed that light rays from distant stars were deflected by the gravitational force of the sun, just as Einstein's theory of relativity had predicted—a stunning result that played a key role, he says, in winning scientists over to the Einstein theory.²⁶

This general argument is certainly too extreme, and testing plays a greater role in natural science than Lakatos was prepared to admit. The 1919 experiment, for example, though very important, was not taken as absolutely conclusive. The relativity theory had also predicted the displacement of certain spectral lines, and Einstein himself recognized that the experimental test of that prediction was of crucial importance. “If it were proved that this effect does not exist in nature,” he wrote, “then the whole theory would have to be abandoned.”²⁷ Similarly, the Darwin theory of the “survival of the fittest” is often said to be tautological (because fitness is defined in terms of survivability) and therefore untestable. Darwin himself, however, took pains to point out ways in which his theory could be tested empirically—ways in which the empirical evidence would cause the theory to “break down.”²⁸

But even though Lakatos took the argument too far, there certainly was something to what he was saying, and in fact his basic point applies with greater force to a field like international relations theory than it does to fields like physics or even biology. In international relations theory, hard-and-fast predictions are rarely made; such theories thus cannot be confirmed or falsified in a relatively simple, straightforward way, as the term “testing” implies. Even in natural science, theories are normally not defeated instantly in a “simple battle” with the facts.²⁹ In the international politics literature, where general claims are much less precise, the assessment process is even less cut-and-dry. It is really the spirit of a theory that is being assessed—whether it gives you some real insight into how the world works, whether it helps you see things you otherwise would have been unable to see, whether it can explain things that you otherwise might find hard to understand. And the key point here is that such judgments simply cannot be made in a mechanical way. Even in a field like physics, such judgments are governed “not by logical rules but by the mature sensibility of the trained scientist.”³⁰ So in a field like international relations, where there is even less reason to assume that such decisions can be made in an essentially mechanical way, serious judgments have to draw on the “mature sensibility” of the trained scholar.

This is the real reason why history is important for the theorist. History is not to be thought of as a great reservoir of facts that can be gathered up like “pebbles on the beach” and drawn on for the purpose

of theory-testing. It is important because by studying history the scholar can develop the kind of sensibility that makes intelligent judgment possible. Indeed, it is hard to see how a scholar *can* develop that kind of sensibility without studying history in a more or less serious way. Purely abstract analysis can only take you so far. It can sometimes take you quite far.³¹ But at some point theory has to connect up with reality. At some point, it has to help you understand something important about the real world. So the key thing is to do the sort of work that can draw theory and history together.

Doing that kind of work allows you to take your measure of particular theoretical approaches and thus to develop your own sense for the sort of general theoretical framework appropriate for the analysis of the questions you are concerned with. You might in some cases be surprised by the degree to which a particular theory helps you understand a particular historical episode. You might be struck by how well the history and the theory resonate with each other. You might, in those cases, say to yourself: “I wouldn’t have expected to find something like this, but lo and behold, there it is, a finding very much in line with the way that theoretical argument says the world works.” When this happens, you sense that the theorist might be on to something important, something you previously had not seen. On the other hand, when the theory does not help you understand much of anything, that also has to be taken into account when you are making up your mind about these issues—that is, when you are developing your own understanding of how international politics works. Both positive and negative results feed into the assessment, and in both cases the real world connection is crucial.

Or to put the point another way: theorists often come up with interesting ideas about how international politics works—indeed, with ideas that are at times at odds with each other. But the fact that an idea is interesting or clever does not mean that it necessarily tells you much about how things work in the real world. You thus have to develop some sense for how important these various dynamics are. You need to develop some sense for how they stack up against each other, and thus for what dominates the international political process. And it is only by studying history in some depth that you can make those kinds of judgments. This is particularly important because a major body of theory commonly makes a

kind of meta-claim: the theorists who hold those ideas are in effect asserting that the sort of thing they are emphasizing counts for a lot more than many people think. Indeed, a body of theory needs to make that sort of meta-claim if it is to be of real value: theories that simply sum up what everyone already knows are not worth much. So one of the things that normally characterizes a theory in this field is that it is not universally accepted. Realists, for example, in effect claim that power political factors are a good deal more important than many people are prepared to admit. But judgments about the relative importance of various sets of factors can only be made when you get a real sense for how things actually work, and you can develop that sense only by studying the historical record.

But how exactly do you go about doing this? The basic technique is to take some major theoretical claim, bring it down to earth by thinking about what it would mean in specific historical contexts, and then study those historical episodes with those basic conceptual issues in mind. Exercises of this sort—exercises that bring the conceptual and empirical sides of the broader intellectual effort together—are a way of getting a handle on a problem. Abstract argument, as I said before, has a certain cloud-like quality. Theoretical claims are hard to deal with on a very general level. But those general claims translate, or should translate, into expectations about what you are likely to find if you study a particular historical episode. You can then look at that episode with those expectations in mind. The problems are now more concrete. The questions, being narrower and more specific, are more answerable. And given the way you have set up the question, the answers you reach are bound to give you a certain insight into the more general issues those questions are connected to.

This is in fact a very standard way of approaching major theoretical problems, and I'll talk more in chapter 6 about how in practice this sort of work can actually be done (or at least begun). But for now let me just say that in principle it is generally not that hard to see what theoretical claims "translate into," in terms of specific historical interpretations. Indeed, theorists themselves often give historical examples as a way of backing up their arguments. If your goal is to assess those arguments, the historical cases they themselves cite are the first ones you would want to study. Waltz, for instance, says that one of the

reasons why bipolar international systems are more stable than multipolar ones is that under multipolarity, the “weaker or the more adventurous party” to an alliance can drag its partner into a war. He then cites Austro-German relations in the “prelude to World War I” as a case in point.³² If your goal is to assess that Waltz argument about multipolarity, one of the first things you would want to do is to see what relations between Austria and Germany during the July crisis were actually like. You would like to see whether Austria was able to drag a reluctant Germany into the war. Did the Austrians, for example, feel that they could do whatever they wanted, knowing that the Germans would not be able to abandon them no matter what happened, or did they feel that they had to clear things with the Germans before they did something that might get them into real trouble with Russia? Did the Germans feel that they would have to support Austria no matter what, or did they feel that Austria could not move ahead unless Germany first gave her the green light? These are all studiable issues, and answering them will throw some light not just on Waltz’s historical claim about the July crisis, but also on the general argument that that claim was meant to support.

The basic point here is that if you want to get a real handle on a major theoretical issue, you often need to go into key historical questions in some depth. You’ve been exposed to all sorts of historical arguments in the course of your education, but many of them have to be taken with a grain of salt. Waltz might have been taught that Austria dragged Germany into war in 1914; I also remember hearing something like that when I was in college. But historical arguments you pick up in that way are often very much open to question. For serious academic purposes, it scarcely makes sense to take them at face value. If you accept them uncritically, you’re in a sense just shooting yourself in the foot. You’d be building on an unnecessarily weak base.

Let me give what to my mind is a particularly striking example of a failure on the part of theorists to do the sort of historical work I am talking about. For me this case is of particular interest because it involves two of the theorists I most admire. In 1965, Thomas Schelling was writing his book *Arms and Influence*, and one of his key arguments in that book had to do with the role the military system could

play in bringing on a war. Both sides in a conflict, in Schelling's view, could be trapped by the sort of military system in place during a crisis. That system, he thought, could bring on a war that no one really wanted. This was a very important argument, with all kinds of major implications, and the coming of war in 1914 was the one great historical example Schelling gave to support it. The first seven pages of the chapter in which he made that argument were in fact devoted to a discussion of that case.³³

Schelling sent a draft of that chapter to his friend Bernard Brodie, another giant in the strategic studies field, and Brodie wrote back with detailed comments. He suggested that Schelling, in that section, use the famous story about how, on the eve of the First World War, the Kaiser, thinking that it might be possible to fight the war only in the east, tried to get General von Moltke, the chief of the general staff, to change the plan calling for an initial attack in the west, but was told that this was impossible. (Brodie himself had used that story to make a point about how a rigid military mindset could bring on a war that “no one wanted” in an article he had published a decade earlier.) Barbara Tuchman, Brodie told Schelling, had described the incident her recent bestseller, *The Guns of August*. Schelling replied that he had “thought of using that business about the Kaiser's being told the trains couldn't be turned around,” but he “had a nagging impression that Barbara Tuchman or somebody thought the story was possibly undocumented, and maybe a little too good to be true,” so he “let it go.” If, however, Brodie or some “other genuine scholar” could assure him “that the story was correct” or tell him “where to find a reference,” he said he would like to use it. But he was too busy to try to “trace down any documented version” himself.³⁴

This exchange I found quite revealing. I was struck, first of all, by what it showed about Brodie. The evidence (including the evidence that Tuchman had presented in her discussion of this particular episode, if you read it with any care) shows very clearly that it was Moltke and not the Kaiser who was overruled on the key issue of whether the attack on France had to proceed as planned without regard to changed political conditions. The Kaiser, having been led to believe that Britain might stay out of the war if it were fought only in the east, decided to call off the attack in the west. Moltke pleaded with him to change his mind, but as Tuchman herself points out, “despite all his pleading, the Kaiser refused to

budge.” “‘Crushed,’ Moltke says of himself,” Tuchman writes (quoting from Moltke’s own memoir); “he returned to the General Staff and ‘burst into bitter tears of abject despair.’” The orders calling for an attack in the west were cancelled, and it was only after new information came in from Britain showing that an east-only war would not be possible that the attack in the west was allowed to proceed as planned.³⁵

So Brodie had missed the real point of the story, but why? The problem might have had something to do with the way the Tuchman book was written. Tuchman had framed the issue in a way that probably led Brodie to think that her analysis supported his view of the incident. Here, for example, is the concluding paragraph from her short but very dramatic introduction to the part of the book dealing with the outbreak of war:

War pressed against every frontier. Suddenly dismayed, governments struggled and twisted to fend it off. It was no use. Agents at frontiers were reporting every cavalry patrol as a deployment to beat the mobilization gun. General staffs, goaded by their relentless timetables, were pounding the table for the signal to move lest their opponents gain an hour’s head start. Appalled upon the brink, the chiefs of state who would be ultimately responsible for their country’s fate attempted to back away but the pull of military schedules dragged them forward.³⁶

This is the kind of passage that makes an impression on people. Tuchman’s account of the confrontation between the Kaiser and Moltke might have been factually accurate, but in terms of what people took away from the book, what mattered was not the detailed account but the way it was packaged. “*The pull of military schedules dragged them forward*”—this, according to Tuchman, was the point of the story, and unless you are in the habit of reading historical texts critically, you tend to assume that this was what her detailed account actually showed. And that, of course, is a major reason for getting into the habit of reading such texts critically. But let’s not focus too narrowly on the reference to the Tuchman

book. The more serious point is that Brodie had never really studied the July Crisis the way it should have been studied, above all by an expert of his stature, and was content to repeat the standard clichés.

What makes this case particularly striking is that Brodie was unusual among strategic theorists of his generation in that he *did* pay a lot of attention to history, believed deeply in the importance of historical study, and criticized many of his fellow strategists for not knowing much about diplomatic or military history.³⁷ And yet here was Brodie himself, perhaps the most historically-minded of the leading American strategic theorists, making an historical argument that he would never have made if he had studied this very specific but fundamental issue—the role of the military system in bringing on the war in 1914—the way it ought to have been studied.

The case of Schelling was perhaps even more striking. Here he was, producing a book that was destined to be the single most important work in the field, but he could not be bothered to do the work needed to get to the bottom of the issue that Brodie had raised. He could not even go to the library or to a bookstore and look up the story in the Tuchman book. It was as though the historical evidence had purely ornamental value. If this story did not work, then there was no need to use it. Although he talked a lot about the July Crisis in that key chapter in *Arms and Influence*, an accurate understanding of what happened in July 1914 was not seen as crucial in its own right.

But Schelling was writing a work of theory, so why, you might ask, was it important for him to get to the bottom of these historical issues? After all, one *could* imagine the Kaiser giving way to Moltke. The standard account of this episode has a certain plausibility, and for the purposes of theory-building, isn't that all that really matters? But there is obviously something wrong with the notion that from the point of view of the theorist, fiction, as long as it is plausible, is as good as history.³⁸ To take one of the key issues people like Brodie and Schelling were concerned with: doesn't it matter, from the standpoint of the theorist, whether war was in reality often brought on by the workings of the military system in place at the time, or whether war is virtually never to be interpreted in such terms? Given the role the story about the confrontation between Moltke and the Kaiser plays in supporting the view that military factors could

play a key role in bringing on a war that nobody wanted, doesn't it matter whether that story is correct or not? Suppose you couldn't find any historical evidence at all to support that view, and all you had to go on were purely fictional accounts of how a war could begin virtually by accident. Wouldn't the absence of powerful historical evidence be an important "clue" in Kuhn's sense? Shouldn't the absence of evidence, in that case, play a certain role in shaping your thinking about what makes for war?

So doesn't all this mean that people like Brodie and Schelling could have done better, as theorists even, if they had gone into what for them were important historical questions in greater depth? I think not only that they could have done better, but also that it would not have been all that hard for them to have done so. And this, in fact, is one of the main points I want to make in this book. You might think that I am asking too much. You might think that I am in effect saying that theorists need to become historians. And you might feel that that expectation is not just unrealistic, but that it ignores the fact that international relations theory is a field with an intellectual personality of its own—that no one can do everything and that those who choose to do theory simply cannot be expected to become historians and do professional-quality historical work.

But to do the kind of work I am talking about, a theorist does not have to become an historian. A theorist can reach relatively solid conclusions on key historical issues in a reasonable amount of time, provided he or she uses the right method—and in the next two chapters, I want to show you what that method is.

¹ Elie Halévy, “The World Crisis of 1914-1918: An Interpretation,” first published in 1930, and republished in Elie Halévy, *The Era of Tyrannies: Essays on Socialism and War* (London: Allen Lane, 1967), pp. 161-190. The paragraph in question is on p. 179.

² Poincaré notes of meeting with Russian foreign minister Sazonov, August 1912, *Documents diplomatiques français (1871-1914)*, 3rd series, 3:34. For the key evidence on Russia’s Balkan policy at the time, see Barbara Jelavich, *Russia’s Balkan Entanglements, 1806-1914* (Cambridge: Cambridge University Press, 1991), pp. 246-247; Bernadotte Schmitt, *The Coming of the War, 1914*, 2 vols. (New York: Scribner’s, 1930), 1:135; and Luigi Albertini, *The Origins of the War of 1914*, 3 vols. (London: Oxford University Press, 1952-57), 1:375, 486.

³ A.J.P. Taylor, *The Origins of the Second World War* (New York: Atheneum, 1961), p. 24.

⁴ *Ibid.*, p. 103.

⁵ *Ibid.*, p. 24.

⁶ Kuhn, *Trouble with the Historical Philosophy of Science*, pp. 6, 10. Emphasis added.

⁷ For the view of some historians, see, for example, Paul Schroeder letter to the editor, *International Security* 20, no. 1 (Summer 1995): 195. The attitude of some theorists comes out in some remarks made in passing. Thus Martin Wight says: “Guicciardini was a historian; he described but did not analyse.” Martin Wight, “The Balance of Power and International Order,” in *The Bases of International Order*, ed. Alan James (London: Oxford University Press, 1973), pp. 88-89. Hans Morgenthau makes the same kind of very sharp distinction. “But where is the line to be drawn,” he asks, “between the similar, which is susceptible to theoretical understanding, and the unique, which is the proper province of history?” Hans Morgenthau, “The Purpose of Political Science,” in *A Design for Political Science: Scope, Objectives, and Methods*, ed. James Charlesworth (Philadelphia: American Academy of Political and Social Science, 1966), p. 64.

⁸ The point played a particularly important role in the writings of Imre Lakatos. See, for example, Lakatos, “Falsification and the Methodology of Scientific Research Programmes,” p. 116; Imre Lakatos, “History of Science and its Rational Reconstructions,” in *Method and Appraisal in the Physical Sciences: The Critical Background to Modern Science, 1800-1905*, ed. Colin Howson (Cambridge: Cambridge University Press, 1976), pp. 7, 11; Imre Lakatos, “Lectures on Scientific Method,” in Imre Lakatos and Paul Feyerabend, *For and Against Method: Including Lakatos’s Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence*, ed. Matteo Motterlini (Chicago: University of Chicago Press, 1999), p. 99, 100; and Lakatos quoted in Brendan Larvor, *Lakatos: An Introduction* (New York: Routledge, 1998), p. 55. But Lakatos was not the only philosopher of science to stress the importance of results of this sort. See, for example, Karl Popper, *Conjectures and Refutations* (New York: Basic Books, 1962), p. 36, and Kuhn, *Structure of Scientific Revolutions*, p. 155.

⁹ Jervis email to the author, February 1, 2005.

¹⁰ On this issue, see the very interesting exchange between Robert Jervis and Paul Schroeder in Colin Elman and Miriam Fendius Elman, eds., *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations* (Cambridge: MIT Press, 2001), pp.385-416.

¹¹ Waltz, *Theory of International Politics*, esp. pp. 76-77, 127-128.

¹² Hardenberg Riga Memorandum, quoted in Mack Walker, ed., *Metternich’s Europe* (New York: Walker, 1968), p. 8. See also Thomas Nipperdey, *Germany from Napoleon to Bismarck, 1800-1866* (Princeton: Princeton University Press, 1996), p. 20.

¹³ Stephen Van Evera, “The Cult of the Offensive and the Origins of the First World War,” *International Security* 9, no. 1 (Summer 1984): 63, and the sources cited there in Van Evera’s n. 25. See also James Morrow’s review article, “International Conflict: Assessing the Democratic Peace and Offense-Defense Theory,” in *Political Science: The State of the Discipline*, ed. Ira Katznelson and Helen Milner (New York: Norton, for the American Political Science Association, 2002), esp. pp. 183-191.

¹⁴ See especially R.W. Seton-Watson, *Disraeli, Gladstone, and the Eastern Question* (London: Macmillan, 1935), pp. 217-218.

¹⁵ See, for example, Kenneth Waltz, "Realist Thought and Neorealist Theory," *Journal of International Affairs* 44, no. 1 (Summer 1990).

¹⁶ Hanson, *Observation and Explanation*, p. 81, and more generally, pp. 79-83. Emphasis in original text.

¹⁷ Waltz, *Theory of International Politics*, p. 7.

¹⁸ *Ibid.*, pp. 89, 121.

¹⁹ Kenneth Waltz, "Evaluating Theories," *American Political Science Review* 91, no. 4 (December 1997): 914; the internal quotation is from a well-known book on method, Gary King, Robert Keohane and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994), p. 20. Waltz, of course, is not the only political scientist who makes this point. Note, for example, the line of criticism developed in Jonathan Bendor and Thomas H. Hammond, "Rethinking Allison's Models," *American Political Science Review* 86, no. 2 (June 1992): esp. 318.

²⁰ The argument, worked out originally at the end of the nineteenth century, that any theory "can be permanently saved from 'refutation' by some suitable adjustment in the background knowledge in which it is embedded," came to be called the "Duhem-Quine thesis." The thesis has, as Lakatos pointed out, both a strong and a weak form. But even the weak version "asserts the impossibility of a direct experimental hit on a narrowly specified theoretical target and the logical possibility of shaping science in indefinitely many ways." See Lakatos, "Methodology of Scientific Research Programmes," pp. 184-185.

²¹ Lakatos, "Methodology of Scientific Research Programmes," pp. 100-101; Lakatos, "Lectures on Scientific Method," pp. 69-70.

²² Lakatos, "Methodology of Scientific Research Programmes," pp. 96-97. Lakatos is quoting here from a book by Braithwaite.

²³ *Ibid.*, p. 102.

²⁴ Lakatos, "History of Science and its Rational Reconstructions," p. 10.

²⁵ Lakatos, "Lectures on Scientific Method," p. 95.

²⁶ *Ibid.*, pp. 99-100. On p. 99, Lakatos gave another example, having to do with the astonishingly accurate prediction of the reappearance of Halley's comet after a 72-year lapse. Newton's theory had served as the basis for that calculation, so the amazing accuracy of the prediction provided powerful support for the theory. Kuhn gives a number of similar examples, one of which was particularly striking. In France, he says, resistance to the wave theory of light "collapsed suddenly and relatively completely when Fresnel was able to demonstrate the existence of a white spot at the center of the shadow of a circular disk. That was an effect that not even he had anticipated but that Poisson, initially one of his opponents, had shown to be a necessary if absurd consequence of Fresnel's theory." Kuhn, *Structure of Scientific Revolutions*, p. 155.

²⁷ Einstein to Eddington, December 15, 1919, quoted in A. Vibert Douglas, *The Life of Arthur Stanley Eddington* (London: Thomas Nelson, 1956), p. 41.

²⁸ See Richard Alexander, *Darwinism and Human Affairs* (Seattle: University of Washington Press, 1979), pp. 7-8.

²⁹ Lakatos, "History of Science and its Rational Reconstructions," p. 31. For a good example of current thinking on the subject, see the section on "Problems of Falsifiability" in Alex Rosenberg, "Biology and Its Philosophy," in Yuri Balashov and Alex Rosenberg, eds., *Philosophy of Science: Contemporary Readings* (London: Routledge, 2002), pp. 28-31; originally published in Alexander Rosenberg, *The Structure of Biological Science* (Cambridge: Cambridge University Press, 1985), pp. 6-8.

³⁰ See chapter 1, p. xxx.

³¹ In science itself, it is amazing how far the greatest thinkers were able to go on sheer brainpower. Galileo's disproof of the Aristotelian theory that heavier bodies fell more quickly than lighter ones is one extraordinary case in point. See James Robert Brown, *The Laboratory of the Mind: Thought Experiments in the Natural Sciences* (London: Routledge, 1991), pp. 1-3. The origin of the relativity theory provides another important example. The famous Michelson-Morley experiment of 1887, often cited in this context as a "crucial experiment," in fact played a minor role in shaping Einstein's thinking. The real source of the theory was Einstein's intuitive sense, a sense he had even as a teenager, for the way things had to be. Leo Sartori, *Understanding Relativity: A Simplified Approach to Einstein's Theories* (Berkeley: University of California Press, 1996), pp. 51-54, and esp. p. 53; Gerald Holton, "Einstein, Michelson, and the Crucial Experiment," in his *Thematic Origins of Scientific Thought: Kepler to Einstein* (Cambridge: Harvard University Press, 1973). In international relations theory, the work of Thomas Schelling is probably the best example of a penetrating body of thought that was generated essentially by pure brainpower. See especially Thomas Schelling, *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960) and Thomas Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966).

³² Waltz, *Theory of International Politics*, p. 167. See also Waltz's "The Origins of War in Neorealist Theory," *Journal of Interdisciplinary History* 18, no. 4 (Spring 1988): 621.

³³ Schelling, *Arms and Influence*, pp. 221-227.

³⁴ Brodie to Schelling, February 8, 1965, and Schelling to Brodie, February 19, 1965, Bernard Brodie Papers, Box 2, University Research Library, University of California at Los Angeles; and Bernard Brodie, "Unlimited Weapons and Limited War," *The Reporter*, November 18, 1954, p. 21.

³⁵ Barbara Tuchman, *The Guns of August* (New York: Macmillan, 1962), p. 81. A detailed discussion of this episode could also be found in what was then by far the most thorough account of the immediate origins of the war available in English, Luigi Albertini's *The Origins of the War of 1914*, 3:171-181 and 3:380-386.

³⁶ Tuchman, *Guns of August*, p. 72.

³⁷ Bernard Brodie, *War and Politics* (New York: Macmillan, 1973), p. 475.

³⁸ Note what Schelling says in the preface to *Arms and Influence* (p. vii): "I have used some historical examples, but usually as illustration, not evidence. For browsing in search of ideas, Caesar's *Conquest of Gaul* is rich reading and Thucydides' *Peloponnesian War* the best there is, whatever their historical merits—even if read as pure fiction."