



# Is something rotten in the state of Denmark?' Constructivism and European integration

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It has been twelve years since Alexander Wendt's article on the agent-structure problem signaled the advent of a self-conscious 'constructivist' theoretical approach to the study of world politics (Wendt 1987). Wendt, to be sure, has consistently presented constructivism not as an international relations *theory*, but as an ontology – a social theory. Yet he and other constructivists have nonetheless long claimed that their ontology facilitates the development of novel mid-range theoretical propositions.

This is a felicitous claim, for it promises to expand the debate among fundamental theories of world politics. Currently there are three. Realism highlights the distribution of resources. Institutionalism highlights the institutionalized distribution of information. Liberalism highlights the distribution of underlying societal interests and ideals as represented by domestic political institutions. The advent of constructivism promises to add a wider and perhaps more sophisticated range of theories concerning the causal role of ideational socialization.

For such theories, the European Union (EU) is as promising a substantive domain as we are likely to find. In few areas of interstate politics are ideals so often invoked, identities so clearly at stake, and interests so complex, challenging, and uncertain. In few areas is so much detailed primary data, historical scholarship, and social scientific theory available to assist analysts in tracing the role of ideas and the process of socialization. It is thus no surprise that there has been for some years an emerging constructivist analysis of European integration in security studies. This approach is often referred to as the 'Copenhagen school.' It is so named because the force of continental constructivist theories appears to radiate outward from the Danish capital, where it is the hegemonic discourse.

Thomas Christiansen, Knud Erik Jørgensen, and Antje Wiener, the editors of this special issue, do us an important service by posing an intriguing and timely question: What has constructivist theory contributed to our social scientific understanding of the EU? In doing so, they have commissioned a fascinating set of articles most notable for their intriguing conjectures about the possible role of collective ideas and socialization in European integration. Ben Rosamond, for example, openly questions whether a compelling economic justification for internal

market liberalization ever existed. Martin Marcussen, Thomas Risse, *et al.*, seek to rewrite the history of integration in terms of ideational shifts, rather than the succession of economic opportunities most analysts invoke. Thomas Diez reinterprets the first British bid for membership in the EEC as the action of a country caught in its own evolving discursive net. These are bold claims, and there are many more in this volume.

Despite high hopes for constructivism – hopes that any open-minded social scientist in the field must share – and the intriguing nature of some of the empirical claims above, however, my conclusion in this comment is a sobering one. Constructivists, to judge from the volume, have contributed far less to our empirical and theoretical understanding of European integration than their meta-theoretical assertions might suggest – certainly far less than existing alternatives. This disappointing finding may simply reflect the modest role of ideas in the process of European integration, but I doubt it. My analysis of this volume suggests that the true reason lies instead in a characteristic unwillingness of constructivists to place their claims at any real risk of empirical disconfirmation. *Hardly a single claim in this volume is formulated or tested in such a way that it could, even in principle, be declared empirically invalid.*

This failure to test stems fundamentally from the near absence of two critical elements of social science, each designed to put conjectures at risk: (1) distinctive testable hypotheses, (2) methods to test such hypotheses against alternative theories or a null hypothesis of random state behavior. Today most leading constructivists are committed to the proposition that their claims must be, in one way or another, subject to empirical confirmation – and, more important, disconfirmation. Most, including authors in this volume, accept that claims derived from constructivist-inspired theories compete with and should be tested against other mid-range hypotheses. This development is to be warmly welcomed, for it creates a common conceptual, methodological, and theoretical discourse among proponents and critics of constructivist theories alike.<sup>1</sup> It is in this spirit of internal, constructive criticism – that is, criticism of constructivism for failing to live up to its own publicly acknowledged standards – that I write.<sup>2</sup>

## CONSTRUCTIVISM AND EUROPEAN INTEGRATION: ARE THE PROPOSITIONS TESTABLE?

The constructivist approaches to European integration represented in this volume, in my reformulation, share two core propositions. The first is that *governmental elites choose specific policies, policy ideas, strategies, and concrete interests because they (or their justifications) are consistent with more general, deeper, collectively held ideas or discourses.*<sup>3</sup> What is distinctive about this claim, it is essential to note, is not that interests are ‘constructed’ in ‘historically specific circumstances.’ Nearly all international relations (IR) theories, indeed nearly all social science theories, rest on the premise that actor policies, strategies, and even preferences emerge out of interaction with the external environment and, moreover, such interaction varies across time and space in response to complex social interaction. What is distinctive here is

instead the claim that governmental élites calculate on the basis of consistency with collective ideas or discourses irreducible to material interests.

Whence do these ideas and discourses come? The second core proposition shared by nearly all participants in this volume states that *underlying ideas and discourses change only at rare 'critical junctures,' which arise in response to political crises*. In the interaction with the political world, policies may be perceived to fail, meaning that they may be perceived as inappropriate to the social circumstances. This inappropriateness may be perceived for instrumental reasons (the policies do not generate appropriate outcomes) or more sociological reasons (independent of substantive consequences, the policies are not those which other actors expect or appreciate).

The social scientific challenges facing those who seek to move from these two meta-theoretical claims to testable mid-range theory are clear. With regard to the first proposition, constructivists must specify concrete causal mechanisms through which the process of choosing policies and defining interests takes place, with the ultimate goal of saying something about *which* ideas and discourses influence (or do not influence) *which* policies under *which* circumstances. With regard to the second proposition, constructivists must seek to specify concrete causal mechanisms that help to explain *which* political crises lead to a change in *which* ideas and discourses under *which* circumstances. Propositions of this kind are testable.

How do the participants in this volume seek to meet these two theoretical challenges? What testable hypotheses result? In this section I seek to demonstrate that many articles in this volume (despite their stated intent) advance no testable propositions at all, while many others advance testable propositions that are in no way distinct to constructivist theory.

Let us begin with those authors who advance theories that are *in principle* indeterminate and, therefore, untestable. These claims are not merely under-specified; they predict behavior that is *contradictory or in principle indeterminate* behavior *and* tell us nothing about how the contradiction and indeterminacy should be resolved. They are therefore in principle untestable. Space permits only two examples: the work of Rosamond, and Marcussen, Risse *et al.*<sup>4</sup>

Rosamond addresses the first theoretical challenge listed above, namely to specify the relationship between ideas and policy. In an explicit challenge to theories that explain European integration as a response to rising economic interdependence, Rosamond advances two central hypotheses. First, (see p. 656) 'the deployment of ideas about globalization has been central to the development of a particular notion of European identity among élite policy actors.' Second, "'globalization" remains contested within EU policy circles.' These two claims provide, at first glance, an intriguing speculation about where we might begin to look for the sources of state policy.

Taken together, however, Rosamond's two claims remain *in principle* indeterminate and therefore cannot be tested in any way, because between them they subsume the entire range of possible state behavior. Anyone, including Rosamond, seeking to assess the validity of these claims necessarily must resolve one fundamental theoretical issue. Should we expect any given situation to be a

case of ‘the development of a particular notion’ (i.e. convergent views), a case of ‘contestation’ (i.e. divergent views), or some combination of the two? Absent a more precise specification, *any observed outcome* – except, perhaps, a policy debate with no reference to globalization or no contestation at all, which we would know to be false simply by glancing at the *Financial Times* – is ‘explained’ by this theory. It is inevitable, if a tribute to the author’s honesty, that Rosamond’s concluding summary is fundamentally indeterminate.

[Globalization] is used to signify external realities which define the EU’s environment [but] is understood as having multiple and often contradictory consequences . . . this pattern is not uniform and the evidence suggests that different clusters of actors can deploy the idea of globalization with quite distinct effects.

(pp. 666–7)

This in turn leads Rosamond to paper over perhaps the most intriguing and important question in modern studies of globalization. ‘The evidence,’ he concludes, ‘suggests that there is widespread adherence to neo-liberal conceptions and that globalization appears as either/both (a) a structural fact associated with the development of circuits of capital, production, trade and technology or/and (b) a set of policy preferences for economic openness and market-driven policies of budgetary restraint’ (p. 666). The relative weight of these two factors has been the subject of articles and books. Rosamond restates rather than resolves this fundamental theoretical issue.

The second example is the article of Marcussen, Risse *et al.*, who address both theoretical challenges. On the first major challenge, the relation between deep ideas (‘identities’) and policy (or policy ideas), they remain vague:

We do not promote an ‘interest vs. identity’ account, but try to figure out the precise way in which both interact. On the one hand, embedded identity constructions, mentioned above, define the boundaries of what élites consider to be legitimate ideas – thereby constituting their perceived interests. On the other hand, perceived interests define which ideas political élites select in their struggle for power among those available to actors. The precise relationship remains a matter of empirical study.

(p. 617)

To be sure, it is always prudent to remember that the world contains more complexity than any single theory can encompass – a point to which I shall return below. Marcussen, Risse, *et al.* are also quite correct to insist on the need for empirical analysis. Yet theoretical innovation and empirical testing requires that we focus on specific causal mechanisms. In this context, the ‘precise relationship’ between ideas and interests or policies is not simply a matter for ‘empirical study.’ Instead, as Marcussen, Risse *et al.* concede elsewhere, *it is (or ought to be) one of two central theoretical issues in the constructivist research program.* Without a theory of the

interaction between ideas and interests, it is not possible to generate hypotheses that distinguish views based on interests or institutions, and thus it remains impossible to confirm or disconfirm any one or combination of them. 'Empirical study' on this basis is of questionable utility, since any observation would confirm the underlying theory.

Marcussen, Risse *et al.* also address the second theoretical challenge, namely to explain why ideas and discourses change or remain stable in particular circumstances – with equally indeterminate results. The only attempt at a theoretical answer I can discern is buried in the final note, where they observe:

When old visions about political order remain unchallenged, they tend to become increasingly embedded in national institutions and political cultures, as a result of which they become difficult to deconstruct and to replace. . . We will not be able to expand on this point in this article, but we have a broad range of institutions in mind, such as the media, the educational system, the electoral system, the legal system, political decision-making procedures, etc. What they have in common is that they tend to consolidate and reify existing and consensually shared ideas about just political order.

(pp. 630, 633)

This construct evades theoretical analysis. Surely it is *prima facie* untrue as a general proposition that education, elections, law, and the media invariably have a conservative effect on existing social practices. (This is almost precisely the *opposite* of what conservative thinkers and professional sociologists alike have traditionally believed about liberal democratic societies, where their dynamics are often highly destructive of underlying social norms.) The only way we could know when the effect of these institutions is conservative and when it is dynamic would be to specify a theory of such socializing institutions. One would expect – and the authors appear to agree above – that this is the proper direction for constructivist theory. Yet their note tells us next to nothing about what such a theory or theories would look like. They neither set forth testable hypotheses, nor lay the theoretical foundation for the development of such hypotheses. Instead, they restate – albeit in an impressively sophisticated way – the basic theoretical problem.

Why so few testable propositions? Though some of the meta-theoretical speculations in this volume imply the opposite, there is no reason why claims about the 'constitutive' effect of ideas should be difficult to test. One piece of evidence for the ease with which hypotheses can be derived is the existence of promising propositions scattered throughout this volume. Two examples come from Jeffrey Checkel's analysis – in many respects a refreshing exception in its willingness to directly engage theoretical, as opposed to meta-theoretical, questions. Checkel advances at least two such distinctive and potentially testable propositions.

The first is that *an individual's specific policy ideas are most likely to change when other ideas are held by 'authoritative' members of an 'in-group' to which the persuadee belongs or wants to belong.* Institutional hierarchy imposes ideational conformity as a quid pro quo, implicit or explicit, for membership. Obviously this

notion would require more elaboration, yet it points us in a clearly focused direction. We can easily imagine measuring the membership in in-groups (or the desire to do so) independently of ideas and tracing through the political consequences.<sup>5</sup>

A second causal proposition advanced by Checkel and others, most notably Marcussen, Risse *et al.*, is that *influential ideas about political order remain stable unless 'challenged' by a 'crisis.'* This is, in fact, the most common proposition found in the volume; hence I have treated it above as a core assumption. To be sure, at this level of abstraction, the claim is underspecified to the point of near-tautology. We can always find some sense of dissatisfaction, something that could be called a 'crisis,' motivating a change in ideas. And surely not all things that could be termed 'crises' lead to changes in relevant ideas. More precise specification is required for this insight to be useful. Still, the notion that crises are connected with change is *not*, in contrast to some of the claims we considered above, internally contradictory. We can imagine a more precise definition of crisis and a more precise specification of causal mechanisms that might generate testable causal propositions. Such work should be encouraged.

## CONSTRUCTIVISM AND ITS CRITICS: WHERE ARE THE THEORETICAL ALTERNATIVES?

The assertion of a causal connection between crisis and ideational change has a second and more fundamentally troubling characteristic, however, in addition to its abstract character. *It is in no way distinct to constructivism.* Indeed, it is somewhat hard to see why it should be considered constructivist at all. To understand this criticism, it is instructive first to consider briefly the alternatives to a constructivist analysis of ideas.

Rationalist theories of integration, like rationalist theories of IR (realism, liberalism, and institutionalism, etc.), do not maintain that actors in international affairs have no ideas at all, as some authors in this volume would seem to imply. Collective ideas are like air; it is essentially impossible for humans to function as social beings without them. They are ubiquitous and necessary. In this (trivial) sense there is little point in debating whether 'ideas matter.' *Existing rationalist theories claim only something far more modest, namely that ideas are causally epiphenomenal to more fundamental underlying influences on state behavior.*<sup>6</sup>

To see precisely what this implies, consider, for example, a liberal inter-governmentalist (LI) analysis of European integration. Such an account seeks to explain decisions for and against deeper European integration in terms of three factors. These are: (1) underlying economic interests, with geopolitical ideas playing a distinctly secondary role; (2) relative power, understood in terms of asymmetrical interdependence; and (3) the need for credible commitments to certain policies, with ideology playing a distinctly secondary role (Moravcsik 1998). This explanation does not deny that individuals and governments have ideas in their heads or that we should observe them espousing ideas consistent with rational interests and strategies. It denies *only* that *exogenous* variation in other sources of those ideas decisively affects ideas and therefore policy. In sum, in the LI account of

integration, ideas are present but not causally central. They may be irrelevant and random, or, more likely, they are important transmission belts for interests. In the latter case, they are endogenous to other underlying factors.<sup>7</sup>

One important implication is that *both an LI theory and constructivist-inspired theory* predict some correlation between collective ideas and policy outcomes.<sup>8</sup> What distinguishes rationalist and constructivist accounts of this correlation is *not*, therefore, the simple fact that state and societal actors hold ideas consistent with their actions, but the causal independence of those ideas – their source, variation, and the nature of their link to policy. *Hence the minimum we should expect of any effort to test constructivist claims is not just the derivation of fine-grained empirical predictions, examined above, but also the utilization of methods capable of distinguishing between spurious and valid attributions of ideational causality.* In short, studies that seek to show the impact of exogenous variation in ideas must be controlled for the causally epiphenomenal or ‘transmission belt’ role of ideas. In a social scientific debate, this is the minimum that proponents of a new theory owe those who have already derived and tested mid-range theories.

The articles in this volume, I submit, do little to meet this minimum methodological standard. Returning to the argument above, one example is the proposed link between political crisis (or policy failure) and changes in ideas – a link central to almost every article in the volume. This relationship is *precisely* what an LI account would predict. Indeed, one might argue that *only* an LI account generates such a prediction. Why should real world events undermine the confidence of decision-makers in their ideas if those ideas are not meant to be serving underlying instrumental purposes? And if they are so intended, why is this causal argument presented as an alternative to, rather than a confirmation of, traditional theories of integration and international political economy? In this regard, it is striking that the number of *purely* sociological (or even clearly ideational) claims about variation in fundamental discourses and state behavior in this volume is surprisingly low. Instead, we tend to see extensive, if somewhat *ad hoc*, recourse to rationalist and materialist (or formal institutionalist) causes – a tendency I shall document in a moment.

This dependence on (or, at the very least, ambiguity with respect to) the predictions of existing rationalist and materialist theories is disguised in part by the tendency of authors in this volume to misspecify alternative theories in a way that renders them little more than straw men. Such obfuscation is surely not deliberate, but the result is nonetheless to make it almost impossible to disconfirm constructivist claims, since the stated alternatives are absurd or, in some cases, not theories at all. This undermines our confidence in the resulting empirical analysis. The editors of the volume go even further, seeking to make a virtue of this by seeking to demonstrate – unconvincingly, in my estimation – that *only* constructivist theory can explain many aspects of integration.<sup>9</sup> This tendency to reject alternative arguments without testing them takes a number of different forms.

The simplest way to reject alternative theories without testing them is to restate them as ideal types, rather than theories – that is, as constructs that do not explain variation in state behavior. Any variation – of course there is always variation – can

thus only be explained by constructivist theory, which carries the day by default. We see this methodological move in the articles of Kenneth Glarbo and Marcussen, Risse *et al.* Glarbo asserts that:

When subjected to theoretical analysis, European political co-operation has traditionally been the prerogative of realists . . . however diverse in appearance, [the realist narrative] can be reduced to one ‘hard core’ hypothesis, from which all the realist theoretical statements of EPC/CFSP are derived . . . the interests of single European nation states will eternally block integration within the ‘high politics’ realms of foreign, security and defence policy.

(p. 634)

This is manifestly incorrect as a statement about realism, which has in fact generated a number of highly refined theories of alliances. The most charitable thing that could be said is that it selects out of that extensive and sophisticated literature the *least* interesting and *least* plausible alternative hypothesis – namely a static ideal-type of non-cooperation. With this as the only alternative – for Glarbo ignores entirely institutionalist, liberal, and more sophisticated realist theories of alliances and collective security, as well as synthetic approaches like that of Stephen Walt – a constructivist theory need only explain some variation from ‘eternal’ non-cooperation to be ‘proven’ correct. If a fact already known to all – namely that the EU has taken some modest steps toward common foreign and security policy (CFSP) – settles the issue, why bother with empirical analysis?

Consider next Marcussen, Risse *et al.*’s article. These authors also do not take neo-functionalism and liberal intergovernmentalism seriously enough even to test them. They ‘reject [both] out of hand’ in the space of exactly seven sentences. This they do, moreover, by misspecifying each as an ideal-type assuming static, constant behavior, rather than as a theory – just as did Glarbo. LI gains the following treatment, *quoted in its entirety*:

[O]ne could deduce from intergovernmentalism – either its realist (Hoffmann 1966) or liberal versions (Moravcsik 1993, 1997) – that European integration should not affect nation state identities, since the European polity consists of intergovernmental bodies which do not require much loyalty transfer to the European level. The French and the German cases appear to contradict this argument.

(p. 627)

No effort is made to consider a more (if still minimally) sophisticated formulation of LI in which shared identities and symbols are correlated with policy but are epiphenomenal – as I have suggested above. Nor is any effort made to provide evidence of the causal importance of loyalty changes, beyond a conventional history of changing ideas about Europe over the past half century. Neo-functionalism gains the same cursory treatment – it purportedly predicts constant change and is therefore rejected. Constructivism prevails by default rather than by surmounting the challenge of honest empirical validation.



A more direct way to reject plausible alternatives without an objective empirical test is simply to ignore them entirely. We see this in the work of Diez, who maintains that new policies are more likely to occur if they are consistent with the underlying assumptions of prior ones – their language, symbolism, and images. An example is Diez's bold, parsimonious causal account of why the British applied to join the EC in 1961: '[T]he language of a free trade area in the British case facilitated the move towards the articulation of an economic community that would otherwise have been much harder, if not impossible' (p. 608). This is a refreshingly concise claim, yet Diez makes no effort whatsoever to substantiate it.

In particular, the unsuspecting reader would have no inkling that the existing literature contains at least two rationalist explanations far more strongly supported by the archival record. One is that Harold Macmillan was influenced by further relative economic decline and the rejection of his efforts to mediate between the superpowers, visible by 1960 (Kaiser 1996). The other is that Britain, skeptical of supranationalism and wary of any preferential trading area in agriculture, first attempted to negotiate its preferred policy, the free trade area (FTA), and, when it failed, sought the more onerous European Community (EC) in order to avoid economic and geopolitical isolation (Moravcsik 1998). Either renders linguistic and symbolic influences epiphenomenal. If language and symbolism also shifted, traditional accounts presume, it was because the government, business, and political parties were justifying self-interested policies that grew more pro-European over time. Perhaps they even manipulated the debate. British policy change was a strategic adaptation to new circumstances, rather than a shift in the deep structure of British values and preferences.

A simple empirical test can help to determine whether Diez's account or one of the traditional accounts is more accurate.<sup>10</sup> If the rationalists are correct that British policy was a strategic adaptation, not a fundamental transformation, British politicians and officials should have been able to foresee and plan for the future scenario. They should have understood *even before the FTA was proposed* that the failure of some commercial accommodation with the Continent would force a membership bid. If the alternative was linguistically and symbolically unthinkable, we should observe no such foresightedness. Unfortunately for Diez's claim, we observe the former. British officials argue as early as 1956 that if the EC negotiations succeed and any British alternative fails, Britain will soon be forced to join the EC. There is, moreover, considerable evidence that Macmillan, like subsequent leaders, considered elite and public opinion a constraint to be manipulated. One former top British adviser once told me that 10 Downing Street's working assumption was that public opinion could be moved to support any European initiative in eighteen months. Certainly Macmillan took this view.<sup>11</sup>

One could cite many other examples, but the central point is clear. Constructivists in this volume do not test their claims against plausible alternatives.

## CONCLUSION: 'TO WHAT ISSUE WILL THIS COME?'

Given the multitude of citations to the likes of John Searle, Anthony Giddens,

Alexander Wendt, and other social theorists, it would seem perverse to criticize constructivists for being insufficiently theoretical. Yet this volume reveals just that. We see a striking unwillingness to set forth distinctive mid-range hypotheses and test them against the most plausible alternatives in a rigorous and objective way. There is not a point in this article – with the single exception, perhaps, of Fierke and Wiener’s claim about NATO and the EU, now to appear in issue 6:5 – where one sensed that a claim by the author is in any danger (even in the abstract) of empirical disconfirmation.

This reticence to place empirical claims at risk cannot be explained as a function of the empirical material itself. Surely few domains are more promising than the study of ideas in the process of European integration. Even the most materialist explanations of European integration – such as those advanced by Alan Milward and myself – concede an important secondary role for ideas (Milward 1993). Nor can this unwillingness to test clear hypotheses be a function of the novelty of the claims being advanced. Surely few topics have been as extensively researched and subtly illuminated as the role of ideas in European integration – whether by historians like Walter Lipgens, political scientists like Stanley Hoffmann (1974), or practitioner-scholars like François Duchêne (1995).<sup>12</sup> Nor can this unwillingness be a function, as the editors imply in their introduction, of fundamental philosophical (ontological or epistemological) rejection of hypothesis testing. In fact when the authors in this volume turn to empirical analysis, they prove philosophically conventional, aspiring to test theories by presenting decisive evidence and so on. Nor, finally, can this unwillingness to test theories rigorously result, as authors in this volume repeatedly claim, from the inherent difficulty of testing ideational or sociological claims. Surely, as we have just seen in the case of Diez’s analysis of Macmillan, the empirical material often lends itself to straightforward and decisive empirical tests easily within the grasp of anyone minimally acquainted with the archival and secondary sources. If this volume contains few such tests of competing theories, it is not because they are inherently difficult, but because authors chose not to conduct them.<sup>13</sup> Why?

The editors of this volume have an answer: Not enough meta-theory. They write in their introduction that the discovery of ‘promising avenue(s) for future integration research’ has been hampered by the lack of ‘suitable meta-theoretical perspectives.’ We need, they argue, ‘heightened awareness of the implications of meta-theoretical positions.’ Get the meta-theory right, they promise, and empirical theorizing will be ‘important and fruitful.’ This special issue provides a useful test of this claim. By my estimation, fully 50 percent or more of this volume is given over to meta-theoretical analysis, rather than theory or empirics – just as the editors recommend. A panoply of arguments drawn from ontology, social theory, epistemology, and philosophy of science are deployed. Yet the resulting empirical propositions are few, relatively conventional, and barely tested.

Perhaps, then, an opposite view is worth considering, namely that *meta-theory is not the solution but the problem*. Philosophical speculation is being employed not to refine and sharpen concrete concepts, hypotheses, and methods, but to *shield* empirical conjectures from empirical testing. Meta-theoretical musing does not

establish but evades points of direct empirical conflict between sophisticated rationalist and constructivist theories. Abstract discussions of competing modes of positivism, ideational causality, rationalist explanation, the relationship between agents and structures, often serve as principled excuses for *not* engaging in competitive theory testing. At the very least, such speculation expends a great deal of time, effort, and space that might have been devoted to the elaboration of concrete concepts, theories, hypotheses, and methods.

All this distracts constructivists from the only element truly essential to social science: the vulnerability of conjectures to some sort of empirical disconfirmation. Only if one's own claim can be proven wrong are we able to conclude that it has been proven right. In this personal modesty and relentless skepticism toward the conjectures of any single scholar lies the real power of social science as a collective enterprise. Yet very few, if any, empirical propositions in this volume, I have sought to show, are advanced in this spirit or meet this standard. When constructivists 'wax desperate with imagination,' is it therefore not the responsibility of outside observers – most especially those, like myself, who wish the enterprise well – to ask, like Horatio and Marcellus watching Hamlet follow the ghost: 'To what issue will this come?'

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## NOTES

- 1 Five or ten years ago, even this minimal implicit commitment to theory testing – theories should be distinctive and tested against other theories – might have elicited spirited rejection. Yet leading constructivists have since broken with postmodernism and its rejection of any objective standards for empirical theory testing. Such attacks, generally based on the notion that ideational causation cannot be studied causally or objectively, were never very convincing anyway, given the extensive and refined empirical literatures in political science on public opinion, elite values, transaction costs, structure-induced equilibria, political culture, analogical reasoning, entrepreneurship, social capital, strategic culture, cognitive biases, symbolic politics, and other such topics.
- 2 In doing so, I have restricted my analysis to those in the volume that raise relevant issues. Other authors, in particular those engaged in purely normative analysis, I have unfortunately left aside. Jo Shaw's analysis is particularly interesting, not least because, in seeking a social theoretical grounding for a normative theory of European constitutionalism, she ultimately rejects a more 'constructivist' or 'top-down' sociological analysis. Instead she opts on normative grounds for a more liberal, 'bottom-up' view that privileges the pre-existing interests and identities of individuals and groups in civil society.
- 3 Some, such as Glarbo, flirt with the idea that constructivism does not explain state behavior at all, but just 'shifts in diplomatic agency *identity* caused by intersubjective social structure.' This view, not consistently pursued by any of the authors – and therefore – need not detain us.
- 4 Another example are Karin Fierke and Antje Wiener (Fierke and Wiener, forthcoming)

addressing the link between the normative principles in the Helsinki Agreement and European willingness to enlarge to the East. This is a bold and intriguing interpretation of a particular historical circumstance, but it does not appear to contain a testable general theoretical proposition.

- 5 This intriguing idea, also explicable in rational choice terms as an inter-temporal contract, has potentially wide applicability. See Bates 1996.
- 6 Note that other IR theories also specify a distinct role for ideas. For realists, broadly speaking, the distribution of ideas and information is a function of the underlying distribution of material power resources. For liberals, the distribution of ideas and information is a function of underlying social preferences and institutions, such as economic interests, structures of political representative, and fundamental ethnic and political identities. For institutionalists, the distribution of ideas and information is a function of international institutional commitments contracted by national governments.
- 7 There is, of course, at least one important exception. Liberal theories examine the exogenous impact of collective ideas concerning public goods provision, which help to define national preferences. These 'ideational liberal' (or 'liberal constructivist') factors include collective preferences concerning national, political, and socioeconomic identity. These ideas can be thought of as reflective of underlying societal demands and values – collectively determined, perhaps, but intelligible as individual political preferences.
- 8 Consider, by analogy, the telephone. Telephones have many characteristics generally applied to deep ideas and discourses. Telephones constitute an ubiquitous, absolutely essential network for collective decision-making in the EU. Their existence is a necessary condition for – indeed, it is constitutive of – social interaction as practiced in this particular historical context. The network of telephones collectively empowers individuals to speak and act; without them, social interaction would grind to a halt. Yet it would be absurd to argue that telephones 'caused' European integration.
- 9 The tendency of the editors of this volume, as well as some authors in it, to assume that no other theory could possibly explain the phenomena they observe or that no non-theoretical writing could offer the same conjectures amounts to a level of confidence in social science in general, and their own theory in particular, that can only strike an outside observer as astonishing.
- 10 There is, in addition, a materialist account of preference change. The direction of British exports was shifting, despite discrimination by the EEC, from the Commonwealth to the Continent. In 1955, around 25 percent of British exports went to Europe and twice as many to the Commonwealth. By 1965, these figures had reversed. See Moravcsik 1998: chs 2–3.
- 11 For detailed evidence, see Moravcsik 1998: chs 2, 3, also on later decisions, chs 6, 7.
- 12 The editors of this volume assert at one point in the introduction that constructivist theory is necessary to free us to think of explanations otherwise inaccessible to us. Yet few if any of the hypotheses in the volume hardly seem out of the ordinary in light of traditional history, daily journalism, or political criticism of the EEC in post-war Europe. This seems to place rather too much emphasis on the public influence and personal creativity of social scientists, as compared to others in society.
- 13 This is, of course, self-defeating behavior. The more generous the analyst is to opposing theories, the more confidence we should have in any positive empirical finding she reports. In *The Choice for Europe*, for example, I specify an alternative ideational explanation of national preferences and test it across the five most significant decisions in EU history, employing a method that, I maintain, is *biased in favor of ideational and geopolitical explanations* and against my economic account. The preponderance of evidence confirms three empirical conclusions: (1) Ideational factors played only a secondary or insignificant role in nearly all cases. (2) There is nonetheless interesting cross-national and cross-issue variation in how much ideational factors mattered, and I go on to suggest some hypotheses about the conditions under which ideas matter most.

(3) One cannot trust the public rhetoric or interview statements of government officials and politicians. Politicians are professional experts at manipulating rhetoric opportunistically; only confidential sources tell the real story. In that work, however, my primary concern was not to specify a detailed ideational theory. Hence there remains much room to improve such theories and engage in a far more intensive and focused *empirical* debate grounded in the rich archival sources available on European integration. On ideas, see also Moravcsik (forthcoming).