Second edition

THE CONDUCT OF INQUIRY IN INTERNATIONAL RELATIONS

Philosophy of science and its implications for the study of world politics

Patrick Thaddeus Jackson

The Conduct of Inquiry in International Relations

The Conduct of Inquiry in International Relations provides an introduction to philosophy of science issues and their implications for the study of global politics. The author draws attention to the problems caused by the misleading notion of a single unified scientific method, and proposes a framework that clarifies the variety of ways that IR scholars establish the authority and validity of their empirical claims. Jackson connects philosophical considerations with concrete issues of research design within neopositivist, critical realist, analyticist, and reflexive approaches to the study of world politics. Envisioning a pluralist science for a global IR field, this volume organizes the significant differences between methodological stances so as to promote internal consistency, public discussion, and worldly insight as the hallmarks of any scientific study of world politics.

In this second edition, Jackson has centralized the philosophical history of the "science question" into a single chapter, providing a clearer picture of the connections between contemporary concerns about the status of knowledge and classic philosophical debates about the relationship between human beings and the world they inhabit. The central chapters feature more detailed and pedagogically useful illustrations of the methodological positions discussed, making the book even better suited to clarify the philosophical distinctions with respect to which a scientific researcher must locate herself.

The second edition will continue to be essential reading for all students and scholars of International Relations, Political Science and Philosophy of Science.

Patrick Thaddeus Jackson is Professor of International Relations and Associate Dean for Curriculum and Learning in the School of International Service at the American University in Washington, DC. He is the author of *Civilizing the Enemy* (2006) and the co-editor of *Civilizational Identity* (2007).

In this nicely revised edition, Patrick Jackson makes a significant contribution to IR meta-theory with an impressive range of knowledge and a reassuring depth of understanding on important philosophical issues.

Hidemi Suganami, Emeritus Professor of International Politics,
Aberystwyth University, UK

Praise for the first edition

The Conduct of Inquiry in International Relations outlines a constructive and convincing path for getting beyond unproductive debates about the relative merits of the various methodologies that inform IR. Calling for a post-foundational IR that rests on a more expansive definition of science than that which is conventionally accepted by the field, Patrick Jackson makes a compelling case for an engaged pluralism that is respectful of the different philosophical groundings that inform a variety of equally valid scientific traditions, each of which can usefully contribute to a more comprehensive and informed understanding of world politics.

J. Ann Tickner, School of International Relations,
University of Southern California, USA

This is a book that will have a deep and lasting impact on the field. It displays impressive and sophisticated scholarship, but lightly worn and presented in an engaging manner, student-friendly but never patronising or afraid to challenge the reader. I know no better account of the various ways by which one can study IR scientifically and I am confident that this is a text that will be very widely adopted.

Chris Brown, Professor of International Relations,
London School of Economics, UK

Neatly framed, balanced, informed, lucid and, yes, important, this is the rare book I wish I had written myself. Not that I could have done it nearly as well.

Nick Onuf, Professor Emeritus of International Relations,
Florida International University, USA

In this vigorously argued, incisive and important book P.T. Jackson liberates us from the misplaced polarity between "hard, scientific" and "soft, interpretive" approaches that has bedeviled international relations scholarship for half a century. Neither approach has any grounding among philosophers of science with their insistence on the irreducibly pluralist nature of science. The immense value of this book is its accessibility and the intimate connections it builds between theories of international relations and their philosophical foundations—or lack thereof. Neo-positivist, reflexivist, critical realist and analytical stances can now engage in ecumenical dialogue rather than shouting matches or with silent scorn. If you are accustomed to worship only in your favorite chapel, here is an invitation to visit a magnificent cathedral. Graduate field seminars in international relations now have access to a first rate text.

Peter J. Katzenstein, Walter S. Carpenter, Jr. Professor of
International Studies, Cornell Universtiy, USA

Not only is *The Conduct of Inquiry in International Relations* a breathtakingly original and rigorous analysis of the scholarly work in the field, it is also an excellent teaching tool for graduate and upper level undergraduate students. By showing how ontological starting points lead to a variety of methodological options, Patrick Jackson opens up a broad toolkit for the production of knowledge in IR. His use of philosophy of science is both rich and accessible to the unacquainted reader, and brings to the light numerous misunderstandings, false argumentations, and incorrect presumptions that have become common to the field. As a result, *The Conduct of Inquiry* is both revealing and instructive, and a must-read to all who have an interest in reflecting on what's actually being done in IR.

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The Conduct of Inquiry in International Relations

Philosophy of science and its implications for the study of world politics

Second edition

Patrick Thaddeus Jackson

This book is dedicated to the memory of
Hayward Alker
and
Charles Tilly
in the hope that something of their pluralist spirit
lives on in its pages
and in its readers.

There is *only* a perspective seeing, *only* a perspective "knowing;" and the *more* affects we allow to speak about a thing, the *more* eyes, different eyes, we know ourselves to apply to the same thing, the more complete will our "concept" of this thing, our "objectivity," be.

—Friedrich Nietzsche

As we approach the third millennium, our needs are different, and the ways of meeting them must be correspondingly rethought. Now, our concern can no longer be to guarantee the *stability and uniformity* of Science or the State alone: instead, it must be to provide the elbowroom we need in order to protect *diversity and adaptability*.

—Stephen Toulmin

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Series editor's preface

Things should be made as simple as possible—not simpler. So, if this is not exactly philosophy of science made easy, it is definitely highly accessible philosophy for social scientists. It is also the most accomplished attempt to date at linking debates internal to International Relations (IR) to the history and philosophy of science generally. In Chapter 1, Professor Jackson reviews the normative debate on how to delimit science. For Jackson, science is defined by its goals, and not by its methods or theories. It is systematic, communal, and empirical production of knowledge. Social science is the systematic production of empirical, factual knowledge about political and social arrangements. Since the discipline is defined by its empirical object of study, it stands to reason that it should also take care of non-scientific tasks, such as evaluating political orders normatively or forging political arguments. Jackson is skeptical of prescribing more rigorous standards to practicing scholars, preferring instead to celebrate a broad church and pushing ecumenical dialogue. He defines philosophy of science as reflection on how we produce knowledge. Its tasks are to defuse indefensible claims about knowledge and truth, warrant specific ways of producing knowledge, and clarify implications of specific assumptions.

Chapter 2 discusses what these different ways of doing science are. For Jackson, this is first and foremost a question of philosophical ontology—that is, our hook-up to the world, how we are able to produce knowledge in the first place. There is also scientific ontology, questions concerning what kind of stuff the world consists of (individuals? theories? practices? witches?), but that is secondary. The key fissures in overall debates about science concern, first, what kind of hook-up the scholar has to the world. Am I a constitutive part of the world, or do I follow Descartes in thinking about my mind as radically cut off from the (rest of the) world? In the former case, I am a mind–world monist. In the latter case, I am a mind–world dualist. There is a choice to be made here, one consequence of which is what kind of methodology is suitable for doing research. Methodology—the logical structure and procedure of scientific inquiry—must necessarily follow the scholar's type of hook-up to the world. Jackson sees the key problem of the discipline in the doxic status accorded to mind–world dualism. The only places in the book where Jackson is scathing of his colleagues are the ones where he dissects how scholars who had their heyday in the 1970s spent the 1980s and

1990s attempting to discipline younger colleagues who attempted to enrich the discipline by trying out other ways of doing science:

> Putatively radical insurgencies have their critical edges blunted by the seemingly reasonable offer of being taken seriously by the rest of the field as long as they formulate testable hypotheses and join the search for systematic cross-case correlations arranged so as to approximate covering-laws.
>
> (p. 60)

The fissure between monists and dualists is not alone in dividing the discipline, however. A second key fissure turns on another question of philosophical ontology—namely, what kind of status our theories are given. Are they transfactual, meaning that they are based on the real existence of structures that generate observable stuff that we may then study, or are they phenomenalist, meaning that they are based on the scholar's experiences (and not rooted in any further claim about something really existing outside of those experiences)?

Note that Jackson privileges these two fissures at the cost of a number of other candidates, such as positivist versus interpretivist and qualitative versus quantitative. Such fissures easily degenerate into questions of methods—techniques for gathering and analyzing bits of data—questions that are less foundational than the questions of ontology and methodology singled out for discussion here. Note also the lack of interest in debates about epistemology. If philosophical ontology concerns the choice of how to hook up to the world and methodology how to order the proceedings of doing it, then epistemology may be safely occluded.

Depending on what philosophical wagers scholars place regarding the two key fissures, they place themselves in one of four cells in a two-by-two matrix. Chapters 3 through 6 give the historical preconditions for the emergence of the ensuing four positions—neopositivism, critical realism, analyticism, and reflexivity—and discuss their internal debates and aporias. Here we have a neat ideal-typical heuristic device for presenting ongoing research in IR in terms of philosophy of science orientations. Each cell gives a different answer to the problem with which we have wrestled since Descartes, namely how to overcome the mind/world split when we hook our inquiry up with the world. Neopositivist workhorses find the answer in falsification. Critical realist ones find it in the best approximation between abduced dispositional properties and the object under study. To analyticists and reflexivists, the answer is not to put Descartes before the horse, however, but to put the horse before the cart. Rather than let the old Cartesian legacy drag them along, they try to dissolve Descartes' question, either by drawing up an ideal-typical analytic, or by using themselves as effects of structures, structures that may be found by looking at one of its effects: me and my social relations.

Neopositivism is "neo" because of Popper's insistence that falsification, and not verification, should be our guiding star of hooking up to the externally given world. A key point in Chapter 3 is, however, that IR neopositivism is not particularly "neo," inasmuch as its methodology usually comes down to "tossing hypothetical conjectures against the mind-independent world, in the hope that at least some

of them will survive repeated attempts to refute them" (p. 82) The joy seems to be in evading falsification, not in actually locating it. Inasmuch as neopositivism remains the wholesaler of IR theory, far outstripping other providers, from a mainstream point of view, any other way of doing research remains controversial. For this second edition of the book, Jackson has consolidated the argument by elaborating on how US positivism battens down the hatches.

Among the small subset of IR scholars who preoccupy themselves with philosophy of science questions, critical realism seems to be almost all the rage. The underlying theme in Chapter 4 is the continuity from Marxist to critical realist methodologies. In order to get from the postulation of really existing transfactuals to the inquiry into observables, critical realists avail themselves of abduction, the act of positing or conjecturing the existence of some process, entity, or property that accounts for observable data. The ultimate point of the exercise seems to be to delineate "the real limits of the possible, in the hope that a politically savvy agent will take advantage of them in transformative ways," as Jackson puts it.

The hero of Chapter 5 is Max Weber, whose ideal-type procedure is paradigmatic of the mind–world monist phenomenalist approach. Jackson stresses that constructivism is "the generic term for non-dualist approaches to the production of knowledge that limit themselves to the empirical realm," but that since that term is already in use within the discipline with another address, analyticism will have to do. This is the home of IR theorists such as the Weberian Morgenthau and the structural-functionalist Waltz, who stresses how theories may only be overtaken by another theory (since there simply does not exist for him an independent world against which to "test" the theory). Practice theory of a Wittgensteinian kind, which is now finally reaching IR, does also belong here.

Most practice theory would, however, end up with the reflexivists, who are discussed in Chapter 6. Where analyticists stick to the empirical realm, inspired by a tradition stirring in Kant, fleshed out by Hegel, and coming into its own in Karl Mannheim's sociology of knowledge, as well as in the work of sundry continental philosophies, reflexivists go further in one (or more) of three ways. They postulate further knowledge claims to round out accounts of social worlds; they claim to be able to approximate knowledge that is constitutive of a certain social group (and so is not necessarily there to be experienced directly, but must be postulated to exist transfactually); and/or they "make space for … [a social] group's perspective to contribute to a potentially broader grasp of things." Jackson draws his argument to a close with a blistering defense of pluralism.

In the preface to the first edition, I hazarded the guess that there would be an interesting reception in store for this book, not least because young scholars would be wont to find different ways to hook up their research to the world. It gives me professional and personal pleasure to report that, since the publication of the first edition five years ago, *The Conduct of Inquiry* has gone on to become the new gold standard for philosophy of science debates within the discipline.

Iver B. Neumann

1 Playing with fire

Although an innovative astronomer and an important contributor to the development of planetary science, the late Carl Sagan is probably best remembered among the general public for two of his other activities: his popularization of contemporary natural science (especially astrophysics), and his highly public and unapologetic condemnation of "pseudoscience" concerning crystals, ESP, and alien abductions. The two activities fit together quite well, as they are united by a commitment to spreading a particular sensibility out beyond professional specialists and into the wider community. In a collection of essays entitled *The demon-haunted world*, Sagan borrows a metaphor from Thomas Ady's seventeenth-century tract condemning witch-hunts, to describe his public and popular work as an effort to shine an illuminating light into the dark corners of the contemporary world: to light a candle in the hopes of banishing the shadows. The candle he sought to light and to wield against the darkness was what he called *science*:

> In science we may start with experimental results, data, observations, measurements, "facts." We invent, if we can, a rich array of possible explanations and systematically confront each explanation with the facts. In the course of their training, scientists are equipped with a baloney detection kit. The kit is brought out as a matter of course whenever new ideas are offered for consideration. If the new idea survives examination by the tools in our kit, we grant it warm, although tentative, acceptance. If you're so inclined, if you don't want to buy baloney even when it's reassuring to do so, there are precautions that can be taken.
>
> (Sagan 1997, 209–210)

Sagan's account of the mechanics of science is probably fairly familiar to us, as it tracks quite closely with the notion of "falsification" famously propounded by Karl Popper (1992): science, in Popper's formulation, proceeds and progresses through successive efforts to *dis*prove conjectures, rather than through efforts to verify or justify them. But Sagan's metaphor—science as a candle in the darkness—should be scarcely less familiar, drawing as it does on a longstanding tradition in the philosophy of knowledge that equates knowing with seeing, and reason—often exemplified by science—with a source of light. Famously, John

Locke drew on this metaphor in his *An Essay Concerning Human Understanding*, admonishing his readers to use their natural faculties of reason to the best of their ability:

> It will be no excuse to an idle and untoward servant, who would not attend his business by candle light, to plead that he had not broad sunshine. The Candle that is set up in us shines bright enough for all our purposes.
>
> (Locke 1959a, 30)

Further, Locke deployed the notion of reason as a defense against popular deception in a manner quite reminiscent of Sagan's stance:

> Reason is natural revelation, whereby the eternal Father of light and fountain of all knowledge, communicates to mankind that portion of truth which he has laid within the reach of their natural faculties: revelation is natural reason enlarged by a new set of discoveries communicated by God immediately; which reason vouches the truth of, by the testimony and proofs it gives that they come from God. So that he that takes away reason to make way for revelation, puts out the light of both, and does muchwhat the same as if he would persuade a man to put out his eyes, the better to receive the remote light of an invisible star by a telescope.
>
> (Locke 1959b, 431)

Setting aside the language of divinity for a moment, we can see a clear continuity between Locke and Sagan. Both point to a natural faculty that can be developed and deployed against error, and both symbolically equate that faculty with "light"—and oppose it to the "darkness" of misconception and superstition. Similarly, both privilege science as a superior way of gaining and evaluating knowledge—Sagan uses the term "science," while Locke, preferring the term "reason," explicitly associates himself and his argument with great scientists of the day such as Newton and Boyle. Whatever else it is good for, science appears in their conception as our best defense against error.

Of course, such arguments are not only advanced by philosophers and astronomers. Closer to home, as it were, David Laitin (2003, 169) advances a very similar image of science—including social science—as containing "ample procedures for figuring out if our best judgments are misplaced" and hence serving as "the surest hope for valid inference." Laitin pairs this declaration with a denunciation of Bent Flyvbjerg's *Making Social Science Matter* (2001) for allegedly violating the strictures of science and opening the door to a kind of anything-goes relativism—the ultimate nightmare about what the abandonment of the ground of "science" might mean in practice.[1] In their popular and oft-cited methods handbook, Gary King, Robert Keohane, and Sidney Verba flatly declare: "research designed to help us understand social reality can only succeed

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1 Quite a debate ensued; see the papers collected in Schram and Caterino (2006).

if it follows the logic of scientific inference" (King, Keohane, and Verba 1994, 229). And King, in a triumphalist article about his Institute for Quantitative Social Science at Harvard University, declares that "areas of scholarship dedicated to understanding, or improving the well-being of, human populations" are well served by the construction of an infrastructure explicitly modeled on the organization of research in the natural sciences (King 2014). The juxtaposition of science and (potential) error, therefore, seems just as prominent in our field as it is in other domains.

Arguments such as these pose extremely fundamental questions about the character of our scholarly enterprise. Scholars of politics who advance such claims are quite clearly drawing on the cultural prestige associated with the notion of "science" in the contemporary age (Litfin 1994) as part of an effort to shape the practices of their colleagues involved in the effort to produce knowledge about the social world. To invoke "science" is to call to mind a panoply of notions connected with truth, progress, reason, and the like—and, perhaps more importantly, to implicitly reference a record of demonstrated empirical success. These sorts of appeals function this way particularly in internal debates among scholars of the social world, as tossing an appeal to "science" into such debates is like playing a very valuable trump-card that implicitly, if not explicitly, calls the entire status of the scholarly field into question. Within the field of International Relations (IR)[2] in particular, the "science question" has long vexed scholars, coming to a head in the field's second "great debate" between self-identified traditionalists and scientists (Knorr and Rosenau 1969) but never really getting resolved or losing its scholarly resonance (see the discussion in Kratochwil 2006). Especially under such circumstances, it is impossible to invoke the notion of "science"—let alone to propose turning to either the practice or the philosophy of science in an effort to clarify or improve our own scholarship!—in any kind of neutrally descriptive manner. Playing the science card raises the stakes.

The science question in IR

It is important to note at the outset that the role played by "science" in our field is at least conditionally, if not completely, independent of any detailed philosophical or conceptual sense afforded to the term. In debates about the proper conduct of IR scholarship, we typically operate with caricatures and generalities rather than precise specifications, speaking loosely of "*the* scientific method" or "*the* philosophy of science" as though either of those two things actually existed. Although there have been some notable exceptions in recent years, most references to and invocations of "science" in the field seem to operate with

2 I follow conventional scholarly usage in distinguishing between "international relations" (or "international affairs") as an object of analysis and "International Relations" or "IR" as a scholarly enterprise. Although increasingly my thinking prefers "international studies" to "International Relations / IR" as a way of naming the scholarly field, I adhere to common usage throughout the text.

an image of knowledge-production that is a curious amalgamation of Sagan's skeptical "baloney detection kit," an embrace of mathematical formalism, and a desire for law-like generalizations that hold true across cases (given appropriate scope conditions, of course). This is a curious amalgam because the first defines a skeptical *attitude*, the second defines a formalist *method*, and the third defines an epistemic *goal*—and none of these are perfectly characteristic of any actually existing scientific practice. In debates about knowledge-production in our field, what is most often in play is not a specific account of science, but a vague and general sensibility.

Of course, this is in no way just a comment on the present state of the field. Throughout the history of IR, the term "science" has been flung around in extremely cavalier ways, serving most often as the positive pole of a contrast that an author wishes to draw between her or his approach to generating and evaluating claims about international affairs and some reviled alternative. For example:

> This book has two purposes. The first is to detect and understand the forces that determine political relations among nations, and to comprehend the ways in which those forces act upon one another and upon international political relations and institutions. In most other branches of the social sciences this purpose would be taken for granted, because the natural aim of all scientific undertakings is to discover the forces underlying social phenomena and the mode of their operation.
>
> (Morgenthau 1985, 18)

Thus Hans Morgenthau claimed early in his textbook *Politics among nations*, characterizing his approach as a "scientific undertaking" with little more than a vague gesture in the direction of "forces underlying social phenomena." There is no more specific discussion of the character or value of science in the book, although Morgenthau generally takes it for granted that only a scientific study can provide the basis for a responsible pursuit of a peaceful world; that, indeed, is the second "purpose" of his book (ibid., 20). The general notion or idea of "science," and the cultural prestige associated with it, suffices to legitimate Morgenthau's enterprise.

Morgenthau was very aware of this cultural prestige, having railed at length against the over-scientizing of the contemporary age in his 1946 masterpiece *Scientific man vs. power politics*:

> Politics is an art and not a science, and what is required for its mastery is not the rationality of the engineer but the wisdom and the moral strength of the statesman … The age has tried to make politics a science. By doing so, it has demonstrated its intellectual confusion, moral blindness, and political decay.
>
> (Morgenthau 1946, 10)

The problem, Morgenthau argued then, is that we put too *much* stock in science, and thus overlook the distinctiveness of the political and social world.

In his typically Weberian fashion, Morgenthau argued that we make a category mistake when we expect science to solve our political problems; instead, we should respect the limits of human knowing, and keep science in its place. "For the liberal, science is a prophecy confirmed by reason; for the conservative, it is the revelation of the past confirmed by experience" (Morgenthau 1946, 32). Casting himself on the "conservative" side of the ledger, Morgenthau engaged in a very interesting double intellectual operation: on one hand, criticizing the over-reliance on science, but on the other hand, claiming some of its cultural prestige for his own project of knowledge-production. The result, whether by accident or by design, is the simultaneous preservation of the notion that we ought to have "scientific" knowledge of international affairs, along with a good deal of ambiguity about precisely what that might mean in practice.

In pursuing this line of argument, Morgenthau was in a way simply following the precedent laid down by E.H. Carr in *his* announcement of a scientific study of international affairs.[3] Carr talked about science, but never precisely defined the term except to contrast science with both unchecked idealism and unchecked realism (Carr 2001, 87). The science Carr announced would avoid both of those partisan-political stances, instead aiming for a more comprehensive view. But the scientific study of international affairs, Carr acknowledged, would not be a simple transplantation of procedures from the natural sciences:

> The laboratory worker engaged in investigating the causes of cancer may have been originally inspired by the purpose of eradicating the disease. But this purpose is, in the strictest sense, irrelevant to the investigation and separable from it. His conclusion can be nothing more than a true report on facts. It cannot help to make the facts other than they are; for the facts exist independently of what anyone thinks about them. In the political sciences, which are concerned with human behavior, there are no such facts. The investigator is inspired by the desire to cure some ill of the body politic. Among the causes of the trouble, he diagnoses the fact that human beings normally react to certain conditions in a certain way. But this is not a fact comparable with the fact that human bodies react in a certain way to certain drugs. It is a fact which may be changed by the desire to change it … The purpose is not, as in the physical sciences, irrelevant to the investigation and separable from it: it is itself one of the facts.
>
> (Carr 2001, 4–5)

This does not tell us much about what it *means* for something to be a science. Indeed, Carr's claim is quite difficult to elucidate, because it is unclear just what is "scientific" about *both* a report on facts that are independent of human recognition *and* a report on facts that can be changed by the desire to change them—and Carr

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3 Of course, the U.S. context within which Morgenthau's claim was advanced also decisively affected both his strategy and the eventual—unintended—outcome of that strategy. For an extended discussion, see Guilhot (2011).

gave his readers little explicit guidance on this issue. Neither did Morgenthau, who similarly claimed that "social conditions" are more closely interwoven with scientific inquiry in the social sciences (Morgenthau 1946, 162). Both of these seminal IR scholars were quite confident that the study of international affairs can and should be a "scientific" one, but it was not a central concern of either author to spell out precisely what it means for a study to be scientific. Instead, both were content simply to invoke the notion of "science" in the course of justifying their approaches.

Matters became more specific with the next of the field's "great debates"—a controversy "over the merits of the traditional and scientific approaches to the study of international politics," in which the main protagonists were Hedley Bull, arguing for tradition, and a diverse cast of characters arguing for science (Knorr and Rosenau 1969, iii). Bull characterized the opposition between these two approaches as mostly a matter of style and technique, with the traditional approach emphasizing "judgment" derived from an intimate experience with the history and philosophy of politics, and the scientific approach aspiring "to a theory of international relations whose propositions are based either upon logical or mathematical proof, or upon strict, empirical procedures of verification" (Bull 1969, 20–21). That this was largely a tactical difference became clear with Bull's declaration that:

> The theory of international relations should undoubtedly attempt to be scientific in the sense of being a coherent, precise, and orderly body of knowledge, and in the sense of being consistent with the philosophical foundations of modern science. Insofar as the scientific approach is a protest against slipshod thinking and dogmatism, or against a residual providentialism, there is everything to be said for it.
>
> (Ibid., 36)

In this broad sense, Bull's definition of science was strikingly similar to that of Carr or Morgenthau. What he objected to were quantitative and formal techniques, and the drive towards generalization—precisely the features privileged and defended by self-identified "scientists" such as J. David Singer and Marion Levy. Levy was quite clear that "a generalized system of theory … hopefully with deductive interdependencies among the members of the set" (Levy 1969, 92) is the ultimate goal of any science, and he agreed with Singer that "we will never build much of a theory, no matter how high and wide we stack our *beliefs*" (ibid., 71)— the conduct of science means moving beyond beliefs and evaluating those beliefs in the light of systematic empirical evidence. In this debate, scientists took traditionalists to task for simply resting, content with their intuitions; traditionalists took scientists to task for their remoteness from the subject-matter.

But all sides of the debate agreed that the point of studying international affairs is to produce empirically grounded and justified claims. This made the controversy a disagreement about the relative contribution of general propositions and hypothetical models, on one hand, and detailed historical reconstructions,

on the other, to the understanding of international affairs. Read in this way, the debate featured much less of an unbridgeable divide than might have at first appeared: everyone wanted to be "scientific" in the broad sense, and to produce coherent and orderly knowledge, but they disagreed as to which techniques were actually "scientific" in the relevant way. However, it is significant that this was *not* Bull's rhetorical strategy; instead of defining and defending a broad account of science against the more elaborate and specific account advanced by his (largely American) opponents, Bull in effect *conceded* the notion of "science" to his opponents and took his stand elsewhere. The fact that Bull's broad definition of science is buried within the sixth of his seven critiques of formalist quantification and the quest for general propositions indicates something of how far it was away from the main thrust of his argumentative strategy.

Thus, the actual, if unintended, result of the "second great debate" in IR was to link "science" with quantification, formal models, and general propositions, replacing Carr and Morgenthau's vague notion of science with something more precise, while retaining the cultural prestige of the notion. Singer, Levy, and other self-identified "scientists" made numerous references to the successes of physics and economics, holding out hope that IR could enjoy similar successes by becoming equally "scientific." The editors of the volume containing many of the important essays constituting the controversy even pioneered a strategy of reconciling the two approaches under a common banner, a strategy that further reinforced the equating of "science" with the formulation of general propositions:

> [W]hy could not the traditionalists take on the burden of casting their conclusions in the form of hypotheses testable in other situations? This would not undermine their inquiries, but it would maximize their possible contribution to the work of their more scientific colleagues. Likewise, why could not the scientists append summaries to their studies that straightforwardly identify their major propositions and findings? Such additions would not jeopardize their procedures, but they would make the products of their research more accessible to those who prefer nonscientific modes of inquiry.
>
> (Knorr and Rosenau 1969, 18)

Notice that, in this passage, the main "burden" falls on the traditionalists, who have to adopt a form of presentation that makes their claims ready for evaluation by the techniques preferred by self-identified "scientists." The only thing that the "scientists" have to do, apparently, is to produce a plain-English account of their study—a communicative, rather than a methodological, modification. Testable hypotheses and general claims are thus portrayed as almost unquestionable goals of IR scholarship, hardly even needing the label "science" to distinguish them from alternatives. But the label continues to serve a useful function in reaffirming the status of those fundamental assumptions—as when, a quarter-century later, King, Keohane, and Verba declared that "the social science we espouse seeks to make descriptive and causal inferences about the world" (King, Keohane,

and Verba 1994, 7) and passed quite seamlessly from that claim to a series of discussions about strategies for testing hypothetical generalizations.

In fact, "science," in IR, has come to mean more or less precisely what Bull's opponents asserted that it meant, and the historical controversy between the traditionalists and the scientists has been recoded or reconceptualized as a dispute about styles of presentation or argumentation. "'Science' versus 'tradition'" has morphed into "'quantitative' versus 'qualitative'," a characterization that effectively strips any fundamental philosophical or conceptual issues out of the disagreement (Yanow and Schwartz-Shea 2006, xv–xix). Knorr and Rosenau noted this at the time of the initial debate:

> Why, then, could not the traditionalists employ rather than deplore the quantitative findings of the scientists, refining them as seems suitable to their own way of thinking? And why could not the scientist use rather than abuse the qualitative insights of the traditionalists, subjecting them to the rigors of their procedures in the same way they do their own ideas?
>
> (Knorr and Rosenau 1969, 18)

While it remains a bit unclear how traditionalists uninterested in general propositions might "employ" quantitative findings, the idea that a "scientist" could take a traditionalist's conclusion or insight and subject it to procedures of hypothesis testing (especially if the traditionalist had followed their advice to state the insight in the *form* of a testable hypothesis, thus relieving the "scientist" of any conceptual labor of translation) is both a well-defined intellectual operation and a clear example of the priority accorded to "science" understood as the quest for generalized theoretical knowledge. The persistence of this priority of general propositions over insight based on intimate familiarity with particular situations can be seen in King, Keohane, and Verba's suggestion that "nonstatistical research will produce more reliable results if researchers pay attention to the rules of scientific inference—rules that are sometimes more clearly stated in the style of quantitative research" (King, Keohane, and Verba 1994, 6). This applies above all to "qualitative" studies, where researchers can only guarantee their "scientific" status by seeking to distinguish systematic from nonsystematic components of a situation even in their descriptions of that situation (ibid., 56). Every scholarly practice, then, is to be subordinated to the specific notion of "science" established as dominant in the discipline during the debate with Hedley Bull.

Of course, this outcome was somewhat foreshadowed by Bull's own confused position about science (Kratochwil 2006, 9). Because Bull failed to articulate a clear *alternative* to systematic generalization across historical cases, for example, he opened his position up to the rejoinder that there was no compelling reason *not* to subject the results of a detailed empirical-historical account to broader evaluation. Especially since this technique seemed to have proven so helpful in other fields of inquiry, the argument in favor of the "scientists" appeared almost unassailable. In practice, the most prominent dissenters focused more on pointing out the shortcomings of the "scientific" position than on elucidating a

concrete alternative, calling for greater reflexivity among scholars (Lapid 1989) or affecting a whole-scale turn towards political and normative theory (Connolly 1989). Critics of generalized theoretical systems, such as Richard Ashley (1983; 1984), followed in Bull's footsteps by leaving the notion of "science" itself untouched in the field and permitting the self-proclaimed "scientists" to continue their monopoly on defining the term.

This strategy was evident even in the most successful effort to garner some "thinking space" (George and Campbell 1990) in the field for empirical scholarship not particularly interested in the formulation and evaluation of theoretical generalizations. Martin Hollis and Steve Smith's *Explaining and understanding international relations* was one of the first books to elucidate cogently a form of empirical knowledge-production that was not simply a deficient or low-tech version of the hypothesis testing/generalization approach. Hollis and Smith began with the delineation of two "intellectual traditions" animating the production of empirical knowledge in the social sciences: one derived from the natural sciences and the other derived from nineteenth-century hermeneutics. "Explaining" designates the first approach; "understanding," the other. Hollis and Smith then quickly proceeded to draw a series of other distinctions that map onto this same basic division: "outsider" versus "insider" accounts, causes versus meanings, and preferences versus rules (Hollis and Smith 1990, 1–7). The authors argued that these two bundles—causal outsider accounts using preferences to explain what actors do in international affairs, and meaningful insider accounts using social rules to understand what actors do in international affairs—were virtually incommensurable, leaving us with a situation in which there are always two separate stories to tell about any given empirical situation. The authors were also meticulous in avoiding any kind of comparative analysis of the two approaches, concluding the book with a dialogue between themselves that highlights the strengths and shortcomings of each approach in terms of the other (ibid., 203–214).

The clear implication of the Hollis and Smith depiction of empirical inquiry in IR was that "scientists" did not have a monopoly on knowledge-construction; there was an established, vibrant tradition operating with very different assumptions about how knowledge ought to be produced, and it was in some sense equal in value to its "scientific" alternative. The argument established a diversity of modes of inquiry, but at a fairly significant cost. "Explanation," rooted in "the attempt to apply the methods of natural science to the world of international relations" (ibid., 45), received causation and preferences, while "understanding" was left with the explication of social rules and the delineation of the motives of actors[4]—a stance that, incidentally, left many understanding-accounts vulnerable to critiques that they were actor-reductionist or perhaps even idealist.[5] More to the

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4 Understanding might also have received constitutive explanation, but that is a more complex issue, which I will defer discussion of until Chapter 4.

5 "Motives lead to outcomes" is, in fact, the classic statement of reductionism criticized by Waltz (1979) and Singer (1961). And "social rules help us understand outcomes" is only a small step away from "ideas and beliefs cause outcomes," which is how IR "scientists" typically misunderstood idealism (Ashworth 2006).

point, the Hollis and Smith strategy allowed the self-proclaimed "scientists" to continue to claim both the centuries-old tradition of the natural sciences *and* the cultural prestige associated with that tradition. Practitioners of "understanding" had no such proud parentage to claim, but instead had to be content with a bevy of German philosophers and British anthropologists.

From this potted history of some key debates in the field of IR, I would like to draw two conclusions. First, "science" has been a notion in play in IR debates since the very beginning of the scholarly study of international affairs. Indeed, we could easily go back *before* the establishment of the study of international affairs as a distinct scholarly endeavor and find "science" playing an important role in debates about the status of international law (Schmidt 1998, 104–106; Orford 2014) and in the efforts of scholars of politics to distinguish themselves and their work from purely partisan-political activity in the very early part of the twentieth century (Adcock 2003, 501–506)—to say nothing of the continuing role played by "science" in the shaping of the discipline of Political Science, within which so much of Anglophone IR scholarship is located (Gunnell 1993). For the moment, it is sufficient to note that the shapers of the field of IR have been concerned about the scientific status of their scholarship for a very long time. Because of this long-standing history, "science" remains a notion to conjure with in the field of IR; it is a veritable "rhetorical commonplace" (Jackson 2006, 27–32), which is available for deployment within all kinds of controversies. And a powerful resource it is, too: charging that a piece of work is not "scientific" carries immensely negative connotations, both because of the field-specific history I have sketched here and because of the broader cultural prestige enjoyed by "science" (Moses and Knutsen 2007, 155–156).

This leads to my second conclusion: the *function* of the commonplace "science" within IR is primarily a *disciplining* function. When "science" makes an appearance, it is a pretty good bet that the text in which the term is invoked is more or less explicitly trying to reshape how inquiry is conducted, and doing so by drawing on the rhetorical power of "science" in order to privilege some modes of inquiry at the expense of others. If "science" is a good and valuable thing, then non-"science" cannot be as worthwhile an endeavor. Simply rejecting "science," or elaborating an alternative such as "understanding," leaves the whole discursive arrangement intact, and does not really offer a reasonable or effective rejoinder to the charge that the non-"scientific" work that one is doing is not somehow of lesser value. There is no effective way around this unless the whole field abandons any claims to or aspirations of being scientific. Absent this unlikely possibility, the question of science remains almost unavoidable for IR scholarship.

The demarcation problem

Philosophers of science sometimes refer to the "science question" as the *demarcation problem*: the quest for a set of criteria that can adequately demarcate science from non-science. "Adequately" here generally means something more profound than the disciplining deployment I have been discussing; philosophers

working on the demarcation problem are looking for defensible logical or conceptual criteria, powerful enough that their application to a given scholarly controversy will yield a philosophically valuable determination of the scientific status of a given claim or position or approach, and help to explain the success of that science. Such philosophical work does, of course, draw on the cultural prestige of the commonplace "science," but seeks to give content to that label such that the claim to be "scientific" might rest on firm foundations rather than on a vague appreciation for modern technological marvels such as the computer or the airplane.

Inasmuch as philosophical elaborations of demarcation criteria are based on detailed study of successful (and sometimes unsuccessful) sciences, a philosophical solution to the demarcation problem would provide an answer to the question of how IR ought to proceed as a scientific field. In fact, the most prominent use of philosophy of science in IR has been precisely along these lines and has featured efforts to spell out concrete steps that need to be undertaken in order to make IR more, or more properly, scientific. The basic structure of the argument is quite simple: according to some philosopher, successful science S engages in scientific practices $sp_1 \ldots sp_n$; we want IR to be a science too; ergo, we ought to engage in $sp_1 \ldots sp_n$ in IR. Elaborating such sets of practices by referring to something that is rather uncontroversially a science, such as evolutionary biology (Bernstein et al. 2000) or paleontology (Van Belle 2006), implicitly invokes a set of demarcation criteria that both define the science in question as a science, and encompass the subject matter of IR in such a way that practices the author identifies in one domain can be easily transported into the other domain. The uncontroversial identification of the "scientific" domain *as a science* spares the person making the argument from having to spell out explicitly just what it is that defines something as a science: we know it when we see it, after all, and if something works in physics or in paleontology it ought to work in IR, right?

The problem, of course, is that without a clear explication of the criteria that make a given practice of knowledge-production scientific, we have no good way to answer that question. Maybe there is something specific about, say, the empirical domain of physics that enables it to be uniquely scientific in a way that simply will not work if applied to the study of human beings and their social relations.[6] Or maybe different approaches to knowledge-production have their own internal standards and practices, such that trying to apply techniques and procedures from one domain to another is nonsensical at best and harmful at worst. It is impossible to make a decision about matters such as this without a much clearer and more precise elaboration of what a science *is*, which is where philosophers of science might enter the picture. If philosophers agreed on a set of criteria that served to demarcate science from non-science, then we would have a defensible basis on

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6 Elizabeth Anscombe suggests that this may just be the case with the motion of planets in the solar system, which erroneously gave rise to the notion that Newton's laws provided a paradigm for scientific explanation *per se* (1993, 99).

which to examine claims about particular ways in which knowledge-production practices in IR ought to be disciplined.

Unfortunately, philosophers have come to no global consensus about what defines a field of inquiry as a "science" or a practice of knowledge-production as "scientific." Even worse, different attempts to determine such criteria proceed in wildly divergent directions and elucidate incompatible or contradictory positions on the importance of logical consistency, empirical observability, and predictive accuracy (among other criteria) to a compelling definition of science. Under these circumstances, a turn to the philosophy of science is unlikely to be able to put an end to the science question in IR, precisely because philosophers of science have not themselves reached a consensus about these issues.

The roots of the traditional demarcation problem in the philosophy of science go back to the early twentieth-century "logical positivists" of the Vienna Circle. Confronted with Marx, Freud, Einstein, and a whole slew of theories about racial and national "destinies," the logical positivists sought to elucidate a foolproof way to distinguish between a scientific and a non-scientific statement. Besides being an interesting intellectual puzzle, the scientific status of a claim was also a pressing political and social problem: it mattered a great deal whether a denunciation of the received wisdom about sexuality, time, space, or governmental authority should be considered "scientific" and thus worthy of respect, or unscientific and hence intellectually valueless (Moses and Knutsen 2007, 38–39; Lakatos 2000, 22–24). The logical positivists' major criterion for distinguishing a scientific from a non-scientific claim was *verifiability*, which maintained that a claim could only be scientific if all of its terms could be checked or confirmed through an examination of the empirical world (Ayer 1952, 38).[7] The verifiability criterion would rule out claims involving "'entelechy' in biology, 'historical destiny of a race' or 'self-unfolding of absolute reason' in history," because they were not verifiable—but were instead "mere metaphors without cognitive content" (Hempel 1965b, 237).

However, the verifiability criterion also raised problems for notions such as "force" or "cause," which had long been staples of natural-scientific work. Indeed, a sensibility in many ways quite akin to that of the Vienna Circle led Ludwig Wittgenstein to banish causality from the scientific lexicon altogether: "There is no compulsion making one thing happen because another has happened. The only necessity that exists is *logical* necessity" (Wittgenstein 1961, §6.37). In general, logical positivists preferred to speak of a nomological explanation of an event, "showing that its occurrence could have been inferred … by applying certain laws of universal or of statistical form to specified antecedent circumstances" (Hempel 1965c, 302). Causality was thus redefined to mean a law-like relationship between phenomena. But this only displaced the problem, because *law-like claims are not verifiable*. All that exists, empirically, are specific objects and entities inhabiting particular situations, and if we were to confine ourselves strictly to what we can

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7 However, not all of the members of the Vienna Circle were content with the verifiability criterion, and many of them moved beyond it in their own subsequent thinking. See the discussion in Chapter 2, below.