
Toward a New Economic Sociology of Development

ABSTRACT What explains the differential growth rates that foster international income inequality? The leading sociological answers have taken conflicting positions on the assumptions of *self-interest* and *diminishing returns* that are taken for granted in the neoclassical literature. While modernization theorists traced the periphery's inability to take advantage of diminishing returns in the core to "traditional" values that allegedly militated against savings, investment, and growth, and thus denied the universality of self-interest, their neo-Marxist successors traced underdevelopment less to the values of the poor than to the "cumulative" advantages of the rich, and thus denied the inevitability of diminishing returns. The result is a two-front assault that suffers from a serious coordination problem, and I therefore take issue with both the neoclassical accounts and their critics by, first, calling the validity of their assumptions—self-interest and diminishing returns—into question and, second, defending an alternative approach that treats the *subordination* of self-interest to norms of fairness, trust, and cooperation in the short run as the sine qua non of increasing returns and growth over the long run. The research challenge, therefore, is to unearth the roots of collaborative social norms in particular historical contexts—a challenge that will prove more tractable if development sociologists not only abandon the assumptions of self-interest and diminishing returns but embrace the tools and insights of the new economic sociology.

INTRODUCTION

Neoclassical economists tend to assume, first, that individuals are motivated by self-interest and, second, that their investments are subject to diminishing marginal returns. While the former implies that investors look for the highest rates of return on their capital, the latter implies that the rates they find are "very large when the stock of capital is small and vice versa" (Sala-i-Martin 1995:10), and neoclassical growth theories thus take the assumptions of self-interest and diminishing returns to their logical conclusions by positing that poor countries with low capital-labor ratios will outgrow rich countries with high capital-labor ratios over time (Michelis and Neaime 2004:477). In fact, Jeffrey Sachs (2000) concludes that the neoclassical model "has an ingrained optimism" (pp. 30–31) about the prospects for growth in the developing world. "Market institutions are a given," he explains. "Countries are assumed to save and accumulate capital, whereas technology and capital are assumed to flow readily across national borders. Since the marginal product of capital is higher in capital-scarce countries than in capital-rich countries, and since the technologically lagging countries can import the technologies of the richer countries, the poorer countries are expected to grow faster than the rich countries."

The rich countries have nonetheless been growing faster than the poor countries for the better part of the past two centuries, and the gap between the average incomes of former and the latter has therefore been increasing rather than decreasing. “Even when we contrast the fast-growing ‘emerging economies’ of China and India with the rich world,” explains Branko Milanovic (2011), “the gap in the first decade of the 21st century is greater than it was around 1850” (p. 12). When we instead examine “basket cases” (Patrick 2011) like Haiti, Liberia, and Yemen, the picture is darker still, and Kemal Derviş (2011) thus bemoans the fact that “in some very poor countries, many of them experiencing conflict and government failure, hundreds of millions of people have real incomes that are not much higher than they were 200 years ago” (p. 14).

Skeptics have addressed the developing world’s failure to live up to neoclassical expectations by relaxing one, but rarely both, of the neoclassical assumptions. While modernization theorists traced the capital-poor world’s inability to exploit diminishing returns in the rich world to “traditional values” (Rostow 1956:28) that are inimical to growth, and thereby denied the generality of self-interest, their descendants—including Marxists and heterodox economists—traced underdevelopment not to the “traditional values” (Wallerstein 1995:231) of the poor countries but to the “cumulative” (Wallerstein 1974:309) advantages of the rich ones, and thereby rejected the inevitability of diminishing returns. The result is a two-front assault that suffers from a serious coordination problem, and I therefore take issue with both the neoclassical accounts and their critics by, first, calling the validity of their assumptions—self-interest and diminishing returns—into question and, second, defending an alternative approach that treats the *subordination* of self-interest to norms of fairness, trust, and cooperation in the short run as the sine qua non of increasing returns and growth over the long run. I have divided the rest of the paper into four main sections: first, a critical review of the “grand paradigms of modernization and dependency” (Kim 2006:119; see also J. Collins 1996:56; Knight 2001:748; Garretón et al. 2003:iii) that dominated the development debate in the postwar era; second, a reconsideration of the state-centered approaches that did as much to obscure as to address the limitations of the grand paradigms toward the end of the last century; third, an alternative that draws upon core sociological principles to address lacunae in the state-centered account; and fourth, an effort to justify the development and utilization of the alternative in the twenty-first century.

DEVELOPMENT SOCIOLOGY IN THE ERA OF THE GRAND PARADIGMS

Neoclassical growth models hold that *diminishing returns* to the investments of *self-interested individuals* militate toward the “convergence” of per capita incomes across countries over time. “Poor countries, with low ratios of capital to labor, have high marginal products of capital and thereby tend to grow at high rates,” explains Robert Barro (1991:407). “This tendency for low-income countries to grow at high rates is reinforced in extensions of the neoclassical models that allow for international mobility of capital and technology.” Neoclassical models thus tend to posit the convergence of per capita income over time and space.

The empirical record is nonetheless ambiguous. While neoliberals hold that international inequality has indeed “declined dramatically” (Ferguson 2012) in recent years, and

TABLE 1. Relative versus Absolute Convergence in Per Capita Income: A Hypothetical Illustration

Country	Year 1	Growth _{y1-y2}	Year 2	Growth _{y2-y3}	Year 3
Country A	\$40,000	1%	\$40,400	1%	\$40,804
Country B	\$4,000	1%	\$4,040	4%	\$4,202
Relative gap	10		10		9.7
Absolute gap	\$36,000		\$36,360		\$36,602

Note: The relative gap is simply the ratio of Country A's per capita income to Country B's per capita income; the absolute gap is the difference between the two values. Monetary values are rounded to their nearest dollar; ratios are rounded to the first decimal.

underscore the contributions of India and China in particular, their critics take issue with their samples, starting points, weights, and indicators and find “insufficient evidence to reject the null hypothesis of no change in global interpersonal inequality over 1970–2000” (Anand and Segal 2008:91). The problem is not only that—unlike China and India—*most* developing countries are falling further behind their developed country counterparts (Wade 2011) but that the intensification of inequality *within* China belies the neoclassical prediction “that inequality would have fallen as wages of low skilled workers relative to skilled workers rose” (Milanovic 2011:8) in the course of export-led industrialization. In fact, China’s Gini coefficient has jumped from less than 30 in the 1970s to approximately 45 today. “Once again,” explains Branko Milanovic (2011), “fact confounds theory” (p. 8; see also Lakner and Milanovic 2013).

Nor is the relative gap between rich and poor the only consideration. A large body of evidence implies that people “think about inequality in absolute rather than relative terms” (Wade 2014:327), and the absolute gap between rich and poor is rising by any metric. Consider, for example, two hypothetical countries with per capita incomes of \$40,000 and \$4,000 respectively. If both countries grew at 1 percent for a year, the ratio of their per capita incomes (or relative gap) would stay constant at 10, but the average income in country A would be \$360 higher in year 2 than in year 1 (see table 1).

In fact, the aforementioned tendency for poor countries to outgrow rich countries offers cold comfort when “something analogous to gravity hinders upward mobility” among the former and “something analogous to magnetic levitation” props up the latter (Wade 2014:322). If country B began to outgrow country A by a factor of 4 in year 3, for example, and continued to do so for the foreseeable future, the relative gap would begin to decline (i.e., to 9.7 in year 3) but the absolute gap would continue to expand for more than five decades—and absolute convergence would take approximately 80 years.

Nor is my hypothetical example unrealistic. “Absolute income gaps between countries and individuals have been widening fast,” explains Robert Wade (2014:328). “The present absolute income gaps in the world economy are so large that they will go on increasing for another half-century at the least, even if developing countries experience significantly faster economic growth.” They will in all likelihood go on increasing for

well over half a century if the developing countries begin to decelerate or contract, as recent estimates imply (Talley 2015).

Economists have traditionally explained the gap between their predictions and reality by referring to “counteracting forces, sociological, educational, and other” (Baumol and Wolff 1988:1157). Examples would include not only the “wars, breakdowns of internal order, and misguided ventures into centralized economic planning” decried by Robert Lucas (2009:23) but norms, values, and identities as well (Francois and Zabojnik 2005). “These noneconomic motivations in general are not analyzed by economists,” explained the late Everett Hagen (1960) more than half a century ago (p. 623). “Rather, since they can be assumed to remain constant they are treated as parameters, are explained on an ad hoc basis or not at all, and then further analysis is in terms of economic theory.” While Hagen would condemn his fellow economists for their insularity and myopia, he would simultaneously complain that anthropologists, psychologists, and sociologists “have all been so busy advancing theory in their own field that they have not yet had the time or interest to reach out toward each other to develop an interdisciplinary theory of any sort, much less an interdisciplinary theory of the type of social change economists are interested in; namely, a theory of how economic growth begins in an erstwhile traditional society” (p. 626; see also Hagen 1957).

A number of sociologists embraced the interdisciplinary challenge by turning “noneconomic factors”—like honesty, creativity, nationalism, and trust—from “parameters into variables” (Hagen 1960:623) in their models. The results included “evolutionary” accounts of modernization that portrayed societies as “autonomous units which change according to internal forces” (Portes 1976:66) and “value-normative” (p. 80) alternatives that took the *diffusion* of norms, attitudes, and preferences much more seriously. But the most familiar variant of modernization theory—the “Lamarckian scheme” that economic historian Walt Rostow “adopted implicitly from Talcott Parsons” (Gilman 2007:162–63) in the 1950s—was simultaneously evolutionary in nature and sensitive to the possibility of “demonstration effects” (Rostow 1959:5) that would allow for the diffusion of norms, attitudes, and preferences from the “more advanced” to the “less advanced” parts of the world. In fact, Rostow posited a “traditional society” marked by growth-retarding values, a “mass consumption” society vulnerable to diminishing returns, and a process of diffusion whereby the former would learn from the latter over time.

What were the underpinnings of the diffusion process? “In part the transmission consisted in making men in less advanced societies perceive that new positive choices were open to them,” explained Rostow (1959:5). “At least equally powerful, however, was the negative demonstration that more advanced societies could impose their will on the less advanced, through the exercise of military force. A reactive nationalist sentiment—rooted in a perception of the link between industrialization and effective power in the world arena—came to be an extremely important factor in leading men to take the steps necessary to unhinge and transform the traditional society in such ways as to permit growth to become its normal condition” (p. 6).

In other words, Rostow traced modernization at the national level to geopolitical competition at the international level. Nor was he alone. Martin Bronfenbrenner (1961) interpreted Japan’s Meiji Restoration through a distinctly Rostovian lens. Angus Maddison (1971:36) offered a broadly parallel account of Chinese development (see also Maddison 1998).

And H. W. Arndt (1987) concluded that reactive nationalism “had later echoes elsewhere in Asia and Africa, both in countries like Thailand and Egypt, which had maintained a precarious independence, and in colonial territories” (p. 21). While Latin American countries “had neither colonial status nor external threats to react to,” and therefore had much in common with the “non-European countries of what is now the First rather than the Third World,” they would fall behind the former—and join the latter—by the mid-twentieth century. “In the following decades,” explains Arndt, “reactive nationalism began to play a part also in Latin America, in the guise of popular anti-imperialism, resentment against *dependencia*, and demands for a New International Economic Order” (p. 22).

By the 1970s, therefore, Latin American scholars had abandoned modernization theory for “neo-Marxist” (Foster-Carter 1973) alternatives that implied increasing, rather than diminishing, returns (Schwartz 2007; Wibbels 2009). For instance, Theotonio Dos Santos (1970) condemned modernization theory as “nothing more than ideology disguised as science” (p. 235) and developed an alternative that emphasized the “the transfer of surplus” from the so-called periphery to the core of the world economy. Osvaldo Sunkel (1973) portrayed development and underdevelopment as opposite sides of the same coin and underscored their “cumulative” and “self-enforcing” natures (pp. 135–36). And Rodolfo Stavenhagen (1974) blamed Latin America’s “relative impoverishment” (p. 132) on the region’s tendency—first identified by Raúl Prebisch (1959)—to export low-cost primary products and import costly manufactured goods. “That is, while the prices of export products tend to decline,” he argued, “those of imports (coming mainly from industrial countries) tend to increase or remain relatively stable” (Stavenhagen 1974:132).

The imbalances thereby engendered proved pivotal not only to Latin American “dependency theory” (Kay 1989) but to a broader body of radical sociology—including world-systems analysis (Wallerstein 1974), the theory of “dependent development” (Cardoso 1972; Stavenhagen 1974; Evans 1979), and their successors (see, e.g., Portes and Kincaid 1989)—that broke with conventional understandings of development and underdevelopment in the 1970s and 1980s. While modernization theorists traced the poverty of the periphery to the “irrational” or “nonrational” (Moore 1955:157) attitudes of the poor, for example, their critics invoked the “self-reinforcing accumulation” (Sunkel 1972:519) of the rich—accumulation that would neither reinforce itself in a world of diminishing returns nor dissipate in a world of *laissez-faire* (Herrera 2006:44)—and thus brought increasing returns back into the development debate.

The radicals thus anticipated the new (or “endogenous”) growth theories that would come to prominence—if by no means hegemony—in mainstream economics a generation later (Krugman 1991; Martin and Sunley 1996; Crowley et al. 1998; Hounie and Pittaluga 1999; Evans 2005; Schwartz 2007), and their descendants feel vindicated by the results. After all, the new growth theorists parted company with both “neoclassical and modernization theories” that posited “economic convergence among nations” (Mahoney 2003:58) by abandoning the assumption of diminishing marginal returns and acknowledging the possibility of *increasing* returns that had long been taken for granted on the left. “When increasing returns are present,” explains Erik Wibbels (2009), “each additional investment attracts more investments, local job growth promotes migration, and large markets beget larger

markets. In such cases, the self-interest of market participants can produce economic asymmetries that closely mirror those between dependency theory's core, periphery, and semi-periphery" (p. 443; see also Therborn 2001:463).

The consequences are at best ambivalent, however, for in their effort to exorcise the ghost of Parsons from their subfield the radicals simultaneously embraced the "economic fallacy" (Polanyi 1977; Somers 1990) of universal market rationality. Thus Immanuel Wallerstein (1971) acknowledged "that, in the absence of specific social pressure to the contrary, men will tend to define their self-interest in terms of expanded personal consumption" (p. 359). Andre Gunder Frank (1970) derided the very notion of a "traditional society" devoid of self-regulating markets (see also Frank and Gills 1993). And Ronald Dore (1975) would therefore bemoan the "curious alliance" between mainstream and Marxist scholars "in favour of overwhelming emphasis on the rational pursuit of material self-interest" (p. 205).

Dore was by no means the only critic to condemn the rationality assumption, however, for Albert Hirschman (1981) would bemoan the "unholy alliance of neo-Marxists and neo-classicists" (p. 21) a few years later. Paul Warwick (1990) would question the "acquisitive" portrait of human nature that underpins "both Marxist and rational-choice theory" (p. 200) more generally. And Randall Collins (1985) would highlight the "Smithian" aspects of Wallerstein's framework (p. 119) in particular and would go on to place the Roman Catholic Church at the heart of the "institutional revolution" (R. Collins 1986:45) that opened the door to capitalism in Europe.

When taking the radicals to task, therefore, the skeptics reacted not only to their "neglect of the political" (Mouzelis 1988:39; see also Zolberg 1981:255) but to their "neglect of the cultural" (Smith 1997:556; Bukovansky 2002:19; see also Jackson et al. 1979:20; Ragin and Chirot 1984, esp. p. 303) as well. For instance, Randall Collins (1981) worried that Wallerstein ignored the medieval church on the grounds that religion is "essentially ideological" and thus unimportant, and he went on to note that what matters about religions are not their spiritual beliefs but their organizational resources and legacies. "The church is a material institution like any other," Collins argued, "and its property and its economic consumption—and for the medieval period its innovative economic productivity (as well as its cultural productivity)—are key elements in the pattern that made possible the birth of capitalism" (p. 55).¹

Others would express similar misgivings in the decade to follow, but their warnings would go all but unheeded by radicals who believed "that most human behavior is motivated by the pursuit of self-interest" (Sanderson 1994:68) and rejected the very possibility of socialization to the contrary. "That the individual organism is highly self-oriented should be obvious to any parent undertaking the socialization of a child," explained Stephen Sanderson (1994), "and it should also be obvious that even the most intensive forms of socialization cannot eliminate the desire to give priority to one's own interests" (p. 68).

By the late 1980s, therefore, the principal divide between economists and sociologists had shifted from the issue of individual motivations to the question of their collective consequences. While radical sociologists treated foreign direct investment as an impediment to peripheral growth, for example, and thereby parted company with mainstream economics

(see Crowley et al. 1998:36–39), they agreed that investors of all nationalities pursued their rational self-interest, and Edward Crenshaw (1991) thus portrayed the investment flows that animated dependency theory and world-systems analysis as “rational outcome[s] of market-oriented behavior” that could be “explained without reference to noneconomic (political or military) relationships between core powers and the developing world” (p. 1179).²

Economic reductionism was by no means the only alternative to the Parsonian division of academic labor, however, for the discipline as a whole was beginning to treat rationality as a “variable to be explained” (Stinchcombe 1986:7) by way of reference to social structure rather than an assumption to be invoked by mainstream economists or Marxists. While the consequences included the new economic sociology’s “campaign to regain lost territory” (Ruttan 2001:24) from the economists, they were all but foregone by the Marxists, who denied the importance of noneconomic relationships (Frank 1998:19) and refused “to budge from the level of global generalization” (Portes 1997:233).³

The best-known sociological approaches to development thus constitute mirror images of each other. While modernization theorists accept diminishing returns and doubt “that rational behavior is a universal human characteristic” (Valenzuela and Valenzuela 1978:539; see also Wallerstein 1971:359; Frank 1998:324), their critics reject diminishing returns and “assume that individuals in widely different societies are capable of pursuing rational patterns of behavior” (Valenzuela and Valenzuela 1978:545).

By the late twentieth century, however, the debate had reached an impasse. While modernization theorists pointed to the occasional miracle economy and portrayed their neighbors as miracles in embryo (Form and Bae 1988; Fukuyama 1989; Inkeles 1997), their critics underscored the unforgiving nature of the international division of labor and portrayed the Asian miracle, in particular, as the virtually inimitable product of an unusually “developmental” collection of states endowed with atypically favorable human and geopolitical resources (Wade 1985; Evans 1989; Bello and Rosenfeld 1990). “Many are called but few are chosen,” noted Bruce Cumings (1984) in his by now classic discussion of Asia’s success (p. 35). But the chosen few almost invariably had powerful bureaucrats at the helm, and the peripheral state has therefore assumed pride of place in contemporary development sociology (Evans 1995; Ó Riain 2004; Sandbrook et al. 2007).

BRINGING THE STATE BACK INTO DEVELOPMENT SOCIOLOGY

Alexander Gerschenkron is arguably the father of the state-centered approach (Evans and Stephens 1988:716; Hart 2004:46; Breznitz 2006:3). He underscored not only the advantages that are “inherent in delayed development” but the state’s role in their exploitation, and thus posited a link between the “degree of relative backwardness,” on the one hand, and the development of “deliberate industrialization policies,” on the other (Gerschenkron 1962:156–57). What are the industrial policies in question? While Gerschenkron focused on loans, tariffs, and the provision of physical infrastructure, his descendants tend to emphasize research and development, and Ronald Dore (2000) therefore portrays “the volume of public funding in the total research effort” as one of the “simplest measures” of the “role of industrial policy” in the national innovation system today (pp. 237–38; see also Evans 1995:147; Merito, Giannangeli, and Bonaccorsi 2010:25).

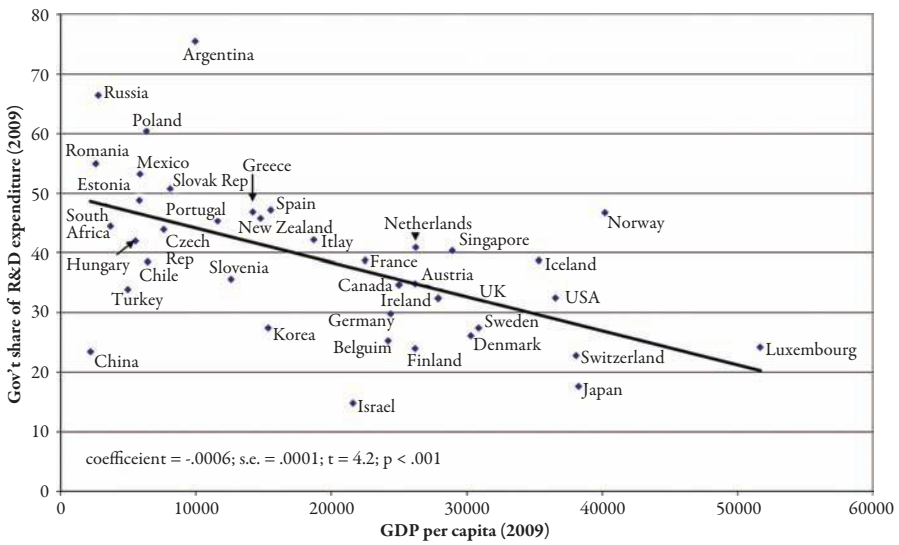


FIGURE 1. Government R&D expenditures and GDP per capita in 40 countries c. 2009. Data: OECD (2014); World Bank (2014).

Nor is he alone. Chalmers Johnson (1982) portrayed “government-sponsored research and development” as a key aspect of Japan’s “market-conforming” industrial policy in the postwar era (p. 318). Peter Evans (1995) discussed the ratio of public to private R&D spending in late twentieth-century Korea (p. 147). And Monica Merito and her colleagues (2010) hold that “public support to private R&D has been a traditional measure of industrial policy in Western countries” (p. 25) as well.

Is the Gerschenkron thesis robust by their metric? Figure 1 speaks to the question by plotting the public share of R&D expenditure by GDP per capita in 40 upper- and middle-income countries in 2009.⁴

The results not only bolster Gerschenkron’s account of industrial policy making in developing countries but gainsay the idea that policy choices are more “consistent within countries” and “diverse across them” (Dobbin 1994:351) than his theory allows. After all, the variation in question is longitudinal as well as cross-sectional. The Japanese government’s share of R&D has fallen from 27 percent in 1981 to 17 percent today, and similarly dramatic falls are under way—or have already occurred—in the course of economic development in China, Taiwan, Turkey, Mexico, and Korea.

The latter country provides a particularly apposite illustration of the latecomer effect. According to Evans, the government share of Korean R&D expenditure fell from 97 percent in the early 1960s, when the war-torn country was frequently portrayed as a virtual basket case (Cumings 1997:310; Chibber 2003:57; Chung 2007:12), to less than 30 percent in the late 1980s, when membership in the Organisation for Economic Co-operation and Development came to be a *fait accompli*. While public R&D expenditures grew by a factor of 50 over the period, their private counterparts grew even more rapidly, thanks in part to the “initial leadership” of the state (Evans 1995:147; see also Yoo 2004), and by the early twenty-first century the Korean government’s share of the country’s R&D spending was well below the OECD average.

The state-centered accounts that have gained ground in recent years are nonetheless the offspring—rather than opponents—of modernization theory and neo-Marxism. After all, the alleged “advantages of backwardness” that are exploited by the “developmental state” (Wong 2004:493; Crafts and O’Rourke 2014:304) are in part by-products of diminishing returns in the core (Weede 2002:152; Van Ark and Timmer 2003:130; Pike, Rodriguez-Pose, and Tomaneý 2006:78), and no less an authority than Gerschenkron (1962) derided the “routine and prejudice” (p. 24) that militated against rational profit seeking in the absence of active state support. Insofar as they embrace the assumption of diminishing returns and doubt the universality of individual utility maximization, therefore, Gerschenkronian approaches are readily portrayed as more sophisticated versions of—rather than damning challenges to—modernization theory (Kindleberger 1963:361; Renton 2004:189; Breznitz 2006:3; Thomas 2006:409; cf. Szepter 2011:76).⁵

But Gerschenkron’s successors tend to assume that rationality is universal and backwardness a liability (Gootenberg 2001:60) and therefore have more in common with neo-Marxism than modernization theory. “Obviously, Gerschenkron’s catch-up postulate only works below a certain threshold,” explained the late Alice Amsden (1992:56) by way of illustration. “Below it, the laws of industrialization change.” The problem lies not in the realm of values or culture, however, for late developers have been immersed in a “global culture of modernism” (Amsden 2001:75) for more than a hundred years. The problem lies in the fact that the laws of late industrialization “do not reward backwardness and do not permit technology transfer alone to close huge productivity gaps” (Amsden 1992:56). And “post-Gerschenkronian” (Austin 2010:55) proponents of the developmental state are therefore best portrayed as heirs to neo-Marxism.

The neo-Marxist affinities are particularly clear in the case of Robert Wade, who at times espouses a “world-systemic perspective” (Wade 2005:17) on inequality, but they are no less present in the work of Amsden, who rejects dependency theory (Amsden 2003) but acknowledges the declining terms of trade for primary commodities first identified by Prebisch, the theory’s intellectual godfather (Amsden 2007:83; see also Hechter 1979:384; Chirot and Hall 1982:90; Barton 2014:38), and invokes Asia’s recent success in part to vindicate his ideas. “Prebisch ‘lives’ in Asia,” she declares, “because leading Asian governments still actively promote import substitution of high-tech parts and components” (Amsden 2004:75).

The point, however, is not simply to distinguish the different approaches to “bringing the state back in” (Evans, Rueschemeyer, and Skocpol 1985) but to note that they bear the distinct scars of their respective predecessors. While Gerschenkronians embrace diminishing returns and treat industrial policy as a “substitute for ‘missing prerequisites,’ in particular a lack of ‘entrepreneurship’” (Crafts 1999:145; see also Austin 2010:54) in the developing world, their descendants doubt both the *inevitability* of diminishing returns and the *efficacy* of entrepreneurship in their absence and thus call for more drastic policy intervention. “Under these circumstances,” says Amsden (1992), “governments have to intervene and deliberately distort prices to stimulate investment and trade. Otherwise industrialization won’t germinate” (p. 53).

In fact, the Gerschenkronians and post-Gerschenkronians not only depart from different assumptions but address different challenges to different countries in different time periods

TABLE 2. Gerschenkronian versus Post-Gerschenkronian Approaches to Late Development

Dimension	Gerschenkronian	Post-Gerschenkronian
<i>Focal time period</i>	18th and 19th centuries	20th and 21st centuries
<i>Focal countries</i>	Peripheral Europe and Japan	Asian NICs, Brazil, India, etc.
<i>Principal development challenge</i>	Capital formation	Learning and upgrading
<i>Policy recommendations</i>	Low-cost loans	Conditional subsidies
<i>Key examples</i>	Chalmers Johnson, Ronald Dore	Alice Amsden, Robert Wade

and, therefore, offer different policy recommendations. The former portray capital formation as the principal development challenge and thus emphasize “not ‘picking winners’ but rather public efforts to mobilize capital” (Kriekhaus 2004:10) in countries like Germany, Russia, and Japan in the nineteenth century. And the latter portray learning as the principal development challenge and thus favor conditional subsidies (e.g., local content requirements, export targets) designed to give contemporary firms the “support-cum-discipline” (Amsden 2001:227) they need to upgrade their products and processes.

The key differences are summarized in table 2 and reflect both the assumptions and analytical postures of their forebears. While Gerschenkronians assume that “backwardness” is an advantage and therefore trace the growth of international inequality to the debility (or dearth) of entrepreneurs in the developing world (Dore 1975:205; Johnson 1987:442; Crafts 1999:145; Crafts and O’Rourke 2014:304–308), their successors treat entrepreneurial behavior as the norm and thus doubt the “advantages of backwardness” in the first place.

Like all social scientific assumptions, the assumptions of self-interested behavior and diminishing returns are simplifications. Neither the possibility of other-regarding behavior nor the potential for increasing returns is in doubt. In fact, the bulk of the available evidence suggests, at a minimum, that self-interest is culturally bounded (see, e.g., Henrich et al. 2010b) and that increasing returns are a feature of many industries and activities (Romer 1993; Warsh 2007).

The question, therefore, is not whether self-interest and diminishing returns are *accurate* assumptions but whether they are *useful* ones. Do they add more light than heat, or obscure more than they reveal? Unfortunately, the answer is anything but obvious. After all, the diminishing-returns assumption is hard to reconcile with the underperformance of most developing countries and is hard put to explain—as opposed to merely describing—which countries constitute the exceptions (Romer 1993; Evans 2008). And the self-interest assumption is hard to reconcile with either the emergence of states that “formulate collective goals instead of allowing officeholders to pursue their individual interests” (Evans 1995:25) or the occurrence of rapid growth in their absence (Önis 1995:109; Brautigam 1997:1070).

For all of their myriad faults, the Parsonian were acutely aware of both the possibility of increasing returns and the limits to self-interest, as were their progenitors in Europe and their descendants in economic sociology. Émile Durkheim ([1893] 2014) therefore emphasized the “non-contractual relationships” (p. 162) and the “moral ties” (p. 216) that underpinned the

division of labor. Mark Granovetter (1985) worried that in their absence “you would be afraid to give the gas station attendant a 20-dollar bill when you had bought only 5-dollars worth of gas” (p. 489; see also Sen 1977; Basu 1983; Chang 2002).⁶ And Parsons himself maintained that “moral norms, which are nonnegotiable, do not follow laws of diminishing returns and resist incorporation into a hedonistic calculus, subverting the logic of both neoclassical economists and behavioristic psychology” (Wiley 1985:192; see also Parsons 1977). But these lessons were for the most part lost on development sociologists in thrall to materialism and rationality, and, ironically, to the extent that they have gained traction in recent years they have done so less in development sociology than in economic sociology, psychology, and behavioral economics. For instance, Joseph Henrich and his colleagues recently reviewed the results of hundreds of cross-cultural behavioral experiments and found that people from Western, Educated, Industrialized, Rich, and Democratic (WEIRD) societies are decidedly more likely to reward cooperation and punish free riding than their counterparts in non-WEIRD societies (Henrich et al. 2010b). While their results have been disseminated in the popular as well as the scientific press (Henrich et al. 2010a; Giridharadas 2010; Watters 2013), and arguably speak to “the strength of formal institutions, norms of civic cooperation, and Gross Domestic Product (GDP) per capita” (Henrich et al. 2010b:70) in the West, they have yet to make their mark in development sociology—though economic sociologists are, at least to some degree, aware of their work (DiMaggio and Markus 2010; Spillman and Strand 2013).

By way of summation, therefore, I have not only highlighted the key differences between the mainstream economic approach to the study of development and the alternatives produced by sociologists in the Cold War era—modernization theory and neo-Marxism—but have portrayed the state-centered accounts that eclipsed the latter in the wake of the Cold War as their unacknowledged offspring. After all, Gerschenkron and his disciples accept diminishing returns but doubt that most actors pursue their self-interest most of the time, and post-Gerschenkronians assume self-interest but question diminishing returns. Table 3 distills the three perspectives into a two-dimensional space and adds a potential fourth perspective—a new economic sociology of development—that would abandon both assumptions.

The warrant for the fourth perspective should by now be obvious. Whatever their historical contributions, the assumptions of self-interest and diminishing returns have themselves reached the point of diminishing returns, and their eclipse—by behavioral economics on the one hand and by the new growth theory on the other—constitutes both an opportunity and a challenge for development sociology. The opportunity lies in the growing realization that mainstream approaches are untenable and that a more sociological alternative therefore holds great promise. But the challenge lies in the fact that economists themselves are beginning to abandon their traditional assumptions and are thus beginning to encroach on sociological terrain. What they have not done, however, is bridge the macro/micro divide that “is institutionalized in economics, from beginning courses in ‘principles of economics’ through to postgraduate studies” (Rodrigo 2012:1)—so behavioral approaches, which are dominated by microeconomists, and the new growth theory, which is the province of macro, are therefore developing on parallel tracks, with little or no cross-fertilization or overlap between

TABLE 3. A Typology of Economic and Sociological Development Theories

Self-interest			
		Assumption	Variable
Diminishing returns	Assumption	Mainstream/neoclassical development economics	Modernization theory and Gerschenkronian accounts
	Variable	Neo-Marxist and post-Gerschenkronian accounts	New economic sociology of development

them. The time is therefore ripe for a sociological counterthrust that would bring a thicker portrait of human behavior to bear in an effort to explain increasing as well as diminishing returns in the twenty-first century, and nowhere is sociological insight more vital than in the debate over the state's role in late development.

A NEW ECONOMIC SOCIOLOGY OF DEVELOPMENT

An effective state is all but indispensable to late development. Some want the state to play an activist role in an effort to capture increasing returns in the periphery. Others want the state to defend laissez-faire in an effort to take advantage of diminishing returns in the core. But the latter are no less aware of the limits to contemporary states than the former. Take, for example, the ostensibly neoliberal economists (Wade 2002:219) at the World Bank. "Even with more selectivity and greater reliance on the citizenry and on private firms," they declare in an influential report on the state's role in the development process, "meeting a broad range of collective needs more effectively will still mean making the state's central institutions work better" (World Bank 1997:3) than they do in most developing countries. With less selectivity and more reliance on fiat, of course, the state's job will be harder still, and the report therefore goes on to draw a distinction "between institution-intensive and institution-light approaches to regulation and industrial policy, stressing how the choice of approaches might appropriately vary with a country's institutional capability" (p. 75).

What determines a country's institutional capability? The self-interest assumption encourages the Bank to emphasize merit-based recruitment and promotion criteria that not only "winnow the more skilled from the less skilled applicants" (p. 93) for public sector jobs but give the former an incentive to do their jobs effectively *ex post facto*. "Where instead promotions are personalized or politicized," the Bank explains, "civil servants worry more about pleasing their superiors or influential politicians, and efforts to build prestige through tough recruitment standards are undercut" (p. 93; see also World Bank 1993:175).

Of course, the World Bank is neither alone nor offbase in advocating meritocratic personnel policies. The merits of meritocracy have been demonstrated time and again by qualitative investigators (Wade 1990; Evans 1995) as well as quantitative investigators (Evans and Rauch 1999; Henderson et al. 2007; Dahlström, Lapuente, and Teorell 2012; Arezki and Quintyn 2013), and merit-based management has thus been portrayed as virtual prerequisite to good governance in the developing world (see, e.g., Chaocheng 2012:100; Bugarcic 2014:154; Rodrik 2014:99). But the self-interest assumption that provides the rationale for

merit-based measures leaves a key question unanswered: *If public officials are self-interested utility maximizers, why do they embrace meritocratic institutions that tie their own hands in the first place?* Durkheim invoked the “non-contractual elements of contract” in an effort to highlight the innate tension between individual self-interest and institution building. “In exact analogy,” explains Dietrich Rueschemeyer (1986), “we can speak of the *non-bureaucratic elements of bureaucracy*. An effective program of institution-building must reshape the goals, priorities and commitments of at least the core participants and inculcate shared assumptions, attitudes and value commitments on which rational organizational action can be based” (p. 59). The nonbureaucratic elements of bureaucracy are unlikely to be forged by mutual agreement, however, in response to a policy imperative or crisis, and Rueschemeyer therefore goes on to imply that state building is an “uncertain and protracted process” (p. 60; see also Rueschemeyer and Evans 1985; Rueschemeyer 2005).⁷

What are the origins and nature of the nonbureaucratic elements of bureaucracy? While Rueschemeyer (2005) never develops a full-fledged theory, he notes that developed market economies are marked by “norms, values, and understandings” that not only are “conducive to effective collective action that is independent of close personal and kin ties” but “are institutionally and organizationally grounded, both in state and in society, in education, in the economy, and so on” (p. 159), and his answer therefore anticipates and enriches the aforementioned literature on WEIRD people and their tendency toward prosocial behavior (see, e.g., Henrich et al. 2010b; Bowles and Gintis 2011). “In more developed countries,” Rueschemeyer (2005) explains, “the purposive creation of formal organizations becomes a routine option, even though we know from organizational sociology that the interplay between formal and informal organization and of legal and nonlegal social control is critical for a smooth functioning of the new unit; and it is clear that working these interactions out may take some time” (159–60).

Rueschemeyer’s account thus serves as a bridge, of sorts, between the more sophisticated variants of modernization theory and the contemporary literature on political development. After all, Clark Kerr and his colleagues (1960) turned “Marx on his head” by portraying educated professionals and managers, rather than industrial workers, as the “vanguard of the future” (p. 39), and worried that their efforts to build a “meritocracy based on technical skill” would be derailed not by the “machine-breakers” who exercised Marx and his contemporaries but by “expert-breakers” drawn from the lower classes (Kerr 1969:115). Ernest Gellner (1983) placed the professor, rather than the executioner, at the “base of the modern social order” (p. 34) and held that by the late twentieth century the “monopoly of legitimate education” had replaced the “monopoly of legitimate violence” as the basis of state power. And Rueschemeyer (1986) placed “competent personnel” at the heart of his own analysis of the nonbureaucratic elements of bureaucracy, paying particularly careful attention to their common origins, educations, careers paths, and privileges (pp. 59–60).

The point, however, is less to defend education’s (relatively uncontroversial) contribution to political development than to better specify the mechanism. While political scientists hold that the marginal utilities of immediate, particularistic payoffs are higher for less skilled or educated voters than for their more skilled or educated counterparts

(see, e.g., Kitschelt 2000:857; Calvo and Murillo 2004:743; Cleary and Stokes 2006:148; Calvo and Murillo 2012:16; Weitz-Shapiro 2014:12), and thereby offer a rational choice account of the link between education and meritocracy, their critics point to a number of problems in their account, including not only the fact that the alleged quid pro quo is hard to monitor in the world of the secret ballot and hard to demonstrate in practice (see, e.g., Auyero 2000) but the fact that voting itself is irrational under all but the most forgiving definitions of the term (see, e.g., Meehl 1977; Margolis 1982; Dubner and Levitt 2005).⁸ The alternative therefore holds that middle-class or professional support for merit-based management, and opposition to personalism and pork, derive neither exclusively nor primarily from self-interest but from “middle-class values and identities” (Ozarow 2014:181) that are at least nominally—and at times sincerely—sympathetic to the ideal of meritocracy and hostile to patronage and personalism (see, e.g., Dick 1985; Owensby 1999; Silva 2009).

The obvious problem is that “education is too good a variable” in that “it explains too much” (Stinchcombe 1974:105) of the variation in question. “Knowing how many things influence the amount of education, or are influenced by it,” argues Arthur Stinchcombe (1974), “it would be rather surprising to find an attitude that was not related to education” (p. 105; see also Stinchcombe 1965:150–51). Nor is he alone. On the contrary, Aaron Benavot and Phyllis Riddle (1988) note that rates of school enrollment not only predict educational attainment but are themselves indicators of “the extent to which young people are organized in rational, bureaucratic frameworks” (p. 193). Joel Migdal (1988) agrees that they capture the state’s ability to “mobilize the population into state institutions and rules” (p. 286; see also Palloni 1981:641–42) more generally. And Edward Glaeser and Bruce Sacerdote (2008) explain the seemingly paradoxical correlation between education and church attendance in the United States by noting that “education is correlated with all forms of formal social activity” (p. 205).

The methodological challenges thereby imposed are more formidable than overwhelming, however, for the fact that many, if not all, “good things go together” (Hughes 2001:451) in the course of development is hardly surprising, and over the years social scientists have expended considerable energy developing research designs and indicators to unpack their individual causes and consequences (see, e.g., Meyer 1977, esp. pp. 70–71; Lieberson and Horwich 2008; Kurtz and Schrank 2012a, 2012b). But the implications are of theoretical as well as methodological import, for the apparent synergy of education, rationalization, and mobilization implies that state building not only demands but facilitates *prosocial behavior* and, in so doing, fosters *increasing returns* that are both indispensable to the new economic sociology of development and incompatible with the neoclassical account. While the feedback loops thereby engendered spell good news for the advanced industrial countries, where virtuous circles have long been the norm, they are anathema to their less developed counterparts for corresponding reasons. “Once institutions take hold,” explains Peter Evans (2004), “they are likely to endure even if they have a long-run negative effect on development, limiting the likelihood that more efficacious institutions will emerge” (p. 32).

The challenge, therefore, is to understand the production of prosocial behavior that staves off diminishing returns by “culture-producing institutions” (R. Collins 1981:1930)

more generally. While educational and religious institutions are by far the most familiar, they are by no means unique, for the attitudes, values, and preferences that shape market—and, for that matter, nonmarket—exchange are produced by unions, guilds, political parties, bureaucracies, the military, the media, courts, and corporations, among others, and are susceptible to study at both the micro and the meso levels. Jeffrey Haydu's (2008) study of the development and transposition of "standards of merit and nonpartisanship" (p. 142) between professional and political organizations in the United States during the Progressive Era offers one example. Herbert Kaufman's (1960) classic account of the U.S. Forest Service's efforts to build a "coherent pattern" (p. 39) out of tens of thousands of agents dispersed across millions of acres of territory performing hundreds of different jobs offers another.

Unfortunately, however, similar studies of developing country actors and organizations have been neither common nor influential in North American and European sociology. The discipline as a whole is parochial (Lie 1995), and insofar as northern sociologists have turned their attention to the global South, they have viewed it through the lenses of grand paradigms that operate at a fairly low level of resolution.⁹ But middle-range studies of organizations, institutions, and fields are relatively common in economic sociology more generally, and they have much to offer the developing world, for neither the emergence nor the study of "effective state action" presupposes "a global change in attitudes, value orientations, and understandings across the whole population," according to Rueschemeyer (2005:151). "Changes that make for mutually productive exchanges between state and civil society are likely to be restricted to certain enclaves where groups have interests that can be served by interaction with the state as well as orientations that are compatible with such interaction." Where do these enclaves emerge? Which groups constitute their stakeholders? What are their interests and value orientations? Do their efforts to build and exploit state capacity at the enclave level spill over or scale up to the macro level over time? Why, why not, and to what effect? By exploiting core sociological concepts like—but not limited to—roles (Turner 2006), reciprocity (Gouldner 1960), reference groups (Merton 1957), embeddedness (Granovetter 1985), networks (Powell 1990), brokerage (Stovel and Shaw 2012), and isomorphism (Meyer and Rowan 1977; DiMaggio and Powell 1983), I believe, development sociologists can go a long way toward answering these questions and, in so doing, can bring their subfield back into the heart of both development theory and the discipline as a whole.

By way of summation, therefore, I have argued that the assumptions of self-interest and diminishing returns that underpin the neoclassical development model are neither empirically nor theoretically warranted; that the leading sociological alternatives have been compromised by their failure to make a clean break with the neoclassical assumptions; that a more propitious alternative would recognize that political and economic development presupposes norms of fairness, trust, and cooperation that are incompatible with self-interest and subject to increasing returns; that the origins and consequences of these norms deserve more attention than they have received to date; and that sociology as a discipline, and economic sociology in particular, boasts a powerful tool kit with which to make sense of the normative underpinnings of development success and failure.

CONCLUSION

Robert Solow (1970) claims that discussions of growth and development invariably end up in a “blaze of amateur sociology” (p. 103; see also Krugman 1991:95), and his fellow economists have been quick not only to echo but to substantiate his claim by engaging in “amateur sociology” of their own (Krugman 1995:718). While professional sociologists have been at best indifferent and at worst openly hostile toward the amateurs, they would do well to take their rivals seriously. After all, the emergence of sociological thinking among economists—manifest in behavioral approaches and the new growth theory, in particular—is not only a threat to sociology’s “jurisdictional monopoly” (Abbott 1988:25) over noninstrumental behavior and nonequilibrium outcomes but a product of sociology’s failure to challenge economics head on in the second half of the twentieth century. I argue that sociologists, by clinging to one of the neoclassical assumptions (i.e., self-interest or diminishing returns) as they abandoned the other, developed a number of powerful critiques that would ultimately constitute less than the sums of their individual parts.

The time is therefore right for a new approach that would abandon both of the neoclassical assumptions simultaneously and, in so doing, would open the door to a richer portrait of individual and group life, and their consequences, in the developing world. While economic sociologists have the tools required to build the new approach, and would thus seem well positioned to engineer a sociological counterthrust, they have for the most part shied away from the developing world—preferring to work in North America or Europe. The unfortunate result is a gap between economic sociologists, who have tools that would be invaluable to the study of development but have almost no interest in the global South, and development sociologists, who are committed to the global South but tend to ignore the tools they will need to make a difference there.

Nowhere are the consequences more obvious than in the World Bank’s most recent World Development Report, which is entitled *Mind, Society, and Behavior*: it is described as being designed to transcend “the standard economic model” (World Bank 2014b:29) of human behavior, and it is almost devoid of references to the work of actual sociologists. While the latter will almost certainly cry foul and blame their marginalization on the Bank’s domination by mainstream economists, they are at least in part responsible for their own fate. After all, the report’s editors wanted to go *beyond* the mainstream economic models that, I have argued, continue to infect development sociology, and it is therefore unsurprising that the sociologists who *do* garner attention in the report are for the most part identified not with the development subfield but with economic sociology more generally (e.g., Granovetter, DiMaggio, Powell).

What is to be done? A new economic sociology of development would bridge the gap between economic sociology and development sociology by bringing core insights from the former to bear on the latter and, in so doing, would address problems of fundamental sociological, as well as moral, importance. Few sociological subfields could be more fertile. Few social problems could be more important. And few intellectual opportunities could therefore be greater. ■

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NOTES

1. Stinchcombe (1965) traces the link between Protestantism and capitalism to the former's emphasis on literacy, which arguably constitutes a cultural innovation. "Protestants, especially sectarians who had to read the book themselves, were much more literate during early industrialization than Catholics" (p. 151), he argues. See also Gellner (1983:142) for a broadly similar account.
2. Crowley et al. (1998) discuss the alleged consequences of FDI in light of mainstream, endogenous, and radical growth theories.
3. See Dore (1973) and Evans (1979) for noteworthy exceptions. The latter's discussion of the cultural constraints on rationality (p. 36) is particularly insightful.
4. See Gerschenkron (1978:76) on the conceptualization and measurement of relative backwardness and Bendix (1967) for a "sociological extension of points first suggested by" (p. 316 n. 55) Gerschenkron's work on economic history.
5. See in particular Kindleberger (1989) on Gerschenkron's inability to "differentiate his notion of discontinuity, sometimes called 'the big spurt,' from Rostow's take-off" (pp. 13–14).
6. See also Gerhardt (2002) on the link between Durkheim and Granovetter through Parsons and "generalized morality" (p. 37).
7. Rueschmeyer's point was underscored for me in February 2015, when I interviewed a former Latin American head of state about a public health agency that had by all accounts improved dramatically on his watch. When asked how the improvement had occurred, he began with the appointment of a trusted associate who put together a "good team" to run the agency. (Having interviewed the associate in question, and several of her team members, in late 2014, I knew that upon being appointed she too had surrounded herself with trusted colleagues of long standing.) When asked if the reforms would

survive her eventual departure, he replied, “Good question,” and acknowledged his doubts. And when asked what, if anything, he could have done to solidify the reforms, he repeated, “Good question,” and acknowledged that he could have imposed “protocols and the like,” but that he could not guarantee that they would have been respected by his successors (February 11, 2015).

8. Proponents of rational choice accounts tend to place a good deal of faith in doubts about ballot secrecy, whether they are warranted or not. For instance, Susan Stokes (2005) highlights the “widespread perception among Argentine voters and party operatives that voting is a less than fully anonymous act” (p. 317). But Latinobarometer data from 2006 implied that Argentines who expressed confidence in the country’s electoral institutions were decidedly more likely to vote for the incumbent Peronists in the upcoming elections (38 percent) than Argentines who expressed doubts (26 percent)—a finding that not only is statistically significant ($X^2 = 15.83$; $p < .0001$) but is at least superficially hard to reconcile with an account that emphasizes fear of retribution among rational voters.

9. Sociology is by no means alone in this regard. See Luna et al. (2014) for a discussion of similar problems in political science.