Second Edition

Comparative Politics

RATIONALITY, CULTURE, AND STRUCTURE

Edited by

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INTRODUCTION – AND A FEW CAVEATS

The first edition of this volume featured a chapter on political economy by Peter A. Hall (Hall 1997). In it, Hall sought to define political economy by asking the following question: In the subset of those scholars who study the comparative politics of the advanced industrial states, how are political economy explanations constructed? Seen from this vantage, political economy, Hall answered, appears as a field defined by a specific set of concepts; interests, institutions, and ideas within comparative politics. Ten years later, I still find Hall's specification of the boundaries of the field, by reference to this troika of concepts, to be most useful for defining what political economy is and what it is not. However, in replicating Hall's analysis, two caveats are in order.

First of all, in following Hall's troika of "interests," "institutions," and "ideas" as defining political economy, I necessarily break with this volume's emphasis on "rationality," "culture," and "structure" as defining comparative politics. Why then do I prefer Hall's troika of boundary-setting concepts to the one offered by the editors? I do so since taking this route allows me to focus on interests rather than rationality as one of the three defining concepts of the subfield. This positioning is helpful, I suggest, insofar as while many political scientists see rational choice and political economy as synonymous (Weingast and Wittman 2006; Alt and Shepsle 1990), a focus on interests rather than rationality as a core concern allows me to place political economy in a broader frame that engages a more variegated set of literatures, particularly those on institutions and ideas, than would at first blush appear to constitute it. When one makes this distinction, political economy does not become coterminous with rationality, and hence with rational choice theory. Rational choice is masterfully surveyed in this volume by Margaret Levi, and indeed, much of the work she covers is and should be considered political economy. However, in terms of the framing of this chapter, if rational choice gets to "own" the concept of rationality, then much of the work I would wish to include here must be recoded as some kind of cultural or structural "other than political economy" approach. Rather than privilege any particular literature, Hall's framework allows us more freedom to broadly define and survey political economy.

Despite having just made the case for the usefulness of these concepts in defining the boundaries of political economy, my second caveat concerns how developments in the subfield over the past decade may have actually made sustaining these boundaries more difficult for two reasons. That is, while political economy is indeed still very much a way of doing comparative politics, one can argue that it has grown from its "reborn" beginnings in the 1970s to become a distinct "way of doing" political science in and of itself. For example, as well as scholars working with interests, institutions, and ideas developing their own research programs, we have also recently seen the decay of the boundary that traditionally set so-called international political economy apart from comparative political economy, increasing conceptual borrowing across the subfield, and the emergence of new approaches that go beyond the troika of interests, institutions, and ideas (Hobson and Seabrooke 2007; Langley 2004; MacKenzie 2006).

With those caveats in order, I first discuss what political economy is and how its modern form came about. Following this, I suggest that the modern troika of political economy perspectives just outlined can be read as extensions of three books published almost a quarter of a century ago: Peter A. Gourevitch's *Politics in Hard Times* (1986), Peter A. Hall's *Governing the Economy* (1986), and Peter J. Katzenstein's *Small States in World Markets* (1985). I link each of these books to one part of the troika; interests, institutions, and ideas, respectively, and show how the evolution of each of these perspectives can be understood as an extension of the core insights of each of these classic statements. In noting this increasing plurality of approaches, however, I assess two things. First, I return to the relationship of political economy, as it is bounded here, to rational choice theory. Second, I ask whether the plurality of approaches this broad view of political economy establishes is a problem for a putatively scientific field of study. Comparative politics, as a subfield, is sometimes criticized for its intellectual pluralism (Skocpol et al. 1995; Bates 1997b). I argue instead that as far as political economy is concerned, such pluralism is its signal strength. When one considers that the objects of political economy are open-ended evolutionary social and economic systems, the idea that a "theory of everything" can help us understand the world has surely proven fallacious (Blyth 2003a, 2007). As such, the increasing pluralism of political economy should be embraced rather than rejected.

Political Economy: What It Was and How It (Unexpectedly) Came About

The answers to these questions are not as obvious as is typically portrayed. What one might call the "standard story" is one where the birth of capitalism in Europe transforms property relations and patterns of distribution, and "political economy" thinkers theorize about it (Heilbroner 1953; cf. Halperin 2003). Reflecting upon these momentous changes, thinkers such as Smith, Malthus, Ricardo, and Marx explained these events by reference to factors as different as the division of labor, changes in population, the "average" level of profits, and the unfolding of class contradictions, respectively. Despite their ostensible differences, what these classical political economists agreed upon – and this is the correct part of the standard story – was the essential unity of the economic and the political as equal components in understanding the way the world works (Watson 2005).[1] However, such unity was not to last into the modern era, and here lies the less well-known part of the standard story.

Later political economists, such as Jevons, Walras, and Marshall, separated economics from political economy during their "marginalist revolution" by focusing on the moment of exchange rather than the process of value creation. But in making such a move, they did something else: They removed from political economy a concern with distribution – who gets what, when, where, and why – and hence any notion of politics and economics as mutually constitutive. In this reductionist moment, Pareto efficiency trumped political expediency, and "the politics" of political economy was evacuated. The result of this separation was that economics forgot history in the search for timeless generalizations about a field of action called "the economy," where politics appears only as a distortion of (or a distraction in) an otherwise self-regulating world (Hodgson 2001). Although there were some holdouts to this view, such as Veblen (1899) and Kalecki (1944), the early years of the Cold War effectively put paid to political economy, at least in the American academy. In part because it smacked of Marxism, but also because the move toward ever greater formalism further "depoliticized" the subject, economics ascended as a legitimate field of inquiry during a period in which the social sciences as a whole were politically suspect, and as a consequence, "political" economy dropped out of view (Samuelson 1997; Amadze 2003).

The (Unexpected) Rebirth of Political Economy

With economics ascendant and political economy out of fashion, it would take a shock to the system to bring it back in. That shock came in the 1970s, when the gloss came off the post–World II economic boom and the developed world underwent its first postwar recession (Lindberg and Maier 1985; Krugman 1994). Stagflation, oil shocks, unemployment, and low growth all dented the prestige of economics since economics, as a discipline, didn't see it coming, and economists as a group couldn't agree on what to do about it once it came (Bell and Kristol 1981). In this moment of crisis, many scholars began to argue that the separation of politics and markets that economics instantiated was perhaps part of the problem all along.

First out of the block were Marxist theorists, eager to denounce the latest dip in capitalist performance as "the end of times." James O'Connor (1974) diagnosed the postwar welfare state as being in a terminal fiscal crisis, while Jurgen Habermas (1975) identified a parallel "legitimation crisis" in which the overstressed mechanisms of state redistribution worked against efficient mechanisms

[1] One need only reflect that Adam Smith's *Meisterwerk* is called *The Wealth of Nations*, not *The Wealth of Individuals*, to see this.

of capitalist exploitation. In other parts of the academy, liberal theorists such as Charles Lindblom (1977) pondered the "privileged position" of business as a political actor in capitalist states.[2] Meanwhile, other scholars began to wonder if it wasn't democracy that was the problem, with politicians meshing their electoral cycle to the mechanisms of state intervention to produce inflation and low growth through a "political business cycle" (Nordhaus 1975; Lindbeck 1976; Buchanan and Wagner 1977).

Political scientists embraced these new ideas and pondered, for example, which states would survive the downturn better than others, and in doing so began to see the organization of the state itself as a critical variable in explaining economic outcomes (Katzenstein 1976; Krasner 1976). Building upon these insights, some scholars sought answers in analyzing how a post–Bretton Woods financial environment would impact states (Block 1977; Strange 1970). Others worried more specifically about the United States in its role as world hegemon and provider of "global public goods" (Kindleberger 1973). From this angle, an entire body of work concerned with the stability of the global economy and the role of the United States therein, a distinct international political economy (IPE) began to take shape.

Indeed, by the mid-1980s, political economy, in a multiplicity of forms, was back. It was back in part because the excision of the political from the economic that economics as a field relied upon had seemingly failed. It was back in part because with states around the world appropriating, taxing, and spending between 30 and 50 percent of gross domestic product (GDP), the idea of an "economy" that ran by its own transcendental laws apart from politics became an increasingly questionable assertion. Most importantly, however, it came back because hard-won empirical research showed that the economy was inseparable from politics. Modern political economy showed that if one wanted to understand significant variations in economic outcomes, then embracing the mutual imbrications of states and markets was a pretty good place to start.

INTERESTS, INSTITUTIONS, AND IDEAS AS EXPLANATORY ALTERNATIVES

Concepts and Questions

Despite their desire to place markets and states in the same equation, what this new generation of political economists had in common were more questions than answers. The question some scholars asked was "Qui bono?" – who benefits? (Gourevitch 1986; Strange 1988). This question privileges the concept of "interests" by forcing the analyst to ask, "In whose benefit would it be for outcome X to pertain over outcome Y?" Doing so, in turn, leads the analyst to ask how people come to want what they want, and then try to link those wants to specific outcomes worth explaining. Given this desire to link intentions and outcomes, interest-based arguments tend to be underpinned by a materialist

[2] Lindblom was by training an economist, not a political scientist.

theory of action, based upon the not unreasonable notion that where one sits economically may guide one's preferences politically.[3] Actors' class positions, what assets they have, how fungible those assets are, how exposure to particular economic shocks impacts agents' resource portfolios: these variables become empirical grist to the theoretical mill of interest-based accounts (Block 1977; Frieden 1991b; Rogowski 1989; Swenson 2002).

A second group of theorists asked another question: "Who varies and why?" Here institutions rather than interests come to the fore. One influential definition of institutions views them as "the formal rules, compliance procedures, and standard operating practices that structure the relationship between individuals in various units of the polity and the economy" (Hall 1986: 19). Another defines institutions, more broadly, as "humanly devised constraints that shape human interaction" (North 1990: 3). Basically, institutionalists want to know whether agents act according to their materially derived interests or because of the institutional context in which they find themselves. Institutional explanations focus our attention on how economies are organized and how such configurations impact agents' interests. Seen in this way, institutions link larger economic-structural changes and interests but become causally important in their own right. There are, however, two versions of institutionalism that have evolved from this common position that institutions matter.

The first version, as Hall's definition suggests, sees institutions as historically specific and ontologically prior to the agents who occupy them. Such institutions *structure agents' choices* (Hall 1986; Steinmo et al. 1992). North's quote, in contrast, suggests a different but equally relevant way of viewing institutions. Rather than institutions structuring choices, humans are seen to design institutions to achieve their goals given their preexisting material interests. Whether to overcome collective action problems or reap gains in trade, *institutions are chosen structures* (North 1990). In either case, while interests are important, it is how they are refracted through institutions that is the explanatory *causa prima*.

Building on these prior positions, a third group of political economists asked another question: "Who constructs?" Given that many of the causes that generate outcomes in the political economy are highly complex and not directly observable, some theorists began to attend to the social constructions agents use to decode and navigate the political economy, seeing those constructions as causally important in and of themselves (Blyth 2002; Parsons 2003). Take globalization, for example. Standard materialist accounts of globalization's effects on politics tend to focus on how particular actors are impacted by, for example, greater integration into financial markets (Frieden 1991b, 2005), labor markets (Rodrik 1997; Iversen 2005), or product markets (Keohane and Milner 1996b). In contrast, scholars who attend to the social construction of globalization ask instead how the multiple causes of the global economy come to be known as having such specific and unusually linear outcomes in the first place. They point out that what globalization "is" is itself constructed differentially across nations. In the United Kingdom

[3] Hence the common locution, "an agent's material interests." See Blyth (2003).

and the United States, globalization is portrayed as an imperative to be embraced. In France and Germany, it is seen instead as a political choice that, when embraced, undermines national conceptions of welfare (Hay and Rosamond 2002; Schmidt 2002). Such scholarship points to something important: How particular constructions of the political economy agents develop and deploy helps bring into being that which is described, rather than simply describing an already existing state of affairs (MacKenzie 2006). That is, how agents think about, and hence act, in the political economy is causally important.

Clearly, such a variety of perspectives invites a plurality of methodological stances. Rather than adjudicate the one "true" version of political economy or suggest that a single approach actually covers all the bases, in the next section I show how these three strands of modern political economy can be usefully understood as the outgrowth of three particular works that all sought to explain the same thing: the economic policy choices of states. These texts can be seen as the springboards for our troika of approaches. Following this discussion, I address the relationship of this version of "what political economy is" to rational choice theory, a body of work that not only has a strong lineage *in* political economy but sometimes aspires to define itself *as* political economy.

Interest-Based Political Economy: Origins

Gourevitch's *Politics in Hard Times* (1986) is in many ways the touchstone for contemporary interest-based explanations in political economy. For Gourevitch, interests are the primary explanatory concept, and in this regard he is thoroughly materialist. As he puts it, "what people want depends on where they sit" (Gourevitch 1986: 56). However, although Gourevitch seeks to reduce politics to the materially derived preferences of social actors, the way he does this shows an affinity with other approaches that later works in this tradition eschew.

Gourevitch explains how changes in the international economy impact a state's "production profile" – the configuration of its economic sectors – and alters the preferences of domestic actors, given their assets and resources within such sectors. For Gourevitch, changes in the global economy create moments of political crisis that alter agents' preferences, and into this breech enter politicians who act as "brokers" who attempt to forge new coalitions of common interest. These coalitions then seek electoral or other forms of power in order to advance public policies to benefit themselves, rather than other contending coalitions whose assets are differentially affected by the same changes (Gourevitch 1986: 55–60, 32–34). Examining the economic crises of the 1870s, 1930s, and 1970s, Gourevitch shows how these variables and causal mechanisms produce political coalitions that vary as his theory predicts.

What makes *Politics in Hard Times* particularly interesting, however, is how Gourevitch's key category of material interest is actually bound up with, and only understandable through, a host of secondary variables. Specifically, Gourevitch notes how exogenous economic changes rarely, if ever, telegraph into agents' heads "what has gone wrong" and "what should be done." As such, political parties, the institutional configuration of the state, the economic ideas mobilized by agents, and even "a country's placement in the international state system of political-military rivalries" all come into play (Gourevitch 1986: 21). Indeed, one of the most striking findings of Gourevitch's book was that as one moved temporally from one crisis to the next, the role of such secondary factors became *more* important in explaining outcomes over time (Gourevitch 1986: 227–228). Yet, despite this, the take-home message was that to explain states' policy choices, materially derived preferences, political brokerage, and coalitional politics were what should be attended to.

Institutional Political Economy: Origins

A contemporaneous text that sought to explain state policy choices by reference to institutions rather than interests was Peter A. Hall's *Governing the Economy* (1986). Hall's puzzle was that if Britain had been in relative economic decline since 1913, and if agents' interests were in the driving seat, then why did none of these agents seem to have an interest in arresting this decline? Or even if they did, why were they unable to do so? For if political parties are brokers of broad social coalitions, then Britain has seen an alteration in governing parties and coalitions but little alteration in economic policies (Hall 1986: 26–37). Hall's answer was to see agents' interests as mediated by their institutional position rather than being telegraphed straight from their structural location.

In brief, Britain was the first industrializer, an imperial power, and a financial center, all of which led to an economy that was an agglomeration of small firms with fractious labor–management relations, small banks, and externally oriented capital flows (Hall 1986: 37–47). While this set of institutions worked well in the nineteenth century, once other countries caught up and surpassed the United Kingdom in the early twentieth century, these institutions became increasingly dysfunctional. However, since such institutions served as the context in which policymakers' choices were made, *they structured agents' choices rather than being themselves objects of choice*; key actors' interests became a derivative function of their institutional rather than their material position. In short, British politicians' efforts at reform were thwarted by their particular institutions. Like Gourevitch, however, Hall also relies upon other secondary variables as part of his explanation. After all, how do institutions structure choices? Hall says that they do so by processes of social learning such that "off-the-path" policy thinking, and hence radical policy choices, remain off the table (Hall 1986: 233, 277). Institutions may be granted analytic primacy, but Hall sees them as acting on and through economic ideas, political parties, and even the mass media (Hall 1986: 277–280).

Constructivist Political Economy: Origins

A third major contribution was Peter J. Katzenstein's *Small States in World Markets* (1985). Katzenstein sought to explain why, in the aftermath of the economic shocks of the 1970s, it was the smaller and more vulnerable European economies that seemed to better weather the economic storm. What makes *Small States* particularly important was that it embedded one explanation (a materialist-coalitional one) in another (institutional) that, in turn, opened the door to other approaches (ideational) that attempt to transcend both. Katzenstein's puzzle was that while large economies adjusted to external shocks either through the market (via wages and prices) or through the state (via policy interventions), given their relative openness to the international economy, small states had to find other, less destructive ways to adjust (Katzenstein 1985: 24–27). That alternative was flexible adjustment through domestic institutions, specifically corporatist institutions, where peak organizations and the state shared the costs of adjustment among encompassing coalitions of social partners (Katzenstein 1985: 30–38).

On the one hand, *Small States* was a straightforward materialist IPE story.[4] Given their degree of openness and exposure to the international economy, these smaller European states, through processes of domestic bargaining, developed two mutually supporting sets of institutions: corporatist intermediation and welfare compensation. This occurred as exogenous economic shocks altered agents' interests such that coalitions favoring compensation won the day and built institutions to serve their interests. But on the other hand, this Gourevitchian story was a bit too neat and functionalist for Katzenstein. After all, why compromise? Why not conflict as different coalitions push the costs of adjustment onto one another, as happened in the larger European states in the 1930s?

Herein lies the deeper *institutional* version of events that actually drives the argument as a whole. The lack of a strong feudal past and the consequent weakness of the landed aristocracy, as far back as the Late Middle Ages, sets the scene for compromise (Katzenstein 1985: 159–160). Given this *longue durée*, evolving links between industrial and agricultural sectors, the emergence of proportional representation, and late industrialization all combined to favor compromise rather than conflict among business and labor groups by the time we get to the critical period of the 1930s (Katzenstein 1985: 171, 174). It is this domestic long-run historical and institutional development that drives the ostensible materialist-coalitional story. Each alone is insufficient as an explanation, but together they have more explanatory reach.

Yet, *Small States* also suggests a move beyond interests and institutions as explanatory categories, noting, for example, that the "first trait" of such economies was "an ideology of social partnership" (Katzenstein 1985: 32). Motivating such patterns of compromise was

> the perception of vulnerability . . . which . . . generated an ideology of social partnership . . . [and] . . . acted like a glue for the corporatist politics of the small European states. . . . Yet none of the reviews of the book published after it appeared paid any attention to it. Why? A decade before the constructivist turn in security studies and international relations, scholars of comparative and international political economy simply did not know what to do with ideology as an explanatory construct. (Katzenstein 2003: 11)

[4] Especially if one reads chapters 1, 2, and 5.

This is why *Small States* is more than a synthesis of coalitional and institutional approaches. It also opened the door to moving beyond them, to put ideas and social constructions front and center in political economy explanations rather than treat them as a residual category to mop up variance unexplained by these other approaches (Blyth 1997).

EXTENSIONS OF INTEREST-BASED POLITICAL ECONOMY

Trade, Mobility, and Politics

Taking *Politics in Hard Times* as our starting point, we can trace the development and refinement of interest-based approaches in political economy.[5] Eschewing the richness of Gourevitch's analysis in favor of parsimony and predictive power, one strand of work that emerges from this focus on the effects of the international economy on domestic politics is Rogowski's work on shifts in trade patterns and their effect on domestic political alignments (Rogowski 1989). Using Hecksher–Ohlin/Stolper–Samuelson models of trade to determine who benefits and who loses under protection and free trade, Rogowski predicts the effects of rising and declining trade on capital-rich and capital-poor economies with different land/labor endowments (Rogowski 1989: 16). Rising and declining trade are seen to have differential effects on relatively abundant and scarce factors, respectively, with different combinations of factors promoting different social cleavages, and from there divergent coalitional outcomes ranging from the U.S. New Deal to Asian fascism.

An important extension of this logic is provided by Hiscox (2002). While Gourevitch (1986) focuses on narrow sector-specific interests and the coalitions they generate, Rogowski (1989) focuses on broader class- and factor-based coalitions; Hiscox wondered if they might both be right. Might it be the case that sometimes we see trade politics played out on a broad class-based canvas and at other times in narrow sectoral politics, and if so, why? Hiscox links both bodies of work together by focusing on the degree of interindustry factor mobility, and in doing so he expands the reach of both theories. His argument is simple and elegant.

If factors are mobile between industries, we can predict broad-based class politics. If they are specific and immobile, we can predict narrow sectoral conflict. In other words, "class conflict is more likely when levels of factor mobility are high . . . industry-based conflict is more likely when levels of mobility are relatively low" (Hiscox 2002: 5). The logic is that in the early stages of industrialization factor mobility is high since skills are basic and transferable, technology is simple, and transportation innovations have large effects. As such, both capitalists and workers have broadly similar, albeit orthogonal, interests. More developed political economies, however, produce "more specific forms of human

[5] Rather than briefly survey a large number of works in each of these three traditions, as other authors in this volume have done, I have decided to focus on a few critical exemplars in each tradition.

and physical capital and far greater complementarity between technology and labor skills" (Hiscox 2002: 11). This, in turn, implies that workers' and capitalists' interests become more cross-cutting, giving rise to new nonclass or factor based coalitions. Using this framework, Hiscox is not only able to show how and why forms of trade politics vary, but also offers insight into important questions of economic history and economic policy.

Assets, Skills, and Compensation

Another significant contribution to this literature that uses a similar "interests and assets" framework is made by scholars who highlight the importance of skills in the political economy. Insofar as the skills held by labor and sought by employers are valuable assets, focusing on skills might shed light on, for example, the highly varied forms that welfare states and labor markets take around the world. Although more obviously linked to the varieties of capitalism literature in political science (Thelen 2004; Hall and Soskice 2001), this literature also owes its existence to the focus on sectors, assets, mobility, and exogenous changes in the world economy that lie at the heart of Gourevitch's concerns.

Pioneers of this literature are Mares (2003) and Iversen (2005). Mares takes as her point of departure the observation that in explaining the development of social policies, it is usually assumed that workers and employers have orthogonal interests (Korpi 1978; Esping-Andersen 1985). But given employers' need for skills and labor's need to acquire them, might it not be possible that Gourevitchian coalitions of interest between different employers and workers across sectors could have played a role here (Mares 2003: 2–9)? Conceiving of skills as mutual investments in specific assets by both employers and workers, Mares is able to view social policies, and indeed entire regimes of social protection, in a new light. Rather than view such outcomes as something "won or lost" by labor in its contest with capital, Mares views social policies as employer-produced devices to encourage workers to invest in skills and thereby insure against specific production risks. Mares's logic is powerful and compelling.

If the marginal productivity theory of wages is correct, then returns to skill account for the differential in wages observed across an industry. As such, it is reasonable for workers to learn skills and earn more returns to their factor. Skills in this sense become tradable assets. The problem is, of course, that learning skills is a risky business for both workers and employers. From the worker's point of view, not only is it costly to train, but the more skilled you are, the more redundant (substitutable) one may become if technologies change or terms of trade turn against you (Iversen 2005: 3). Yet, from the employer's point of view, if you spend a lot of money training workers and they can easily move to a rival firm, why bother doing so in the first place? But you still need skilled workers.

Mares argues that in order to convince workers to share some of the costs of training, the possibility of nonemployment due to their skill set has to be factored into workers' estimations of their future income streams in making their decision to undertake training (Mares 2003: 24). Social policies are best seen, then, as a subsidy paid by either the state or the employer (itself a function of the degree of risk an employer faces) to support the reservation wage of skilled workers such that the returns to their skills (assets) do not fall below the market-clearing rate of the unskilled when they are unemployed. For if they did, why would anyone bother to learn a trade? Variations in employment protection can then be understood as employers' responses to trading off heightened production risk against securing adequate skilled labor via mechanisms of internal firm or external state control. Social policies can then, in the aggregate, benefit employers as well as workers, which is why employers help set them up. Meanwhile, the coalitions of workers and capitalists that promote such policies can then be deduced given the nature of assets, risks, and skills in a given economy (Mares 2003: 21–63).

Taking this logic further is Iversen (2005).[6] Building on Mares's analysis, Iversen argues for a micro-level "asset theory of the welfare state" (2005: 11). Like Hiscox, Iversen seeks to unite and extend two interest-based theories. The first is Mares's foil, the "power-resources" school of welfare state development, which sees labor and labor-backed parties as pushing business into producing welfare policies against their will (Korpi 1978; Esping Andersen 1985). The latter is Mares's own approach, in which employers actually set up the welfare state (Mares 2003). Key in reconciling these positions is to see the former's focus on redistribution and the latter's focus on insurance as two sides of the same coin rather than as opposing positions. Making this move allows Iversen to "understand how popular preferences for social insurance and redistribution are rooted in people's position in the economy, how these preferences are aggregated into social policies, and how these policies in turn affect individual investments into assets that shape economic performance and interests" (Iversen 2005: 13).

Drawing on the varieties of capitalism literature (Hall and Soskice 2001), Iversen posits that economic systems that rely on general skills (typically, the Anglo-American model) create wage inequality and poverty since they "limit . . . incentives for skill acquisition at the low end of the academic ability distribution" (Iversen 2005: 18). Such systems specialize in low-skill manufacturing and increasingly in services, areas in which productivity enhancement through skills is limited. Consequently, few such skills are supplied, and over time the wages of those at the bottom end of the distribution fall. However, despite their position in the labor market, such workers may not prefer higher taxes and more redistribution since "without an investment in distributive insurance, investment in general skills is the best defense against adverse changes in the labor market" (Iversen 2005: 24). In contrast, where the economic system relies upon more specific skills (typically, in continental Europe), training regimes and compensation via insurance come to the fore. Given this alternative set of institutional complementarities, workers demand greater welfare transfers and pay more taxes to shoulder the costs of becoming, in effect, "specific assets." Just as Hiscox unites Rogowski and Gourevitch through a focus on mobility, Iversen unites

6 Iversen's piece is an extension of his work with David Soskice. See Iversen and Soskice (2001).

rival views of the origins of the welfare state through a focus on insurance and redistribution as complementary products and as the basis of political coalitions.

Interest-Based Theories: Strengths and Weaknesses

Taken together, this body of literature has several strengths. First of all, it links macro-level changes in the economy to a set of plausible microfoundations grounded in agents' material environments; there are no explanatory "leaps of faith" from one level of analysis to another. Second, such approaches are parsimonious and predictive. Clear and testable propositions can be deduced from them. Third, they put the politics back into political economy by putting the question of distribution front and center. These approaches take seriously the question "Who benefits?" They do, however, have some characteristic limitations, perhaps the most salient of which is how the powerful beam of illumination such theories produce has over time become perhaps somewhat distorting of the historical record.

Take Rogowski's description of mid-nineteenth-century Britain as an alliance of abundant factors (capital and labor) against the scarce factor (land) (Rogowski 1989: 10). While this has intuitive plausibility, it also sidesteps the following question: To what extent, in a predemocratic United Kingdom, where political action by labor was met with imprisonment and exile to the colonies under the Transportation Acts, could a "political coalition" in any meaningful sense have actually existed? Similarly, Hiscox's focus on the effects of factor mobility leads him to impute to the authors of the postwar Swedish model of capitalism, Gøsta Rehn and Rudolph Meidner, a desire to promote intraindustry factor mobility as a way of limiting rent-seeking (Hiscox 2002: 6, 162–163). Again, this is a logical consequence of the model, but it is hard to find direct evidence that Rehn and Meidner ever thought this way. In fact, what they did say shows a desire to use mobility to enhance productivity while squeezing profits, which is an entirely different rationale (Meidner 1980; Blyth 2002).

These reinterpretations of history become even more pronounced in the work of Mares and Iversen. Here the works of generations of labor historians, and quite a few political scientists, are thrown into question in what is a rather apolitical reading of events where strikes, lockouts, shootings, revolts, Communist Party agitations, socialist Sunday schools, and hunger marches all become quite puzzling. After all, employers apparently *wanted* to give workers a welfare state, so what was all the trouble about? This is a rather bold position that may risk sacrificing historical accuracy for theoretical fit by reducing violent political struggles where interests were opposed to a coordination game where labor activists and social scientists, each lacking the correct theory, somehow misread business' intentions. Countering such claims, Mares notes that this scholarship argues that "the recognition that employers have been active participants in the creation of the welfare states does not imply that . . . workers have been passive by-standers . . . [n]or does it imply that employers have been agenda-setters in welfare state reform". . . [because] . . . "it is often difficult to distinguish empirically among [actors] underlying preferences and strategic motives" (Mares this volume). Yet, if one cannot make such distinctions, can one really link actors' intentions to outcomes via their material interests, as this literature presumes?

For example, during the 1920s, Belgian capitalists insisted on including transportation subsidies in welfare packages. In this sense, they helped build the welfare state. But the question remains: Why did they do so? Under this framework, one could argue that they did so to solve a coordination problem – getting labor to work – but one could equally argue that they wanted such subsidies so that workers did not live near their factories and therefore could not organize (Liebman 1979).[7] So, did employers build the welfare state to secure skills or demobilize labor? It is perhaps worth recalling that the labor power thesis that this "skills-based" explanation of welfare institutions is often juxtaposed to is a bit of a straw man. In the original power-resources literature that this school juxtaposes itself to, no one ever called Saltsjöbaden "the historic victory"; it was always "the historic *compromise*" – a moment when capital *compromised* because labor had the whip hand (Blyth 2002; Hacker and Pierson 2002; Korpi 1978). Reducing all politics to a series of coordination problems and incentives might fit the model, but it might also, inadvertently rob interest-based political economy of its politics.[8]

DEVELOPMENTS IN INSTITUTIONAL POLITICAL ECONOMY

Chosen Structures That Structure Choices?

As argued previously, what differentiates an institutional from an interest-based explanation is the contention that *institutions structure choices* and are prior to interests. However, as also noted previously, there is a parallel reading of institutions that views them as *chosen structures* designed to reap gains in trade and/or solve collective action problems (North 1990). In examining extensions of this body of scholarship, what stands out is how later institutionalist work has attempted to synthesize both sides of this literature. This desire to do so is seen most clearly in recent game-theoretic extensions of institutionalist analysis (Hall and Soskice 2001; Greif 2006).

Parallelling work by Iversen et al. (1999), Hall and Soskice (2001) develop a micro-level explanation of the continuing institutional diversity found among the advanced industrial economies despite globalization's supposed "regression to the institutional mean." This "varieties of capitalism" approach takes firms to be the primary agents of institutional construction and focuses on their strategic interaction in game-theoretic terms. Firms, seen as actors in a series of infinitely sequenced games, are both the product of and, more importantly, the producer of specific institutions. These institutions come into being and evolve over

[7] I thank Mimi Keck for this example.

[8] One could also argue that the model is an "as if" analogical/predictive, rather than an "as is" empirical model, and hence the preceding critique is redundant. But in response, one could counter, why put weight on archival evidence of what employers actually thought and did if the exercise is "as if"? See Friedman (1953) for the relevant distinctions.

time as a function of nationally based firms' attempts to find solutions to five coordination problems that jointly determine the particular variety of capitalism that a country has (Hall and Soskice 2001: 9–21).[9] Over time, such strategic interactions among firms pursuing their interests produce self-reinforcing institutional complementarities via feedback loops and increasing returns, which in turn produce two distinct regime clusters, Liberal Market Economies (LMEs) and Coordinated Market Economies (CMEs).

In this version of institutional theory, the weighting is very much on firms' choices creating institutions, which then serve as the context of subsequent choices. But note that those institutions are causally powerful only to the extent that these firms find the institutional complementarities created by them to be useful (Blyth 2003b; Howell 2003). In this sense, firms' interests are conditioned by and endogenous to the institutional content, but the origins and maintenance of these institutions are exogenous properties of firm's prior strategic interests. Therefore, while the varieties of capitalism literature represents a progressive extension of institutionalist theory, it does so by cleaving to what is, at base, a less "institutionalist" understanding of institutions.

Also working within a game-theoretic tradition is Greif (2006). Greif, however, uses game theory to move institutionalist political economy in another direction entirely. While viewing institutions as "chosen structures" that "structure choices," for Greif the weighting is the other way around. For Greif, institutions are no mere context or instrument. Rather, "institutions are the engine of history" (Greif 2006: 399). Greif's empirical purpose is to explain the role of institutions in producing the late medieval economic expansion, and to analyze how different institutions led to different outcomes among European and Islamic communities (Greif 2006: 25, 252–255, 300–301).

Greif begins by attacking the chosen structures/structuring choices dichotomy. As he puts it, "[t]hese two seemingly contradictory views on institutions . . . must be bridged because each captures an important feature of reality" (Greif 2006: 41). He aims to do so by rethinking what an institution is and to then embed this in a game-theoretic framework (Greif 2006: 14–28, 35–39). Greif views institutions as "elements" that can be creatively recombined by agents rather than as objects cut from whole cloth. As he puts it, "[t]hese institutional elements are exogenous to each individual whose behavior they influence. They provide individuals with the cognitive, coordinative, normative, and informational micro-foundations . . . [that] . . . motivate them" (Greif 2006: 14).[10] Key

[9] Those coordination problems are bargaining between business and labor over wages and working conditions, securing suitable skills, gaining access to finance, issues of interfirm cooperation and competition, and adverse selection in employees. Institutions here certainly structure firms' choices, but they are first and foremost the product of those choices. This marks a departure from Hall's earlier work. As Hall and Soskice note, "[h]ere we depart from our own previous formulations as well" (Hall and Soskice 2001: 12, fn 11).

[10] His other definition of institutions stresses the same elements where institutions are "man-made non-physical factors that generate regularities of behavior while being exogenous to each individual whose behavior is influenced" (Greif 2006: 21).

here is the stress on motivation and cognition, or, as he puts it, "cultural beliefs . . . influence the selection of institutions," since the "motivation provided by beliefs and norms . . . is the linchpin of institutions" (Greif 2006: 28, 45). Viewing institutions as a compound of beliefs and motivations leads Greif to explain variation in the practices of Christian and Muslim traders as a function of their particularistic beliefs and the institutions such beliefs made possible. In brief, individualism and weak kin ties brought about the corporation and growth in Europe; strong kin ties and collectivist beliefs, in contrast, brought about in-group sanctioning, a mistrust of difference, and restricted trade networks in the Arab world (Greif 2006: 25, 253, 300–301, 389–400).

In contrast to Hall and Soskice (2001), where game theory is largely used metaphorically, Greif actually uses game theory, but his employment of it is rather unusual. Greif is well aware of the standard criticisms of game-theoretic models as being too determinate if perfect information is assumed and completely indeterminate if information is less than complete (McKelvey 1976). As such, its relevance for "the games real actors play" is often questioned (Scharpf 1997; Munck 2001). Greif accepts these criticisms and then uses institutions to rehabilitate game theory. Accepting that the assumptions underlying game theory are completely unrealistic, Greif argues that institutions "compress" information into rules and norms that allow agents to act as if they were the fully informed agents of game theory (Greif 2006: 126, 138).[11] As such, Greif is able to give a game-theoretic *institutional* account of institutional stability and change, growth and decline, where material interests pale before beliefs and ideas.

Non-Game-Theoretic Extensions: The Developmental State Literature

A final body of institutional scholarship worth examining is the literature on the developmental state in East Asia. This scholarship took an entirely different path from the previously discussed works and developed a form of institutional analysis that is much more classically "historical institutionalist." That is, this literature is more obviously related to works such as Katzenstein's *Small States* (1985) and Hall's *Governing the Economy* (1986) since it deals with institutions as contingent historical products that structure choices, but it does so from the basis of the inductive observation of particular cases.

Deyo's *The Political Economy of the New Asian Industrialism* (1987) was the first major statement of this approach. Deyo and his contributors sought to explain why East Asian industrialization succeeded while most developing states' industrialization efforts failed. In explaining this outcome, Deyo et al. highlighted the role of the state in industrial development and, like Hall (1986), stressed the importance of institutional linkages. Specifically, the volume highlighted how institutions

[11] Note, Greif does not model "as if" agents were fully informed, but assumes that agents can actually act as if they were fully informed given the informational proxies that are institutions and norms. This turns Friedman's famous dictum about the realism of assumptions inside out (Friedman 1953).

that promoted savings for investment capital, insulated state bureaucracies, and facilitated credit controls were unique parts of the East Asian experience.

Three contributions built upon this opening: Haggard (1990), Wade (1990), and Amsden (1989). Haggard (1990) examined why Brazil, Mexico, Taiwan, Korea, Hong Kong, and Singapore all began their industrialization drives with a strategy of import substitution industrialization (ISI), yet only the East Asian states managed to change to a more sustainable export-led growth (ELG) strategy. Haggard's answer to this puzzle invokes factors as diverse as egalitarian land reforms and the size of the domestic market, but what really drives his argument is state autonomy and, crucially, the *institutions* that make coherent long-term industrial policy over the heads of the powerful possible (Haggard 1990: 23–48, 261–264).[12] This is why, according to Haggard, despite having similar initial conditions and strategies, East Asia flourished while Latin America stagnated.

While Haggard situated critical domestic institutions within a global context, Wade (1990) and Amsden (1989) grappled with the details of East Asian industrialization to see how rather than "governing the economy," East Asian states sought instead to "govern the market." For both of these authors, how the state organized, rationed, and authorized inputs was crucial in explaining their success. At base, rather than "getting the prices right" via wholesale liberalization, these authors show how East Asian states actively sought to get the prices wrong in order to direct capital into specific sectors to overcome the problem of the small size of their domestic markets and move up the value chain via ELG (Wade 1990: 108–112, 157–158, 191–194). Such studies had a clear purpose: to explain how these states succeeded while others failed, with the answer being "the right institutions." But such an answer simply begged another question: What made those institutions the right institutions?

Key in addressing this issue was Evans (1995), who sought to explain variations in the growth and development of the information technology sectors of Brazil, India, and Korea. Creating two ideal types of predatory and developmental states, both of which are interventionist but only one devolves into chronic rent-seeking, Evans argues that what matters is how the state simultaneously penetrates and yet remains uncaptured by society. Key in doing this is a specific type of bureaucracy, one that is meritocratic and efficient. Evans argues that developmental states with such "Weberian" bureaucracies exhibit a quality he calls "embedded autonomy," where "highly selective meritocratic recruitment creates . . . corporate coherence" inside the state, which facilitates the degree of autonomy necessary to formulate longer-term developmental projects. However, this autonomy is "embedded in a concrete set of social ties that [allows for] . . . continual negotiation" by societal actors. As he puts it, "only when embeddedness and autonomy are joined can a state become developmental" (Evans 1995: 12). To make this explanation more than tautologous, Evans brings

[12] As Haggard puts it, "a critical test of the institutionalist perspective is whether state structures can insulate political elites from the demands of the powerful. This is what happened in . . . Korea, Taiwan, and Singapore" (Haggard 1990: 264).

institutions to the fore, arguing that institutions give content to interests since they "have a reality that is prior to 'individual interests'" (Evans 1996: 28). As such, institutions, historically contingent and hard to replicate, once again appear as the source of growth and development.

Extensions to this literature are voluminous and form a critical part of contemporary political economy approaches to development. Some scholars have moved away from the notion of autonomous and enlightened elites maximizing pigovian welfare functions to stress how mutual hostage taking in different parts of the state explains variations in development (Kang 2002). Others have examined cases of failure and found that even the best Weberian bureaucracies can be thwarted by noncooperation by capitalists (Chibber 2003), while some are indirectly aided by an ideology of isolation (Woo-Cummings 1999). Regardless, this literature has made a significant contribution to our understanding of the complex roles played by states in development.

INSTITUTIONALIST POLITICAL ECONOMY: STRENGTHS AND WEAKNESSES

Like the interest-based approaches discussed earlier, institutional approaches exhibit both considerable strengths and some signature weaknesses. One major strength is that they "move beyond the tendency of conventional economic analysis to treat all developed [and developing] economies as if they were institutionally identical" (Hall 1997: 182). In doing so, institutionalist accounts give us some leverage on why similarly placed agents in similar structural positions do not in fact act the same way across cases. They are also a reminder of how complex political economies are and how thinking institutionally allows theorists to compare across cases. What we also see in this literature is a useful "coming together" of both sides of the institutional chosen structures/structured choices dichotomy. Scholars such as Hall and Soskice have become far more attuned to how institutions are chosen structures, while scholars on the other side of the divide, such as Greif, are increasingly concerned with how they structure choices. There are also, of course, some general limitations to institutionalist analysis, such as the availability of comparative cases, but as each branch of this literature has developed, each exhibits its own characteristic strengths and weaknesses.

The approach to institutionalism exemplified by the "varieties" approach of Hall and Soskice has quickly become one of the most copied, and critiqued, positions in the literature. Criticisms of this type of institutionalist political economy tend to revolve around three issues. First, there is the tendency to confuse ideal types with real existing political economies, with the result that the more one takes the ideal to be real, the fewer actual cases the model seems to fit (Crouch 2005; Campbell and Pedersen 2007). Second is the tendency, similar to that found in Mares and Iversen, to see political struggle as a "too-rational" process where functional fit and self-regulating equilibrium institutions replace more basic political struggles (Howell 2003). Third is the attribution of causes to sets of institutions whose purported effects may be generated elsewhere (Blyth 2003b).

Greif's institutionalism, emblematic of the recent "sociological turn" of economics (Greif 2006: 22), is burdened by a separate set of concerns. Perhaps the most basic concern is whether the game (theory) is worth the candle. While multimethod interdisciplinary work is to be applauded, such explanations must add value to other approaches. Subtract the game theory exposition of the cases and the reinterpretation of everyday economic and sociological concepts, and one is arguably left with a rather strong culturalist framework where, because agents think in certain categories, given the institutions that their unique ideas and beliefs allow them to build, such agents act in specific ways, given the institutions that their unique ideas and beliefs allow them to build (Seabrooke 2006: 26). Why one needs higher math to reach a rather tautological conclusion that anthropologists grew dissatisfied with a generation ago is an open question.

Finally, the analyses that grew out of the focus on the developmental state also have several limitations. In contrast to our other two versions of institutionalist political economy where "theoretical overreach" may be a problem, what this literature exhibits is arguably a *lack* of theoretical reach. Being inductively generated from the examination of a few select cases sets up a classic "survivorship bias" problem (Taleb 2007).[13] By correlating the attributes of "winners" as causes, when "losers" are encountered they are examined from the premises generated by analyses of the successful cases, thus biasing the results. Given this, the portability of the framework outside of the cases where it was developed, a strength of deductively derived interest-based approaches, is severely attenuated, as recent discussions of the relevance of the developmental state model for the Chinese experience demonstrate (Tsai 2002, 2007).

BEYOND COALITIONS AND INSTITUTIONS: IDEATIONAL POLITICAL ECONOMY

As argued previously, Katzenstein's *Small States* (1985) opened the door to moving beyond material and institutional approaches, suggesting that ideas and ideologies needed to be taken seriously as explanatory concepts in their own right. This call has been taken up by a diverse group of political economists. While such approaches began with, and to some extent retained, a focus on how elites instrumentally use ideas as resources, more recent literature has sought to go beyond this instrumentalist position. To trace how this branch of political economy has evolved over time, I group ideational works under two headings: ideas as resources and conventions, and ideas as governance technologies.[14]

[13] I prefer this term to the more usual "selection on the dependent variable" term since it more accurately specifies the problem.

[14] I use the terms "ideational" and "constructivist" interchangeably. Although constructivism has its home in international relations and ideational scholarship has its home in comparative politics, they ultimately investigate similar phenomena. While the first generation of such scholars used different categories and concepts – ideas versus norms, for example – what is of interest is how both schools increasingly come together and, in doing so, dissolve what is left of the barrier that separates international and comparative political economy. See Hobson and Seabrooke (2007).

Ideas as Resources and Conventions

An important early statement of this approach was Peter Hall's study of the spread of Keynesian ideas across nations (Hall 1989). Building on his prior work on how institutions structure choices, Hall examined different national receptions of Keynesian ideas as a way of explaining institutional change. Specifically, how individual actors used Keynesian ideas as resources to forge new social coalitions during the dislocation of the 1930s comes to the fore in this analysis. In contrast to materialist models, in Hall's view, exogenous shocks to agents' material positions do not unproblematically translate into new political preferences without some kind of elite mediation as to what such shocks signify; hence the importance of Keynesian ideas. For Hall, what determines the degree to which such elite mediation is possible, and whether such ideas can recast political debate and thus possible lines of cleavage, is the ideas' degree of "fit" with the existing "structure of political discourse" of a nation (Hall 1989: 383).

Building on Hall's opening were several contributions that created a head of steam for ideational scholarship during the 1990s. Notable and influential were Sikkink's examination of how developmentalist ideas impacted economic policymaking in Latin America (Sikkink 1991); how ideas can serve as "road maps" and focal points in situations of multiple equilibria (Goldstein and Keohane 1993); and how neoliberal ideas informed the project of EMU (McNamara 1998). What these contributions all have in common is the *instrumental* use of ideas. That is, ideas are used by agents to realize their goals. However, within this literature there is, implicitly at least, another position: that ideas are not reducible to agents' a priori material interests or institutional position (McNamara 1998: 6–9). Once "let out of the box," ideas "have a life of their own" and can take interests in new and unexpected directions. To understand this subtle but important distinction, consider the work of Berman (2006), Blyth (2002), and Jabko (2006).

Berman asks two questions: First, why, during the crisis of the 1920s and 1930s, did two sets of political ideas came to prominence: social democracy and fascism? And second, why was the set of fascist ideas so successful in its time? Berman focuses squarely on ideas and how elites use ideas, not just to fashion coalitions, as stated earlier, but in a prior step, to interpret what changes in the political economy *mean for other agents* (Berman 2006). In doing so, Berman moves beyond ideas as individual resources and stresses the importance of ideas as intersubjective conventions that can restructure agents' interests.

Berman argues that fascism came to power because of the "cognitive locking" Marxist thinking encouraged among left parties in the 1920s and 1930s. Marxism was, especially for the German Social Democrats (SPD), the paradigm through which contemporary developments were understood. As such, rigid adherence to the axioms of historical materialism prevented the SPD from reacting in any positive way to the Depression. After all, if capitalism was going to come tumbling down, and Marxism told you why, and also told you that this was a good thing, why try to stop it? Trapped within this logic, left parties became forces for political inaction, which paradoxically laid the groundwork for the rise of fascist parties. Unencumbered by such ideas, fascist parties were able to offer alternative diagnoses of and hence alternative policies to the Depression that differed from those of liberals ("just wait until things get better") or those of Marxists ("just wait; it can only get worse"). Fascist parties were then able, through argument and action, to create a new narrative of what the Depression meant (and who was responsible for it) that recast agents' interests and served as the basis of their new coalitions. For Berman, ideas are indeed instruments, but they are instruments that can recast interests. They are social technologies that frame and decode complex environments such that intersubjective understandings can be created among differentially located actors as a necessary step in mobilizing collective action.

My own work very much follows in this tradition of viewing ideas as both resources and conventions. In *Great Transformations* (Blyth 2002) I offered an explanation of why one common phenomenon, deflation, produced institutional responses as varied as Japanese imperialism, Italian fascism, and Swedish social democracy. Focusing on ideas as interfaces with the world constructed by agents to make sense of complexity and uncertainty, I sought to show how economic ideas formed the basis of both the distinctive social coalitions and new institutions that emerged in these periods in the United States and Sweden. I argued that ideas matter for political economy explanations to the extent that phenomena such as deflation and inflation can produce nonprobabilistic Knightian uncertainty, a situation where agents can be unsure of what their interests are because following their first-best strategies leads to Pareto-inferior outcomes (Blyth 2002, 2007, 2008). In such moments, when individually rational actions lead to collectively suboptimal outcomes, uncertainty over what has gone wrong and what to do about it leads to an indeterminacy between structure and action where, as Ira Katznelson has put it, "structurally induced unsettled times can provoke possibilities for particularly consequential purposive action" (Katznelson 2003: 274). In these unsettled times, the power of ideas *to make agents powerful* comes to the fore.

This theme is developed further in the work of Jabko (2006). Jabko asks where the ideas for the institutional design of the European Union (EU) came from. For Jabko, a focus on French elites (Parsons 2003) or on a transnational "neoliberal consensus" (McNamara 1998) is necessary but insufficient to explain the institutional form of the EU. Jabko focuses instead on the role of the European Commission, a weak actor at the heart of the EU as the generator and transmitter of some very constituency-specific ideas (Jabko 2006: 42–57). As a weak actor that nonetheless held a critical position in the emerging European system of transnational governance, Jabko argues, the Commission "worked at constructing a particular integrationist agenda . . . premised on the renewed popularity of market ideas" (Jabko 2006: 48). The Commission thus "sold" the project of integration to different constituencies according to logics those constituencies wanted to hear.[15]

[15] To telecommunications and finance firms the drive for market integration was sold as a constraint to be overcome. To monopolistic power generators the market was appealed to as a norm to aspire to. To other weaker EU members, the market was sold as a common space of interaction

By "playing the market" in this way, the Commission was able to bring other, more powerful actors on board and create momentum for a series of institutional reforms that were in no way reducible to the supposed material interests of the actors in whose name these reforms were made, including the Commission itself.

In all of these accounts, what comes across clearly is the contingency of political change and institutional development. Rather than the linear equation of "structural position → interests → actions" that we find in interest-based accounts or the path dependence of institutional accounts (Pierson 2004; Mahoney 2000), these approaches stress, as do constructivist accounts in general, how things "could have been different" had it not been for certain socially constructed (ideational) factors (Hacking 1999; Hay 1999). Given this, what other perspectives take as almost overdetermined, these accounts portray as underdetermined and dependent upon the particular constructions wielded by less than powerful actors. Once again, ideas are not simply weapons wielded by powerful agents, although they can be that. Instead, ideas are contingent properties that can make agents powerful (Epstein 2008).

Ideas as Governance Technologies

A final contribution to this literature worth mentioning goes further still. These authors seek to understand how particular ideas not only serve as resources or intersubjective conventions, but are themselves technologies of governance. One such attempt of note is the work of Paul Langley, who applies actor-network theory to the study of pension privatization and mortgage securitization (Langley 2004, 2006a, 2006b, 2008). Again, while materialist and institutionalist analyses of these phenomena do point to important determinants of choice and action (Brooks 2005), what Langley brings to our attention is how such "top-down" projects rely upon not just an appeal to agents' interests, but an attempt to craft particular subjectivities and subject positions.[16]

Langley analyzes how pensions in the United States and the United Kingdom have increasingly moved from the province of the state to the market, characterized by a decline in state-based funding and a general shift from defined benefit to defined contribution schemes (Langley 2006a: 920). This much is well known. What is not so obvious is how such schemes rest upon creating a particular subject, what Langley calls the "entrepreneurial investor subject" (Langley 2006a: 921). Crucially, such subjects are not simply "out there" as already constituted individuals waiting around for the financial services industry to pick them up.[17] After all, why should, for example, a technical worker in an

and a level playing field. Finally, the project of EMU as a whole was sold as a talisman of "all good things going together" to the EU member states as a whole (Jabko 2006: 57–179).

[16] On the limits of "top-down" approaches where elites deploy ideas and masses passively accept them, see Seabrooke (2006).

[17] Indeed, as the British pensions debacle of the 1990s showed all too well, without massive governmental intervention to promote the individualization of pensions, financial firms want to have nothing to do with such small investors (Langley 2004).

export industry have a given "interest" in a defined contribution program? Such subjects must be discursively constructed before they can act in the political economy as entrepreneurial agents.

For Langley, the creation of such subjects rests upon "the sidelining of insurance as a means of . . . managing . . . risks in favor of the promotion of investment . . . to bear risk." In such a world, risk is no longer "represented as potential dangers to be collectively managed . . . [instead] 'risk' [is] represented as . . . a reward for individuals" (Langley 2006a: 921). Creating such subject positions is then not simply a matter of appealing to agents' pre-given interests. Rather, it is a process of discursive construction where instruments as varied as popular television programs on investment, state-sponsored savings programs (401[k]s), and calculative technologies of credit reporting are brought to bear to create particular risk-bearing subjects who "perform" risk-bearing practices.

Taking this focus on "performativity" further, that is, investigating how the action of employing ideas that seek to represent or measure a given phenomenon brings the phenomenon into being, is MacKenzie's work on the performative role of financial theory in the economy (MacKenzie 2006). For MacKenzie, financial ideas do not merely describe the world; they can also help bring that world into being. As he puts it, "financial economics . . . did more than analyze markets; it altered them. It was an 'engine' . . . an active force transforming its environment, not a camera passively recording it" (MacKenzie 2006: 12).

For MacKenzie, economic analyses do not stand outside of reality, "analyzing it as an external thing" (MacKenzie 2006: 16). Rather, as is common in complex social systems, there is an *interdependence* of subject and object such that beliefs about the former influence the behavior of the latter. As a consequence, applying an idea changes the nature of the system where it is applied (Blyth 2008). This is more than Merton's famous idea of a "self-fulfilling prophecy" (Merton 1968). In this case, by performing theory, MacKenzie draws attention to how the use of specific models in financial markets becomes part of the infrastructure of markets. That is, such ideas constitute the "algorithms, procedures, routines, and material devices" that *are* markets (MacKenzie 2006: 19).

Given this object/subject interdependence, MacKenzie shows how the use of particular financial technologies, their "performance" over time in the world, made the world more like the theory (MacKenzie 2006: 143–179). In particular, the use of the Black–Scholes options pricing model by market participants, which made assumptions about markets having zero transactions costs and being efficient, helped legitimate arguments and policies designed to make markets more efficient (MacKenzie 2003: 854). Doing so, in turn, enabled agents to employ the model in ever wider areas of finance such that the fit between the model and the reality it purported to describe increased over time. As he puts it, using the model "altered patterns of pricing in a way that increased the validity of the model's predictions" (MacKenzie 2003: 852).

In a wonderful case of causal recursion, it was not that the model's fit got better over time as its accuracy grew. Rather, the model's usage by more and more actors made the markets themselves behave more like the model

(MacKenzie 2006: 263–268; 2003: 852–856). These developments, in turn, enabled agents to deploy ever more sophisticated risk-calculating technologies based on these same ideas that, in turn, allowed the development of the "risk society" and risk-bearing subjects Langley is concerned with (MacKenzie 2006: 211–243). In sum, these approaches show how ideas not only inform but *constitute* structures of governance. Following Hirschman (1978), Langley and MacKenzie show how ideas themselves can constitute and alter the world they supposedly describe. By constructing subjects and performing theory, we gain insight into how governance is not simply a property of government, but is something that occurs on and through individual agents.

Ideational Political Economy: Strengths and Weaknesses

Like interest-based and institutional approaches, ideational approaches have specific strengths and particular weaknesses. One strength is that such work enables analysts to ask questions that other approaches do not even consider asking. After all, if one takes complex mediated phenomena such as the causal generators behind deflation, globalization, and multigenerational institution building projects to be equally obvious to all agents, then there would be no point in attending to ideas. However, as these examples show, while interest-based accounts can point to coalitions that "should be there," given sectoral alignments, and while institutional accounts can point to path dependencies in policymaking, ideational approaches drop down below the level of the possible to investigate what real actors thought and did. As such, the problematic "reimagining of history" noted earlier is avoided. Second, that agents' subjectivities and interests can be reconstructed despite their ostensible structural positions, as this literature also demonstrates, is ruled out of bounds by other approaches. In this literature, interests are treated as something to be explained rather than something to do the explaining with. Third, this literature also excels in explaining change. Its focus on the contingent and dynamic nature of political and economic change offers a very different vantage to the soft functionalism of interest-based and institutionalist approaches that sometimes see the world as all too stable. However, like our other approaches, it has certain problems.

First of all, disentangling the effects of ideas from other factors, if seen as possible at all, is extremely difficult. That agents thought in a certain way may be established, but the extent to which outcomes are directly attributable to these ideas is still difficult to ascertain. Strategies can be applied to attack this problem (Parsons 2003, 2007), but the "How much do ideas matter?" question never really goes away.[18] A second perennial question is "Where do such ideas come from?" This is a problem because if ideas are reducible to agents'

[18] Unless one sees interests as indistinguishable from "ideas about interests," in which case the problem disappears completely (Wendt 1999; Widmaier 2004; Blyth 2003a). This is a step few American political economists are willing to take. In contrast, such a position is arguably the dominant one in other parts of the world. See Blyth (2009) for a discussion of these issues.

interests, and if they really are just instruments to help people get what they want, then the ideational story collapses into an interest-based account.[19] Finally, there is the quite reasonable question "What do such approaches do to our ability as social scientists to generalize and predict?" After all, if contingency, construction, and interdependence effects are as replete as at least some of these scholars say they are, then whether political economy can aspire to the status of a predictive social science is questionable at best.[20] Given these concerns, while ideas may matter for political economy explanations, it is not clear that they need matter for all the questions that political economists ask.

The Link Not Made? Rational Choice Theory and Political Economy

I return now to the first caveat we opened with: the relationship of political economy to rational choice theory. After all, one could tell several other versions of this story that include rational choice as a core component of political economy. One version would be a more expansive set of four concepts rather than three: rationality, interests, ideas, and institutions, for example. This version of events would have Arrow (1948) and Von Neumann and Morgenstern (1953) laying the conceptual foundations upon which Downs (1957), Riker (1962), and Olson (1965) build the basic concerns of the field: spatial models, bargaining, collective action, strategic interaction, to name but a few. Following this, one could point to Bates on development (1981), Elster on technical change (1983), Przeworski on left parties and social democracy (1986), and North on institutions (1990) as the "killer apps" of the period, and then, again as Levi has done in her contribution to this volume, map recent developments from there. Indeed, there would be nothing wrong with doing so, except for the fact that it would essentially repeat Levi's chapter. Since she has done such a good job of this, I see no point in repeating her effort here. Apart from redundancy, however, there are other good reasons for not including rational choice theory, and rationalism as a concept, as its own category in *this* discussion of political economy: namely, that interests as a concept are not exhausted by rationality as a behavioral postulate, and that political economy is broader than rational choice and vice versa.

The first issue is best exemplified by going back to the extensions of interest-based political economy discussed earlier. One could argue, quite reasonably, that while Iversen, Mares, Hiscox, et al. can trace their substantive ideas to Gourevitch, their thinking, both theoretical and methodological, is rationalist. As such, they are more "rational choice theorists" than "interest-based" political economists. I would not even be surprised if the aforementioned

[19] The response, that ideas are emergent properties of complex adaptive systems, irreducible to the conditions of their emergence, worries many political economists since it introduces a very large slice of indeterminacy to our explanations.

[20] That political science can ever be a predictive science given the nature of the social world is tackled in Blyth (2006).

scholars would self-identify in exactly this way. My point here in mapping the field in this way is somewhat different. Even though this chapter seeks to draw boundaries using discrete concepts, if one makes interests a derivative function of rationalism, then one misses how other schools of political economy also deal with interests: in terms of how our interests are endogenous to the institutional context or from analyzing our "ideas about our interests." That is, theories of institutions are also theories of interest generation, and theories of ideas are also theories of institutional change. By reducing "acting on one's interests" to "acting according to a rationalist behavioral postulate," one risks ignoring what large numbers of scholars who also regard themselves as political economists have to say about these same questions.

Second, political economy is broader than rational choice theory, and rational choice theory is broader than political economy. For some rational choice theorists, rational choice theory is political economy – period. Consequently, if you are not doing what they do, then you are not a political economist. Consider, for example, the introduction to the *Oxford Handbook of Political Economy* (2006). According to the editors of this *Handbook*, "[I]n our view, political economy is the methodology of economics applied to the analysis of political behavior and institutions" (Weingast and Wittman 2006: 3). Political economy is a field where "the unit of study is typically the individual. The individual is motivated to achieve goals . . . the theory is based in mathematics . . . [and] . . . sophisticated statistical techniques" (ibid.: 4). Moreover, political economy is defined by its common foci: "endogenous institutions," "the revelation and aggregation of information," and "the concepts of survival and equilibrium" (ibid.).

Now, while I fully admit that this is *an* approach to political economy, one that can happily sit within the interest-based approaches outlined here, it also defines itself in relation to sets of concepts and categories that effectively says to many of the other scholars discussed here that whatever it is they are doing, *it is not political economy*. Rather than adjudicate the "one true version" of the field, I find it more useful to point out here that political economy may be seen as a broader field with more concerns than are often portrayed by some rational choice theorists. Indeed, as Professor Levi notes in her chapter, far from being monolithic, rational choice theory "now tends to be multimethod and catholic in its approach" such that rather than squeezing out other approaches, "the competitiveness I described between the camps [rationalists and historicists] in the earlier edition of this volume has subsided" (Levi this volume). In sum, I argue that rather than rational choice positioning itself *as* political economy, it is more usefully seen as an extremely important general position in comparative politics that has evolved into far more than an application of the methodology of economics to any and all political economy questions as defined solely by the concepts of equilibrium, endogenous institutions, and the like. That this is the case is clearly seen in the embrace of ideational and other variables by rational choice theorists, not as a takeover bid, but as a broadening of the perspective itself.

CONCLUSIONS: COMPARING APPROACHES AND THE QUESTION OF BOUNDARIES

Having said all that, I do not want the reader to leave this survey with the idea that the field started with interests, moved to institutions, and now embraces ideas as some kind of teleological progression. This is simply not the case. All three of these positions are vibrant research programs. That they emerged at the same time out of the core concerns of three exemplar works is significant, but one did not succeed the other. What I wish to return to here is the issue of the boundaries of political economy and our second opening caveat: the extent to which this diversity of approaches is something to be concerned about or embraced. Key in determining this is the extent to which these different approaches speak to one another. To the extent that they do, political economy as a field has a certain coherence. To the degree that they do not, it remains simply a collection of approaches within comparative politics that happen to focus on economic questions.

As we have seen, each strand of political economy has a distinctive evolution. Interest-based political economy has, when seen in terms of the overall history of political economy, had the most peculiar evolution. We noted earlier how political economy was reborn in the 1970s as a partial alternative to formal economic analysis. In its canonization in Gourevitch (1986) this strand of political economy put interests front and center, but saw those interests as nonetheless mediated by a host of secondary factors. As noted, however, later contributions have been far more reductionist. This strand of the literature has increasingly become more like mainstream economics with an emphasis on equilibrium modeling and formal theory, which is somewhat ironic when one remembers that what "kick-started" this approach was a rejection of economics' separation of politics and markets and its attendant reductionism. This strand of the literature now seems to be repeating this separation insofar as politics in these approaches is increasingly reduced to a series of coordination games and/or equilibrium states to be modeled. For some scholars this is a concern; for others, it is a sign of progress.

Indeed, the evolution of one strand of institutional political economy in part reflects this "back to economics" tendency, while other parts of it have headed off in entirely different directions. As seen in the approaches reviewed here, theoretical divergence and convergence coexist in this literature. Varieties-type approaches explicitly set as a goal a desire to bring the literatures of economics and political science together in a game-theoretic synthesis (Hall and Soskice 2001). But approaches are not defined simply by particular technologies. Greif's use of game theory, for example, productively steps beyond its usual materialist/rationalist formulations insofar as it is embedded in a cultural and sociological approach that rejects notions of fixed interests and institutions as products of conscious design. As such, this type of institutional political economy can deploy economic technologies without pushing political economy back into economics. Finally, there is more mainstream historical institutionalist political economy, here identified with, but not limited to, work on the developmental state. This body of work has continued the type of scholarship that Hall (1986) exemplified, and has become perhaps the most common form of institutional political economy practiced in the field.

Ideational political economy is the most variegated type and is also the most recent vintage. As such, it is harder to give an account of its evolution. What one can say is that it has become an approach to be reckoned with in its own right. Although this approach contains within it everyone from "thin" constructivists to actor-network theorists, it has established itself as more than just a helper for other approaches (c.f. Goldstein and Keohane 1993) or as appropriate only for peculiar "limit cases" where normal dynamics break down. Its diversity of approaches, and the types of questions it asks, certainly add to the breadth of political economy approaches, and do so in a way that, even more than institutional works, pulls away from a "reeconomizing" of political *economy* toward a genuine postdisciplinary stance.

In sum, if one does not reduce political economy to a singular approach, it appears as a diverse and thriving area of study. Given that the object of study of political economy contains everything from microphenomena to large-scale social processes, such diversity is probably, on balance, a good thing. After all, in an uncertain world, diversification is generally regarded as a prudent strategy – not just to protect us from the "all the eggs in one basket" problem, but to position us to take advantage of the unexpected events that often destroy singular theories (Taleb 2007; Blyth 2006).