

1 *Introduction*

What is international relations (IR) theory? Is it an arcane academic enterprise or relevant to the real world? What do we mean by theory? What kinds of knowledge do theories seek or represent? How do they stipulate it should be found? How should we evaluate any resulting knowledge claims? What do answers to these questions tell us about the theory project in IR, and more generally in the social sciences? I address all of these questions in this book and set of companion videos. The videos can be accessed free of charge at nedlebow.com.

I cover epistemological ground with which you may be familiar, but perhaps not. I lay bare and query the assumptions of positivist and interpretivist approaches to knowledge in general and to IR in particular. I identify and explore the most important inner tensions of these epistemologies and question the assumptions they make about the world. I identify problems that need to be addressed, suggest some possible strategies or reformulations toward this end, and discuss how adherents of the two epistemologies might learn from one other. Above all, I am interested in the extent to which IR theories can tell us something useful about the world.

This book is about theory in IR, not about theories of IR. This distinction is an important one. It focuses on theory, what it is, what it purports to be, and how it organizes our understanding of the discipline and the world. It engages some IR theories and research programs, but only as illustrations or for the insights they offer into the questions I have posed. It differs significantly from most other books on IR theory. They are organized around paradigms, theories, or substantive areas of inquiry.¹ Some books that claim to be about theory are really methods texts that insist on shared standards and make an uncritical pitch for those rooted in neopositivism (defined in due course).² Methods are metaphorical tips of icebergs. They rest on a much larger structure that may as well be submerged as it attracts so little attention, as least in the neopositivist literature. I look beyond

methods and paradigms to “epistemology,” which derives from the Greek word *epistēmē*, meaning knowledge. I identify the different kinds of knowledge people seek, how they believe it can be discovered, when and why they believe they have found it, and what they think it is useful for. We need to articulate and interrogate the epistemological foundations of all relevant research traditions so we can proceed in an informed way to theories, methods, and empirical research.

We can evaluate theory internally and externally. Is the theory logically consistent, are its terms well defined, is its scope carefully specified, and is it subject to empirical evaluation? If it meets these criteria, we can go on to ask if the theory or propositions derived from it offer good explanations or predictions or, alternatively, say something else valuable about the world? External criteria for validation are highly controversial both within and across positivist and interpretivist epistemologies. Positivists tend to believe in objective criteria for establishing truth claims. Interpretivists deny this possibility. Many, perhaps most, acknowledge the importance of protocols for conducting and assessing research and the possibility of achieving an consensus about both kinds of protocols among the community of researchers. Exploring differences between and within these epistemologies offers insights into the nature of and impediments to disciplined inquiry.

Theories of all kinds rely on reason in the form of logic. Reason is generally treated as universal and unproblematic, especially by positivists. It is a meta-concept, as it is foundational to all other concepts. Following Max Weber, I contend that reason is historically and culturally specific in understanding and use. It is closely related to values because people use different kinds of reason depending on the ends they seek. Some theories assume a high degree of instrumental rationality – one kind of reason – on the part of actors whose behavior they study. We confront an empirical question here, not one to which an answer can merely be assumed. To construct good theories, we need to know more about the kinds of reason actors employ, in what circumstances, and the degree to which they use these formulations in a consistent manner. Some social scientists insist that theories are free to incorporate any assumptions they want, including that of instrumental rationality. I devote Chapter 12 to this controversy.

The other concept central to theory is cause. It is understood in different ways by different research programs. They also put different values on its utility. Some assume cause to be the cement of the universe

and the goal of inquiry. For others, it is a human artifact that we use with varying degrees of success to make sense of the world. Some research programs finesse cause or dispense with it altogether. I devote Chapters 13 and 14 to these different understandings of cause, their implications for knowledge, and the problems to which they give rise.

A century of IR theory might be regarded as an experiment from which we might learn something about the nature, appeal, and life cycle of research programs and the theories they have spawned. It also has the potential to teach us something about the relationship between theory and praxis. Theories come and go. Epistemologies also rise and fall in appeal, but over a longer time period. Both kinds of turnover reflect changing conceptions about what theory is or should aspire to be. These conceptions are closely connected to the ends we want theory to serve. It makes little sense to examine the rise and fall of paradigms and theories in isolation, as so much of the literature by IR scholars does with their fascination with so-called first, second, and third debates unless they are linked to more fundamental assumptions about knowledge and its purposes.³

Although we cannot agree about what theory is or should be, there is no escaping the fact that it has become more central to our field. It might be said to help constitute it because it is what separates us from foreign policy analysis and history. Foreign policy analysts and historians may use theories but IR creates them. There is nevertheless a certain irony in our reliance on theory to demarcate and legitimate IR because so many of our theories are borrowed from other fields and disciplines and so much IR is deeply involved with the analysis of foreign or national security policy. To the extent that IR theory is imbricated with theory in other fields, it offers an opportunity to address the problem of theory more generally.

Questions, Tensions, Controversies

International relations theory is often described as a twentieth-century phenomenon. The conventional date for its birth is the immediate aftermath of World War I when the first chair in the subject was created at the University of Wales in Aberystwyth. In reality, people had thought and written about relations between political units since at least the time of Thucydides (460–400 BCE). Kautilya, Niccoló

Machiavelli, Jean Bodin, Thomas Hobbes, John Locke, Immanuel Kant, Jean-Jacques Rousseau, and Max Weber all stand out in this regard.⁴ What changed in the twentieth century was the creation of a discipline devoted to the study of IR. In its effort to establish itself, it claimed these thinkers and others as de facto IR theorists and representatives of a long-standing tradition – but in truth, a largely imagined one. Beginning with realism and liberalism, every IR paradigm would create a lineage for itself and co-opt distinguished thinkers from the past as its founders. By doing so, advocates of these paradigms hoped to add legitimacy and luster to their projects.⁵

Another distinguishing feature of IR theory is its policy focus. This too is a function of its origins. David Davies endowed the first chair in Aberystwyth as a memorial to the students killed and wounded in World War I. He named it after Woodrow Wilson because he embraced the American president's vision of restoring and maintaining peace through the League of Nations.⁶ After World War II, IR scholars were equally committed to finding ways of preventing war and addressed what they thought were its immediate and underlying causes. This commitment is evident in the research and writings of, among others, Hans Morgenthau, John Herz, Ernest Haas, Ernst-Otto Czempiel, Karl Deutsch, Alexander George, Ole Holsti, Stanley Hoffman, Hedley Bull, and the English School. They produced landmark studies of war, war prevention, crisis management, nationalism, public opinion, and regional integration. Many of their research programs still find contemporary resonance.

More than other social sciences, IR is engaged with practice, in analytical and hands-on ways. Theorists aspire to develop insights into the causes of international conflict and cooperation in security, economics, immigration, and other substantive domains. Most hope their research will not only advance our knowledge but also inform the policy of leaders, states, and regional and international organizations. Ethics also enters the picture. Many IR scholars are committed to developing norms for foreign policy, drawn for the most part from theories of ethics. I, for one, have mustered empirical evidence to demonstrate that ethical policies are more likely to succeed than unethical ones, and vice versa.⁷

Students of IR are nevertheless of two minds about policy engagement. I think it fair to say that for most it is what makes our field relevant and attractive. However, some prominent theorists, including

Quincy Wright, Morton Kaplan, and Kenneth Waltz, questioned the coupling of theory and policy. They might be considered purists, committed to theory as an end in itself and to maintaining a firewall between international relations and foreign policy. For Waltz, international relations is a system-level phenomenon, and the goal of IR theory is to capture the character of different systems and their consequences for war and peace. By his own account, his theory has no practical utility.⁸

Even those scholars who want their research to inform foreign policy are generally keen to distinguish IR theory from foreign policy analysis, area studies, and journalism. PhD programs have tried hard to distance themselves from these enterprises, and many professors, especially in the United States, have been wary of them, if not downright hostile. At the same time, many IR theorists, including such luminaries as E. H. Carr, Hans Morgenthau, and Raymond Aron, had active lives as journalists or governmental advisors. Noted scholars like Harold Lasswell, Quincy Wright, John Herz, and Joseph Nye Jr. held government office. Not a few contemporary IR scholars are active bloggers with sizeable followings. The New York-based Council on Foreign Relations has for many decades offered fellowships to faculty to spend a year in Washington, DC, working in the government. I was the recipient of one of these fellowships in 1974–75.

A related controversy concerns the extent to which the profession of IR should engage in politics. Hans Morgenthau frequently maintained that the goal of IR scholarship was to “speak truth to power.” He was an early and outspoken opponent of the Vietnam War, debated National Security Advisor McGeorge Bundy on television, and spoke at antiwar rallies.⁹ He was punished by pro-war conservatives who lobbied hard to deny him the presidency of the American Political Science Association (APSA) and pass a resolution that the APSA was a professional association that should avoid taking any political position.¹⁰ The conservative hold on APSA led to the creation of a breakaway organization, and some years later to a reconciliation.¹¹ In the decades since, IR theorists have not been shy about speaking out on political issues. Many signed petitions against the 2003 invasion of Iraq or spoke out publicly against it. Some of their British colleagues did the same. IR scholars in many countries have tried in this century to have their voices heard on a range of political questions.

Morgenthau's plea for independence and courage has been taken to heart by many IR scholars. A few, as he feared, have been co-opted by governments or national security establishments and have become partisans of national or ideological causes. Still others act this way because they are embedded in their nation's political culture and socialized into accepting a particular view of the world and their nation's role in it. Simon Reich and I contend that this is true of many American realists and liberals who maintain that the United States is a hegemonic power and deserves special privileges because its leadership is essential to preserve global security and economic intercourse.¹² This kind of tension and co-option is inescapable in a field so closely related to real-world developments and where it is possible for scholars to become practitioners, as many have. It is more likely still in great powers like the United States, China, and Russia, who all too often use their power in ways not sanctioned by their neighbors or the global community.

The difficulty of separating IR from politics has always touched a raw nerve in the American academy.¹³ Behavioralism became increasingly dominant in the early postwar decades. Steeped in the positivistic tradition, its advocates insisted that social science was "objective," "fact based," and "value free." They denounced other approaches as unscientific. The pendulum began to swing in the other direction by the end of the century with poststructuralists contending that social science is little more than ideological window dressing for existing power structures and their hierarchies. I follow the great *fin-de-siècle* German sociologist Max Weber in staking out a middle position. Our values influence, often determine, the conceptual categories we use to organize the world, define problems of interest, construct theories used to study them, and determine what constitutes relevant evidence and how it should be evaluated. Nevertheless, protocols for conducting good research distinguish good scholarship from mere advocacy. These protocols are constantly challenged and revised over the course of time. However, there is no escaping the fact that what we do reflects our position in society, life experiences, beliefs, and commitments. Again pace Weber, I contend that the first step in doing ethical research is to recognize and acknowledge as far as possible our parochial perspectives and reflect upon the ways in which they shape our research agenda and even our findings.¹⁴ We will see just how the ultimately unresolvable tension between the social nature of our enterprise and our desire

to do science creates serious problems for positivist and interpretivist epistemologies alike.

The close connection between the field's agendas and concepts and what it studies has another pronounced effect. Research agendas, theories, even paradigms rise and fall in importance in response to changes in the political world. IR's first hundred years witnessed phenomenal upheavals, among them the Bolshevik coup and Russian civil war; the Versailles settlement and its profound consequences for the map and politics of Europe, the Middle East, and Africa; the Great Depression, the rise of the dictators, World War II, communist victory in China's civil war; the Cold War; nationalist struggles and decolonization in the Caribbean, Africa, and Asia; divided nations and partitioned countries; the European project, the advent and spread of nuclear weapons, globalization and related economic crises; mass movements of peoples, many of them refugees; the end of the Cold War and implosion of the Soviet Union; the information revolution and the advent of cyber warfare; the phenomenal rise of China as an economic, political, and military superpower; the self-mutilation of the United States; the resurgence of the nationalist right in Europe and the United States; and, most recently, a global pandemic. Many of these events generated intense interest, and some were catalysts for paradigm shifts. More importantly, they influenced or redefined our conceptions of what global, international relations meant and even what it is for something to be characterized as political.

Fluidity in substance, theory, and methods gives further urgency to the question of just what IR is. There has never been any consensus as to the scope of the field. In its early days, IR was almost entirely focused on war prevention and conflict management among the great powers, and from a political-historical, legal, or geographic perspective. Gradually IR scholars became interested in nationalism, modernization, deterrence, crisis management, human rights, refugees and migration, women, and the reconstruction of civil society after civil wars. International political economy developed as a subfield in the 1970s and came to rival security as a focus of interest. More recently, international political theory emerged as a subfield, making efforts to link IR theory to political theory and using the latter to formulate new questions for empirical research.¹⁵ IR is not unique in this regard. In my student and early teaching days, comparative politics was largely synonymous with the governments of the great powers. To the extent that political processes came into the picture, they were largely limited to elections and interest groups.

Comparative politics has arguably evolved in its interests even more than IR.

Any definition of our field is also closely tied to paradigmatic and institutional commitments. In the United Kingdom and most of Europe, IR is considered a discipline and universities have freestanding IR departments that augment and sometimes compete with departments of politics or political science. In North America, IR is one of several fields within the discipline of political science. To justify IR as a separate discipline, or even as a separate field, it was necessary to distinguish it from departments or fields to which it had previously been attached. The initial move in this direction was to break away from history and international law. This began in the 1920s and gathered steam in the 1930s. Realists in international relations, among them Carr and Morgenthau, invoked the Kantian distinction between “is” and “ought.” They argued that IR should describe the world as it was and not as it ought to be and denied that law was useful in resolving questions that were fundamentally political in nature. In making this move, realists propagated an unfair depiction of international lawyers, whom they branded as misguided and even dangerous “idealists.” In reality, these scholars were among the first to recognize the limits of law and to describe and publicize the threat posed by Hitler.¹⁶

New arguments were coined in the aftermath of World War II. Once again Morgenthau was in the forefront and was joined by such theorists as Frederick Schumann, William T. R. Fox, and John Herz. International politics was distinguished from its domestic counterparts by the lack of any central authority.¹⁷ In contrast with the domestic politics of well-established states, there was no Leviathan to enforce laws. In keeping with this distinction, realists mobilized Weber’s characterization of a state as a territory over which a government exercised a monopoly on the use of force. This definition also suited them because it stressed power, not community, and authority, not society, as the essence of politics at every level of aggregation. In 1979, Kenneth Waltz turned this distinction into a sharp binary in his *Theory of International Politics*. He described the international “system” as one of utter anarchy, which sharply set it off from domestic politics and made security the first concern of states.¹⁸

These claims about the unique character of international relations were the conventional wisdom for decades. They strengthened the hold

of realism, which, for most of the postwar era, was the dominant paradigm in the United States. Its success, I believe, had little to do with the intellectual power of these questionable conceptual moves. Rather, it was attributable to the Cold War and concern – even fear – about the possibility of another world war. Power-centered theories that emphasized military might and offered jaundiced views of law, treaties, and international cooperation seemed appropriate to those who regarded the Soviet Union as a serious threat. Government officials and two generations of college students lapped up realism. Denuded of the sophistication found in the writings of Morgenthau and Herz, it rapidly became indistinguishable from *Realpolitik*: power politics with no concern for ethics. It provided the justification for Washington's support of right-wing dictators, coups against popularly elected left-leaning governments, assassinations of their leaders and other politicians thought to be friendly to the Soviet Union, and military interventions culminating like that in Indochina. One of the academy's leading realists, Henry Kissinger, became a principal architect of these policies.¹⁹

The relationship between theory and policy in the Cold War is not without irony. Realism was developed and propagated by scholars like Morgenthau in the hope that it would encourage policymakers to reframe their conflict with the Soviet Union. Officials and the informed public were encouraged to see it less as an ideological confrontation between good and evil and more as a power struggle between two superpowers anxious to avoid war. Rather than a fight to the death, it was a conflict that might be managed by the balance of power, diplomacy, and self-restraint. Deterrence was mobilized in a similar way. It was intended to keep war at bay by restraining the Soviet Union. However, as practiced by both sides, deterrence became a source of tension in its own right and the principal cause of the Cuban missile crisis, the most acute confrontation between the superpowers.²⁰ Subsequent arms deployments ratcheted up tensions and prolonged the Cold War.²¹ Sophocles's *Oedipus Tyrannus* dramatizes the possibility that our actions can produce results diametrically opposed to those we intend. A compelling argument can be made that deterrence theory has had this effect.

Waltz's success, which occurred late in the Cold War, was also very much a matter of timing. The behavioral revolution conquered American social sciences in the postwar era and economics was increasingly regarded as a science. Its academic practitioners won Nobel Prizes

and were recipients of high status and pay within the academy. Political scientists sought to emulate them. Waltz's theory was a particularly crude attempt to colonize IR with concepts drawn from the theory of the firm. He sought to make IR more scientific by defining theory in a way that all but excluded history, context, agency, and ethics. I read his *Theory of International Politics* when it first appeared in 1979 and could not believe anybody would take it seriously. How wrong I was! In Europe it had little impact, but in the United States it all but defined the field the next decade.

Waltz and his brand of neorealism dropped like a stone in water at the end of the Cold War. The peaceful resolution of this conflict and the breakup of the Soviet Union stood in sharp contrast to the expectations of his theory. A new set of theoretical and policy concerns emerged in response to the resurgence of identity politics, nationalism, terrorism, globalization, and regional integration. Neorealism was regarded as irrelevant to all of them. A notable exception was John Mearsheimer, whose *The Tragedy of the Great Powers*, published in 2001, tried to adapt neorealism to foreign policy.²² He stipulated that great powers have two strategic goals: to acquire as much power as possible and to prevent the hegemony of other powers. He insists that great powers have always been willing to go to war for either end.

The fate of neorealism is more evidence of the sensitivity of IR theory to real world developments. The rise and fall of paradigms and theories are hardly ever due to critiques, research, and internal debates. Major shifts are invariably responses to outside events, as indeed was the origin of international relations itself. This linkage has important implications. Like all social theories, IR theories are meant to shape our view of the world and help us cope more effectively with it. The history of IR suggests that theories do not do a good job of explaining the world, in part because they are the products of preexisting understandings of it. Waltz's neorealism and John Gaddis's best-selling book on the so-called long peace found receptive audiences because so many people in the 1950s and 1960s – especially Americans – thought war between the superpowers was highly likely or even inevitable.²³ This belief rested on the assumption that Stalin and Khrushchev like Hitler before them were intent on world conquest. The Cold War declined in intensity, although it did heat up again in the aftermath of the Soviet invasion of Afghanistan in 1979. The unexpected absence of superpower war required an