

Review section symposium The choice for Europe: Social purpose and state power from Messina to Maastricht

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Andrew Moravcsik (1998) *The Choice for Europe: Social Purpose and State Power from Messina to Maastricht* (Ithaca: Cornell University Press; published in the UK by UCL Press, London, December 1998. ISBN 1 85728 191 8 (hb) £40.00. ISBN 1 85728 192 6 (pb) £14.95.

PIECING THE INTEGRATION JIGSAW TOGETHER

Helen Wallace

Andrew Moravcsik's long-awaited volume assembles a huge jigsaw puzzle. The sharply defined picture of west European integration reveals calculating statecraft by highly strategic key governments to promote the core economic interests of Britain, France and Germany. The picture, drawn on a large canvas, spreads from the early years of the European Economic Community to the Treaty on European Union in Maastricht. No concessions are made to *pointillisme* or impressionism in the execution of this picture, with its careful observation and enriching detail, combined with a grand sweep of unified design.

The result is an unusually wide-ranging analysis of five history-making episodes in the evolution of what is now the European Union (EU). The breadth, depth and variety of primary sources underpin a compelling interpretation of the underlying dynamics of west European integration. This jigsaw is composed of many thousands of pieces, painstakingly sorted – and re-sorted – into a coherent whole. The training in history shows in the emphasis on substantive evidence, while the commitment to robust theorizing about the international political economy permeates the flow of the analysis. The central objective is to contest the sweeping, soft and often sloppy assertions about European integration as either a special form of supranationalism or driven by 'geopolitical ideology'. On the contrary, the process and the outcomes are much more 'normal' (p. 5), in that the representatives of the leading states have for fifty years behaved logically in using the EU to

promote their economic interests. The volume constitutes a formidable statement of liberal intergovernmentalism, which Moravcsik has, himself, done so much to develop. It builds on historical argument – Moravcsik has a great deal in common with Milward as a debunker of carelessly received wisdom. It contributes to international relations theory, by removing the study of west European integration from its idiosyncratic corner and locating it in the mainstream of theorizing about the relationship between the state and the international system.

Much hangs on the attempt to disprove two alternative interpretations of European integration. One is the view that geopolitical ideology explains the process. The other is that some form of supranational entrepreneurship or *engrenage* has been at work, whether as defined by neofunctionalism or as reinterpreted in versions of historical institutionalism. Moravcsik produces vigorous arguments against, and detailed rebuttals of, both camps. He is, for this reviewer at least, more focused and persuasive in his attacks on historical institutionalism than in his onslaught on geopolitics. His targets are clearer in the former case, a lively and productive arena of academic contention. Somehow the rebuttal of geopolitics is more contrived, in aiming at something of a straw man; few authors rely on geopolitical explanation alone. To recognize the primacy of political economy factors need not require us to throw all geopolitical considerations out of the window. Indeed, as Moravcsik himself honestly acknowledges (p. 478), in Germany, a non-trivial example, geopolitical factors seem periodically to have been quite influential. An analysis that weighted, rather than ‘exceptionalized’, the geopolitical component in relation to political economy considerations might have served better.

Thus, in spite of the vigour and richness of the argument, the volume presents a picture, not a photograph. The eye of the artist is selective, capturing some parts of the process more thoroughly than others; and it slides over some of the elements that displease the eye. Or, to put it another way, this is one of those irritating jigsaws (like those hideous ones of only baked beans) where the pieces can be put together in more than one combination; or, even more irritating, a jigsaw with similar, but not identical, pictures on each side. And not quite all the pieces of the jigsaw are on the table.

SOME MISSING PIECES OF THE JIGSAW

One obvious criticism of the volume arises from its tight focus on British, French and German policies. Their relative importance is undeniable in the history of the EU. None the less, other member countries, albeit much less powerful, have played some part in the intriguing form of complex multilateralism that comprises the EU. The pattern of coalition-building that underpinned strategic bargains and which linked the three to the rest might have merited more explicit attention.

A second underdeveloped element lies in the description of how state preferences are formed, a process at the heart of Moravcsik’s analysis. The argument and evidence concentrate on the behaviour of incumbent governmental élites in the three countries and their symbiosis with the interests articulated by leading economic agents. This begs some questions about the nature of politics within the

three countries, about the capacity of incumbent governments to call the shots and to act as gatekeepers, and about the primacy of particular economic agents. I would have preferred a more finely grained assessment of domestic politics. The British story might, for example, have mentioned the weakening influence of peak associations and the privileged relations, during the 1980s especially, of a partisan government with particular firms in the business and financial sectors. More attention could have been paid to the deep political controversies in Britain about the goals and implications of European integration, these being only partly contested over political economy preferences and often over other elements of raw politics. The ousting of Mrs Thatcher from office barely rates a mention in the account of the period leading up to Maastricht. British preferences as regards integration have not been quite so consistent and stable as Moravcsik would have us believe. In France and Germany too those engaged in the wider political arena have not always been quite so convinced that their incumbent governments were making wise policy choices about when and whether to delegate to EU institutions.

A third dog that does not really bark is the voice of comparative politics. The study somewhat too easily identifies similarities in the approaches of the three countries, arguing that differences on particular issues (agriculture, liberalized internal and external trade, European currency, and so forth) are in each case related to consistent differences of economic interest. Surely there is more to it than that. I am myself quite nervous about the assumption so widely found in the available comparative assessments of individual EU member states that there is a more or less standard model for evaluating national responses to integration. Some glimmerings of this concern can be founded in Moravcsik's admission that in some respects the German picture is at variance with the French and British, in embracing more diverse elements of state policy.

A fourth lacuna has to do with absent pieces of evidence. Moravcsik has assembled so much data that it becomes hard to believe that in his 501 pages he might have left the odd stone or two unturned. However, the long-running argument about the British position *vis-à-vis* the Community budget comes out in rather a muddle. It was incumbent British politicians who chose (rightly or wrongly) not to press the budget issue in the accession negotiations, and their successors who in 1974 accepted a Dublin mechanism that manifestly did not solve the problem of budgetary inequity. Rational and well-judged statecraft could surely have produced a more nuanced and possibly more effective negotiating strategy. Indeed the long shadow cast by the subsequent debilitating arguments surely merited more weight in the analysis. One footnote here – the Commission had in 1970 recognized this and of its own initiative calculated alternative figures and sketched a formula that might have avoided the subsequent rows. Why these figures were never fed into the accession negotiation, and how it was that the Commission a few years later categorically blocked as not allowable under EC law a mechanism that was close to that eventually agreed in Fontainebleau in 1984 are episodes that deserve scrutiny to identify Commission influence on these negotiations between governments. The close relationship between the Commission and the British in devising the European Regional Development Fund (ERDF) in 1973/5, not least

the famous ‘Thomson map’ (devised with the lubrication of a good whisky), had more of a role than Moravcsik admits, so concerned is he with linking the ERDF story to the discussion of economic and monetary union and the European monetary system. There is a confusion here of time, events and causality. One confusion is the reference (pp. 350–2) to the eventual British budget solution as a form of ‘generalized *juste retour*’. If only it had been that, the EU would not now be facing such a potentially bruising argument over net national contributions.

MONOCHROME OR POLYCHROME?

Moravcsik’s picture is drawn parsimoniously in black and white only. I agree with the thrust of his argument that domestic politics and domestically sourced preferences explain a great deal about both the delegation of policy powers to the EU and refusals to delegate. Yet to rely quite so heavily on incumbent political élites’ own evidence on their goals and behaviour is to build in something of a distortion. Too much is drawn from the memoirs and witness of these protagonists for my taste. Too little account is taken of the self-justification and self-rationalization of such accounts. Few national politicians or officials are likely to credit the Commission with critical influence or to admit to uncertainty or inconsistency.

Moreover, the asserted hierarchy of decision in three stages – preference formation, negotiation and institutional choice – is too neat and tidy. Of course, actor-centred rationality is more important and more amenable to systematic analysis than many students of European integration have acknowledged. But the implied control of process and outcomes needs more justification than Moravcsik adduces. Space needs to be made for irrationality, for confusion and for mistaken judgements (*vide* here the management of Britain’s policy over the Ioannina compromise in 1994, or the infamous ‘non-cooperation’ policy prompted by BSE in 1996, two events outside the scope of this volume). In brief, a little more nuance, more varied colours, and a little less emphasis on successful rationality would make the core of the analysis more persuasive.

THE PICTURE ON THE OTHER SIDE OF THE JIGSAW

Quibbles apart, does Moravcsik’s overall argument stand up and has he proved his case beyond reasonable doubt? This volume is an extraordinarily powerful and well-substantiated statement of the case for bringing and keeping the state at the centre of the analysis of European integration. But methinks he doth protest too much about the limited and mostly uninfluential role of supranational policy entrepreneurs and transnational coalitions, whether of political or economic agents. My own view is not that supranational entrepreneurs can control or direct the process, but that in some circumstances they can be – and sometimes have been – important catalysts of plausible ideas and brokered agreements. It is simply not the case that in the Council, the Committee of Permanent Representatives (COREPER) or the Special Committee on Agriculture the Commission is uninvolved (pp. 224–5, 306). Indeed, all of us who at one stage or another in our careers have played committee

politics know that often getting the conclusion you want is more important than winning credit for its adoption.

The Thomson map for the distribution of ERDF funds is one example. The never-publicized meeting, convened in the Ardennes by the Commission in December 1983, of British, French and German officials to stitch a deal on mutual recognition is another example. The ability now and again of Commission officials to feed into the Council presidency usefully defining texts for others to claim parentage, even on critical and contentious issues, is not to be discounted so easily. Or, a different kind of illustration, the skill of a behind-the-scenes coalition of big employers to get their text on pensions and the Barber judgment adopted in the Maastricht IGC suggests that forces other than statecraft are sometimes at work. Sure, there are principal-agent relationships in play, but the agency function is sometimes more than responsive.

Two other points might emerge from looking at the picture on the other side of the puzzle. One is that the institutional setting of the EU has interesting impacts on the behaviour of state actors, both constraining their behaviour and providing opportunity structures. In May 1982, when the then British minister sought to invoke the Luxembourg Compromise on an issue of agricultural prices, there was a pause before the result was clear. The Danish and Greek ministers followed the presumed code in supporting the British in principle, as their routine instructions required. The French, omitted on this point from Moravcsik's account (p. 350), sought instead clarifying instructions from Paris; to the surprise of the French on the spot the instruction was, for the first time, to block the application of the formula. The consequential procedural uncertainty was to deter British negotiators from invoking the Luxembourg Compromise even on more serious issues.

The second point has to do with the role of ideology or doctrine. Over the period of EU evolution more of a tussle has occurred between competing economic doctrines than Moravcsik acknowledges. In their turn the common agricultural policy (CAP), the single market and the single currency have been achieved as a particular political-economic doctrine gained a kind of transnational ascendancy at a particular moment. 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 in France of the CAP outcomes sought – and won – by French ministers.

These criticisms notwithstanding, Moravcsik's careful analysis provides formidable arguments against those who portray European integration as necessarily a cumulative and irreversible process. The deliberative and calculated strategies of leading negotiators can just as well be directed at denying or unravelling supranational agreements as at consolidating collective arrangements. The pendulum can indeed swing back towards the individual countries when the costs of co-operation seem too high or the rewards for commitment too uncertain. Credence for this interpretation can be found in recent and current attempts, not least from German negotiators in Amsterdam, to 'repatriate' policy powers (notably in competition policy) or to entrench 'subsidiarity'. Among both practitioners and commentators the arguments will continue, much fortified by this stimulating study.

TOWARD A NORMAL SCIENCE OF REGIONAL INTEGRATION

James A. Caporaso

In *The Choice for Europe: Social Purpose and State Power from Messina to Maastricht*, Andrew Moravcsik provides a new synthesis for the field of regional integration studies generally and European Union (EU) studies in particular. This major new study combines three theoretical strands with five important, history-making case studies (the Rome Treaty, the common agricultural policy, the European monetary system, the Single European Act (SEA), and European monetary union). For all five cases, Moravcsik provides an account of how the preferences of national politicians are formed, drawing on and extending liberal theory. Second, he utilizes negotiation theory to describe and analyse efficient as well as distributive bargaining. And, third, he creatively applies institutional theory to help us understand how bargains are enforced and maintained. Despite great complexity in the realm of the evidence, Moravcsik leaves us with a central message. There is a master variable at each of the three stages. He states the conclusion boldly and efficiently: 'I argue that a tripartite explanation of integration – economic interest, relative power, credible commitments – accounts for the form, substance, and timing of major steps toward European integration' (p. 4).

This is not a harmless book. Many idols are smashed. Neofunctionalism suffers regarding the autonomy, influence, and agenda-setting power of international entrepreneurs. Functionalists will have to concede that transnational society (Monnet's *unite des faits*) does not have the influence that many once thought. Realists will have to give ground regarding the systemic determination of preferences (security externalities explain little). Indeed, Moravcsik argues and adduces strong evidence to show that national interests are produced through a national political process, albeit a process that operates on domestic and transnational economic interests. Regulatory theorists will have to recognize that the EU is decidedly not 'government by information', by experts, by scientists, or by task-oriented specialists, except to the extent that experts are carefully chartered by governments to carry out specific tasks. There are quite simply too many hard distributive bargains in the EU's history to come to the conclusion that the EU is solely a collective gains project. For functionalists, the good news is that the EU is for real, i.e. it counts in a broad variety of ways. The EU is not limited to the management of market imperfections at the regional level. The bad news is that the process is controlled by state politicians.

The strengths of this book are easy to discuss and should not be dismissed too quickly in the interest of getting to the critical comments. Since the book has been a long time coming, the fear was that it would be a patchwork, reflecting incremental adaptation to critics along the way, but diluting itself in the process. What we have instead is a major synthesis, an integration of three major lines of theoretical development in comparative and international politics. To write a book such as *The Choice for Europe* requires the analyst to embrace three daunting questions. First, why do state leaders have the preferences they have? A corollary but still important question has to do with whether central decision-maker preferences are the most

important, in contrast, for example, to the preferences of transnational societal actors. Second, once preferences have been accounted for, how do they (along with other factors such as power) translate into negotiated outcomes? Three, how do leaders get the results of negotiation to stick?

The first question, in some ways the most difficult, is tackled through a sophisticated application of liberal theory, which Moravcsik himself has done a lot to develop in this book and elsewhere. The second question is the province of negotiation theory, a well-developed literature in its own right. Moravcsik skilfully uses this theory and applies it to his cases of non-coercive bargaining. Finally, the third question leads us to institutional theory, particularly the theory of credible commitments. The author takes us to the frontier in each of these bodies of theory, isolates what is essential for his story, and skilfully applies all three.

The second strength of the book is that the scholarship, i.e. the empirical research, is extraordinarily good. Moravcsik has carefully investigated each of his five cases and has judiciously weighed and sifted the evidence. He spends a considerable amount of time informing the reader about his research procedures, lending an air of transparency and reproducibility to how he arrives at his conclusions. He relies on statistical evidence when appropriate, archival material when available, in-depth interviews, the documentary record, biographies, diaries, newspaper accounts, and secondary literature. The case studies cover a wide range, from the formation of the European Economic Community (EEC) to the Treaty on European Union. The important eleven-year period after the EEC came into existence is subjected to a close scrutiny. I am impressed at the depth, thoroughness, and creativity of the scholarship, all the more so because it is not done in the spirit of 'all the facts we can get'. Quite the contrary, the evidence provided is implied by the theories, sometimes distinctively so. I am trying to recall the last time I read a book in the social sciences where the author told me, in great detail, how he would treat the secondary literature in terms of its primary evidence, conclusions, and theoretical claims.

A third strength is that *The Choice for Europe* mainstreams EU studies and regional integration theory. Many scholars consider the EU to be quite distinct and therefore that it calls forth its own genre of research. It is this assumption, perhaps more than any other, that has retarded the growth of cumulative knowledge in this area. Indeed, in descriptive terms, the EU is quite different from other regional associations (it is larger, more institutionally developed, has a greater share of intra-regional trade). However, in Moravcsik's hands, these descriptive differences (across sectors, countries, time periods) are simply the raw material for a general theory. This book helps to locate integration studies within the overall body of knowledge of international relations, comparative politics, and political science, and 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.

The Choice for Europe also prompts a number of critical comments and questions. Let me raise the first point as a question. Do domestic institutions matter? Do differences in institutions among the member states affect the cooperative outcomes and choices analysed? In the abstract, one would think the answer is 'yes'. In a way, the field of comparative politics depends on a positive answer. The most important thing about domestic politics is how a society constructs its institutions and the way these institutions are built is thought to affect political outcomes. Yet Moravcsik's underlying model is one of societal pluralism, particularly economic pluralism. Economic factors, particularly the distribution of commercial and productive interests, work their way up toward the national leaders, who in turn carry these preferences into international negotiations. The results of the negotiations are then locked in by institutions. Presumably the story of regional co-operation can be told with a universal Olsonian pluralism (economic interests plus a logic of collective action), a negotiation analysis driven by the relative cost of non-agreement to the parties, and institutional lock-in. This is the essential story, despite the episodic causality of geopolitical factors, ideology, and the autonomous force of supranational technocrats.

If Moravcsik's basic model is correct, should we draw the implication that differences in domestic institutions do not matter for either the preference formation or negotiation phase of his study? Admittedly, this would be a tough pill to swallow for many comparativists who are inclined to think that differences in the organization of interest groups (pluralist vs. corporatist), political parties (two party vs. multiparty), and executive-legislative relations (parliamentary vs. presidential) make a difference. I would like to have seen this issue addressed in systematic fashion, if not explicitly modelled. I, of course, do not have evidence that the results would be changed, but there are good theoretical reasons for thinking that variables such as the number of veto points, the distribution of domestic political power, and the degree of divided government count.

A second point concerns the logic of case selection and how this bears on the test of rival theories. Moravcsik has chosen to focus on five very important, history-making cases. While this seems reasonable (why not look at the most dramatic cases?), how does it affect the functionalist focus on the repeated, normal, and mundane? We can ask pointedly 'what is a case?' Moravcsik has a meta-theory of cases and case selection. For him, a case is a relatively discrete, bounded event or set of events. While his cases are protracted and have considerable temporal range, they are distinctively not what we would call processes. I do not think that the functionalist alternative would imply cases at all, in the sense used here, of instances where a problem exists, there is serious conflict among the relevant parties, and negotiations to solve problems result in some policy or explicit non-agreement. For evidence, a functionalist would look at the slow accretions in domestic and transnational society resulting from trade, capital flows, movements of workers, capitalists, and tourists, cross-border activities of professional organizations, and so on. Moravcsik is perfectly aware of these methodological differences (indeed, many of the factors I mention are incorporated into the liberal side of his theory) but the point still remains: the selection of cases makes it easier to confirm the

intergovernmentalist story and implies less ability to probe the merits of process-based theories. The difference between intergovernmentalism and functionalism is somewhat redolent of the differences between pluralists and structuralists in the much older community power structure debate, with Moravcsik standing in as the modern Robert Dahl but no one to represent Floyd Hunter and the sociologists.

Two criteria of case selection are internal and external validity. External validity has to do with generalization, i.e. with the extension of research results to data realms outside the ones directly studied. Large-N studies are best equipped to do this. Internal validity has to do with the nature of causal inference. While not completely separate from external validity, it has its own logic. It asks 'how do we know it is X and not Y that is causing Z?' Since Moravcsik is doing a small-N study, what is the best way to sample cases, given his particular meta-conception of cases?

All five cases represent major choices for members of the EU. With the partial exception of the single market, I believe all the cases represent a mix of gains and losses. It is not clear that these cases can be ordered on any underlying continuum so it is also not clear that we should (again with the exception of the SEA) expect differences in outcome. While the results of the case study analyses are therefore descriptively robust (economic factors and government preferences are important), variation in cases does not lead us to expect differences. 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.

Third, the theoretical formulation employed in this book obscures one of the most important theoretical divides in integration theory, that between neofunctionalism and intergovernmentalism. While intergovernmentalism obviously owes a debt to realism, Moravcsik divests himself of realism's albatross of systemic determination of preferences and paves the way for a more modest 'domestic realism' in which government leaders take on the preferences of key domestic groups. The predictions of intergovernmentalism are less bold but more accurate.

The move with regard to neofunctionalism is more controversial. For interpretive purposes, Moravcsik retains only one of two legs of the neofunctionalist approach, the importance of international technocracy and international institutions. He definitionally cuts away the importance of transnational society, severing the link between society and supranational institutions, and reattaches it to liberalism, admittedly in a much more sophisticated form. Neofunctionalism is left standing on one leg (international technocrats without transnational society) while intergovernmentalism acquires a new ally, domestic and transnational society. We have a much more refined analysis of the social, more in tune with modern theories of preferences, but neofunctionalism is impoverished.

Perhaps I should not be disappointed at the failure of an ultimate showdown between intergovernmentalism and neofunctionalism. I must admit the wish is partly for nostalgic reasons in any case. Moravcsik's new synthesis does a better job of theorizing the importance of the economic and the social and he has provided us with a richer and more theoretically productive formulation. His synthesis is not a

casual eclecticism but rather a reconceptualization of the older terms of debate in which he upgrades previous insights in light of modern theory, and places regional integration studies, of which the EU is a part, within the fields of comparative and international political economy. It will be hard to please both EU specialists and theoretically minded students of comparative politics and international relations. Indeed, Moravcsik intends to change our minds about the way many of us look at the relations among these fields. We should read the book carefully and grapple with the issues raised by it. It may be the most significant book on the subject since Haas's *The Uniting of Europe*.

SELECTING CASES AND TESTING HYPOTHESES

Fritz W. Scharpf

This book has been long in coming, after so many previews, colloquia, seminar presentations, and papers based on the underlying research. Now that it is finally here, 500 pages of text, there is a great sense of satisfaction in seeing so much of this material integrated into a well-organized and well-written opus that backs up earlier arguments by impressively researched historical case studies of the substantive preferences, bargaining strategies and institutional arrangements pursued by France, Germany and the United Kingdom in five 'constitutional' negotiations that have shaped the process of European integration. Somehow, however, the excitement that one had been prepared for isn't quite there – perhaps because some of the most controversial and most interesting propositions of these earlier papers were simply left out here.¹ But this in no way detracts from the value of this important book.

Moravcsik presents the most complete, theoretically disciplined and methodologically self-conscious historical account yet available of the antecedent conditions, bargaining processes and outcomes of five intergovernmental negotiations that have shaped European integration – the original Treaties of Rome, the settlement of fundamental disputes preceding the establishment of the common agricultural policy in the 1960s, negotiations about the co-ordination of monetary policy in the 'Snake' and the European monetary system in the 1970s, the Single European Act in the 1980s, and the Maastricht Treaty and European monetary union in the 1990s. Even more remarkably, these case studies, each of them amounting to about seventy pages, are used to 'test' a small set of competing hypotheses, all with respectable pedigrees in the theoretical literature on European integration. Moreover, the historical descriptions are extremely well documented, with an emphasis on primary sources and a critical awareness of the limitations of secondary sources.

The hypotheses derive their salience from highly generalized, theory-oriented controversies within the subdiscipline of international relations: Who are the relevant actors, supranational organizations or *national governments*? What shapes their preferences, geopolitical or *economic interests*? What explains the outcomes of distributional conflicts, international institutions or the *relative bargaining power*

of the states involved? What explains the delegation of decision-making authority to supranational institutions, federalist ideology, the superior information efficiency of international organizations, or the interest of governments in assuring the *credibility of mutual commitments*?

As the author repeatedly reminds the reader, it is one of the strengths of this book that it explicitly specifies and tests *competing* hypotheses. It must also be said that, although there is never any question of the author's preference for one answer to each of his questions (always the last-mentioned one in the above listing), all the hypotheses are fully elaborated and presented with great care to suggest their *prima-facie* plausibility. The same is true of Moravcsik's treatment of the evidence in the case studies. Their length is mainly owing to the fact that he bends over backwards in searching for, and then disproving, suggestions that might favour one of the hypotheses that he is determined to reject. There is never any doubt, nevertheless, that in the end they will be rejected; in fact, the book reads very much like an *ex-parte* brief against most of the existing literature.

But that is not meant to suggest that Moravcsik's conclusions are unconvincing. If anything, one might say that – given his selection of cases – most of his preferred hypotheses have such a high degree of *a priori* plausibility that it seems hard to take their competitors quite as seriously as he does. Since only intergovernmental negotiations are being considered, why shouldn't the preferences of national governments have shaped the outcomes? Since all case studies have issues of economic integration as their focus, why shouldn't economic concerns have shaped the negotiating positions of governments? And since only decisions requiring unanimous agreement are being analysed, why shouldn't the outcomes be affected by the relative bargaining powers of the governments involved?

The only one of Moravcsik's hypotheses with which I am tempted to take issue is the last one regarding the choice of an implementation mechanism for agreements reached through intergovernmental bargaining – the choice being between self-implementation by the negotiating governments (requiring the resolution of disputes through further negotiations and unanimous decisions in the Council), or a 'pooling of sovereignties' (allowing future decisions to be reached by qualified majority vote in the Council), or finally the 'delegation' of decision powers to the Commission and the European Court of Justice. With regard to the last two options, Moravcsik claims that the choice is explained by a concern for the credibility of commitments in the face of ubiquitous temptations to defect. That explanation presupposes that the problems governments face in the implementation stage are in the nature of a (perhaps asymmetrical) Prisoner's Dilemma constellation. That is plausible enough for some issues – mainly in the fields of 'negative integration', competition policy and government subsidies, where the common interest in having open and undistorted markets needs to be safeguarded at the implementation stage against the ubiquitous protectionist temptations of individual member states.

But delegation seems also a plausible device in policy areas in which the temptation to defect plays no role at all. This is true of a wide range of non-tariff

barriers that were to be removed in consequence of the Single European Act. In this context, the harmonization of product standards was delegated, on the basis of rudimentary specifications by the Council, either to the Commission and its elaborate 'Comitologie' (that is not discussed at all in the book) or to non-governmental standard-setting bodies. Here the constellations of national interest resemble the battle of the sexes, rather than the Prisoner's Dilemma – implying that it is in everybody's interest to apply a common standard once it is agreed upon. Thus the justification for delegation is not the fear of defection, but the high transaction costs associated with intergovernmental negotiations in a situation where countries differ in their preferences for specific definitions of a common standard. That justification, however, seems relatively close to the 'information efficiency' hypothesis that Moravcsik rejects.

The reason, I suggest, is not an insufficient understanding of game-theoretic distinctions. While Moravcsik does not aspire to membership in the 'analytical narratives' club, he has a good intuitive grasp of analytical approaches, and he is clearly much too sophisticated simply to assume that all the world is a Prisoner's Dilemma. Rather, the reason seems to be an overly strong commitment to the idea that the social sciences should produce *general* theories about empirical *regularities*. In this quest, theories that apply without exception are to be preferred to 'sometimes true theories' whose conclusions depend on contingent antecedent conditions; and studies that are able to demonstrate a universal regularity are considered more successful than studies producing explanations of significant differences among superficially similar cases. In general, I see little point in criticizing such methodological predilections (which may also be reinforced by membership of a particularly theory-happy sub-discipline of political science) – what matters is what we can learn from a study. But since Moravcsik pushes his methodological views with so much indignation against students of European integration who have followed different drummers, it is perhaps appropriate to discuss the price he is in fact paying for his choices.

First, the preoccupation with general theory tends to impede theoretically interesting discoveries. 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. The example discussed above points to such opportunity costs. Others include the under-theorized discussions of the 'exceptionally' strong role of the Commission in defining the agenda for the Single European Act, or of the institutionalized capacity of the Bundesbank to influence the negotiating positions of the German government in the 1970s and the 1990s in ways that were at odds with the interests of industrial producers (which according to Moravcsik should 'generally' control government preferences).

More subtle but equally costly is the influence which the universalistic aspirations seem to have on case selection. In the ongoing debate between 'intergovernmentalist' and 'supranationalist' interpretations of European policy choices, most authors (including Moravcsik in some of his earlier contributions) have focused on a wide range of decisions, some of 'constitutional' significance,

some of a more 'everyday' variety. The attempt to settle these controversies with a definitive study that focuses only on decisions of constitutional significance may appear a bit tricky unless care is taken to theorize the distinction between the two classes of decisions, and the reasons why and how that distinction should make a difference. Moreover, the focus of the present book is restricted to those constitutional decisions that have been reached by way of intergovernmental negotiations, while constitutional changes reached through Commission strategies and Court decisions (which have constitutional force when they interpret the Treaty) are completely excluded from consideration. Thus, we do learn that demands for the liberalization of transport policy ran into 'complete deadlock' in the negotiations of the 1960s (p. 217), and that France and Germany remained opposed to it in later rounds of Treaty negotiations as well – but we learn nothing about the 'supranational' processes and mechanisms through which air, road and rail transport (or telecommunications, postal services and electric power, for that matter) were nevertheless brought under the rule of European competition law in the late 1980s and the 1990s. In other words, the generality of confirmed hypotheses is in part bought by the exclusion of 'deviant' cases – which would, of course, have to be accounted for in a fully specified *general* theory.

I would not have dwelled on these points if Moravcsik did not insist so much on the superiority and uniqueness of his own approach.² In any case, they should not overshadow my conviction that this is a very good book. Its empirical chapters are so rich in well-documented detail and insightful interpretation that their intrinsic value far exceeds the function they have in supporting or disconfirming the few and somewhat thin theoretical hypotheses they are supposed to test. Moreover, both the introduction and the first theoretical chapter contain a wealth of theoretical and methodological insights that have, in fact, not yet been grasped by many students of European integration. To mention just two: it is indeed true that the causal influence of an actor, say, the Commission, on negotiating outcomes cannot be inferred from the observable activities of that actor, but must be assessed in the context of an explicit bargaining theory that takes account of the structure of asymmetrical interdependencies among the actors involved (pp. 9, 53–4). Even more important is the clarity with which Moravcsik has defined the concept of preferences as 'an ordered and weighted set of values placed on future substantive outcomes' (p. 24). The concept, in other words, is meant to describe relatively stable criteria of evaluation, rather than the contingent choices among alternative courses of action that may be more or less useful for reaching preferred outcomes. If this were generally understood and accepted, many confusing controversies over the need to 'endogenize preferences' in rational choice approaches and to allow for the possibility of 'policy learning' could be avoided.

But these are merely appetizers – there is so much more food for thought in this book, as well as an immense wealth of historical information, that no serious student of European integration can afford to ignore it. Highly recommended.

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, Jeffrey 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, Großbritannien, 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.