Preliminary Draft- Comments Welcome

Economic History, Historical Analysis, and the "New History of Capitalism"

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Abstract: This paper presents a critical survey of ten books from the history of capitalism, a newly emerging subfield of history. The books include Sven Beckert's *Empire of Cotton*, Edward Bapitst's *The Half Has Never Been Told*, and others on finance, risk, and conservative economic doctrines. The critical perspective of this new literature, which emphasizes the human costs of economic development, distinguishes it from the field of economic history. At their best, the books offer provocative insights and vivid descriptions of some of the darker episodes of our economic past. Yet their neglect of social scientific methods and lack of engagement with the economic history literature undermines their analysis and their effectiveness as social criticism. In this paper I highlight insights from the field of economic history that would strengthen the future work of historians of capitalism. I also suggest some questions that might create opportunities for cross-pollination, if not collaboration, between the two communities of scholars.

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I. Introduction

Economic history was once a deeply interdisciplinary field. The Economic History Association was founded by members of both the American Historical Association and the American Economic Association, and the early volumes of the *Journal of Economic History* included numerous contributions by historians. But over time, as historians' interest gravitated toward cultural topics, and as the Cliometrics Revolution led to the incorporation of quantitative methods and economic theory into the field, it came to be dominated by economists.[1] Today, the *Journal of Economic History* and the association's annual conference include few contributions from scholars trained in history departments. Research in economic history has flourished in their absence, and the growing accessibility of historical sources such as census microdata, coupled with technological advances such as those related to geospatial information systems (GIS), have led to rapid advances in the field. Important questions that would have been unimaginably difficult to address in a convincing way just two decades ago are being answered conclusively. Yet for all its methodological sophistication, some of the work by economic historians would have benefitted from the insights and criticisms of scholars trained in history departments, and the absence of many historians from the field has limited its intellectual diversity and the depth of its analysis.

The past decade has seen a resurgence of interest in economic history among historians. Whether it is the product of waning interest in other topics, or a desire to understand the historical forces behind the recent crisis and Great Recession, many young historians have chosen to focus on economic history in their work. Particularly among Americanists, this shift is gathering momentum, and works on economic history have been among the most celebrated books produced by historians in recent years. Yet this new wave of research has not brought historians back to the academic field of economic history. The scholars producing it do not think of themselves as participants in the field, and have worked in isolation from it. Instead, many of them have styled themselves "historians of capitalism." A separate community of economic historians seems to be emerging.

[1] Lamoreaux (2015) describes the EHA's founding and evolution. Goldin (1995) chronicles the Cliometrics Revolution and its legacy.

In this paper I present a survey of ten prominent books by the new historians of capitalism, which focus on a variety of topics including finance, slavery, and conservative economic doctrines. My aims are to critically assess their contributions, and to point out works that may be of interest to economic historians. In response to some of the weaknesses I identify, I also highlight insights from the work of economic historians that would strengthen the analysis of historians of capitalism, and what their books bring to economic history. Finally, I suggest some research questions that might create opportunities for cross-pollination, if not collaboration, between the work of economic historians and historians of capitalism. My hope is that this paper will begin a much-needed conversation between the two communities of scholars.

In selecting the works to discuss in this essay, I encountered the problem that the boundaries of the field of the history of capitalism are not well defined, and it is sometimes unclear whether historians working on economic history topics intend their work to be part of it. The name "history of capitalism" could be interpreted to embrace any work of historical analysis that is focused on capitalist economies, and an early overview of the field by one its foremost practitioners, Sven Beckert of Harvard University, defined it in this way (Beckert, 2011). Yet this definition is so broad that it includes most of the fields of labor, political, social, and business history, as well as economic history.[2] And in spite of the popularity of the new brand, some historians working on economic history topics do not wish to adopt it and prefer to identify with other fields such as business history. The history of capitalism is not simply work on economic history topics by scholars trained in history departments. It is work with a particular perspective, and it is this perspective which I believe defines the field, and distinguishes it from the field of economic history, or from business history.

This is the significance of the name: rather than adopting the approach of the field of economic history, which seeks to analyze historical economic behavior, historians of capitalism aspire to something closer to social criticism, and focus on the human costs associated with historical economic

[2] Indeed, Beckert's (2011) essay, which is something of a catalogue raisonné of the history of capitalism as it stood at the time, lists economic historians Naomi Lamoreaux and Gavin Wright as among the historians of capitalism.

Table 1:
Works Discussed in this Essay

Slavery, Cotton and Capitalism

Beckert, Sven. 2015. *Empire of Cotton: A Global History*. Cambridge: Harvard University Press.

Baptist, Edward. 2014. *The Half Has Never Been Told: Slavery and the Making of American Capitalism.* New York: Basic Books.

Johnson, Walter. 2013. *River of Dark Dreams: Slavery and Empire in the Cotton Kingdom*. Cambridge: Harvard University Press.

Finance, Risk and Insurance

Levy, Jonathan. 2012. *Freaks of Fortune: The Emerging World of Risk in America*. Cambridge: Harvard University Press.

White, Richard. 2011. *Railroaded: The Transcontinentals and the Making of America*. New York: W.W. Norton.

Hyman, Louis. 2011. *Debtor Nation: The History of America in Red Ink*. Princeton: Princeton University Press.

Ott, Julia. 2011. *When Wall Street Met Main Street: The Quest for an Investor's Democracy*. Cambridge: Harvard University Press.

Conservative Economic Doctrines

Phillips-Fein, Kim. 2009. *Invisible Hands: The Businessmen's Crusade Against the New Deal*. New York: W.W. Norton

Burns, Jennifer. 2009. *Goddess of the Market: Ayn Rand and the American Right.* New York: Oxford University Press.

Moreton, Bethany. 2009. *To Serve God and Wal-Mart: The Making of Christian Free Enterprise*. Cambridge: Harvard University Press.

change.[3] The "history of capitalism" would seem to denote a research agenda focused on writing the history of capitalism, but this does not characterize the new literature well. Most of the scholars producing it are in fact Americanists, and many are focused on the twentieth century. With few exceptions, this literature is not concerned with the early origins or deep institutional foundations of capitalism, and does not seek to analyze the full evolution of capitalism over time. None of the books even define what is meant by capitalism. But then again, capitalism itself is not actually the subject of their analysis. Instead, these works present a critical analysis of economic development in the context of capitalist institutions. The name "history of capitalism" calls attention to the presence of capitalism, but it should mainly be seen as signifying critical social analysis by historians.

The ten books discussed in this paper are listed in Table 1, in the order in which they appear in this essay. I selected the books with input from historians of capitalism and from consultations of reading

[3] Rosenthal (2016) mentions an "activist impulse" that seems to animate this new work.

lists of courses in the field.[4] All of them are well-regarded, and several have won one or more major prizes. Although not a comprehensive list, the ten books are quite representative of the field. I have grouped them into three broad topics: slavery, cotton and capitalism; finance, risk and insurance; and conservative economic doctrines.

Together these books present a critical account of the development of the American economy. There is America's original sin, slavery, and the forcible expropriation of native peoples. There is an ever-growing financial sector, crony capitalism, and struggles over the allocation of newly created risks. And, in the twentieth century, there is a backlash against the New Deal and the welfare state, and the rise of economic doctrines and political ideologies friendly to business interests.

To understand the perspective of historians of capitalism, one has to recognize two sources of influence that figure prominently in much of their work. The first is Karl Polanyi's *The Great Transformation: The Political and Economic Origins of Our Time* (1944). Although Polanyi was himself an economic historian and an occasional contributor to the *Journal of Economic History*, his work is not influential among economic historians today. Written partly in response to the work of the Austrian School economist Ludwig von Mises, *The Great Transformation* makes several arguments that are clearly reflected in the work of historians of capitalism. Polanyi's analysis proceeds from the insight that market economies can only be understood by considering their "embeddedness" within their social and political contexts, which may in fact be hostile to market forces. As we will see, many of the historians of capitalism have focused their analysis on those political and social contexts, rather than on markets themselves.

Polanyi presents an account of the Industrial Revolution and the emergence of national markets and capitalism in Britain, and argues that these developments were made possible only by a series of interventions by the state to dismantle feudal institutions and protect manufacturers through tariffs and other subsidies. This insight enabled Polanyi to point out a fallacy in arguments against government

[4] I have not directly confirmed whether each of the authors considers herself or himself a historian of capitalism, and at least one (Johnson) does not. But I do know that each of these books is considered a part of the new literature.

"interventions" in markets made by his contemporaries on the right, such as von Mises: they presume that markets are natural institutions, whereas "there was nothing natural about *laissez-faire*; free markets could never have come into being merely by allowing things to take their course…*laissez-faire* was enforced by the state."[5] Although most of the historians of capitalism are not focused on the early emergence of capitalism, they are deeply concerned with interactions between the state and markets, and they share Polanyi's perspective that the state plays a constitutive role.

Polanyi also argued that unfettered markets are inherently unstable and pose great "perils to society," which made the transition to capitalism a "double movement." One movement was aimed at the establishment of capitalism itself, but the other was a "countermovement" aimed at social protection, which used various forms of legislation and "restrictive associations" to mitigate the impact of capitalist development.[6] Polanyi's arguments about the ravages of unfettered markets on society resonate within all of the books of historians of capitalism.

The second major influence on historians of capitalism is the recent financial crisis and Great Recession. These events stimulated interest in questions related to the history of the economy, and may have influenced some historians of capitalism. Although many of the books discussed in this essay were already in preparation when the crisis occurred, the context in which they were written likely colored the perspective of their authors. Many of the books emphasize the darker side of capitalism and its propensity for crises, and this contributes to their distinctive perspective.

At their best, the books discussed in this essay offer provocative insights, and present vivid descriptions of historical contexts drawn from archival sources. Most of the research of economic historians focuses on questions originating in economic theory, which tend to be quite narrow. In contrast, these books present expansive narratives and explore questions and concepts that may not be amenable to the analytical tools of economists. The critical perspective of these authors also distinguishes their work from that of economic historians, and makes it relevant to the concerns of many popular

[5] P. 249; p. 139.
[6] P. 71; p. 132.

readers. The historians of capitalism remind us rightly that economic growth and development can have human costs not captured in average incomes; that our economic history includes no small measure of cruelty, coercion and expropriation, rather than free exchanges occurring in the context of secure property rights; and that the economic system we have today is not a natural condition, but the outcome of policy choices that could have been made differently.

Yet the critical perspective of these works sometimes distracts their focus away from rigorous historical analysis. Too frequently, the historians of capitalism present arguments without examining the validity of their assumptions, or exploring alternative explanations of their implications. None of the books makes any attempt to falsify major elements of their arguments. In some cases the authors seem to have arrived at insights that are consistent with evidence obtained from some historical sources without investigating them any further. As intuitively appealing as these insights may seem, they sometimes rest on specious reasoning. Their neglect of social scientific methods undermines the accuracy of their analysis and their effectiveness as social criticism.

For example, in their zeal to highlight the consequences of capitalist development, the historians of capitalism tend to exaggerate the degree of change that has occurred over time. The analytical frameworks of many of these books draw a sharp distinction between a "modern" or "capitalist" or "developed" period, and what came before. In arguing for the unprecedented nature of the developments they observe, the authors sometimes assume that the period prior to their study was fundamentally different. Yet often the phenomena presented as new had clear historical antecedents, and a careful investigation of the connections between periods would have enriched the analysis significantly.

The influence of the recent crisis and Great Recession in these works also creates something of a pitfall for their analysis. Just as poor historical analogies can distort our understanding of the present, modern analogies can produce fallacious or unsound historical analysis if misapplied. Although financial development often leads to volatility, and although venality and corruption among financiers seems to be as close to a historical constant as one can find, not all finance is harmful. In fact the financial sector performs a vitally important function, but the nature of that function makes it particularly vulnerable to manipulation, fraud, and instability. The growth of the financial sector over the long run has been made possible only by institutional developments—some imposed as regulations, some developed within the financial sector itself—which have sought to bring these problems under control. The books of the historians of capitalism include scarcely any recognition of these concepts.

A closely related problem with these books is that their authors have failed to engage with the economic history literature. Economic historians have produced sophisticated analyses of the issues of interest to historians of capitalism, even if they have approached them from a different perspective. Ignoring the insights of those works has led historians of capitalism to make assertions that have long been refuted, and to get important elements of their arguments wrong. In some cases, they have unwittingly revived old debates without reference to any of the evidence that has been produced. Later in this essay, I describe some analytical techniques and insights from the field of economic history that I believe would strengthen the work of historians of capitalism. But before describing those insights, I first comment on each of the ten books individually.

II. Discussion of the Books

A. Books on Slavery, Cotton and Capitalism

Among the most prominent works of historians of capitalism are three books on cotton slavery. Perhaps not surprisingly given their topic, these books present the bleakest vision of American history, and emphasize the vast cruelty and coercion in nineteenth century economic life. Yet slavery has also been one of the most important topics in the field of economic history, and among all the books of the historians of capitalism, these would have benefitted the most from greater engagement with the economic history literature.[7]

[7] Economic historians have already begun to critique these works; see Olmstead and Rhode (2016) and Majewski (2015) in particular. On Baptist's *The Half Has Never Been Told*, see the roundtable of reviews in the *Journal of Economic History* (Murray et al, 2015). These works have also been critiqued from within the field of history; see, for example, Oakes (2016).

The most ambitious of these is Sven Beckert's *Empire of Cotton: A Global History.* This book presents an international account of the causes and consequences of the Industrial Revolution, but it does so by focusing exclusively on cotton.[8] The great strength of the book is its account of the spread of the Industrial Revolution into different regions of the world, as it follows the adoption of new textile machinery within many different countries and regions. Beckert's account of this process is quite consistent with the ideas of Polanyi, and emphasizes that the role of the state in "[f]orging markets, protecting domestic industry, creating tools to raise revenues, policing borders, and fostering changes that allowed for the mobilization of wage workers" was crucial for industrialization to occur. Protectionist trade policy and the Napoleonic blockades are shown to be particularly important.[9]

Beckert argues the colonial empires of the European powers, with their heavy reliance on the violent expropriation of indigenous peoples and on slavery, constituted an early phase of capitalism, which he denotes "war capitalism." One of the main arguments of the book is that the industrial revolution and the "industrial capitalism" it spawned were made possible by war capitalism. Thus Beckert presents a theory of the industrial revolution that emphasizes its origins in "slavery, colonial domination, militarized trade, and land expropriations" (p. 60).

Any theory of the industrial revolution needs to address the question: Why did it begin when and where it did? Why 1780s Britain and not Amsterdam, or for that matter Song China? Beckert identifies a number of different mechanisms by which war capitalism enabled industrial capitalism to develop in Britain in the 1780s. Like Eric Williams in *Capitalism and Slavery* (1944), Beckert argues that the fortunes made in the slave trade and on sugar plantations in the Caribbean created a source of finance for the innovations in cotton textile production at the center of the Industrial Revolution.[10] But he also argues that the trading networks within the colonial empire were critically important: the British

[8] Scholars interested in the historical significance of cotton should also read Reillo (2013), which has unfortunately been overshadowed by Beckert's book.
[9] Research by the economic historian Réka Juhász (2016) offers strong empirical support for the importance of the Napoleonic Blockade in stimulating industrialization in France.
[10] Somewhat surprisingly, Beckert does not feature the work of Eric Williams very prominently in his book. He does, however, draw on the work of Joseph Inikori (2002), which offers the strongest recent defense of the Williams hypothesis.

merchants of the 1780s were uniquely positioned to exploit the vast global market for cotton textiles, since they "dominated the transoceanic trade in cottons, and they had firsthand knowledge of the fabulous potential wealth that could come from selling cloth" (p. 64). War capitalism therefore created both a demand-driven inducement to expand textile production, and also helped provide the wherewithal on the supply side to develop new technologies to serve that demand.

Research by economic historians casts doubt on some of these arguments, and lends support to others. For example, Eltis and Engerman's (2000) careful study concluded that relative to other British industries at the time of the Industrial Revolution, the total value added and growth-inducing linkages of the British slave trade and Caribbean slave plantations were not particularly large.[11] On the other hand, Beckert's emphasis on the role of trade in the growth of the Industrial Revolution is quite consistent with the theoretical model of Findlay and O'Rourke (2007). But more importantly, the vast literature on the industrial revolution that economic historians have produced shows that it originated in the creation and adoption of a wide range of technologies, such as the steam engine and coke blast furnace, which were not directly connected to textile trading networks. The steam engine rather awkwardly appears out of nowhere in Beckert's cotton-focused account, whereas its emergence is explained coherently by research that emphasizes British labor and energy costs, or the scientific advances associated with the enlightenment.[12]

Although some of the new technologies of the Industrial Revolution may not have been closely linked to war capitalism, the slave plantations of the Western hemisphere certainly did provide an elastic supply of raw cotton to European manufacturers, which fed the textile industry's expansion. Like

[11] As Eltis and Engerman (2000) note, if there was some element of the slave trade or colonial slave plantations that was essential to industrialization, then one might have expected the Industrial Revolution to have begun in Portugal, whose colony Brazil accounted for a much greater share of its economic activity, and which had much more extensive links to the slave trade.

[12] Allen (2009) argues that the high wages and low energy costs that prevailed in eighteenth century Britain induced industrialists there to create and adopt labor-saving machines that made intensive use of energy, such as the steam engine and the water frame. Beckert acknowledges a role for high labor costs, but argues that "the demand for machines ultimately derived from the existence of vast markets for cotton goods and the ability of British capitalists to serve them" (p. 468 n. 10). Mokyr (2010) attributes these innovations to the rise of ideas resulting from the Enlightenment, particularly the resulting scientific advances of the eighteenth century and their effects on the outlook of British entrepreneurs. Beckert doesn't directly address the latter, but he is dismissive of theories of the Industrial Revolution focused on "Enlightenment traditions" (p. xiv).

Kenneth Pomerantz's *The Great Divergence*, Beckert's book emphasizes the importance of colonial land holdings in enabling the British economy to expand beyond its ecological limits.[13] Initially, sugar plantations in the Caribbean and South America converted some of their lands into cotton production. The industry's rapidly growing demand for cotton then fueled what Beckert characterizes as war capitalism within the United States, as the native peoples of the South were violently driven into other regions, and cotton production using slave labor quickly spread into those territories. Beckert also argues that rapid productivity growth in cotton production during the first half of the nineteenth century was due in part to a "systematic intensification of exploitation" of American slaves.[14]

The role of cotton slavery in America's economic development is the focus of Edward Baptist's *The Half Has Never Been Told: Slavery and the Making of American Capitalism.* This book seeks to challenge the perception that slavery was somehow separate from the rest of the economy, and argues that it was crucial to the development of American capitalism. But it also seeks to tell the story of cotton slavery in a way that focuses on the lived experiences of the enslaved people themselves, and the cruelties they endured.

Drawing on the narratives of escaped slaves and the papers of slave owners, Baptist presents a detailed portrait of many elements of the southern economy. Considerable attention is devoted to the internal slave trade and the movement of slaves into the Mississippi River delta region, but there is also a lot of detail on plantation life, finance and banking, religious practice, and political debates over the expansion of slave territory, among many other topics. There is a particularly engaging account of musical and social rituals on plantations, which Baptist uses to argue that enslaved African-Americans were the "true modernists, the real geniuses" of their time (p. 163). Baptist also plays with the language

[13] See Pomerantz (2000), p. 46, 264-69. An alternative perspective on the Great Divergence that does not emphasize colonial land holdings is presented in Rosenthal and Wong (2011).
[14] P. 115. In making this argument, Beckert cites the work of Edward Baptist, discussed next.

used by slave owners to characterize themselves and their slaves in an effort to explore different dimensions of slaveholders' power. Some of these passages are insightful, some less so.[15]

Yet the focus of *The Half Has Never Been Told* is not cultural but economic, and much of its economic analysis is so flawed that it undermines the credibility of the book. For example, Baptist makes the astonishing claim that *all* of the increase in productivity in cotton picking observed by economic historians in the antebellum era was due to increases in the whipping and torturing of slaves. This claim enables him to make the central argument of the book, which is that the "ultimate cause" of the Industrial Revolution was the "systematized torture" of American slaves (p. 135, 141; see also 413). Notwithstanding the fact that the Industrial Revolution began before American slaves were producing cotton in any significant quantities, Baptist's claim regarding torture and productivity is false, and the evidence he offers in support of it consists of a selective account of the quantitative and narrative record.[16] Baptist also offers a calculation of the contributions of cotton slavery to American GDP in 1836, from which he concludes that "almost half" of economic activity in that year "derived directly or indirectly" from slave-produced cotton (p. 321-22). This is a disastrously mishandled undertaking, full of obvious manipulations that overstate cotton's contribution.[17] And yet Baptist's focus on GDP causes him to miss

[15] An example of the latter: Baptist ascribes particular significance to references to slaves as "hands," arguing that modern readers miss the "non-neutrality" of the term. To Baptist, a hand was "the ideal form of the commodity" of a slave: the malleable embodiment of the master's will. The "handness" of a slave, to Baptist, was the outcome of a process of "handification" (p.100-104). Yet in the nineteenth century many free laborers—members of ships' crews, workers in factories, and laborers on farms, for example—were also referred to as "hands." Rather than calling attention to the unique status of slaves, Baptist's frequent references to "hands" suggests commonalities with free laborers, which is surely not his intention.

[16] Baptist relies on quantitative evidence produced by Rhode and Olmstead (2008), and separate narrative evidence. But Rhode and Olmstead (p. 26-31) show that the increase in productivity they observe was due primarily to biological innovations—the introduction of new cotton varieties that were more productive and easier to pick—and rule out any substantial changes resulting from plantation management or greater intensification of slave effort. On the narrative evidence presented by Baptist, see Olmstead and Rhode (2016).

[17] GDP can be measured by adding up all incomes earned within a country, or by adding up all expenditures or value added. Baptist's calculation includes a smattering of each, resulting in substantial double-counting. For example, he begins with the value of the cotton crop and then adds, among other things, the value of inputs used in cotton production, the value of incomes received by the producers of those inputs, and the spending on luxury items by the owners of slave plantations who produced the cotton. His calculations also add items that were part of the GDP of other countries or not part of measured GDP at all. As Majewski (2015:11) memorably observes, similar calculations could be used to claim that more mundane commodities such as hay also accounted for enormous shares of GDP.

one of cotton's most important contributions to American economic development: the foreign currency revenues it produced, which increased specie reserves in the banking system.

As has been said of other polemical but problematic works, *The Half Has Never Been Told* is perhaps best understood as 'history as rhetoric' rather than 'history as scholarship.'[18] To impress upon modern readers the brutality that enslaved African-Americans endured, in vivid and affecting language, is an important contribution that historians are uniquely positioned to make. But Baptist's account contains the very same kind of overstatements that those who have sought to minimize the viciousness of slavery have made. The importance of slavery in American economic development, and the cruelties that enslaved people were subjected to, need no exaggeration.

Like *The Half that Has Never Been Told*, Walter Johnson's *River of Dark Dreams: Slavery and Empire in the Cotton Kingdom* describes economic life in the antebellum Mississippi River delta in some detail. But to a much greater extent than Baptist's book, *River of Dark Dreams* is, at its core, an ethnography of the Cotton South: the analysis focuses on cultural interpretations of economic behavior. The book vividly describes many spectacles—slaves working at night by torchlight; the feeding of enslaved children in a common trough, like livestock; the improvised trials of slaves accused of capital crimes—and offers provocative interpretations of their cultural meanings. Many of the book's arguments seek to challenge the work of other historians, for example by pointing out the analytical limitations created by their emphasis on slaves' "agency" (p. 217), or their characterizations of the writings of advocates for slavery as "dehumanizing" (p. 207).

Yet there is also some economic analysis in the book, and it often falters. One major problem is Johnson's repeated claim that "planting and productivity were measured by a calculation of bales per hand per acre" (177; also 13, 153, 197, 217, and 254), which as Wright (2014) notes "makes neither mathematical nor economic sense." It is telling that this mischaracterization of productivity measurement ultimately makes little difference for the book's argument; Johnson is concerned only with the general

[18] Drescher (1999: 391) characterizes Eric Williams' *Slavery and Capitalism* in this way, arguing that it is important because "it challenged the heirs of a complacent historiography to take note of the neglected dimensions of the story," in spite of its flaws.

implications of slaves' position as inputs into production for their well-being, and does not engage with the specific details of plantation management techniques or input ratios. Virtually any measure of productivity, no matter how erroneous, would serve his purpose.

More importantly, Johnson ascribes enormous significance to attempts to conquer Cuba and Nicaragua by private armies of pro-slavery "filibusters," and to the push in some Southern states to re-open American ports to the slave trade. Arguing that it is anachronistic to limit our conception of "the South" to the American states that seceded from the Union, Johnson claims that it is important to consider "where Southerners (and slaveholders in particular) thought they were going," and take their "imperial aspirations" seriously (p. 14-16). It is fascinating to read Johnson's accounts of the filibusters' military campaigns, many of which descended into slapstick and farce. And Johnson uses the correspondence and promotional literature that these filibusters produced, which read like transcriptions of white supremacists' fever dreams, to analyze the ideologies of slave society in interesting ways.

But historians have been right to marginalize these movements. Slaveholders, who had configured the political institutions of Southern states to ensure their dominance, would have suffered substantial capital losses if the importation of slaves had been permitted, and had nothing to gain from access to land in places like Nicaragua. Wright (1978: 150), in fact, uses the efforts to re-open the slave trade as a "test case" for his analysis of the importance of slave values in Southern politics, and finds the rejection to be entirely consistent with efforts to protect the value of slavery.

These three books serve as an interesting counterpoint to the economic history literature on slavery. Whereas that literature has sought to investigate the sources of productivity in slave agriculture, these works by historians have emphasized the suffering that accompanied those productivity gains—from the violent expropriation of native peoples, to the forced migrations and sales of enslaved people, to the brutality that many slaves were subjected to.[19] Yet each of these books would have been more

[19] This is not to say that economic historians have not also examined the human costs of slavery; see, for example, the work of Richard Steckel (1986a, 1986b, 1986c).

compelling, and better works of history, if they had engaged with the economic history literature more fully.

The lack of engagement with the work economic historians leads these authors to make assertions that have been refuted conclusively. For example, each of these books argues that slavery depended on the acquisition of new territory for its survival, or at least for its economic vitality.[20] Yet the amount of unexploited land suitable for the cultivation of cotton within the slave states at the time of the outbreak of the Civil War was immense.[21] There is also a revival of the notion that plantations grew only cotton and were dependent on imports from other regions for food. This view, which figured prominently in works such as North (1961), was based on a misinterpretation of trade statistics that did not account for the fact that agricultural products shipped to New Orleans were re-exported to other destinations at high rates.[22] There are discussions of productivity gains in cotton production that minimize the role of the introduction new cotton varieties, which are now known to be among the primary drivers of those gains. Among the most labor intensive phases of cotton production is the harvest; biological innovations that produced taller plants and bolls that were easier to pick increased labor productivity significantly.[23] And the narrow focus on cotton among these works also prevents them from assessing the full significance of slavery in the development of the American economy. Although cotton was certainly the most valuable slave crop, large numbers of slaves in Kentucky, Missouri and Virginia were employed in the production of crops that are traditionally associated with small family farms, such as wheat.[24]

[20] Baptist states that supporters of slavery "consistently pressed to expand slavery's territory" since "experience had taught all of these white people to associate slavery's expansion with its prosperity" (p. 346) and that Southern politicians considered slavery "a system that needed geographic growth in order to function" (p. 382). Beckert states that "Cotton-growing slave owners…depended on the state's willingness to secure more lands for plantation agriculture" (p. 240) and that "Continued territorial expansion of slavery was vital to secure" its economic viability (p. 245). Johnson's analysis of the "filibuster" campaigns rests partly on similar assumptions.

[21] As Fogel (1989) notes, "the 1850 [cotton] crop was grown on just 6 percent of the improved land in the farms of the cotton states."

[22] Johnson (2013: 8, 151-52, 176) repeats the cotton monoculture hypothesis. For refutations, see Gallman (1970) and Fishlow (1964).

[23] See, for example, Baptist (2014: 126-28), and Beckert (2014: 115-116). On the role of biological innovation in cotton productivity, see Olmstead and Rhode (2008a, 2008b), and Rhode (2015.)

[24] On the significance of wheat production, see Wright (2006).

More importantly, the lack of engagement with economic historians limited the analytical perspectives of each of these books. Most of them seem aware of Fogel and Engerman's *Time on the Cross* (1974), and some repeat its arguments about the profitability of slavery or the efficiency of slave plantations.[25] But they do not seem to have taken seriously the debates among economic historians that followed the publication of that book.[26] Some of that literature has challenged Fogel and Engerman's claims regarding scale economies in plantation agriculture, and the role of gang labor.[27] But the literature that developed in the wake of *Time on the Cross* analyzed slavery in new ways.

For example, Gavin Wright (2006) argued that the literature up to that point had overemphasized slavery as a form of work organization, and instead argued that it is best understood as a system of property rights. And the form of property held by owners of slaves had important implications for society, and for economic development. For example, whereas property owners typically have a strong incentive to support urbanization or the provision of public goods, since the benefits will be capitalized in land values, slave owners have no such incentive: their property is moveable, and its value is not affected by local development.[28] Slave owners therefore had "little to gain from improvements in roads," and "no particular desire to attract settlers by building schools and villages and factories."[29] They also had weaker incentives to produce labor-saving innovations; only after emancipation did patent rates for mechanical devices for cotton harvesting and processing approach those associated with corn or wheat.[30] Baptist

[25] Baptist (2014: 478 n. 11, n. 12) actually cites Fogel (1989), which somewhat recasts the arguments of Fogel and Engerman (1974). Johnson (2014: 464 n. 7) cites Fogel and Engerman (1974).
[26] Examples of some of the fierce criticisms of *Time on the Cross* by economic historians include David and Temin (1974, 1979).
[27] Wright (1978) notes that comparisons between the productivity of large and small farms suffers from the problem that small farms pursued a different crop mix, with a higher share of subsistence crops. The higher share of cotton in the output of large plantations may be responsible for the higher levels of measured productivity. Unmeasured differences in soil quality between small and large plantations may also account for observed scale economies. Fogel (1989) addresses these criticisms.
[28] Johnson (2013: 87) notes the significance of the partibility and moveability of slaves for their use as collateral in credit markets, but does not consider any further implications of the ownership of assets with these characteristics.
[29] Wright (1986: 18).
[30] Wright (1978: 108). This issue, of course, has implications far beyond cotton production; as Majewski (2015) notes, all categories of patents were produced at lower per capita rates in southern counties relative to northern counties.

(2014:128) notes that cotton picking was not mechanized until the twentieth century, and characterizes this as an obstacle slaveholders overcame, rather than an effect of slavery itself.

Later in this essay I suggest some further insights and analytical approaches from economic history that would benefit further research by historians of capitalism on slavery.

B. Books on Finance, Risk and Insurance

A separate strand of the history of capitalism literature has focused on finance, and the closely related topics of risk and insurance. This marks a significant departure for the field of history, which until relatively recently has not focused on finance at all.

Among the most creative of the works of historians of capitalism is Jonathan Levy's *Freaks of Fortune: The Emerging World of Capitalism and Risk in America.* Levy's work is an account of the evolution of economic risks, and their distribution, over the nineteenth century. At the center of the argument is the notion that Americans developed a "vision of freedom that linked the liberal ideal of self-ownership to the personal assumption of "risk"" (p. 5). But this, in turn, led to new efforts to manage those risks, and Levy focuses on the ideas, legal doctrines, and market innovations that determined the allocation of new risks as they emerged. Over the course of the book's eclectic narrative, the reader is presented with discussions of whether an insurrection on a slave ship absolved insurers of liability; fraternal societies that offered life insurance to their members, and competed with life insurance corporations; the use of mortgage securitizations in the 1880s; the emergence of futures trading, and debates about its legitimacy; and strategies by financiers to control competitive pressures and manage market risks by merging competing firms together, which they actually characterized as a form of "socialism." As Levy notes, it is in the twentieth century that the public sector became an important absorber and manager of risks, but the book's narrative does not extend into that period.

Among all the history of capitalism's works, Levy's reflects the influence of Karl Polanyi most strongly and even makes frequent use of Polanyian terms such as "double movements" and "countermovements." In a sense, *Freaks of Fortune* can be thought of as a re-telling of elements of *The Great Transformation* for the United States, with a focus on private efforts to contain risk. This leads to some fascinating insights, but it also creates problems for the analysis. For example, Levy ignores changes in the "economic chance-world" that did not originate in capitalism itself, and therefore excludes some of the greatest sources of "freaks of fortune" in American history, such as the Pennsylvania oil rush, or the California gold rush. But more importantly, the analogy between the experiences of Britain and the United States is imprecise. Capitalism arose in Britain through a dismantlement of feudal institutions that fundamentally changed society; few of those institutions existed in colonial North America, and the rise of capitalism there may have been somewhat less transformative. Much of Levy's argument proceeds from the unexamined assumptions that the changes that occurred over the nineteenth century were fundamental in nature, rather than merely an acceleration of processes that had already existed, and that economic fortunes became much more volatile over time.[31]

For example, Levy claims that the growth of mortgage markets and institutions meant that by the early 1890s, "the logic of American farming had been utterly transformed. The farmer's distinctive hedge—his land—was lost…Their land could no longer shield them from the markets' vicissitudes" (p. 151-2). The claim is: farmers could only obtain land and equipment through mortgages, and a mortgaged farm was risky and required the operators to produce cash crops rather than food for consumption. Yet it seems doubtful that farmers in earlier eras did not also utilize mortgages at high rates, as the practice of agriculture generally entails a need for credit, and American land titles were uniquely friendly to the commodification of land.[32] It also seems doubtful that there was ever an era in which American famers focused purely on subsistence.[33] Levy also notes that the development of mortgage markets tended to

[31] The contributions of Farley Grubb on colonial fiscal and monetary affairs remind us that the pre-industrial economy in North America was subject to tremendous volatility (Grubb 2016a, 2016b).
[32] On the creditor-friendly nature of American land titles and their differences with those of traditional English law, see Priest (2006). Levy cites data on rising levels of total mortgage indebtedness over the 1880s, but this was a period in which large amounts of acreage came under cultivation, and the indebtedness of the average farmer may not have risen at the same rate.
[33] Winifred Rothenberg's (1992) data suggests extensive market-orientation, as well as an extensive use of mortgages, among farmers in early Massachusetts.

lower mortgage interest rates (p. 153), which would seem to pull the rug out from under his own argument.

Economic historians will find this book fascinating but also frustrating. The allocation of risks, such as those related to retirement, health, or individual economic fortunes, is an issue of paramount importance. Levy explores some unusual and often-neglected elements of its history. However, there are some confused statements about finance and financial markets that are difficult to overlook. Levy makes the claim, for example, that "antebellum America was relatively poor in finance capital. Much of the nation's wealth was in the physical capital of land and slaves" (p. 78). This is specious reasoning: when someone bought land or slaves, they paid the seller, whose holdings of "finance capital" increased by exactly as much as the buyer's decreased, so that the total amount of national "finance capital" was not affected.[34] There is also a discussion of futures trading that barely notes its effects on commodities markets.[35] Instead, it focuses on the fanciful legal reasoning that some jurists resorted to in order to distinguish futures trades from bucket-shop wagers or other forms of gambling. And some of the most important mechanisms by which Americans sought to manage and limit risks, namely by imposing regulations on banks (or in some cases, banning banks), are conspicuously absent from the story.

Risk also plays a role in what is sometimes referred to as "crony capitalism," in which well-connected figures in the business world use their political influence to get the state to underwrite their ventures. This issue figures prominently in Richard White's *Railroaded: The Transcontinentals and the Making of Modern America*. Drawing on the correspondence and business records of the industry's leading figures, this important book details the mismanagement, corruption, deceit and fraud that were ubiquitous among the early transcontinentals, whose construction was heavily subsidized by the state.

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[34] This is the so-called "capital absorption" thesis, which dates back at least to Phillips (1905:60), and holds that Southern plantation owners invested all of their capital in slaves and land, and "never accumulated any surplus for any other sort of investment." Levy also applies this reasoning specifically to the South (p. 95). On the capital absorption thesis, see Wright (1986: 20).

[35] Well-established results from economic historians have shown that commodity price volatility is decreased by futures trading; see Jacks (2006). Davis, Hanes and Rhode (2009 note that the emergence of cotton futures trading led textile manufacturers to hold larger inventories of raw cotton, since it enabled them to hedge the risk of price fluctuations. Larger inventories led to smaller price changes in response to supply fluctuations.

Beyond the losses borne by the federal government and by investors in defaulted securities, White chronicles other impacts of these firms and concludes that "in terms of their politics, finances, labor relations, and environmental consequences, the transcontinentals were not only failures but near-disasters" (p. 507).

The book is huge and does indeed explore issues such as labor relations and the impact of the transcontinentals on the settlement and environmental exploitation of the West. It also presents an interesting and sophisticated discussion of railroad rate-setting practices, and a discussion of the role of the state with the transcontinentals, and in the economy more generally, that is quite consistent with Polanyi's. But its great strength is its analysis of the financial manipulations of railroad insiders and its careful and precise descriptions of scandals such as Crédit Mobilier, which reflect an enormous amount of archival research. It colorfully chronicles how figures like Henry Villard became "superheroes of bad management—powerful, daring, able to destroy railroads at a single blow" (p. 217), in contrast to works such as Chandler (1977), which have emphasized the managerial successes of the railroad industry. Particularly fascinating are the descriptions of the tactics employed by rival operators to strike at each other by manipulating the legislative process, and the betrayals and deceptions within groups of promotors of the same railroads. This book will be a valuable resource for anyone interested in a clear account of the problems of railroad finance during this period.

Yet, like *Freaks of Fortune*, this book will frustrate the economic historians who read it. White documents a number of episodes of deception of investors—insider trading schemes, accounting fraud so extreme that executives had little idea of their own railroads' financial condition, the sale of bonds of completely bankrupt railroads that were represented as being perfectly sound—that resulted in substantial losses. Given the amount of money that was subsequently raised by the industry, this cannot have been the whole story. Insiders who have privileged access to information can indeed swindle investors, but in response to deception, investors will adapt their behavior. They may simply refuse to invest, or they may invest only in circumstances in which they are protected from fraud–perhaps by purchasing only senior mortgage bonds on excellent collateral. White does note the paradoxical nature of his observations of "a world dominated by large, inept, but powerful failures" and ultimately concludes that their survival "demanded the intervention of the state" (p. 509). But he does not explore the consequences of the fraud and deception for the railroads' interactions with financial markets, nor does he present any clear information on the composition of outstanding railroad securities and how it may have evolved over time.[36]

This is where analogies to the recent crisis may have led White astray. He views the bankers underwriting these securities as generally complicit in the fraud, or unwilling to do anything about it (p. 375, 380, et seq.). And early in the industry's history, that may have been the case. But the all-important reputations of the major securities underwriters of the late nineteenth century would have been severely harmed by repeatedly marketing fraudulent securities. It is worth remembering that there were no ratings agencies at the time—investors had to rely on the representations made by bankers when securities were sold. And in response to fraud, the bankers who underwrote railroad securities did indeed make some changes. They began to take active roles in railroad governance where they could, in order to police the behavior of railroad insiders (Carosso, 1970; Hilt 2014). It is noteworthy that White discusses the failure of the Northern Pacific in the Panic of 1893, but he does not mention the highly successful reorganization of the railroad following the panic by James J. Hill and J.P. Morgan, which led to two Morgan partners joining its board (see Campbell, 1938).

Each of the preceding books has focused on the nineteenth century. But the historians of capitalism have also studied later periods, and the two remaining books on the subject of finance and risk focus on the development of financial markets during the twentieth century. Both are focused on the social and political contexts within which the development of financial markets was embedded, to use the Polanyian term. Louis Hyman's *Debtor Nation: The History of America in Red Ink* chronicles the development of American consumer credit markets over the twentieth century. Hyman begins with

[36] White does offer the conjecture that "falling returns on investment in Europe and low interest rates in the 1870s and 1880s drove European capital to seek higher returns in developing countries such as the United States…" (p. 380). But this is, at best, an incomplete explanation: falling returns on safer assets should not have much of an effect on investors' demands for assets issued by railroads known to be operated by serial fraudsters.

innovations that occurred around 1920, such as the passage of small loan laws by the states, and the emergence of finance companies. The book then follows the legal and institutional developments that occurred over subsequent decades, such as the changes made to mortgage markets by the New Deal, regulations intended to restrain consumer credit introduced during World War II, and the evolution of postwar credit markets and the rise of credit cards and securitization. Hyman's treatment of these and other issues is quite thorough and detailed, and emphasizes the role of public policy in shaping the growth of credit markets. Among all the works discussed in this essay, Hyman's includes the most thorough use of quantitative data, and includes meticulous documentation of the legislative initiatives and judicial decisions at the center of the story. This book is a valuable resource on twentieth century credit market development.

Readers interested in the institutional foundations of modern mortgage markets will find Hyman's chapter on the subject particularly useful. He presents a succinct account of the Housing Act of 1968, which privatized the Federal National Mortgage Association and created the modern mortgage-backed security, and follows subsequent innovations in mortgage markets—often initiated by the government, in partnership with the financial sector—into the 1980s and 1990s, which created the collateralized mortgage obligation (CMO), and which led to similar innovations in the financing of credit card receivables.

Hyman's book makes a valuable contribution, but it has two important shortcomings. First, it is framed as a study of the "modern credit system," but this is based on an exaggerated distinction between the "modern" period and the apparently pre-modern one. Hyman argues that prior to the 1920s—the period where his study begins—personal debt was "confined to the margins of the economy," and that the changes documented in the book created "a new role" for personal debt "within American capitalism" (p. 1).   Although the credit market institutions that served ordinary households in the nineteenth century were organized differently, they functioned in similar ways, and more importantly, excluding them from the analysis causes Hyman to miss some potentially important insights. For example, beginning in the mid-nineteenth century, households seeking credit for home purchases could turn to building and loan associations, which proliferated quite rapidly (see Bodfish, 1931). And in the 1880s, western mortgages were securitized as part of "debentures" or "covered bonds" and marketed to eastern investors. Both of these institutional structures collapsed in financial crises.[37] Including them in the study could have added historical insight to the discussion of the instability of later credit-market institutions.

The second shortcoming of Hyman's book is that the narrative does not continue into the early 2000's, so it does not document the changes in mortgage market institutions, such as the enormous growth in subprime mortgage securitizations, which occurred immediately prior to the crisis. It does, however, offer a brief discussion of the financial crisis at the book's end, which includes some confused statements.[38] Interestingly, Hyman claims that "instead of addressing the core problem of widening income disparities, legislators and businesspeople pushed consumer credit to rectify income inequality" (p. 284). This may be true in a general sense, but as an explanation for the crisis it comes close to those advanced by conservatives who held that the government, not the private sector, was at fault.[39]

Finally, Julia Ott's *When Wall Street Met Main Street: The Quest for an Investor's Democracy* chronicles the emergence of mass participation in securities markets in the early twentieth century. Ott argues that the increase in securities ownership among ordinary households in the 1920s was not the inevitable outcome of industrialization or financial development. Instead, it was the product of deliberate efforts on the part of both policy makers and leaders from the financial sector and industry to promote the ownership of financial assets. In this interesting account, households were induced to become investors as part of a social and political agenda that followed from theories of "proprietary democracy."

Like many historians before her, Ott ascribes enormous significance to the liberty loan campaigns of World War I. These were conducted not only to raise funds to finance the war effort, but also to elicit

[37] Building and loans collapsed during the Great Depression, and covered mortgage bond lending collapsed in the Panic of 1893. On the demise of building and loans, see Fleitas, Fishback and Snowden (2015). On the demise of covered mortgage bonds, see Snowden (2010).
[38] For example, he describes the securitization of consumer loans as "diversion of capital into nonproductive investments" (p. 286); it's not clear what's meant by "nonproductive" but so long as there is no fraud such lending certainly helps facilitate valuable economic activity.
[39] Most economists would probably agree with Blinder's (2013:28) list of the main causes of the crisis, which emphasizes "disgraceful banking practices" and excessive risk taking and leverage in the financial sector, among other factors.

participation by virtually every element of society, from women's clubs to religious organizations, in the promotion of the bonds in a series of highly publicized "bond drives." Ott argues that business leaders learned from the example of the liberty loans, and in response to their own political concerns, such as those relating to labor relations or the imposition of new regulations, held their own campaigns to market their securities to their employees and to other households. In accordance with theories of a New Proprietorship, business leaders sought to give ordinary households a stake in their success, and the New York Stock Exchange even styled itself "the people's market."

But like *Debtor Nation,* Ott's book presents the developments of the early twentieth century as historically unprecedented, and in doing so, exaggerates the distinctions between that period and earlier ones. For example, Ott argues that prior to 1900 "most Americans believed that bond- or stockholders warranted little consideration in economic theory or policy" (p. 2). In fact, corporations and the rights of their shareholders and creditors were among the most important concerns of nineteenth century Americans, and highly charged rhetoric about them is nearly ubiquitous in that era's political discourse.[40] One can even find early antecedents of some of the precepts of the New Proprietorship within early nineteenth century doctrines of corporate regulation.[41] What was indeed new in the twentieth century was the emergence of a truly national market for securities, and the intervention of the federal government in that market during the New Deal. Yet the evolution of local securities markets and state corporation statutes over the nineteenth century created the foundations for the twentieth century phenomena that are the focus of Ott's book. Excluding any discussion of the earlier history forecloses any possibility of understating those foundations.

C. Books on Conservative Economic Doctrines of the Late Twentieth Century

[40] The growing literature on the political significance of corporations and corporate shareholders by economic historians is too large for me to summarize here; important works include Lamoreaux (2015) and Wallis (2006).
[41] For example, some of the theorists of the New Proprietorship argued for measures to increase the influence of small shareholders within corporate governance; this was a principal concern among nineteenth century corporate promoters (see Hilt, 2008; 2013).

There is a long tradition in the field of history of studying the social movements of the left. Historians have produced a number of excellent books on the 1960s and on the radicalism of the 1930s and courses on those movements were once quite commonly offered by history departments. In contrast, in 1994 Alan Brinkley described American conservatism as “something of an orphan in historical scholarship,” in spite of its importance for understanding the twentieth century. But since then, interest in conservatism has grown among historians. Some of this new work has been produced as part of the field of the history of capitalism, which has shown growing interest in conservatism and conservative economic doctrines.

In contrast, economic historians have shown relatively little interest in the study of the history of economic ideas and doctrines. There is, of course, a small subfield of economics devoted to the history of economic thought, and some of its practitioners have produced studies of conservative intellectuals (for example, Goodwin, 2014). But in general, the research on conservative economic doctrines by historians of capitalism represents a unique contribution with no parallel in the field of economic history. Although these books are quite different from the others reviewed in this essay, in a sense they the most complementary to the work of economic historians. They do not analyze the content of the ideas they study or editorialize about them, but instead focus on the individuals and organizations devoted to promoting them.

Kim Phillips-Fein’s *Invisible Hands: The Businessmen’s Crusade Against the New Deal* chronicles the efforts of business executives beginning in the 1930s to promote economic ideas critical of the welfare state. The book includes carefully researched accounts of the origins of think tanks and other organizations that were funded by business beginning in the 1950s, which “helped to form the infrastructure for the rise of the conservative movement” (p. 86). Among these were Friedrich von Hayek’s Mont Pelerin Society, the organization that became the American Enterprise Institute, and a few others that never grew beyond obscurity, such as a group called Spiritual Mobilization.[42] The book highlights the role of a handful of particularly influential figures, such as Lemuel Boulware of General Electric, and follows the growing efforts of businessmen to promote conservative economic ideas up through the 1970s. Its detailed narrative presents a clear account of the rise of conservative economic doctrines and their connection to anticommunism and to conservative religious thought. The book concludes with the election of Ronald Reagan, a former General Electric spokesperson, to the presidency.

This thoroughly-researched book raises a number of interesting questions. Why did some businesses and executives support the New Deal (and related policy changes), whereas others recoiled? And why did some conservative organizations become quite influential and successful, whereas others failed in obscurity? Phillips-Fein does offer some insight into the latter, in her suggestion that the Mont Pelerin Society "had the misfortune to spin their theories at a time when the economy was stable and growing," whereas the think tanks of the 1970s "worked in an era when liberalism seemed no longer able to deliver on its promises" (p. 167). But thoroughly addressing questions such as these was not her aim. It is my hope that future historians of capitalism will adopt a more social scientific approach and pursue such questions.

Among the individuals discussed in Phillips-Fein's book is Ayn Rand, who is the focus of Jennifer Burns' *Goddess of the Market: Ayn Rand and the American Right*. Born Alisa Rosenbaum in Russia in 1905, Ayn Rand led a fascinating life, and Burns' book, written with access to Rand's papers, is enormously enjoyable to read. Best described as an intellectual biography of Rand, *Goddess of the Market* details the development of her philosophy, which came to be known as objectivism, and the influences that produced it. At the outset she notes the "nearly universal consensus among literary critics that [Rand] is a bad writer" (p. 2), but Burns' book is free of such condemnations of Rand's ideas, and explores them in depth.

[42] Although this group received donations from a number of prominent businesses, it languished in obscurity, and in its waning years its leaders turned to "inner spiritual exploration" and began to experiment with taking LSD (Phllips-Fein, 2009: 76). It was shut down in 1961; perhaps it was simply ahead of its time.

Goddess of the Market complements *Invisible Hands*, as it details Rand's interactions with some of the same individuals and institutions, and explores their intellectual agreements and disagreements. Rand was a difficult personality who frequently came into conflict with figures who would have been her natural allies. One of the most interesting insights of the book is that Rand had little patience for academic economists. She regarded von Hayek as "pure poison" (p. 104), and characterized Milton Friedman and George Stigler's booklet on rent control, *Roofs or Ceilings*, as "pernicious" (p. 117), in both cases due to their sympathies for some limited role for intervention by the state. She was more sympathetic to von Mises, whose views were closer to hers, although she had a troubled relationship with him as well. Burns notes that Rand's objectivism "left no room for elaboration, extension, or interpretation" (p. 6) and it is possible that her influence has grown after her death in part because her absence has enabled her ideas to evolve somewhat in the hands of others. But the analysis of the book makes clear why Rand has remained influential, and "part of the underground curriculum of American adolescence" (p. 282).

One of the most prominent corporate supporters of conservative think tanks and activist groups is Wal-Mart, which is known for its conservative culture. Bethany Moreton's *To Serve God and Wal-Mart: The Making of Christian Free Enterprise* presents a history of that culture. Moreton's book describes the early efforts of the firm to establish its appeal among conservative rural communities, by adopting cultural values consistent with theirs. For example, its workforce was divided along gender lines, with women working as sales clerks, and men in managerial positions. Moreton argues that this enabled Wal-Mart to appeal to local customers, and to gain access to an inexpensive source of labor, married women. Drawing heavily on internal publications such as *Wal-Mart World*, Moreton chronicles the early growth and development of the firm.

Although Moreton's book offers some interesting insights, it also illustrates the limitations of a purely cultural approach to the analysis of a business. She ascribes enormous significance to Wal-Mart's efforts to assimilate the values of the region where the firm originated, the Ozarks. I have no doubt that these did indeed matter to the firm's early customers and employees, but the book fails to make any attempt to evaluate their importance relative to other major elements of the firm's business model, such as its procurement policies or pricing strategies. More importantly, Wal-Mart quickly grew well beyond the Ozarks to become the world's largest private employer, and its workforce now rivals that of the People's Liberation Army of China in size.[43] The book does not explore the ways in which Wal-Mart's efforts to appeal to rural evangelicals in Arkansas and Missouri shaped its growth in places like Mexico. Did it replicate its labor practices from the Ozarks there, or did it need to adapt them? Too many such questions arise when reading this book.

III. Insights from Economic History: Institutions and Counterfactuals

The field of history has its own strengths, quite different from those of economics. I do not wish to suggest that historians of capitalism must adopt the methods of economic historians. Yet historians of capitalism would benefit from assimilating into their own thinking some of the analytical constructs and techniques developed by economic historians. The most conspicuous of these are counterfactuals, and institutional analysis. It is worth noting that these are among the contributions for which the two Nobel laureates for economic history, Robert Fogel and Douglass North, were best known.

By institutions, economists mean the "humanly devised constraints that structure political, economic, and social interaction," which encompasses the formal, as in laws or constitutions, and the informal, as in customs or traditions (see North, 1990, 1991). To be sure, all of the books by historians of capitalism discussed in this paper are concerned with institutions—slavery, evangelical Protestantism, or proprietorship, for example—and with cultural concepts that are closely related to institutions.[44] The Polanyian concept of "embeddedness," so influential among historians of capitalism, naturally points towards incorporating institutions into the analysis. Yet with the exception of Beckert (2014), these books

[43] In 2015, Wal-Mart had 2.1 million employees, whereas the People's Liberation Army employed 2.3 million ("The World's Biggest Employers," 23 June 2015, Forbes.com.)

[44] Economists distinguish culture, which encompasses social norms and individual beliefs, from institutions. Culture sustains institutions, and culture may also determine the effects or significance of particular institutions. On the relationship between the two, see Alesina and Giuliano (2015) and Tabellini (2010), and the references cited therein.

lack any analysis of the role institutions play in shaping or constraining economic development over time. The incorporation of this latter form of institutional analysis into the research agenda of historians of capitalism would enable them to produce works of greater insight.[45]

Consider the institution of slavery. In a series of highly influential works, Stanley Engerman and Kenneth Sokoloff (2000, 2005, 2015) have argued that slavery has been associated with high levels of inequality, and with institutions designed to perpetuate that inequality. Focusing on comparisons among former colonies in the Western Hemisphere, they note that slave societies tend not to provide broad access to the franchise, have poorer educational systems, more restrictive immigration and land policies, and, in general, tend to offer low levels of economic opportunity and mobility for ordinary people. The books on slavery discussed here observe the lack of industrialization, urbanization, and economic mobility in the cotton south. But they fail to appreciate the implications of these characteristics for economic development. The preoccupation of Beckert, Baptist, and Johnson with demonstrating the productivity of slave agriculture and the compatibility of slavery with capitalism causes them to overlook the harmful effects of slavery on institutions, and development.[46] Slavery is compatible with capitalism in general but incompatible with what Majewski (2015) calls Schumpeterian or dynamic capitalism, and the institutions needed to support it.

Slavery in the American South had more far-reaching implications than the brutal exploitation of African-Americans. As the Engerman-Sokoloff analysis predicts, the institutions of Southern states were configured to serve the interests of slaveholders, and did not offer much opportunity for non-slaveholders. These institutions have evolved over time, but institutions generally change slowly, and the parts of the United States in which slavery was most important remain quite different today from the rest of the

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[45] Some historians have begun to consider institutions in their work (eg, Conti-Brown, 2016) but to my knowledge this is not true of the historians of capitalism.

[46] Typical is Baptist's (2014: 348) discussion of the comments of Frederick Law Olmstead, who observed German immigrants headed up the Mississippi to Iowa and noted they would build a society very different from that of the south, and replete with "mills and bridges, and schoolhouses, and miles of railroad." Baptist sees this as the biased perspective of a northerner who was "primed …to find evidence that slavery was inefficient." But Olmstead's argument that Iowa had superior institutions and a more diverse and dynamic economy was completely correct, and does not contradict the notion that slavery was an efficient mode of production of cotton.

country. Through its influence on institutions, slavery has had a variety of malign effects on the American South that continue to be felt. Slavery was certainly profitable, and it created a number of large fortunes. But its legacy has made whole regions of the United States poorer today than they would have been.

In contrast to the impulse to show dramatic change over time, a focus on institutions might also highlight sources of intertemporal continuity in the analysis of historians of capitalism. Levy's *Freaks of Fortune*, Hyman's *Debtor Nation*, and Ott's *When Wall Street Met Main Street* all describe economic developments that they characterize as new. But these changes were shaped at least in part by long-established institutions, such as legal doctrines governing relationships between debtors and creditors. An investigation of the extent to which such institutions influenced the economic changes at the heart of those books would have both deepened the analysis and made claims about dramatic change more credible.

Another analytical tool that historians of capitalism would benefit from adopting from the field of economic history is the counterfactual. These are, of course, thought experiments in which some condition is changed, contrary to fact, and the consequences are considered. Many historians apparently have a strong distaste for counterfactual histories.[47] Yet the reason economic historians think about counterfactuals is not due to an interest in specifying and analyzing full counterfactual histories.[48] Instead, it is because all statements about causal relationships contain counterfactuals. To say that the gold standard caused the Great Depression, for example, is to say that absent the gold standard, the Great Depression would not have happened; the two statements are equivalent. There is of course no way to know exactly what the international monetary system of the late 1920s would have looked like in the absence of the gold standard without making a lot of strong and potentially contradictory assumptions, but thinking about that world helps identify forces unrelated to the gold standard that may have

[47] See, for example, Evans (2013) and Tucker (1999).
[48] Fully specifying a counterfactual history, as Fogel (1964) famously does for the American railroad system, is sometimes possible, and can produce valuable insights. But this may require the imposition of unreasonable assumptions and lead to fallacies or logical inconsistencies.

contributed to the Great Depression. Economic historians typically investigate counterfactuals not by specifying counterfactual histories, but by comparing cases where a condition is present to cases where it is absent.[49]

The books of historians of capitalism all make causal statements, which contain implicit counterfactuals. But few of them are expressed in clear terms, and none of them are evaluated in any depth. Carefully analyzing these counterfactuals may have led to different, or perhaps more nuanced, conclusions. For example, the books on slavery argue, in slightly different ways, that American slavery was necessary for the Industrial Revolution.[50] This sounds reasonable: American slave plantations became the most significant producers of raw cotton for the British market, and raw cotton was a necessary input for the British textile producers who were the vanguard of the Industrial Revolution. But analyzing the counterfactual embodied in that statement may have revealed it to be incorrect.[51]

Let's assume that American raw cotton was actually necessary for the Industrial Revolution. The question then is whether slavery was essential for the production of that cotton, or stated in counterfactual terms, whether in the absence of American slavery, the raw cotton necessary for the Industrial Revolution could have been produced. The production of American cotton was indeed dominated by slave plantations, and the small yeoman farmers of the South were often engaged in subsistence farming. Yet the inference that slave agriculture was necessary for large-scale cotton production in the American South, or that smallholders could not be viable producers of cotton there, is unwarranted. Research by David Weiman (1985) has shown that following the Civil War and emancipation, the expansion of

[49] With the causes of the Great Depression, some of the best evidence economic historians have produced relies on the timing of different countries' abandonment of the gold standard in the 1930s to identify its role in propagating the slump (see Eichengreen, 1992). Countries' actual experiences with and without the gold standard are compared; a full counterfactual world is not specified.

[50] For example, Beckert (2014) states that "while in the eighteenth century, slavery had enabled industrial takeoff, it now [in the nineteenth century] became integral to its continued expansion" (p. 117). He also quotes the *American Cotton Planter* from 1853 apparently with approval, as stating "it is idle to talk of producing Cotton for the world's supply with free labor" (p. 119). Baptist (2014) claims not only slavery but the torture of slaves was the "ultimate cause" of the Industrial Revolution (p. 130). Johnson, however, is not as explicit or direct on this point, and rejects counterfactual analysis, stating simply that "in actual historical fact there was no nineteenth-century capitalism without slavery" (p. 254).

[51] The validity of this counterfactual is evaluated in greater detail in Olmstead and Rhode (2016) and Majewski (2015).

transportation infrastructure and the growth of commercial centers in the Upcountry region of Georgia enabled the small yeoman farmers there to reorient their production away from subsistence and toward cotton—and their cotton production grew dramatically in the 1870s. The political and economic institutions of slavery likely constrained the development of railroads and market infrastructure in the Upcountry, and effectively blocked many smallholders from the market access that would have enabled them to become viable cotton producers.[52] Once slavery was gone and problems associated with market access were resolved, the yeomen of the Upcountry became major cotton producers. Given that the soil and climate of parts of the American south made it among the most productive areas in the world for growing cotton, it is not unreasonable to expect that smallholders without slaves could have produced very large cotton crops there. Absent large slave owners, the region would have been settled in different ways, perhaps resembling what unfolded in parts of the American West. The quantity of cotton produced may not have increased as rapidly, but American cotton farms would still have helped serve the demand of British producers.[53] American slavery was not actually necessary for the Industrial Revolution.

These authors may not have intended to claim that in the absence of slavery the Industrial Revolution would not have happened. But this is a clear implication of their argument that slavery was "crucial" (Baptist, 2014: 141) to the Industrial Revolution. Johnson's *River of Dark Dreams* contains a rejection of such counterfactual analysis, stating that "[h]owever else industrial capitalism *might* have developed in the absence of slave-produced cotton and Southern capital markets, it did not develop that way" (p. 254). Richard White's *Railroaded* answers this point beautifully: "Considering only what happened is ahistorical, because the past once contained larger possibilities, and part of the historian's job is to make those possibilities visible; otherwise all that is left for historians to do is explain the inevitability of the present" (p. 517). Anyone wishing to argue for the centrality of slavery in capitalist

[52] Weiman (1985) shows that rail access, coupled with the processing and storage facilities that developed in the Upcountry cities, substantially increased the price local farmers could obtain for their cotton, and provided them with better access to market information.

[53] As Majewski (2015) and Olmstead and Rhode (2016) note, after the Civil War ended and the slaves were emancipated, cotton production continued. There was an initial decline in total cotton output, but enormous quantities of raw cotton were nonetheless produced without slavery. This is another indication that slavery was not actually necessary.

development needs to consider what could have been possible without slavery, and world in which there was no American slavery but the Industrial Revolution still occurred was indeed possible. Historians of capitalism wish to highlight the tragedy of American slavery by claiming it was essential for industrialization. I would argue that it is more tragic that slavery may not actually have been necessary.

Some of the other authors, such as White (2011), actually do state explicit counterfactuals. However, there is often little elaboration or critical analysis of relevant evidence.[54] For example, in *When Main Street Met Wall Street*, Ott states that theories of the New Proprietorship "provided the stock market with a legitimizing ideology. Without it, the advent of broad-based, direct investment in corporate stock in the 1920s could not have occurred" (p. 131). Ott should be commended for making this clear counterfactual statement of her argument. But beyond an assertion that the liberty bond drives of World War I and some related policy changes were alone insufficient to lead to ordinary households owning stock, she offers no evidence in support of her argument. And yet, further analysis of this assertion suggests that it may be false. The "roaring twenties" witnessed something of a speculative mania, with extremely high returns on the stock market, particularly beginning in 1927. It is not unreasonable to imagine that even in the absence of the New Proprietorship, many American households would have chosen on their own to invest in the stock market, to participate in those high returns. Ott's focus on the social and political contexts within which the market developed, rather than on the market itself, leads her to neglect these factors in her analysis.

More broadly, thinking about counterfactuals may constitute a useful way for historians of capitalism to refine their ideas, or potentially identify new ones. For example, Phillips-Fein's *Invisible Hands* and Burns' *Goddess of the Market* both follow the evolution of conservative economic doctrines, and efforts to promote them. These books are intellectual histories, but lurking within them are counterfactuals about the impact of those ideas and the think tanks behind them. What if the American

[54] White's (2011: 517) counterfactual is that "without the extensive subsidization of a transcontinental railroad network, there might very well have been less waste, less suffering, less environmental degradation, and less catastrophic economic busts…" He actually does present evidence relative to some of these assertions, but he does not engage with the analysis of economists on closely related issues (eg, Fogel, 1964, and Fishlow, 1965) and does not pursue the implications of the counterfactual in detail.

Enterprise Institute and other such organizations had never been created, or Ayn Rand had never lived? How much impact did they actually have? Did they cause changes to occur, or did they merely create the ideological foundations for movements that would have occurred anyway? Such questions are fundamentally important, and are ideal areas for future investigation by historians of capitalism.

IV. Economic History and the History of Capitalism: The Prospects for Dialog

In this essay I have argued that the analysis of historians of capitalism would be improved by the assimilation of analytical techniques and insights from economic history. But it is also the case that economic historians' understanding of the concepts and contexts that they study would be deepened by engagement with the work of historians. Economic historians might also find ideas and conjectures in the work of historians of capitalism that they could pursue further using the methodological approach of economics. Each field would be strengthened by insights and criticisms that would come from serious engagement with other. But any level of engagement—even if it constitutes little more than scholarly critiques and debates--would be worth pursuing.

One opportunity to create such engagement might be for historians to take up some of the fundamental questions related to institutions that economists have been unable to answer. In recent years, economists and economic historians have produced an unrelenting wave of empirical research on the modern effects of historical institutions.[55] Yet little is known about the specific mechanisms by which particular institutions influence economic development over time, or the factors that may cause institutions themselves to evolve. Some progress has been made on these questions using the tools of game theory (eg, Greif and Laitin, 2004), but scholars trained in the field of history might be uniquely able to shed some light on them, by focusing on in detail on the origin or evolution of particular institutions, or on the specific effects of institutions over time in particular historical contexts. In a sense,

[55] This literature is far too large to summarize here. An enormously influential example that illustrates the analytical weaknesses typical of such research is Acemoglu, Johnson and Robinson (2001).

a great deal of the analysis of historians is related to such questions, but the insights from their work have not been connected to the literature on institutions. Historians who engage with that literature might also be inspired to critique of some of the claims made by economists regarding the origins or effects of particular institutions, which would help improve that literature.

Another opportunity for greater interaction between the two communities of scholars might be for economic historians to join historians of capitalism in focusing explicitly on capitalism in their research. Economic historians have analyzed economic interactions in many different contexts, generally without addressing whether those contexts conform to a definition of capitalism.[56] Yet the success of the books reviewed in this essay demonstrates the value of highlighting the role of capitalism in historical analysis. To do so requires a definition of capitalism, or perhaps a typology of different forms of capitalism pertaining to different historical contexts.[57] But it also requires an analytical framework capable of distinguishing between the effects of capitalism itself from the effects of other forces or institutions. This is precisely what is lacking in the work of historians of capitalism, and economic historians should be well equipped to make progress on this front.

The closely related question of the origins of capitalism in North America may represent another opportunity for interaction and dialog. The effects of capitalism itself might be clarified if any pre-capitalist or non-capitalist contexts could be identified and compared to conditions under capitalism. Given the obvious value of such analysis for understanding capitalism, it is somewhat surprising that historians of capitalism have not already begun to pursue it. Of course there is a literature on the Market Revolution of the early nineteenth century (for example, Sellers 1992), and an older literature on whether or not the colonial economy constituted a “moral economy” or was always market-oriented. But much of

[56] A noteworthy exception is Neal and Williamson (2014), which chronicles the emergence of capitalism in many countries and regions around the world.
[57] Many definitions of capitalism exist, including those of Marx (1993 [1894]), who emphasizes private property, markets, wage labor, and profits, and Schumpeter (1939), who emphasizes private property and credit. Hodgson (2015) presents a useful analytical discussion of these and other definitions of capitalism. The economic historian Larry Neal (2014) proposed a definition of capitalism which focuses on the presence of institutions related to property rights and contract enforcement.

the evidence on which the participants in that debate have relied is rather fragmentary, and the mechanisms by which capitalism came to North America may not be completely understood.[58]

V. Conclusion

The emergence of the history of capitalism marks a return to research on economic history by scholars trained in history departments. Yet this new work is quite distinct from the field of economic history, which is both a strength and a weakness. On the one hand, it proceeds from a very different perspective, which emphasizes social criticism. On the other hand, its neglect of social scientific methods, and of the insights of the field of economic history, often substantially weakens its analysis and undermines its credibility as social criticism. In this essay I have described a number of those insights, in the hope that future work by historians of capitalism may adopt them. I have also tried to identify opportunities for greater engagement between the two fields, which would be of mutual benefit.

The growing popularity of the books by historians of capitalism also suggests some lessons for economic historians. Their books are engaging and interesting to read, and address the concerns of popular readers. Perhaps too much of the work of economic historians is written purely for academic economists, even though it holds important insights that are relevant to much broader audiences. Economic historians need to do much more to engage with popular readers, and make their work more accessible. The public face of economic history should not be limited to historians of capitalism.

[58] Rothenberg (1992) finds it was the latter. Jan de Vries (2008) argues that the notion that early Americans were not market-oriented "is an illusion—the Jeffersonian myth—to which American historians even now remain astonishingly loyal…" (p. 95). There is of course a significant literature on the emergence of capitalism in Europe (for example, DuPlessis, 1997) and whether or not early modern peasants were market-oriented (for example, Hoffman, 1996).

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