

**(European) Integration Theory, EU Studies and the
Sociology of Knowledge**

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DRAFT – COMMENTS WELCOME

Introduction

Sooner or later, students of any subject run into quite profound questions about the nature of their discipline.¹ In particular theoreticians are confronted by a set of issues that relate to the ways in which knowledge is gathered and to the very objectives of theorising. What, for example, are the purposes of theory? What are the criteria for the establishment of rigour and the insurance that the 'science' used in any sub-field amounts to 'good science'? It is hardly news that such questions court controversy. Political science in particular has become something of a battleground between (crudely) those who advocate a singular pathway towards social scientific rigour on the one hand and self-conscious methodological pluralists on the other.² This (heavily stylised) opposition has been played out most conspicuously in the 'perestroika' movement's attack on the dominance of rational choice approaches in American political science. The compelling point made by perestroikans is that such issues are not confined to the rarefied high ground of scholarly debate amongst theoreticians. Rather they impact deeply upon – *inter alia* – the outlook of professional associations, the decisions of funding bodies, the nature of graduate education in political science and the publication strategies of leading journals. These in turn create powerful incentive structures within the discipline that most affect aspirant junior faculty. Moreover, there is the (not insignificant) concern – as expressed by one prominent observer – that political science has actually forgotten about politics (Cohn, 1999).³

One of the issues that remains largely implicit in perestroika discourse is the question of progress within disciplines. The monist position that perestroika attacks seems – in its most extreme forms at least – to deploy a particularly 'Whiggish' understanding of how political science has developed. In this account rational choice becomes the latest

¹ Earlier versions of this paper were presented to the 42nd Annual Convention of the International Studies Association, Chicago, IL, 20-24 February 2001, the Political Science and International Studies Research Seminar, University of Birmingham, 6 March 2002 and the First Pan-European Conference on European Union Politics, Bordeaux, 26-28 September 2002. For their comments on earlier iterations and general input, thanks to Shaun Breslin, Jeffrey Checkel, Thomas Diez, Andreas Gofas, Markus Jachtenfuchs, Knud Erik Jørgensen, Ian Manners, David Marsh, Alan Milward, Ulrich Sedelmeier, Philippa Sherrington, Steve Smith, William Wallace, Antje Wiener, Michael Williams and Daniel Wincott.

² I emphasise the word 'crudely'. For a more nuanced discussion of the divisions that currently (and, in the view of the author, have always) beset political science, see Grofman 1997.

³ For interesting discussions of the perestroika movement, see Dryzek, 2002; Lubomudrov, 2002; Mearsheimer, 2001 and <http://chronicle.com/colloquy/live/2001/09/perestroika>. Strong articulations of the perestroika position include Kaska n.d.

stage in the achievement of 'better' political science. This positivistic orientation sees the project of political science as the development of tools and modes of reasoning that bring us ever closer to a truthful explanation of the world that we study. This involves the setting of (positivistic) standards in social science. These include, as is well-known, the creation of benchmarks concerning research design, the accumulation of data, the formulation of hypotheses, as well as the establishment of the central criteria of falsifiability and replicability (King, Keohane and Verba, 1994). This brings with it a tendency to tell the story of the field in a way that sees newer methodological tools – such as the 'mathematicisation' of the discipline – as advances on previous modes of enquiry. We are told that political enquiry is more rigorous than it once was. Often, this claim is given a Kuhnian slant so that the apparent dominance of rational choice reflects the achievement of a period of 'normal science', itself a condition for progress (according to this discourse). It should be noted that aggressive proselytisation of this sort is largely found in the circuits of American political science, and it is undoubtedly the case that movements like rational choice have much less purchase in the mainstream of political studies elsewhere (Bevir, 2001; Hay, 2002; Stoker and Marsh, 2002).

Indeed the particularity of the American political science pathologies and the perestroika backlash may have deep roots in the sociological factors that contributed to the formalisation of the social sciences in the United States in the late nineteenth century. In his magisterial discussion of the sociology of the social sciences, Peter Mancias (1987) describes the particular complex of intellectual and socio-political conditions that led to the emergence of formal, scientifically aspirant and discipline-based enquiry in the US. Present pathologies might be seen, therefore, as path dependent consequences of these conditions of foundation (see also Geyer, 2003). In the US context, the discourse of scientism became embedded – more than anywhere else – as something for social and political enquiry to aspire to. Arguably, it is only in economics that this aspirant naturalism has become properly globalised.⁴

⁴ That said, organisations such as the Post-Austistic Economics Network (<http://www.paecon.net>) offer conspicuous sites of resistance to the hegemonic conception of rigorous economic enquiry. Indeed, a very large proportion of professional academic economists working in the UK were not entered into the Economics and Econometrics unit of assessment for the 2001 Research Assessment Exercise. The

So why insert this preamble to a paper on European integration theory? The simple point is that EU studies – like any other area of enquiry – merits a ‘sociology of knowledge’ treatment. The sociology of knowledge label tends to cover issues such as (a) the study of processes of the growth and diffusion of knowledge within and beyond scientific communities; (b) the social systems of knowledge-generating communities – particularly in relation to (i) the (historically variable) organisation of disciplines and (ii) the norms that these communities develop for evaluating the validity and admissibility of work (bound up obviously with Kuhn’s philosophy of science and the broader issue of how deviant/nonconformist work is treated); (c) the relationship between the manufacture of knowledge and wider social and political institutions and (d) the relationship between knowledge, the manufacture of world-views and the perpetration of particular interests.⁵

The aim of this paper is to think through some – but by no means all – of these questions as they apply to the study of the European Union, particularly – and in this case pretty much exclusively – in political science and International Relations treatments of the EU (the term EU Studies is used here as a shorthand). Its premise revolves around a basic claim about the importance of knowledge generation as a *social* process (like any other). This means, in simple terms, that our evaluation of academic work cannot be targeted solely upon its capacity (or otherwise) to conform to the real world.

This issue of theoretical evaluation touches upon the question of how EU studies has developed as a field of enquiry. As Ole Wæver (2003) notes, the intellectual evolution of a field is often thought of as being closely tied to developments within the object of study. Thus it might be argued that the trajectory of EU studies in general its theoretical repertoire in particular is a function of the changing nature of the EU over time. So to pick out some random examples, neofunctionalism might be read as an intellectual expression of the strategies employed by European elites that were

contrast between political science and economics in the UK is that the norms that specify the highest standards of professional practice are much more embedded in the latter.

⁵ Much of this has been applied to the ‘hard’ natural sciences and some contributions (such as Thomas, 1979) suggest that social sciences are inherently pluralistic and, therefore, conform less to sociological models of the natural sciences. This may be correct, but the point is that the monist advocates described above tend to seek naturalistic standards and see normative value in a Kuhnian model of scientific development.

embodied in the Schuman Plan. Similarly, the appearance of intergovernmental critiques and the collapse of the neofunctionalist project appear to be reactions to the growing visibility of national executives and intergovernmental institutional expressions in the Community system from the mid-1960s. The increasing tendency of current literature to conceptualise the EU as a political system can be traced to the obvious salience of the EU as a supplier of authoritative outputs and the attendant complexity of the multi-actor policy process that surrounds the EU's institutions. Finally the rapid recent growth of studies of the external dimensions of European integration may seem an obvious consequence of (a) the growth of a foreign, security and defence policy agenda, (b) the emerging status of the Euro as an alternative reserve currency and (c) the widening issue base of international trade that has forced issues of European integration (such as the Common Agricultural Policy) onto the agenda of the WTO.

Following Wæver (2003), there are two variants of such a position. The first celebrates this process as a sign of disciplinary progress in which EU studies has drawn valuable lessons from its object of study though a process of intellectual 'catch-up'. From this stance, it is imperative that EU studies remains an academic expression of the 'real world' of European integration and EU governance. Therefore, approaches to the EU that no longer 'fit' their object are candidates for disposal, although there may be cases where reinstatement is merited if the tide of integration shifts back in the direction of certain perspectives.⁶ The second position is rather more critical. Here scholarship is interrogated for its potential to act as the intellectual legitimisation of particular ideologies associated with the object. A good example from EU studies is to be found in Milward and Sørensen's energetic critique of neofunctionalism, where the latter is portrayed as both (a) a Cold War theory offering an intellectual justification for US foreign policy priorities of the 1950s and (b) an attractive set of categories for the emerging supranational European elite to deploy in defence of their claims for the growth of Community-level governance capacity (Milward and Sørensen, 1993).

⁶ The most obvious example is the partial revival of neofunctionalism during the mid to late 1980s in light of the single market programme. Another example might be found in Alex Warleigh's call for a

The alternative is to think about the intellectual history of a field in terms of explanations that are primarily internal to that field. Wæver maintains that 'external explanations can sometimes ... be better at accounting for the overall directions of change [in a field], but they can never explain the form that theory takes' (2003: 5). So institutionalist approaches may appear to sit well with the broad treaty-induced pattern of EU politics, but this cannot explain why *rational choice* institutionalism (for example) has been applied so readily to the EU and why rationalist epistemologies are claimed to offer the basis for a coherent research programme that brings together the various insights of the three institutionalisms (Schneider and Aspinwall, 2001). This paper adopts this 'internalist' stance, not least because the object of study and the categories that we impose upon it are (at least in part) interpreted, constructed and defined by the field. They are not purely exogenous to the field. Steve Smith takes this point a little further by arguing that the application of 'rationalist' theory to European integration 'far from being the explanatory theory that it claims to be, instead provides a political and normative account of European integration whereby (positivist) notions of how to explain a given "reality" in fact constitute the reality of European integration' (2000a: 33).

This position also reminds us that our knowledge about the world is produced amidst broad scientific and more specific disciplinary structures, norms, practices and institutions. These in turn relate in complex ways to broader social and political practices. This requires, therefore, that we historicise and contextualise our discussions about knowledge production. To think otherwise – to reify science in other words - is simply to discount the status of academic work as a social activity. (Wagner, Wittrock and Whitley, 1991; Whitley, 1984) and leads us to rather sterile accounts of disciplinary history.

Political Science, Naturalism and EU Studies

The trajectory of EU studies is closely bound up with that of political science. If nothing else, the fact that many scholars of the EU work within the US political science community means that EU studies is – partially – exposed to the tensions described at the beginning of this paper. As discussed below, for a variety of reasons

re-evaluation of the efficacy of Mitranian functionalism in light of debates about 'flexibility' in the EU

there are diminishing returns in being identified as an area studies specialist (i.e. 'a Europeanist') in the US context. Scholarship needs to have demonstrable disciplinary added value and, given the pathologies of the parent discipline, those working on the EU have multiple incentives to produce work of a particular type.

The founding figures of integration theory were political scientists and claims about the legacies of the early theoretical jousts about European integration – more often than not cast in terms of a great debate between neofunctionalists and intergovernmentalists – usually form the starting point for contemporary theoretical interventions. Many of these interventions seek to transcend or to go beyond what is claimed to be the hackneyed or outdated opposition between intergovernmentalists and neofunctionalists. For some (notably Hix 1994, 1996, 1999), this manoeuvre seeks explicitly to settle the status of 'political science' as the parent discipline for EU studies. This in turn has two purposes. The first is the displacement of a supposed theoretical straightjacket in EU studies where International Relations paradigms have held sway within the sub-discipline. As is well known, Hix has argued that IR (as a discipline) is not capable of asking the most appropriate questions about the EU because the latter has developed into a mature political system. The key questions about the EU, he claims, are classically Lasswellian and thus best handled by political science (for critiques, see Hurrell and Menon, 1996; Rosamond, 2000: ch.7).⁷ The second claim about the value of political *science* is that such a move brings much needed rigour to EU studies. Employing the established tools of political science, runs the argument, challenges the excessively empiricist tendencies of much work on European integration. Instead of dense, primitive description, the scrupulous qualities of good political science give social scientific purpose (and thus greater direction) to the discussion of EU politics (Hix, 1996: 804). In this account the move toward naturalism lends legitimacy to EU studies and raises its status within the political sciences because it raises the quality of the field. Moreover, by making the key ontological claim about the EU as a political system 'like any other' this moves firmly embeds 'legitimate' EU studies within the political science mainstream. This in turn

(Warleigh, 2002 ch. 2).

⁷ Without wishing to rehearse old material here, I simply say that to caricature ante-Hix EU studies as dominated by IR is unsustainably crude. Moreover, to write off IR as a potential source of insight in EU studies for anything other than (perhaps) elaborating the structure of the international system within

refutes the notion that thinking beyond disciplinary orthodoxies to capture the nature of contemporary European transformations is a necessary or urgent task. Such a claim devalues any position that begins with the idea that the EU represents something unfamiliar, such as a radical experiment in post-sovereign politics, for which established disciplinary discourses lack appropriate vocabulary. Such defences of the extant disciplinary status quo are, of course, perfectly arguable. The problem with a position like this emerges when it is extended to the point where certain ontological claims and epistemological strategies are ruled inadmissible as valid 'science'.⁸

This sort of argument is increasingly commonplace in EU studies. It is worth tabling some illustrative examples. In his discussion of Moravcsik's *The Choice for Europe* (Moravcsik, 1998), James Caporaso writes that

This book helps to locate integration studies within the overall body of knowledge of international relations, comparative politics, and political science, and it takes EU studies out of its self-constructed theoretical ghetto. The result may well be that students of integration will have to take more seriously the professional literatures of international relations, comparative politics and political economy. Standards that apply in other sub-fields, for example, with regard to research design, data collection, and analysis, are more likely to extend to regional integration studies also.
(Caporaso, 1999: 161)

The claims to scientific status are also advanced by Moravcsik himself. Take this very definite – and not untypical – attempt to define what 'social science' is:

I take the view of a social scientist. Some group of scholars, I feel, needs to be responsible for analysing, correcting, and generalising our collective memory of significant events as objectively as possible ... For the social scientist, this means being, above all else, relentlessly *self critical*. The social scientist does this by employing explicit social scientific methods, that is, by clearly stating theories and hypotheses, the nature of alternative claims, and

which the EU operates simply misunderstands many of the contemporary IR tendencies, concerns and debates.

⁸ This also connects to the problems associated with the 'mainstreaming' of European integration into a series of disciplinary discourses. For more discussion, see Wallace (2000) and Manners (2002).

the nature of confirming and disconfirming evidence, all *ex ante* (Moravcsik, 1999b: 388-389, emphasis in original).

We should, he argues, be looking to generate findings that are replicable (Moravcsik, 1999a: 170) – in the sense defined by Keohane King and Verba (1994: 26-7) – that is to use methods that allow others to replicate the use of the data and trace the thread of logic that produces conclusions. This is a typical instance of the claim that we need to formalise political science of the EU to conform to a particular type.

By explicating the precise methodological, theoretical and empirical bases on which I reach conclusions, I have given potential critics a leg up. In contrast to non-replicable studies based upon inductive theory, I thereby render it far easier for historians and political scientists to challenge the objectivity and accuracy of my analysis (1999a: 170)

Note also the founding complaint of the journal, *European Union Politics*:

When the editors started to contribute to the field, integration studies often fell victim to debates between the paradigmatic approaches in International Relations. In the 1980s and early 1990s, it was still possible to launch a career on a two-by-two table which described the ‘nature’ of the European Union. Fortunately, times are changing and the number of papers which offer general theories of European integration is seemingly decreasing (Schneider, 2000: 6)

The same issue of the journal contains a review article on institutionalist approaches to the EU (Dowding, 2000), in which the author makes an explicit claim that rational choice institutionalism has acquired the status of a ‘normal science’ of EU studies.⁹ Elsewhere, in a jointly authored paper, one of the editors of *EUP* makes a similar point: ‘[n]eoinstitutionalist research has played a central role in the *professionalisation* of EU politics, and it does not seem inconceivable that the sub-field will become an exporter of new analytical tools rather than the passive importer it has been for decades’ (Schneider and Aspinwall, 2001: 177, my emphasis).¹⁰

⁹ This is one of a series of provocative interventions by this author. See in particular Dowding, 2001 and the reply by Marsh and Smith, 2001.

¹⁰ For now it is worth noting that this particular piece contains no EU studies citation earlier than 1988!

While intermittent, and possibly untypical of general sentiments amongst scholars of the EU, these interventions might be read as attempts to ‘Americanise’ EU studies. By ‘Americanisation’, I mean a tendency to import the norms that have come to prevail within mainstream US political science, whilst seeking to discredit or invalidate approaches to the EU that do not subscribe to those norms.¹¹ This is a two-stage move. The first step is the emergence of a particular political science of the EU drawing upon positivistic and rationalistic premises and proceeding through certain epistemological standards. The second step involves pushing this particular political science of the EU as *the* (‘normal’) political science of the EU.

At one level, this becomes a political argument along the lines of the perestroika critique of mainstream US political science. One way of thinking through this issue is to use a ‘sociology of knowledge’ frame as a means to interrogate the position discussed in the previous paragraph. A relatively innocuous starting point is the observation that the development of theory and method within a sub-field should not simply be contextualised by that field’s object of study, but also by the broader disciplinary and social scientific environment within which that field is located. So the way in which we evaluate theories cannot simply be a (positivistic) matter of how theories correspond to/predict/explain their object of study (Rosamond, 2000: ch. 8).

Foundational Myths

In addition to the recent interventions on behalf of a particular notion of rigour, there is a commonplace account of ‘integration theory’ that uses precisely the kind of argument to suggest a precise and unproblematic relationship between theory and object of study. The story of the rise and fall of neo-functional theories of regional integration has been told often enough and, in many ways, has acquired the status of a foundational myth in EU studies. The failings of neofunctionalism are invariably taken to reside in its mis-depiction and mis-prediction of what was actually going on in the European Communities from the mid-1960s. In particular the rise to prominence of de Gaulle, together with the ‘empty chair crisis’ and recalibration of the Communities’ institutional balance in an intergovernmental direction

¹¹ See Wæver, 1998 or a discussion of the dynamics of ‘Americanisation’ in IR.

demonstrated (a) the enduring significance of nation-states and nationalist sentiment in Europe, (b) the continuing importance of international bargains to the fate of European integration and, therefore, (c) the perpetual salience of self-seeking governmental agents in the European-level policy process. Moreover, the lacklustre record of regional integration schemes elsewhere and their seeming incapacity to replicate the European model or to even survive provided a severe blow to the predictive and generalising ambitions of neofunctionalists. Key interventions acknowledging these dilemmas by Ernst Haas, the dominant integration theorist of the 1950s and 1960s, were taken as a sign of neofunctionalism effectively 'falling on its sword'.

The narrow 'truth as correspondence view' is one of several purposes that theory may have. It begets a certain sort of social science and a particular type of social scientific reasoning. It also invites us to evaluate theory in a particular way. One of the interesting things about the parable of neofunctionalism is that the early integration theorists themselves readily bought into this mode of evaluation (a point that Haas makes for the umpteenth time in his recent essay – Haas, 2001). They did so because neofunctionalism itself aspired to certain sorts of (context bound) social scientific norms. But – as Moravcsik notes (1998: 11) – the demise of integration theory as a discrete pursuit (in the 1970s) was also bound up with a general emerging distaste for 'grand theory' in political science and a consequent embrace of the mid-range. Thus neofunctionalism was also unsustainable because of the broader shifts in contemporary conceptions of what made viable 'theory'. Indeed, in one of the most prescient - but least-read - intergovernmentalist critics of neofunctionalism, Roger Hansen (1967) pointed to matters of theory construction and epistemology as his explanation for the alleged theoretical blind-spots and the lack of explanatory power of Haas et al's work.

How we tell stories about the genesis and development of a discipline and how we construe of its major debates are major features of both (a) pedagogy and (b) theoretical exegesis in any sub-field. Recent studies in IR have sought to explore how these 'foundational myths' are not simply benign misconstruals, but active shapers of present concerns and debates in the discipline. Thus partial readings or mis-readings have the ongoing and cumulative effect of (a) defining what is *legitimate* IR and what

is *not* IR as well as (b) constructing present oppositions between theoretical schools in the context of previous great debates.

Brian Schmidt's (1998; 2002) detailed re-reading of the foundations of disciplinary IR yields the central insight that the (conventional) stories filling textbooks about the origins and early development of IR are misconceived in all sorts of ways. This isn't just about the reproduction of a set of errors (although the mechanics of ongoing misconstrual is an interesting subject in its own right), but as Steve Smith notes 'the discipline gets defined as one founded *solely* on the problem of inter-state war. Thus, explaining this specific problem becomes the litmus test for the admissibility of approaches to the 'authentic' community of IR (2000b: 378). It also sets up a common sense chronology of IR as a series of great debates, which runs something like this: idealist origins – realist correctives – behaviouralist 'science' – neorealist formalisation – neoliberal institutionalist challenges – constructivist critiques of rationalism and the displacement of neorealism as the rival to neoliberal institutionalism, with various syntheses along the way.

Scholars of the origins of realism the tendency to present (classical) realism as a deeply rationalist and de-ethicised theory (i.e. roughly in terms of Morgenthau's 'six principles') has come under intense scrutiny from political theorists and historians of IR. In particular, the headline claims are that a deeper reading of say Morgenthau will demonstrate (a) a sophisticated historical sociology and a proto-constructivist ontology and (b) a post-Augustinian ethical commitment (Williams, 2002; Murray, 1997). The surgical removal of these elements (which we see most clearly with the emergence of neorealism) is a process that Wæver sees as part and parcel of what he calls the 'de-Europeanisation' of American IR (Wæver. 1998: 689). This is all bound up with the 'Americanisation' of the discipline of IR, by which Wæver seems to mean the development of state-centred theory of international politics commensurate with the emergent presentational and theory-building norms of US political science. Moreover both the 'neo-neo' debate and the more recent emergence of constructivism take place within intersubjective understandings about the easy relationship between realism and neorealism. So Alexander Wendt's project (Wendt, 1999) can be read in interesting ways. Its self-conscious attempt to displace neorealism as the rival to neoliberal institutionalism might be thought at one level as the unwitting rediscovery

of the social constructivist themes that always lay within realism (but which had been imagined away). But (Wendtian) constructivism's embrace of social theory clearly does not contribute to any re-Europeanisation of IR (to borrow Wæver's non-geographical meaning of the term). (IR-variant) constructivism's terms and strategies of engagement are clearly rooted in the contemporary 'normal science' of US IR.¹² Aside from Wendt's recent attempts to break bread with rational choice (Fearon and Wendt, 2002; Wendt, 2001), a good example of this tendency can be found in Checkel's (2001a; 2001b) attempts to locate his constructivist take on European studies in the epistemological mainstream of conventional discipline-based (US) social science.¹³

Explorations of the intellectual history of a field are not, as Schmidt reminds us, exercises in 'antiquarianism'. Rather they represent

an attempt to increase our capacity to examine critically the contemporary nature of the field by an understanding of the intellectual roots from which it has evolved. There is an intimate link between disciplinary identity and the manner in which we understand the history of the field.

(Schmidt, 2002: 16)

All of this should serve to remind us of Ole Wæver's point that theoretical development does not proceed through either (a) a progressive process of coming closer to unravelling the truth about the world through the development of ever more sophisticated conceptual tools or (b) the effect of external, 'real world' stimuli upon theoretical and disciplinary configurations. Rather we need to factor in what Knud Erik Jørgensen (2000) calls 'cultural-institutional context': a complex mixture of political culture, academic culture/institutions and norms of professional discourse. Thus theoretical choice is an equally complex matter. It is intimately bound up with professional contexts and norms. Put another way, agents of social science have choices, but they make them in institutional, discursive and cultural contexts that are not of their own choosing.

EU studies has less in the way of foundational myths, but arguably are several routine discursive (mis)constructions of the discipline, that tend to serve particular sorts of

¹² See the author and critics discussion involving Wendt in *Review of International Studies* **(*), ****.

claim about the emergence of rigorous and 'scientific' approaches to the study of the EU. The 'parable' of neofunctionalism mentioned above is one. To that might be added that the image of a 'great debate' between intergovernmentalism and neofunctionalism seems to be an *ex-post* reading of what was actually going on (Rosamond, 2000: 74-75). The integration theory project of the 1960s and 1970s was conducted largely in terms of a conversation between neofunctionalists. Intergovernmentalist critiques were taken very seriously (see, for example, the Preface to the second edition of *The Uniting of Europe* – Haas, 1968) and integration theorists sought to modify and improve upon the basic pre-theoretical insights of neofunctionalism well into the 1970s (see especially the contributions to Lindberg and Scheingold, 1971). In many ways, the story of neofunctionalism between 1958 and 1975 is a story of impeccable theory building in accordance with the norms of rationalist political science. This masks the complexity of the field – which leads to another common reading of the history of the discipline, namely that (a) EU studies has only recently broken out of 'IR' analysis and (b) the treatment of the EU as a political system, amenable to analysis with the tools of political science has only just begun.

A re-reading of Carole Webb's theoretical audits of what was going on in the field 20-25 years ago (Webb, 1977 and 1983) demonstrates that this is at best a fanciful reading of the situation. What might now pass for innovative theorising seems to have been *in situ* in EU Studies at least a quarter of a century ago.¹⁴ Leon Lindberg began writing about the EC as a political system as long ago as 1965 (Lindberg, 1965, 1967) and Lindberg and Scheingold's pathbreaking *Europe's Would-be Polity* was published 32 years ago and, lest we forget, Puchala's oft-cited 'Of Blind Men, Elephants and International Integration' (Puchala, 1972) prefigured themes of the literature on multi-level governance and contemporary institutionalism by a good two decades.

This may amount to using a sledgehammer to crack a nut, but this particular nut has a particularly hard shell. All of this is bound up with attempts to classify the pre-history

¹³ Moravcsik (2001) commends Checkel for his efforts to develop a 'rigorous' constructivism, but regards the project as ultimately doomed to 'crash land'.

¹⁴ I am indebted to Ian Manners for pointing this out to me

of EU studies as an *IR* enterprise – a claim, as we have seen, that is made with frequency to conveniently position a new mainstream political science of the EU as novel, rigorous and scientific.

This stylised account misses the nuance of much pre-existing work on European integration. In a fascinating recent essay, Haas has sought to clarify the ‘lines of intellectual descent’ as they relate to both neofunctionalism and contemporary theorising. While positioning neofunctionalism as ‘a manifestation of a type of [liberal pluralist] theorizing that stresses the disaggregation of the state in the search for explanations’ (2001: 23), he makes the point in a footnote that the underlying political science ‘straddle[s] the undemarcated boundary between comparative politics and international relations’ (2001: 30, fn.11).

A further component of this historiography of the subdiscipline is the explicit claim that EU studies has only recently become ‘rigorous’. The citation above of the founding editorial of *European Union Politics* (Schneider, 2000) reveals some quite breathtaking claims, first about ‘IR paradigms’ and second about the ubiquity of bad theorising (which *ipso facto* drew on grand theory) until quite recently. At best this relies on an extremely partial and blinkered reading of the development of integration theory from the 1950s to the mid 1970s (see Rosamond, 2000: chs 3 and 4 for more detail), where (a) the search for rigour in accord with broad disciplinary norms was commonplace (see the 1970 *International Organization* special issue as a case in point¹⁵), and (b) the field was wholly un-parochial. Haas, Lindberg, Schmitter et al were not unreflective practitioners of European area studies, but – as noted above – political scientists seeking to advance the discipline in ways not entirely unlike present advocates of ‘scientism’.

A further confusion concerns the frequent conflation of intergovernmentalism and realism/neorealism. One of the contributors to the opening issue of *EUP* (Schmidt, 2000: 39) insists that

‘Despite some refinements, European integration theory still revolves around the debate between neorealism and neofunctionalism’

The equation of intergovernmentalism with 'neorealism' is a common misconception that seems to get past peer referees with an alarming frequency. The bottling of Moravcsik (and for that matter Hoffmann before him) as 'neorealist' helps to discursively construct the classical terms of engagement in EU studies as hopelessly entrapped in an unproductive IR problematic.¹⁶ But it also misunderstands both (a) the nuances of Hoffmann's intergovernmentalism (about which Hoffmann, 1989, 1995 – in autobiographical mode – has written in some detail) and (b) the neo-liberal institutionalist turn in IR that spawned Moravcsik's liberal intergovernmentalist work on European integration. Neoliberal institutionalism has stood for a decade and a half as the principal *rival* to neorealist IR in the US debate. For example Caporaso sees connections between LI and realism (1999: 163) and he praises Moravcsik (1998) for bringing domestic factors into intergovernmental analysis, something, it has to be said, that Hoffmann (1966) had already done over 30 years previously.¹⁷

EU Studies and Area Studies

As suggested above, one of the features of contemporary (US) political science is the diminishing validity of area studies. Several oppositions seem to be prevalent in the debates. The most simple is tendency to set up an opposition between 'history' and 'theory'. 'Theory', more often than not in the shape of neofunctionalism, routinely suffered withering critique from historians of the origins of the EU. Alan Milward (1992: 12) remarks that the likes of Deutsch, Haas and Lindberg

¹⁵ Later published as Lindberg and Scheingold, 1971.

¹⁶ This is not to say that neofunctionalism did not represent a significant intervention in IR theory. For some it did indeed represent the most significant challenge to realist-dominated IR of the 1960s (Katzenstein, Keohane and Krasner, 1998; Smith, 2000). The point to make here once again is that participation in IR discourses did not signify ghettoisation within IR.

¹⁷ Hoffmann is all too often read as a crude realist – he is not, as a glance at his autobiographical writings as well as a deeper reading of his work will testify – I see clear linkages between Hoffmann's critiques of integration theory and Bulmer's domestic politics approach (1983) – see Rosamond, 2000: ch.4. Hoffmann himself (1966, 1995) was evidently concerned with the significance of domestic politics for the preferences of member-state governments.. 'Proper' neorealist work on the EU is comparatively rare. As Alec Stone (1994: 499) rightly notes 'neorealism is a theory of why, in "international political" society, the establishment of stable norms is either unlikely or impossible, why formal institutions do not develop meaningful autonomy, and therefore why a constitutional international regime is unimaginable'. An ingenious exception is found in the work of Joseph M. Grieco (1995, 1996).

simplified history unacceptably...by exaggerating the incapacity of the state. From the beginnings of detailed historical research into the origins of the European Community, it became clear that nation-states had played the dominant role in its formation and retained firm control of their new creation

In a similar vein, the distinguished British economic historian turned archivist scholar of the EU Keith Middlemas claims that 'I have tried to not to confine myself to any one interpretation, whether federal, functional or intergovernmental, and to proceed empirically, taking account of all the significant players' (1994: xiv). Most philosophers of science would presumably be able to drive a coach and horses through this sort of reasoning which creates an opposition between 'history' (objective, empirical, inclusive, bias-free) and 'theory' (subjective, conceptual, partial and potentially value-laden). That said, historians of European integration have tended to develop a powerful defence of research strategies based upon methods of induction as opposed to the dominant approach of political science that proceeds from deductive theory-based strategies.¹⁸

This relates to a second opposition under this heading: discipline-based theory-driven work versus detailed, area-specific knowledge. There is a strong empiricist tradition in EU/integration studies, especially in British academic circles where much of the work on integration has, until quite recently been densely descriptive. A recent 'state of the art' piece by Helen Wallace makes this point: 'The British talent for thick description ... has to be contrasted with much of the American literature where the objectives are often more theoretical than empirical' (2000: 103). The point is made slightly differently by Ole Wæver '[t]he European literature is a historicist attempt to capture an epochal transformation in the European order and its corresponding political lexicon; the American literature is driven by methodology and general theory' (1998: 724). Of course, we should not conflate the points made by Wæver and

¹⁸ Moreover, Milward's powerful critiques of integration theory (see especially Milward, 1992 and Milward and Sørensen, 1993) deserve closer scrutiny than they usually receive within the EU studies mainstream. They raise three issues of theoretical evaluation that this paper suggests are often taken for granted. First, there is the matter of the extent to which theories should be judged in terms of their capacity to offer sustainable narratives of empirical reality. Second, Milward flags the question of the ways in which 'good' theories correspond to prevailing social scientific norms. Finally, Milward signals 'sociology of knowledge' questions about the relationship of theory-driven research to particular values. For more discussion, see Rosamond, 2003.

Wallace, since they may illuminate interesting and possibly important distinctions between different European (including British) traditions in EU studies.¹⁹

The 'Anglo-American' opposition has fuelled one or two resentful critiques of recent American work about the EU. In particular, William Wallace has used the review pages of *International Affairs* to write concise but pointed critiques of works such as Sandholtz and Stone Sweet's *European Integration and Supranational Governance* (1998) and Walter Mattli's *The Rules of Integration* (1999). He argues that such work is driven by theoretical fashion – crudely making the facts fit the theory rather than developing a detailed knowledge of the EU and European countries before contriving fanciful models. In this account 'Americans' privilege the theoretical game over the generation of knowledge about the Union and the processes of integration. Crudely, the argument is that the attempt to fit facts into a theoretical box is the basis for the generation of ignorance and false assumptions (both books, in Wallace's view, make simple historical or factual errors. Knowledge about the EU and integration should come prior to the generation of theory runs this argument.

This is one route through which the incipient 'Americanisation' of EU studies is resisted. Yet it relies on a simple 'truth as correspondence' dictum and – paradoxically is relatively easily outmanoeuvred by proponents of US orthodoxy. There are hints of this in the exchange between Helen Wallace and Moravcsik in the 1999 *JEPP* review symposium on *The Choice for Europe* where Moravcsik neatly hits back at Wallace with an argument about replicability.

Perhaps more relevant here is the diminishing validity of 'area studies' as a sustainable academic pursuit in the United States. Incentive structures (particularly in the job/tenure markets) are such that American scholars of European integration need to construct themselves as 'political scientists' rather than 'Europeanists'. Figures like Robert Bates have argued that the only utility of 'area studies' resides in the accumulation of particular empirical data. This in turn should be incorporated into universal forms of disciplinary knowledge - notably rational choice accounts of the

¹⁹ For an interesting discussion of the divergences between US and 'British' studies of international political economy, see Murphy and Nelson (2001).

political. In this account, 'area studies' has to be a means to an end rather than an end in itself.

The arguments of Bates are worthy of fulsome quotation:

'within the academy, the consensus has formed that area studies has failed to generate scientific knowledge' (Bates, 1996: 1)

'Many see area specialists as having defected from the social sciences into the camp of humanists. Their commitment to the study of history, languages and culture, as well as *their engagement with interpretivist approaches to scholarship*, signal this defection. This perception is shared by many political scientists. Rare is the political science department wherein those who study Europe, South Asia, Africa etc. do not reside within area studies programs. Rare too is the department wherein the area specialists fail to constitute a center of resistance to new trends in the discipline. They lag behind the others in terms of their knowledge of statistics, their commitment to theory, and their familiarity with mathematical approaches to the study of politics' (Bates, 1996: 1 – emphasis added)

'The fact that area studies centres possess clients beyond social science results in the application of criteria other than *disciplinary standards* to appointments, promotions and course offerings' (Bates, 1996: 1 – emphasis added)

Bates finds distasteful the tendencies of area studies programmes to resist theory and rigorous methods, but in the course of these two pieces he becomes worried about their demise. He argues instead that rational choice needs area specific knowledge if it is to prosper and deliver its promise. Rational choice and game theoretic advances (that allow the 'rigorous' explanation of small n phenomena or unique events) mean that 'our discipline is becoming equipped to handle area knowledge in rigorous ways' (Bates, 1996: 2) and 'the tools cannot be applied in the absence of *verstehen*' (ibid). So, the task at hand should be the engineering of a divorce between area studies and political science; rather we should look to engineer a 'mutual infusion' (see also Bates, 1997 and Bates et al 1998)

But the point to make in response is a simple one. The use of 'area studies', according to Bates, is to service disciplinary advancement. Moreover, throughout these remarkable pieces, 'social science' is a term reserved for rigorous, disciplinary

forms of knowledge, allied to particular norms of theory building, research design etc. As David Ludden (1998: 5) puts it, 'social sciences want *to use* area knowledge more than *to have it*.' This tendency seems to have been reinforced by recent funding decisions and by the re-organisation of the US Social Science Research Council (Ludden, 1998: 1-3). In addition the 'power-knowledge' rationale for area studies has virtually evaporated following the end of the Cold War (Cumings, 1996). Although, as Craig Calhoun points out, the growth of European studies was not motivated by Cold War security concerns. Its roots and dynamics are rather deeper in the US academy. His point is that European studies 'was much more readily integrated into the non-area specific disciplines of the social sciences' so that '[m]any researchers worked on Europe without identifying specifically with European studies' (2003: 15). If correct, this further dents the mythology of EU studies of the 1950s, 1960s and 1970s as disconnected and ghettoised.

Nonetheless, these debates have spilled over into discussions of European (Union) studies in the US. Christopher Makins' report *The Study of Europe in the United States* (www.eurunion.org/infores/studyof/exec.htm) prompted debate over a number of issues, not least the *policy relevance* of European studies research and what Donald Hancock calls 'the potentially debilitating tension between epistemological and methodological expectations on the part of various disciplines (notably Economics, Political Science, and Sociology) and area studies (including in-depth country and regional expertise)' (Hancock, 1999). The construal of threats to the latter in the form of 'oftentimes faddish disciplinary orthodoxy' represents one discursive strategy through which resistance to dominant US academic discourses of the EU might be resisted (it is one that also resembles the slightly recidivist resistance to US-style theory-driven research in (predominantly) British critiques of current US work – see above). However, as Glenda Rosenthal implies in the same forum in *ECSA Review*, neither (a) the public funding structures for European Studies in the US nor (b) the configuration of departments and programmes in American institutions are conducive to the effective organisation of resistance to the disciplinary hegemony of rational choice-inspired political science over area studies forms of knowledge about Europe (Rosenthal, 1999: 5).

Four sorts of reaction have emerged to what, as Ian Lustik (1997) has pointed out is a re-run of some old debates about ‘nomothetic’ versus ‘idiographic’ approaches to scholarship. This point is also picked up on by Helen Wallace (2000: 96), who refers to old conversations about whether area studies has its own disciplinary identity (e.g. do European and Latin American studies have something in common?)

Three of these reactions are critical. The first insists that European area studies requires preservation precisely because Europe is such an empirically interesting area, characterised as it is by profound social change and radical experimentation (Calhoun, 2003). This defence drifts into an argument about the limitations of discipline-bound studies that happen to use Europe as an empirical case, where the motivation is likely to be disciplinary consolidation rather than deep understanding. The second is exemplified by Chalmers Johnson’s spirited reply to Bates (Johnson, 1997) that takes on the rational choice project and its imperialising ambitions. The third response is not unrelated and pushes the argument that the universalising ambitions of disciplines cannot capture the nuance of particular context-bound localities and that the import of universal categories is a recipe for misunderstanding (Appadurai, 1996). Interestingly enough, this emerges from a sociological commonplace about globalisation, but one that doesn’t really permeate political science/political economy very much – that globalisation is about the heightening of particularism and the exacerbation of fragmentation rather than universalisation and integration. The danger of mainstream views – as Peter Hall and Sidney Tarrow noted in a piece in *The Chronicle of Higher Education* (Hall and Tarrow, 1998) – is that the rationale for area studies would be squeezed by the enthusiastic search for global trends.

The fourth response is to take what Helen Wallace (2000) labels the ‘colonisation’ of the study of Europe by approaches like rational choice as a largely benign phenomenon. She cites favourably Fritz Scharpf’s observation that rational choice institutionalism forces us to think seriously about the behaviour, preferences and strategies of actors involved in the EU process. This seems to me to be disingenuous and also falls for some of the claims made on behalf of their approach by proponents of rational choice. For example, what precisely was neo-functionalism about if it wasn’t about ‘the behaviour, preferences and strategies of actors involved in the EU process’? The key difference is that while neofunctionalists bought into conceptions

of instrumental rationality, they did not subscribe to the hard rational choice claims about (a) actors maximising their utilities in formally predictable ways and (b) rationality being homogenous across all individuals being studied. The point is made by Haas himself recently, when he retrospectively labels neofunctionalism as a form of 'soft' rational choice (2001: footnote 4 is especially helpful). We are back, it seems, to the discursive (mis)construction of integration studies past as a means to shoe-horn in rational choice as in some ways mould-breaking in the hitherto un-rigorous domain of EU studies present.

Conclusion

In some ways the recent politicisation of the 'area studies versus disciplinary knowledge' issue is most connected to fears of the seepage of hard rational choice into the study of political phenomena in general and the EU in particular. In some ways this is relatively easily contained. More interesting perhaps is the increasing propensity for some political scientists to make claims about *their* form of EU studies as more thoroughly scientific.

Take again Dowding's (2000) claims about rational choice institutionalism as the 'normal science' of EU studies and a little less explicitly by the emergent editorial line of *EUP* and statements such as the concluding chapter to Aspinwall and Schneider's extremely interesting edited collection on institutionalist approaches to the EU. The latter is – frankly - an example of Bates-style reasoning where the appearance of accommodation/inclusion actually masks an assertion of the singular virtues of the epistemological strategies of rational choice. Institutionalists of various hues may have lingering ontological disagreements but they 'take questions of research design seriously and believe that standard social scientific methods can be used to explain European integration' (Aspinwall and Schneider, 2001: 178). This is a clear appeal to the standards of scientific 'rigour' associated with the likes of King, Keohane and Verba's (1994) manual for the training of American graduate students in political science and resembles the move made by Robert Keohane in his presidential address to the International Studies Association in 19** (Keohane, 19**). It is part of a general commitment (shared with Moravcsik), as Scharpf implies, to social science as a vehicle for the production of 'general theories about empirical regularities'

(Scharpf, 1999: 186). The downside is the treatment of deviant cases as exceptions, rather than as interesting phenomena in their own right.

EU Studies has always acted as an interesting microcosm of the political sciences from its foundations – contrary to the claims of a lot of recent scholarship that seems to be determined to pigeonhole the field as hopelessly ghettoised or parochial. Steve Smith (2000b: 376) maintains that

the discipline of IR remains an American social science; and in the UK there is a far more pluralistic approach to questions of epistemology and methodology, which results in a much wider set of questions being seen as legitimate ... the situation in the UK is far more likely to permit the development of an IR discipline relevant to the dominant global questions of the new millennium.

Perhaps this is a peculiar IR concern – a pathological incapability to ask questions about non-state forces or to draw the boundaries of the discipline in ways that render particular lines of enquiry inadmissible or not IR-proper (Smith, 2000b). This is certainly a hot issue with respect to the current theoretical triad in (US) IR (neorealism, neoliberal institutionalism and constructivism).

This paper's suggestion is that we should be careful to prevent this from happening in EU Studies. As so much work (often conducted beyond the disciplining/disciplinary gaze of US political science) argues, the EU is neither fish nor foul – it is a peculiar phenomenon about which we need to 'think otherwise' to use John Ruggie's felicitous term. In the hands of writers like Kohler-Koch the EU is bound up with the denationalisation of politics in Europe and the opening up of multiple political spaces (see also Wallace, 2000: 100-101). The 'Americanisation' or 'scientisation' of EU studies seems to want to do as Bates suggests when he talks about the disciplinary excellence of rational choice political science against the poverty of area studies. Indeed the impulse of some is to apply concepts and theories derived from the study of political systems (especially the US) to the EU because it is a polity like any other. There is, of course, nothing necessarily malevolent about such an enterprise, but the claims made its proponents are more often than not monistic and seek to discredit

those strands of work that fail to pass the test of 'rigour'. Should this become an embedded orthodoxy in EU studies then we are faced with two potential consequences, one intellectual, the other sociological. The first is that we will be asking only partial questions about the European experiment. The second is that particular definitions of 'good' science will come to affect the ways in which EU studies is done and how scholars' work is evaluated. In a recent intervention, Robert Geyer (2003) uses complexity theory to show that the EU is a complex system, which – out of necessity – requires ontological and epistemological pluralism involving the mutual toleration of rationalist and reflectivist approaches.

These are also matters for pedagogy. The presentation of a sub-area as either pluralistic and open or in the phase of a closed 'normal science' will have a critical impact upon the next generations of scholars of the EU. Consciousness of 'sociology of knowledge' issues and ongoing reflection on the intellectual history of the field is imperative. After all, we need to know who we are, how we got here and where we might be going. But how we read theoretical 'progress' and how we tell stories about disciplinary evolution is not a neutral choice. To understand the history of EU studies as a matter of increasingly sophisticated theoretical choice that brings us ever close to the truth is to (unwittingly or otherwise) buy into a philosophy of science that disembeds academic work from social life.

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