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Philosophy of Social Science and International Relations

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When a discipline begins to reflect on its own practices there are various resources on which it can draw and a range of foci upon which the gaze can be turned. In Chapter 1 of this volume Brian Schmidt has addressed the history and historiography of the discipline. Inevitably, many of the issues deemed important in any historical account of disciplinary development will necessarily touch upon issues of relevance to the philosophy of social science (Gordon, 1991; Manicas, 1987). The philosophy of social science is inseparable from the history of social science, and many of the debates that have shaped international relations (IR)[1] have been concerned with issues integral to the philosophy of social science. Where Schmidt deals with the manner in which these issues have historically (mis)shaped the discipline, this chapter will focus on their content and attempt a critical analysis of them in relation to their deployment in terms of disciplinary development, disciplinary politics and wider sociopolitical concerns.

A key issue for any social science discipline is the extent to which it might be considered a science,[2] and Schmidt identifies this question as a 'defining goal of the field' (See Chapter 1). However, where Schmidt sees the development of IR in terms of a continuing attempt to provide scientific credentials for its knowledge claims, I see a discipline that is structured around a set of deep contestations over the very idea of science itself and the extent to which IR can, and should, be a science. The development of IR cannot be understood as the inexorable march towards science since many within the discipline are opposed to a science of IR, irrespective of any benefits that might derive from the label. What science is and whether IR can or should be a science is a subject of impassioned debate within the discipline (Bull, 1969; Ferguson and Mansbach, 1988; Hollis, 1996; Hollis and Smith, 1990; Kaplan 1969; Nicholson 1996a, 1996b; Ogley 1981; Reynolds, 1973; Wendt, 1999). For many working within the philosophy of social science this issue effectively defines the content of its subject matter (Bhaskar, 1979: 1; Brown, 1979: vii; Fay, 1996: 1). Following conventional usage within the philosophy of social science I shall call this the problem of 'naturalism' (Bhaskar 1979; Hollis, 1996).[3] Within the context of this overarching question a range of subsidiary issues are typically subsumed: the nature of explanation, the nature of causation, the nature of laws and so on (Bunge, 1996; Nicholson, 1996a; Reynolds, 1973; Suganami. 1996).

Inevitably, answers to this question have been legitimated by recourse to the philosophy of social science. The philosophy of social science, however, is itself parasitic upon the philosophy of science, and to a large extent much of the literature that addresses the science question in IR bypasses the philosophy of social science completely (Vasquez, 1995, 1998; Waltz, 1979).[4] This is a regrettable, although understandable, development, and the unreflective importation of the frameworks of philosophers of science to either legitimate a scientific IR (Kuhn, Lakatos, Popper), or to defend IR from science (Kuhn, Feyerabend) has done perhaps serious damage to the discipline (Ferguson and Mansbach, 1988). This damage pales in comparison, however, to that inflicted by the assumption that what science is, is self-evident.

None of this, of course, is to argue that the philosophy of social science, and hence by extension the philosophy of social science in IR, is only concerned with the question of science. Another fundamental question has revolved around what is known in IR as the agent–structure problem (Carlsnaes, 1992; Dessler, 1989; Wendt, 1987; Wight, 1999a). This issue defies easy definition, and within IR the confusion over what exactly is at stake in the agent–structure problem has led one pair of commentators to suggest that it is not at all clear if the contributors to the debate in IR are referring to the same problem (Friedman and Starr, 1997). Whatever this problem does involve, however, all parties agree that a substantive element of it concerns a conundrum best elaborated by Marx: 'Men make their own history, but they do not make it just as they please; they do not make it under conditions chosen by themselves' (Marx, 1962). The agent–structure problem then, is concerned with the relationship between active and self-reflecting agents and the structural context in which their activity takes place.[5] There are many aspects to this problem and it has surfaced under various guises within the philosophy of social science[6] (Singer, 1961). When combined with the issue of naturalism, it is tempting, as indeed many have done, to picture these problems in terms of a matrix such as Figure 2.1 (Hollis and Smith, 1990; Wendt, 1999; see also Carlsnaes, Chapter 17 in this volume).[7]

The problems with such pictorial representations go well beyond the self-evident point that they have their limitations in terms of how much detail they can represent (Bourdieu, 1977; Hollis and Smith, 1992: 216; see also Carlsnaes, Chapter 17 in this volume). The real difficulty with such diagrammatic devices is that their inability to deal with the complexity of the issues introduces a high level of distortion as to what the actual fault lines are. That is, the matrix provides an image of rigid boundaries that do not hold when the issue is considered in other discursive and less dichotomous ways. Moreover, taking seriously the fact that its practitioners largely construct IR, we can see how the fault lines of contemporary IR might themselves be an artefact of the pictorial representation of them in two-by-two matrix form. In short, the use of such devices to explain disciplinary divisions contributes to their construction. Such devices may be valuable aids in teaching and understanding complex issues, but we should always be aware of what Mario Bunge calls the 'Myth of Simplicity' (Bunge, 1963; see also Carlsnaes, Chapter 17 in this volume).

The aim of the chapter is not simply to outline the various uses of the philosophy of social science within IR. Nor is it simply to reiterate the well-worn, and overused, claim that things are more complicated than the literature portrays them. The primary aim of the chapter is to provide an account of the philosophy of social science within IR in

Figure 2.1   *A landscape of philosophical positions in relation to social study*

order to demonstrate that the contemporary theoretical cleavages that structure the discipline are unable to contain the weight they are being asked to bear. In short, the contemporary meta-theoretical framework the discipline employs is: a bar to constructive dialogue; a hindrance to much-needed research into issues of vital concern; a confused misrepresentation of the issues; and most importantly, a construct of those working in the field, hence they have it within their power to change it.

I begin by providing legitimations for taking the philosophy of social science seriously and give a brief sketch of the development of the philosophy of social science. In the following section I briefly discuss the early development of the discipline in the context of claims to be a science of social affairs. The philosophy of social science is largely missing from this period of the discipline's development, as, of course, it must be given that the philosophy of science had not yet emerged as a sub-discipline of philosophy. The third section deals with the first genuine attempt to constitute IR as a science on the basis of literature drawn from the philosophy of science and the philosophy of social science. A key component here will be understanding the role of positivism and its use within the discipline.[8] In the fourth section I will concentrate on contemporary debates and will, in particular, attempt to throw some light onto what is increasingly becoming what one commentator has called 'a philosophical swamp' (Walker, 2000). Here I demonstrate how the current ways of framing disciplinary debates are rapidly deconstructing themselves.[9]

LEGITIMATION: DOES IR NEED THE PHILOSOPHY OF SOCIAL SCIENCE?

The utility of examining the philosophy of social science within IR is not self-evident. Critical voices

have often doubted whether the discipline has either the intellectual resources, or the need, to engage in such an exercise (Griffiths and O'Callaghan, 2001: 199; Skocpol, 1987). Many would prefer to leave such esoteric speculation to those more able – philosophers perhaps (Wallace, 1996). Others doubt whether philosophy as a different 'order of discourse' can provide the kind of legitimation claimed on its behalf (Gunnell, 1975: 54; 1998: 6). Often this skepticism towards disciplinary self-reflection derives from a belief that such inquiries lead to the neglect of more substantive forms of knowledge generation (Gunnell 1998: xii; Halliday, 1996: 320; Mann, 1996; Skocpol, 1987). There are also legitimate concerns about naïve appropriations of 'Gurus' from cognate disciplines (Hollis and Smith, 1991).

There is, of course, something deeply ironic in the fact that the social sciences feel the need to legitimate their activities in relation to the philosophy of social science. After all, apart from some notable exceptions, scientists rarely legitimate their practices in terms of the philosophy of science (Gordon, 1991; Gunnell, 1998; Nicholson, 1996a). Indeed, modern science only emerged as a science once its autonomy from philosophy was firmly established (Easton, 1965; Gordon, 1991; Gunnell, 1975; Little, 1980). Given the success of the natural sciences, allied to the desire to emulate them, some have argued that it was inevitable that social inquiry and philosophy would likewise divorce if such forms of inquiry were to constitute themselves as sciences (Little, 1980: 3; Nicholson, 1996a: 8–10).

To view this process as inevitable, however, is probably too strong a characterization. Whilst most natural scientists were happy to leave speculative philosophy behind, many concerned with social inquiry were not (Gadamer 1977; Winch, 1958; in IR see Bull, 1969; Garnett, 1984; Hollis and Smith 1990; Little, 1980). This is an intellectual split that still structures the contemporary social sciences, but it is important to note that it emerges not only out of a desire to maintain a philosophical presence within social inquiry, but also from a desire to keep a certain form of science out (Bull, 1969; Reynolds, 1973). In general, those who reject a scientific IR are not against systematic inquiry *per se* (Garnett, 1984; Reynolds, 1973). Indeed Vico, often cited as an authoritative source by those against a social science, entitled his major work *New Science*[10] (Vico, [1744] 1984). When hermeneutics first emerged as a distinctive approach to inquiry, its early proponents still conceived of themselves as being engaged in the development of a science of meaning (Bauman, 1978; Dilthey, 1976; Husserl, 1982; Outhwaite, 1975). Often the rejection of a science of the social world is derived from deep-seated fears in relation to some claimed dehumanizing aspects at the heart of science itself (Aliotta, 1914; Ashley, 1987, 1989; Morgenthau, 1946; Thompson, 1981).

The philosophy of science only really emerged as a recognizable field of study in the 1930s (Dingle, 1952; Gordon, 1991; Gunnell, 1998; Oldroyd, 1986). Early understandings of science were rudimentary and were generally based upon accounts developed by Thomas Hobbes, John Stuart Mill, David Hume and Rene Descartes (Gordon, 1991). However, conscious reflection on the nature of human inquiry can be said to have played a role in the human sciences ever since reflection on the human condition became a recognizable activity (Gordon 1991; Manicas, 1987). Thucydides, for example, is said to have been the first scientific historian (Abbott, 1970; Gilpin, 1986: 306; Tellis, 1996), or perhaps even a positivist (Bluhm, 1967).

It is doubtful if this characterization of Thucydides as a positivist can be sustained (Bagby, 1994; Garst, 1989), particularly if one places the development of positivism in a historical perspective (Kolakowski, 1969; Oldroyd, 1986). Yet, it does highlight the manner in which positivism and science became interchangeable terms in the twentieth century (Bhaskar, 1986). Equally, it points to an important reason for considered reflection on the nature of the knowledge claims of all social sciences. For despite doubts concerning the ability of the philosophy of science to provide a justificatory framework for natural science, the results of science, particularly in the form of technological innovation, can hardly be doubted (Gunnell, 1998; Nicholson, 1996a, 1996b). This success has given science enormous prestige in modern societies – a prestige, which despite some dissenting voices, it still largely holds (Appleyard, 1992; Dunbar, 1995).

If social inquiry is to emulate the natural sciences it needs to examine its methods, procedures and underlying rationale. It needs a yardstick against which claims to be science can be measured. Where better to look than the philosophy of science? Hence, whereas the natural sciences became sciences through an enforced divorce from philosophy, social science turned to philosophy for legitimation. Since knowledge claims in social science are almost always couched in terms of some philosophical justificatory framework, the various disciplines have felt the need to examine the status of them (Reynolds, 1973: 14). Not least because claiming that one's research is science is exactly to claim legitimacy not accorded to other forms of knowledge (Ashley and Walker, 1990; Smith, 1987).

Gunnell (1975: 54) sees this as an impossible enterprise and argues that political 'science must chart its own methodological route, and that the defence of that route cannot be achieved by invoking the authority of science'. There are two problems with this claim. First, the influence of the philosophy of science on social inquiry is not simply methodological, and second, his argument relies on the assumption that the philosophy of science can tell us nothing about the practices of

science; and, of course, if this were the case then he would be correct. But the philosophy of science *does* claim to reflect on the practice of science and to pronounce on some of its essential elements. No doubt it will get much wrong, but there is no a priori reason to assume it will get it all wrong. Since the philosophy of science does claim some legitimacy in terms of its understanding of science, then it is perfectly appropriate for social inquiry to look to it for resources. If Gunnell's argument were to be followed to its logical conclusion, political science and IR would be excluded from drawing on any resources other than those developed within the discipline (see Reynolds, 1973 for arguments counter to Gunnell's). Moreover, academic disciplines are not as hermetically sealed as Gunnell seems to suggest and include philosophical concepts as essential elements within their frameworks.

Many of the concepts developed in the philosophy of science have been thoroughly integrated into the fabric of the discipline (Gunnell, 1975: xiii) and, perhaps more than any other factor, have shaped the discipline's self-image and continue to do so (Nicholson, 1996a, 1996b; Smith, 1995). In this respect, IR has little alternative but to engage with the philosophy of social science. This accounts for the fact that introductory sections and chapters on this issue feature in almost all textbooks. Another reason is that conceptual inquiry is a prerequisite to empirical research (Walker, 1993: 82). Before empirical research can proceed, researchers need to have some idea of what it is they are attempting to explain and how best to explain it. All inquiry begins from certain premises, and understanding the basis of these is an essential part of inquiry.

The final reason why such abstract conceptual inquiries are important is that whereas natural scientists may disagree on the actual content of specific explanations, they at least agree on what an explanation of a given phenomenon would look like (Nicholson, 1996a: 2; Reynolds, 1973). Social scientists, on the other hand, do not (Hollis and Smith, 1990; Reynolds, 1973). For a discipline supposedly born out of a desire to uncover the causes of war, not knowing the conditions under which such a discovery might be made seems a damning indictment (Nicholson, 1996a: 3). Knowing the causes of war is one thing; knowing that we know them is an altogether different matter.

Yet engagement alone does not guarantee success, and it has to be admitted that many of the complaints against the use and abuse of the philosophy of social science within IR have some substance (Halliday, 1994: 23; Kratochwil, 2000; Wallace, 1996). In general, these problems occur due to a lack of conceptual clarity, the misuse of key terms and the naïve appropriation of key concepts developed in cognate disciplines with little awareness of the specifics of their use or the context of their

development. The most glaring examples of these concern the use of terms such as ontology, epistemology and methodology, although the widespread and uncritical adoption of Kuhn's notion of paradigms comes a close second (Banks, 1985; Vasquez, 1998). Within the philosophy of social science and the philosophy of science these terms have very specific uses and function to maintain analytical clarity and as ways of delineating very specific aspects of the field. In IR, on the other hand, these terms are often thrown around like philosophical hand grenades, with little consideration given to how they are deployed, or to what end.

Michael Nicholson, for example, in a series of otherwise exemplary works, has variously referred to positivism as an 'epistemology' (Nicholson, 1996a, 1996b), a 'methodology' (despite the chapter title being 'The Epistemology of International Relations') (Nicholson, 1996a; Nicholson and Bennett, 1994), 'behavioralism'[11] (Nicholson, 1996a; 129) and any 'sort of scientific approach to social behaviour' (Nicholson, 1996a: 190) – although admittedly this latter is with a sense of regret. Likewise, Steve Smith refers to positivism as: an epistemology (Smith, 1996: 24); as *having* an 'empiricist epistemology' (Smith, 1996: 22); and as being the 'methodology' that underpins realism (Smith, 1997: 166). I highlight these two eminent scholars not as the worst examples of this tendency, but merely representative ones. But clearly, there is some confusion here.[12]

EARLY IR: A SCIENCE WITH NO PHILOSOPHY

There was a time in the discipline's pre-history when science was not a problematic term (Bluhm, 1967; Boucher, 1998; Dougherty and Pfaltzgraff, 1996; Tellis, 1996). Early practitioners were perhaps not clear on how the term was deployed, but there was a general acceptance that IR could and should be a science. Ashley J. Tellis argues that the development of realism from Thucydides to the present day can be understood as a 'Long March to Scientific Theory' (Tellis, 1996). And despite a number of critiques questioning the extent to which Thucydides can be considered a realist, few have doubted that his discussion of the Peloponnesian War is 'severe in its detachment, written from a purely intellectual point of view, unencumbered with platitudes and moral judgments, cold and critical' (Bury, 1975: 252).

Hobbes, of course, had provocative views about which subjects could be deemed to be scientific, but there is little doubt that he considered his own work a science and he perhaps even thought of himself as the inventor of political science (Ryan, 1996; Sorell, 1996). Within Hobbes's notion of political

science there were already the seeds of a very clearly demarcated difference between what he called 'political science' and 'political prudence' (Ryan, 1996). According to Hobbes, Thucydides's analysis was based at the level of political prudence; in general it equated to practical wisdom and was achieved through the best advice we could draw from a range of historical examples. Political prudence was a genuine form of knowledge, yet it is inevitably knowledge of particulars. Charles Reynolds seems to suggest that all historical explanations are of this form (Reynolds, 1973). It is a form of knowledge based upon experience of the past and of what has happened. It is not, however, knowledge of how things *must* work and what *must* happen. Science, for Hobbes, must be hypothetical, general and infallible. But none the less, politics could, and indeed should, be a science.

Even interwar idealism can be interpreted as committed to the role of science in human progress (Carr, 1946; Long, 1995: 306). And insofar as this period of IR was driven by Enlightenment ideals of progress based on knowledge, this point seems hardly in doubt (George, 1994: 74–7). Richard Little, however, argues that early IR differed from other social sciences that emerged at the time in that it did not attempt to model itself on the natural sciences and was not 'concerned with uncovering laws which would assist in the comprehension of an infinitely complex reality' (Little, 1980: 7; see also Smith, 1987). Little's position, however (see also Smith, 1987), suffers from two problems.

The first demonstrates the validity of Schmidt's claim that bad histories of the discipline can distort current understandings (See Chapter 1 in this volume). For Little's sharp demarcation between IR and other social science disciplines only makes sense if one accepts that when the first academic department was set up in 1919 in Aberystwyth this constituted a unique moment with no disciplinary prehistory. What Schmidt very clearly shows, is that although 1919 does mark the emergence of a specific academic department of IR, knowledge production of the subject had a much longer prehistory. In this respect the disciplines that Little claims did attempt to model themselves as sciences were the sources upon which the new discipline of IR was to draw (Schmidt, 1998; see also Butterfield, 1951).

The second problem with Little's analysis is that he is projecting a very particular account of science back onto the work of the interwar idealists. He seems to assume that a normative dimension to inquiry precludes it from being a science; that science is concerned with factual analysis and value-driven inquiry is something different (Little, 1980: 7). This is a very particular, and contentious, account of the fact/value relationship within science, and there is no evidence that it is one held

by the early members of the discipline. Moreover, there are many defenders of a scientific IR who are committed to providing scientific explanations precisely in order to bring about social change (Nicholson, 1996a: 3; 2000: 197; Wright, 1962).

The charge that the early origins of the discipline were 'unscientific' is located within the damning critique launched by E.H. Carr. In what can only be considered a strategic polemic, Carr argued that the 'science of international politics is in its infancy' (Carr, 1946: 14). According to Carr, realism could provide such a science through its emphasis on 'the acceptance of facts and on the analysis of their causes and consequences' (1946: 14). The alternative to this science, according to Carr, was idealism, which he characterized as 'alchemy' (1946: 14).

Interestingly, despite Carr's avowed commitment to science, some have argued that he is best considered part of the interpretive tradition within the discipline (Dunne, 1998: 7), whereas others see him as operating with both a scientific and interpretive outlook (George, 1994: 77). But whichever tradition (if indeed there are clear borders) Carr should be considered to be within, his critique of the idealists does indicate something important about the disciplinary politics of such labels. Carr's claim that realism was based upon acceptance of the facts and analysis of their causes and consequences is mirrored by Norman Angell's plea for the development of education about international political affairs. The lack of such education, claimed Angell, was a barrier to the 'impartial search for truth, the true interpretation of all the facts' (Angell, 1947: 17); without this belief we render 'inoperative the only method by which we can hope to make steady progress: the correction of social theory and doctrine in the light of fact and experience; the scientific method applied to society' (Angell, 1947: 23). Given the similarities between Angell's idealist approach to IR and Carr's more realistically inclined tendencies, Carr's science/alchemy dichotomy can only be seen as a conscious attempt to seize some 'scientific' high ground – a ground to which Angell also staked a claim.

Hans Morgenthau is an interesting figure in terms of this development because he was one of the first major figures in the discipline to openly argue against IR as a science. His anti-scientific turn, however, had very specific origins. His early work was conceived as an attempt to provide a 'scientifically unassailable classification of international disputes' (Honig, 1996: 289). And this commitment to science was still evident in his 1940 essay 'Positivism, Functionalism and International Law' (Honig, 1996; Morgenthau, 1940). In this piece he bemoaned the attempt to construct international law at a technical level devoid of scientific principles (Morgenthau, 1940: 284). This position was completely reversed in *Scientific Man and Power*

Politics, where he rejects all hope of a scientific IR (Morgenthau, 1946, 1972). Still, Morgenthau's clear renunciation of science and positivism, which he claimed was fictional, metaphysical and dogma (Griffiths, 1992), has not stopped scholars from aligning him with a science of IR (Hollis and Smith, 1990: 23), with some even going as far as to label him a positivist (George, 1994; Hollis and Smith, 1990: 28; see Bain, 2000 for an alternative view, Garnett, 1984; Nicholson, 1996a).

The assertion that Morgenthau should be viewed as committed to a science of IR is generally made on the basis of his claim that politics was governed by 'objective laws that have their roots in human nature' (George, 1994: 93; Hollis and Smith, 1990: 23–4; Morgenthau, 1948: 4). But to construe this claim as supporting a commitment to scientific IR is to miss the point. In conceding that politics is governed by objective laws of human nature Morgenthau is actually saying that there is no need for a science of IR, because IR is governed by laws that are explained by biology, not social science (Griffiths, 1992: 39). There is nothing for a science of IR to discover. Morgenthau's theory is best viewed as a manual for state leaders. It is a technical guide to policy based on an understanding of the laws that govern human behavior. More important is the fact that Morgenthau does not ground his arguments about human nature in any scientific content, but in metaphysical ones (Griffiths, 1992: 38, 43; Honig, 1996: 305).

What is interesting about these developments is the absence of any sustained discussion on the nature of the science that was either being advanced or rejected. There was little attempt to legitimate claims about science by recourse to bodies of literature developed in other disciplines, and no real attempt to spell out the actual content of the science being proposed. Indeed, for someone like Herbert Butterfield, science simply *was* traditional forms of inquiry (Butterfield, 1951; Dunne, 1998: 123). This lack of legitimation in terms of the philosophy of science is understandable given the underdeveloped state of the philosophy of science at the time. However, developments were moving on rapidly and a consensus was emerging which was, for better or worse, to stamp its mark on IR in ways that could not have been envisaged. The science of IR was about to rediscover some philosophy.

Adolescent IR: The Legitimation of Science

The systematic use of the philosophy of science within IR begins with what John Vasquez terms the 'behavioral revolt' (Vasquez, 1998: 39). Although this 'revolt' had been taking place within political science and other social sciences since the early 1950s, it did not begin to emerge into IR in a substantive way until the l960s (Knorr and Rosenau, 1969a). There had been calls for its introduction into IR prior to this (Guetzkow, 1950), and some argue that works such as Quincy Wright's 1942 book on war are behavioralist (Knorr and Rosenau, 1969b: 5; Schmidt, Chapter 1 in this volume). Vasquez, however, sees these developments, whilst validly described as behavioral in intent, as not substantively contributing to the coming 'revolt' (Vasquez, 1998: 40). Given this periodization of the 'revolt', the sources of the 'behavioral revolt' are generally located in Deutsch (1953, 1964), Kaplan (1957), Schelling (1960) and Snyder, Bruck and Sapin (1954, 1962); (Hollis and Smith, 1990; Vasquez, 1998; Schmidt, Chapter 1 in this volume). Schmidt, however, claims in Chapter 1 that the role of the Chicago School of political science generally goes unrecognized in the dominant accounts of the development of behavioralism. And from the perspective of the philosophy of science Schmidt's point seems broadly correct.

In 1950, Harold Lasswell and Abraham Kaplan explicitly argued that their attempt to provide a framework for political science was informed by developments in logical positivist philosophy of science (Gunnell, 1975; Lasswell and Kaplan, 1950).[13] This turn to the philosophy of science was validated by David Easton (1953, 1965), who very clearly did influence the 'behavioral revolt' in IR, and Robert Lane, who argued that 'the widespread acceptance of the philosophy of science as a basis for social inquiry represents a "take off" phenomenon in social science, promising sustained growth in social interpretation' (Lane, 1966).[14]

A key component of logical positivism that served to legitimate the turn to the philosophy of science was its 'unity of science thesis' (Nagel, 1961). This, of course, is self-validating; logical positivism declares that the sciences can be unified and logical positivism defines the content of science. So any social science deserving of the label science needs logical positivism just as logical positivism provides the legitimation for the turn to the philosophy of science (Bhaskar, 1986). This usurping of the label science was to be an important move in the 'great debate' (Dunne, 1998) between traditionalists and scientists, because essentially the label *science* was conceded to logical positivism.

This is an important point and highlights something often missed in disciplinary discussions relating to the study of IR, for the model of science that underpins the 'behavioral revolt' in IR is based upon a very specific philosophy of science and not the practices of scientists (Gunnell, 1975: 19). Despite claims to be following the scientific method, behavioralism was actually an attempt to implement a particular philosophy of science that

was dominant at that time. The relationship between the actual practices of scientists and logical positivism was not yet a question that would be subject to challenge (Chalmers, 1992). Once IR had turned to the philosophy of science to legitimate its practices it was inevitable that when the philosophy of science began to question the account given by logical positivism then IR would follow. This has led to various modifications to logical positivism and eventually the term 'logical' would be dropped in favor of a less austere version under the label of positivism (S. Smith, 1996: 14–18).

This also helps explain many of the contemporary confusions surrounding science in IR, since it is never clear whether it is science *per se* that is being rejected, the logical positivist version, or other less extreme positivist versions. This problem is compounded by the fact that there is no longer a consensus on what positivism is, with one commentator identifying twelve versions of it (Halfpenny, 1982). Moreover, the philosophy of science itself was soon to reject positivism and to claim that the practices of scientists did not conform to the positivist model. This held out the rather paradoxical prospect that all approaches that had attempted to emulate the positivist model were not actually following scientific procedures. Yet, whatever problems emerged in terms of philosophy's own quarrel with logical positivism, when the behavioralists turned to philosophy a consensus had emerged within the philosophy of science around the validity of positivism, hence it was perfectly correct for the discipline to adopt that model. In fact, given the level of consensus that existed within the philosophy of science around logical positivism/positivism it would have been perverse not to adopt it (Chalmers, 1992).

Before proceeding to examine its reception within IR it is important to consider something of the claims being made on its behalf that had a significant impact on IR. Two in particular stand out: operationalism and instrumentalism were at the heart of the 'behavioral revolt', and both are firmly embedded within logical positivism/positivism (Gunnell, 1975). The commitment to operationalism is generally well understood: since, the validity of a theory ultimately rests on the 'facts', all concepts that are considered to be scientific or empirical must be defined operationally. Within behavioralism this has generally being taken to mean the language of observation (Gunnell, 1975; Nicholson, 1996a). Less well understood is the closely related instrumentalism that pervaded logical positivism/positivism.

Instrumentalism was the device employed by positivists to get around some tricky questions concerning the status of non-observable terms in theories. From the instrumentalist perspective, theoretical concepts are judged not by their truth or falsity, but by their theoretical utility (Singer, 1969: 76; Waltz, 1979: 8; Wasby, 1970: 66). For the instrumentalist, theories cannot be taken as assertions about the way the world is. Theoretical terms that could not be translated into observational ones were to be treated 'as if' they existed. Facts are what matter and theory is simply a better way of collecting them (Gunnell, 1975: 26–7). This incipient instrumentalism helps explain why a philosophy so firmly embedded within the requirements of validity through observation became so adept, and so insistent, on the need to build models and, in particular, models of the system.

From this instrumentalist perspective, 'truth' was not part of the lexicon of positivism, nor was any search for underlying causes (see Griffiths, 1992: 96–8, for an account of why Kenneth Waltz is not concerned with truth). Indeed, positivism since Comte had long given up according ontological status to anything beyond the phenomena or the search for truth (Comte, [1854] 2000: 28). According to Comte:

> In the final, the positive state, the mind has given over the vain search after Absolute notions, the origin and destination of the universe, and the causes of phenomena, and applies itself to the study of their laws – that is, their invariable relations of succession and resemblance … I merely desire to keep in view that all our positive knowledge is relative, and, in my dread of our resting in notions of anything absolute … (Comte, [1854] 2000: 68, 190)

This also helps illuminate how some contemporary confusions emerge in relation to positivism. For example, Hollis and Smith's claim that Morgenthau's version of realism is 'an essentially positivistic way of analysing events, since it relied on a notion of underlying forces producing behaviour' (Hollis and Smith, 1990: 23) is problematic given positivism's rejection of the search for underlying causes.

Underpinned by logical positivism, a more overt scientific approach took a firm hold in the discipline (Alker, 1965; Dunne, 1998; Hollis and Smith, 1990; Hoole and Zinnes, 1976; Rosenau, 1971). When viewed from the perspective of the philosophy of social science, four aspects stand out. First, whatever the merits of logical positivism, behavioralism in IR was at least consistent with its fundamental principles and attempted to validate its 'scientific' credentials as opposed to simply taking them as given. Abraham Kaplan's *The Conduct of Inquiry* (1964) is perhaps the most important work in this respect, but others had preceded it (Brecht, 1959; Van Dyke, 1960; see also Meehan, 1968). The behavioralists seemed to understand the philosophy and applied it consistently; something which could not be said of many of its detractors, both then and now.

Second, its critique of realism, which it claimed was not scientific enough, injects a real tension in

any subsequent account that attempts to claim that realism is positivist (George, 1994; Smith, 1996). The behavioralists were scathing about the lack of rigour within classical realism (Hollis and Smith, 1990: 28). Consistent application of their logical positivism entailed that assumptions about human nature were metaphysical, non-observable and hence unscientific. Given the variations in realism and the variations in positivism, it is highly unlikely that a blanket claim that realism is positivist can be sustained.

Third, the importation of this approach to IR was not without sustained resistance. At the forefront of this resistance was Hedley Bull's polemical attack on what he called the scientific approach (Bull, 1969: 361). Against this scientific approach, which he clearly sees embedded within logical positivism (Bull, 1969: 362), Bull argues for the 'classical' approach embodied within the works of Zimmern, Carr and Morgenthau (for a detailed and sophisticated treatment of the debate see Dunne, 1998). Because of the polemical nature of Bull's attack and Morton Kaplan's (1969) rejoinder, there is a tendency within the discipline to see this 'debate' in terms of a growing rift between American social science and academic communities in the rest of the world (Hoffman, 1977; Smith, 1987; see also Schmidt, Chapter 1 in this volume).

Donald J. Puchala, however, argues that within American IR the new version of science peddled by behavioralists was rejected by major American figures in the field (Ferguson and Mansbach, 1988; Puchala, 1991). Stanley Hoffmann, in an early critique characterized as a 'wrecking operation', was scathing about Kaplan's proposed science of IR (Hoffman, 1961). But also Leo Strauss (1953) attacked the onwards march of 'scientism in political science' and Michael Haas (1969) identifies many American critics. As already noted, an important aspect of this debate was the manner in which all of the critics allowed the behavioralists to take control of the label science. From this point on, science became inextricably linked to positivism and any reference to science was taken to imply positivism.

Fourth, whilst the introduction of behavioralism was initially hailed as a dramatic stride forward in terms of the development of a 'scientific' IR (Lijphart, 1974a, 1974b), later accounts now argue that this debate did not fundamentally change underlying assumptions and was essentially only a very limited debate about methodology (Guzzini, 1998; Hollis and Smith, 1990; Holsti, 1985, 1998; Vasquez, 1998). This is a problematic claim (Dunne, 1998: 124; see also Schmidt, Chapter 1 in this volume); debates about science can never simply be 'methodological'. Positivism is a philosophy of, and for, science and its adoption requires the taking of a series of implicit ontological and epistemological

assumptions as well as methodological ones. It is for partly this reason that contemporary claims that positivism is an epistemology are wide of the mark (Nicholson, 1996a).

Positivism embodies certain epistemological commitments, but it is not itself an epistemology; unless, that is, one is stretching the use of the term epistemology to such lengths as to make it meaningless[15] (Smith, 1996: 17). But one only has to examine the substance of Bull's arguments to see that they were primarily ontological not methodological. His critique of the scientific approach was precisely that the following of its methodological strictures left a large, and important, area of international politics unexamined. So even though his target might be considered to be the proposed new procedures of science, these were based on ontological assumptions. Moreover, as a philosophy of science with well-formulated accounts of cause, explanation, law and the nature of the world, it is also incorrect to consider positivism as simply a methodology.

Another neglected aspect of the behavioral revolution within IR is the extent to which its adherents conceived of themselves as going beyond social science and instituting a 'behavioral science' (Easton, 1965: 18). The 'behavioral revolt' was not only about placing IR on a more scientific basis, but about taking part in an ambitious attempt to unify all of the human sciences into a seamless whole. David Easton accepted that prior to the 'behavioral revolt' the social sciences were deserving of the label science (Easton, 1965: 22). He also accepted that the 'behavioral revolt' could not only be about the introduction of more scientific rigor. Indeed, he argued that more rigour would mean 'rigor mortis, as its critics from the traditional points of view … have been so quick and correct to point out' (Easton, 1965: 22). In a very Comtean manner, Easton saw the behavioral movement as the next stage in the development of human knowledge, where the human sciences would be united into one research programme, centered on the notion of behavior. This was a very strong version of the unity of science thesis.

Whatever the overall impact of the 'behavioral revolt' on the discipline, it legitimated the turn to the philosophy of social science and the philosophy of science. References to Hempel, Nagel, Popper, Kuhn, Feyerabend and Lakatos became commonplace. Waltz devoted a chapter of his *Theory of International Politics* (1979) to the philosophy of science, and strongly defended an instrumentalist treatment of theoretical terms (Griffiths, 1992: 93). And, of course, Thomas Kuhn has shaped the discipline in fundamental ways. Moreover, Kuhn's framework implicitly continues to shape the discipline today, even if the language used is no longer that of paradigms. That Kuhn's

framework was adopted so universally across the discipline is puzzling when one considers that Kuhn himself thought that the social sciences were in a pre-paradigmatic state and doubted whether they could ever be 'mature sciences' (Kuhn, 1962: 164–5; see also Kuhn, 1970: 245; see Ferguson and Mansbach, 1988 for a critique of the attempt to apply Kuhn to IR).

Yet, reasons for Kuhn's success in the social sciences are not hard to find. Political scientists, sociologists and anthropologists recognized in their own practices and disciplinary conflicts Kuhn's picture of paradigms. They were delighted to hear that what had previously been thought an embarrassment was the way it was done in respectable sciences. Traditionalists could now portray themselves as working in a different paradigm, thus making themselves immune to critiques from the scientists. The scientists could continue unabashed, safe in the knowledge that they were actually contributing to knowledge growth under the guise of normal science. And dissidents could now portray themselves as revolutionary heroes of a new paradigm. Here was a philosophy of science that not only seemed to put science in its place, but legitimated what social scientists already did and required little in the way of change. Kuhn's ambiguous terminology was also a key factor. His master concept, that of paradigm, was particularly subject to various interpretations; Margaret Masterman (Masterman, 1970) identified twenty-one different ways Kuhn used the term – a criticism Kuhn accepted (Kuhn, 1970). This ambiguity allowed the framework a large measure of flexibility and ensured its welcome into disciplines that made definitional debate a key component of their research practices.

Kuhn's framework was almost universally adapted. Arend Lijphart saw the 'great debates' of the discipline in terms of paradigms (Lijphart, 1974a, 1974b). From the 1980s onwards, IR caught the paradigm bug so comprehensively that paradigms and Kuhn became part of the unreflective subconscious of the discipline. Textbooks were organized according to paradigms, and Kuhn was perhaps cited more than home-grown disciplinary figures (Banks, 1984; Hollis and Smith, 1990; Little and Smith, 1991; Viotti and Kauppi, 1987). But Kuhn's framework came with two related and major problems.

The first was an incipient conservativism (Guzzini, 1993: 446; Smith, 1992: 494; Wight, 1996). Science progressed, argued Kuhn, in periods of normal science (Kuhn, 1962; see Toulmin, 1970 for a critique). This claim had normative force. It meant that if progress in terms of knowledge production were to be achieved, then IR scholars needed to find themselves a dominant paradigm. Realism seemed an obvious candidate, but it would have come as no surprise to Kuhn to see competitors quickly emerging. The inter-paradigm debate that developed in IR vindicated Kuhn's assertion that the social sciences were pre-paradigmatic (Kuhn, 1962: 164–5). But if IR scholars were to achieve progress and move into normal science then the discipline needed a dominant paradigm. This meant that pluralism could be seen as a threat to progress. But Kuhn had already built into his framework a mechanism where paradigms could flourish, even if progress could not.

This was the issue of incommensurability (in IR see Guzzini, 1993; Neufeld, 1995; Nicholson, 1996a; Rengger, 1989; Waever, 1996; Wight, 1996; see also Sankey, 1994, 1997). Kuhn had seemed to suggest that the move from one paradigm to another was a revolutionary process and that there was no way to compare paradigms (Kuhn, 1962, 1970). Paradigm choice, Kuhn seemed to suggest, was a matter of faith; or what Imre Lakatos would call 'mob psychology' (Lakatos, 1970: 178). This made any notion of an inter-paradigm 'debate' oxymoronic (Nicholson, 1996a: 82). Which, of course, did not deter people from continuing as if there was a debate. However, incommensurability became another Kuhnian buzzword that seemed to offer non-mainstream approaches some shelter. After all, did not incommensurability leave the world safe for critical theory?

Dissenting voices, however, were soon to see the perils in the incommensurability thesis (Guzzini, 1993; Waever, 1996; Wight, 1996). Incommensurability not only provided a safe haven for critical theory, but also for the mainstream (Guzzini, 1993). If incommensurability meant that cross-paradigmatic conversation was in principle impossible, how could the critics critique the mainstream (however defined)? Steve Smith, invoking ontological grounds for incommensurability, argued that it meant that proponents of different paradigms literally lived in different worlds (Smith, 1992, 1996). If so, there is little point in trying to critique the world of the mainstream from another world. However, it is very doubtful if Smith's reading of incommensurability was Kuhn's interpretation of it. Kuhn went to great lengths to dispel the idea that incommensurability meant that theories were non-translatable (Kuhn, 1970, 1982, 1990). Also, some in the discipline began to challenge the philosophical grounds of the incommensurability thesis itself (Wight, 1996).

There is little doubt that Kuhn's work has fundamentally – for better or worse – shaped the discipline. However, the discipline has typically seen this as a resource to be mined as opposed to displaying any awareness of either the complexities of his ideas, or the many trenchant critiques of his position. Even in those instances where the difficulties are acknowledged these are brushed aside in the attempt to apply the framework (Vasquez, 1998; see Katzenstein et al., 1998 for similar treatment of

Lakatos). Often, Kuhn's notion of paradigms was grafted onto a Lakatosian framework for theory choice with little in the way of justification (Christensen and Snyder, 1997; Elman and Elman, 1997; Vasquez, 1997; for a critique see Waltz, 1997). Philosophy of science was now in IR and the discipline needs to consider it much more carefully if it is to play such a fundamental role. Unfortunately, before the discipline could reflect on its turn to the philosophy of science there was to be an explosion of alternative philosophical sources of inspiration.

CONTEMPORARY IR: PHILOSOPHY, BEGINNING AND END?

If the Kuhnian experience within the discipline once again vindicated the turn to the philosophy of science then the philosophy of social science was surely everywhere. Unfortunately this was not the case. Despite a vast body of literature on the philosophy of social science the number dealing with these issues specifically in relation to IR is small (George, 1994; Hollis and Smith, 1990; Neufeld 1995; Mackenzie, 1967, 1971; Nicholson, 1983, 1996a; Reynolds, 1973; Sylvester, 1993). There are, of course, many references to the philosophy of social science, but these are scattered around the discipline in fragments (Alker, 1996; Campbell, 1988; Carlsnaes, 1992; Dessler, 1989; George and Campbell, 1990; Wendt, 1987). Hollis and Smith, in the first sustained presentation of this argument within IR, argue that the discipline could do better than turning to the philosophy of science and that there were models of social science not based on the natural sciences that might be more appropriate (Hollis and Smith, 1990: 68–91). The philosophical inspiration for their argument is Peter Winch, although they also draw on a range of hermeneutic thinkers as well, particularly Weber (Weber, 1949; Winch, 1958).

In fact, Hollis and Smith's argument had already played a fundamental role in structuring the discipline, even if those arguing against a science of IR have never specifically located their argument in a sustained engagement with the philosophy of social science. Reynolds (1973) perhaps stands out as a notable exception, but his work is concerned with the distinction between science and history, as opposed to that between science and hermeneutics. Moreover, Reynolds still draws heavily on the philosophy of science and includes no specific references to Winch, although Winch's book does appear in his bibliography (Reynolds, 1973). More importantly, and contrary to Hollis and Smith, Reynolds argues that the traditionalists and the scientists have 'more in common than their advocates have perhaps realized' (Reynolds, 1973: 15).

Likewise, W.J.M. Mackenzie (1967, 1971) might also be considered an early contributor but he sees no fundamental conflict in the attempt to integrate a scientific IR with more traditional forms of inquiry. Even Bull's attack on a 'scientific' IR is notable for its lack of references to a philosophical rejection of the natural science model, though his arguments seem to imply an awareness of the issues (Bull, 1969).

Hollis and Smith's book emerged in the context of what has come to be called the post-positivist turn (Biersteker, 1989; George, 1989, 1994; Holsti, 1989; Lapid, 1989), and has given the anti-science wing of the discipline a series of formidable philosophical arguments on which to draw. Hollis and Smith argue that one can have either an explanatory account (based on scientific principles), or an understanding account (based on hermeneutic principles); what one cannot have is some combination of the two (Hollis and Smith, 1990, 1994). In reality, Hollis and Smith's 'two stories' thesis is not wholly consistent with that of either Winch or Weber (Hollis and Smith, 1990, 1991, 1992, 1994, 1996). Winch (1958) had rejected all attempts to construct a science of the social, and Weber (1949) had insisted on the necessity of both forms of analysis.

Weber rejected both the positivist contention that the cognitive aims of the natural and the social sciences were basically the same and the opposing historicist doctrine that it is impossible to make legitimate generalizations about human bevavior because human actions are not subject to the regularities that govern the world of nature. Against the historicists Weber argued that the method of science, whether its subject matter be things or men, always proceeds by abstraction and generalization. Against the positivists, he took the view that the explanation of human behavior could not rest only on its external manifestations, but required also knowledge of the underlying motivations. Hence Weber's definition of sociology as that science which aims at the *interpretative understanding (Verstehen)* of social behavior in order to gain an explanation of its causes its course and it effects. According to Weber, what distinguishes the natural and social sciences is not an inherent difference in methods of investigation, but rather the differing interests and aims of the scientist. Both types of science involve abstraction. Hence there is no insurmountable chasm between the procedures of the natural and the social scientist; they differ only in their cognitive intentions and explanatory projects (Weber, 1949).

Weber saw the notion of interpretative understanding as only a preliminary step in the establishment of causal relationships. The grasping of subjective meaning of an activity, he argued, is facilitated through empathy *(Einfuehlung)* and a reliving *(Nacherbleben)* of the experience to be analyzed. But any interpretative explanation

(verstehende Erklaerung) must become a causal explanation if it is to reach the dignity of a scientific proposition. *Verstehen* and causal explanation are correlative rather than opposed principles of method in the social sciences (Weber, 1949).

Given the philosophical justification of the arguments of Hollis and Smith, however, the only alternative is a philosophical refutation, not simply a rejection of the position, or a creative redescription (Suganami, 2000; see Patomäki, 1996 for a philosophical engagement). This task is complicated by the fact that many of the labels currently being deployed in the discipline are not clearly delineated, or the content of them sufficiently explained (see Smith, 1995 for an account of the discipline's self images; see also Waever, 1996). In this respect, despite the appearance of philosophical sophistication, the discipline has moved from throwing philosophical hand grenades to a largely untargeted artillery barrage against an ill-defined series of enemies.

Often this phase of disciplinary development is called the 'third debate', (Dougherty and Pfaltzgraff, 1996; George, 1989; Lapid, 1989; Neufeld, 1994, 1995; Sylvester, 1993) but there are problems with such a designation. In particular, it is not clear what the content of the 'third debate' is, or who the debaters are (Smith, 1995: 14; Vasquez, 1995: 217–18; Waever 1996). Mark Neufeld, for example, claims both that the 'third debate' is the 'inter-paradigm debate' between realism, pluralism and structuralism (Neufeld, 1994: 19; see also Banks 1984, 1985), and that it represents the discipline's attempt to move beyond the positivist orthodoxy (Neufeld, 1994: 19). Christine Sylvester treats it as simply the move beyond positivism (Slyvester, 1993: 140–68). Ole Waever provides a solid critique of the confusion surrounding the 'third debate' (Waever, 1996).

The dominant way the discipline views this period is in terms of a vehement set of reactions to a scientific IR; or what has been called a post-positivist phase (Biersteker, 1989; Holsti, 1989; Lapid, 1989). Many of the current meta-theoretical debates are primarily concerned with the extent to which the positivist model of science can, or should, be applied to IR (Hollis and Smith, 1990; King, et al., 1994; Kratochwil, 2000; Nicholson, 1996a; Smith, 2000; Wendt, 2000). And all of the contributors to the current meta-theoretical debates have addressed the nature of inquiry itself, as opposed to the nature of the international system, or some other chosen object of inquiry (Ashley, 1987; Biersteker, 1989; Hollis and Smith, 1990; Holsti, 1989; Lapid, 1989; Nicholson, 1996a, 1996b). However, as Yosef Lapid suggests, this period is not simply a continuation of debates about the relevance of the philosophy of science to IR, but is also the 'confluence of diverse antipositivistic philosophical

and sociological trends' (Biersteker, 1989; Holsti, 1989; Lapid, 1989: 237). For the purposes of this last section I will label this the 'post-positivist turn' and attempt to indicate the contemporary landscape of IR, highlight some of the problems, and indicate some potential avenues of future research.

The post-positivist turn began in the mid-1980s. Just as Kuhn was becoming well embedded within the literature a number of other developments were being imported into IR. Often these interventions would include references to Kuhn and Feyerabend as ways of delegitimating claims to science (George, 1989: 271; Neufeld, 1994: 14); with defenders of science tending to draw on Kuhn, Popper or Lakatos (Dougherty and Pfaltzgraff, 1996: 5; Herman and Peacock, 1987; Keohane, 1989; King et al., 1994; Nicholson, 1996a; Vasquez, 1998). But the philosophy of science no longer provided the only fertile ground for sources of legitimation. Moreover, the overturning of the positivist orthodoxy within the philosophy of science now meant that there was no 'secure' account of a scientific methodology on which to draw (Chalmers, 1992; Hollis and Smith, 1990; Oldroyd, 1986; Stockman, 1983; Trigg, 1993; Tudor, 1982). This meant that a range of disparate positions was now being imported into the discipline, with the relationships between them being unclear and unspecified.

Critical theorists criticized mainstream commitments to science (Cox, 1981; Hoffman, 1987; Linklater, 1990; see also Habermas, 1988; Horkheimer, 1982, 1993; Morrow and Brown, 1994). The extent of this critique, however, is not clear. For some, critical theory is seen as a replacement for a positivist form of social science (Brown, 1994; S. Smith, 1996: 24). Yet, as Mark Hoffman points out, critical theory did not denigrate positivism, but rather aimed to show how scientific knowledge aimed at mere technical control was not the only legitimate type of knowledge (Hoffman, 1987: 236; see also Adorno et al., 1976). Certainly, Habermas viewed positivist, hermeneutic and critical research as legitimate components of all social inquiry (Habermas, 1988). Likewise, Andrew Linklater seems to accept the validity of positivist informed research, whilst rejecting the idea that it exhausts the possibilities (Linklater, 1990). Positivism as a valid philosophy of science is accepted and only the boundaries of its legitimate use within social science are disputed. As such, a critical theory approach to social science will incorporate elements of positivism as well as hermeneutics, but attempt to go beyond them in terms of emancipatory potential (Morrow and Brown, 1994).

Feminist approaches in IR, as in other social science disciplines, critiqued science on the basis of its male-centered assumptions and lack of attention to gendered forms of knowledge construction

(Elshtain, 1997; Enloe, 1990, 1993; Sylvester, 1993; Tickner, 1992; Zalewski, 1993). However, while many seem happy to view feminism as a project dedicated to the critique of something called the 'positivist mainstream', there is within feminist approaches very little in the way of agreement about appropriate standards of inquiry within feminism (Zalewski, 1993; see also Tickner, Chapter 14 in this volume). Some feminists view their work in terms of science, even if they would not accept the label positivist (Enloe, 1990; Harding, 1991; Hartsock, 1983). In general, the discipline, following Sandra Harding's framework, tends to divide feminists into empiricist, standpoint and postmodern positions (Zalewski, 1993), although it is doubtful whether this characterization comes close to engaging with the nuances of this important body of work (Harding, 1991).

Often described as the most radical attack on the assumptions of social science, postmodernism and post-structuralism are difficult bodies of thought to characterize (Ashley, 1987, 1989; Ashley and Walker, 1990; Campbell, 1998a; Der Derian and Shapiro, 1989; Devetak, 1996; George, 1994; Jarvis, 2000; Rosenau, 1990, 1992; Smith, 1995; Walker, 1993). Also, the discipline seems unable, or unwilling, to attempt to make any differentiation between postmodernism and post-structuralism, and tends to treat the two terms as synonymous (Rosenau, 1990: 84–5; Vasquez, 1995). This is problematic in terms of the philosophy of social science.

Post-structuralism emerges out of a general critique of structuralism (Harland, 1987). It is critical of structuralism's attempt to develop an objective science of social structures, but equally important is that post-structuralism expresses no desire to return to a form of inquiry based upon the subjectivity of agents (Harland, 1987, 1993; Rabinow, 1982; Rosenau,1990). Structural forms of inquiry had come to dominate many forms of social science (Althusser and Balibar, 1970; [1938] Durkheim, 1964; Harland, 1987, 1993). Structuralism proposes that understanding social practices requires the decentering of individual subjectivities and a focusing of attention on the structural modalities and organizing principles within which social practices are framed (Harland, 1987, 1993; Kurzweil, 1980). Structuralism was an attempt to scientifically describe the structural principles under which activity could be explained (Harland, 1993; Jackson, 1991). Waltz's structural realism, although not specifically embedded with a structuralist meta-theory, can be understood as a structuralist theory of IR (Waltz, 1979; see Ashley, 1984 for a critique of Waltz that makes this explicit).

Post-structuralism departs from two central tenets of structuralism (Harland, 1987, 1993). First, the logic of structures, which structuralism had thought was clear and determinate, is challenged

(Derrida, 1988). For post-structuralism, structures do not operate according to one organizing principle or logic (Harland, 1987). Indeed, for post-structuralism there is no underlying logic to structures and hence there is structural indeterminancy (Doty, 1997; Harland, 1987; see Wight, 1999b for a critique). Social outcomes, which are products of social structures, are also indeterminate (Doty, 1997). Attempts to ascribe a logic to social activity must necessarily either fail or impose a logic on the situation through claims to some form of legitimacy – generally science (Derrida, 1988).

But science, as a social practice dependent upon structures, also falls to the same logic, and its outputs are either indeterminate, or such determinacy that does emerge can only be the outcome of practices that attempt to tame the indeterminacy of structures (Ashley, 1987, 1989; Ashley and Walker 1990). This means that all claims to scientific objectivity are actually social practices imposing order through practices of power (Ashley, 1987, 1989; George, 1994; Walker, 1993). Postmodernism expands on this post-structuralist position and grafts onto it various other wholesale critiques of reason, reality, truth and so forth (Brodribb, 1992; Callinicos, 1990; Dews, 1987; Eagleton, 1996; Farrell, 1996; Nicholson, 1993; Owen, 1997; in IR see, Brown, 1994; Devetak, 1996; Jarvis, 2000; Rengger and Hoffman, 1990; Vasquez, 1995).

The fourth source of influences and ideas that began to be imported is that of social theory. This position has been labelled constructivism within the discipline (Adler, 1997; Guzzini, 2000; Hopf, 1998; Kratochwil, 1989; Onuf, 1989, 1998; Ruggie, 1998; Vasquez, 1997a; Wendt, 1987). This is a very problematic term because there are some very conflicting positions being imported under this label (Adler, 1997; Hopf, 1998; see also Chapter 5 in this volume; Ruggie, 1998). The confusion is evident when one considers that John Ruggie, in his typology of constructivism, includes post-structuralism (Ruggie, 1998: 35; see also Adler, Chapter 5 in this volume), whereas Smith sees a clear demarcation between them (Smith, 1995, 1996, 1997). David Campbell likewise sees certain forms of constructivism as inimical to his version of post-structuralism (Campbell, 1998a, 2001).

The philosophy of social science can help throw some light on this situation. In relation to the science question, Ruggie's neoclassical constructivism and Alexander Wendt's scientific realist version are united; both are committed to the idea of social science (Ruggie, 1998: 35–6).[16] Friedrich Kratochwil, on the other hand, is much closer to Winch's anti-science perspective (Kratochwil, 1989, 2000). Why Ruggie draws such a firm distinction between his neoclassical constructivism and Wendt's more naturalistic form is not immediately clear. Ruggie sees the work of philosopher

John Searle as playing a fundamental role in his neoclassical constructivism (Ruggie, 1998: 35). Yet, there is very little in Searle's *The Construction of Social Reality* (1995) that Wendt and other scientific realists would find objectionable (compare Bhaskar, 1979; Outhwaite, 1987).

Indeed, Searle begins with a statement that could function as a *leitmotif* for scientific realism: 'We live in exactly one world, not two or three or seventeen. As far as we currently know, the most fundamental features of that world are as described by physics, chemistry, and the other natural sciences' (Searle, 1995: xi). Moreover, Searle openly declares his hand with both philosophical realism and science (Searle, 1995: xiii). Equally, Weber's attempt to combine *eklaren* and *verstehen* into one seamless account is exactly the project that scientific realists, such as Roy Bhaskar, are engaged in (Bhaskar, 1979; Weber, 1949). Indeed, Ruggie actually accepts 'relational social realism' as an accurate description of his account, of international structure (Ruggie, 1998: 34; for a scientific realist account, see Porpora, 1987).

This raises the question of just why Ruggie feels it so necessary to distinguish his neoclassical constructivism from that of Wendt. The answer, of course, is the label 'scientific', in scientific realism. Kratochwil also objects to Wendt's constructivism on similar grounds (Kratochwil, 2000). Ruggie's depth of engagement with scientific realism, however, does not seem to extend any further than an almost verbatim restatement of Hollis and Smith's rejection of it (Hollis and Smith, 1991; Ruggie, 1998: 36). And Hollis and Smith can hardly be said to have provided a sustained assessment of it (Hollis and Smith, 1991; S. Smith, 1996). As a philosophy of science that is non-positivist, scientific realism is very poorly understood within the discipline and this is certainly one area where much research is still required.

Wendt (1987, 1999) and David Dessler (1989, 1991, 1999) provide good introductions to scientific realism (see also Shapiro and Wendt, 1992). Ashley J. Tellis (1996) writes of something called 'scientific realism' and aligns it with Karl Popper's 'critical rationalism'. It seems unlikely, however, that by 'scientific realism' Tellis means the philosophy of science version of it, and his scientific realism can only be political realism that attempts to be scientific. None the less, precisely because the labels are deployed with little clarification, confusion abounds. Kratochwil provides a recent attempt to address scientific realism, but ultimately his treatment lacks, an understandable, depth of analysis (Kratochwil, 2000; see also Doty, 1997, and the critique by Wight, 1999a, and the subsequent exchange: Doty, 1999; Wight, 2000). Heikki Patomäki and Colin Wight have begun what might be a closer examination of scientific realism, although the tenacity of the view

that science equals positivism is a serious obstacle to any serious evaluation of alternative views of science (Patomäki and Wight, 2000; see also Patomäki, 1996, 2001; Lane, 1996; Wendt, 1999).

Smith calls scientific realism an epistemology, which is a strange reading given that scientific realism is a philosophy of science that does not privilege any particular epistemological stance (S. Smith, 1996). The problem here is the use of the term epistemology within the discipline. Smith, for example, talks of something called a 'postmodern epistemology', and of postmodern work on epistemology being diverse (Smith, 1996). But this can only be to misuse the word epistemology, since epistemology is the branch of philosophy concerned with the theory of knowledge and not a philosophy of science; or an account of the reality (Haack, 1993; Taylor, 1987). In fact, very few books on epistemology include references to positivism (Haack, 1993; Taylor, 1987).

The main problems with which epistemology is concerned include: the definition of knowledge and related concepts; the sources and criteria of knowledge; the kinds of knowledge possible and the degree to which each is certain and the exact relation between the one who knows and the object known (Haack, 1993; Taylor, 1987). Epistemological questions are typically concerned with the grounds we have for accepting or rejecting beliefs. Insofar as many postmodern positions reject these as valid questions they also reject epistemology; which is evident in Smith's own table, since he indicates 'no' in every category pertaining to postmodern positions on criteria of assessment (S. Smith, 1996).

In short, postmodernism as yet has no epistemology, and is unwilling to advance one (see the debate between Campbell, 1998b, 1999 and Wight, 1999b; and between Doty, 1999 and Wight 2000; also Osterud, 1996, 1997; Patomäki, 1997; Smith, 1997). It is for this reason that Peter Katzenstein, Robert Keohane and Stephen Krasner argue that it falls outside the social science enterprise (Katzenstein et al., 1998: 678; Sørenson, 1998: 88). Equally, however, Smith locates Michel Foucault as representative of postmodernism, which would seem to imply he had no criteria of assessment, whereas Foucault declared himself an empiricist (Foucault, 1990: 106). No doubt he was being 'ironic'! Unfortunately the discipline tends to use epistemology to mean any generalized approach to study. But this only serves to hide a range of hidden ontological assumptions.

A key factor that the discipline has yet to take seriously is that the demise of the positivist orthodoxy within the philosophy of science now means that there is 'no definitive or agreed cannon of scientific explanation' (Hollis and Smith, 1990: 67). This means that science is not synonymous with positivism. This should have been the lesson drawn
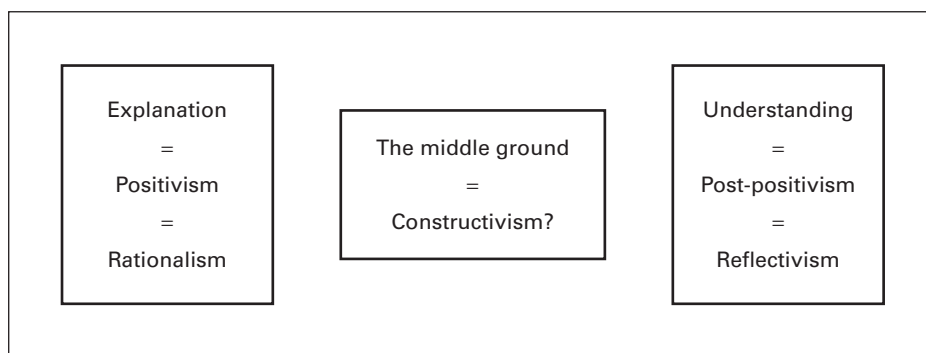
Figure 2.2   *Contemporary IR*

from developments within the philosophy of science (Vasquez, 1995). Yet the discipline seems tenaciously wedded to the idea that science is positivism (Nicholson, 1996a, 1996b, 2000; S. Smith, 1996). Even Hollis and Smith, despite a highly sophisticated discussion of this issue, draw the line between explaining and understanding on positivist principles (Hollis and Smith, 1990). This demonstrates the problem with simplistic diagrammatic representations of complex theoretical landscapes (see Figures 2.1 and 2.2). Explaining, for Hollis and Smith, seems to suggest a unitary scientific approach, whereas recent work within the philosophy of science shows just how untenable this is (Chalmers, 1992), with many, including Paul Feyerabend and Roy Bhaskar, maintaining that there is no generalized account of the 'scientific method' that could facilitate the drawing of such a line (Bhaskar, 1978; Feyerabend, 1975).

For Bhaskar, and other scientific realists, there cannot be one scientific method, or one appropriate epistemology, because each of the sciences is concerned with differing object domains, and no one method, or epistemology, could be expected to fit all cases (Bhaskar, 1978; Mackinnon, 1972; Psillos, 1999). For scientific realists, the correct epistemological stance is one of epistemological opportunism. As Einstein put it, '[c]ompare a scientist with an epistemologist; a scientist faces a complicated situation. So in order to get some value in this situation he cannot use a simple rule, he has to be an opportunist' (see Feyerabend, 1995). Equally, given that there is no agreed cannon of scientific explanation, post-positivism should not be interpreted as anti-science.

The term 'post-positivist' is ambiguous as to whether it constitutes an outright rejection of positivism, an outright rejection of science, or a reformulation of the idea of science on the basis of new developments within the philosophy of science (Laudan, 1996). Indeed, many of the developments within the philosophy of science that deserve the label 'post-positivist' are certainly not anti-science, although they may well be anti-positivist (Bhaskar, 1978, 1986; Kuhn, 1962, 1970, 1982, 1990; Laudan, 1996). This opens up the possibility of a non-positivist, yet still scientific IR; a science of IR, that is, that does not follow positivist principles.

There is little doubt, however, that for many within the discipline a commitment to science still remains a commitment to positivism (Nicholson, 1996a, 1996b, 2000). Even Wendt, whilst advocating a scientific realist philosophy of science, can declare, 'I am a strong believer in science … I am a "positivist"' (Wendt, 1999: 39). This is an impossible position to hold. One cannot be both a scientific realist and a positivist; the two accounts of science are diametrically opposed on some very fundamental issues (Bhaskar, 1978; Feyerabend, 1981; Hollis, 1996; Mackinnon, 1972; S. Smith, 1996).[17] Positivism, in this sense, has lost all meaning. Indeed, the discipline's understanding of positivism seems a caricature of what is a very sophisticated, although in my opinion highly flawed, philosophy of science.

This confusion surrounding the meaning of positivism threatens to destabilize any attempt to employ it (Nicholson, 1996a, 2000; S. Smith, 1996). And if positivism cannot be given coherent content, then post-positivism is equally meaningless. Many seem to equate positivism with realist (in the philosophical sense) accounts of science (Campbell, 2001; George, 1994); or treat it as meaning any approach that relies on a belief in a 'world out there' – a form of philosophical realism (Campbell, 2001; George, 1994). However, Hollis argues that positivism, insofar as it is committed to an empiricist epistemology, is actually an anti-realist (in the philosophical sense) philosophy (Hollis, 1996: 303; George also admits this, 1994: 53).

There have been some serious attempts to clarify the content of positivism in the discipline (compare

George, 1994; Hollis, 1996; Nicholson 1996a, 1996b, 2000), but it is doubtful, given the disciplinary baggage surrounding the label, if there is anything to be gained from its continued deployment (Nicholson, 1996a, 1996b, 2000). Smith provides a good account but one that omits many of the most fundamental issues – particularly positivism's commitment to a Humean account of cause; its anti-realism and associated phenomenalism and instrumentalism; and the covering law model of explanation (S. Smith, 1996; see Kowlakowski, 1969, for a more in-depth account of positivism).

More problematic is that Smith's (1996) own positivistic (on his own terms) attempt to spell out four essential characteristics of positivism simply begs the question of how many of the chosen principles a given theorist need commit to before being deserving of the label?[18] Is it a case of 'three strikes and you're out', or are you a positivist if you just accept one of them? Nicholson (1996a, 1996b) also produces a good account, but it suffers from the conflation of positivism with empiricism (see Smith, 1996, for a critique of this conflation). Hollis makes an often-missed point in his claim that all positivists are naturalists, but not all naturalists are positivists (Hollis, 1996: 303).

All of this adds up to a very confused picture in terms of the philosophy of social science. IR has struggled to incorporate an increasingly diverse set of positions into its theoretical landscape. In general, the discipline has attempted to maintain an unsophisticated and outdated two-category framework based on the science/anti-science issue. The terminology of this framework may have changed, but ultimately contemporary disciplinary categories seem to be mirror images of a Carr's distinction between science and 'alchemy'. Currently there are three continuums that the discipline seems to consider line up in opposition to each other. The first of these is the explaining/understanding divide (Hollis and Smith, 1990). The second is the positivism/post-positivism divide (Lapid, 1989; Sylvester, 1993). The third is Keohane's distinction between rationalism and reflectivism (Keohane, 1989). The newly emerging constructivism claims the 'middle ground' in between (Adler, 1997; Price and Reus-Smit, 1998; Wendt, 1999). This constitutes a field configured as in Figure 2.2.

The problems with such a framework should be evident from the above discussion, but it is particularly worth noting the irony of aligning something called 'rationalism' with positivism. Particularly if the claims that positivism embodies an empiricist epistemology are correct (Nicholson, 1996a, 1996b; S. Smith, 1996); rationalism and empiricism are normally considered epistemological opposites[19] (Haack, 1993). Moreover, if the 'science = positivism' equation is accepted this would mean that post-positivism is necessarily anti-science. But, this cannot be the case since many post-positivist positions are pro-science. Moreover, Marxist approaches to IR sit uneasily in this framework as they are also committed to science, but not positivism (Maclean, 1981; H. Smith, 1996). There is a move within some sections of the discipline to substitute the rationalist/reflectivist axis for a rationalist/constructivist one, and this is certainly evident in many of the chapters in this volume. However, this can only make sense if the category of constructivism is further disaggregated into competing, and sometimes incompatible positions (see Chapter 5 by Adler in this volume for an attempt to construct just such an account). It is difficult to see what is gained by such a move since to use one label to cover a range of positions can only be of benefit if they share substantial elements in common.

Another complicating factor is that of causation (Harré and Madden, 1975; Lerner, 1965; Suganami, 1996; Wright, 1974). Hollis and Smith ultimately reduce the distinction between explaining and understanding, and by implication positivism and post-positivism, to the issue of causation: 'To understand is to reproduce order in the minds of actors; to explain is to find causes in the scientific manner' (Hollis and Smith, 1990: 87). This would suggest that all causal accounts are necessarily positivist. Indeed, David Campbell, in accepting the logic of this framework, argues: 'I embrace the logic of interpretation that acknowledges the improbability of cataloguing, calculating and specifying the ''real causes''' (Campbell, 1992: 4). This seems to suggest that interpretative (understanding) accounts eschew causation. But what kind of causation is being rejected here? Hollis and Smith view cause in Humean positivist terms, whereas Campbell offers no explanation of what he means by 'real causes' (Hollis and Smith, 1991: 407; 1994: 248–50).

Ruggie, presumably still on the post-positivist/reflectivist side, is committed to causation, but discusses it in the context of the covering law model of explanation and contrasts this with a narrative form of explanation (Ruggie, 1998: 34). Hidemi Suganami has also addressed the issue of cause in a very similar manner, but the ontology of his account is unclear and he seems to imply that the narration itself is the cause (Suganami, 2000). This is a very idealistic account of cause, and would seem to suggest that Thucydides's narrative of the Peloponnesian War was actually its cause (Patomäki and Wight, 2000; Suganami, 2000). Missing from Suganami's discussion is the difference between 'narration-of-causes' and 'narration-as-cause'. Both are equally valid in terms of social science, but the distinction is important in temporal terms. A narration of the causes of the First World War cannot literally

be the cause of the First World War, whereas a narrative that portrayed certain groups as inferior could be part of the cause of their being treated as inferior. Dessler (1991) has a good discussion of cause from a non-Humean position and contrasts this to correlation.[20]

The distinction between constitutive and explanatory theory is another issue that has emerged within the discipline as a result of the contemporary way of framing the issues (Burchill and Linklater, 1996; Smith, 1995; Wendt, 1999). Steve Smith sees this as the main meta-theoretical issue facing the discipline today (Smith, 1995: 26). Smith clearly sees explanatory theory as being essentially positivist in orientation and constitutive theory as post-positivist (Smith, 1995: 26–7). According to Smith, explanatory theory seeks to offer explanations of international relations, whereas constitutive theory sees 'theory as constitutive of that reality' (Smith, 1995: 26–7). It is difficult to know how to interpret this distinction. Smith formulates it as a basic ontological difference embedded within competing visions of the social world (Smith, 1995: 27). But underlying Smith's formulation is still the science/anti-science schema; is the social world to be 'seen as scientists think of the "natural" world, that is to say as something outside of our theories, or is the social world what we make it' (Smith, 1995: 27)?

But just whom does the 'we' refer to here? Setting this distinction in opposition to explanatory theory that attempts to explain international relations, we can presume that Smith means 'we' IR theorists, not 'we' members of society. But this seems implausible. It seems to suggest that 'we' IR theorists make the world of international relations. On the other hand, if the point is simply that the world is socially constructed then it would be difficult to find many social scientists, whether on the science wing or not, who think otherwise (Holsti, 1998: 29; Searle, 1995). Even such a mainstream scholar as Kenneth Waltz accepts that the social world is socially constructed (Waltz, 1979: 48).[21]

It may well be that academic theories eventually filter down into society and fundamentally change it, but as yet, there is little to suggest that 'we' are in a privileged enough position to say 'we' IR theorists make the world we study. Wendt's reply to Smith on this issue seems basically sound, and even though social objects do not exist independently of the concepts agents have of them, they do exist 'independent of the minds and bodies of the individuals who want to explain them' (Wendt, 1999: 75). Wendt rejects Smith's science/anti-science framing of this issue, and argues that both explanatory theory and constitutive theory transcend the natural–social science divide (Wendt, 1999: 78; see Smith, 2000 for a reply). According to Wendt, constitutive theory is concerned with 'how' social objects are constituted, and what is 'X' (Wendt, 1999: 78). State theory would be a good example here. It asks 'what is a state?' and does not attempt to link causes in time (Bosanquet, 1899; Jessop, 1990; Laski, 1935). Wendt also argues that some of the most important theories in the natural sciences are constitutive – the double helix model of DNA for example (Wendt, 2000: 107).

The issue of constitutive theory and explanatory theory is often linked to that of whether reasons can be causes (Hollis, 1994; Smith, 2000). This used to be a major issue of concern for the philosophy of social science (Winch, 1958, although compare Winch, 1990; Davidson, 1963; MacIntyre, 1973). Today the construal of reasons as causes is generally accepted as a necessary component for interpretative accounts; although Smith suggests that it is still hotly disputed, but does not indicate by whom (Smith, 2000: 158). In general, understanding reasons as causes has come to be seen as necessary in order to preserve the difference between action and behavior (Bhaskar, 1979; Carlsnaes, 1986; Collin, 1985; Davidson, 1963; Porpora, 1987).

For if the reason for an act is not part of the causal complex responsible for the act, then the contrast drawn between an act and a bodily movement, upon which hermeneutic accounts insist, is negated; such as that between signalling to a friend or scratching one's head, for example (Bhaskar, 1979: 169–95). The difference between a waving arm and signalling to a friend depends upon the possession, by an agent, of a *reason* to wave one's arm in that manner, namely, the desire to signal to a friend. In this respect, the desire to wave to one's friend can rightly be considered as part of the causal complex responsible for the waving of the arm in the appropriate manner (Carlsnaes, 1986; Patomäki, 1996). If reasons are stripped of their causal function, behavioralism beckons.

This issue again demonstrates the tenacity of the positivist vision of science, for Smith's rejection of reasons as causes is derived from his acceptance of a positivist account of cause. Winch accepted that his rejection of causal accounts in social explanations was based on a Humean/positivist account of cause, and that devoid of such an account causal talk was not only appropriate, but necessary for social explanation (Winch, 1990). Because of this Wendt has suggested that Hollis and Smith's 'two stories' thesis is 'a legacy of positivist conceptions of explanation' (Wendt, 1991: 391).

The explanatory/constitutive divide is linked to the rationalist/reflectivist dichotomy by a number of authors (Adler, 1997; Laffey and Weldes, 1997; S. Smith, 1996; Wendt, 1999). The division of the discipline into rationalist and reflectivist camps is generally attributed to Robert Keohane (Keohane, 1989), although in recent years it has played less of a role, with many within the discipline preferring

to talk of a rationalist/constructivist divide. The original distinction was specifically formulated by Keohane to capture the difference between two approaches to international institutions, but the terms have rapidly come to signify two radically opposed approaches to the study of IR itself (Keohane, 1989; S. Smith, 1996; Wendt 1992). According to Keohane, rationalists are theorists who accept what he calls a 'substantive' conception of rationality. By this he means that behavior can be considered rational insofar as it can be adjudged objectively to be optimally adapted to the situation (Keohane, 1989: 160). Reflectivists, on the other hand, take a 'sociological approach to the study of institutions' and stress the 'role of impersonal social forces as well as the impact of cultural practices, norms, and values that are not derived from a calculation of interests' (Keohane, 1989: 160). Reflectivists emphasize 'the importance of "intersubjective meanings" of international institutional activity' (Keohane, 1989: 161).

As formulated, this is an ontological difference, not an epistemological or methodological one. Keohane claims that the study of international politics will require both approaches if empirical research is not to suffer (Keohane, 1989: 161). Keohane's rationalist/reflectivist distinction can be understood as one in which rationalists focus their attention on how institutions function; whereas reflectivists are more interested in how institutions come into existence, how they are maintained and how they vary across cultural and historical contexts (Keohane, 1989: 170). According to the reflectivist critique, rationalist theories are said to be one-dimensional, static, universalistic, ahistorical and decontextualized (Keohane, 1989: 170–3). Keohane acknowledges all of these limitations, yet argues against a wholesale rejection of rationalist approaches in favour of a broadening of the research agenda to incorporate the reflectivist perspective (Keohane, 1989: 171). The problem is that, although these reflectivist critiques of the rationalist perspective seem telling, the reflectivists have yet to develop what Keohane calls a 'research program' able to demonstrate the veracity of their claims (Keohane, 1989: 173). Without such a 'research program' reflectivist criticisms of the rationalist mainstream will remain marginal to the discipline (Keohane, 1989: 173).

In essence, Keohane's Lakatosian call for a 'research programme or perish' intervention can be understood as a plea, or perhaps challenge, to reflectivist scholars to move beyond incessant critique and to demonstrate empirically the validity of their claims (Katzenstein et al., 1998). The reflectivist response has, predictably enough, been to ask on whose terms (S. Smith, 1996)? After all, isn't the demand to develop a 'research programme' based upon empirical validation an appeal to exactly those same positivist principles that the reflectivists are challenging (George, 1994)? To many reflectivists still wedded to an outmoded view of science this is to accept positivism. It is in this manner that positivism comes to be aligned with rationalism.

There is something to this alignment at the level of ontology. Positivism, in all its varied manifestations, has always been ontologically coy, preferring to either remain agnostic about the ontological status of theoretical terms, or denying outright that they have any ontological status. This, of course, is its instrumental treatment of theoretical terms. Keohane's rationalists do not believe that any actual agents meet the rational man model; any more than economists think that any firms are perfectly rational utility maximizers (Katzenstein et al., 1998; Keohane, 1989). Rationality is an assumption deemed necessary in order to get research under way. Reflectivist critics can be interpreted as either rejecting the validity of the 'as if' (assumptive) mode of theorizing, or merely rejecting the particular assumptions being made; or perhaps both.

Whereas Keohane originally based the distinction on ontological grounds and accepted the need to broaden the ontological horizon of investigation, the reflectivist reaction to it is based upon the epistemological criteria that Keohane sees as nonnegotiable (Keohane, 1989: 174; Katzenstein et al., 1998). That the reflectivist reaction to Keohane's position has been primarily based upon epistemological issues demonstrates the depth of the science/anti-science split within the discipline. Moreover, the fact that the vast majority (if not all) of so-called reflectivists within the discipline do indeed supply empirical support for their claims throws yet more doubt on the validity of this particular cleavage (Campbell, 2001; Wendt, 1999: 67; 2000: 173). If the distinction between a rationalist and a reflectivist is made on these epistemological grounds alone then there are simply no practicing reflectivists in IR today. Even the severest critics of Keohane's epistemological concerns enlist empirical support for their arguments (Ashley, 1987, 1989; Ashley and Walker, 1990; Campbell, 1998b, 2001; George, 1994; Smith, 1997; Walker, 1993).

There is one final dichotomy that demonstrates the inability of this crude framework to contain the weight it is being asked to bear. This is the material/ideational split. There is little constructive to be said about the way the discipline currently frames this issue. From a philosophy of social science perspective it makes little sense. Rationalists, explainers and positivists are said to concentrate on material factors; reflectivists, understanders, constructivists and post-positivists are said to focus on ideational ones (Laffey and Weldes, 1997; Ruggie, 1998; S. Smith, 1996, 2000; Wendt, 1995, 1999, 2000).

This issue again is derivate of the science/ anti-science split. But there is simply no philosophy of science position that can legitimate this split. Positivists of all sorts of persuasion can legitimate analysis of ideational factors; it is how they treat them that matters (Haas, 1991: 190; Laffey and Weldes, 1997). Likewise, non-positivist philosophies of science and social science can privilege material factors (Marx, 1966). Of course, different theorists can focus their attention of these factors to varying degrees, but even in these instances this would be an ontological choice related to the object of inquiry, not one derived from an a priori commitment to some mythical epistemological position. If the difference between rationalists and reflectivists, or positivists and post-positivists, or even constructivists and rationalists, is based on the material versus ideational issue, then Keohane, given his claim that 'institutions can be defined in terms of their rules', is not a rationalist or a positivist (Keohane, 1989: 163).

Many on the so-called non-rationalist/post-positivist side of the current landscape seem to assume that Wendt's argument for maintaining a social science embedded within nature suggests that *only* material factors matter (Campbell, 2001: 445). But Wendt is not suggesting this (1999). What he is suggesting is that IR should leave open the possibility that material factors play a role; why this should be interpreted as saying that *only* material factors matter is not clear, although understanding the logic of the contemporary framework partially explains it since the framework sets up an either/or distinction. Ultimately, however, this issue is not helped by the lack of conceptual clarity that is deployed when discussing it. David Campbell, for example, can both claim that 'nothing exists outside of discourse' (Campbell, 2001: 444), and that the 'undeniable existence of that world external to thought is not the issue' (Campbell, 2001: 444).

There may, of course, be coherent ways in which these two claims can be reconciled, but this would require much greater conceptual clarity. Moreover, despite the commitment to objects external to thought, Campbell is still essentially advocating a form of philosophical idealism in tying the existence or those objects to discourses; without humans no discourses; without discourses no objects; in a sense a version of positivism. To say more on the material/ideational issue within IR would confer on it a legitimacy that it clearly does not deserve. It does, however, demonstrate how the current way of framing the issues throws up such absurdities.

CONCLUSION

Mervyn Frost once declared IR the 'backward discipline' (Frost, 1986). It was 'backward', he argued,

due to a lack of self-conscious reflection concerning its analytical and research endeavors (Frost, 1986: 39). On these grounds IR can hardly be considered 'backward' today. However, it would be a mistake to consider that self-reflection necessarily constitutes progress. It may be that Holsti's characterization of the discipline as dividing is a more accurate description (Holsti, 1985). And even then there is the difficult question of where the dividing lines are and whether division is something the discipline desires? When positivism dominated the philosophy of science the choice for the discipline was simple, but stark. Either science, or not science; which effectively translated into 'positivism or perish'. When the positivist orthodoxy began to crumble hopes were high for a more pluralistic IR: one less grounded in austere visions of a deterministic science and one much more amenable to the introduction of alternative patterns of thought. Is this where we are today?

Unfortunately not. Unable to shake the positivist orthodoxy because it never really understood it, the discipline simply poured the newly emerging patterns of thought into the old framework. But, as any mathematician could testify, a 'thousand theoretical flowers' into two will not go, and hence the current framework bursts at the seams. Simply adding a new 'middle ground' category does not help and nor does subsuming a range of differing categories under one label. And so the current framework 'disciplines' and demands that one declares one's allegiance. Once declared, one's analytical frame of reference is specified and one's identity firmly fixed. As a rationalist you *will* privilege material factors, causation and science; as a post-positivist/reflectivist you *will* privilege ideational factors, deny causation and are anti-science. Any attempt to challenge this categorization is tamed and forced into one or other extreme. This is exactly the reaction from both sides of the divide to Wendt's attempt to occupy the middle ground. The idea that one has to declare which tribe one belongs to and that this determines one's ontological frame of reference, epistemology and appropriate methods seems a bizarre way for a discipline to proceed. However, some within the discipline have begun to question the validity of the framework itself (Ashley, 1996; Patomäki and Wight, 2000; Sørenson, 1998; Waever, 1996).

These objections notwithstanding, and given the long history of the discipline's attachment to this framework, its rejection looks unlikely. Part of the explanation for this deeply embedded attachment is surely a form of disciplinary identity politics that stakes out borders over which only the foolhardy might tread (Campbell, 1998a, 2001). After all, without borders what would the border police do? If this is the result of the philosophy of social science in IR, then perhaps the discipline can do without it. But such an assessment would miss the point. The

philosophy of social science is not something the discipline can use or discard in that manner. The subject we study is not wholly empirical, hence philosophy constitutes part of what we study, part of what we are and helps inform what we do. In this case, perhaps the best we can hope is that we can do it better. In the final analysis, it is worth keeping in mind that meta-theoretical debate on the issues I have covered in this chapter tend to be much more tribalistic in language than in practice. When it comes to concrete empirical research it is doubtful if anyone could consistently occupy any one of the positions and still maintain coherence. Hopefully the following chapters in this volume will demonstrate the veracity of this claim.

Notes

1 Throughout this chapter the abbreviation IR refers to the institutionalized academic discipline of international relations.

2 It would be normal to indicate the contested nature of this label by enclosing it in 'inverted commas'. Given that this chapter is centrally concerned with the meaning of the term such a form of enclosure seems unnecessary.

3 The problem of 'naturalism' is concerned with the extent to which society can be studied in the same way as nature (Bhaskar, 1979: 1).

4 The success of modern science led to the emergence of the philosophy of science. The philosophy of science reflects on the practice of science and attempts to examine what is distinctive about scientific explanations and theoretical constructions; what marks science off from guesswork, speculation and pseudo-science; what makes the predictions of science worthy of confidence; and, to question whether science reveals a hidden truth about an objective reality. In short, the philosophy of science attempts to grasp the nature of science. The philosophy of social science attempts to grasp the nature of social science. Both attempt to give a generalized account of what might constitute the practice subsumed under the label. It should be noted, of course, that given the success of science, philosophies of science are not simply explanatory schemes, but represent normative claims. A philosophy of science that claims to grasp the nature of scientific practice implies that if you want to practice science you 'ought' to follow the principles explicated in the philosophy. Equally, it should be clear that any philosophy of science will include ontological claims (claims about existence); epistemological considerations (claims about what would constitute a valid knowledge claim, and the grounds for such claims); and methodological implications (if you believe in X (ontology) and wish to ground the claim re X in Y (epistemology) then you should follow method Y). It is for this reason that a philosophy of science is much more than an epistemology or methodology. There are no ontologically neutral philosophies of science.

5 Again, subsumed under this question are a range of issues relating to the nature of the entities; for example, what is a 'person'; the collective action problem; the nature of social structures and so on.

6 Although this debate was labelled the agent–structure debate, it has been argued that this was simply a different terminology for what used to be called the individual/society problem, or the macro/micro problem. However, although these problems are related there are good grounds for considering them as distinct problems (see Layder, 1994).

7 Figure 2.1 is said to represent four possible positions that can be taken when the problem of naturalism is combined with the agent–structure problem. The top left box, where explanation meets structure, can be understood as a scientific approach to social study that concentrates its attention on structural forces. The bottom left box (explanation and agents), a scientific approach focussing on agents. The boxes on the right-hand side of the diagram represent a non-scientific approach to social study (hermeneutics perhaps), which, of course, can either focus on structural factors (top right) or agential ones (bottom right).

8 I view positivism as a philosophy of science. As such, it is only one account of what constitutes science. There are many other accounts of science that reject many of the central tenets of positivism. As should be clear, one of the main aims of this chapter is to problematize the idea that positions such as positivism can be given a clear and unequivocal meaning. There are many versions of positivism and much that divides those who claim to be positivists. However, these caveats aside, positivism can be characterized in the following manner. (i) Phenomenalism: the doctrine that holds that we cannot get beyond the way things appear to us and thereby obtain reliable knowledge of reality – in other words, appearances, not realities, are the only objects of knowledge. (ii) Nominalism: the doctrine that there is no objective meaning to the words we use – words and concepts do not pick out any actual objects or universal aspects of reality, they are simply conventional symbols or names that we happen to use for our own convenience. (iii) Cognitivism: the doctrine that holds that no cognitive value can be ascribed to value judgements and normative statements. (iv) Naturalism: the belief that there is an essential unity of scientific method such that the social sciences can be studied in the same manner as natural science (see Kolakowski, 1969). From these philosophical assumptions most positivists adhere to the following beliefs about the practice of science. (1) The acceptance of the 'covering-law' model of explanation (often referred to as the D–N model). An explanation is only valid if it invokes a law which covers, in the sense of entailing, all cases of the phenomena to be explained. (2) An instrumentalist treatment of theoretical terms. Theoretical terms do not refer to real entities, but such entities are to be understood 'as if' they existed in order to explain the phenomena. There is, however, no epistemological warrant (grounds for belief) that such entities really exist. The proper way to evaluate theoretical concepts and propositions is not through the categories of truth and falsity but through judging their

effectiveness. (3) A commitment to the Humean account of cause. To say that event *a* necessitated event *b* need be to say no more than when *a* occurred, so did *b*. This leads to causal laws being interpreted as 'constant conjunctions'. (4) A commitment to operationalism, which entails that the concepts of science be operationalized – that they be defined by, and their meaning limited to, the concrete operations used in their measurement. For example, the meaning of a mental term is *exhausted* by the observable operations that determine its use. So '*P* is thirsty' means *P* says she is thirsty if asked, drinks water if given the chance, and so on.

9 My analysis is an Anglo-American perspective on the issues, and it might be argued that Continental European IR would address the issues in a different manner. However, many of the anti-science positions that I address in this chapter take their inspiration from German idealism, and in this respect, one could argue that the underlying issues are the same even if the terms of debate might differ (see Jørgenson, 2000).

10 The original title of the text was *Principles of a New Science Concerning the Common Nature of Nations*.

11 Adopted from political science, behavioralism in IR is a strictly behavioral approach in which explanations are based on agents' overt, expressed and observable behavior; on 'what is really going on' rather than on non-measurable values and motives. Behavioralists emphasize that theories should be 'operational'; that is, capable of being empirically tested.

12 It is important to maintain the distinctions between ontology, epistemology and methodology. *Ontology*, in philosophical terms, was originally understood as a branch of metaphysics; it is the science of being in general, embracing such issues as the nature of existence and the categorial structure of reality. In the philosophy of science and the philosophy of social science, it is used to refer to the set of things whose existence is claimed, or acknowledged, by a particular theory or system of thought: it is in this sense that one speaks of 'the' ontology of a theory, or of a theory having such-and-such an ontology (for example, an ontology of anarchical structures, or of material substances). The term *epistemology* comes from the Greek word *epistêmê*, meaning knowledge. In simple terms, epistemology is the philosophy of knowledge or of how we come to know. *Methodology* is also concerned with how we come to know, but is much more practical in nature. Methodology is focused on the specific ways – the methods – that we can use to try to understand our world better. Epistemology and methodology are intimately related: the former involves the *philosophy* of how we come to know the world and the latter involves the *practice*. It is common in IR for these aspects to be conflated and confused. Adler (in Chapter 5 of this volume), for example, claims that '*Materialism* is the view that material reality exists, regardless of perception or interpretation, and that what we know is a faithful representation of reality out there.' It should be clear that two claims are being advanced here; two claims that do not *necessarily* follow from one another. First, there is the *ontological* claim that

'material reality exists'; second, is the *epistemological* claim that what we 'know is a faithful representation of reality'. But it is important to see that a materialist might accept the first ontological claim, without necessarily accepting the second epistemological claim. Materialism is a theory of existence (an ontological claim) and the epistemological claim is either superfluous, or will require further support. However, I doubt that anyone within IR would argue that what we 'know is a faithful representation of reality'.

13 Logical positivism, sometimes also known as logical empiricism scientific empiricism and consistent empiricism, was a school of philosophy founded in Vienna during the 1920s by a group of scientists, mathematicians and philosophers known as the Vienna Circle. Among its most prominent members were Moritz Schlick, Rudolf Carnap and Kurt Godel. They derived much of their inspiration from the writings of Ernst Mach, Gottlob Frege, Bertrand Russell, Ludwig Wittgenstein and George Edward Moore. The logical positivists made a concerted effort to clarify the language of science by showing that the content of scientific theories could be reduced to truths of logic and mathematics coupled with propositions referring to sense experience. Members of the group shared a distaste for metaphysical speculation and considered metaphysical claims about reality to be meaningless. For the logical positivists only two forms of knowledge were valid; that based on reason and that based on experience. The main theses of Logical Positivism may be briefly stated as follows. (1) A proposition, or a statement, is factually meaningful only if it is verifiable. This is understood in the sense that the proposition can be judged probable from experience, not in the sense that its truth can be conclusively established by experience. (2) A proposition is verifiable only if it is either an experiential proposition or one from which some experiential proposition can be deduced in conjunction with other premises. (3) That which cannot be experienced cannot be said to exist. Theoretical entities are treated instrumentally, 'as if' they existed. (4) A proposition is formally meaningful only if it is true by virtue of the definitions of its terms – that is, tautological. (5) The laws of logic and mathematics are all tautological. (6) A proposition is literally meaningful only if it is either verifiable or tautological. (7) Since metaphysical statements are neither verifiable nor tautological, they are literally meaningless. (8) Since ethical, aesthetical and theological statements also fail to meet the same conditions, they too are cognitively meaningless – although they may possess 'emotive' meaning. (9) Since metaphysics, ethics, philosophy of religion and aesthetics are all eliminated, the only tasks of philosophy are clarification and analysis. Thus, the propositions of philosophy are linguistic, not factual, and philosophy is a department of logic; hence the label logical positivism.

14 Easton, in a claim that is a mirror image of contemporary calls for a 'return to normative theory' (Frost, 1986, 1996; Smith, 1992), argued that 'the dominance of historical and ethical theory' had excluded empirical theory from the discipline (Easton, 1953, 1965: ix).

15 Understanding why positivism came to be referred to as an epistemology is a simple task once one understands the manner in which logical positivism claimed only scientific knowledge could be considered real knowledge (a position few positivists would hold today; Nicholson, 1996a). There are two important reasons why this conflation of epistemology and positivism should be rejected. First, those working on issues related to the philosophy of social science within IR should be able to take a much more sophisticated approach, and second positivism should no more be allowed to appropriate the label knowledge (epistemology) than that of science.

16 Again, as with positivism and other such positions, there is no easy definition of scientific realism. However, within the philosophy of science scientific realism has been the dominant alternative to positivism. Hence, one way to understand scientific realism is as a non-positivist philosophy of science. As such it rejects the tenets of positivism outlined in notes 5 and 12. Scientific realism is the belief that the objects posited in scientific theories should be considered to be real and their ontological status subject to test. Scientific theories and hypotheses, even about unobservable entities, are attempts to grasp the nature of real entities and processes that are independent of our theories about them – even non-observable ones. Scientific realism does not deny that theories are dependent on minds (or languages or judgements) if only because such theories have to be expressed by minds and in languages. It accepts that we construct theoretical accounts of the world, but it denies that these theoretical accounts exhaust the world. As should be clear, scientific realism is not committed to the view that all the objects posited in theories exist. Whether or not an entity posited in a theory exists is what science tries to discover. Some theories simply get the world wrong. Its point is that, and contrary to a positivist philosophy of science, scientists, in their practices, do treat theoretical entities as real. It has a fallibilist view of knowledge, since knowledge claims constructed in scientific theories are of a realm independent of specific claims. This means that scientific realism accepts epistemological relativism; all knowledge claims are socially constructed. Moreover, given that the world is populated by a diverse range of objects that science tries to grasp, no one epistemological and/or methodological position can be privileged. This is essentially what Feyerabend meant by 'anything goes' (Feyerabend, 1975). However, since competing knowledge claims are claims about a realm of independent objects, then some claims may be better than others. This means that despite the acceptance of epistemological relativism, judgmental rationality (the possibility of rational judgement) may well be possible. Social realism refers to the assumption that social reality – social structures and related social phenomena – has an existence over and above the existence of individual members of society, and independent of our conception or perception of them. Contrary to positivists, social realists consider that the purpose of science is to provide explanatory knowledge. For the realist, there is an important distinction between explanation and prediction, a distinction which positivism conflates. Social realists believe that explanation should be the primary objective. They claim that explanation in both the natural and social sciences should entail going beyond simply demonstrating that phenomena are instances of some observed regularity, and uncovering the underlying and often-invisible mechanisms that causally connect them. Frequently, this means postulating the existence of unobservable phenomena and processes that are unfamiliar to us. Realists believe that only by doing this will it be possible to get beyond the mere 'appearance' of things to deeper forms of explanation.

17 The most important of which are: (i) the treatment of theoretical terms; (ii) the account of causation – scientific realists reject Hume's account and focus their attention on causal mechanisms rather than constant conjunctions; (iii) no epistemological position is privileged in scientific realism. In fact, the only thing scientific realism shares with positivism is a commitment to science. Where they differ, however, is what they think science entails (Psillos, 1999).

18 Adler (Chapter 5 in this volume) accepts Smith's account. However, Smith's own account is essentially positivist in his own terms; (i) Smith must believe that there are people who regularly hold such views (his own regularity principle); (ii) Smith can only be understood as asserting that his account of positivism accurately reflects something of the 'facts' of the position and these four principles are not simply a reflection of his values (the fact/value distinction); (iii) Smith supplies empirical evidence in support of his factual claims (the commitment to empirical validation); (iv) Smith applies all of these principles to a social object (positivism) (the commitment to the unity of science). Hence, Smith's account of positivism is a positivist account if his definition is correct. The point of this is not to demonstrate that positivists would reject Smith's four criteria. In fact, most positivists would accept them. But then again so would many others who would not wish to be considered positivists (including Smith himself).

19 Empiricism is the philosophical belief that all knowledge is ultimately based on experience, that is, information received through the senses. It is opposed to rationalism and denies that we have any a priori knowledge or innate ideas: we owe all our concepts to experience of the world. Rationalism is the opposite epistemological position that claims that reason rather than sense-experience is the foundation of certainty in knowledge (Aune, 1970).

20 See King et al., 1994, Nicholson, 1996a, and Patomäki, 1996 for alternative discussions of cause; see also Deutsch, 1996.

21 Waltz's acceptance that the social world is socially constructed problematizes the use of the label 'constructivist' to indicate that those falling under the label share at least one thing in common – the idea that the social world is socially constructed; if this is the key factor, then Waltz is also a constructivist – a conclusion few constructivists would be willing to accept.

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