

DISCUSSION FORUM

Institutional change and globalization

At the 18th Annual Meeting on Socio-Economics in Trier 2006, an 'Author Meets Critics' session, organized by Christel Lane, President of SASE, debated John Campbell's book, 'Institutional Change and Globalization'. Critics were Colin Crouch, Wolfgang Streeck and Richard Whitley; John Campbell responded. Subsequent to the session, the participants wrote up their statements for publication in Socio-Economic Review.

Keywords: institutionalism, institutional change, globalization

JEL classification: B15 historical, institutional, B52 institutional approaches

How to 'do' post-determinist institutional analysis

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Campbell (2004) has correctly identified a new stage in neo-institutionalist analysis, one which finds space for innovative, entrepreneurial actors. Analyses that take account of such actors avoid the determinism of which much earlier work within the institutionalist genre can be accused and, as Campbell most importantly argued, show how such actors *make use of* institutions in order to make their innovations. Appreciation of this latter point saves us from throwing out the valuable institutionalist baby along with the deterministic bath water. Concepts like *bricolage*¹ and recombination then enable Campbell to model how

¹This term was first applied to human innovative behaviour, but only in primitive societies, by Lévi-Strauss (1962). Its application in advanced societies was perceived by Mary Douglas (1987), but like Lévi-Strauss she uses it explicitly to mean minor adjustments among existing resources that fail to achieve 'breakthroughs'. Campbell removes this limitation. In his approach, and that taken here, we can never be certain that minor adjustments will not accumulate to the point where a change that retrospectively seems to have been radical takes place.

these institutional entrepreneurs go about their business. Since his book appeared, and partly influenced by it, a number of other works has continued the same line of thinking (e.g. Crouch, 2005; Streeck and Thelen, 2005b), so that we might now claim that actor-centred institutionalism, as Mayntz and Scharpf (1995; see also Scharpf, 1997) called it some time ago, is an analytical approach capable of addressing social change. Mention of Mayntz and Scharpf's work reminds us that the attempt to combine creative human agency with a sociologically informed approach to institutions is not new, but Campbell has taken it some new steps forward. He has synthesized recent debates and has applied the approach to historical contexts and to large-scale institutional contexts (globalization) in a highly useful way.

Here he makes use of the idea of within-system incremental changes gradually accumulating until a system change can be considered to have occurred. This builds on the concept of 'layering' developed by various authors (notably, Mahoney, 2000; Ebbinghaus and Manow, 2001; Thelen, 2003; Streeck and Thelen, 2005a). This has enabled a number of scholars to account for eventually radical change while accepting much of the logic of path dependence theory. It is also useful in breaking down the artificial distinction between structure and change. Although for methodological reasons it is entirely appropriate for theory to model equilibrium points for a certain kind of social structure, and for research to take 'snapshots' of equilibrium moments in actual societies, it is essential to recognize that these bear the same relationship to social reality that a photograph of a cyclist bears to the act of bicycling. A person can present the illusion of being balanced on a bicycle only by being in movement, repeatedly throwing oneself off balance in alternate directions, only momentarily achieving actual balance on the way to going off-balance in the opposite direction. The cyclist's position achieves equilibrium only because it constantly changes. The illusion of social stability similarly conceals a mass of adjustments as a particular social order is repeatedly thrown off-balance in successively contrasting directions by internal challenges and external shocks.

Even if the adjustments always taking place within society often seem to have the effect of restoring the *status quo ante*, this is unlikely really to be true. Nothing is ever fully restored in a social order, certainly not among a species that possesses both memory and mortality. What varies is the frequency and impact of disturbance, the chances that the interests of this or that social interest will be advanced or reduced in substantive ways. We become aware of social change when the combination of challenge/shock and response produces institutions that we regard as new, unprecedented, for the context concerned; but this is often a particular moment in processes of fluctuation that have been going on for a long time and which will continue. Often that point of recognition that change has taken place may be only a 'last straw', 'Kuhnian' moment when the gradual

accumulation of ostensibly minor, within-system changes can no longer be seen as coming within the old frame, but requires us to start reaching around for new concepts.

This perspective leads us to ask two questions, familiar enough to general social science, of the approach to change of actor-centred institutionalism: When can we say that changes have accumulated to constitute a system change? Can social science tell us anything of the limits of actors' capacities to make changes to particular kinds of institution and within particular contexts?

Changes and system changes

Above I spoke about changes in terms of what is 'seen', being ambiguous whether I am talking about change in phenomena themselves or in the perception of them. There is further ambiguity whether those perceiving are engaged social actors or scientific observers. The ambiguity is deliberate because, as a mass of writing about hermeneutics and double hermeneutics has taught us, it is not clear whether changes in social relations can be said to have taken place if they are not perceived and defined as such (the idea embedded in the intriguing French phrase *avant la lettre*).

For example, let us take the prolonged series of changes that seem to have started some time in late 19th century western Europe, when poor families started to allow their daughters to have shares of meat and other important foods that had been previously reserved for sons. Before long this became evident in an improvement in female life expectancy rates. The change runs alongside wider social and political debates about women's rights, but it is not at all clear whether it was these ideas that were affecting parental behaviour in peasant and working-class households. Possibly, both changes were affected, largely tacitly, by the increasing mechanization of working life in agriculture and industry that were subtly undermining men's claim to superiority based on the importance of physical strength.

By the 1920s, another change, probably related to similar fundamental causes, started to appear, again across a number of societies: girls' performance in the early years of schooling, previously inferior to boys', began to improve (Blossfeld and Shavit, 1993). Over succeeding decades this improvement can be tracked across more and more national education systems and affecting later and later age groups (Müller *et al.*, 1997). By the late 20th century, this process, initially unannounced, unnoticed, perhaps quite unconscious, had reached the point where in a number of countries *boys'* educational under-achievement had become a major issue.

More generally, these changes in the relative treatment of sons and daughters had become part of major political debate and public comment and considerable

amounts of academic research and social theorizing about relations between the genders. Looking back across more than a century it becomes possible to declare with considerable confidence that a major change has taken place in gender relations within European and other industrialized societies. However, not only is it difficult to say at what points a more than incremental change had occurred, but also the points differ whether one looks at change in underlying phenomena, in social appreciation of these changes or in social science's conceptualization of them. It also remains possible for there to be lively debate as to whether gender relations have really passed a 'last straw' point of change or whether we are still observing minor adjustments to a primarily patriarchal family form. Clearly, the practices of sacrificing daughters' nutrition to that of sons' and of encouraging boys far more than girls to take education seriously are all of a piece with the idea of patriarchy as a social form. But how many changes must occur, and how extensive must these be, before we announce that patriarchy can no longer be used to describe family arrangements, and that something new has arrived?

Before we turn to that question, which concerns our ability to distinguish systemic from incremental change, some other aspects must be addressed. Since social relations comprise an ultimately inextricable combination of facts (for example, comparisons between boys' and girls' diets) and social actors' perceptions of those facts (parents' views of their sons' and daughters' relative dietary entitlements), in the long run 'objective' changes and perceptions of them have to come into some alignment. Put slightly differently, an objective change that remains forever unperceived is only a partial change, as perception is a part of a social reality. Such a change is unlikely to be perceived to be a system-altering one. Following our example, a society in which the nurture and education of male children continued *to be seen* to be more important than those of females in all private and public discussion of the matter would continue to be in some senses a male-dominated society even if girls' life expectancy and educational performance had started to exceed those of boys.

Social science then has the additional problem of the judgements that it must make if there are discrepancies between the performance of certain 'objective' indicators and widespread beliefs about them. To ignore social actors' perceptions is to fail to appreciate that social reality *includes* actors' perception of it. To take another historical example, one far more widely remarked and in fact at the centre of Weber's distinction between class and status: a political system that continues to grant formal recognition to aristocracy, even though capitalist wealth and political power has become *de facto* more important and the aristocracy has become dependent on and in debt to it, has not so thoroughly moved from being an aristocratic to a bourgeois system as one in which aristocracy is no longer recognized.

On the other hand, to refuse to perceive changes until social actors themselves announce them is to abdicate social scientists' own professional role, turning it into the mere reporting of the not necessarily well-informed beliefs about themselves of social actors. This was the blind alley into which ethnomethodology took much sociology in the 1970s. A different example again illustrates this point: it would be foolish for political scientists to ignore the claims of all participants and contemporary observers that a very significant change took place in the government of Russia from October 1917 onwards. On the other hand, it is equally valid for them to point out that certain institutional practices—such as extreme centralization and bureaucracy, official brutality, a highly extensive network of secret police as a major organ of state—showed considerable continuity across that juncture and should not therefore be seen as part of the system change that took place. To have this debate now about that period is spared the added hermeneutic complication of social scientific comment itself possibly affecting actors' perceptions of themselves; the same cannot be said of the virtually identical debate now in progress about the changes that may or may not have taken place in Russia around 1989–91.

We should have doubts about the systemic nature of a change when it comprises a set of adjustments that leave many elements of existing structures still in place and when those involved either do not perceive the change or inaccurately perceive it. But this formulation does not take us far in identifying when a system change *can* be said to have occurred, as it specifies neither how much of an existing structure can be permitted to remain in place, nor the extent and scale of measurement of the accurate perception it proposes.

We can ask a completely different question: Does it matter if we can decide whether a change is systemic or not? It does matter when, in order to make sense of the world, we need to know whether a particular phenomenon with which we are dealing is a member of one class of things or another. In the case of discussions of change, this is a question of when the phenomenon can be considered to have passed from being a member of one class to another. This is largely a question of the adjectives that are to be attached to empirical cases of nouns such as 'society', 'system', and 'world', the adjectives in question being descriptors that are often found limiting these nouns, such as 'democratic', 'industrial', 'Christian' and 'modern'. Communication would be impoverished if we felt so lacking in confidence about their applicability that we never used them. Sometimes the knowledge conveyed by these adjectives can be very important in signalling characteristics of the entities in question. But we rarely have really good criteria for determining whether they are fully appropriate.

The ability to use such adjectives matters, particularly to those social scientists who need to classify whether a social form approximates to one or another type within an overall typology. Such an approach requires, literally, a categorical

answer to the question of whether a particular adjective can be applied. Under what circumstances can a society cease to be categorized as patriarchal? Did Russia become state-socialist after 1917? What is it now?

I have argued elsewhere (Crouch, 2005) that social analysis would become more scientific if it followed the analogy of chemical and biological analysis, and if instead of asking 'to what type does this case correspond?', it asked 'examples of what type do I find in this case and in what proportions?' This does not dispose of the task of labelling. It remains relevant and sometimes important to debate whether so many elements of a new type have entered a case that it can be considered to be fundamentally changed and no longer an example of what it was once thought to be but of something else. What is achieved is that this stage of the research becomes separated from and subsequent to that of analysis. It then becomes possible, if preferred, to make a good and detailed analysis of all the changes that one can perceive without necessarily moving on to address the question 'Does this constitute system change?': one's account might consist solely of the analysis, without a subsequent naming exercise. This might be particularly legitimate where the typology in use comprises a challengeable set of constructs that have not gained general scientific consensus over their utility.

Take an example from outside social scientific analysis, but in a neighbouring field. Most people who listen to Beethoven's third symphony would say that there is something distinctly new there, not found in his own earlier works or those of his older contemporaries; this seems very much to be a case of radical innovation, a move away from path-dependent musical composition, and Beethoven himself and those who heard the symphony at the time of its composition would have agreed on that. Musicologist can analyse for us, quantitatively, the differences that we are noticing and which seem to mark a qualitative change in musical history; but they may also point out that traces of all these components are anticipated in works by various contemporaries. So there are elements of *bricolage* here, or of Schumpeter's entrepreneur as the maker of new combinations fashioned from existing materials. Through such an account, we can gain a rich view of both the extent of Beethoven's innovations and the limits to them, of his individual agency, his embeddedness in certain institutions of musical composition and his use of those very institutions in order to make his innovations.

If we now ask our musicologists whether the third symphony therefore marks the transition from the Classical to the Romantic period, they may answer positively, particularly if we are at an early stage in our musical appreciation and they think that such signposts will help us on our way. But if they judge us to be ready for sterner stuff, we may well receive a lecture on the artificiality and misleading nature of such labels and be told that we should do better to hold in our heads the more detailed and nuanced analysis that they have given us.

At one level, this is what we should do when we appraise the complex interplay of actors and institutions around change: actors using institutions to advance/retard change, institutions inhibiting/facilitating change. Describing and analysing these processes, and measuring the many different components of change, is a task in itself, and possibly more valid than answering the question to which we are then tempted: Was this a system change, then? This is, however, unrealistic, as we continue to need to use the adjectives if we are to give summarizing accounts, and particularly if we are to give accounts of social scientific findings that are intelligible to non-professionals. Chemists and biologists, whose analytic approach I commended above, have largely given up on the latter task. They do label the phenomena they identify, but by using neologisms that cannot be understood outside their own circles. Most of the time they have gained enough respect for the utility and reality of what they do that this does not matter to them. On the occasions when doubts are raised about their activities, as with nuclear fission, genetic modification and pesticides, they suddenly run into enormous social and political difficulties and begin to complain about public scientific ignorance. The things that we study in political science and sociology (not so much anthropology, economics or psychology) are sufficiently close to everyday experience and to phenomena observable (as it were) to the naked eye that they have been named in normal speech. Therefore, we cannot so easily get away with precise but incomprehensible terminology. Talcott Parsons's attempt to do otherwise ultimately failed, though he did manage to influence a generation.

There is therefore no escape from the demand for labels. People have a reasonable expectation that social scientists should be able to tell them, for example, at what point one has to cease to talk of 'Keynesian' economies and start talking about 'neo-liberal' ones. And it is almost impossible to talk coherently about economic management during the past 30 years without using these terms. And yet, every so often, it becomes necessary to knock down the comforting house of cards on which we build so much of our discourse: Germany was never really Keynesian (Matzner and Streeck, 1991); Danish neo-liberalism is quite different from the Anglo-American variety (if that is one variety) (Kjær and Pedersen, 2001) We shall go on using the labels, but we ought to put health warnings on them.

Identifying institutional limits to change

The second question—whether social science can tell us anything about institutional limits to change—requires a similar approach. First we should note, consistently with Campbell's arguments, that it may not always be a case of activist actors forcing change against refractory institutions: the logic of institutional

development may force change on reluctant actors. However, for the sake of simplicity in presenting an initial argument, let us assume the 'activist actors versus refractory institutions' stereotype. At one extreme in representations of that encounter, we have Carlylian history, presenting the history-shaping individuals who bent institutions to their determined will. At the other extreme are deterministic accounts which propose that, try as they might, individuals or indeed whole groups of actors will never be able to shift the path dependence to which the logic of their institutional history condemns them. An example of the latter is the account I once gave of the history of European industrial relations systems (Crouch, 1993). Try as they might, I argued, for example, neither the British nor the French would produce neo-corporatist industrial relations arrangements, because their albeit different institutional histories had bequeathed them both with states that could not share political space with non-state actors.

Both these approaches miss out Campbell's institutional entrepreneurs, picking around among the institutional litter with which they are surrounded in any complex, changing society, looking for bits and pieces that they might put together in new ways. Particularly important will be the judgements these actors make, rightly or wrongly, over which new combinations will favour their own particular interests within their societies and which will oppose them, and the subsequent outcomes of struggle between contesting groups of actors who have made opposed assessments of that puzzle. Followers of the Carlylian school fail to observe how their Great Men of History scabble around in their institutional legacies to make their innovations. Accounts like mine of European industrial relations systems fail to notice how internally diverse those legacies are and therefore take insufficient account of the extent of choice and discretion that powerful actors had over which bits of their history they decided to use, which to ignore, which to make pliable and which had a path dependence to which they had (perhaps happily) to bend.

Yes, it is true (as I had argued in Crouch, 1993, p. 85) that the way in which German trade unions became incorporated in the management of social insurance schemes during the Bismarckian period provided the germ of the idea that unions could share in the management of public business. This idea flourished, transitorily during the ill-fated Weimar republic, far more durably under the successful Bonn republic. However, similar arrangements for managing pension funds emerged around the same time in France, and are still in force today, but did not provide a basis for structuring other elements of social and labour policy. Retrospectively, these different choices can be fitted together to make a coherent account of how actors followed an increasingly strong path dependence, but at certain moments that logic occurred only because certain powerful actors decided which elements of the complexity of

institutions with which they were surrounded would be emphasized and which kept in by-ways.

Again, it would have been very helpful to my overall thesis of historical continuity had British unions, emerging from a pre-socialist, liberal tradition and jealous of the autonomy of collective bargaining throughout the 20th century, also maintained the autonomy of their own social insurance schemes until government delegated to them the administration of a national public scheme. Unfortunately, it was the thoroughly social democratic Swedish unions who did this, whereas British unions gave up any role in the management of social insurance during the 1920s. British and Swedish labour-movement institutional entrepreneurs happened on a paradoxical difference in their *bricolage*.

Since, virtually by definition, bold innovators will always take social analysts by surprise, we shall always be running around after the decisions they make, trying retrospectively to weave these into the coherent tapestry we try to make of their history. The improvement that Campbell's approach offers is essentially one of specifying probabilities. We cannot predict which institutional entrepreneurs will make use of which components of their institutional legacies, but we can assess the relative likelihood of their doing so. Thus, we might well predict that cave dwellers living in an area rich in fragmented flint will be more likely to discover how to make fire than those living somewhere where such stones are rare. But it is always possible that the former will be geologically incurious, whereas the latter happen one day to chance upon a couple of pieces of flint and strike them together, leading, against the odds, to an earlier discovery of fire-making in the flint-poor area. The prediction that the former group would have been more likely to make the find is not actually refuted, unless the second group's probability of doing so had been set at zero. It might also have been possible, and extremely useful, to have predicted what the second group would have to have done if they were to realize their against-the-odds achievement.

Thus, anthropologists and political scientists might work out how likely it will be that the Iraqi political class will find something in the institutions surrounding it capable of building a sustainable democracy, and also specify the kinds of circumstances in which such an against-the-odds outcome might be achieved.

This final example brings us back to the first issue discussed here: the identification and labelling of systemic change. How do we know when something like 'sustainable democracy' has arrived? We should do better to break that complex idea down into a number of empirically definable components, and then assign probabilities to each of them separately. In this way, social analysis is able to make a reasonable attempt at predicting and defining change, even if the resulting account is longer and less decisive than the world would prefer to hear.

Conclusion

Onwards then to the new stage of non-determinist, actor-sensitive institutionalism. For this enterprise, we require a willingness, and a methodological capacity, to present:

- (i) nuanced and analytical, and probably quantitative, rather than categorically summary accounts of institutional change, especially when estimating its potentially systemic character;
- (ii) clear ideas about when we shall use labels, probably involving some distinction between rough generalizations couched in normal speech for wider communication, and more precise but less readily comprehended terminology for scientific discourse;
- (iii) probabilistic and detailed inventories of the institutional resources available to actors in various different empirical contexts;
- (iv) accounts of what institutional entrepreneurs are likely to have to do if they are to produce against-the-odds outcomes given the institutional contexts in which they find themselves.

It is within this kind of contextual framework that we can deal with embedded institutional entrepreneurs and their *bricolage* while maintaining an analytical capacity that keeps its poise, lurching towards neither individualistic historiography nor deterministic sociology.

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'Globalization': nothing new under the sun?

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Unlike what its title suggests, Campbell's book (Campbell, 2004) is primarily on institutional change and only marginally on 'globalization'. Most of what the book has to say on institutions—meaning most of what the book has to say—I find entirely plausible, and some of it quite enlightening. The treatment opens with a chapter on the famous 'three institutionalisms', ending with yet another, probably futile attempt somehow to merge them into one (Ch. 1). This is followed by a conceptual discussion of institutional change (Ch. 2), where the message is that for empirical study, it is imperative that institutions

be decomposed into ‘bundles of dimensions’, and relevant time frames be specified beforehand, to enable judgment as to whether change is ‘revolutionary’ or ‘evolutionary’. Such judgement may be arrived at by either qualitative or quantitative analysis. I sympathize with Campbell’s methodological Catholicism, and also with his allowance for conflicts and contradictions within his ‘bundles’ of institutional dimensions, and with what he makes of them conceptually. The book then (Ch. 3) proceeds to discuss causal mechanisms of institutional change, where ‘path dependence’ is supplemented by ‘*bricolage*’, emphasizing the ‘entrepreneurial’ capacities of actors and preparing the ground for passive ‘diffusion’ of institutional forms to be distinguished from active ‘translation and enactment’. Generally, the thrust is to open up a space for creative action to avoid getting stuck in institutional (self-)determinism. All this makes a lot of sense, more—at least to me—than Ch. 4, which is devoted to the role of ideas in institutional change. Here I miss a convincing solution to the dilemma between idealistic constructivism and sociology-of-knowledge type reduction of ideas to the social conditions in which they are embedded. But then, Campbell is in good company in not being able to offer such a solution.

‘Globalization’ comes in only in the fifth of six chapters, and only as a test case, as it were, for Campbell’s theory of, or approach to, institutional change. Actually the book deals less with ‘globalization’¹ as such than with what Campbell calls ‘the so-called globalization thesis’, which is summarized as making two big claims: that ‘the pressures of globalization are forcing advanced nation-states to pursue a common set of neoliberal programs’ (Campbell, 2004, p. 125) and that ‘globalization is transforming national economies’ and homogenizing their institutional structures on the American neoliberal model (Campbell, 2004, p. 127). The chapter sets out to show that, ‘contrary to much conventional wisdom . . . globalization has not precipitated the sort of dramatic institutional changes . . . that are often attributed to it. Instead, the effects of globalization on key institutions have been much more modest and evolutionary. . . This misunderstanding could have been avoided if observers had taken institutional analysis into account. . .’ (Campbell, 2004, p. 124) Had they done so, that is, they would have known that ‘institutions create incentives and constraints for action that affect the degree to which actors adapt to globalization pressures by transforming current institutions, policies and practices. In other words, institutions mediate globalization in ways that do *not always lead inexorably toward wholesale neoliberal reform. . .*’ (Campbell, 2004, p. 127; italics mine, WS).

¹I like to use the concept in quotation marks to register my preference for the more modest alternative, internationalization. Not all of the social relations that may put pressure on national institutions are truly global; it is enough for them to be international, i.e. to extend across national borders.

If one wanted to be nasty, and who does not sometimes, one might ask whether with enemies as feeble as Campbell's stylized 'globalization theorists' one really needs friends. Moreover, that national institutions 'mediate' between external pressures and internal outcomes should not be all too difficult to prove, especially as Campbell, true to his approach, decomposes the possible impact of globalization on national institutions to look at only one policy area, national tax regimes and taxation (Campbell, 2004, p. 132). We all occasionally prefer to fight a straw man of our own making on a turf of our own choice, but some of us are better at hiding this than others. What concerns me much more, however, is that Campbell's institutionalist analysis may suggest, and sometimes appears to be meant to suggest, that the structures and practices of modern nation-states are too firmly settled in their distinctive ways to be ever profoundly altered by changing economic pressures. Thus when it comes to the famous 'varieties of capitalism', to which it ultimately does come (Campbell, 2004, 169), the institutions in which capitalism is incorporated clearly matter more for Campbell than capitalism as such. 'Neoliberals,' as I read Campbell, may insist on what to them are universal functional demands of changing markets for goods and production factors, just as radical political economists may insist on a basic logic of capital accumulation and class power. Institutionalists, in contrast, know that all of this is effectively overlaid by the *Eigendynamik* of social orders, their stickiness, the slowness of their evolution proceeding in a continuing complex interplay between tradition, culture and politics—between socialized preferences and perceptions and institutionalized constraints and opportunities—where functional pressures for efficiency, changing power relations between actors and the class logic of capital accumulation are necessarily and inevitably filtered through and importantly modified by nationally distinctive institutional arrangements.

Campbell's book, precisely argued and clearly written as it is, offers an excellent opportunity to explore the validity of some of the basic beliefs underlying some institutionalist views of the social world. How sanguine can we be with respect to the impact of economic internationalization on the institutional apparatus—overwhelmingly national for historical reasons—with which modern political communities undertake to govern their collective fate? How robust are today the shelters constructed in the post-war era to protect societies from the uncertainties generated by free, price-setting markets?² In the following, I will offer a number of arguments and questions

²Protection of human society from unpredictable fluctuations of relative prices is the real theme of Polanyi (1957 [1944]), not the contribution of social 'embeddedness' to an efficient operation of the market. Polanyi's concern is shared by much of the post-war literature on democratic capitalism, economic planning, social citizenship, etc.

that to me cast doubt on the reassuring mantra of much of current institutionalism that national political institutions, if intelligently deployed, can continue to provide for collective-democratic control over the forces of the market and protect social life from the dictatorship of economic necessity or economic power. I am aware that Campbell avoids general claims of this sort; but I also think that precisely for this reason, he is at risk of being read as implying them. My remarks are offered in a spirit of dialogue and out of curiosity whether and how the points I am about to make can be accommodated in the institutionalist discourse of which John Campbell is one of the most eloquent representatives.

1. Is it not far more the case today than it was in the immediate post-war past that in political economy we are dealing no longer with *markets within states*, but rather with *states within markets*? And if the answer is yes, does this not matter in a major way? To me, states that encompass most of the markets on which their citizens depend, disregarding a limited and therefore more or less governable amount of cross-border transactions, would seem to be in a very different position than states that must act within and accommodate markets that encompass them. Policies to *control* economic forces and policies to *accommodate* them would appear to be quite different kinds of policies—and institutions changing from market control to market accommodation would seem to undergo quite fundamental change, *even if it proceeded gradually*. In Europe, at least, we have observed in the past two decades a striking coincidence between the opening up of post-war national economies in the course of European integration and a previously unimaginable imposition of economic and political discipline on national states in the name of free trade.³ Supranational institution-building was an important part of this: see, for example, the application of competition law to government policies, or the European legislation requiring national governments to privatize their postal, telephone and railway services. Actually, European integration was and is above all else an incredibly effective liberalization machine, at least in the rich countries of the post-war settlement, the wishful thinking of trade unions and the ‘social dimension’ rhetoric of politicians notwithstanding. Should not the fact that most member states of the European Union no longer have a central bank worth the name, and have thus lost any capacity they may previously have had for an independent monetary policy, be regarded as a major institutional change related to internationalization? The consequences, in any case, for the options available to national policy-making are nothing short of dramatic, and so are the implications for the relationship

³To what extent this was actually self-imposed by national governments enlisting each other’s help in dealing with increasingly ungovernable domestic political economies does not really matter here. One may well be concerned about ‘globalization’ without believing it to be exogenous to politics.

between politics and the economy, and in particular for the extent to which states can hope to govern, or at least to moderate, the impact of economic forces on their citizens' living and working conditions.

I know the numerous qualifications the literature on European integration offers to any attempt at generalization. In forestry, it is indispensable for a conscientious inventory that one takes a careful look at every single tree; but does this mean that one must lose sight of the forest? European economic governance differs by sectors and countries, allowing for the impact of tradition, technology, market form, etc. Also, researchers differ by national experience, ideology and personal disposition, and social science is necessarily deeply imbued in the hopes and intentions of those that produce it. There are indeed good reasons to welcome the abolition of state post offices, and the same holds for the imposition of budget discipline on critically indebted national states. Still, should it not be possible to agree that states embedded in markets require a quite different toolkit for economic policy than states embedding markets—and one whose efficacy still awaits confirmation?

For clear thinking, it is sometimes useful to return to the classics: to the first comprehensive formulation of an important insight, without the fine detail that is later inevitably added by painstaking empirical research—which, meritorious as it certainly is, may have the unfortunate tendency to obscure the fundamental pattern that conditions the variations on the surface that alone tend to count for the empiricist. I am referring here to an essay by Hayek, first published in 1939 (Hayek, 1980 [1939]), in which he points out with breathtaking sweep and clarity the implications for economic governance of an 'interstate federation', which he hoped would at some time be created to preserve international peace. If political integration was to be stable, Hayek argued, it had to be accompanied by economic integration. Economic integration, however, was to be had only if national states gave up their economic sovereignty, thus as a side effect setting the economy free from political intervention. But this was only the beginning. Because of the greater heterogeneity of the enlarged political community, the powers of intervention that had existed, and had to be given up, at the national level could not, the pious hopes of interventionist politicians notwithstanding, be recovered at the federal level. Thus the new political economy was, to Hayek's express delight, bound to be much more liberal than the political economies of its member states before federation. Moreover, given that member states were now, in the language of the present essay, themselves embedded in markets, there were almost irresistible functional reasons constraining them to delegate powers, not just to the federation, but also to smaller subunits, such as regions and local communities, so as to enable them to compete in the liberated markets of the federation against which their national governments could no longer protect them. Is this not, by and large, what has been going on in

Europe at least since integration got serious around the early 1990s? Like Hayek, one may welcome this direction of institutional change. Others may hope that what one sees, and what Hayek saw coming, may simply be too bad to be true. But why should this prevent us from agreeing that internationalization has brought about tremendous, fundamental institutional change in the post-war European nation-state, in the direction of economic liberalization?

2. Sometimes it seems as though Campbell's book would want to be counted among the 'nothing new under the sun' literature on 'globalization', where we are again and again bombarded by information of the sort that the share of foreign trade in national economies was not much lower at the end of the 19th century than it was in the 1990s.⁴ What I find misleading about historical comparisons of this kind is not so much that the national economic context in which the liberal world economy of more than a 100 years ago was embedded was quite different from today; even in the most developed countries, most people continued to have access to the land and could in times of need rely on a village where their relatives were still living. More importantly, I believe that the dictum of Heraclitus, that 'you can't step twice into the same river', can serve as an extremely fruitful heuristic principle: suggesting the possibility that if today we try to do the same thing as in the past, it may in fact no longer be the same. Confronted with the grinding 'satanic mill' (Polanyi, 1957 [1944]) of expanding free markets, late 19th century society had, as it were, an important trump card up its sleeve that it could eventually pull and play: the modern nation-state with its capacities for economic intervention, both tested and vastly extended in the war from 1914 to 1918 that ended Polanyi's 'long peace'. Nationalization of the political economy enabled societies politically to manage some of the manifold economic uncertainties associated with capitalism, while it created others associated with protectionism and the international hostility to which it gave rise or which it made possible. It was only after World War II when, for the two or three decades of the 'Golden Age', international Keynesianism allowed for a peaceful coexistence of economically interventionist national democracy with the civilizing forces of more or less free international trade—until the turmoil of the 1970s and 1980s (Hobsbawm, 1994; Judt, 2005) and the new 'great leap forward' in economic internationalization with which it was associated.

The question, then, is what sort of 'counter movement' we can today hope will return to political control an international economy that is so much more than in the post-war era governed by free markets? (Unless we assume that liberal theory is right and no such control is needed.) Are national institutions, more than ever disciplined by international 'market forces', enough to govern the capitalist

⁴As we are abundantly informed by Campbell as well (Campbell, 2004, p. 134).

economy of today? What concerns me is that questions like these do not seem to worry Campbell a great deal. ‘Globalization’, we read, ‘also occurred at the beginning of the twentieth century’ when it ‘was not associated with the same types of institutional changes that it is said [by ‘globalization theory’, WS] to have caused more recently. *National institutions were being built and fortified, not incapacitated and hollowed out*’ (italics are mine, WS). This, Campbell continues, ‘raises serious questions about the causal mechanism that lies at the heart of globalization theory’ (Campbell, 2004, p. 130, 135f.)—meaning, to me, that *if national institutional responses were enough to tame the economy then, they will be enough also today and in the future.*

3. The problem here is not so much one of the *time frame* of analysis (but see Campbell, 2004, 133ff.) as one of the *historical character* of its subject. As to the former, we know that sometimes institutional change proceeds with great speed, as when national institutions break down, or are broken down, after defeat in war or in a revolution. Then change and the differences between before and after are easy to detect. What, however, if institutional change, including major, transformative change, moves on only gradually, as typically it seems to today (Streck and Thelen, 2005)? Observing and explaining this sort of change requires that one sees the present in the context of a *longue durée*, or at least a medium-term *durée*—a capacity that is unfortunately lacking in most of contemporary social science. Inside a time horizon of 5 or 10 years, observed gradual change, even if in fact it was part of a protracted process of fundamental transformation, can always be construed to be conjunctural or even accidental—a superficial fluctuation of little consequence within a world that is and remains the way it is. Compared to the long run of history, the life of individuals working as social scientists may simply be too short for slow fundamental change to be easily perceived. Without artificially, as it were, distancing ourselves from what in our personal life we experience as the natural condition of the world—which one does best by comparing it to its past—we may easily overlook what is essential, or confuse it with what is ephemeral. Campbell knows this and warns against it. The reason why he believes that time will not change what he thinks he sees today is, he would claim, not based on naïve extrapolation but on theory: on the causal mechanisms he has found to be in place at the end of the 19th century, and on their assumed continuing effectiveness at the beginning of the 20th (see Campbell, 2004, 167ff., under the subheading of ‘What If We Wait a Little Longer?’).

Here, however, is where *history* comes in, and with it the—I think: unfortunate—tendency in much of social science, seeking to ‘theorize’ the world in terms of universally valid causal relations, to neglect the fact *that chronological time may also be historical time.* This mistake, of course, is by no means new:

Another hidden source of error in historical writing is the ignoring of the transformations that occur in the condition of epochs and peoples with the passage of time and the changes of periods. Such changes occur in such an unnoticeable way and take so long to make themselves felt, that they are very difficult to discern and are observed only by a small number of men (Ibn Khaldûn, 1950 [1377], pp. 29–30).

Periodicity is well-known to some econometricians today, but many social scientists apparently still have to hear about it. Would one not think that it makes a difference that the 20th century has used up, first, the nationalist and then the cold war-international-hegemonic approach to the management of international economic interdependence, while at the same time moving economic internationalization to a new level, e.g. by adding integrated production systems, internationalized capital and labor markets, international migration and the like? Historical institutionalism has rightly taken on board the notion that time and timing are crucial for the evolution of social arrangements—see its various versions of path dependency or its forming recognition that age may be an important structural property of an institution. But a truly *historical* perspective must allow for the possibility *that the present may be governed by different laws than the past*, so that what may on the surface be the same may, depending on its *historical location*, in fact have a very different meaning and causal significance. Rather than rolling out general formal principles of institutional change in a world that fundamentally remains what it always was, it might be more productive to consider the present impact of ‘globalization’ on socio-economic institutions as embedded in a historical process—one of renewed expansion of market relations undermining the post-war set of institutional safeguards against economic uncertainty, and carrying the conflict between the dynamism of free markets and the traditionalism of the human life-world to new levels and previously unknown forms.

4. Placed in historical context, constant spending on welfare programs, for example, would not in itself be an indication that things are still the way they were. It could also mean that the secular growth of the welfare state has come to a halt: that its expansion is no longer on the agenda *even though markets continue to expand rapidly*. Moreover, rising levels of inequality, poverty and unemployment would suggest that *constant* spending might in fact be *declining* spending, given an increasing problem load. Drawing attention to this is, to be sure, not popular in times of ever tighter constraints on public budgets. Or does the fact that constant spending yields declining results mean that the money is somehow misspent, or needs to be spent differently? Institutional analysis might want to get below the surface of aggregate spending figures and look for a slow and silent process of ‘conversion’ (Thelen, 2002): of old

bottles being used to contain a new sort of wine. Indeed, if one digs a little deeper, one finds an almost universal restructuring of national welfare states in the direction of investment rather than consumption, re-commodification instead of de-commodification, strengthening 'employability' instead of raising workers' reservation wage, 'activation' for the market instead of protection from it, etc. (Streeck, 2000). Together, this seems to amount to a profound, if gradual, penetration into social policy of an ethos of production and competitiveness—with flexible adjustment of 'human capital' to competitive markets taking the place of older attempts at breaking market pressures on the human lifeworld. As a result, the welfare state seems to be about to be turned into yet another educational tool for instilling in the population and the culture at large a spirit of enthusiastic participation in paid work. Again, there may be legitimate practical as well as ideological reasons to find this desirable or, at least, inevitable, given the financial and other corners into which social-democratic social policy has painted itself. But surely it does not imply an obligation not to take notice of it?

Economic internationalization, it would appear, imposes new agendas on established national institutions even where it leaves them on the surface unchanged. Only detailed qualitative analysis that is conscious of historical context can detect how old settlements are undone, new functions are assumed, and emerging problems are solved or, as the case may be, not solved. A good example is industrial relations regimes like the German one where corporatist class solidarity is being gradually displaced by workplace alliances between management and labor, in an institutional framework that has remained basically unchanged since the mid-1970s. Clearly, 'globalization'—the increased possibility for firms to relocate production beyond national borders—was a powerful driver of this slow transformation as it freed firms from formal and factual obligations to their place of origin. But it is also true that the books are not yet closed on the matter as labor may eventually devise more effective counter strategies to protect itself from the pressures of a multinational market for employment. The conflicts likely to be associated with this may well take place within the inherited institutional framework. But this should not be construed as meaning that institutions would have basically remained unchanged.

5. 'Globalization', as Campbell repeatedly reminds us, does not change institutions directly since change is always mediated through institutions' past and present, and indeed through policy. The social effects of changing constraints and opportunities for strategic economic action are therefore not predetermined: they are in principle contingent on the more or less wise choices of those governing relevant institutions. But true as this may be, it cannot really be meant to rule out forever a deep neo-liberal recasting of the post-war political-economic order, of the kind that actually happened in the USA in the erosion of the New Deal

during the 1970s and 1980s. That institutions change according to a specific institutional logic is one thing; that the relationship between institutions and markets may become very different from what it was in the politically embedded capitalism of the post-war years is quite another. Policy plays its part, but it does so under economic conditions that are, to paraphrase an almost forgotten founding father of historical institutionalism, ‘not of its own making’. Are we to believe that the relative autonomy of institutional-political responses to economic change will always be sufficient—and economic conditions always sufficiently malleable—for countries once in the ‘coordinated capitalism’ camp to prevent forever, say, an American-style polarization of their citizens’ life chances? ‘Wholesale neo-liberal reform’, as Campbell has it, may or may not be in the offing.⁵ But perhaps it would be enough if its rejection, as a result of political blockade or institutional ‘path dependence’, was penalized by declining welfare state performance and growing budget deficits eroding and ultimately neutralizing the governing capacity of the state?

Where in the past ‘globalization’ may have encouraged the building of national coalitions between classes for the purpose of protective organization of the national economy, today class coalitions more often than not pursue political-economic *disorganization*, to open the domestic market, to make societies ‘fit for competition’ and to whip up enthusiasm for what the policy makers of today feel to be inevitable: a collective transition towards a flexible lifestyle adjusted to the requirements of a liberated market economy. It is quite true that states in markets, if they can get rid of past social commitments, can do a lot to make their citizens internationally competitive, i.e. to teach them how to live in a world of de-regulated relative prices and increasingly unregulated contracts. But it should be noted, and in fact cannot be emphasized enough, that this is different, and very different indeed, from the equalization of living conditions and the de-commodification, however selective, of labor and work that was once part and parcel of the modern welfare state.

No doubt, some countries’ institutions are doing better under the impact of ‘globalization’ than others’. Comparative research emphasizes the differences, and with good reasons. For example, small countries are, or were for some time, more successful than large ones in rebuilding their economies and their welfare states to the specifications of an internationalized economy. But this need not remain so forever—see the fate of the ‘Dutch model’. Citizens of countries who now happen to be on top of the international rankings tend to

⁵Although in several places Campbell observes that even Sweden underwent ‘neoliberal reform’ in the 1980s, the supposedly generic resilience of national institutions to this sort thing obviously notwithstanding (Campbell, 2004, pp. 122, 145). Can one not be impressed by the fact that such ‘reform’ was almost universal in the respective period?

be proud of themselves, and particularly so if their larger neighbors are doing less well. Also, American progressives, of which I take Campbell to be one, need credible role models if they try to convince their fellow citizens that a European-style welfare state would be worth having after all. But sometimes their insistence that all can be well with the European welfare state provided it is managed well sounds a bit like whistling in the dark, a pep talk to a team that has long forgotten what it feels like to win, or even like an attempt at faith healing. Comparative research has shown that small countries can perhaps ride the world economic tiger a little longer than large ones. But is this reassurance that they will be able to continue doing this forever, without having to throw off more and more of their citizens, like the USA and the UK did and Germany and France have long refused to do, at rising costs and with increasingly unsustainable effects?

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Varieties of institutionalism and their problems: some comments on John Campbell's *Institutional Change and Globalization*

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The problem of institutional change

According to John Campbell, a central problem in much institutionalist analysis concerns how we are to understand institutional change. This becomes a critical issue when dominant institutions are considered to determine the nature and behaviour of socio-economic groups and organizations in market economies such that they are largely limited to fulfilling pre-ordained roles. To the extent that institutionalist analyses presume that institutions are cohesive and mutually reinforcing in their implications for economic and other actors, they find it difficult to account for major socio-economic change. Given that many social scientists believe that substantial change has occurred in the leading OECD states since the collapse of the Bretton Woods system, this poses a serious problem for institutionalist analysis in the view presented here.

This problem is considered to have three major aspects: how to identify the nature of institutional change, how to explain it and how 'ideas' help to account for it. Although it is clearly important to be able to describe a phenomenon adequately if we are to understand it, and identifying the key mechanisms involved in producing it seems crucial to providing satisfactory explanations of institutional change, I am less convinced of the centrality of the role of ideas, or beliefs, as a key issue in accounting for such change. In what sense are explanations based on moral and other beliefs a problem of the same order as arriving at adequate descriptions and providing causal accounts of institutional change? As potential explanatory factors, they are one set among many candidates and do not seem to pose a problem in the same way as the previous two might. Furthermore, it seems odd to separate 'ideas' from 'interests' given that, as the author claims on page 91, 'Interests are a particular type of idea among many. . . . like other types of ideas. [They] are socially constructed'. It would seem more productive to explore the ways in which different kinds of interests and other motivating beliefs are constructed by differently constituted groups of actors to affect patterns of institutional change.

However, that would call into question the tripartite division of institutionalist analyses that underpins this book. Like others, Campbell distinguishes between rational choice, organizational and historical varieties of institutionalism as three distinct 'paradigms', or frameworks of analysis, that deal with his three sub-problems in different ways. Aside from the misleading use of Thomas Kuhn's term in this context—perhaps not surprising in the light of Kuhn's own ambiguities about its meaning and use (Masterman, 1970; Martins, 1972)—this summary of three recent strands of thought about institutional change and their central role in structuring this book highlights an apparent conflict between two purposes.

First, in presenting an overview of some approaches and how they have dealt with certain issues, or failed to do so adequately, the book attempts to offer an introductory survey of the field for new researchers, in a comparable manner to Scott's *Institutions and Organizations* (1995). This objective might account for the frequent labelling of some authors as being a particular type of institutionalist, or, in Burawoy's case, of not being one at all, which appears rather odd and unnecessary to many European social scientists. Second, the book also purports to make a substantive contribution to the analysis of institutional change, to the extent of suggesting 12 propositions about it that could form part of a new theory. Although it may be possible to combine these two goals in a single volume successfully, it is not a straightforward matter to do so, and in this case, the desire to incorporate a discussion of currently influential approaches, particularly those fashionable in North America, seems to me to have seriously inhibited the substantive contribution that could have been made.

This wish to incorporate many of the present frameworks governing social scientific research about socio-economic phenomena is especially unfortunate in the construction of rational choice institutionalism. At least in its more reductionist variants, it would be odd to regard this approach as reflecting institutionalist presumptions at all, except in the very general and usually implicit sense that it takes for granted the key institutions that underpin modern market economies and strategic decision making in them. By assuming hyper-rational actors with perfect knowledge of their interests and of how to make trade-offs between alternative actions and preferences, many rational choice theorists exclude institutional arrangements from any explanatory role in their accounts, especially in constituting socio-economic groups as particular kinds of actors with properties that vary between institutional contexts.

However, once it is agreed that actors' interests are institutionally constructed, and that rational action reflects socially constituted preferences and cognitions, then the processes through which different groups come to have particular beliefs and conceptions of the world, their roles in it and their interests

become central to any adequate account of socio-economic and institutional change. In this sense, ideas and beliefs are necessarily crucial to the understanding of all social phenomena and should not be separated as a distinct 'problem'.

Furthermore, to suggest that the institutional constitution of actors is simply a matter of institutions guiding and enabling them, as is done on page 62, is to ignore, or downplay, the significant role of societal institutions in constructing different types of collective socio-economic groups and organizations as particular kinds of actors that are able to carry out different kinds of activities in varied societal environments. Similar to much institutionalist economics, this view focuses on the regulative aspect of institutional rules at the expense of their constitutive role, which many Anglophone philosophers of the social sciences have been emphasizing at least since the publication of Winch's *The Idea of a Social Science* in 1958. This feature of institutions specifies the ontological status of socio-economic actors and their capabilities, whereas their regulative aspect specifies the nature of appropriate behaviours. It is the unwillingness of most rational choice approaches to socio-economic phenomena to take account of the institutionally variable nature of actors, and their presumption of the protean, universal and non-socially differentiated nature of firms as economic actors, which distinguishes them from institutionalist accounts.

Returning to the two major issues in the analysis of institutional change, John Campbell suggests that the key concern is to identify both the degree of change involved, especially whether it is evolutionary or revolutionary in a comparable manner to much of the literature on technological change, and the nature of the social mechanisms producing it. It is not especially clear why the extent of change is seen as the central problem or how we should decide what constitutes an adequate description of it. In any event, we are urged to specify the number of dimensions on which change has occurred and the period of time within which it is deemed to have happened. Radical change is seen as involving more of these dimensions within a certain period, whereas incremental shifts in dominant institutions are those that occur on only a few of them.

In addition to the difficulty of deciding which are the central dimensions for assessing the degree of institutional change, how many we should take into account and for which intellectual purposes, this focus on the number of separate aspects of institutions tends to ignore the interdependence of institutions and the extent to which they are complementary in their impact on actors. Radical qualitative change in societal institutions is usually understood to involve the transformation of the whole configuration governing the nature of dominant actors and their resources, abilities and strategies, rather than changes in a number of discrete variables (see, e.g. Amable, 2003, pp. 66–73). It is precisely the inter-related modification of institutional arrangements in different fields that indicates a major change in the environment of socio-economic actors, including, as

Amable emphasizes, the composition of the dominant social bloc. Parenthetically, it may be worth noting here the curious omission of the work of the French regulationist school in this book.

The importance of complementary changes in dominant institutions and coalitions for achieving transformative shifts in socio-economic systems is a major reason why many observers consider the recent modifications in the governance of actors in the capital markets and banking systems of Germany and Japan not to herald radical institutional change of the kind that took place in the former state socialist societies of Eastern Europe (Deeg, 2005; Jacoby, 2005; Vogel, 2005). Not only did many other institutions governing important parts of the economic system in these countries not undergo qualitative change in the 1980s and 1990s, but the impact of the shifts that have occurred has been restricted to particular sectors, firms and social groups. In considering the degree of institutional and socio-economic change, then, the connections between dominant institutions are at least as important as the number of their characteristics that have changed—even if we can agree on what these are and how we should assess changes in them.

Turning now to consider the mechanisms that might account for institutional change, the key ones considered here are path dependence, bricolage and translation. Path-dependent explanations are regarded as being better suited to explaining continuity rather than change, while bricolage reshuffles existing institutional elements to create novelty and translation involves the importation, transformation and application of elements from external sources. Bricolage is seen as primarily a mechanism for evolutionary change since it necessarily limits the range of institutional principles and practices available for innovation. With this process, institutional entrepreneurs achieve incremental change by reshaping existing ways of thinking and legitimating practices within the available repertoire of justificatory norms and values.

More radical changes depend on the translation and adoption of new elements from outside the social system. As John Campbell rightly emphasizes, traditional diffusion studies tended to assume that what was being transferred between people, organizations and cultures was largely unmodified by the diffusion process. This is both theoretically and empirically improbable, as extensive studies of material and social technology transfers within and between organisations have confirmed (see, e.g. Abo, 1994; Hibino, 1997; Boyer *et al.*, 1998; Hancke and Casper, 2000). It therefore becomes important to consider how institutional elements are transformed when translated, as well as how such processes affect institutional arrangements in different kinds of 'receiving' society. As with many processes of change, such translation is by no means a simple, linear and 'rational' adoption of new, more efficient, practices but often takes the form of a struggle between various groups pursuing distinct objectives with opposing principles in which the outcome is often uncertain.

Although this account of bricolage and translation mechanisms may be helpful in explaining institutional change, albeit perhaps not as novel as seems to be implied (see, e.g. Lanzara, 1998), it is unclear why these particular processes have been singled out for special attention or how we should identify and compare different institutional principles and practices. If they are indeed central to the study of institutional change, then presumably any substantive theory should suggest how different kinds of actors are able to recombine and/or translate varied sorts of institutional elements for particular purposes and achieve specific changes in different circumstances. Although some examples of these processes are provided in this book, it remains unclear how such a theory will develop and it is difficult to avoid entirely the suspicion that using this sort of terminology will result more in relabelling commonsense descriptions than providing substantive explanations of institutional change.

Similar points can be made about the discussion of how cognitive and normative ideas can help to generate institutional change. Distinguishing between paradigms, programmes, frames and public sentiments in terms of their relative focus on outcomes—or cause–effect relations—and the extent to which these ideas are explicitly used in public debates, John Campbell suggests how different kinds of actors develop and mobilize support for them to create change. Again, though, it is not obvious why these dimensions were selected, or how we are to identify different kinds of paradigms, frames, etc., their likely development by particular groups in various situations or probable outcomes. If these types of beliefs, frameworks and ideas are so critical to explaining institutional change, then we should be able to suggest how different kinds will be more or less likely to be used by certain groups to accomplish specific objectives and be relatively effective in doing so in different circumstances.

The intellectual utility of these distinctions and guidelines is then demonstrated by considering how ‘globalization’ is affecting political and economic institutions. Reducing this multifaceted and often vaguely defined phenomenon to the increasing international mobility of investment capital since the breakdown of the Bretton Woods system, it is seen here as creating a major problem for the sorts of institutional analysis discussed in this book. This is because some scholars think that the growing freedom of investors to move capital across national borders enables them to put increased pressure on national policy elites and the strategic managers of large firms to follow investor-friendly strategies. These are usually held to involve neo-liberal state policies and the maximization of shareholder returns. As a result, globalization is supposed to encourage the standardization of both national-state economic policies and corporate priorities regardless of nationally distinct institutional frameworks. According to the more enthusiastic proponents of what Bill Emmott termed ‘globaloney’ (1992, pp. 29–40; compare, Doremus *et al.*, 1998), then, nationally

specific institutions are increasingly irrelevant to the explanation of socio-economic phenomena.

Even if the degree of capital mobility has grown as much as some have claimed, and this remains very much in dispute, particularly relative to that prevailing during the Gold Standard period, it remains unclear exactly why this should be such a 'problem' for institutional explanations of socio-economic phenomena. First of all, increasing external financial pressures need not necessarily imply the homogenization of state and corporate policies if, as most of the authors considered in this book would agree, national and other institutional arrangements help to generate particular kinds of competences in leading strategic actors. Firms are more likely to respond to growing 'outsider' influence—and it is worth noting that most large companies in the major OECD economies rely relatively little on external finance for investment purposes—by focusing on their different capabilities rather than all competing in the same way across international markets. Slavishly imitating what is considered to be current 'best practice' by, for instance, imitating lean production techniques is impossible to do without some translation, as this book emphasizes, and also implies an inability to innovate successfully. By always trying to follow the industry leader, companies will never reap first-mover advantages and be permanently condemned to competing on other firms' terms.

Similarly, states in institutional regimes that provide certain competitive advantages for their leading firms in some technologies and markets would be ill-advised to abolish these in the hope of catching up with the apparently more successful ones. Insofar as globalization is creating common financial pressures for leading companies and state élites in the major OECD economies, then, these forces are just as likely to increase the heterogeneity of policy responses and firm strategies as decrease it, as indeed happened in the early part of the 20th century.

Second, as many critics of the hyper-globalization argument have emphasized (see, e. g. Weiss, 2003), it is peculiar to treat globalization as an external force compelling states to adapt to its apparent imperatives when it is largely the actions and policies of states that helped to create the international capital market in the first place. Especially within the European Union, but also more widely, many states have agreed to share some sovereignty and reduce national barriers to economic interdependence in order to reap collective advantages and improve their joint capabilities to manage market economies. How they deal with the consequences can equally be expected to reflect national elites' perceptions of their strategic interests and their ability to mobilize their own and other states' resources, as well of course as the nature of dominant institutional regimes in different societies and allied coalitions. As Laurence's (2001) study of the 'Big Bang' deregulations—or perhaps more accurately re-regulations—of

the British and Japanese capital markets reveals, both the nature and effects of these superficially similar state policies were quite different as a result of the very different institutional contexts and dominant coalitions.

Third, insofar as some states have succeeded in creating relatively stable and effective transnational institutions governing economic activities, such as the WTO and various free trade agreements, as well as more political arrangements such as the European Union, in the latter half of the 20th century, and these have intensified cross-border competitive pressures, this highlights the continued importance of institutions in structuring economic phenomena. However much significance one attributes to the increase of global business regulation (Braithwaite and Drahos, 2000), and the extension of multilevelled governance (Bache and Flinders, 2004), for the perceived internationalization of competition and shareholder pressures, it remains clear that the relevance of the institutional framework within which economic actors are constituted and governed, at subnational, national and transnational levels of collective organization, is still considerable. Overall, then, there does not really seem to be a 'problem' of globalization for institutional analyses of economic phenomena, whether these are organizational, historical or any other variant one cares to construct.

In the final chapter of this book, John Campbell summarizes his ideas in the form of 12 propositions that he suggests could constitute a rudimentary theory of institutional change. This kind of change is understood as a process of constrained innovation, a term that is curiously reminiscent of Kuhn's account of the 'essential tension' between novelty and tradition in the history of science (1977, chapter 9; see also, Whitley, 2000, pp. 9–29). However, many of these are so general and remote from empirical phenomena, not to say tautologous, as to be of limited use in characterizing and explaining patterns of such change.

Proposition 1, for example, asserts that institutional change can be triggered by problems that are either exogenous or endogenous to the institution in question, whereas proposition 7 claims that change is more likely when the entrepreneurs in favour of it command more resources than their opponents. One does not have to agree with Popper's strictures about the assessment of scientific knowledge claims to wish for rather more empirical content than is contained in these and other suggestions. In particular, some idea of which kinds of actors are likely to become effective institutional entrepreneurs and bring about specific forms of change in particular circumstances would be welcome.

It also remains obscure how we should decide which types of programmes for institutional reform could translate well to particular contexts and in what sense they could be said to fit specific circumstances. Overall, many of these propositions seem unlikely to provide much additional explanatory value by, for instance, identifying the causal processes through which particular types of institutional changes are likely to occur in specific kinds of ways in given contexts.

To do this, we would need to develop a systematic framework that: (a) specifies what is changing that requires explanation, i.e. what kinds of changes in which institutional arrangement need to be accounted for and suggests how we should decide that a new account is better than previous ones, (b) identifies the key groups and the nature of their interests and beliefs that were involved in supporting and opposing such changes, together with their material, organizational and ideational resources and (c) shows how and why they were, or were not, effective in different societal contexts. Such a framework would have to show both how particular kinds of changes, institutions, actors, beliefs, resources, mechanisms and logics of action differed and how these different types were interconnected in certain situations to result in varied outcomes.

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Challenges for institutional theory: a response to Crouch, Streeck and Whitley

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Overall, Colin Crouch, Wolfgang Streeck and Richard Whitley raise five basic questions about my arguments in *Institutional Change and Globalization*. In my view, these also point to more general challenges for institutional theory

and comparative political economy. Before I respond, let me review the book's key points in order to put my comments into proper context.

Synopsis of the book

Historical, rational choice, and organizational institutionalists have been struggling with three problems for years. The first is how to measure institutional change and how to determine the degree to which change has occurred. I argued that this can be resolved by selecting the appropriate dimensions of the institution in question and tracking change in them over an appropriate period of time.

The second problem is specifying the mechanisms by which change occurs. I maintained that this often involves a process of *bricolage* whereby actors recombine bits and pieces of already existing institutional principles and practices in their local environment to create new institutional forms and practices. Occasionally, they also add bits and pieces that diffuse to them from elsewhere—a process I called *translation*. These two processes tend to produce path-dependent change in the sense that the new institutional arrangement resembles its predecessor due to the fact that it contains institutional artefacts inherited from the past.

The third problem is how to incorporate an ideational perspective into our understanding of institutional change. I identified several types of ideas and explained how actors use some types to alter institutions but in ways that are constrained by other types.

Next, I demonstrated the utility of all these arguments by using them to analyze globalization in a *limited* way. I showed, contrary to much conventional wisdom, that during the late 20th century national tax regimes did not converge on a neo-liberal model. This was because national institutions mediated the degree to which global pressures for convergence, notably the threat of capital flight, affected national policy making. I also suggested that globalization posed a problem for institutional theory in the following sense. If it is correct that cross-national institutional differences are disappearing due to globalization, then the utility of theories that focus on institutional variation to explain national differences in socio-economic performance will be compromised.

Finally, in order to summarize my arguments, I offered a rudimentary theory of institutional change that also synthesized various insights from the three versions of institutional theory. This took the form of a series of general propositions about the conditions under which institutional change would likely occur.

Institutional determinism versus constrained innovation

The first question the reviewers raise concerns the degree to which my version of institutional theory is excessively deterministic. Streeck implies that, like other

institutionalists, I am too much of a determinist who believes that institutions determine everything—a concern that seems to stem from my argument that institutions tend to be resilient in the face of globalization. On the other hand, Whitley seems to think that I am not enough of a determinist insofar as he wants sharper predictions from my propositions. Both miss the point, although Crouch does not. Mine is a probabilistic theory of institutional change—not a deterministic one. The philosophy of science involved need not concern us here other than to note that because we are concerned with human behavior we can never model perfectly what people will do in a given situation (see Lieberson and Lynn, 2002). This is why we need a theory of *constrained innovation* to explain institutional change, such as the one I offered, where creative institutional entrepreneurs take center stage, and where their behavior can never be fully predicted in advance (p. 173). The best that institutional theory can aspire to are probabilistic statements about people's tendencies within constraints.

This is why I believe that it is important to specify very carefully the *mechanisms* (i.e. processes) by which institutions change. Specifying mechanisms helps to guard against excessive determinism. This is also why I offered a theory of institutional change that revolves around the processes of bricolage and translation. This actor centered institutionalism, as Crouch calls it, recognizes that although actors are constrained by institutional conditions of various sorts, they enjoy some—although by no means unlimited—creative leeway in how they transform institutions. My purpose in making this argument was to offer a way to think about path-dependent change without excessive determinism.

Of course, as Whitley and others remind us, bricolage and translation are not the only mechanisms by which institutions change.¹ There are others like institutional drift and decay, where the functions of institutions shift, often unintentionally, over time (e.g. Streeck and Thelen, 2005). But bricolage and translation seem to be especially important mechanisms insofar as researchers have found them operating in a wide range of circumstances.² Moreover, if we want to account for intentional changes in institutional structures, as opposed to unintentional changes in institutional functions, then we need to pay close attention to the processes of bricolage and translation precisely because they involve intentional action (see also Campbell, 2006).

¹Whitley reports incorrectly that I discussed three mechanisms of change: path dependence, bricolage and translation. There were only two. Bricolage and translation are processes that result in path-dependent change.

²For example, the collection of studies edited by Streeck and Thelen (2005) show several mechanisms of institutional change. It appears that bricolage, or what they call layering, is among the most frequently occurring mechanisms in their empirical cases.

That said, we still need to know more about how bricolage and translation operate. How do entrepreneurs utilize their material resources and interpersonal and interorganizational networks to advance new institutional options? In particular, how do they imagine and then promote new institutional ideas? My concern with this is why I devoted a chapter to a discussion of ideas and how actors strategically manipulate one type of idea, frames, in order to convince other actors of the utility of pursuing another type of idea, programs, that may precipitate institutional change. This is also why I explained how this process transpires within ideational constraints imposed upon these actors by two other types of ideas—cognitive paradigms and public sentiments. This is not to say that I am an idealist. Material conditions matter too. But to understand fully how institutions change, I believe, like Weber, that one must account for the effects of both factors.

Whitley seems confused about this. He notes initially that he is ‘less convinced of the centrality of the role of ideas, or beliefs, as a key issue in accounting for such change’ than I am. But later he acknowledges that the processes by which actors come to have ideas and interests are ‘central to any adequate account of socio-economic and institutional change’. Moreover, while he thinks that it is important to understand how the ideas and interests that motivate change are constructed, he implies that I do not. Yet, as I have just explained, the social construction of new institutional ideas is a central feature of my argument. He also wonders why anyone who is trying to understand the relationship between ideas and institutional change would waste their time with rational choice theory, which tends either to ignore or to take for granted institutions and their idealistic underpinnings. But several rational choice theorists now incorporate institutions into their arguments—and some have also carved out analytic space for an ideational account of rational action. These are the rational choice theorists that interested me (pp. 29–30; see also North, 2005). Of course, historical and organizational institutionalists have also studied the relationship between ideas and institutional change. I cleared up some of the confusion in these disparate literatures by showing how different types of ideas affect institutional change.

Parsimony versus eclecticism

The next question the reviewers broach involves whether my theoretical position is too eclectic and should have been more parsimonious. If understanding institutional change requires that we account for the effects of actors, their material resources and interests, their social and institutional locations and the ideational conditions within which they operate, then we need to draw on the insights of several heretofore distinct versions of institutional theory. In other

words, we need a more theoretically eclectic approach to the subject of institutional change. This may make it difficult—at least initially—to offer a parsimonious theory. But my concern was more with pushing our understanding of institutional change in a direction that acknowledges its complexity than with defending one narrow albeit parsimonious theoretical position versus another.

Hence, I take exception with both Whitley, who criticizes me for developing a set of causal propositions that are, in his view, poorly grounded, and Streeck, who comments without explanation that my effort to synthesize elements from the three institutionalist paradigms is ‘probably futile’. First, the propositions that I offered in the concluding chapter summarize arguments that I made in earlier chapters of the book and that were developed from the three prevailing institutionalist paradigms. I omitted references—that is, grounding—to the empirical and theoretical literature upon which these propositions were based because these references were provided in previous chapters (p. 173). If they wished, readers could return to those chapters for grounding in the relevant literature.³ In any case, the propositions bring together various strands of my argument into a coherent whole so that my position on how institutions change is clear. When linked together, they tell a coherent story about institutional change—one that differs sharply from other accounts, such as those based on population ecology theory, functionalism, and transaction-cost analysis (e.g. pp. 20, 67–68, 72–73, 81). Second, I phrased these propositions in such a way that other scholars could then derive more specific hypotheses from them, operationalize the variables involved and test them empirically. I intended them as the initial basis for a rudimentary theory of institutional change—one that I welcomed others to challenge and revise where necessary. And given the fact that these propositions have helped generate considerable dialogue within this symposium, which in my view can help improve institutional theory, I suspect that my effort was not futile (see also Guillén, 2006).

Institutional change as objective versus subjective reality

The third question the reviewers pose concerns whether we should view institutional change through objective or subjective lenses. In my discussion of the

³Had Whitley recognized this, he might have understood, for instance, the significance of my first proposition, which specified that episodes of institutional change can be triggered by either exogenous or endogenous factors. Although much has been made of the exogenous forces that can trigger change, the fact that much less attention has been paid to endogenous forces is a serious omission in the literature that I discussed in an earlier chapter.

selection of institutional dimensions to track in order to determine how much change has occurred, I wrote that we need to decide whether we want to focus on objectively given indicators, those that are viewed subjectively as important by actors operating within the institutions we are studying or some combination of both (pp. 37–38). Crouch presses for further clarification on this issue.

Insofar as I defined institutions as rules and the monitoring, sanctioning and meaning systems associated with them, both objective and subjective indicators are implicated. However, I may have given the impression that the former are more important than the latter insofar as the empirical part of the book focused on the objective rule-based side of tax regimes as a dependent institutional variable. Although Crouch agrees with many of my arguments, I am perhaps more of an empiricist than he would like. Still, we agree that to refuse to perceive changes until social actors themselves announce them is to abdicate our professional role as social scientists.

But let me be clear. If one is interested in determining the degree to which an institution as a set of rules, monitoring and enforcement procedures has changed, then tracking objective indicators will suffice. (The analysts can always pay special attention to those objective indicators that actors believe to be most important, if they want.) If one is interested in determining the degree to which an institution as a meaning system has changed, or whether actors believe that an institution is legitimate, then tracking the subjective perceptions of the actors involved is more important. It all depends on the dimensions of the institution with which the analyst is concerned.

This leads to another thorny question: When do we know if relatively minor rather than major or, as Crouch puts it, ‘systemic’ change occurs? That is, when does change accumulate to the point that an institutional configuration slips from one analytic category or type into another? How many dimensions of an institution must change—and to what degree must they change—in order to tip the balance from minor to major change? This has not been resolved in institutional theory. As Crouch points out, this is partly an exercise in picking adjectives and labeling. However, this dodges the issue, which can only be resolved, to reiterate, by specifying all the important dimensions of an institution, and determining how many of them change and by how much over an appropriate period of time. In turn, these methodological decisions must be guided by the analyst’s theoretical concerns and by the particular empirical context in question.

Analyzing national political economies: institutional versus inter-institutional change

Fourth, the reviewers ask how we should analyze national political economies. National political economies are, of course, institutionally complex, consisting

of many different institutions. So it is not surprising that Whitley, Streeck, Crouch and others argue that institutional interdependence should be the focus of our attention, not a single institution in isolation from the rest. This is because it is the complementary relationships among institutions that matter—not so much because they affect how institutions are *structured*, but because they affect how they *function*. And Streeck suggests that if I had adopted this inter-institutional perspective, then I would have concluded that globalization has led to more fundamental institutional changes in national political economies than I acknowledged.⁴ Two issues are at stake here.

The first one is methodological and concerns levels of analysis. The call for an inter-institutional analysis of change is not incompatible with my approach. Rather than tracking change in a set of dimensions within a single institution, like a tax regime, change can be tracked along critical dimensions in several institutions (e.g. Amable, 2003; Crouch, 2005). And although the results of such an analysis might reveal that there was significant change in some areas, it might reveal much less change in other areas. For instance, Danish labor markets are influenced by three relatively distinct institutions: employment protection laws, welfare programs and vocational training. Since the late 1980s, the last two have changed in important ways, but the first has been rather stable (Campbell and Pedersen, 2007). Indeed, as I noted, change is often uneven and lumpy both within and across institutions (p. 39). No one in their right mind would deny that change in national political economies has occurred of late. The question is how much and in which institutions. This can only be determined by systematically tracking change across a variety of national institutions using the multidimensional approach that I advocated.⁵

The second issue involves exactly what sorts of institutional dimensions we should track. Recall that my focus was primarily on institutional structure. I paid much less attention to the functions of institutions, which seem to be of greater concern to Streeck, who, for instance, notes that many welfare states have shifted from equalizing living conditions and decommodifying labour to different purposes despite relatively little change in formal programs or funding levels. Of course, underlying this concern about inter-institutional change is an important debate about institutional complementarities. The argument is that the efficiency—that is, the functional performance—of a national

⁴In fact, I anticipated this sort of criticism and so reminded readers that my goal was not to provide a comprehensive analysis of all the effects of globalization, but, more modestly, to illustrate the utility of my earlier arguments about institutional change by examining a single institution—national tax regimes (pp. 132–33).

⁵I will not discuss here the potentially wide institutional variation across sectors within any national political economy (e.g. Campbell *et al.*, 1991).

political economy depends largely on how well different institutions fit together. Two institutions fit well or are complementary when the presence of one increases the efficiency of the other (Hall and Soskice, 2001, p. 17) or when they compensate for each other's deficiencies (Crouch, 2005; Campbell and Pedersen, 2007).

But taking a functionalist approach to measuring institutional change is fraught with difficulties. (a) Determining precisely what functions an institution performs can be a devilishly tricky business, especially if we take seriously the notion that some functions may be less obvious than others (e.g. Merton 1967). (b) Picking important functions is not always straightforward. Even proponents of the institutional complementarities perspective seem to disagree about which functional indicators are most important to track (cf. Hall and Gingerich, 2004; Kenworthy, 2006), if for no other reason than that selecting the 'most important' functions is partly a normative matter. For instance, we might disagree whether equality of living conditions is more important than growth of employment possibilities, productivity or national income. (c) There can be a disconnect between institutional structure and function. That is, institutions may persist for long periods of time despite the fact that the functional outcomes associated with them may have changed (p. 181; see also Streeck and Thelen, 2005). (d) It is easier to determine the degree to which institutional structures have changed than it is to determine the degree to which these changes have affected functional performance. This is because it is often hard to determine precisely how much institutions rather than other factors affect national performance (Schwartz, 2001).

This last point is worth elaborating. Knowing how much institutions (i.e. rules and meaning systems) change is one thing; knowing how much their performance (i.e. function) changes is quite another. For instance, it is relatively straightforward to determine how much Danish employment protection, welfare and vocational training institutions have changed. It is more difficult to determine what effect this had on Danish socio-economic performance (Campbell and Pedersen, 2007). Both structure and function are important, but it is not clear to me why an analysis of the former *necessarily* entails an analysis of the latter.

Hence, I agree with Crouch that change is often ambiguous—especially when we are talking about national political economies. And it is ambiguous because it is multidimensional. There may be important differences between the changes that we can observe, on the one hand, in the formal or informal structure of institutional arrangements and, on the other hand, in the functions of those institutions. Nevertheless, if we agree on which structural and/or functional dimensions to track, the methodological guidelines I offered in my book and the techniques that I discussed (e.g. cluster analysis)

are entirely appropriate—even if we want to explore inter-institutional change (e.g. Amable, 2003).

Globalization then versus now

The reviewers also raise questions about globalization. On the one hand, Whitley wonders what all the fuss is about globalization in the first place and whether rising international economic activity is really so significant. The answer is yes. There is plenty of evidence, including a variety of OECD indicators, that things like trade, foreign direct investment, portfolio investment, and the like have increased substantially over the last quarter century and that nation-states have responded to them (e.g. Kapstein, 1994; Weiss, 2003). Although some of these indicators have increased more than others, and these increases have not been uniform across countries, these are not phenomena that can be dismissed easily.

On the other hand, Streeck wonders whether I should be classified among those scholars who find ‘nothing new under the sun’ in the sense that rising international trade, financial flows and the like occurred in both the early 20th and the late 20th centuries. The answer is no. There is much that is new under the sun, but not everything. Although I suggested that there are some similarities in both periods, I also acknowledged that there were some important differences that make the recent period somewhat unique, such as its much greater market volatility, the faster speed with which capital, goods and services flow across national borders, and the changed nature of today’s capital, goods and services (pp. 134–135). I compared globalization in these two eras simply to show that the effects of rising globalization on national institutions were very different in each era and that, therefore, we should not assume automatically that globalization inevitably leads to a particular type of institutional change regardless of time or place. Thus, I agree with Streeck that we should make generalizations only within clearly specified scope conditions along these lines (pp. 63–64).

I am much less sanguine about Streeck’s argument that where once markets were embedded in states, today states are embedded in markets. To begin with, remember that nation-states contributed to the rise of economic globalization in the first place (Campbell, 2003). So did transnational organizations (e.g. IMF, World Bank, WTO, etc.) that were created by nation-states. And among nation-states, the USA stands out insofar as it was the world’s hegemonic political and economic power throughout the period in question and was decisive in fostering the international liberalization of trade and capital flows. Of course, this does not mean that once these global forces were unleashed nation-states were immune from their effects. Nor, however, does it mean that once these global forces were unleashed, nation-states were entirely at their mercy. Let me explain.

Some have argued that the nation-state has been hollowed out or otherwise incapacitated by the forces of globalization and that the institutional tool kits that states once used to contend with market forces are now largely useless. In reality, states do not throw away their tool kits. They are more apt to set aside some tools, add some new ones and continue to work with many of those inherited from the past. And if some tools are jettisoned initially, as the Swedes tried to do when a conservative government implemented a big regressive tax cut in 1991, they can be retrieved later. After all, when the Swedish Social Democrats returned to power a few years later they implemented a big progressive tax increase (pp. 161–162). Streeck himself seems open to the possibility of retrieval as he wonders whether the German labor movement may yet ‘devise more effective counter strategies’ to the recent tendency to undermine the old industrial relations settlements. The point is that to assume that the development of a more internationally oriented form of capitalism will inevitably overwhelm national political economic institutions—and to suggest that states are now entirely embedded in markets—is excessively deterministic and fails to recognize that history ebbs as well as flows (e.g. Campbell, 2003).⁶

Last but not least, Streeck suggests that I am battling a straw man. He worries that nobody really believes that rising economic globalization will lead to convergence on a set of neo-liberal institutional arrangements, including lower taxes, or that national economies necessarily falter unless they adopt such reforms. I wish he were right. However, many scholars and pundits have made just this sort of argument (pp. 125–127). International agencies have also expressed this view (OECD, 2000). And one need look no further than the current Republican administration in Washington to be reminded that conservative politicians continue to espouse this neo-liberal mantra. Far from being a straw man, this is an argument that continues to be heard in academia and the press and that continues to influence politics. It is well worth fighting. Continuing to improve institutional theory can help with that fight.

Conclusion

In sum, institutional theory needs to pay more attention to the processes of constrained innovation in order to avoid excessive determinism in its arguments. It needs to adopt a more theoretically eclectic position, at least for now, in order

⁶For this reason, if metaphors are necessary, then I would prefer not to talk about states being ‘embedded’ in markets, or vice versa, which implies one-way causality. Instead, perhaps we should talk about the institutional ‘membrane’ that always connects the state and market with reciprocal albeit historically variable and politically contested effects.

to grasp the complexities of institutional change—even if this results in less parsimonious theories than we might prefer. It needs to acknowledge that there are both objective and subjective aspects to social reality and that each reflects different aspects of that reality. It needs to recognize the multidimensional and interconnected nature of national political-economic institutions. But it also needs to be careful not to reduce the analysis of national institutional change to an inquiry about national institutional functions and performance. And it needs to recognize for both scholarly and political reasons the reciprocal relationship between national institutions and international political and economic forces.

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