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How the World Became Rich by Mark Koyama and Jared Rubin and Slouching Towards Utopia, by J. Bradford DeLong: A Review Essay

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ABSTRACT

This essay provides a review of two important recent books on economic growth: How the World Became Rich by Mark Koyama and Jared Rubin and Slouching Towards Utopia, by J. Bradford DeLong. Each book is noteworthy for its erudition and breadth. I explore strengths and weaknesses of these books and make some proposals on new ways to conceptualize and study long run socioeconomic development. My discussion emphasizes the importance of contingency in determining long run inequalities across countries as well the potential for ideas from complexity theory to augment standard growth modelling.

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How the World Became Rich by Mark Koyama and Jared Rubin and Slouching Towards Utopia, by J. Bradford DeLong: A Review Essay

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I. Introduction

Scholars, teachers, and students will profit from two overviews of economic growth that appeared in 2022: *How the Word Became Rich*, by Mark Koyama and Jared Rubin, and *Slouching Towards Utopia* by J. Bradford DeLong. These books share a clear and enjoyable exposition. Both are brimming with fascinating details. Most important, each book takes an integrative conceptual approach to understanding growth, one that explores a range of growth mechanisms. This common integrative approach also may be seen in the way that evidence is amassed to interpret growth patterns and explanations via qualitative and quantitative sources of knowledge. One suspects this modus operandi reflects the fact that all three authors are economic historians and so reason from broad patterns to explanations, without demanding that this reasoning is initiated through the lens of formal models. The economic historian's perspective also explains a related shared virtue of the books-a focus on explaining particular events using proximate (in time) causes as opposed to claims about "deep roots" of economic development and growth. While deep roots explanations such as geography, institutions, and culture are important parts of the books' narratives, in particular that of Koyama and Rubin, they are used to explain specific phenomena such as differences in patterns of European industrialization. Put differently, facts are not marshalled to demonstrate the universal applicability of a particular grand theory.

The two books naturally complement each other because of their differing time scales. Koyama and Rubin focus on slower-moving phenomena, in particular the transition to the Industrial Revolution, than does DeLong, whose book is meant to the economic history of the long 20th century, 1870-2010 (with some attention to the 21st). As a result, while Koyama and Rubin's longer time scale leads them to emphasize (though not exclusively) the mechanisms that explain the patterns of industrialization and the dynamics towards sustained growth, DeLong gives much more attention to the role of contingencies in explaining the specifics of

the last 150 or so years.

There is an obvious large challenge in producing a general interest book on growth that may be read with value by the layperson and the scholar alike. How can one present a vision of the path towards modern economic development that simultaneously describes the important empirical contours of these changes and provides a serious interpretative framework that respects the complexity of the phenomena under study? To an unhealthy extent, the economic growth literature does not provide such a vision. It is perhaps inevitable that researchers and research programs within growth prioritize specific theories. Too often, particular growth explanations are presented as theories of everything rather than as important determinants or mechanisms. In addition, the literature often conflates causes that originate in the far past with the idea that these causes are ultimate explanations and somehow more fundamental to understanding growth than temporally proximate ones. These books generally avoid such pitfalls. *How the World Became Rich* navigates the growth literature with remarkable evenhandedness, to the extent that it can serve as an introductory course text. *Slouching Towards Utopia* is so wide-ranging in its historical discussions that a purely evenhanded account would be intellectually desiccated, but it is admirably broad in its discussion of growth and distributional dynamics. So, it is easy to say that layman and scholar will find much to enjoy in each book.

II. The Books

How the World Became Rich

Koyama and Rubin explore five explanations of the rise of the Industrial Revolution and the rise of modern prosperity. Four of these are examples of deep roots: geography, institutions, demography, and culture. One other explanation is also considered: colonialism and exploitation, differs from these deep roots as it is a manifestation of the other factors. With reference to all of these, the authors are careful to avoid determinism and are exceptionally skillful in indicating strengths and weaknesses. A good example of nuances of their exposition is demography. Koyama and Rubin show the strength of the argument that the demographic transition associated with the Second Industrial Revolution helped sustain per capita income

growth is appropriately defended, while at the same time explaining why claims that declines in fertility triggered the First Industrial Revolution are acknowledged to be problematic, though the position is properly not dismissed altogether. A second example is colonialism and exploitation, where the authors give a superb explanation of what I think is the correct position that while the slave trade and colonial extraction contributed to the divergence of the West from other regions, the classic 1944 thesis of Eric Williams in *Capitalism and Slavery* that the profits of slavery were essential for the Industrial Revolution is incorrect.[1]

Koyama and Rubin's evenhandedness comes at the cost that a reader may come away from thinking that the story of economic growth is one damned explanation after another. Koyama and Rubin try to avoid this but not with complete success; consider their discussion of why the Industrial Revolution started in the United Kingdom. They do a terrific job of eschewing monocausal theories per se but are less successful in clarifying how different mechanisms interact. At the end, Koyama and Rubin conclude

> *…why did the Industrial Revolution take place when and where it did? We identified many preconditions that Britain had on the eve of industrialization (around 1750). These included relatively limited and representative governance, a large domestic economy, access to the Atlantic economies, and a large base of highly skilled mechanical workers. Other places had some of these prerequisites, but only Britain had all of them. (175)*

The seriatim presentation of mechanisms that appear in the British case does not provide a principled way of understanding how they interact. Why should one think that industrialization is produced by a Leontief meta-technology that links these factors? To be fair, the main reason for this limitation in their analysis is the absence of an integrative theoretical framework in growth theory.

These observations lead me to the first of the two inadequacies I found in *How the World Became Rich*. First, despite the major focus in the book on why Britain came first, there is a lack of attention to France, as opposed to comparisons with the Netherlands, where the authors are excellent. The lacuna is surprising in light of what Koyama and Rubin have to say about culture:

[1]See Heblich, Redding, and Voth (2022) for a recent study that illustrates the contours the effects of these factors is still a matter of debate.

> *…none of these explanations can account for why British entrepreneurs were so adept at taking advantage of recent advances in science. This is the last part of the puzzle in need of explanation. (171)*

Joel Mokyr's (2016) idea of the enlightened economy is invoked, which Koyama and Rubin describe as

> *…the role that cultural elites of the Enlightenment played in propelling European- and particularly British-technological innovation. (171)*

The French origins of the Enlightenment and its fundamental role in the nation's political evolution and revolution are of course standard ideas[2] and, along these dimensions, one can argue France was the world leader. And the effects of Enlightenment ideas on France mattered in terms of factors that matter for industrialization. As documented in the definitive studies Gillispie (2004a,2004b), France was arguably the world leader in science in the late 17th and early 18th centuries. Second, the Revolutionary era itself links the Enlightenment to a host of dramatic reforms starting with the abolition of the Ancien Régime on August 4, 1789; many of these achievements are well summarized in Woloch (1994). One achievement is the creation of a national public educational system and the creation of institutions such as the École Polytechnique.[3] Other reforms include a new patent system and the abolition of many privileges for the aristocracy. As shown by

Acemoglu, Cantoni, Johnson, and Robinson (2011), the "scientific hubris" that the revolutionary reforms, which plausibly may be linked to Enlightenment ideas, had major effects on economic development in places where they were forcibly exported by French armies. In recent work, Chambru, Henry, and Marx (2022) demonstrate the powerful consequences of the Revolution's effects on state capacity for France itself.

These facts about the Enlightenment and France all suggest that France should have

[2]The importance of the Enlightenment in the French Revolution remains controversial. Baker (2011) and Linton (2006) are useful surveys. Modern work on the ideological origins of the French Revolution, stemming from Darnton (1995), emphasizes a much more complicated matrix of ideas than those traditionally associated with Rousseau and elite thinkers. This parallels arguments in Mokyr about the broad diffusion of ideas.

[3]Interestingly, Hayek (1952) singles out the École Polytechnique as the source of much of the wellspring of inappropriate government interventions.

been the world leader in industrialization. There are, of course, many reasons that have been given as to why the UK industrialized before France and this has been a subject of debate for decades. This debate has even ranged into questioning whether the UK was indeed first, cf. Roehl (1976). Among recent work, an important contribution is Kelly, Mokyr, and Ó Gráda (2014), which makes a compelling case that the comparatively good physical condition and high mechanical skills possessed by UK workers, as opposed to France and elsewhere, allowed and incentivized the adoption of new technologies in ways that explain why Britain was first; see as well Kelly, Mokyr, and Ó Gráda (2023). Nevertheless, one would be hard pressed to argue a scholarly consensus has emerged and as I have argued, the timing of Britain versus France is puzzling for various reasons. Even if I am wrong, a full delineation of France/UK differences and an explanation of why Britain rather than France needed to render any story of modern economies complete.

A related but distinct limit to their discussion of timing is Koyama and Rubin's failure to adequately discuss the role of contingency. Crafts (1977, 1995) makes the argument that there was no preordained set of reasons why Britain's industrialization preceded that of France. He argues, for example, that the timing of specific inventions was a matter of luck, even if the conditional probabilities of their having occurred differed in the two countries. Is this a distinction without a difference? I think not. For one thing, the differences in the conditional probabilities need to be assessed if one wants to argue that mechanisms rather than chance are the main reason for the observed differences.

The second gap in the book concerns the lack of sufficient discussion of the Scientific Revolution. I concur with Hodgson (1993) that the Scientific Revolution represents the fundamental divergence between Europe and the rest of the world. I therefore think the focus on why the UK industrialized first is secondary to the larger question of how modern science emerges in Europe.[4] While Koyama and Rubin do not ignore this, they say little beyond commenting on the inadequacies of conventional explanations of the stagnation of science in China. It is strange that endogenous growth theory as initiated in Romer (1986, 1990) and broad idea of the autocatalytic generations of ideas and innovations is ignored here.[5] While

[4]This is distinct from the debate over role of applied science in triggering the First Industrial Revolution, for example Jacob (2014) versus Ó Gráda (2016).

[5]Paul Romer himself appears unmentioned in either text. My impression is that, more

there is hardly any consensus in the history of science about the Scientific Revolution (with some questioning whether "revolution" is even the right way to think about the evolution of science in this period), I believe the preponderance of evidence and argumentation makes clear that something extraordinary happened in Europe in the 17th and 18th centuries that is fundamental to any story of modern affluence.[6]

Slouching Towards Utopia

Slouching Towards Utopia complements the mechanisms discussed in *How the World Became Rich* by focusing on three explanations for modern prosperity that are more proximate than those examined by Koyama and Rubin. In DeLong's telling,

> *From the techno-economic point of view, 1870-2010 was the age of the industrial research lab and bureaucratic corporation. One gathered communities of engineering practice to supercharge economic growth, the other to deploy the fruits of invention. It was only slightly less the age of globalization: cheap ocean and rail transport that destroyed distance as a cost factor and allowed humans in enormous numbers to seek better lives, along with communications that allowed us to talk across the world in real time. (17)*

As such, I think DeLong provides an important addition to conventional deep roots approaches to growth and thus provides a valuable additional perspective on the microfoundations of growth-facilitating institutions.

DeLong makes two additional correctives to standard growth narratives that I think are even more important. First, he really does emphasize the importance of contingency in

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generally, Romer's thinking is substantively neglected, as opposed to perfunctorily cited, in historical growth surveys such as these two books. I think one reason for this is that some of the empirical implications of the first generation of endogenous growth models were shown to fail to track various time series properties of cross-country growth patterns; Jones (1995a,1995b) are seminal in this regard. (Another is the emergence of the deep roots literature in growth economics.) But the importance of his general idea that interactions between ideas beget new ideas, is independent of particular empirical failures. In my view, the failures involve ways the evolution of ideas is embodied in particular mathematical structures and an excess emphasis on steady states versus transitions, and an inadequate consideration of nonstationarities in the stochastic processes underlying growth.

[6]Cohen (2012) is a magisterial account.

explaining the contemporary world. Contingencies can range from the World War I to the Russian Revolution to recent events such as the Bush-Gore presidential election; DeLong's discussion of World War I is especially insightful. Why is this so important? Here is one example. It is common to start analyses with figures that reveal bivariate relationships between one's preferred development theory and per capita income or a similar measure. The slope of the bivariate relationship is then employed as a motivation for a deeper theoretical and empirical exploration, the empirical one often amounting to a multivariate regression that includes the development theory, but this time with a host of controls.

To see how misleading such comparisons may be, consider the Cuban missile crisis. It is now evident, cf. Sherwin (2020) and Plokhy (2021), that an all-out nuclear war was avoided by luck, whatever assessment is made of the actions of the US and USSR leadership in stepping back from the brink. Had this occurred, many of these bivariate relations to deep roots would have the opposite signs than what proved to be the case; the story of modern development would be one of the Industrial Revolution leading to self- destruction of richer and more scientifically advanced societies. And of course, DeLong's own proximate explanations would have had opposite predictions had such contingencies been resolved in a different direction in terms of relative rankings of countries; see Durlauf (2023) for elaboration.

Second, DeLong emphasizes the history of the distribution of the fruits of unprecedented growth as integral to the modern growth story. He provides an interesting, albeit idiosyncratic (in the sense of ignoring the massive historiographical literature, which is essentially unreferenced) view of the rise of the modern welfare state and the partial retreat from it associated with so-called neoliberalism. Delong's insistence on coupling growth and distribution in understanding the modern economy should be applauded. It provides the grand vision that makes work on distributional growth accounting such as Piketty, Saez, and Zucman (2018) and Smith, Yagan, Zidar, and Zwick (2019) so important. While distributional accounting involves complicated measurement questions even beyond the ones known from traditional growth accounting, so that the work in this area is still emerging, this does not diminish its importance.

One great virtue of DeLong's discussion of the distributional issues is that it leads him to place ideas at the center of the story. This matters both for assessing the wellbeing of unprecedented growth and because it is a path to understanding the government policies

that jointly determine (and are determined by) growth and distribution. DeLong frames much of the evolution of policies as mirroring the contrasts between the ideas of Friedrich Hayek and Karl Polanyi on the meaning of the market and the requirements that market outcomes conform to notions of social justice.

One criticism of this framing is that more attention should have been given to the evolution of ideas in this "great debate." In particular, Hayek is just the (foundational) beginning of a body of thought. Gerald Gaus (2007, 2016, 2021) has enriched the Hayekian approach in important ways, by emphasizing the value of markets in adjudicating diverse values of different citizens. Gaus (2007, 2021) explicitly links Hayek to complex systems theory thereby providing a novel perspective to arguments on why government interventions may not be predictable.[7] While I am unsympathetic to much of the Hayekian perspective on policy, it seems to me his position is stronger than portrayed.

A second criticism is that the book really does not establish that Polanyi's ideas warrant the prominence DeLong gives them to sustain his narrative. The comparative irrelevance of Polanyi in contemporary political philosophy, as opposed to Hayek, is a problem for the book's narrative and not really explained.[8] Substantively, all a reader is given of Polanyi's thought is a claim he believed in some set of economic rights and the claim that Polanyi believed that moral beliefs of society should constrain the market and that the failure to do so could create political instability. It would have been more effective to contrast Hayek with a social liberal, such as Leonard Hobhouse, or for that matter John Rawls (I don't think temporal proximity to Hayek that important). Or one could have compared Hayek with representatives of the French and German liberal traditions; as emphasized by Rosenblatt

[7]I think Gaus is not fully correct in this defense of Hayek. Complex systems can exhibit universal properties, so that the qualitative effects of certain types of policies to be assessed without knowledge of fine-grained details of the environment. This seems a route to developing a theory of robust growth and development policies that is analogous (although based on different mathematical foundations) to the work of Hansen and Sargent (2007). See Durlauf (2012) for details.

[8]This irrelevance is suggested by the fact that the index of Rawls (1971) has no reference to Polanyi, nor indices in classic critiques of Rawls, such as Sandel (1982) from the communitarian perspective or Mills (2017) from the perspective of Rawls' racial liberalism and failure to engage in nonideal theory. To be fair, Hayek does engage with Polanyi. To be fair, these authors really do not much, if at all, engage with Hayek; but here I think the reason is that Nozick provides a substitute to the extent libertarian ideas are engaged.

(2018), traditions had relatively little sympathy for laissez-faire liberalism. One could just as well have used classic socialist critiques of liberalism to the similar effect. It was hard for me to discern any special Polanyi vision that gives unique clarity to DeLong's stories about the dynamics of social democracy and neoliberalism. References to murky Polanyian rights do not elucidate political disagreements any more than other ways to describe ideological conflicts over the economy. To my eyes, the introduction of Polanyi has some rhetorical but little substantive value.

A distinct limitation is that DeLong's links between ideas and the dynamics of policy transition from social democracy to neoliberalism gives too little attention to deep cultural values as opposed to the ideological debates of the left and right. At least for the United States, I think his story has too much contingency, i.e. focusing on the slowdown in economic growth after the 1960s, and the role of bad policies, while giving inadequate attention to aspects of the American character. When Cowie (2016) describes the end of the New Deal coalition as

> *Labor declined, the ideology of individualism became central to all things, race divided politics, tensions over immigration returned, and the state slipped back to being hostile to workers' rights. (27)*

he is referring to the social democratic period as an aberration from low-frequency aspects of American culture, one rife with racial and ethnic divisions that have worked against class-based politics and deep-grained attitudes about individualism and skepticism about government. These low frequency dimensions of course are often discussed in the context of the historical absence of a major socialist party in the US. Explanations on this absence, ranging from Sombart (1906) to Foner (1984) to Lipset and Marks (2001), surely bear on the dynamics that DeLong wishes to address. Generally, while DeLong's history of ideas and policy is engaging, it is unsystematic and to my eyes, is not careful about phenomena that operate at different time scales such as culture and politics. In his defense, there are no good formal frameworks that achieve this integration. It is less understandable that he is so unwilling to engage with many historians, sociologists, and political scientists who have studied this topic.

Finally, the centrality of Hayek/Polanyi market debates to DeLong's discussion of

ideas misses important dimensions of the normative and positive underpinnings of policy debates. While possibly useful for understanding the emergence of neoliberalism, it misses much in terms of the past and current egalitarian controversies. A different framing would involve the evolution of ideas about social justice and the assessment of policies due to different conceptions of fairness and desert, asking how consensus has ebbed and flowed along normative and positive dimensions, and how these intellectual dynamics map into the evolution of policy. This perspective is meant as a complement to the Polanyi/Hayek social goods/market axes of the narrative. This complement, to my mind, is required to fully integrate growth and distribution in the ways that DeLong wants. It also resonates with the debates about markets and regulation and liberalism versus socialism in terms of procedural and substantive justice.

Consider the concept of equality of opportunity as developed by Richard Arnesen and John Roemer, among others; well surveyed in Roemer and Trannoy (2016), as modified by Fishkin (2014). Roemer's work emphasizes that equality of opportunity for jobs or education and the like is determined by whether inequality in outcomes is determined by factors for which a person should be held responsible versus those for which they should not. Fishkin's key advance is to recognize that the structure of opportunities can either be narrow or broad; bottlenecks represent aspects of the socioeconomic structure that few can move through. In other words, equal opportunities should be combined with a rich set of possible good outcomes. The admissions rules for a school such as the University of Chicago can embody one type of equal opportunity fairness, but the size of a UC Berkeley opens a bottleneck when compared to Chicago.

These concepts could have been used to frame the evolution of many of the fundamental policies that address inequality. DeLong does address equality of opportunity in his chapter "Inclusion", but this discussion does not link well with his discussion of social welfare states and in my reading becomes too secondary a perspective. I see another postwar intellectual history on the role of government in addressing inequality from that given by DeLong. Here, the great story of 20th century policies would be the rolling back of morally arbitrary barriers to opportunity due to ethnicity, race, gender, or religion that have defined the evolution of equality. For the US, this history would center policy evolution on the transition from Jim Crow laws and the many discriminatory laws and regulations of non-

Southern states and the Federal government to the antidiscrimination law of the present day. This trajectory could be paralleled by an interpretation of the history of education policy, one that emphasized the decoupling of educational opportunities from the socioeconomic status of one's parents and community and in turn would lead to affirmative action as a tool to combat inequalities. Yet another dimension to consider is the emergence of civil service examinations and the professionalism of government agencies. While the Imperial examination system of China may date from the Tang dynasty, William Gladstone's 1870 Order in Council to open civil servant positions in the UK and The Pendleton Civil Service Reform Act of 1883 fall in the beginning of DeLong's long 20th Century. This framing naturally segues with debates on reparations, comprehensively advocated by Darity and Mullen (2020) and Darity, Mullen, and Hubbard (2023).

The inclusion perspective naturally leads to questions of the social determinants of inequalities which links persistent differences across groups. As such, these are not questions about the efficiency of markets per se. They involve the time series properties of individual and group outcomes, specifically whether processes are ergodic, i.e. there is long run convergence, and, if so, what is the speed of convergence. One mechanism for nonconvergence or slow convergence involves the sorts of spillover effects that Paul Romer emphasized, albeit at the community level,[9] as well as intercommunity interactions; this is where discrimination, segregation, and prejudice come into play. Investments in children can produce social increasing returns in education via peer effects; Sacerdote (2011) contains much evidence. There is important evidence, for which Sampson, Raudenbush, and Earls (1997) is a classic example, showing that neighborhood quality in Chicago is affected by collective efficacy, which refers to levels of social cohesion and the willingness to contribute to public goods. Further, neighborhood segregation creates deep inequalities in exposures to various harms. Manduca and Sampson (2019) demonstrate how in Chicago, neighborhoods with Black majorities expose children to qualitatively higher neighborhood toxicity, measured by exposure to lead, violence, and high rates of incarceration among adults. When these influences are strong enough, inequality is not self-correcting across generations, or does so extremely slowly. Durlauf (1993, 1996) provides a theoretical

[9]Loury (1977) is a remarkable early contribution to this line of thinking.

framework demonstrating these possibilities.

Social influences are examples of the mechanisms that underlie what I have called the memberships theory of inequality (Durlauf, 2002, 2006).[10] The memberships theory is based on the claim that many socioeconomic disparities derive from the processes by which families are formed, children are located in neighborhoods and schools, and workers are located in firms. Markets matter, but the key features of these processes are matching and the possibility that the social groupings exhibit segregation. Further, race, ethnicity, and gender interact with these "endogenous" groupings to create a complex matrix of forces that produce persistent inequalities at the individual levels.

A focus on equality and breadth of opportunity through the memberships theory covers much of the domain of currently contested areas of egalitarian policies. For example, many education debates involve affirmative action, free college, curriculum choice, school vouchers, charter and magnet schools, all of which involve memberships. These debates involve positive questions about the determinants of contemporary inequalities and rates of mobility within the life course and across generations as well as normative questions that fall outside the domain of Hayek and Polanyi. To give one example, many of these frontiers of policy debate involve the meaning and ethical significance of meritocracy when jobs and school admissions reflect the current effects of past injustices.[11] The Hayek/Polanyi dichotomy does not add much to one's understanding of disagreements on inequality and policy along the axes of the demands of justice in addressing current and especially past

[10]The memberships theory of inequality possesses important commonalties with the work ideas surrounding categorical inequality (Tilly, 1998) and to stratification economics (Darity, 2022), each of which implies the same value to the organizational principle I propose for 20th century economic history/policy debates as an alternative to the Hayek versus Polanyi dichotomy.

[11]Guinier (2015) and Sandel (2020) challenge meritocracy as a social goal. I believe much of the content of these critiques may be addressed by employing suitably prospective version of meritocracy as developed in Durlauf (2008). In my judgment, meritocratic college admissions maximize a social welfare function where the value added to individual students of match, accounting for social interactions that occur in college and the intergenerational effects of contemporary admissions, determines who should get in, as opposed to a backwards-looking rule that equates merit with rewards for past accomplishment. This is exactly the sort of division that is relevant in understanding an important axis of policy disagreements.

violations of equality of opportunity and is thus an inadequate lens through which to understand important recent policy dynamics. Many past and contemporary policy debates may be interpreted by disagreements on the respective roles of responsibility and desert in evaluating whether individuals in fact have equal opportunities.[12]

The memberships perspective can also help in understanding recent history. As emphasized by DeLong, inequality waxes and wanes; certainly the 21st century increases in inequality tell a very different story from the very optimistic strands I have emphasized for the 20th. Here, Tilly's (1988) idea of opportunity hoarding (see Reeves (2017) for a wide-ranging discussion) is important. Another related set of issues involves the evolution of technology from organizations such as the Ford Motor Company which, in most of its history, integrates workers of different classes and skill levels, to Apple, which does not. DeLong does a nice job of discussing how technological change has reduced the need for the "geographic concentration" of (p.475) factors of production. This "unbundling," to use a phrase by Richard Baldwin, is another way to understand how memberships determine inequality.

I end with a general substantive criticism of the book: the treatment of the Soviet Union. First, there is surprisingly little engagement with one of the obvious seminal events of a 20th century economic history: the decline and collapse of the Soviet Union. An anecdote by Yegor Gaidar is not an explanation of an event that should be a primary focus in a story that emphasizes the role of ideas and politics in economics and purports to be an overarching history of 20th century growth. In my opinion, the correct way to frame the question is not *Why did the Soviet Union collapse?*, but *Why did the Soviet Union survive so long given the absence of markets and other institutions so integral to Western success?* Here DeLong should have taken more seriously the role of ideas and ideals in the Soviet case. While the Stakhanovite movement may be as much myth and propaganda as reality (Siegelbaum, 1988) it is very much the case that belief in communism and the building of a better world mattered in the preservation of the Soviet Union for as long as it existed; see Kotkin (1995) for a remarkable study of the development of Magnitogorsk by Stalin, arguing

[12]Roemer (1998, 2003) combine to provide the best overview of his approach to responsibility-sensitive egalitarianism. Sher (1987) is valuable in defending the legitimacy of desert as a feature of distributive justice.

> *Nowhere was the euphoric sense of the revolution's renewed possibilities in the 1930s more in evidence than at the Magnetic Mountain. (54)*

In turn, Hollander (1999) is compelling on the role of disillusionment in the end of the regime.[13] The Soviet experience is a critical part of any 20th century economic history. It is also surprising that DeLong is mute on the post-Communist transition of Russia. Had he engaged with this catastrophe and the role of terrible economic policies, he might have identified a different set of economists to castigate for policy advocacy based on rigid ideology than the usual University of Chicago suspects.

A more systematic exploration of the economic failures of the Soviet Union would have strengthened some of Delong's arguments. A clear example of the importance of ideas that Delong correct emphasizes is the general weakness of the ideas that animated the Lenin and Bolsheviks from the start. Here I think Delong in fact errs in neglecting the misunderstandings that were foundational to the thinking of Lenin and comrades. Consider Delong's claim that Germany's government interventions in the economy during World War I were an inspiration for War Communism. The evidence for this amounts to a quotation from Lenin, taken from Boettke (1988). But this quotation is not really mean much. Lenin had already developed dogmatic beliefs that finance capitalism created the means for an immediate successful takeover of the economy under a Communist regime. Harding (1981) explains:

> *The direct continuity between Lenin's economic analysis of imperialism and his conception of the content and mechanisms of socialist construction is immediately apparent in his emphases upon the role of big banks. Just as they had been the organizational foci of finance capital so, under the direction of a socialist state, they would be transformed into a single, universal agency of accounting and control over the entire systems of production and distribution. (130)*

and describes Lenin's thinking as "derived" (53) from Rudolf Hilferding's *Finance Capital*, published in 1914. Boettke (1988) emphasizes this view of Lenin as well. Lenin's views on monopoly and finance capitalism led him to believe nonsense about the feasibility of

[13]Any systematic examination of the end of the Soviet Union also must engage with the effects of World War II on the beliefs and normative universe of Soviet citizens, e.g. Weiner (2001), and its role in sustaining the postwar system.

successful nonmarket economic organization under his rule. DeLong's discussion of Lenin's thinking on German wartime experiences does not distinguish claims that were made in debates evidence about the efficacy of policies with his deep political and economic commitments and his fundamental misunderstandings of what makes advanced capitalist economies function. Given Delong's emphasis on ideas, this seems a lost opportunity.

Further, Delong's ideas on the modern corporation and industrial laboratories would be all the stronger if ought to have been used as the basis for a broader examination of the reasons for the economic successes of the West and the failures of the Soviet Union and its satellites. The scientific achievements of the Soviet Union are too well known to require any summary here. The relevant aspect of these, with reference to DeLong's focus, is the staggering failure of these achievements to translate into economic productivity. An easy explanation is that the USSR directed science in channels oriented toward defense, but this is far too narrow an explanation. The deeper explanation is that the Soviet system essentially decoupled science and technology so that scientific advances were not linked to the civilian economy in a way analogous to the commercialization of scientific advances that occurred in the West. Part of this failure involved the fact that Soviet mathematics and scientific developments tended to be concentrated in areas that did not require equipment (Borjas & Doran, 2012), which naturally delimited effects on productivity. Beyond economic constraints, Soviet science was decoupled from technology and economic activities in ways that were inconceivable in the US. Whatever comparisons one wishes to make between Soviet and American science, Route 128 and the Silicon Valley were unique to the US. At the risk of oversimplifying such a rich body of ideas, Joel Mokyr's thinking may be interpreted as arguing that the interplay of science and technology via commercialization is a key factor in the industrial revolution and the same arguments apply here.

This absence of commercialization was matched by the absence of information flows in the Soviet system, which spilled over into the scientific domain. Graham (2013) provides a remarkable documentation of how Soviet scientists preceded Western counterparts in areas such as transistors and cybernetics, with these ideas (and sometimes their authors) dying in obscurity. Secrecy was embedded in all dimensions of Soviet society. Harrison (2023), while focusing on the effects of secrecy on state capacity, makes clear how efficiency was impeded in all areas of Soviet society. The ecology of scientific knowledge did not, in

such cases of scientific achievement, produce growth via the autocatalytic processes of idea creation underlying endogenous growth theory.

III. What is to be Done?

Turning from the books to a broader set of questions they raise, *How the World Became Rich* and *Slouching Towards Utopia* are also valuable as they hint at areas of inadequacies in the state of growth economics. As emphasized earlier, contemporary formal growth models have not achieved a successful integration of the array of mechanisms found in Koyama and Rubin and in DeLong. Nor do they really capture the sort of conclusions drawn by Mokyr (1990), which I think stand the test of time.

> *Analysing the long-term record of technological progress suggests that it is difficult to identify factors that are either necessary or sufficient. The best we can do is to identify elements that can inhibit it: destructive civil wars and invasions, religious persecution, and prolonged epidemics and famines were incompatible with sustained progress. Apart from this…Resource-rich societies found ways to create new technologies that used their resources; resource-poor societies found ways to save on expensive resources, or invented ways to transport them cheaply. Societies with scarce labor produced labor-saving machinery, while societies with high population densities found ways to save and reclaim land from seas and deserts. Still other societies, with similar endowments of labor and resources, simply lived with their allocations and resigned themselves to a standard of living determined solely by population size and nature. (116)*

I conclude this essay by proposing that conventional theoretical and empirical practice in growth would benefit from some rethinking. I speculate on what a more explicit attention to high-dimensional nonlinear stochastic processes might add to growth economics. It is a banality to start with the proposition that national growth experiences are stochastic processes involving interactions across many actors defined at different levels of aggregation, from families to firms to governments. My claim is this perspective calls into question some conventional forms of growth modelling.

The stochastic processes I have in mind include complex systems but are not confined to these. Rather, I am interested in three properties these models can produce that, at least metaphorically, speak to broad features of growth and development: multiple steady states,

emergence, and phase transition.

-Multiplicity occurs for two reasons: the interdependences between different elements at a given time and because of interdependences between elements at different points in time. These interdependences, when strong enough, can produce multiple steady or metastable states.[14]

-Emergent properties of environments are those defined at a higher level of aggregation than the objects that produce them. The state of water is an emergent property of many water molecules.

-Phase transition occurs in systems that are characterized by parameters such that there are thresholds in the space of parameter values such that the behavior of the system qualitatively changes as the threshold is crossed. Water exhibits a phase transition at 0 degrees centigrade.

The characteristics of institutions and the forms of culture norms, beliefs, and practices are natural candidates for emergent and universal properties of societies. Each of these is essentially definitionally a property of a population, not an individual and so are self-evidently emergent. History of course repeatedly demonstrates how different configurations of types of individuals in a population, historical experiences, and geography can produce similar institutions. Acemoglu and Robinson (2012) contain much discussion of this type. The ability of qualitatively different characteristics of countries to produce common institutional features, is consistent with a vision of institutions as emergent phenomena whose features are robust to certain perturbations of these characteristics.

What substantive import do these metaphors provide to the study of growth? I see several messages:

First, if growth is a function of emergent phenomena such as inclusive institutions, then there is no sense that the determinants of these emergent properties are deeper explanations than the properties themselves. This implies a different way to think about deep roots versus more proximate causes. For example, consider Galor (2022)'s arguments on genetic diversity. To the extent it plays a role in growth (and I severely criticize these claims in Durlauf

[14]Metastable states are configurations that take an extremely long time to leave, even if they are ultimately transient.

(2023), genetic diversity is at best an input into a socioeconomic environment that helps determine the emergent properties of institutions and culture. Similar things may be said about geography. Distant roots are not deep in a substantively meaningful sense. Beyond the theoretical implications, this formulation also suggests that the search for evidence of growth determinants becomes less credible the farther back one goes in time, when emergent phenomena constitute the proximate factors of interest, and nonlinearities are present; the arguments in Durlauf (2023) along these lines with respect to linear models apply *a fortiori* here.

A second message is that periods of rapid institutional change can coexist with periods of stasis. This is the import of a system exhibiting phase transitions. Similarly, complex systems can be thought of as addressing contingencies in a natural fashion. Large shocks, such as the realization of the Cuban missile crisis, are straightforward to interpret. Perhaps more interesting, one can think of factors such as institutions, culture, ideology as creating the preconditions that determine the range of shocks that can produce a phase transition. This is evident when one considers the French Revolution, where the specifics of the bankruptcy of the French government because of aid to the fledgling United States and political errors such as the King's flight to Varennes, are plausibly interpretable as fundamentally important because the system was near a phase transition given the background determined by the exclusivity of French institutions and diffusion of Enlightenment ideas. Similarly, this is the import of Crafts' arguments about contingency and invention at the beginning of the Industrial Revolution. Here I think the key idea is what Gell Mann (2015) has called "frozen accidents"-events that because of positive feedbacks interdependences, lock a system into one of multiple possible long- term modes of behavior. Put differently, contingency is especially important when the parameters of a system place it near a phase transition.[15] Further, it is the case that lower frequency variables can place a system near a phase transition while higher frequency ones induce a phase transition.

These considerations matter for empirical work. For example, a different way to organize the study of long-term growth is to make the identification of these events integral

[15]As such, the idea of phase transitions has parallels to the rich discussion in Collier and Munck (2022) of critical junctures.

to empirical work; this in turn will help elucidate mechanisms. Frozen accidents, multiple equilibria, and the like all imply that the data under study exhibit deep dependences, possibly leading to nonergodicity, and growth analysis needs to respect this possibility. These are not insuperable problems. For example, models that are nonergodic at the aggregate level may be identified using data measured at a more disaggregated level to reveal the parameters that produce aggregate nonergodicity-this is the basis for the positive identification results in Brock and Durlauf (2001, 2008).

To give another example, the current practice of starting with linear additive models for growth and then exploring ad hoc types of nonlinearities and interactions between explanations is unlikely to capture the substantive phenomena I have described. In other words, I am advocating basing empirical work on statistical models that embed phenomena such as multiple steady states and phase transitions. The natural objection is that I am retrograde in my willingness to conduct empirical work based on particular functional forms. I am happy to plead guilty. The data limits we face require compromises. And I prefer a strategy of starting with rich, theoretically motivated functional forms and then exploring robustness to one where the forms are an ex post modification of a linear model or so nonparametric as to invoke the curse of dimensionality.

Note that all of my arguments imply the inadequacy of the ubiquitous (for economists) causality framework for studying growth; this is not the world of meaningful counterfactuals. The work in economic history on path dependence, e.g. David (1985), has been most persuasive when focused on thick descriptions of particular episodes to analyze path dependence. Beyond the importance of sources of evidence, it may be illusory to think that any statistical growth model will capture the full panoply of growth determinants and the nature of their interactions. The best we will be able to do is to consider sets of low-dimension approximations and see how each is corroborated by available data, with inference to the best explanation, aka abduction representing the best way to evaluate evidence.[16]

A natural response to these observations is that researchers (including me) have been arguing for decades in favor of using complex systems models as a language for social

[16]See Lipton (2004) for general philosophical background and Heckman and Singer (2017) for discussion of abduction in economics.

science, and that they have failed to make a persuasive case.[17] There is much merit in that view. The reasons for this failure are twofold. First, complex systems mathematics can never substitute for good substantive social science and all too often credible or interesting microfoundations were sacrificed in order to employ the mathematics. By this, I mean that agents were too often modelled as following ad hoc behavioral rules which rendered the implications of their interactions uninteresting.[18] Second, this work did not have strong links between theory and empirics, so that properties of models as explanations for broad phenomena often never left the realm of metaphors and the few quantitative exercises failed to take identification seriously (Durlauf, 2005). So, it would be too much to say that I have described a road not traveled; at best this is a suggestion for a road not yet constructed.

IV. Conclusions

To conclude I end with where I began. *How the World Became Rich* and *Slouching Towards Utopia* are well worth reading. The books reflect the complexities of the growth process and

[17]Within growth economics, a number of complexity perspectives appeared in the 1990s and early 2000s: Durlauf (1993), Kelly (1997), Weitzman (1998), and Auerswald, Kauffman, Lobo, and Shell (2000). Arthur (1989) and Krugman (1996) pursued these ideas as well but were focused on technology adoption and urban structure respectively. While some of these are well cited, the citations are pro forma and the work cannot be said to have materially affected growth economics. This does not mean future work will be so limited. I note here more recent work on the complexity growth nexus. Network ideas have also been linked to growth, via the evolution to commodity spaces in interesting work by Hausmann and Hidalgo (2008) and Hartmann, Guevara, Jara- Figueroa, Aristaran, and Hidalgo (2017) articulate a theory of growth based on information and network evolution. Bettencourt (2021) provides a remarkable vision of city evolution and the associated rich dynamics of growth, opportunity, and inequality that jointly evolve. This work follows the physics approach to modelling, starting with empirical regularities and then constructing of formal models in which the regularities naturally emerge. While I have not fully absorbed its lessons, I am confident in saying economists will be the poorer if this work is not studied carefully. It also important to note that work such as Abry, Bastidon, Borgnat, Jensen, and Parent et al (2022) is reviving work on the complexity/economic history interface that originated with W. Brian Arthur and especially Paul David.

[18]This is not an objection to the fact that complex systems models almost universally have avoided assumptions of rational expectations in modelling behaviors where beliefs about the future matter, but rather to cases where the individuals are acting in ways that are uninteresting, because, for example, arbitrage opportunities go unexploited. Blume (2005) is unique in careful modelling of forward-looking agents in evolutionary games.

the richness of the subject that theories must address. No reader will come away from these books without a sense of the many successes of historians and social scientists in advancing knowledge of the development of the modern world as well as some of the controversies. While I disagree with a number of the claims and judgments made in each book, such disagreements are the stuff of normal academic debate over contested issues.

To my eyes, the books also reflect a continuing divide between growth theory and growth empirics and reflect some of the limitations in how growth is conceptualized and operationalized by economists. While empirical and theoretical growth research engages and responds to each other, the field is far from achieving a synthesis. Beyond this, as I have argued, conventional practice on each side fails to fully address issues involving the high-dimensional and nonlinear nature of the growth process. Both the student and the scholar can read these books and appreciate the progress in understanding economic growth while simultaneously feeling excitement about what may yet be accomplished.

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