This article explores the selective affinities between the study of West European politics and historical institutionalism. We divide the last 30 years into four phases: the foundational ideas of the late 1970s and early 1980s; the evolution of these ideas from structuralism to institutionalism in the late 1980s and early 1990s; more radical revision under the turbulent 1990s and early 2000s; and the future outlook at the end of the first decade of the 2000s. We emphasise the ways in which the field of West European politics has shaped the direction of historical institutionalism as a distinctive approach to the study of politics, particularly historical institutionalism’s focus on explaining actors’ interests and behaviour. We also discuss recent debates within historical institutionalism concerning the role of history and path dependence, ideas, and institutional origins and change in the context of developments within West European politics. We conclude by discussing several challenges for both historical institutionalism and the study of West European politics: maintaining and improving analytical rigour as politics in Western Europe become even more fluid; continuing to build middle range theory; and extending our comparative analysis of Western Europe to include regions outside of Western Europe.

The ‘new institutionalism’, and in particular, the branch of the new institutionalism known as ‘historical institutionalism’ has not only influenced the study of West European politics, but indeed, in some respects, this approach emerged out of the study of West European politics itself. As the editors point out in the introduction to this volume, the field of West European politics in the founding days of West European Politics was very much concerned with the institutions of West European politics. In contrast to a much earlier generation’s concern with institutions in the interwar period – that termed ‘formalist’ in the field of domestic politics, and ‘idealist’ in the field of international politics – the focus of the new generation of West Europeanists was not in the main on formal political institutions, such as constitutions, but on institutions understood as the...
informal rules of the game, organisational patterns of political behaviour, and organisational structures both within and outside of government.

The world of West European politics also contrasted, however, with the post-war successors to the formalists and the idealists – the behaviouralists and the realists, respectively – in several respects. First, the state was neither ignored nor treated as either a neutral arena or as just another interest group, as in the behaviouralist perspective. Nor was it reified to be viewed as a unified actor and the sole source of political activity as in the realist view. Second, the geographic range of interest was restricted considerably by comparison to the ‘grand theories’ and search for universal generalisations that had characterised comparative politics in the immediate post-war period and especially the ‘political development’ or ‘modernisation’ approach. This reduced range, with its greater respect for historical context and particularity, was also counterpoised to the enthusiasm for Marxist theory in the late 1960s and early 1970s. Thus, in place of functionalist universals of modernisation or an ineluctable logic of capitalism, the focus was on the unique and conflictual pattern of political and economic development in Western Europe, and the institutional legacies that these political crises had left in place. Consequently, the preferred methodology was one of comparative-historical analysis, and the level of theoretical generalisation was focused on the middle range, or even on the peculiarities of a single case, viewed in comparative perspective. Furthermore, the regional focus allowed sustained interdisciplinary cooperation, for example with sociology (Goldthorpe 1984) and history (Maier 1987). The following sections will detail, first, the influence that the field of West European politics has had on the new institutionalism; second, some problems that have emerged; and third, it will point to potential solutions and avenues for future research.

Founding Ideas of the 1970s and Early 1980s

The Artefactuality of Politics

In turning from the class struggle to corporatist theories and studies of trade unions as organisations in a historical and institutional context, the field of West European politics put the issue of interest representation (or interest intermediation, to use Gerhard Lehbruch and Philippe Schmitter’s term; Schmitter and Lehbruch 1979) at the centre of its research agenda. Not just the study of economically based interests, but also that of political parties and social movements, emphasised institutions of interest representation and institutional factors that affected the articulation of political preferences, and their expression in politics. As Suzanne Berger put it, institutions for representing interests were no longer viewed as neutral ‘transmission belts’ for relaying citizens’ preferences to political decision-makers, but as relatively stable institutional fixtures that modified the
process of interest representation by amplifying some definitions of interest and attenuating others (Berger 1981). At the same time, it should be noted that Berger’s work stands out not just for her institutionalist focus on institutions of interest representation, but also for her emphasis on the role of individuals in interpreting their own interests; as well as the role of political contestation in deciding which one of multiple potential constructions of interest will prevail (Berger 1972). In some ways, then, Berger should be considered as ‘constructivist’ as well as ‘institutionalist’.

The corporatism literature addressed the ways in which the same basic class interests could be organised in different ways, and how this influenced politics and public politics (citizen unruliness and governability, for example, in part expressed as demonstrations, strikes and inflationary pressures; Schmitter 1981). Indeed, in contrast to the conservative focus in the 1970s on the ‘democratic distemper’ and ‘government overload’ (Crozier et al. 1975), corporatist theory viewed institutions as potentially helpful for public policy, and as in Lijphart’s institutionalist approach to political systems, viewed West European consensus democracies with their systems of social partnership as a ‘kinder, gentler model’ (Lijphart, 1999; and Hayward 1980; Heidenheimer 1980). To be sure, there was also a leftist critique of corporatism that viewed corporatist institutions as a tool for repressing, rather than modulating, the expression of class interests (Panitch, 1977).

Similarly, the parties literature focused both on the existence of different social cleavages, and the translation of these cleavages into party systems that became ‘frozen’ in the 1920es – the same inter-war period held responsible for the ‘recasting of bourgeois Europe’ into a corporatist mould (Maier 1975). A key question was whether these parties would – as Kirchheimer and Downs predicted – start to converge on a ‘catch-all’ model, or whether their social and ideological distinctiveness would remain intact. Would the ‘end of ideology’ prevail (Bell 2000 [1960]) or the ‘resurgence of class conflict in Western Europe’ (Pizzorno and Crouch 1978) prove enduring? (See for example Wright 1978; Lange and Tarrow 1979.)

The field of social movements was very much influenced by the ‘Tilly model’ which viewed the emergence of social movements as a product of resource mobilisation on the part of challengers from ‘below’, and chinks in the armour of repression from ‘above’. The latter ‘political opportunity structures’ were good examples of the role of states as actors and structures. As actors, states actively intervene to shape the landscape of social groups as in the Tocquevillian approach, at the same time that their structural features provide distinct opportunities and constraints, as in the Weberian model (Kitschelt 1986; Skocpol 1985; Tarrow 1994). More generally, this period saw an upsurge of interest in the state, as well as in the relationship between states and social structures (Evans et al. 1985).

What these studies and approaches share as a whole is a recognition of what one could call the ‘artefactuality’ of politics. That is, they all view politics as being (in part) artefacts of political institutions. Institutions of
interest representation intervene in the process whereby citizens can express their interests, and thus shape what appears as a political interest in public arenas. The particular institution in question may vary – be it a state structure, party system or pattern of interest intermediation – but the basic point is the same: the interests that come to be expressed in politics are shaped by those institutions. Indeed, these institutions may even affect whether these interests come to be expressed at all. Thus, the focus on individual attitudes, so stressed by the political culture framework, took insufficient account of the role played by institutions in mediating between individuals and the politicised visions and organisational forms of their interests.

Because there is some confusion on this issue, it is important to state here that the value of the institutions for expressing interests does not mean that interests do not exist or that they are creations of institutions. Instead, this ‘artefactual’ approach calls for an examination of the empirical relationship between individual preferences and the organisational structures of politics. Interests are expressed through institutions – just as behaviour takes place within an institutional context – making it extremely difficult to separate the impact of institutional conduits from the ‘raw’ preferences or interests of individuals and groups. Both the organisation of interests and the political structures within which they organise shape the demands and issues that are put on the political agenda, and whether or not they even get there. This is what Steinmo (1993) called (using Schattschneider’s term) ‘the mobilization of bias’, and Hall (1986) referred to as ‘the impact of organizational factors on politics’. Thus the need to focus on institutional variation within Western Europe is part and parcel of a theoretical focus on the impact of institutions on the articulation of interests.

**Historical Legacies**

A second general tendency of the study of West European politics that has influenced the new institutionalism, and especially historical institutionalism, is the impact of historical legacies. In response to modernisation theorists’ efforts to delineate a universal pattern of political development consisting of a standard set of crises and functions, conflict theory understood historical development – in particular the emergence of capitalism and the rise of the modern nation-state – both in terms of class conflicts and as a result of struggles for political domination. Though some strands of this historical approach were initially engaged with a Marxist focus on the transition from feudalism to capitalism, this developmental approach quickly turned to variation within Europe and historical particularism (Moore 1966; Beer and Ulam 1972; Anderson 1974; Tilly 1975). As the larger scholarly debate was beginning to ebb by the time WEP was founded at the end of the 1970s, this focus on the rise and development of the modern state was not quite as central to contributors to West
European Politics. Nevertheless, the debate was indeed an important precursor to the WEP agenda, and in fact integral to it, as the impact of history remained implicit in the institutional analysis referred to above. This broader engagement with the long-term development of the state is a central point, for example, in the corporatism literature, which views the role of the state and the historical development of state–interest group relationships as key factors in explaining differences in contemporary neo-corporatist institutions of interest intermediation. Similarly, the Rokkan model, so central to an understanding of party systems, was based on the historical sequence and particular alliances chosen during conflicts leading to the incorporation of new cleavages into institutions of political representation. Indeed, the political system as a whole came to be seen as a product of historic conflicts, whose outcomes depended upon both political choices and pre-existing structures, which in turn reflected a previous set of past choices, and thus could be understood only through a sort of ‘political archaeology’.

In the words of Suzanne Berger:

[P]olitical modernization does not clear the political terrain of all vestiges of the past and raise a new house on bare earth. Rather, modern states have been built on top of and with the half-collapsed, half-standing institutions of the past. States differ, however, . . . because [political elites] make different choices about how much of the past to preserve in the modern political system. (Berger 1973: 334)

These institutional sediments are particularly important for understanding the relationship between religion and politics (Berger 1982), and the territorial dimension of European politics (Rhodes and Wright 1987). Thus, again, as with the issue of the artefactuality of politics, one can observe a selective affinity between the turn to detailed, historically grounded case studies and the theoretical interest in the impact of institutions and institutional legacies on contemporary politics.

The State and Capitalism

Finally, the substantive focus on states and class structures was also critical background for WEP’s approach to understanding governmental structures and public policies. A major paradigmatic work was Schonfeld’s (1965) comparison of the role of the state in various political economies, which was followed up by decades of discussion of convergence versus divergence in modern political economies (Berger and Piore 1980; Zysmann 1983; Hall 1986; Berger and Dore 1996; Crouch and Streeck 1997; Hollingsworth and Boyer 1999). Here a key insight was on the impact of politics – and in particular of state power – on the organisation of the economy. One influential framework that emerged from this ‘macro’ approach to political economy was Katzenstein’s (1978, 1985) focus on ‘strong’ versus ‘weak’
states and societies in explaining foreign economic policies, and domestic institutions. Building on Alexander Gerschenkron’s work, Gourevitch (1985, 1995 [1977]) developed a ‘producer groups’ analysis, which focused on specific sectors and their relationship to the international economy, but also on culturally specific histories of economic ideas. Thus, ideas, interests and institutions interacted over time to produce nationally and even regionally distinct political economies, whose development was subject to political contestation rather than functionalist imperatives. Again, only historical-comparative analysis sensitive to the contextual particularities of Western Europe as a unique region could meet the challenge of understanding patterns of continuity and change in these embedded economies, including tendencies of dualism and discontinuous development (Berger and Piore 1980).

Evolution of the Foundational Ideas in the 1980s and Early 1990s

While a coherent set of empirical and theoretical concerns led to an interest in a regional focus, the emphasis on institutional structures with their historical burdens was also, in a sense, set on shifting foundations. For just at this time West European politics and their institutions were undergoing change, and perhaps moving towards disequilibrium. The two oil shocks of the 1970s were arguably the first portent of the phenomenon later to be termed ‘globalisation’, with their destabilising effects both on domestic social and political institutional arrangements, and on international economic institutions. Institutional structures were looked at as important explanatory variables for differences in the ability of national political economies to come to terms with these exogenous shocks; at the same time, corporatist institutions were becoming less effective, and party systems suffering from ‘decomposition’ and electoral ‘de-alignment’. The ten years subsequent to WEP’s founding saw the rise of right-wing radicalism – but also Green parties; the democratisation of Southern Europe; the neo-conservative attack on neo-corporatist arrangements and the post-war welfare state in Northern Europe; and the beginning of a renewed intensity of European integration.

While these developments are addressed in greater depth elsewhere in this volume, here the important question is about the impact of this concern with crisis and transition on theories and approaches to West European politics. In dealing with stability and change in West European political institutions, scholars began to drop the last vestiges of the older concern with states and classes, and to reject these reified concepts, focusing more stringently on human agency and the exact links between individuals and collective action, including the perceptual lenses of individuals, and the shared understandings of members of groups. Further, perhaps spurred by the rise of feminism and other new social movements on both the left and the right, a much more diverse range of dimensions of interest beyond the basic
economic duality of capital and labour became prominent in research on Western Europe. States and political systems came to be disaggregated into concrete institutional configurations, with their own (and sometimes conflicting) logics. As voters dislodged themselves from their social origins, the role of parties as strategic actors became more obvious. By the end of the 1980s, a concern with the ‘new institutionalism’ had replaced the older emphasis on states and social structures (March and Olsen 1984; Steinmo et al. 1992; Hall and Taylor 1996). In the area of political economy, this took the form of an even greater recognition of the role of political conflict and ideas in economic and technological development (see for example Piore and Sabel 1984; Hall 1993; Ziegler 1997). In the area of welfare state research, this meant more fine-grained analysis of particular instances of policy-making in their historical context in place of a more structural approach (cf., e.g., Weir and Skocpol 1985 to Skocpol 1992 or Weir 1992). In the areas of political development and interest representation, it meant greater recognition of the role of strategic choices by political actors, such as party and interest group leaders, and political contingency, in place of historical constraints and state structures (Kitschelt 1989; Bartolini and Mair 1990; Thelen 1991).

From Structures to Institutional Configurations

As stated at the outset, the branch of the new institutionalism most centrally associated with West European politics is historical institutionalism. This approach relies heavily on the insights generated by this regional, qualitative focus on the artefactuality of politics, historical legacies, and the specific development of states and economies in Western Europe. Yet, in order to pin down the effects of institutions and history, historical institutionalists have tended to further narrow the range of analysis, focusing on explaining specific political or policy outcomes, and performing what has sometimes been called a problem-oriented type of research (for positive views, see Scharpf 1997; Steinmo 2007; more critically, Katzenelson 1997; Schmidt forthcoming). At centre stage have been interest groups, parties, governments and public policies, rather than capital, labour, states and the long-term sweep of history. The puzzles to be explained are generally empirical: why was one policy chosen rather than another? How can we explain the greater popularity of right-wing populism in one country rather than another? The preferred method is generally historical-comparative analysis.

To be sure, the influence of historical institutionalism has not been evenly distributed amongst all areas of interest in WEP. In fact, while the range of research can be said to have narrowed over the course of the 1980s, the theoretical perspective can be said to have become more diverse and varied. That is, in the early days of WEP an interest in political economy understood in terms of states and classes, and a critique of the modernisation approach to comparative politics provided a common set
of questions. With the later efforts to deepen the institutional and political dimensions of analysis, however, we can observe further diversification. The parties literature kept a much stronger behavioural focus on individual preferences and the socio-economic basis of interest cleavages, but also focused on the role of party strategies (that is, on institutionalised actors) in mediating electoral change (see for example Mair and Smith 1989). Work on political economy maintained an emphasis on unions, producer groups and interest coalitions, but also focused to a greater extent than previously on the role of historical contingency and the specifics of institutional development, with some authors probing the borders of historical sociology (see for example Sabel and Zeitlin 1985, 1997; Thelen 1991; Herrigel 1995; Locke 1995; Ziegler 1997). And work on public policies – particularly that concerned with ‘privatisation’ and ‘liberalisation’ – was concerned with non-state decision-making and new forms of interest representation, such as policy networks and epistemic communities, (e.g. Dyson and Humphries 1986; Vickers and Wright 1988).

Nevertheless, despite the eclecticism of both historical institutionalism and WEP’s approaches in the 1980s and early 1990s, one can nevertheless observe a concern with a coherent range of issues, even if these issues are not united by a single theory or methodology. One important idea – the one selected by Thelen and Steinmo (1992) as perhaps ‘the’ unique characteristic of historical institutionalism – was the ‘endogeneity’ of preferences. This was the insight that institutions may influence the formation of preferences by political actors. It is linked to the foundational idea that institutions for representing interests affect the politicisation of interests, but goes one step further by looking more intensively at interactions between preferences, interests and institutions – a scrutiny that was made more intense by dropping any assumption of objective economic interests. Steinmo (1993), for example, shows how political institutions affected the strategies used by groups to pursue their interests in different polities, and hence changed their interpretations of their preferences. This idea has a parallel in the discussions of changes in West European party systems and the rise of right-wing populism, which also stress the strategic role of parties in redesigning political space (von Beyme 1988).

A second, related, idea was the importance of political actors’ subjective interpretations, not only of their own interests and preferences, but of their identities, beliefs and capacities. For example, whether union members identified as ‘producers’ or ‘workers’ in the nineteenth century, Hattam (1993) argues, determined whether they allied with farmers and craftsmen in a producers’ alliance against banks, or whether they viewed their interests as industrial workers to be contrary to those of property holders. This focus on identities as a key to actors’ perceptual lenses is relevant to contemporary European politics as well, and has been central to research on both social movements and right-wing populism, as a corrective to the somewhat overly
materialist ‘resource mobilisation’ perspective (Knutsen 1990; Finger and Sciarini 1991; Reiter 1993; Fuchs and Rohrschneider 1998).

A third common theme is the idea of ‘contextual causality’. The causal workings of institutions may depend upon their historic contexts, which include the interpretations that political actors bring to institutions. Constitutional choices made in the context of the transition from state socialism to capitalist democracy in Eastern Europe, for example, were very much influenced by interpretations of the past (Elster et al. 1998). Such narratives of the past seem to be crucial as well for the creation of the institutional coordination needed to sustain such complex institutions as ‘industrial districts’ (Sabel and Zeitlin 1985, 1997). Finally, historical institutionalism is often characterised by attention to historical contingency: accidents of history with long-term consequences, such as, for instance, the choice of the administrative structure for unemployment insurance, which it turns out has substantial consequences for levels of union density (Rothstein 1992).

Radical Revision of the Foundational Ideas in the 1990s and Early 2000s

Whereas political analysis of West European politics of the 1980s could take a combination of stability and change as its point of departure, the 1990s were undoubtedly a period of rapid and radical change in Western Europe. With the signing of the Maastricht treaty, European integration took on new speed and intensity – and entered onto WEP’s agenda for the first time, having previously been left (relatively speaking) mainly to the field of international relations (Goetz and Hix 2000). Whereas privatisation, deregulation and welfare state retrenchment had been associated with the Reagan and Thatcher governments in the 1980s, in the 1990s all European political economies and their patterns of consensus politics underwent significant restructuring – including the previously stable German and Swedish ‘models’, (Lane 1991, 2001; Luther and Müller 1992; Bull and Rhodes 1997; Kitschelt and Streeck 2003). Indeed, Social Democracy itself entered into a period of redefinition and repositioning, both in terms of electoral positioning and policy stances, and in terms of party structures and links to unions (see for example Gillespie and Patterson 1993; Merkel 1993, 2006; Kitschelt 1994). Further, new issues, such as immigration, attained new prominence in this era of change and upheaval (Baldwin-Edwards and Schain 1994).

Given this changing landscape, the working definition offered by Thelen and Steinmo (1992: 2) for historical institutionalism as ‘an emphasis on how pre-existing institutions structure contemporary political conflicts and outcomes’ seemed to need revision or amendment. How could one account for both stability and change? This effort took several forms, ranging from specifying more clearly the impact of history on contemporary politics to developing more thoroughly the two frontier issues flagged from the
beginning by Thelen and Steinmo, namely institutional change and the role of ideas. A third frontier issue, the origins of institutions, has also been the basis for important contributions to recent historical institutionalist scholarship, as well as the basis for a rapprochement between historical institutional and rational choice scholarship.

Pinning Down History

Paul Pierson’s recent work (2004) explicitly addresses the role of history in political-institutional analysis. Pierson cautions scholars not to over-emphasise initial institutional choice points and moments of substantial reform to the neglect of long-term processes of institutional development. In Western Europe, many, if not most formal political institutions and important public policies (especially social policies) are mature enough that they are more or less permanent features of the political landscape. Given these constraints, the most important and relevant analytical task is to explain the sources of institutional resilience rather than to search for immediate sources of institutional change, for institutional development may be better understood, Pierson argues, from the vantage point of the *longue durée*. This perspective allows one to examine various feedback links, demographic developments, threshold effects, and the like, which may be more important than short-term political decisions. Thus Pierson has suggested that the role of history can be better specified by three ‘historical’ effects: increasing returns and path dependence; the role of timing and sequence; and attention to long-term processes.

Although models of path dependency were first developed to explain sociological phenomena such as segregated residence patterns, and economic phenomena such as the inefficient spread of technological innovation, Pierson points out the fruitfulness with which path dependency may be applied to political phenomena. The very nature of politics, with its need for collective action, institutional constraints on behaviour, scope for political authority and power asymmetries, and lack of both transparency and mechanisms for restoring efficiency, such as competition and learning, makes the political sphere ripe with opportunities for path dependency. To the extent that a political outcome is the product of path dependency, political analysis must adopt a much longer time frame than that generally assumed by multivariate quantitative approaches or game theoretic models. One must move, as Pierson puts it, from a ‘snapshot’ to a ‘moving picture’ analysis; this is what it means concretely to place ‘politics in time’ and to move beyond vague assertions that ‘history matters’ in order to conduct what Pierson calls ‘genuine’ historical research. In examining sequential effects, such as the relative timing of the introduction of democracy and the emergence of a professional civil service, or the emergence of cleavages in party systems, Pierson argues that more analytic purchase can be had by re-analysing these effects in terms of positive feedback mechanisms, similar to
those so important to path dependency. Similarly, long-term, slow-moving processes and even sharp reversals in the direction of such processes, can be more compellingly explained if one replaces evocation of ‘History’ with more precise analysis of the feedback mechanisms at the micro-level that explain both stagnation and shifts at the macro-level.

One of the strengths of Pierson’s framework is that it goes beyond the claim that ‘history matters’ and gives us the tools to analyse the ways in which temporal context matters – that events unfold differently in different historical contexts, the order in which events occur matters very much (think of democratisation in Eastern Europe compared to countries that democratised at other points in time), and increasing returns over time help to explain why institutional change is often incremental. Pierson’s insights provide the starting point for much research published in *WEP*, especially his path-breaking work on the ‘new politics of the welfare state’ (Pierson 1994, 2001; Ferrera and Rhodes 2000). Nevertheless, despite the inclusion of feedback effects that can induce changes, such as threshold effects, the emphasis on feedback effects results in far more sustained focus on institutional resilience than on institutional change. Further, although mapping feedback more precisely may be analytically rigorous, in practice, it is extremely difficult to measure feedback effects in politics, particularly if one is using qualitative methods. How would one apply these ideas using a case study research design? Are the methods and measures traditional for West European politics up to this task? Indeed, in order to make progress on this agenda, it would seem to be most feasible to return to the variable approach rejected by Pierson at the outset, and indeed to focus on the political variables that lend themselves easily to precise measurement: electoral results, parliamentary representation, and legislative behaviour. Yet these standard features of political life tend to be downplayed by this take on historical institutionalism, despite the inclusion of chapters in Pierson’s book on institutional change and institutional design. Even more disturbingly, the focus on pinning down history has resulted in the neglect of two basic features of both politics and history: political contestation and actor reflexivity. For it is precisely in the moments when individuals find the courage to contest the status quo that we witness historically relevant change in political behaviour and political structures. Thus, the search for social scientifically relevant regularities should not lead one away from the very stuff of politics: conflict, contestation and power.

**Analysing Institutional Change**

Recent work by Thelen (2004), Streeck and Thelen (2005) takes up Pierson’s (2004) call for a nuanced view of institutional change by investigating the kinds of long-term processes of institutional development that Pierson emphasises, but places much more emphasis on political processes in accounting for both stability and change. The key point here is that what
Thelen (1999) calls ‘mechanisms of reproduction’ are central elements in any account of institutional development. All institutions – and here Streeck and Thelen (2005) focus in their edited book Beyond Continuity on sets of institutions that are backed by political legitimacy and legal sanctions, and can thus be thought of as institutional regimes – are continually renegotiated and reinterpreted, for none can provide complete and unambiguous guides to action. Moreover, as circumstances change, this room for ‘play’ in institutional enforcement is necessary for the successful adaptation of such institutional regimes. Thus, the view of institutions provided by Streeck and Thelen can be termed ‘Artistotelian’: not theoretical models of institutions, but existing historical formations must be the starting point for institutional analysis. This historical residue includes the ideas and practices people bring to the institution as it has developed over time. Thus, like Schumpeter (1975 [1942]) and Polanyi (2001 [1944]), Streeck and Thelen’s claim is that without continual renegotiation and reinterpretation, as well as the support of ancillary institutions, such as customs, beliefs and assumptions, institutions would lose their social embeddedness, and hence cease to function at all. In stark contrast to the more ‘Platonic’ varieties of capitalism (Hall and Soskice 2001) approach, this view on institutions does not assume coherent institutional complementarities which result in continual return to equilibrium. Instead, institutional regimes arise from historical coincidences in combination with the efforts of human agents to solve changing problems – much as in the perspective of Sabel and Zeitlin (1997), which so stresses ‘accidents in the struggle for power’.

Streeck and Thelen (2005) construct a typology of institutional change that encompasses different types of mechanisms through which change occurs. Their central claim is that much transformative institutional change takes place gradually, so we need to analyse the ways in which these processes unfold. This framework is a major step forward for historical institutionalism because it provides the conceptual tools for analysing and categorising different types of institutional change. One of the great virtues of Beyond Continuity is that agency plays a much greater role than in earlier research that largely attributed incremental institutional change to a path dependent process in which political actors seemed almost to be onlookers in a self-reinforcing process; now institutional change is much more directly related to politics. Nevertheless, perhaps precisely for this reason, the book stops short of offering a causal theory for the various types of institutional change. In other words, Streeck and Thelen have done an enormous service by elaborating a typology for distinguishing and understanding different types of institutional change, but they tell us more about how to categorise outcomes and to understand the logics associated with them than how to explain path-breaking institutional change. In fact, it is not their ambition to produce such a theory. As such, the book is typical of the challenges associated with historical institutional research: it is exceedingly difficult (if not impossible) to develop theoretical frameworks powerful enough to
explain more than a few cases, if one is serious about paying sufficient attention to the particularities of historical context, the role played by political contestation, and the intervening impact of human perception and reflection.

Despite these challenges, several recent works represent important advancements in this area. Lynch’s (2006) research on the ‘age orientation of welfare states’ combines qualitative case studies with quantitative analysis to understand why some welfare states privilege programmes for the aged at the expense of measures to help families and younger workers. She shows that the dynamics of party competition within different types of political systems (programmatic versus patronage) lock in decisions taken in the late nineteenth and early twentieth centuries, precluding welfare state modernisation and adaptation to new needs in ‘occupational welfare states’. What makes these processes path dependent is the style of political contestation, and the key mechanism of path dependence is the mechanism that links politicians’ competitive strategies to institutional structures. So path dependence is a decidedly political process. Morgan’s (2006) book on family policy shows how church–state relationships and political conflicts over religion affected both ideologies of gender roles and the incorporation of religiosity in political life, thus shaping the politics of family policy at several critical junctures. Political contestation sets policies on a specific ‘path’ but the steps on the path are continuously renegotiated in an ongoing political process.

Both of these books put political contestation back into the centre of institutional analysis, thus helping the authors better to account for both stability and change. Further, each is sensitive to the role played by political beliefs in political conflict and institutional innovation, and, hence, provides a more ‘historical’ account of welfare state development, in the sense of respecting actor-reflexivity.

The Role of Ideas

Indeed, despite the historicist and idiographic stance of historical institutionalist scholarship, one must fault this approach for its general neglect of the non-material sources of actor preferences. Actor preferences originate in the ways in which policy feedbacks such as lock-in and policy drift shape the organisation of interest groups and their definition of interest; there is little scope for the impact of cognitive framing and ideational processes. This focus on the material sources of preferences was never the exclusive focus of historical institutionalism, however, so the recent ‘ideational turn’ in historical institutionalism is in some ways a return to origins. For example, both Hall (1993) and Rothstein (1998) incorporate the importance of ideas and cognitive understandings into their analyses, and it is worthwhile returning to these ideational/cognitive foundations.
Hall’s (1993) conception of ‘policy paradigms’ incorporates cognitive understandings into his explanation of preference formation. According to Hall (1993: 279) a policy paradigm is ‘a framework of ideas and standards that specifies not only the goals of a policy and kind of instruments that can be used to attain them, but also the very nature of the problems they are meant to be addressing’. Policy paradigms are the world views of bureaucrats, politicians, and other key political actors about the nature of policy problems and the range of potential and appropriate solutions. These paradigms constitute roadmaps that provide actors with cognitive tools and directions about how to interpret how key macro-institutions (like the economy) function. These cognitive understandings, in turn, shape the range of policy alternatives that come into focus.

Rothstein extends these studies’ focus on the ideas and cognitive understandings held by political elites within political parties, unions and public bureaucracies by focusing on the normative dimension of policy feedback. Rothstein (1998) argues that institutions, particularly welfare state institutions, influence the development of social norms; social policies have consequences for how citizens view notions of fairness, solidarity, equality, and trust. Universal welfare state institutions generate strong norms of social trust because no one is cheating (getting more than their ‘fair’ share) and everyone who can, works (and pays taxes). Thus institutional choices shape the emergence of both a ‘political’ and ‘moral’ logic. The ‘political logic’ is similar to Pierson’s (1994, 2001, 2004) conception of path dependence: public policies generate incentives for interest group activity, influence adaptive expectations and generate distinctive patterns of public support. Office-seeking politicians are aware of the electoral constraints that path dependence produces and pursue strategies designed to avoid blame for unpopular policies. The ‘moral logic’ of path dependence is closely bound up with the ‘political logic’ but is decidedly normative. This moral logic suggests why people support institutions when their self-interest would dictate otherwise.

More recent work extends and enlarges this focus on ideas and cognitive processes as sources of actor preferences, but their link to historical institutionalism’s original focus, that is the interplay of temporal and institutional factors to explain political processes and outcomes, is weak. Two examples of influential works will suffice to illustrate this. First, Berman (1998) explains divergence in the Swedish and German Social Democratic parties’ responses to democratisation and economic crisis in the inter-war period with reference to differences in the parties’ ideas. As Berman argues, different ideas produce different policy responses even when the nature of political and economic pressures is similar (ibid.: ix). Divergent responses are explained by ‘each party’s long-held ideas and the distinct policy legacies those ideas helped create’ (ibid.: 4). The German and Swedish Social Democratic Parties each developed their own nationally distinct definitions of what constituted social democracy. The basic line of argument
is that ideas shape political behaviour in ways similar to what Weir (1992), Hattam (1993) and Hall (1986, 1993) have argued (although ideas have more causal weight), but the institutional focus is missing.

Like Berman, Blyth (2002) emphasises the causal impact of ideas on policy choice, specifically Swedish economic policy-making. Blyth conceptualises ideas as roadmaps, weapons in political struggles, and cognitive ‘locks’, stressing the ways in which ‘ideational contestation’ shapes institutional development. Despite his historical focus, Blyth’s account of ideational impact rests on exogenously generated ideas. The neo-liberal ideas that came to dominate thinking on economic policy did not originate in Sweden; the ideas were transposed to the Swedish context by influential ‘carriers’.

These two recent studies share with earlier historical institutionalist research a focus on the impact of ideas and cognitive understandings, or what Berman calls ‘programmatic beliefs’, on policy choices. Berman’s study has more affinities with historical institutionalism’s focus on temporality than Blyth’s because her focus is on ideational/cognitive legacies. The programmatic beliefs of social democratic parties produced policy legacies in much the same way that Pierson (1994) conceives them. Unlike Pierson’s more material focus, however, Berman’s policy legacies are ideational and cognitive (see also Cox 2001; Schmidt forthcoming). Newer work by younger scholars departs from earlier efforts to understand the impact of ideas by examining the available discourse space for new ideas (Naumann 2005), and ways in which framing dynamics construct corridors for action in policy-making (Daviter 2007).

The Origins of Institutions

Not only ideas, but standard political institutions are now becoming more central both to historical institutionalist work and to studies of West European politics. This represents the influence of the rational choice approach’s emphasis on the impact of institutions viewed as formal rules. Thus, the impact of electoral systems, party regulations, and the formal division of powers is increasingly being brought into the study of political parties and public policy-making (Rogowski 1987; Cusack et al. 2007; Scarrow 2006). Further, as discussed above, the feedback effects of political institutions may be an important aspect of historical legacies (Lynch 2006; Morgan 2006). At the same time, however, the recognition of the importance of institutions has prompted scholars to investigate the origins of institutions. And here we see that the process of choosing institutions has generally been far from far-sightedly rational, but instead resembles more the models of bounded rationality and ‘non-rationality’ proposed by cognitive psychologists and ‘sociological’ institutionalism (Powell and DiMaggio 1991; Gigerenzer 2001).

Further, the effects of constitutional structures depend upon the political content with which they are filled: this depends upon partisan preferences,
the ways in which electoral formulas translate votes into seats, the strategies of political parties and on political culture, as well as the beliefs of leaders. Skach (2005), for example, shows how the semi-presidential constitutional designs adopted in the French Fifth Republic and Weimar Germany resulted in very different democratic outcomes because of the ways in which the electoral and party systems, as well as the actions and beliefs of political leaders, worked to aggregate or fragment political majorities. Ziblatt’s (2006) combination of detailed comparison of the politics of German and Italian unification nested within a broader sample of 18 cases belies rational accounts of institutional design, and instead indicates that pre-existing structures and expectations shaped the politics of state formation. Thus, a look at the actual history of the origins of institutions uncovers a messy politics of muddling through, in which assumed structures and ideas play an important role in institutional outcomes.

Taking Stock in the 2000s

What are the achievements of historical institutionalism, the challenges that remain, and what do these suggest about the future of research published in WEP? First, historical institutionalism’s greatest achievement is probably its contribution to our understanding of how interests are constructed. A central analytical concern has always been to investigate the sources of actor interests, and the cumulative achievements of historical institutionalism in this area are considerable. We now know quite a bit about how institutions constitute actors and their interests, as well as the ways in which cognitive and ideational factors shape interests. Second, historical institutionalist scholarship has achieved much in terms of conceptualising and explaining actor behaviour. One of historical institutionalism’s core insights is that the ‘rules of the game’ matter. This analytical precision concerning the workings of institutions has generated powerful insights into how the institutional context structures not just interests but behaviour. Third, historical institutionalism constitutes a powerful, historically based analytical framework for investigating institutional genesis and change. Eschewing the ‘snapshot’ view of politics prevalent in much political science research, historical institutionalism emphasises the ways in which institutions are the product of political contestation and compromise, how they are renegotiated and reformed over time, and how institutions take on forms, functions, and meanings that their creators never intended. Finally, historical institutionalism provides powerful insights into policy-making. WEP has published many articles that analyse the politics of reform in distinct policy areas, and much of this research draws heavily on historical institutionalist insights.

These considerable achievements should not obscure the challenges that remain. First, we currently live in an unsettled period, and our theoretical tools for understanding the causes and consequences of this fluidity are
inadequate. Given historical institutionalism’s core claim that institutions mediate behaviour, the current political context is problematic because it is so fluid. One obvious sign of this instability is the breakdown of corporatism. Across Western Europe, union membership has declined, new social movements have emerged, and new political issues have reached the political agenda. This has occurred in the context of greater voter volatility, the emergence of new political parties, and the breakdown of once-stable institutions for interest intermediation. These developments challenge the viability of corporatism at the same time that they call into question the theoretical frameworks that dominated research published in WEP. Despite the widespread recognition that corporatism is waning (‘new social pacts’ notwithstanding), there is no successor for corporatist theory in sight. It is worthwhile considering whether theoretical insights drawn from regions that are less stable than Western Europe may provide a starting point for reconceptualising interest intermediation in a post-corporatist Europe. If post-war political stability was indeed an anomaly and West European politics is becoming more like politics in other regions marked by more cleavages and higher volatility, i.e. Eastern Europe or Africa, it is worth examining whether models based on these political conditions offer insights for the study of Western Europe.

Second is the challenge of building middle-range theory. One of the strengths of WEP is that it provides an outlet for qualitative research aspiring to particular or middle-range theoretical explanations. Single or comparative case studies of particular policy areas or types of political behaviour provide much of the substance in WEP, much of it informed by historical institutionalism. But even after 30 years it is fair to say that progress in theory-building has been disappointing. Even the most sophisticated attempts to combine analytical rigour with historical institutionalist analysis are prone to some of the same problems prevalent in the 1970s when WEP was founded: theoretical concepts that are difficult to apply consistently to empirical cases, contradictory hypotheses generated by the same theory, theoretical over-determination, the biases of case choice, the problem of measuring both independent and dependent variables, and the like. Whereas the ‘variable’ approach so criticised by historical institutionalists has its shortcomings, the ‘historical’ approach needs to find new ways of addressing theory-building without sacrificing the gains that have been made by increased sensitivity to context and perception. Some steps in this direction have been the increasingly prevalent efforts to combine quantitative and qualitative methods, as well as innovations such as the ‘Ragin method’ (Ragin 1987). Nevertheless, much theoretical, conceptual and methodological work remains.

The third challenge concerns the regional identification of WEP. Is there still today a need to focus on Western Europe? Or is this simply a remnant of Western Europe’s rapidly declining economic and political dominance? Analysis of the particular political-institutional development of Western
Europe may perhaps regain greater contemporary relevance by more emphasis on cross-regional comparison. In a way, this is somewhat of a return to the early focus on states and capitalism, and the foundational period’s focus on broad comparative-historical analysis on an international scale as carried out by Barrington Moore (1966) Reinhardt Bendix (1969), Shmuel N. Eisenstadt (2003), and other historically oriented comparativists. At the same time, some of the focus on general theory and quantitative methods of the post-war political development and modernisation schools might be reasonably brought to bear on problems of comparative analysis, without, however, ignoring the lessons of conflict theory and multiple routes of modernisation, as has been increasingly the case, for example on the topic of democratisation. Here, the work of Samuel A. Huntington (1966, 1968, 1971, 1991) may serve as an outstanding example of an effort to synthesise acknowledgment of historical particularity and a search for general causal relationships.

What do these challenges suggest for the future of WEP? Even as we attempt cross-regional comparisons and middle range theory-building, the tension between an ‘Aristotelian’ focus on historical and cultural context, and in which there is little or no regular logic apart from historical forces, versus a ‘Platonic’ search for logical, empirical regularities is likely to remain. We cannot solve this basic dilemma of the social sciences, but we can point to three different strategies for grappling with this inherent tension. The first entails a basic acceptance of the limits of social science explanation, and an embrace of ‘plain old history’. There may be many things that we cannot explain, but that does not mean that it is not worth trying to find out what happened. We may not be able to identify all causally important variables, or even to be able to demonstrate a particular causal link or causal model, but we can find out what political actors intended as they framed legislation, joined social movements, or started a war. Indeed, political actors constantly document what they are doing, and there are innumerable sets of historical documents that provide rich detail that can be mined to test competing hypotheses to explain political phenomena. As Lijphart (1971), Hall (2003) and Rueschemeyer (2003) have argued, when historical investigation is embedded in a set of competing hypotheses, the results of this kind of work can be theoretically valuable, even if it does not necessarily provide a universal or general explanation. Thus, accepting the limits of explanation does not have to mean abandoning all ambition of providing an explanation: the key parameter is the research design.

And this brings us to the second strategy: improving qualitative inquiry. While discussions of the philosophy and methods of social science are far from new, one can point to an increasing trend in efforts to address the nuts-and-bolts problems of research on politics, particularly at the dissertation level (King et al. 1994; Van Evera 1997; Geddes 2003; George and Bennett 2005; Parsons 2007). These efforts are all valuable, but some may have perhaps overshot their mark. King et al., for example, pay a great deal of
attention to problems that arise in statistical analysis and extend these to qualitative studies. Yet they neglect the side of positive prescription: how do we compensate for these problems in a small-\(n\) study? How many paired comparisons are necessary? Should one focus on outliers? Or more directly on the cases that best fit a proposed explanation, in order to process-trace the causal links? What is needed is more focused guidance on paired case study design in comparative politics (as opposed to international relations), and on combining information on a complete universe of cases with more sustained focus on a smaller set of cases. Furthermore, for all the focus on research design and methodology, in an area where data collection is imprecise, and dependent upon the talents and instincts of the investigator, we may gain more mileage in better coverage of the basics of the historical method (again, ‘plain old history’), and in the guidelines of common sense. After all, there are many classics that have been based on biased case selection but yet remain read because they convince the sceptical reader despite their methodological weaknesses. And there are other studies that, while perfectly executed, do not address a substantive issue of compelling importance, and hence fail to convince.

And this brings us to the final point: the need for substance in the analysis of West European politics in international comparison. Theoretical and methodological quandaries aside, the most important factor in keeping WEP alive and vibrant will be its ability to continue to focus on issues of substantive importance to West European politics. Recent research on varieties of capitalism, constitutionalism and democracy, political integration and cultural diversity shows a return to substantive issues like ‘state and capitalism’ that animated so much of WEP’s early research. What direction is capitalism developing in? What is the role of the state in this process? Are there just and unjust forms of capitalism? How does political development affect the workings of democracy? What are its social, political, cultural and economic underpinnings? This kind of substantive controversy is precisely the kind of niche that WEP has filled in the past and should continue to address in the future. At the same time, however, this conceptual and theoretical work is impossible without the kind of fine-grained qualitative research that WEP publishes. Thus we insist on the centrality of the historical method, case studies, and process tracing. The historical record is as close as we get to observing political behaviour directly, that is ‘politics in action’. Without meaning to diminish the value of alternative approaches, we only want to emphasise the unique potential for historical research in addressing important research questions.

Note

1. The five types of incremental, potentially transformative institutional change are: layering, conversion, displacement, drift and exhaustion.
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