

Chapter 2

The Shared Challenges of Institutional Theories: Rational Choice, Historical Institutionalism, and Sociological Institutionalism



Henry Farrell

The last two decades have seen many calls for an integration of scholarship on spatial patterns of development and scholarship on institutions. Initially, much of the literature on spatial development was defined deliberately in contradistinction to the kinds of institutionalism seen in economics and political science, while sharing significant orientations with sociology. Specifically, it rejected the overt individualism of much institutionalism in political science and nearly all institutionalism in economics. Ash Amin (1999) argued that his approach was institutionalist precisely because it was not based on the individualist assumptions of *homo economicus*, or economic man. While Amin had sharp differences with other scholars interested in localized economies, they all agreed that the kinds of local *thickness* that fostered economic success were inimical to the more individualist orientations that rationalist political scientists and economists saw as the basis of institutional compliance and change (Becattini, 1990; Piore & Sabel, 1984).

These theoretical battles are giving way to a more practical interest in common interchange, focusing on how institutions, however conceived, shape outcomes. Thus, for example, economic historians have claimed that countries' long term trajectories of economic growth are a product of their specific institutional endowments (North, 1990; North, Wallis, & Weingast, 2009). Political scientists have turned to path dependence to explain why welfare states have endured despite substantial changes in party politics (Pierson, 2000). Sociologists have explained long term patterns of political development as a product of path dependence (Mahoney, 2000), while social choice theorists first turned towards institutionalism in order to deal with *chaos theorems*, which predicted irresolvable instability as a likely product of even moderately complex strategic situations (McKelvey, 1976, 1979; Schofield, 1978; Shepsle, 1979). Geographers are examining how institutions mediate between regional policies and regional outcomes (Glückler & Lenz, 2016).

H. Farrell (✉)

Department of Political Science, The George Washington University, Washington, DC, USA
e-mail: farrellh@gwu.edu

However, the institutional turn has come at a cost. On the one hand, social scientists need a theory of how institutions can change, because they self-evidently do change, while on the other, they need a theory of how institutions can have material consequences for human behavior. This obliges them to steer a dangerous course between two obstacles. They need both to have a theory of institutional change and a theory of institutional effects. The former requires them to identify the external factors that lead institutions to change over time. The latter requires them to identify the causal effects that institutions have for other factors.

The problem, as Przeworski (2004) cogently described it, is that if you have a theory which does both at once, why not cut out the middle man? In other words, if Factor X leads to institutional change, which then leads to Outcome Y, why not get rid of the intermediating factor, *institutional change*, because it appears not to be doing any additional work. If institutions are mere transmission belts for other factors, they are not causally interesting. If they are more than transmission belts, one needs to say why and how. In other words, one needs an endogenous theory of institutions, something that does not properly yet exist.

The difficulties of meeting this objection helps explain the volatility of argument around institutional theory. Theories of institutional consequences, which assume that institutions are stabilizing forces that structure human behavior, beg the question of why institutions should themselves be stable, leading theorists to search for theories of what causes institutions, and hence institutional change. These theories, however, raise the question of why institutions are important if they are the mere condensate of some underlying structural force or forces, obliging a return to a proper account of how institutions have visible consequences, so the pendulum of argument swings back.

This raises salient problems for economic geographers who wish to explain, for example, economic growth or innovation. For sure, there are theories of how institutions may have effects for human behavior, and hence shape growth or innovation. Yet these theories are problematic, insofar as they often do not illuminate the underlying factors explaining why one gets one set of institutions (say—growth and/or innovation promoting) and not another. Furthermore, theories that do look to do this—by explaining why one country, or region, or locality has one set of institutions, and not another—are liable to collapse institutions into the underlying forces that are intended to explain them. This means that institutionalists need to think more carefully about what institutions actually are, and how they might have some independence both from the forces that shape them and the behaviors that they shape.

In the remainder of this contribution, I look to contribute to existing efforts to reconcile the study of knowledge in space and the study of knowledge in institutions, focusing on the latter rather than the former. I begin with a brief survey of the rationale among scholars studying knowledge in space for embracing social science accounts of institutions. I then proceed to briefly outline the three major approaches to institutions in the social sciences—rational choice institutionalism, historical institutionalism, and sociological institutionalism—outlining briefly the development of each approach, and how each has faced these enduring problems, despite

their distinct origins and trajectories of development. I then, in conclusion, briefly sketch out an alternative approach, building on joint work with Danielle Allen and Cosma Shalizi, which starts to provide an alternative account of institutional change that arguably helps reframe the problem in some useful ways.

Economic Geography and Institutional Change

Prominent scholars studying spatial development have recently called for better integration of insights from social science institutionalism into their accounts. For example, Farole, Rodriguez-Pose, and Storper (2011) argued that both economic geographers (despite the centrifugal tendencies of the field) and social science institutionalists are interested in the underlying determinants of growth. Bathelt and Glückler (2014; Glückler & Bathelt, 2017) suggest that institutional theory can help economic geographers better understand the underlying dynamics of innovation. Their arguments built on earlier scholarship (e.g., Amin & Thrift, 1995), which sought specifically to understand the contribution of institutions to geographically specific economies.

As these scholars stress, the dialogue should be two-way. The study of spatial phenomena has much to offer to institutionalist theory as well as vice versa. Specifically, attention to the interaction between different scales of economic activity, at the local, regional, national, and international level, could usefully help inform social science debates about institutionalism, which often rest on indefensible and convenient assumptions, such as *methodological nationalism* (Callaghan, 2010; Farrell & Newman, 2014), to wave away such interactions. However, in this chapter, I focus on just one direction of influence—how standard approaches to institutions can inform the study of spatial development and what is currently missing from these approaches.

Social science institutionalism may offer a more systematic account of key topics of interest to economic geographers. Thus, for example, Farole et al. (2011) pointed to the burgeoning literature on the sources of economic growth. They argued that institutionalism offers multiple benefits that economic geographers ought to take advantage of.

First, it potentially provides more theoretic rigor. Specifically, it provides the building blocks for more precise models, which could not only provide a better understanding of how institutions work in practice, but also help scholars move beyond thick description toward a more analytically precise language that would better articulate the relationship between abstract models and complex facts. Second, as a result, institutionalism contains the seeds of better comparisons. If researchers have better defined accounts of institutions, and of the precise ways in which they affect, for example, economic development, they will be able to build better accounts of how (apparently) different institutions may lead to similar outcomes in some instances, while (apparently) similar institutions lead to different outcomes in other instances.

Bathelt and Glückler (2014) were more concerned with innovation than economic growth as such, but they reached similar conclusions. They cautioned that the social science literature on institutionalism is itself often riven by contradictions, for example, concerning what exactly an institution is. However, they argued that institutions provide a valuable conceptual tool for understanding the constraints on economic action. Actors respond to the institutions that they are embedded in, thanks both to broad social logics and individual self-interest.

These accounts highlight how institutions may be valuable for the study of spatial development processes. However, they also plausibly need more than existing accounts of institutions are capable of giving. On the one hand, they call for increased conceptual rigor in understanding how institutions work—it is, in part, this intellectual rigor that can help economic geographers better focus their arguments and build beyond thick description. On the other, they call for attention not only to how institutions shape economic interactions, but also to how economic interactions shape institutions. Thus, in the description of Bathelt and Glückler (2014) institutions involve relational action:

Where real interaction is informed by historical patterns of mutual expectations (path-dependence) and where, at the same time, contextual interaction contributes to the transformation of these patterns based on the principle of contingency. The interplay between experiential action and patterns of instituted expectations drives a recursive process of correlated interactions and transformative institutionalization. (p. 344)

Similarly, Farole et al., (2011) said that:

The relationship between institutions and economic growth is an endogenous one. Institutions and economic growth co-evolve, with changes in capacity building and improvements in governance contributing to the development of economic activity and vice versa. Institutional improvement may more often be the consequence, rather than the cause, of development. (p. 16)

In other words, researchers seek a theory of institutions that is endogenous so that it captures the ways in which institutions are imbricated with the actions that they foster. Institutions are not ahistorical constants; rather, they are themselves the product of human agency, and as humans enact institutions they correspondingly transform them.

Here, however, social science institutionalism is less useful than it might first appear. Different approaches to institutions arose in different disciplines, in response to different imperatives. Yet they all struggle with the questions of how to capture endogenous relations between expectations and action, and how to link expectations to underlying causes. In each discipline, scholars tended initially to focus on explaining stability rather than change, using institutions to explain why patterns of behavior endure under circumstances where one might expect them to change. In each, a subsequent wave of scholars has reacted against institutional determinism, looking to incorporate the possibility of change, by explaining the underlying forces that shape institutions, but creating new perplexities as a consequence.

As Riker (1980) famously argued, one cannot claim that institutions stabilize social interactions, without explaining how institutions are somehow different from the interactions that they are supposed to stabilize.

In the end, therefore, institutions are no more than rules and rules are themselves the product of social decisions. Consequently, the rules are also not in equilibrium. One can expect that losers on a series of decisions under a particular set of rules will attempt (often successfully) to change institutions and hence the kind of decisions produced under them. In that sense, rules or institutions are just more alternatives in the policy space and the status quo of one set of rules can be supplanted with another set of rules. (pp. 444–445)

Without some clear understanding of how institutions differ from the decisions that they are supposed to structure, institutional theory is liable to degenerate into a series of just-so-isms, which posit that institutions have binding force, while providing no specific rationale for why they are binding.

The obvious retort is to turn to some external circumstance—such as power relations, the attractions of efficient outcomes, or the binding power of social ritual—to explain why institutions are enduring and how they are capable of exerting force on people’s decisions. People may comply with institutions because they fear the wrath of more powerful actors, or because they recognize the benefits from coordinating on a salient solution, or because they are caught up by the demands of ritual behavior. Such arguments also provide the basis for theories of institutional change. Institutions may change when power balances shift, or when new, more attractive solutions become available, or when skilled social actors construct new binding myths.

However, as Przeworski (2004) sharply observed, it is hard to invoke such external forces to explain institutional compliance and institutional change without suggesting that institutions are epiphenomenal, and that what is doing the actual work are the external forces rather than the institutions themselves. Przeworski pointed out that most institutionalist accounts do a very bad job at showing that institutions matter in their own right—which is to say that current accounts have difficulty in theorizing how institutions have independent causal force.

It is notable that these theoretical difficulties spring up across quite different approaches to institutions, despite their various origins and emphases. In part, this reflects very broad problems in the social sciences (such as the relationship between structure and agency). In part, it reflects problems that are specific to institutional theory, and in particular to the difficulty of distilling a clear definition of institutions from the murky interactions of beliefs, decisions, and actions and the social forces conditioning all three.

Rational Choice Institutionalism

For rational choice scholars, institutions are usually either *structures*—forces which conduct actors to select one equilibrium or another, or *equilibria*—sets of strategies from which no actor has any incentive to defect if no other actor defects. Under both

definitions, institutions may usually be thought of as rules—regardless of whether these rules are considered to be exogenous regularities that structure choices or chained patterns of equilibrium behavior in which every actor will continue to behave in specific ways provided others do the same. Current rational choice institutionalism is the culmination of two distinct lines of inquiry—one in social choice theory, the other in economics—which intersected in the early 1990s. Social choice theory, building on eighteenth-century work on voting by the Marquis de Condorcet and others, gave rise to an extensive formal literature in theoretical economics in the second half of the twentieth century. Economists such as Kenneth Arrow (2012), Duncan Black (1948), and Amartya Sen (1997) arrived at basic results about the aggregation of decisions, looking to examine the strengths and limitations of various voting schemes and other schemes for collective choice, under assumptions of rationality. This literature soon discovered various paradoxes and instabilities, which began to have important consequences for political science as well as economics.

Basic results such as Arrow's Possibility Theorem (Arrow, 2012) suggested that it was impossible to universally reconcile minimal desiderata for decision making. Others, such as Downs (1957), provided a more optimistic account. For example, under Downs's *economic theory of voting*, political outcomes were likely to converge on the preferences of the *median voter*, creating a centrist equilibrium.

However, it soon became clear that the more optimistic account depended heavily on favorable assumptions, including the assumption that voters' preferences could be expressed on a single dimension (e.g., a single left-to-right scale). Work by McKelvey (1976, 1979) and Schofield (1978), among others, demonstrated that if politics had more than two dimensions, then majority rule could not provide stability. Instead, politics could end up cycling from one alternative to another, without ever necessarily gravitating towards any central solution or set of solutions.

These chaos theorems generated immense frustration among political scientists, because they not only cast doubts on the stabilizing benefits of democracy, but also suggested a degree of radical instability that seemed at odds with empirical observations. Politics appeared to be relatively predictable—so what was the root cause of stability?

The answer—according to a prominent line of argument developed in political science—was institutions. For example, one might think of the institutional structure of the U.S. Congress—which is composed of different committees, each with a specialized jurisdiction—as simplifying politics in ways that produced stability and predictability. Congressional committees could carve out specific issue dimensions, reducing the issue space so that each issue dimension was dealt with separately, and a chaotic space of social choice across multiple dimensions was transformed into a series of iterated decisions taken within discrete jurisdictions (Shepsle, 1979). Hence, institutional arrangements such as congressional committees could avoid the chaos of multidimensional voting spaces, and instead produce so-called *structure-induced equilibrium* outcomes. As scholars began to develop the structure-induced equilibrium approach further, they began to use noncooperative game theory rather than social choice theory to model decision making, seeking to capture

the essential details of even quite complex institutional arrangements as game trees, in which individual strategies potentially lead to equilibrium outcomes.

This literature hence began from a puzzle—invoking institutions to explain why people’s choices remained stable even under circumstances when rational choice theory would predict that they should not. As it was developing, a second body of work in economics began to confront a very different puzzle of observed stability (North, 1990). Economists studying development believed that they had a good sense of what was necessary to produce economic growth—strong markets and free enterprise. Basic rational choice theory suggested that national economies should converge over time on the practices that led to increased economic growth, because otherwise they would be leaving dollar bills on the pavement. Yet in reality, countries continued to stick to dramatically different growth paths, rather than converging on the more efficient possibilities offered by countries with free markets. What explained this anomaly, in which national economies remained stably attached to practices that made no sense?

According to the influential work of North (1990) the answer lay in the relationship between institutions and organizations. Institutions, as sets of rules, shape the incentives in a particular society. Organizations, as collective actors, pursue their self-interest within a given set of rules, perhaps changing those rules in the process. Borrowing from Arthur’s (1994) work on path dependence, North argued that national societies tended to develop along specific trajectories. Societies with institutions that have appropriate incentive structures will tend to develop along a virtuous path, in which institutions and organizations reinforce each other so as to encourage growth-promoting activities. Societies with institutions that tend to promote predatory behavior by the state or other actors may find themselves trapped on long-term, low-growth trajectories, but lack the institutions and organized social actors that might allow them to escape these constraints.

Both of these accounts struggled with the question of why institutions have binding force. Riker’s (1980) initial critique of institutionalism was aimed directly at structure-induced equilibrium approaches, which, he politely suggested, were less a solution to the problem of social instability than an unconvincing *deus ex machina*. Since institutions were themselves the product of choices (presumably made across multiple dimensions) they should be just as subject to problems of instability as the social choices they purportedly structured. Put less politely, invoking institutions as structures—without explaining the choices through which these institutions had themselves arisen and why these choices were enduring—was sharp practice.

Yet North’s (1990) arguments, too, had fuzzy microfoundations. At times, North seemed to argue that actors’ microlevel choices were driven by their desire to secure benefits for themselves, regardless of whether this would help or hurt others. At other times, North seemed to suggest that actors’ choices were driven by the desire to find efficient arrangements (as argued by his sometime rival in the new institutional economics, Oliver Williamson [1975, 1985]). Under the one account, institutions were binding because they produced good outcomes for particular powerful individuals. Under the other, they were binding because they produced good outcomes for everyone. The failure to stick to one or the other allowed North to shift

back and forth between explanatory frameworks without ever committing himself to a fully developed set of microfoundations.

These deficiencies inspired pushback. Structure-induced equilibrium accounts gave way to disagreements over whether it was better to think about institutional equilibrium or equilibrium institutions (Shepsle, 1986). The former reflected the emphasis of the structure-induced equilibrium approach on explaining how specific institutional features might produce one or another equilibrium, depending, for example, on the order within which actors made choices and had power to set the agenda. Typically, it used models based on one-shot games, treating the institutions as part of the game tree.

Equilibrium institution approaches, instead, treated institutions as the outcomes of games rather than structures within the game. Instead of looking to one-shot games with complex structures, they typically treated social interactions as indefinitely iterated games with simple structures (Calvert, 1995). These simple games, however, could give rise to quite complex and sophisticated equilibria, in which actors continued to behave in particular and sometimes quite complex ways, subject to other actors continuing to behave in the expected fashion. Under the so-called *folk theorem* an enormously wide variety of equilibria can arise in many indefinitely iterated games with reasonable parameters. Hence, the equilibrium institutions approach did not provide an account of how institutions arose or changed, so much as an account of which institutions were possible given particular parameter values. To understand how such equilibria arose, one had to turn to selection mechanisms outside the game itself.

This approach was swiftly adapted to understand the kinds of questions that North (1990) and his colleagues grappled with. Hence, for example, Greif (1994) investigated the differences between Genoese and Maghribi traders in the mediaeval period, treating both sets of traders as engaged in an indefinitely iterated *One Sided Prisoner's Dilemma* game, and looking to the ways in which different *cultures* might give rise to different sets of expectations, and hence different self-reinforcing institutions. Milgrom, North, and Weingast (1990) used a broadly similar theoretical approach to understand medieval Champagne Fairs (see also Calvert [1995] for an extensive theoretical overview and framing).

These accounts, however, continue to have difficulty (a) in distinguishing institutions from behavior and (b) in explaining when institutions might change. In the account of Calvert (1995), for example, no very sharp distinction is drawn between strategically implicated behavior, organization, and institution; each being a more or less sophisticated example of behavior conditioned on expectations of the behavior of others. For Greif and Laitin (2004), who adopted a formally similar approach, institutions consisted of factors influencing behavior rather than the behavior itself, so that an institution was “a system of humanmade, nonphysical elements—norms, beliefs, organizations, and rules—exogenous to each individual whose behavior it influences that generates behavioral regularities” (p. 635). While this definition is encompassing, it makes it difficult to capture precisely how these very different elements interact. Such a broad definition of institutions makes it difficult to be sure what—apart from behavior—is not part of the institution under examination.

Even more pertinently, equilibrium accounts of institutions almost by definition have great difficulty in explaining change. A game-theoretic equilibrium, after all, is a situation in which no actor has any incentive to deviate from his or her strategy given the strategies of others. Greif and Laitin's (2004) game-theoretic account of institutional change is less an account of change as such, than an account of how institutions may have unintended consequences for the parameters that they depend upon, leading them to become *self-reinforcing*, or *self-undermining*, depending on whether the behaviors associated with the institution become possible under a broader or narrower range of parameters. It cannot explain within its own formal framework how one institution may change into another. Yet problems of *real* institutional change are endemic in economic development.

This poses the problem of developing equilibrium-based models that can encompass institutional change. Acemoğlu, Johnson, and Robinson (2001) were forerunners in developing methodological answers to Przeworski's (2004) problem—using an *instrumental-variables* approach to argue that institutions have indeed had independent consequences for development (albeit not to Przeworski's own satisfaction). However, for just this reason, they had difficulty in explaining what factors lead to institutional change.

For example, Acemoğlu and Robinson (2006) provided a stylized account of how the transition from authoritarian regime to democracy might take place, arguing that institutional change will be the result of bargaining processes and social conflict (Knight, 1992). However, although such modeling strategies can capture transitions between different political systems that are well defined *ex ante*, they are poorly suited for capturing more open-ended and gradual transitions.

This shortcoming means that these scholars have difficulties in answering the crucial question posed by North (1990), Greif (2006), and others, of how mediaeval European countries with predatory elites and drastically underperforming economies were transformed into modern societies. Though there is a rich body of work that employs comparative statics (Acemoğlu & Robinson, 2012; Greif, 2006; North et al., 2009), the dynamic aspects of this question remain more or less unexplored. Existing accounts provide histories that are notably stronger at comparing systems or stages of development than at capturing the actual mechanisms of transformation. Levi (2013) noted of Acemoğlu and Robinson:

On page 308, they write: "We saw how inclusive economic and political institutions emerge. But why do they persist over time?" This is a good question, but it rests on a problematic statement. They have shown us that inclusive economic and political institutions emerge, but not how they do. They have described the process and provided wonderful examples in which they emphasize political coalitions, interest groups, and other forms of mobilization, but they offer little in the way of a political analysis concerning how such collective actors come into being and enhance their power. The authors simply assume the existence of collective actors or portray a process of evolution over time as a consequence of small institutional advantages granted for other purposes than significant empowerment. (p. 189)

Historical Institutionalism

Historical institutionalists have similarly contradictory understandings of institutions. For one major body of work, institutions are *structures*—vast, enduring, and solid patterns of social organization at the level of the nation state, which are relatively stable over the long run, shaping more particular forms of political and social behavior. For others, they are *processes*—rules, procedures, or policies that change over time.

Historical institutionalism began with a different intent and mission—securing some space for the macrohistorical tradition of social inquiry, which was under threat both from quantitative social science, and from micro-oriented rational choice theories. Controversies between macrohistorical sociologists and political scientists and rational choice antagonists led to nervousness among young scholars in this tradition that they were in danger of extinction, leading them to coin the term *historical institutionalism* to describe an approach that would both focus on institutions, and ground them in processes of change (Steinmo, Thelen, & Longstreth, 1992).

This presented difficulties from the beginning. Macrosociological inquiry—as practiced by Theda Skocpol (1979), Tilly & Ardant, (1975), Stein Rokkan (Flora, Kuhnle, & Urwin, 1999), and others, was grounded in the role of structure—how different combinations of structural factors led to different combinations in different societies. It too, had begun in argument with an antagonist, but quite a different one: Marxism. Macrosociological approaches looked to disagree with Marxism by showing how other factors than the class struggle generated social structure.

However, other tendencies in the social sciences led these scholars to emphasize the potential for change. Thus, in Steinmo, Thelen, and Longstreth's (1992) initial introduction, the relationship between political strategies and institutional constraints was dynamic rather than fixed—actors used the opportunities that institutions provided them, but potentially changed those institutions as a result of those actions. In contrast to rational choice scholars, who tended either to see institutions as structures producing an equilibrium, or as that equilibrium itself, historical institutionalists thought of institutions in terms of processes of change, with no necessary end point.

In short then, historical institutionalists equivocated between two notions of what *history* was. One saw it as a nightmare from which we were struggling to awaken—or more prosaically, as a vast set of structural givens, which led to fixed but potentially very different outcomes in different societies, depending on which specific conjuncture of structural factors a given society had. The other saw history as a process, which was relatively open-ended, in which institutions did not squat on possibilities as stony near-immovables, but instead changed over time as they were worked on by the artful behavior of multiple actors, with the unexpected congregations of those actions leading to new institutions that presented new opportunities and new constraints in an endless dance.

Historical institutionalists were confronted with the challenge of arriving at theories that captured the relationship between structure and process in a more exacting way. The first systematic efforts looked to build on results from economics—but not the standard economics of game theory and equilibria. Instead, Pierson (2000) and his colleagues looked to mathematical work by the Irish economist Brian Arthur (1994), to come up with an account of institutional change based on the notion of *path dependence*. Arthur used so-called *Polya urn processes* to model change over time and to argue against his colleagues who insisted that actors with free choice would inevitably converge on efficient equilibria. Instead, Arthur compared the progress of markets and institutions to one in which individuals' current choices were dependent on their past ones in a self-reinforcing way. Social systems that were initially open to a variety of possibilities tended to converge rapidly on a single path, as the product of sometimes arbitrary initial decisions or interactions that led to self-reinforcing patterns. Thus, for example, patterns of product innovation built upon previous innovations, so innovators tended to get *locked in*, with actors using the same tools and becoming stuck on the same path of development, even when they would have been far better off had they chosen a different path initially.

For historical institutionalists, as for economic geographers (Grabher, 1993), path dependence appeared to offer an account of how history mattered. Paths of institutional change were tightly constrained by initial, sometimes arbitrary choices, just as, in the Polya urn processes that path dependence theory built upon, initial distributions of balls of one or the other color could lead to enduring and self-reinforcing patterns. Path dependence led to the prediction that institutional change would be a succession of *punctuated equilibria* (Gould & Eldredge, 1977; Krasner, 1982), in which long periods of stability (periods when people remained on a given path of institutional change) were interspersed with brief and sporadic moments when change was possible, perhaps because existing institutions had collapsed, providing actors with the possibility of moving towards a different path. These accounts provided a historically grounded account of institution-induced stability, allowing scholars potentially to examine how institutions could lead to continuity in policy, even under circumstances where one might otherwise have expected change.

However, for just that reason, path-dependence accounts had difficulty in explaining institutional change, which they tended to treat as the result of exogenous factors. Some institutions seemed capable of changing radically over time through processes of incremental change. Thelen (2004), for example, studied the vocational training system in Germany and other countries, and found extraordinary transformation happening over long periods of time, in which a system designed for one set of uses and external system became fully adapted to another, and yet another. Crucially, these processes of transformation were not sudden and sporadic—they were slow and incremental. This was at odds with the predictions of path dependence (which suggested that paths will quickly stabilize after an initial period of uncertainty). More broadly, path dependence offered no obvious theory of the mechanisms of institutional reproduction or change (Thelen, 1999), and, by concentrating on *critical junctures*, where anything could happen, emphasized exogenous change to the exclusion of any proper consideration of what paths actually involved

(Streeck & Thelen, 2005). This has prompted historical institutionalists increasingly to emphasize “gradual institutional transformations that add up to major historical discontinuities” (Streeck & Thelen, 2005, p. 8).

These disagreements have led to a new focus on mechanisms of institutional reproduction and change. Here, for example, Hacker’s (2004) explanation of changes in the U.S. welfare state posited four plausible strategies of reform—layering, conversion, drift, and revision—that might be adopted by opponents of the existing institutional status quo.¹ It has been particularly helpful in pointing to the ways in which institutions are continually contested in their application, and how this contestation may have long term consequences. However, as historical institutionalists have moved from considering institutions to examining how agents can change them, they have effectively excluded certain research trajectories. The emerging body of work, because it focuses on the role of agents and agent strategies in incrementally changing institutions, plausibly overstates the importance of incremental, as opposed to radical, change in shaping institutional outcomes (Schmidt, 2012).

Furthermore, these accounts tend to conflate actors’ strategies—that is, the specific approaches to institutional change given their specific situation—with mechanisms of change—that is, the broad social mechanisms through which one might expect to see transition from one institution to the next. In part, this is because historical institutionalists lack a good toolset for thinking about how strategies aggregate—so, for example, the efforts of actors to undermine an institution using one strategy interact with the actions of others (perhaps using different strategies towards the same or related ends), as well as with still others who are looking to defend a given institution (plausibly also via a variety of different strategies). Game theorists have their notion of an equilibrium—a situation in which no actor has any reason to change its strategy given the strategy of others—but historical institutionalism has no cognate concept to equilibrium, or competing concept either. This makes it hard to build from a theory of actors’ individual strategies as prompted by their situation to a theory of how and when institutional change will occur, and what kind of change it is likely to be.

Finally, these accounts have difficulties in explaining what it is that institutions do, and how they are separate from the presumably more evanescent actions that are shaped by institutions, such as policies. By moving from a theory of institutions as structures that lead to outcomes to a theory of institutions as outcomes of agents’

¹Streeck and Thelen (2005) describe five “modes of gradual but nonetheless transformative change” (p. 19)—layering, displacement, drift, conversion, and exhaustion. Most recently, Hacker, Thelen, and Pierson (2013) emphasize how drift and conversion can allow well situated actors to change policy without public scrutiny, while Mahoney and Thelen (2010) look to how different kinds of *change agents* can deploy strategies to reshape institutions. Hall and Thelen (2009) examine how institutions are continually contested by the agents applying them, with important consequences for institutional change. This new direction has surely allowed scholars to identify an important universe of new cases, which would have been invisible to researchers who assumed that large changes in institutional outcomes must be the consequences of abrupt and substantial disruptions.

strategies, the dominant approach to historical institutionalism risks failing to examine why it is that institutions are indeed consequential for political outcomes. To be clear—this is not a particular fault of historical institutionalism. Instead, it is a generic problem faced by all social science institutionalisms.

Sociological Institutionalism

Sociological institutionalism starts from the premise that institutions are organizing myths. This allows the approach to distinguish neatly between institutions and actual behavior, since the ways in which people act day to day are very often distinct from the myths through which our behaviors are legitimated. Equally, however, sociological institutionalism is the approach to institutionalism that has had the most difficulty in accommodating institutional change, in large part because of its origins in the work of Weber and Durkheim. Sociological institutionalists have typically been more interested in explaining continuity than change, and when they do address change they have typically seen it as involving propagation via isomorphism rather than transformation.

Sociological institutionalism is an offshoot of the classical sociology of the late nineteenth and early twentieth centuries. Like the great sociologists of that period—Durkheim, Simmel, Weber, its initial core focus was to explain modernity, and how it was that modern social practices reproduced themselves and spread across the world.

One key line of inquiry extends Weber's famous image of the *Iron Cage* of rationalization (Gerth & Mills, 2009). Weber depicted a world that was becoming increasingly rationalized, deflating the pneuma of prophecy, silencing the warring voices of different gods, and replacing them with a single set of imperatives based around bureaucratic and organizational rationality. Weber predicted that the result would be a more homogenous world, a prediction espoused by DiMaggio and Powell (1983) in a famous article in which they claimed that the world was continuing to become more homogenous, but not because of the mechanisms that Weber predicted. Instead, DiMaggio and Powell argued that rationalization was today being driven by *isomorphism*—the imperative for organizations to copy each other, converging on a similar set of procedures and approaches. Sometimes this isomorphism was coerced by more powerful actors, sometimes resulting merely from actors looking to copy others in an uncertain environment, and sometimes from normative pressures towards conformity.

These pressures led to worldwide convergence on an apparently similar set of institutional practices, as identified in the work of Meyer and his colleagues (Meyer, Boli, Thomas, & Ramirez, 1997), who built on Durkheim as much as Weber. Meyer and his collaborators sought to explain the lack of institutional variation across countries, as they opted to institute similar rules and organizations, despite their widely varying circumstances, adopting parliaments, ministries of education, and a host of other institutional elements. Meyer and Rowan (1977) noted that this

homogeneity coexisted with a wide variety of different behaviors, which were not caused or predicted by formal institutions. Instead, there was often an effective decoupling between the institutions that powerful actors within given states adopted, and the actual practices through which everyday life was organized.

In Meyer and Rowan's (1977) description, institutions served less as structural elements than as organizing myths. While DiMaggio and Powell (1983) saw institutional isomorphism as being in part driven by institutional efficiencies (rationalized institutions sometimes worked better, leading to their adoption in competitive circumstances, Meyer and Rowan stressed the extent to which institutions often would lead to inefficiencies if they were taken seriously. Thus, institutions became ceremonies to be performed as much as structures that shaped action.

This account went together with a considerable skepticism about the notion of the *actor* (Jepperson, 2002). Actors were constructed within the broader frameworks given by institutions and culture. The individual was not a pre-given, outside society; instead, she or he largely enacted the scripts that society gave her. So too, organizations and even states, which existed within what Meyer and his coauthors described as a common *world polity* (Meyer et al. 1997).

Some scholars within this account looked to establish the processes through which institutions came into being. Thus, for example, Dobbin (1994) looked to different political processes surrounding the state to explain why France, the United Kingdom, and the United States had such different understandings of railway markets in the nineteenth century. However, the processes of institutional change were in the background, briefly adverted to; what was in the foreground were the ways in which institutions made certain ways of thinking and enacting policy *natural*, with the effect that it was extraordinarily difficult to escape one's national style of policy making. This literature in general tends to treat institutions as cultural—that is, as being important not so much because they coerce or provide information, as because they shape people's understandings of themselves, of others, and of the appropriate relations between them.

In Clemens and Cook's (1999) description, this led to a strong (and even relentless) focus on institutions as *enduring constraint*, to the extent that the *capacity* of these “institutions to constrain political action and policy variation appear[ed] to marginalize the processes of conflict and innovation that are central to politics” (p. 442). Fligstein and McAdam (2012) noted that:

[sociological] institutional theory is really a theory of how conformity occurs in already existing fields. It lacks an underlying theory of how fields emerge or are transformed . . . Actors follow rules, either consciously by imitation or coercion or unconsciously by tacit agreement. (p. 28)

Stinchcombe (1997), meanwhile, caricatured the theory as “Durkheimian in the sense that collective representations manufacture themselves by opaque processes, are implemented by diffusion, are exterior and constraining without exterior people doing the creation or the constraining” (p. 2).

Again, different approaches within sociology have sought to react against this account in which institutions are seen as constraints rather than the product of

human agency. Clemens and Cook (1999) noted that institutions can be treated either as constraints or as guiding prescriptions and that the two may combine to explain durability. They pointed to how institutions may contain cultural components—schemas, or ways of thinking about the world, which may create the possibility for institutional change. In particular, they emphasized the importance of heterogeneity of viewpoints, network fragmentation, and contradiction between institutional rules in explaining the circumstances under which change is more or less likely.

Fligstein and McAdam (2012), for their part, focused on the important role of entrepreneurs in creating and reorganizing the *fields* that constitute the *rules of the game* in a given area of activity. Here, like latter day historical institutionalists, they focused on how there may be actors who are primarily concerned with maintaining a field the way it is, so-called *incumbents*, and actors who seek to disrupt the field and replace it with a new set of arrangements—so-called *challengers*. Actors with different endowments of resources (including social skill in identifying and forming possible coalitions) vie with each other for advantage.

However, these accounts too have had difficulty in reaching generalized lessons, in part because the theoretical concepts they invoked were very often situation specific. *Skilled social action*, *robust action*, and similar concepts describe something that is real and plausibly crucial in explaining which coalitions form and which do not, but they do not lend themselves easily to the formulation of testable propositions. Social skill only reveals itself partially and indirectly, and is primarily visible through its consequences.

A second set of difficulties for sociological institutionalism lies in demonstrating its effects. To the extent that cultures and rationalism have greater consequences for ritual invocation than for real behavior, their implications for real world behavior are uncertain. As Schneiberg and Clemens (2006) described the problem:

A second set of questions, asked only recently, appears if one looks beyond the moment of adoption of a “legitimate” policy or institutional structure to address the consequences of adoption . . . This question is often truncated by the invocation of “de-coupling,” but it is worth asking “what are the substantive implications of these institutional effects?” To the extent that standard research designs fail to address questions of the consequences of institutional diffusion, they are left open to the charge that institutional effects will be most pronounced in situations that are, among other things, “of relatively little consequence.” (p. 201)

Although Schneiberg and Clemens pointed out that a significant body of recent work in this approach had sought to identify important consequences, this literature still faces two important challenges. First, it does not do an especially good job at distinguishing the specific mechanisms through which institutions operate. In particular, it tends to treat any evidence for the influence of higher order institutions as being evidence of cultural effects, rather than looking to other plausible mechanisms through which institutions could have consequences. Second, because it over-emphasizes the extent to which institutions provide a structuring backdrop, it underestimates heterogeneity of viewpoints and the likelihood that people will have different perspectives on institutions, and indeed perhaps sharply different

understandings (or adhere to different institutions altogether). This not only means that sociological institutionalism's account of institutions themselves is too stylized, but that its account of the consequences of institutions is also over-totalizing. To the extent that people have different perspectives, institutions are more likely to be contested (potentially leading to institutional change) than sociological institutionalists surmise.

A Different Approach to Institutions

These various approaches to institutions started with different goals and have set out to analyze different phenomena, but end up in a quite similar place. Each of them has struggled to provide an account of institutions that shows (a) how institutions may be influenced by other factors and (b) how institutions can in turn influence behavior, without either reducing institutions to a mere transmission belt between external forces and human behaviors or treating institutions as coterminous with the behaviors they are trying to explain. Constructing explanations that tell us at once how institutions change and why they matter has proved to be extremely hard.

Thus, rational choice institutionalism began by arguing that institutions explained stability in situations of multidimensional choice or, alternatively, why it was that some countries prospered while others failed to grow. However, this led to the question of how institutions might change, which have been stymied in part by the difficulties of adapting a set of theories intended to explain stable equilibrium to discuss instead how things may change. Historical institutionalism similarly started from an emphasis on stability and structure, and as it has sought to explain change has found itself moving towards an imperfectly theorized mixture of mechanisms and individual action. Sociological institutionalism has been the most resistant to explaining change of all the major institutionalisms and has also tended sometimes to duck the question of institutional consequences as well, arguing instead that institutional rituals are often decoupled from what real people do. Each of these approaches faces similar conceptual problems.

In this section, borrowing from work in progress by Allen, Farrell, and Shalizi, I lay out an alternative way of thinking about institutions that may offer some clues as to a way forward. Specifically, an account of institutions that (a) stresses that institutions are built of *beliefs*, and (b) looks at how differences in individual beliefs may have consequences for institutional change may serve three useful goals. First—it can offer a clear account of how other factors than institutions may have consequences for institutions. Second, it can offer a clear account of how institutions have consequences. Third, it can do so while demonstrating that institutions are neither reducible to the forces that influence them nor to the behaviors that they influence.

Building on the work of Knight (1992) and North (1990), it is useful to think about institutions as rules, but also to consider exactly what social rules are made from. Specifically, as Knight outlines, a rule is an institution when it is known by

everyone in the community to be the appropriate rule for how parties should behave in a particular situation. What this implies is that institutions are rules that are instantiated in beliefs. In other words, an institution is only an institution because everyone in the relevant community of actors believes it to be an institution. Indeed, an institution has no existence that is independent of the beliefs that compose it.

Thinking about institutions in this way allows us to disaggregate these beliefs, following the arguments of Sperber (1996). Sperber is an anthropologist, who is interested in disaggregating notions such as culture. He pointed out that cultural beliefs—such as a belief in witches—are not shared in the unproblematic way that anthropologists sometimes argue they are. Even if everyone in a community believes in witches, each person's individual belief is slightly different from every other person's belief. While there may be enough rough congruence for social coordination, a culture is not a monolithic entity, but instead (at most) a congregation of roughly similar beliefs.

Similarly, institutions can be thought of as congregations of roughly similar beliefs about the specific rules that apply in particular circumstances. One might go further—under a materialist understanding, the rules have no existence whatsoever independent of the specific beliefs held by particular individuals about how they ought to apply. Individual beliefs about the rules will inevitably vary from person to person. Sometimes, there will be authoritative actors who can partly resolve these disparities. Judges can resolve disagreements over how formal institutions (laws) should be interpreted. Kadi-justice (in Weber's 1922/1978 account) can resolve some, but not all, disputes about less formal rules. Yet even so, under the best possible circumstances, there will be significant dissimilarities between different people's beliefs over the relevant institutions covering a particular situation.

What is valuable about this conceptualization of institutions? First, that it provides an understanding of institutions that is affected by external factors, which has consequences for human behavior, but that is not reducible to either. If institutions are congregations of roughly similar beliefs, it may be easy to see how external circumstances can affect them. Power disparities, the visibility of better solutions, or new ideas about how to organize society may each have powerful consequences for actors' beliefs about how a specific rule ought to be interpreted, and, indeed, for what the appropriate rule ought to be. Furthermore, the beliefs that people have about the appropriate rules in a relevant situation have obvious consequences for their actions, both because of their perceptions of how one ought to act in a given circumstance and because of their (possibly correct, possibly erroneous) assessments of how others will respond should they deviate from the rule.

Second, it identifies ways in which institutions can change that are not reducible to external circumstances, although they surely may be heavily influenced by them. Actors' beliefs about the appropriate rule will differ from actor to actor, leading to social friction (where actors find themselves in awkward situations thanks to different interpretations), social learning (when actors with different understandings of a rule can learn from each other), and social opportunism (when actors seek to push for interpretations of the relevant rules that advantage them, potentially disadvantaging others). These interactions are partly endogenous because they are part and

parcel of the workings of the institution itself—that is, they are in large part the result of the admixture of individuals' varying beliefs about what the institution in fact consists of. Institutions are rules that are made up of individual beliefs, and a very important aspect of institutional change is shaped by contact between the different beliefs that make up the institution, as individuals come into contact with each other in concrete social settings. Yet such processes of admixing are, obviously, potential sources of institutional change. Thus, one cannot treat institutions as being a simple condensate of other forces (power relations, efficiency considerations, social structure, or ritual requirements), since they may be impelled to change by forces (interactions among those in the community interpreting and applying the institution) that cannot readily be reduced to these external factors.

Finally, as well as providing an account of partially endogenous change, it points to a different set of external influences than those emphasized in the major accounts described above. If institutions are instantiated in beliefs, then the social structures through which beliefs are transmitted (changing in the process of transmission) are likely to play a very important role in shaping institutional outcomes. As Clemens and Cook (1999) have suggested, network theory provides one obvious source of insight into how these processes of social transmission might work and be shaped by social relations. Clemens and Cook also point to the role of heterogeneity of institutions—thinking about institutions as heterogeneous congregations of beliefs allows scholars to build heterogeneity into the foundations of our arguments about beliefs, exploring the ways in which variation in heterogeneity may lead to differences in the likelihood that new beliefs may spread across a given community.

This is certainly not the only way in which one might look to remedy some of the difficulties of social science institutionalism. However, it is one that may plausibly fit well with many of the concerns of scholars interested in spatial development. It points towards an account of institutions that does not waver between theories of institutional stability and theories of institutional change, but rather builds the possibility of innovation (a topic of great concern to economic geography) into the theory, by showing how it is likely to be influenced by the degree of heterogeneity and the relevant network structures of propagation and diffusion in a given society.

For example, one obvious implication of this approach is that we should see more rapid institutional change in circumstances where individuals with significantly differing beliefs about the institution come into frequent contact with each other (Allen et al., 2017). This provides some theoretical basis for understanding why some societies, such as Classical Athens, have seen rapid adaptation and learning, while others with similar power and resources have stagnated in relative terms (Allen et al., 2017; Ober, 2008). A second implication is that rough democracy—here conceived of as a general equality in the ability of actors with varying beliefs to affect institutional change—will plausibly result in more rapid and (over the long term) more socially beneficial institutional change than in situations where there are greater power disparities, with the interpretations of a narrow elite of actors with relatively similar understandings prevailing (Allen et al., 2017; Hong & Page, 2004).

These and other hypotheses may open the path to a new way of thinking about differing patterns of spatial development and how they relate to institutions. For example, they provide a practical linkage to Glückler, Lazega, and Hammer's (2017) argument for networks as an organizing metaphor, because it is through networks that beliefs diffuse and change, making it possible for different patterns of power relations and different patterns of exchange between actors with different understandings to be modeled using network percolation models and similar. If studies of economic development in specific regions and localities, and their relationship to international networks of knowledge diffusion began in discussions of *thickness* and the like, they may end up returning there, but with a very different and more specific set of intellectual tools for investigating how beliefs in fact spread and what consequences this has for institutional change.

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