# 7 Historical institutionalism

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Historical institutionalism is neither a particular theory nor a specific method. It is best understood as an *approach* to studying politics and social change. This approach is distinguished from other social science approaches by its attention to real-world empirical questions, its historical orientation and its attention to the ways in which institutions structure and shape behaviour and outcomes. Although the term 'historical institutionalism' was not coined until the early 1990s,[1] the approach is far from new. Many of the most interesting and important studies of politics – from Karl Polanyi's classic *Great Transformations*, to Theda Skocpol's *States and Social Revolutions* and Philippe Schmitter's *Still a Century of Corporatism?* – would clearly be categorized as historical institutionalist were they written today.[2]

The best way to explain historical institutionalism (HI) is to situate this approach in a historical and comparative context, showing where the approach originated and how it is different from other approaches in the social sciences. In short, what follows is an HI account of historical institutionalism. The chapter concludes with a discussion of the implications of this approach for our understanding of political and social science as 'science'.

## Origins

Institutional theory is as old as the study of politics. Plato and Aristotle to Locke, Hobbes and James Madison long ago understood the importance of political institutions for structuring political behaviour. Plato's *Republic* is a comparison of different forms of government in which he tries to understand how institutions shape political behaviour. Aristotle's *Politics* continues the

This chapter grew out of a series of conversations with Ellen Immergut and Bo Rothstein. Their contributions are found throughout this text, although all errors remain mine. I would also like to thank John Campbell, Carl Dahlstrom, Peter Mair, Mark Thatcher and Kathleen Thelen for insightful and very helpful comments on an early draft.

study of political institutions: he specifically examined institutional structures because he believed they shaped political incentives and normative values. Although rarely credited as the political theorists they clearly were, the founders of the American republic were interested in precisely the same sets of questions. Madison's 'science of politics' is a study of how different institutional arrangements will encourage and/or discourage different types of political action.

As the social sciences started to emerge as a modern academic discipline in the late nineteenth and early twentieth century, these classical traditions had a great impact (Almond 1996). Both in Europe and in the United States, students of politics were specifically concerned with the relationship between constitutional design and political (and even moral) behaviour. Indeed, much of what could be called early political science was about how to design perfect constitutions. This was an era of massive political and social upheaval when scholars were sometimes even invited to design institutions that could help build better societies. Perhaps the most famous case (and worst disaster) was Weimar Germany. After the defeat of the Kaiser, constitutional architects attempted to design what they believed to be the world's most perfect democracy. This historic occasion provided a nearly unique opportunity to apply 'political science' to the real world. The new German Republic, it was firmly believed, would be a model democracy that others would soon emulate. Unfortunately, things did not quite work out that way.

The failure of Weimar democracy led to increased disaffection with institutional analysis. This disaffection grew to scepticism – if not hostility – in the post-war years. While prior to the war one could imagine that democracy could be built with proper institutions, as we moved past the middle of the century such an argument became impossible to sustain. As the great European empires broke down, they often attempted to leave behind what they thought were the best practices and institutions in their former colonies. Sadly, however, finely designed democratic institutions fell to dictatorship, autocracy and even chaos, throughout the developing world. No matter what kinds of institutions were constructed, virtually all failed to produce the kinds of political behaviour necessary for democratic society to function.[3]

Increasingly, social scientists came to believe that institutions were mostly the vessels in which politics took place; what mattered was what filled the vessels. Given this understanding, both political science and sociology departments moved in two distinct directions. On the one hand, many believed that to be scientific, social science needed to be more theoretical. At the same time, others held that the study of politics and society should be broken down into

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constituent variables that could be measured, examined and analysed independently. In the process, institutions mostly fell out of the analysis.

It is important to remember that social science was growing within a broader political and historical context. In the post-World War II years, the physical sciences were advancing rapidly and there was no small amount of 'physics envy' in the social sciences.[4] To be taken seriously, it was sometimes thought that social science needed to be a 'real' science. Many believed that *real* science must follow *the* scientific method. If social science was to be a science, these reformers argued, it, too, must build predictive theories that are falsifiable and testable. Mark Blyth quotes Karl Lowenstein, who wrote in the *American Social Science Review* in 1944 that 'to overcome past errors comparative politics would have to become "a conscious instrument of social engineering" because the discipline ha[d] a mission to fulfil in imparting our experience to other nations . . . integrating scientifically their institutions into a universal pattern of government' (cited in Blyth 2006: 493).

After all, the problems of poverty, inequality, injustice, war and underdevelopment are just as important as anything studied by 'real' scientists. What scientists do, in this naïve view, is analyse their part of the physical world, produce hypotheses about how certain features work, and test these hypotheses with repeated experiments. In this account of 'real' science, scientists follow a methodology in which they dissect a complex phenomenon into its constituent parts and analyse these parts separately and independently. The goal is to analyse and understand the most basic units and processes and discover the laws that govern them. The fundamental Cartesian principle is that the world – and everything in it – is governed by basic laws. If we can understand these laws, we can understand and ultimately control the world we live in. This paradigm of science led from Newton's first observation of a falling apple, to more basic understandings of gravitational force, to a more general understanding of how and why the earth circles the sun, and eventually to the ability to send ships into outer space and to walk on the moon.

In its attempt to be more scientific (particularly in America, with the lure of funding from institutions such as the National Science Foundation), the cutting edge of social science moved away from historical analysis and 'thick description'. First, there was significant pressure to be more rigorous and quantitative. In the eyes of many, too much of the previous work had simply been historical and descriptive. History could be interesting, but it did not lend itself to easily testable and falsifiable propositions. It was not science.[5] Social science, the 'behaviouralist' thought, needed to move away from the particulars and treat cases as sets of values on variables. It was also important

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that social science restrict itself to factors that could be measured, counted and then compared and analysed. This meant that we should study *behaviours* that are measurable (such as social or economic position, attitudes or votes) and not *institutions* – which, almost by definition, are unique. Certainly, the behaviouralist agreed, social science was an infant science. The models were crude, the methods rough and the data pathetically incomplete; but all this was once true of physics and chemistry as well.

Surely, the new political scientist argued, the human world is governed by laws of behaviour and action – just as the physical world. If so, then the job of the social scientist is to discover these basic laws so that we, too, can predict, ultimately manage and even positively shape the world in which we live. Questions like 'Why do some countries or people benefit from high levels of democracy, growth and development while others are mired in vicious cycles of poverty, dictatorship and violence?' are big and complex. But if we deconstruct the processes and mechanisms of politics, just as chemists deconstruct the complex phenomena underlying disease, one day we may be able to build a better world – they thought.

The behaviouralists thus saw their role in the scientific process much as the chemist might. In order to understand the larger world around us, we first must break that world down into its constituent parts and try to understand those parts independently of each other. One day, they seemed to believe, we might have a 'Periodic Table of Politics'.

On the other hand, for the grand theorists – whether Marxist, structural functionalist, systems theorist, modernization theorist, or rational choice theorist – the key issue was to understand the basic processes and mechanisms motivating politics across nations, cultures and history. All countries throughout history faced the perennial, basic problems (Parsons and Smelser 1956); the scientist should focus on these great forces, not on the details and institutions. Institutions were either functional solutions to social problems or simple arenas where political battles took place. In either case, the specific construction of the arenas as such were not considered an important variable for determining the battles' outcomes (March and Olsen 1989; Steinmo, Thelen and Longstreth 1992)

It is useful to think of these grand theorists as the 'physicists' of politics. Their main goal was not practical; instead, their ambitions were grander as they focused their efforts on social science's search for the Holy Grail: The Laws of Politics.[6] For example, Adam Przeworski and Henry Teune wrote in their influential *Logic of Comparative Social Inquiry*, 'The pivotal assumption of this analysis is that social science research . . . should and can lead to general statements about social phenomena. This assumption implies that human behaviour

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can be explained in terms of general laws established by observation' (Przeworski and Teune 1970: 4). Whereas the behaviouralists sought out a Periodic Table of Politics, the grand theorists searched for a 'Theory of Everything'.

## Studying the real world

Thus, by the 1960s and 1970s, social science's cutting edges had moved in quite distinct directions: the largely atheoretical micro-analyses of political behaviour on the one hand; and the macro- (and remarkably non-empirical) theorizing of Marxism, functionalism, systems theory and rational choice on the other. Although the work of the grand theorists and their behaviouralist brethren often did not intersect, a political alliance developed in many social science departments. These developments were most obvious in public universities in the United States in which significant shares of institutional funding came from scientific granting organizations.[7] Because the levels of government funding for scientific research in universities were significantly lower in most European countries, there was less pressure to adopt hard-science norms and practices to help fund social science programmes. Mobility between countries and even between universities within particular countries was also far more limited in Europe than in the USA. Consequently, new notions of science were adopted more slowly, as established professors in politics and sociology had fewer incentives to model themselves on the hard sciences.

Many political scientists, however, continued to be interested in studying politics and history. Indeed, it is sometimes said that historical institutionalism harkens back to a kind of social science that dominated over fifty years ago. From some quarters this is meant as an insult (HI is simply out of date); for others it suggests the recognition that many of the classics in political science and sociology were engaged in a kind of scientific inquiry that historical institutionalists would find familiar today. Max Weber, Stein Rokkan, David Truman, Karl Polanyi, Alexander Gershenkron, E. E. Schattschneider or Hugh Heclo would be identified as HI scholars if they were writing today, for they were specifically interested in explaining real-world outcomes, using history as an analytic tool, *and* they were strongly interested in the ways in which institutions shaped political outcomes.

Without necessarily denying the goal of social science *qua* science, many continued to be interested in the *meso*-level analysis and middle-range theory (see Mair, ch. 10). Disappointed with grand theory and bored or simply uninterested in the technical approach of behaviouralism, many political scien-

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tists continued to be interested in real-world *outcomes*. It was here that historical institutionalism was born. Political scientists, some believe, should actually try to explain important real-world events. When they began to ask questions like 'Why do real-world outcomes vary in the ways that they do?', institutions kept popping into their analyses. Most famously, Theda Skocpol wanted to explain the sources and patterns of the great revolutions (Skocpol 1979). But rather than *assume* that class structure or elite power would explain different patterns, she did the hard work of examining actual revolutions and placing them in their comparative and historical contexts. Eventually, Skocpol realized that the structure of state institutions in the pre-revolutionary period had enormous consequences for revolutionary outcomes. In hindsight, this may seem obvious, but at the time it was a revelation to many (especially American) social scientists that the state mattered.[8]

Skocpol was far from the only social scientist interested in explaining important real-world events, but there can be little doubt that her ideas had an important influence on the generation of young scholars who came after her. In the late 1970s and early 1980s there was a concomitant move in comparative politics research, in which students began to compare real-world cases rather than 'variables' (della Porta, ch. 11, and Vennesson, ch. 12). Once again, it may seem strange from today's vantage, but at the time, comparative politics was largely made up of detailed studies of particular countries, unions, movements or political parties. Anyone who studied a country other than his or her own apparently was a comparativist.

One of the most important volumes in this regard was Peter Katzenstein's (1978) *Between Power and Plenty*. This work also came out of a project in which a group of individual scholars were asked to analyse how and why different countries responded to the economic dislocations and hardships created by the oil price shocks of the early 1970s. This was a remarkable book precisely because it offered such careful and focused comparisons (by country experts). Once again, the structure of state institutions quickly came into the analyses of almost all of these scholars.

## Historical *Institutionalism*

Not all political scientists or sociologists who use historical methods and who engage in case studies are *institutionalists*. Institutionalists are scholars who place special emphasis on the role institutions play in structuring behaviour. What are institutions? The most common definition for institutions is: rules.

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Some students in this tradition focus on formal rules and organizations (Streeck and Thelen 2005), while others address informal rules and norms (Hall 1989; Marcussen 2000). Whether we mean formal institutions or informal rules and norms, they are important for politics because they shape who participates in a given decision and, simultaneously, their strategic behaviour.

Some examples are illustrative. Ellen Immergut's analysis of the politics of health care policy asked a straightforward question. Why do some countries develop comprehensive national health care systems while others have decentralized and fragmented insurance programmes? After analysing the political histories of several European countries, she observed that the structure of each country's political institutions offered different interest groups veto points[9] which had to be negotiated around. Looking more deeply into the specific cases, she came to see that the institutions not only provided obstacles to particular policy choices, but also ultimately structured the menu of choices available in different regimes (Immergut 1992). These different outcomes were *not* the products of different basic goals or aims put forth by particular parties or interest groups – but interest groups and parties did have to pursue different political strategies in different countries owing to the different political/institutional configurations established by the individual constitutions. In other words, she found that she could not explain the variation in policy outcomes without explaining the ways in which national political institutions structured both *who* participated in health insurance policies and the '*rules of the game*' in which they participated. The rules (especially differential access and availability of veto points) enabled different political strategies in different countries and ultimately shaped the different policy outcomes.

Similarly, Steinmo was interested in understanding why some countries have much larger welfare states than others do. His initial hypothesis was that political culture and/or public preferences would explain the major differences. But as he looked closer at the actual development of modern welfare states, he found that variation in attitudes could not explain how and why countries developed such wide variance. The evidence showed that citizens liked public spending; citizens in all countries wanted (and continue to want) *increases* in public spending on all of the most important and expensive arenas of public effort. The biggest constraint on these broad and common preferences, it seemed, was financial. Thus, he chose to examine the developments of national revenue systems. If the desire for public spending is constant, perhaps the fear of or resistance to taxes varied. Once again, attitudes and even political culture seemed of little analytic value. Neither of these variables made sense of the fact that countries like Sweden taxed the poor and

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working class much more heavily than the wealthy and the capitalist class. Even more curiously, the United States turned out to have a *more progressive* tax system than even Sweden. Detailed historical analysis of several cases brought this author to the conclusion that the very different political institutions through which public and elite preferences were translated into policy had enormous effects on the structure of actual tax policy outcomes. It was the structure of Swedish corporatist decision-making institutions – versus America's fragmented pluralist institutions – that best explained why specific tax policy choices were taken over time. These specific choices added up to hugely different revenue systems and consequentially different abilities to fund popular programmes like health care, education and labour market policies (Steinmo 1993).

We could continue with many other similar examples.[10] For example, in an effort to understand why some countries have higher levels of unionization than others, Bo Rothstein found that the particular structure of national unemployment insurance institutions was a hugely important mobilizing and organizing tool for unions in some countries but not in others. Countries employing the Ghent unemployment insurance system had far larger union movements than countries that did not (Rothstein 1992). Victoria Hattam wanted to explain the weakness of the labour movement in America and found that the structure of American parties and electoral institutions provided disincentives for union organizers to take a political strategy. Thus this important feature of American Exceptionalism was not a product of America's unique political culture, but instead a product of her uniquely fragmented political institutions (Hattam 1993).

It should be clear that three things distinguish these analyses so far. First, the scholars were not motivated by the desire to press an argument or push a methodology. Second, they were motivated by the desire to answer real-world empirical questions. Finally, they found *through empirical investigation* that institutional structures had profound effects on shaping political strategies, outcomes and, ultimately, political preferences.

## Three institutionalisms

There are at least three types of institutional analysis in the social sciences today: rational choice, sociological institutionalism and historical institutionalism.[11] I will not attempt to rehash the debates among these forms other than to identify what I think is the key difference between historical institutionalists

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and the rest. First, there is considerable *agreement* among institutionalists in that they all see institutions as rules that structure behaviour. Where they differ is over their understanding of the nature of the beings whose actions or behaviour is being structured. The rational choice school argues that human beings are rational individualists who calculate the costs and benefits in the choices they face. Rational choice institutionalists think institutions are important quite simply because they frame the individual's strategic behaviour. They believe that people follow rules because humans are strategic actors who want to maximize their personal or individual gain.[12] We co-operate because we get more with co-operation than without it. We follow rules because we individually do better when we do so.

Sociological institutionalists, in contrast, see human beings as fundamentally social beings. In this view, humans are neither as self-interested nor as 'rational' as rational choice scholarship would have it (March and Olsen 1989), but are 'satisficers' who act habitually. For sociologists, institutions frame the very way in which people see their world and are not just rules within which they try to work. Rather than following rules to maximize their self-interest, humans are thought by sociological institutionalists generally to follow a 'logic of appropriateness' – meaning that rather than asking themselves 'What do I get out of X?', people first ask themselves 'What *should* I do? What is appropriate?' In this view, the important institutions (rules) are social norms that govern everyday life and social interaction.[13]

Historical institutionalists stand between these two views: human beings are *both* norm-abiding rule followers *and* self-interested rational actors. How one behaves depends on the individual, on the context and on the rules. While this statement may seem rather obvious, it has huge implications for how we should study politics. If all three of these variables (individuals, context and rules) are important in choice situations, then there can be no *a priori* way of knowing what one should study when trying to explain political outcomes. A historical institutionalist *does not believe* that humans are simple rule followers *or* that they are simply strategic actors who use rules to maximize their interests. A historical institutionalist can even be rather agnostic to these issues. What the HI scholar wants to know is why a certain choice was made and/or why a certain outcome occurred. Most likely, any significant political outcome is best understood as a product of both rule following and interest maximizing. How do you know which is the more important (self-interested, altruistic/collective or simply habitual) behaviour? The historical institutionalist would go to the historical record (also known as evidence) and try to find out.

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## Taking *history* seriously

These insights have important implications, both for what we study and for how we study it. Historical institutionalists study history because they believe it matters, not merely to increase the reference points for analysis (as is done in time-series analysis). There are at least three important ways in which history matters. First, political events happen within a historical context, which has a direct consequence for the decisions or events. An early example of this is the seminal work of Alexander Gershenkron, who argued that *when* a country industrializes necessarily affects *how* it industrializes. He shows us why late-comers cannot go through the same long trial-and-error process followed by early developers.[14] In other words, the process of industrialization is essentially different for late developers than for early developers. This is a huge insight that is easily missed in large-scale quantitative, cross-national comparisons, which very often pool data across continents and time periods and treat the time/place as inconsequential (or assume that it will 'wash out' of the analysis).

The second reason history matters is that actors or agents can learn from experience. Historical institutionalists understand that behaviour, attitudes and strategic choices take place inside particular social, political, economic and even cultural contexts. Rather than treating all political action as if fundamentally the same irrespective of time, place or context, historical institutionalists explicitly and intentionally attempt to situate their variables in the appropriate context. Thus, by deepening and enriching their understanding of the historical moment and the actors within it, they are able to offer more accurate explanations for the specific events that they explore than had they treated their variables outside the temporal dimension.

E. E. Schattschneider's early work on tariff policy showed how political choices made at time A have important consequences for time B. In this work he famously argued that 'new policies create new politics'.[15] Following Schattschneider, Paul Pierson has shown in several important works how and why policy choices at one point in time affect choices at subsequent points in time.[16] Similarly, Esping-Andersen pointed out in his seminal *Three Worlds of Welfare Capitalism* how, given the fact that we live in modern welfare states with unemployment insurance, health insurance, pension programmes and the like, 'Our personal life is structured by the welfare state and so is the entire political economy' (Esping-Andersen 1990: 141). The existence of the welfare state is a fact of modern political life that itself *shapes* politics, expectations and policy in the countries that have developed it.

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Finally, again as Pierson has shown, expectations are also moulded by the past. While some might point to America's adventure in Iraq as a simple product of power politics and/or the demand for oil, a historical institutionalist would more likely look to the patterns of past wars for an understanding of why this country reacted in the way it did to the 9/11 bombings. Certainly they were mistaken, but there should be little doubt that America's past successes in Germany and Japan – to say nothing of their perceived victory over Communism at century's end – led policy-makers in the Administration to believe that they could assert American power and bring successful capitalism and democracy to a former dictatorship.[17]

In sum, for historical institutionalists, *history is not a chain of independent events*. There is more than the temporal dimension implied in this basic point. Taking history seriously ultimately means that the scholar is sceptical of the very notion that variables are independent of one another. Instead, acknowledging the importance of history suggests an explicit awareness that important variables can and often do shape one another. Historical institutionalists, more than political scientists in some other traditions, are explicitly interested in these interactive effects on the interdependence of multiple causal variables.

The historical institutionalist is something like the environmental biologist who believes that in order to understand the specific fate of a particular organism or behaviour, she must explicitly examine that organism in the ecology or context in which it lives. This implies a different scientific ontology than that commonly found in the hard sciences of physics and chemistry. At the root of evolutionary biology is the assumption that the objects of analysis – living organisms – are fundamentally different from inanimate matter. While objects in the physical world often adhere to constant 'laws' of nature, biological organisms often defy attempts to reduce them to their essential components because of their complexity. Thus, as eminent evolutionary biologist Ernst Mayr points out, the development of biology as a science has required an investigation of 'additional principles' that apply only to living organisms. He argues: 'This required a restructuring of the conceptual world of science that was far more fundamental than anyone had imagined at the time' (Mayr 2004: 26).

Historical institutionalism represents something like this ontological move in social science. In order to understand historically specific events and long-term political outcomes, one cannot strictly apply methods and epistemologies drawn from the study of invariant variables that have fixed relationships across space and time. This, of courses, does not mean that it is not science – unless

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one's definition of science excluded biology as well; rather, it implies that the scientific methods applied should fit the subject being studied.

## Agendas

In recent years, two important intellectual agendas have emerged within institutionalist scholarship. The first is an attempt to understand better the mechanisms of institutional change; the second is an effort to comprehend the role of ideas in politics and history. I will discuss each separately and then argue that these issues are best dealt with when considered together.

It has become commonplace to argue that until recently most institutionalist literature had no fully theorized explanation for *change*. Indeed, the expectation for most institutionalists is that change will be difficult. There are several reasons for this. First, any given institution (whether a formal institution or a norm) is embedded within a larger set of institutions. Changing one set of rules can and often does have implications for others; therefore, there is likely to be significant resistance to change on the part of those who are advantaged in the broader context. Second, human beings form expectations around a given set of rules/institutions. Changing the rules can have long-term effects that may be difficult or impossible to predict. In this case, many would prefer simply to continue with the rules they currently have – even if they are not necessarily optimal. Third, institutions can become locked in because people invest in learning the rules. Changing rules can invoke significant up-front costs and be resisted by those who do not want to bear any new costs. Finally, because institutions affect behaviour, over time they can also shape preferences. Human beings may come to prefer a given institutional arrangement because it is what they are used to.

Given all these sources for stability, how can we explain change? Until recently, the dominant explanation has been 'punctuated equilibrium' (see Thelen and Steinmo 1992). The basic idea here is that institutions remain essentially stable (at equilibrium) until they are faced with an external (exogenous) shock. Increasingly, however, many historical institutionalists have come to criticize this logic, arguing that relying on exogenous shocks gives human beings no agency. There is something basically flawed, they argue, with the idea that political and institutional change is purely a product of fate.

Recently a number of scholars have pressed this agenda, with considerable success. Kathleen Thelen and Wolfgang Streeck brought together a group of younger scholars and asked each to explore the ways in which different

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political institutions are adapting or evolving in the context of global competitive pressures and demographic changes.[18] Through careful historically grounded analyses, these authors were able to identify a set of common patterns of institutional change. Thus they explore common types of institutional change. Unfortunately, Thelen and Streeck do not really offer an explanation for, or theory of, institutional change. Instead they explore various patterns of institutional change.[19]

To explain institutional change, one needs to bring 'ideas' into institutional analysis. If you are not a political scientist, you might be surprised to find that ideas play virtually no role in much current social science analysis. Marxism, rational choice and pluralism alike all assume that interests are the driving forces of politics, and that ideas are either justifications or simply 'noise'. While traditional behaviouralists have no *a priori* reason to argue that ideas are irrelevant to politics, it is clear that ideas are difficult to measure and quantify and are therefore left out of these analyses for practical reasons. Historical institutionalists, however, are not wedded to a particular grand theory or to a specific methodology; consequentially, 'ideas' have come to take a central place in their analyses.[20]

Peter Hall famously wrote about the power of economic ideas in his analysis of the growth of Keynesian economic thought, exploring how and why specific ideas about economic management came to dominate so many countries at roughly the same historical epoch (Hall 1989). Hall demonstrates how these ideas, once embedded, had framing effects and consequently became something like basic templates upon which other political decisions were made. Taking Hall's analysis as a starting point, Mark Blyth went on to explore the rise and fall of Keynesianism in the United States and Sweden, with the specific intent of understanding *both* how ideas develop and influence people *and* how they can be used as weapons in political struggles (Blyth 2002). In other works, Blyth has forcefully argued that the concept of interest itself makes no sense without appreciating how individuals understand their interests (see also Kratochwil, ch. 5 and Pizzorno, ch. 9). In other words, ideas are at the very root of political behaviour.[21]

In my view, much of the most interesting work in the historical institutionalist tradition today is found precisely amongst those who are trying to better understand the ways in which ideas, values and beliefs affect history and politics *and* who are specifically applying these insights to understanding institutional change more broadly (McNamara 1998; Marcussen 2000; Lieberman 2002; Katznelson and Weingast 2005). For these scholars, institutional change is the product of changes in ideas held by actors. I mean 'ideas'

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here in the specific sense that ideas are *creative solutions to collective action problems*. For example, when we normally say 'I have an idea!', we are in effect saying we have a solution to a problem. Seen in this way, institutional change comes about when powerful actors have the will and ability to change institutions in favour of new ideas. A group or collective may agree that a particular idea is a 'good idea' if they agree that there is a problem that needs solving, *and* they agree that this idea might actually solve the problem. Seen in this way, ideas are not 'irrational', but instead are best understood as creative adaptations that can be evaluated both on rational and emotive grounds.[22]

To illustrate these points, let us consider the example of basic welfare state institutions of the twentieth century (unemployment insurance, public pensions or banking regulations). First, it should be obvious that initially these proposals were simply untested ideas (creative problem solutions) whose promise was to help solve some of the social and economic problems created by the mid-twentieth century capitalist economy (economic dislocation, unemployment, increased poverty). As the economically vulnerable in society gained more and more power through the ballot box in Western democracies, and as the economic failures of unregulated capitalism became increasingly apparent, elites' ideas changed. The economic experiences of the 1920s and 1930s led many to see these issues as real problems. Additionally, the performance of the governments in World War II (economic management, regulation of production and quite simply the fighting/winning of the greatest war in history) led many to believe that governments could and would do a good job managing new tasks. Over time, then, there was widespread agreement that capitalism could and should be regulated and that government had an appropriate role in managing the economy and distributing the wealth generated in that economy. The specific tax, welfare and regulatory policies that were implemented over the following thirty or forty years cannot be understood as anything less than ideas which were eventually put into practice (institutionalized).

But, of course, modern democratic capitalism did not stand still (there was no equilibrium). Along with rising standards of living and increased equality, expectations grew as well. Moreover, political leaders kept promising things they were less and less able to deliver. Especially after the oil shocks of the early 1970s and the stagflation that followed, people increasingly came to believe that governments regulated too heavily, taxed some citizens unfairly, and in general were less capable than they promised to be.[23] Neoliberal ideas grew in popularity in the later decades of the twentieth century because more and more people (especially the rich and powerful) came to share the belief that

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'government was not the answer, but the problem' to quote Ronald Reagan's famous phrase. Neoliberal pro-market policies became increasingly persuasive because an increasing number of people (elites and average citizens as well) were persuaded by the logic of the neoliberal argument; they accepted the problem definition and then came to agree to the problem solution. It is important to understand that there was no 'proof' that neoliberal policies would address these problems. The new policies (tax cuts, programme reductions and pro-market re-regulation) were simply 'ideas' that promised to dampen inflationary tendencies of the Keynesian era, put more money into the hands of capitalists who could reinvest, and constrain 'wasteful' government spending. Once again, those who believed these were good 'ideas' shared a sense of the problems facing capitalist democracies and believed that the neoliberal policy solutions would help solve these problems.

To be sure, both the establishment of welfare state institutions and neoliberal policies could be seen as being in the economic interest of the elites who promoted them. But to see it this way assumes that we have an objective and precise understanding of the ways in which the modern economy actually works and that there is an objective and easily knowable way of understanding an actor's 'self-interest'. One can argue that we have neither. First, the modern capitalist economy is far more complex and contingent than even the most sophisticated mathematical tools can hope to model accurately and precisely. Second, the very foundation of an individual's (or a group's) interest is fundamentally rooted in their beliefs (about how the world works), their values (what constitutes good outcomes) and how best to achieve these outcomes (problem solutions).

Consider the following question: did the tax cuts of the 1980s stimulate growth and increase government revenue as was promised, or did they simply create the largest budget deficits in history? The answer to this question depends on who you ask. If, for example, you ask an economist who believes in neoliberal economic theory (with or without a Nobel Prize), she will almost certainly tell you that the tax cuts worked and that the economy grew in the 1990s because of the tax cuts (she could also provide you a massive econometric model to show you this as well). If you asked an economist who does not believe in neoliberal economics (with or without a Nobel Prize) she would just as convincingly argue that the tax cuts did not work as promised and that it took the tax increases of the 1990s to get the economy back on track and back into balance. She too could provide a massively complex mathematical model to 'prove' her argument. Which economist you choose to believe is up to you. But the key point here is that even if economists cannot agree at the

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most basic level on the effects of economic rules or institutions in the past, then surely we have to understand that prospective policy ideas are even more a leap of faith. Second, if we cannot know the effects of past ideas, how can we rationally calculate our self-interest for future policy ideas?

Bringing ideas into our understanding of institutional change, then, brings agents back into institutional analysis. One could argue that a key weakness of institutionalism in the past has been that actors could be simple hostages of the institutions that they inhabit. Integrating ideas into the analysis addresses this problem by making institutions both a constraining/incentivizing force and the object of political contestation.

Bringing ideas specifically into institutional analysis thus allows for a better understanding of institutional evolution. A small, but growing group of historical institutionalists are in fact moving in this direction specifically attempting to bring evolutionary theories and ideas to the study of institutional change.[24] It is outside the scope of this chapter to expand on these theories, but the basic argument is to see institutions, ideas and the environment in a co-evolutionary process. This perspective sees history and politics as dynamic processes that are constantly evolving, rather than seeing history as a process lurching from one equilibrium to another. The evolutionary approach, moreover, sees outcomes as contingent and non-predictable rather than linear and predictable. Finally, the evolutionary approach specifically explores power relations and integrates agency into the analysis rather than seeing actors as prisoners of the institutions they inhabit.

## Political and social 'science'

At the heart of many of the deepest and most difficult battles within social science is a fundamental struggle over the meaning of science. For many, science is the search for systematic regularities and generalizable laws. In this view, one studies the empirical world only because it offers the evidence that can be used to build and test theory. Particular cases or specific events may be interesting – just as a good novel is interesting – but the goal of social science is not to understand any particular event; it is to build theories that can be used to explain many (or even all) events. For these scholars, understanding real outcomes is not the most important point; creating, elaborating, refining a theory of politics is (Weingast 1996). Morris Fiorina describes his scientific orientation in the following way: '[We are] not as interested in a comprehensive understanding of some real institution or historical phenomenon, so

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much as in a deeper understanding of some theoretical principle or logic . . . [F]or most PTI scholars, breadth trumps depth; understanding 90 per cent of the variance in one case is not as significant an achievement as understanding 10 per cent of each of nine cases, especially if the cases vary across time and place' (Fiorina 1995: 110–11).

This reveals precisely the difference between historical institutionalists and their more 'rationalist' institutionalist brethren. Historical institutionalists *are* interested in the specific cases. Being able to explain 10 per cent of the variance in nine cases is probably no better than a semi-educated guess, and not particularly useful or interesting. If we could explain the important events (why revolutions happen, why some countries have large welfare states, why labour is so weak in some countries), I expect that most HI scholars would be happy with even less than 90 per cent.

Historical institutionalists (both political scientists and sociologists) are sceptical of the grand ambitions of social science – at least when understood as Newtonian physics. For most of these scholars, the goals are more proximate and the ideal theory should be less grand. The HI scholar is primarily interested in *explanation* – not prediction (see della Porta and Keating, ch. 2, for this distinction). Though it is rarely explicitly stated, a basic assumption of this view of social science is that meaningful prediction is impossible. For HI scholars, predictions can only be proximate and predictions, *not* because we lack the tools, models, datasets or computing power, but rather because of contingency, and the complex interaction of *inter*dependent variables over time. In history, the very objects of our study (institutions and human beings) change, adapt and are affected by history itself. Prediction and the related conception of science imply a linear analysis of variables that can be distinguished from one another and which react to one another in predictable ways (see Héritier, ch. 4). For many social scientists, such analysis denies the realities of the world in which we live.[25] In this view, the study of politics is not, and cannot be, like physics, because what we study and what we are interested in explaining are not inanimate objects to which absolute, invariant and fixed laws apply. Studying history with methods and models derived from physics is like studying poetry with algebra.

As several have pointed out, HI scholars tend to be interested in important and relatively rare events. A research programme motivated by an interest in real-world puzzles and rare events has advantages and disadvantages over a programme motivated by a desire to find general laws of history or politics. It is well known that some methodologists outside this tradition question the very validity of the HI approach because it tends towards 'selecting on the

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dependent variable'. To be sure, a research strategy that specifically focuses on important cases and big puzzles could potentially suffer from the obvious dangers of selection bias. This is an important criticism worth considering here. Does the very nature of the kinds of questions in which HI scholars are interested undermine the scientific credibility of their work?

First, as Pierson and Skocpol (2006) point out, we must think of social science scholars as a 'multi-generational research community' that results in a 'powerful accumulation of results, including falsifications as well as substantiated arguments'. Each new study contributes to our fund of knowledge about historical events; it retests and re-examines the analyses that went before. Second, as Dietrich Rueschemeyer argues, case studies can do more than generate theoretical ideas. They can test theoretical propositions, and they can offer persuasive causal explanations (Rueschemeyer 2003: 318). Noting the persistent scepticism towards historical case study work, Rueschemeyer goes on to argue, correctly, that it rests on the mistaken idea that a single case marks a single observation. Good historical analysis that is analytically oriented engages the case at multiple points, thereby confronting explanatory propositions with multiple data points (see Vennesson, ch. 12 for an elaboration of these ideas).

It is also important to remember that this research strategy has several methodological advantages. As noted above, HI scholars are interested in the ways in which history itself shapes outcomes. Thus, they specifically and self-consciously examine patterns over time. By extending the time frame, first, one expands the number of observations and thus helps to deal with the small-*N* problem noted above. But historical process tracing also allows the scholar to test for the arrow of causality in a way in which simple correlation analysis cannot. Finally, process tracing is an instrument that helps the researcher to be sensitive to the temporal boundaries, or period effects, with respect to the specific causal claims being forwarded (see Vennesson, ch. 12). If history matters, then looking at processes over time allows the researcher to place particular events in a particular time – without at the same time missing the overarching patterns. It is these patterns, after all, which are very likely to offer the most compelling and interesting dependent variables. Historical institutionalists, in other words, look at the forests as well as the trees (Pierson and Skocpol 2006).

Of course, there are also serious dangers in *not* looking at the big historically interesting puzzles – because they are too rare, or they are not randomly distributed or, most fundamentally, because these big events have an impact upon all subsequent events. Without historical accounts, important outcomes will go unobserved, causal relationships will be incorrectly inferred and, finally,

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significant hypotheses may never even be noticed, even less tested. Jim Mahoney (2000b), who surveyed several decades of scholarship and research on democratic and authoritarian regimes, concluded: 'If one were to strike all comparative historical research from the record, most of what we currently know about the causes of democracy and authoritarianism would be lost'. Indeed, if we were to follow strictly the logic of inquiry promoted by King, Keohane and Verba (1994), then Reinhard Bendix's *Nation Building and Citizenship*, Barrington Moore's *Social Origins of Dictatorship and Democracy*, Samuel Huntington's *Political Order in Changing Societies* and Theda Skocpol's *States and Social Revolutions* – to name just a few classics – could not have been written.[26]

## Conclusion

It may be sadly true that much of 'political science' has moved away from asking important questions about the real world. It is certainly true that many political scientists believe we should ignore analyses that cannot be 'falsified' and eschew variables that cannot be quantified. Theirs is a political science that treats politics and history as if it grows in a Petri dish and can be measured in centimetres or kilos.

The historical institutionalist does not accept that political science *must* be so narrow. To be sure, many interesting things can be learned from formal, behavioural and, certainly, experimental approaches to the study of politics. But to take history out of our 'equations', institutions out of our models, and real people out of our analyses would leave us with an impoverished pseudo-science. Not everyone who agrees with this statement would call herself a historical institutionalist. But if you think history and ideas matter, institutions structure actors' choices but are subject to change by actors themselves, and real people make decisions that not always efficient or purely self-interested, then you probably are a historical institutionalist.

## NOTES

1 The term came out of a small workshop held in Boulder, Colorado in January 1989. Participants included Douglas Ashford, Colleen Dunlavy, Peter Hall, Ellen Immergut, Peter Katzenstein, Desmond King, Frank Longstreth, Jonas Pontusson, Bo Rothstein, Theda Skocpol, Sven Steinmo, Kathleen Thelen, George Tsebilis and Margaret Weir. *Structuring Politics: Historical Institutionalism in Comparative Politics* (Steinmo, Thelen and Longstreth 1992) grew out of this workshop.

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2 Some other examples of social science analysis written before this phrase came into usage but which would clearly be defined as 'historical institutionalist' today include Wilson (1891), Polanyi (1944), Selznick (1949), Truman (1951), Rustow (1955), Eckstein (1960), McConnell (1966), Polsby (1968), Eisenstadt and Rokkan (1973), Schmitter (1974), Tilly and Ardant (1975), Zysman (1977), Katzenstein (1978), Dodd and Richard (1979), Skocpol (1979), Huntington (1982), Rothstein (1982), Skowronek (1982), Esping-Anderson and Korpi (1983), Skocpol and Ikenberry (1983), Katznelson and Weir (1985), Gourevitch (1986), Skocpol and Amenta (1986) and Rokkan *et al.* (1988).
3 For a similar analysis, see Blyth (2006).
4 Indeed, it was in the immediate post-war years that many departments of government and/or politics changed their names to 'political science'.
5 Indeed, the emphasis in much of the historical descriptive work up to that point had been to explain the exceptional character of the particular historical epoch, country, region or revolution under study.
6 For a fascinating and thoughtful exposition of these views, see Wallerstein (2001).
7 Deans and department chairs understood that Institutional Cost Recovery (ICR) moneys could contribute substantially to university and departmental budgets. Thus, foundations such as the National Science Foundation (which were driven by hard-science norms) contributed to the shift.
8 It is worth noting here that this fact seemed obvious to most Europeans and scarcely came as a revelation.
9 George Tsebilis is often incorrectly credited with introducing the idea of veto points.
10 Amazon.com (accessed February 2007) lists 794 books when one searches for the specific phrase 'historical insitutionalism'. 'The New Institutionalism' brings up 1,679 books.
11 For a thorough discussion of these three types, see Peter Hall and Rosemary Taylor's (1996) excellent analysis.
12 I refer here to the standard rational choice (RC) school. Certainly, there are many RC scholars who have relaxed these assumptions considerably. To be frank, the more they do so, the more they sound like historical institutionalists. See Weingast (1996), Bates, Greif, Levi *et al.* (1998) and Ostrom (1998).
13 Still, these distinctions are difficult to sustain. For example, the widely known 'sociological institutionalist' text edited by Paul DiMaggio and Walter Powell, *The New Institutionalism in Organizational Analysis*, is explicitly interested in power and coercion as important variables for framing political behaviour, along with norm-building and pattern development (DiMaggio and Powell 1991). I thank John Campbell for pointing this out to me.
14 An example outside politics may prove illustrative. Many of us recognize that firstborn children have a very different developmental experience than second (or later) children. Not only are the parents more experienced after the first child, they are also taking care of more than one child at a time. Finally, and equally importantly, subsequent children grow up in a home where there are older siblings – something the first child, by definition, cannot do.
15 Cited in Pierson (1993: 595).
16 See, for example, Pierson (1993, 2000, 2004).
17 Just as certainly, the failure of the Iraq experience will shape American foreign policy for decades to come.
18 See also John Campbell's (2004) *Institutional Change and Globalization.*

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19 The five models of institutional change identified by Thelen and Streeck are (a) 'displacement' – where one institution displaces another, (b) 'layering' – when an institution adopts new functions on top of older functions, (c) 'drift' – when the environment in which an institution exists changes, but the institution does not adapt in stepwise fashion (see also Jacob Hacker's chapter in Thelen and Streeck's volume), (d) 'conversion' – where institutions take on new functions, goals or purposes, and (e) 'exhaustion' – meaning institutional breakdown and failure.
20 To be sure, not all historical institutionalists are specifically concerned with the role or power of political ideas, but many are; see Campbell (2002).
21 See Blyth (1997, 2003), see also Marcussen (2000), Pasotti and Rothstein (2002) and Steinmo (2003).
22 There has been an unfortunate and unnecessary tendency to pit 'ideational' analysis against 'rational' choice in a way that appears to argue that one bases decisions *either* on ideas *or* on rational calculations. This is an absurd distinction.
23 Interestingly, there was significant variation in this regard. Quite obviously, some governments were more capable of delivering on their promises efficiently and fairly than others (compare, for example, Sweden and the United States). The best explanation for these variations is, of course, differing institutional structures (see Steinmo 1993).
24 For recent work pointing in these directions, see North (2006) and Lewis and Steinmo (2007).
25 For example, in basic statistical analysis it is common to tell students that they must watch for multicollinearity and take care only to examine questions in which the multiple variables in an equation can be separately identified. This is not because this is the way the real world works, but because unless one takes these precautions the statistical inferences drawn will be methodologically invalid. The problem, of course, is that the method we use can too easily define the questions we ask.
26 This obvious fact was pointed out to me by Jeffrey Kopstein.

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