NOTES

1 The earlier article arguing that 'the European Community strengthens the state' (meaning the national executive) is mentioned in passing in a footnote (p. 76), but its thesis does not fit well with the present conceptualization according to which '"State" actors are treated as proxies for the underlying social forces' (p. 36, footnote 29).

2 For instance, it is surely true that, for identifying the preferences of actors, their strategic calculations, and the information at their disposal, primary sources are to be preferred over secondary sources, and hard primary sources over soft ones (pp. 80–2). But if reliance on hard primary sources is illustrated by the fact that the author himself conducted over a hundred interviews, it is not clear that his procedures differ so much from the practices of most colleagues who are doing serious empirical work in comparative politics.

THE CHOICE FOR EUROPE: CURRENT COMMENTARY AND FUTURE RESEARCH: A RESPONSE TO JAMES CAPORASO, FRITZ SCHARPF, AND HELEN WALLACE

Andrew Moravcsik

It is a pleasure and a privilege to read and respond to three such distinguished and varied critics as James Caporaso, Fritz Scharpf, and Helen Wallace.[1] Trying to do justice to their disparate critiques recapitulates the tensions we all face in conducting basic research on European integration. Relevant scholarship spans the disciplines of history, economics, legal theory, sociology, and political science – and, within the latter, the sub-disciplines of international relations, comparative politics, and policy analysis. No surprise, then, that each commentator, representing a distinct area of political science, situates the major theoretical conclusions of *The Choice for Europe* differently. Caporaso treats them as audacious attempts to smash long-standing sub-disciplinary idols. Scharpf views them as straightforward, occasionally even obvious, generalizations about the political economy of the type of global issues the European Community (EC) handles. Wallace treats them as inherently incomplete, perhaps one-sided generalizations squeezed out of a far more complex historical reality. From his or her particular perspective, each seems justified.

Despite differences of taste and training, however, the commentators converge on at least four concrete criticisms. These are: (1) the historical data and cases are incomplete and perhaps biased; (2) the analysis omits potentially powerful explanations; (3) the theories employed explain ideal-typical generalities but insufficient variation across cases; (4) the cases are biased in favour of intergovernmental politics, because they do not explicitly consider incremental change over time via judicial or administrative politics. In responding to these four concerns, I shall pay primary attention to future opportunities to advance the research programme underlying *The Choice for Europe*.

DOES *THE CHOICE FOR EUROPE* REST ON A BIASED SAMPLE OF HISTORICAL DATA?

I begin with Wallace's accurate observation that I have not related the entire story of European integration. *The Choice for Europe* sharpens and extends the most prominent explanations for preferences, bargaining, and delegation in the EC literature, then collects and analyses limited categories of data pertaining to specific hypotheses.[2] Despite its length, it privileges generalization about broad outcomes over subtle details, thereby quite deliberately omitting many secondary aspects and influences of major EC decisions.

Omission alone, however, is no ground for complaint – as Wallace is well aware. Any effort to illuminate general patterns in complex events must sacrifice some richness of detail. Even if I could convince readers to read – or, in these thrifty times, an academic publisher to publish – *more* than 520 pages on European integration, the mass of journalistic reportage, secondary sources, oral histories, and governmental documents on major European Union (EU) decisions is so vast that one can quite literally cite dozens of plausible conjectures to explain any aspect of these decisions, followed by dozens of sources to support each conjecture. Chasing each one down in complete detail would expand the project to encyclopedic scope.[3]

The real question is thus not whether the history in *The Choice for Europe* is incomplete – it obviously is – but whether it is biased. At the heart of the matter lies Wallace's scepticism that an unbiased sample of the available evidence really resolves theoretical debates as decisively as I claim it does. If I *had* told more of the story or consulted more sources, she hints, my conclusions – notably those about commercial motivations and the weakness of supranational entrepreneurs – would have been more ambivalent. Wallace openly questions whether the evidence so clearly disconfirms geopolitical and ideological explanations and whether the existing literature so strongly asserts them. Even Scharpf, who generously acknowledges my attention to methodology, cannot resist adding that 'there is never any question of the author's preference for one answer to each of his questions . . . always the last-mentioned one' (p. 165).[4] In this, I suspect, Wallace and Scharpf capture the instinctive reaction of many readers. Why, readers will ask, should we accept a series of empirical claims that concede so little to the bulk of existing scholarship? Isn't this because, as Wallace believes, 'the eye of the artist is selective' and 'slides over some of the elements that displease' (p. 156) it?

Underlying the question of bias is a fundamental issue: By what methodological criteria should qualitative analysts select and analyse data and hypotheses? While I can claim neither to have consulted every available document nor to have considered every conceivable conjecture, I do claim to have employed a standardized procedure for selecting, analysing and presenting empirical evidence that strives explicitly to be both *transparent* and *unbiased*.[5] I aim to test explicit hypotheses that express, extend and refine the most prominent arguments about European integration. I repeatedly bend the structure of the book to consider plausible conjectures that did not quite fit the mould. As Scharpf and Caporaso observe, moreover, hypotheses are formulated to offer not just an even-handed test between theories, but in some important ways to bias the results *away* from my final conclusions. In addition, I discard secondary-source speculation entirely, including the citations to the *Financial Times* and *Economist* on which many analyses rest, and rely instead on observable patterns of behaviour and a selection of more reliable primary documents. I weight those sources carefully and openly. The underlying goal of such techniques is to generate findings that are *replicable* – a quality, as Caporaso and Scharpf concede, largely absent from existing studies of the EC.[6]

To see the difference between what Wallace suggests I have done and what I actually did, consider the case of General de Gaulle. Three points here. First, Wallace believes I overstate the existing consensus in favour of geopolitical and ideological explanations of European integration. Yet among the secondary articles and books on the General, which number many thousands, *I was not able to find a single one* that attributed his major EC decisions (e.g. opting for membership, the Fouchet Plan, vetoing the British, provoking the 'empty chair' crisis) primarily to commercial interest. Some mention economic interest as a secondary consideration; some dismiss it altogether. Second, Wallace's scepticism and the secondary literature notwithstanding, the available *primary* evidence runs about 5:1 in favour of economic motivations – with the most reliable evidence also being the most favourable.[7] I am open to suggestions as to why this finding might be spurious. I have thought through most of the propagandistic possibilities and come to the conclusion that it is an accurate representation of the General's thinking. Third, this evidence is not based, as Wallace implies, on selective citation of the opinions and speculations of those memoir writers and interview subjects. I weight the evidence carefully, with memoirs and interviews employed primarily for factual information and greater emphasis accorded to those witnesses backed by corroborating evidence and with little apparent incentive to mislead. Much of it relies on verbatim transcripts of Cabinet meetings and confidential discussions. Sheer speculation after the fact (unless openly presented as such in the text) is discounted to *zero* – no matter who the author is. Where such interpretive judgements are problematic, they are explained in detail to the reader.[8]

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 on inductive theory, I thereby render it far easier for historians and political scientists to challenge the objectivity and accuracy of my analysis. Such challenges are inevitable; some are sure to be telling, particularly where they rest on newly available documents and data. That is the sort of debate we should be having.

Yet, for the moment, Wallace in fact neither questions the methodological criteria I employ nor offers an explicit demonstration of my failure to meet them.[9] While conceding that my conclusions are counterintuitive – as is my intent – she fails to show that they are in any way biased. I do not for a moment mean to denigrate the more detailed and open-ended policy analysis of the kind practised by Wallace and, in a somewhat different way, by leading diplomatic historians. These have been unique and indispensable sources of insight and information for me as well as many others – as exemplified by Wallace's elegantly detailed critique. Yet in the end I side with Caporaso and Scharpf (and also in the end, I suspect, Wallace) in my conviction that our understanding of European integration would also benefit greatly from more disciplined and focused social scientific debates among theoretically and methodologically replicable claims.

ARE IMPORTANT THEORIES NEGLECTED?

A more telling and not entirely unrelated criticism, advanced by all three commentators, is that *The Choice for Europe* neglects important factors to which more attention might fruitfully be given. I do not dispute the value of considering new variables and welcome future research drawing on theories of policy ideas, domestic institutions, and two-level games.[10] To the contrary, testing such theories against a rigorous baseline should be a major priority for future research in this area.

Wallace rightly observes that I fail explicitly to consider, except in passing, explanations based on *economic policy ideas*. While the tight correlation between structural economic interests, interest group mobilization, and national positions calls such explanations into question, they surely merit more rigorous testing.[11] The analysis in *The Choice for Europe* predicts in general that the weaker and more diffuse the domestic constituency behind a policy and the more uncertainty there is about cause–effect relations, the greater the role of economic (like geopolitical) ideas is likely to be.[12] Thus I predict – as is the case in domestic policy – that we shall observe a relatively modest autonomous impact for ideas in agricultural or perhaps industrial tariff policy, a greater role in regulatory policy, and the greatest role in monetary policy, where fundamental uncertainty about the consequences of policy is greater and costs or benefits more diffusely distributed.[13]

Caporaso and Wallace observe that I downplay the role of *domestic political institutions, including political parties*. Again, the correlation between structural economic interests and national policy calls this explanation into question. One is repeatedly struck, for example, by the longer-term continuity of national policy during periods of partisan change. Still, one might point to areas in which partisan differences matter or institutions occasionally play a covert role. We would expect political parties to be more important in those broad redistributive issues – such as social and monetary policy – as they are in domestic politics, as well as in areas of overtly ideological motivation, such as the delegation of power to the European Parliament. Examples of the background importance of institutions might be de Gaulle's role in reforming the French economy, surely a function in part of the centralized constitution of the Fifth Republic, and the role of central banks in monetary policy, which I treat as an exogenous constraint but (as Scharpf notes) do not fully integrate with the generally pluralist thrust of the argument. Endogenous tariff theory, which I appropriate, has moved recently toward more explicit discussion of institutions – a trend of which EC scholars might usefully take note.[14]

Scharpf wonders, in a related criticism, why *two-level game theory* – in particular my claim elsewhere that European integration 'strengthens the state' – was not included.[15] The use of international institutions and foreign policy prerogatives to make domestic policy does not simply involve exchanges among governments representing social interests, but also redistributes domestic influence among state and societal actors. Theories of this kind surely deserve closer scrutiny, particularly when explaining aspects of the EC's 'democratic deficit'. Yet the best way to test whether such factors actually have an impact on policy is to compare them to a baseline unitary-actor theory; anything less only invites confusion.[16] In addition, I believe such forces are empirically secondary. My purpose in *The Choice for Europe* was firmly to establish such a baseline; I invite debate with any equally carefully controlled empirical challenge.

DO THE THEORIES EXPLAIN SUFFICIENT 'SECOND-ORDER' VARIATION?

The Choice for Europe is explicitly multi-causal. It divides major EC decisions into three analytical stages; within each stage, it seeks to assess the relative explanatory power of two or three theories. It seeks to explain major intergovernmental bargains by accounting for variation in national preferences, bargaining outcomes, and institutional choices. All three commentators, led by Scharpf, suggest that more could have been done to explain what might be termed 'second-order' variation – that is, variation in the relative power of the theories that compete to explain each stage. Rather than simply assessing the relative power of each theory in each category – economic vs. geopolitical influences on preferences, intergovernmental vs. supranational influences on bargaining, ideological vs. technocratic vs. commitment considerations on institutional choice – could not more be done to endogenize variation in the relative importance of each across cases? This is an important and subtle observation, the implications of which merit serious consideration.

All three commentators are correct to assert that greater attention to antecedent conditions – and, though this is not made explicit, case selection more attuned to variation on the independent variables – might have permitted us to learn more about the conditions under which each theory holds. The major cost of *not* doing this, Scharpf insightfully observes, is that 'deviant cases are in danger of being ignored or downplayed as "exceptions", rather than being exploited for the development of more reality-congruent theory through the introduction of analytically pertinent distinctions' (p. 166). On the question of preference formation, Wallace notes that 'an analysis that weighted, rather than "exceptionalized", the geopolitical component in relation to political economy considerations might have served better' (p. 156). Caporaso adds that 'if the cases had been ordered along several dimensions (say, more or less controversial, or from collective gain to hard distributive gains and losses, or different types of policy sector), comparison of the results across cases would have given us more leverage' (p. 163).

There is much truth here. Yet Scharpf and Caporaso rightly present this not as criticism, but as recognition of the trade-offs inherent in research design.[17] While openly aspiring to both, *The Choice for Europe* is in the end more problem-driven than theory-driven, in that my primary goal is to isolate the most important determinants of a series of major decisions and mould them into a synthesis that can establish certain baseline expectations about European integration. (It is a bit surprising, given her comments above, that Wallace chides me for this choice.) When forced to choose between the integrity of this goal and optimizing case selection to advance particular lines of theory, I lean somewhat toward the former. The result, as Caporaso hints, is that we gain somewhat more empirical knowledge about the relative importance of each theory in the case of the EC (internal validity) and somewhat less about the extent of its generalizability to other cases (external validity). Refining useful theories by specifying antecedent conditions more precisely and thereby explaining residual variation is surely one of the most important tasks in the research programme that follows from *The Choice for Europe*. That being said, however, it would be quite misleading to conclude, as one might be tempted to do on the basis of Scharpf's comments alone, that *The Choice for Europe* lacks theory sufficiently well grounded in rigorous deductive assumptions to support clear predictions about empirical scope or generalizability beyond the EU. The book in fact extends existing bodies of theory to answer precisely the questions that Scharpf, Wallace, and Caporaso pose. Two examples must suffice.

First, Scharpf and Wallace suggest the need to specify conditions under which supranational entrepreneurs are more or less likely to exercise influence over international negotiations, thereby explaining the 'exceptional' case of the Single European Act (SEA), in which Commission and Parliament officials played a significant (if still secondary) role. My argument in *The Choice for Europe*, drawing on social choice models of entrepreneurship, is that supranational influence is possible only where two conditions are met: national governments face high ex-ante transaction costs and significant informational (or ideational) asymmetries favour supranational entrepreneurs. This, for reasons I elaborate in more detail, appears to require that there should be *domestic* (and transnational) co-ordination problems, which vary in predictable ways. In sum, if we know some basic things about domestic politics, we can predict 'windows of opportunity' for supranational entrepreneurship.[18] In an article forthcoming in *International Organization* I extend this line of argument, testing five fine-grained theories of supranational entrepreneurship, each with subtly different informational assumptions and antecedent conditions, which I then employ – precisely as Scharpf and Wallace recommend – to endogenize the SEA 'exception'. The resulting hypotheses can also be applied across a wide range of international institutions.[19]

Second, Wallace and Caporaso suggest the need to specify more precisely the conditions under which economic interests or geopolitical interests or ideology matter more – thus explaining the 'exceptional' impact of geopolitics that I found in approximately 20 per cent of the episodes of national preference formation. Here, too, *The Choice for Europe* advances a distinctive argument. For the Olsonian pluralist reasons Caporaso correctly attributes to me, when governments balance these two imperatives, the presumption tends to be in favour of the issue-specific (hence generally, in the case of the EC, economic) concerns.[20] Geopolitical ideology is more important where issue-specific consequences are essentially incalculable (e.g. recurrent debates over the powers of the European Parliament) or where core national economic interests are already satisfied (e.g. German acceptance of a customs union over a free trade area in the 1950s). While this hypothesis seems to test out well, there is surely much room for greater specificity.[21]

In sum, we all agree that more precise, deductively grounded theory with clear antecedent conditions should be a central objective of future research on European integration. I believe that *The Choice for Europe* sets forth some significant empirical propositions as steps toward such a goal – including most of those recommended by Scharpf, Wallace and Caporaso – and that their empirical confirmation further supports the underlying research agenda found there.

IS THE CASE SELECTION BIASED?

Caporaso and Scharpf suggest that the case selection may be biased. Rather than selecting cases of major policy change *per se* – which might have included such incremental processes as the establishment of a distinctive Brussels-based bureaucratic style and culture, the development of formal and informal norms around the Committee of Permanent Representatives (COREPER) or the Council of Ministers, and, most important, the gradual assertion and acceptance of the supremacy of European law – I focus on intergovernmental bargains *per se*. Scharpf goes so far as to suggest that each element of my central conclusion that the relative power of governments pursuing commercial interests determines the course of integration has such 'a high degree of a priori plausibility that it seems hard to take [its] competitors quite as seriously as he does' (p. 165).

This criticism need not detain us long, for two reasons. First, while Scharpf, like myself, was trained to believe that basic political economy and bargaining theory has 'a high degree of a priori plausibility' (p. 165), this is not, even now, accepted by most scholars and public commentators on European integration. 'Today,' I write in the book, 'no claim seems more radical than the claim that the behavior of EC member governments is *normal*.' Other critical responses, including those of Caporaso and Wallace, clearly illustrate this.

Second and more fundamental, it is simply not my intention to offer a comprehensive theory of European integration. What I offer instead is far narrower, namely a proposed solution to what is arguably 'the most fundamental puzzle confronting those who seek to understand European integration', namely the determination of which factors most strongly influence major intergovernmental bargains.[22] This constrained focus should trouble only those committed to the venerable notion, of which Caporaso is rightly critical, that a single theory can explain all of EC politics at one go. Following Haas's admirably honest self-critique in the early 1970s, I believe the search for such a theory to be futile, even counterproductive.[23] There cannot be *a* 'theory of regional integration' or 'theory of the EC' any more than there can be *a* 'theory of comparative politics' or 'theory of American politics'. The EC is a complex, institutionally diverse, multi-faceted political system, and I know of no convincing reason why a theory of incremental legal or administrative change under delegated institutional constraints should have the same basis as a theory of intergovernmental bargains in a classical diplomatic setting.[24] Many events in the EC are not properly within the specified domain of my theories – something that Scharpf, with his admirable attention to antecedent conditions, should be the first to concede. The task of synthesizing all the disparate elements of European integration into one model, if possible at all, still lies far before us.

To be sure, I confess to just a bit of nostalgia – vicarious, in my case – for what Caporaso terms an 'ultimate showdown' (p. 163) with neofunctionalism. Like him, however, I feel it inappropriate to indulge it. Instead, the research programme underlying *The Choice for Europe* seeks (in Caporaso's apt colloquialism) to 'mainstream' integration studies (p. 161). Rather than refute neofunctionalism, we should dismember it, either appropriating or challenging selective hypotheses. I appropriate its interest group theory of politics and economic functionalism, while challenging its supranationalist theory of bargaining and technocratic theory of delegation.

Yet Scharpf and even Caporaso fail to acknowledge that, while the research design of *The Choice for Europe* is not optimized for this purpose, the case studies are none the less deployed to conduct a preliminary test of more dynamic 'historical institutionalist' (HI) claims about endogenous processes of integration over time – thereby addressing many concerns of those who believe incremental change is more fundamental than grand bargains. For this task I employ what is surely the most rigorous formulation of HI to date, one proposed by Paul Pierson. Pierson persuasively argues that if HI claims are correct, we should observe over time unintended or unforeseen consequences, unpredictable and unexpected shifts in national preferences, and, perhaps, a powerful role for supranational entrepreneurs.[25] This is consistent with, though does not necessarily imply, movement toward Wallace's view that we need to consider more fully the role of 'irrationality . . . confusion and . . . mistaken judgements' (p. 158).

We observe, I argue in the conclusion to *The Choice for Europe*, none of these things.[26] To be sure, structural circumstances and state preferences evolve over time and some (though decidedly not most) of these changes appear endogenous to prior decisions to integrate. To the extent, moreover, that these adjustments involve 'asset-specific' investments – that is, investments dependent on maintenance and continuation of integration – underlying support for integration is likely to deepen over time as a result. Yet most of the significant endogenous changes in structural circumstances and preferences between major decisions were foreseen, indeed intentional. Though governments have often issued spurious denials, developments such as large common agricultural policy (CAP) surpluses driven by high price supports, the increasing size and influence of international exporters and investors, and the tendency of qualified majority voting to impede opposition by governments with extreme preferences were far from unforeseen or undesired. The primary purpose of European integration was to bring about just such results. Unintended and undesired consequences have been secondary; changes in national preferences have been incremental and linear over long periods.

CONCLUSION: FEDERALISM AND THE FUTURE OF INTEGRATION

I have focused throughout on the future of EC scholarship. My central point is that *The Choice for Europe* in fact addresses a good number of the criticisms raised by Caporaso, Scharpf and Wallace. Despite sub-disciplinary and methodological differences, there is in fact considerable consensus among all four of us concerning the proper path of future fruitful scholarship within the research programme set forth in *The Choice for Europe*.

I would like to close by turning very briefly to the implications for the future of European integration itself.[27] Wallace suggests in closing that my analysis 'provides formidable arguments *against* those who portray European integration as necessarily a cumulative and irreversible process' (p. 159). This assessment rests on the belief – a legacy not just of scholars like Karl Deutsch, but of the European federalist movement – that ultimately only fundamental transfers of sovereignty and shifts in values can lock in integration.

I believe *The Choice for Europe* supports a more optimistic prognosis. In an era where democratization has pacified Western Europe, talk of federalism triggers deep public suspicion, and technocratic planning (central banking excepted) has fallen out of fashion, Europe is none the less proceeding toward enlargement, monetary integration, and an ever deepening single market. There is an underlying functional reason for this, namely the consistent increase in social support, above all from producer interests, for the economic integration of Europe. Over time, underlying socio-economic developments and the prior success of the EU in achieving its objectives have created invested economic interests that are the major guarantors of its future stability. From this perspective, are not the true 'Eurosceptics' those who believe that the EU is fragile because it rests on fears of refighting the Second World War, hopes of realizing federalist dreams, the intermittent 'political will' of national leaders, and the unintended consequences of prior actions? And are not the true 'Europeans' those who view the EU as a stable form of pragmatic co-operation deliberately tailored to the enduring, increasingly convergent national interests of European firms, governments, and citizens?

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NOTES

1 I am grateful to James Caporaso, Robert Keohane, Paul Pierson, George Ross and Helen Wallace for comments on this article.

2 Throughout I employ the name European Community, not European Union, as the book in question covers the period from 1955 to 1991.

3 Such constraints should not be underestimated. A distinguished reviewer for a leading academic press – an outlier, fortunately – recommended that I cut *The Choice for*

Europe in half by truncating the theoretical analysis, citing no more than five sources per page, and eliminating the United Kingdom!

4 For the record, this sequence was imposed only in the last manuscript revision, after consultation with my editor, for presentational reasons.

5 Andrew Moravcsik, *The Choice for Europe: Social Purpose and State Power from Messina to Maastricht* (Ithaca: Cornell University Press, 1998), pp. 77–85, also 10–13. (Hereafter 'CFE').

6 Gary King, Robert O. Keohane and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994), pp. 26–7. Without a shared commitment to replicability, for example, it is hard to know what to make of Wallace's assertion that the outcome of the British accession negotiations in the early 1970s was decisively shaped by policy errors on the part of the British government – a result she believes my approach is too crude to capture. While my book does not investigate this case in depth – the section was cut in revisions – I do consider British accession negotiations a decade earlier, which raised very similar issues. Consistent with my practice of entertaining explanations that are widespread in the literature, even when they are not among the standardized theories I test, I consider and reject precisely this explanation – that is, the near universal view that British leaders were incompetent or benighted. I conclude that the bulk of the primary evidence reveals Macmillan's diplomacy as far more foresighted *and* more economically motivated than most diplomatic histories suggest. For this reason among others, I suspect that the bargain that the British government struck in the early 1970s was about as good as could be expected, given its internal and external weaknesses. The unfavourable outcome to Britain fits, moreover, the pattern of subsequent Greek, Iberian and perhaps also Scandinavian/Austrian accessions, as well as current negotiations with Central and South European candidates, wherein applicants do badly in initial accession negotiations, in which they are *demandeurs*, but subsequently exploit the prerogatives of membership to extort side-payments. The British themselves did just this, if somewhat inadvertently, in 1975. My basic point is not that my interpretation is correct and Wallace's is not. It is instead that there are always many a priori plausible conjectures. To determine which are more accurate, we must commit ourselves to rigorous and replicable interrogation of unbiased data – and then debate the result with equally constrained critics. From this perspective, Wallace's empirical assertion is intriguing, but hardly conclusive.

7 CFE, pp. 176–97, 225–30. For a more systematic presentation of the evidence, see Moravcsik, 'De Gaulle and Europe: historical revision and social science theory', *Center for European Studies Working Paper Series 8.5* (Cambridge, MA: Harvard University, 1998).

8 For explication of sources on de Gaulle, see in particular CFE, p. 178n.

9 But she occasionally seems tempted: 'Whether or not each winning idea necessarily benefited the economies of the countries whose governments accepted them is a matter of judgement or interpretation, not a matter of clear determination. There were after all always critics.' (p. 159).

10 Though there is not space here, a number of other areas might be added, including political socialization, social construction of ideas, and public opinion.

11 Wallace cites the example of French debates over CAP reform as a matter where government officials disagreed fundamentally about the welfare effects of integration. Yet it is fair to say that no major EC policy was, at the time of its creation, recommended by most objective observers – notably academic economists – on aggregate welfare grounds. Certainly the preponderance of the evidence suggests that the decision of the well-intentioned Giscard government, to which Wallace refers, turned on interest group opposition, not aggregate welfare gains. And about the power of French farmers there could be little uncertainty! For details of the Giscard government's calculations, see the sources cited in CFE, pp. 273–4, especially Michael Tracy (ed.), *Farmers and Politics in France* (Enstone: Arkelton Trust, 1991).

12 Also Judith Goldstein and Robert Keohane (eds), *Ideas and Foreign Policy: Beliefs, Institutions, and Political Change* (Ithaca: Cornell University Press, 1993).
13 For sophisticated treatments, see Kathleen R. McNamara, *The Currency of Ideas: Monetary Politics in the European Union* (Ithaca: Cornell University Press, 1997); Paul Krugman, *Peddling Prosperity: Economic Sense and Nonsense in the Age of Diminished Expectations* (New York: Norton, 1994); Alberto Giovannini, 'Economic and monetary union: what happened? Exploring the political dimension of optimum currency areas', in Centre for Economic Policy Research, *The Monetary Future of Europe* (London: CEPR, 1993); Peter A. Hall (ed.), *The Political Power of Economic Ideas: Keynesianism across Nations* (Princeton: Princeton University Press, 1989).
14 James Alt, Jeffry Frieden, Michael Gilligan, Dani Rodrik and Ronald Rogowski, 'The political economy of international trade: enduring puzzles and an agenda for inquiry,' *Comparative Political Studies* 29(6) (December 1996): 689–717.
15 Andrew Moravcsik, 'Why the European Community strengthens the state: international co-operation and domestic politics', *Center for European Studies Working Paper Series No. 52* (Cambridge, MA: Harvard University, 1994).
16 On the perils that befall those who conflate the two lines of argument, rather than treating them as separate, see e.g. Karl-Orfeo Fioretos, 'The anatomy of autonomy: interdependence, domestic balances of power, and European integration', *Review of International Studies* 23(3) (July 1997): 293–320.
17 Indeed, this is not unlike the trade-off made by those Scharpf refers to as the 'analytical narratives club' (p. 166) in focusing on the internal validity of a limited number of cases. Like members of this club, I believe that the strongest support for my empirical results lies in the number of facts, often new and unexpected, about a small number of cases that some theories are able to explain. Accordingly, much of the detailed evidence on the cases – e.g. on the positions of social groups and the distribution of information among international actors – had never before been collected or assembled, and was absent from early drafts of CFE. One difference, as Scharpf notes, is that the 'analytical narratives' school places more emphasis on formal modelling and less on unbiased comparative theory testing, whereas my priorities are in this book the opposite. Which imposes tighter constraint on empirical explanations is open to debate; the answer no doubt varies. Cf. Robert Bates *et al.*, *Analytic Narratives* (Princeton: Princeton University Press, 1998).
18 CFE, pp. 483–5.
19 Andrew Moravcsik, 'A new statecraft? Supranational entrepreneurs and interstate co-operation', *International Organization* (forthcoming). For a preliminary version, see the paper of the same name, *Weatherhead Center for International Affairs Working Paper Series No. 98-10* (Cambridge, MA: Harvard University, 1998).
20 CFE, pp. 50, 477–9. It is important to note that my theoretical formulation is in fact slightly more complex, but thereby more generalizable. The issue is whether issue-specific interdependence or linkage to general foreign policy concerns dominates national preference formation. It is only because the issues with which the EC is concerned are generally (but not always) economic, while general foreign policy concerns are generally geopolitical or ideological, that this formulation generates the expectation that (more focused) commercial interests will trump (more diffuse) political-military motivations. CFE, pp. 26–7, 49–50.
21 This criticism was previously debated in Daniel Wincott, 'Institutional interaction and European integration: towards an everyday critique of liberal intergovernmentalism', *Journal of Common Market Studies* 33(4) (December 1995): 597–609; Andrew Moravcsik, 'Liberal intergovernmentalism and integration: a rejoinder', *Journal of Common Market Studies* 33(4) (December 1995): 611–28. One might extend this analysis by theorizing more rigorously the sources of variation in the strength of geopolitical interest – as suggested by the tendency of Germany to be more influenced by geopolitical concerns than Britain or France. Even more comprehensive, at least in

principle, would be an integrated theory of trade-offs between commercial and geopolitical concerns, rather than a simple recognition of a trade-off between the two – a 'theory of grand strategy'. Wallace's and Caporaso's encouragement notwithstanding, I suspect fine-grained predictions from such models are currently beyond the reach of IR theory – perhaps for very fundamental reasons. For state of the art, see David A. Lake, *Entangling Relations: American Foreign Relations in its Century* (University of California, San Diego, unpublished manuscript, 1997).

22 CFE, p. 1.

23 CFE, pp. 13–17.

24 CFE, pp. 13–17. Of course, it remains an open question to what extent the pattern of national preferences (while mitigated by institutional constraints) remains a, perhaps *the*, decisive factor in daily decision-making or judicial interpretation. Some doubt this, but for evidence (explicit and implicit) of decisive intergovernmental influence on daily decision-making, see Adrienne Héritier, Susanne Mingers, Christoph Knill and Martina Becka, *Die Veränderung von Staatlichkeit in Europa. Ein regulativer Wettbewerb: Deutschland, Grosbrittanien, Frankreich* (Opladen: Leske & Budrich, 1994); Jonathan Golob, 'The path to EU environmental policy: domestic politics, supranational institutions, global competition' (paper presented at the Fifth Biennial International Conference of the European Community Studies Association, 29 May–1 June 1997, Seattle, Washington). For clear arguments that even where supranational institutions and actors are important in daily decision-making, it is appropriate to begin with an intergovernmental explanation, see Mark Pollack, 'Delegation, agency and agenda setting in the European Community', *International Organization* 51(1) (Winter 1997): 99–134; Moravcsik, 'Liberal intergovernmentalism.'

25 Paul Pierson, 'The path to European Union: an historical institutionalist account', *Comparative Political Studies* 29(2) (April 1996): 123–64.

26 CFE, pp. 489–94. A truly rigorous test, it is worth noting, would require sophisticated input–output models of the domestic and international economy.

27 For a more detailed argument, see Andrew Moravcsik, 'Europe's integration at century's end', in Moravcsik (ed.), *Centralization or Fragmentation? Europe Facing the Challenges of Deepening Diversity, and Democracy* (New York: Council on Foreign Relations Press, 1998), pp. 1–58.