INEQUALITY, CAPITAL AND MANY OTHER THINGS IN THE 21ST CENTURY (AND BEFORE)

A Review of *Unequal Gains: American Growth and Inequality since 1700* by Peter H. Lindert and Jeffrey Williamson (2016: Princeton University Press)

Vincent Geloso
London School of Economics
Department of Economic History
v.geloso@lse.ac.uk

Editor's Note: It is *Essays'* tradition to publish the keynote address from the prior year's Economic and Business History Society (EBHS) annual conference as the lead article in each year's volume. Jeffrey Williamson delivered the keynote at the May 2015 conference, "Unequal Gains: American Growth and Inequality since 1774." As the talk was largely a summary of Dr. Williamson's (then forthcoming) book with Peter Lindert, and the talk can be viewed in its entirety on our website, we felt an appropriate treatment of this keynote talk would be a detailed review of the book.[1] The result is the following review by Vincent Geloso of the London School of Economics. Vincent won the Lynne Doti Award at last year's EBHS conference for the best paper presented by a graduate student.

[1] Here is a direct link to the keynote address: http://mymedia.uwlax.edu/Mediasite/Play/14198051eaa245fa96ededc4aa72f3da1d

Introduction

A year ago, members of the Economic and Business History Society met in Wisconsin for their annual conference where the keynote speaker was Jeffrey Williamson. For decades now, Williamson has been one of the foremost economic historians. His work has spanned numerous topics including crowding-out during the early industrial revolution, international migration, the effects of terms of trade on industrialization and the measurement of living standards. However, no topic seems to have been dearer to Williamson than inequality—the topic he chose for his keynote speech. The speech was based on his work with Peter Lindert on inequality in the United States since the colonial era. Between that speech and this writing, *Unequal Gains*, which summarizes their research, has been published. Both Lindert and Williamson have been frequent visitors to the issue of inequality. Lindert has been studying the role of the rise of the public sector on economic growth and the distribution for a very long time and produced one of the key books on the topic, *Growing Public: Social Spending and Economic Growth Since the Eighteenth Century* (2004). For his part, Williamson announced his interest in the issue as early as 1983 and returned frequently to this topic like in his 2005 work with Timothy Hatton on immigration and how it might have affected inequality (2005 [2008], 101-125). In addition, this was not their first joint foray on the topic in the form of a book (1980). Thus, *Unequal Gains* arrives as the culmination of a decades-long research agenda—an empirical magnum opus of the history of inequality in the United States.

Their work is colossal for two reasons. The first is the empirical nature of the contribution. After all, it is no small feat to document the evolution of inequality and growth in the United States across its history. The book's most important empirical contribution concerns the state of inequality in the United States before 1860.[2] While there will likely be debates and attempts to improve the estimates Lindert and Williamson provide, these estimates will likely form the basis of any future discussion. The second reason is that the book is not breathtaking in its conclusions, instead it is breathtaking in the care the authors deploy to avoid sweeping

[2] Readers can find these estimates in Lindert and Williamson (2013: 2016)

conclusions. In stark contrast to Thomas Piketty (2013)[3], Lindert and Williamson eschew the temptation to propose what could be labelled as general laws for inequality and economic growth. As a result, they inscribe themselves in the tradition of Simon Kuznets (1955). From their work, we are meant to understand that inequality is a multidimensional cluster concept that requires reference to a variety of variables to be properly understood: immigration, relative factor price, discrimination, regulation, international trade, urbanization, labor force participation, skill-biased technological change, market integration etc. Like a recipe, different mixes of these ingredients will yield a different course. Therein lies the true value of their contribution: the schematic for studying the evolution of inequality over the very long-run. Addendums can be made to their work so as to build upon it, but it should stand as the firm foundation of any future discussion.

The American Inequality History, in Five Acts

The chronological organization of the book supports this impression. The book documents the evolution of inequality and growth in the United States over five key periods: the colonial era (prior to 1776); the antebellum era (from 1790 to 1860); the postbellum era (1865 to 1914); the Great Leveling (from the 1910s to the 1970s) and the present era (since 1970). The colonial era is their most significant contribution as it is the first wide empirical measurement of inequality for the period. During the colonial era, they show (with detailed appendices at the end supporting their claims) that the American colonies were richer than England and Wales and formed a more economically equal society. To explain this first point, they highlight the main characteristic of the American colonies before the War of Independence: abundant land relative to labor and capital. As a result of this abundance of land, there was a trend in favor of ruralisation after 1680 (p.57-8) since opportunities to farm virgin lands seem to have provided greater returns to settlers. In essence, this first point is not new: As John McCusker and Russell Menard (1985 [1991], 304-307) emphasized, it was cheaper to increase production by simply opening

[3] This author has been working since 2013 with the French edition of *Capital in the 21st century,* since this reviewer is French-Canadian.

new farms than by adopting better farming practices. Relying on the insights of Kuznets (1955), who himself emphasized that cities tend to offer productive advantages that result in greater inequality as urbanization takes place, Lindert and Williamson argue that if ruralisation occurred it must have been because cities did not offer significant advantages. Indeed, they point out that in 1774, a northern unskilled worker in a city earned only 7.1 per cent more than an unskilled worker in rural areas (p. 85). For the overall colonies, the gap stood at 26.2 per cent. By comparison, the data presented by George Boyer (1990 [2006], 178) suggest real wage gaps vis-à-vis London ranging between 32.8 per cent (Kent) and 97.7 per cent (Wiltshire). The increasing ruralisation of America during the colonial era would have reduced inequality. It is worth noting that the low level of urbanization in colonial America is quite peculiar—a fact not noted by Lindert and Williamson. For example, the colony of Quebec in Canada had urbanization rate of 22 per cent in 1739 and 1765, compared with less than 10 per cent in the American colonies (Public Archives of Canada 1876, 60-67). The second feature of land abundant economies like colonial America that may have reduced inequalities relates to its price structure. Poor individuals tend to consume disproportionate quantities of goods that are land-intensive (i.e. food). Thanks to the abundance of land that made food prices low, the cost of a subsistence basket was considerably cheaper than it would have been in Europe. This would have meant that there was an "egalitarian price structure" in the colonies. It is not surprising then that inequalities were low relative to land-scarce and labor-abundant England.

The American Revolutionary War eliminated the lead the colonies had over England. Though, the United States would eventually recover its lead over England, it would experience a simultaneous increase in inequality. The war imposed a considerable trade shock that reduced exports substantially. Given that trade represented roughly 6 to 7 per cent of total income, Lindert and Williamson suggest that the fall in trade would have amounted to a loss of roughly 2 per cent of income per capita (p. 89). Their estimate is probably *too* conservative. Work by Douglas Irwin (2005) places a much greater importance to international trade in the American economy and added that the Jeffersonian Trade Embargo of 1808-1809 reduced per capita income by 5 per cent—an effect that would have compounded the losses from the Revolutionary War. In addition, trade shocks should not be seen as uniformly distributed. If the traded sectors were disproportionately linked to other non-exporting sectors of the economy, output in those sectors could have fallen considerably. Such an argument has been made in the case of the Smoot-Hawley tariff of 1930 by Mario Crucini and James Khan (1996). Since a large share of trade was in material inputs, they argue that tariff increases could have had a considerable effect on capital accumulation and output. It is possible that, following the Revolutionary War, a similar effect materialized. Either some export-oriented industries collapsed, affecting key localized industries or some inputs could not be imported as easily as before, thus constraining output in other sectors. This issue, given the empirical details provided by Lindert and Williamson, suggests that research in that direction could be quite fruitful to the understanding of early American history. In addition, given the paucity of the data for the early years of the republic, it is hard to assess whether or not the war increased or decreased inequality. Nonetheless, Lindert and Williamson confirm Robert Margo's (2000) finding that there was important economic growth in the United States from the end of the French wars in 1815 up to 1860.

On the eve of the Civil War, America had recovered its lead over England. However, there had been divergence within the United States as found in the past by scholars like Richard Easterlin (1960) and Weiss (1992), with the southern region growing more slowly than the north. This was a case of a reversal of fortune (see Daron Acemoglu, Simon Johnson and James Robinson 2002) for the southern states. Initially richer than the northern states (New England was barely equal to England prior to the revolution), the South suffered dramatically from the Revolutionary War and its recovery was more arduous—especially compared with the impressive growth rates observed in New England (p.106). Inequality across regions was a first contributing factor to the rise of inequality during the antebellum era. However, one should tread carefully here. In his initial work, Easterlin (1960) excluded Texas from the southern states for both the 1840 and 1860 estimates. However, when Texas is included, the southern states exhibit faster growth (Jeffrey Hummel 2012, 206-207). The sensitivity of growth estimates for the south has been noted by Roger Ransom and Richard Sutch (2001) in *One Kind of Freedom.* While it seems clear that the “old” southern states did experience slower growth, one should be careful about defining “the South” as states like Louisiana and Texas were frontier-regions where incomes—like with the Pacific states—were above national average and probably muddy the picture about the extent of regional divergence as a contributing factor. Nonetheless, there was also a rise of inequality *within* regions (a 13.6 per cent increase of the Gini coefficient in New England between 1774 and 1850 versus 18.9 and 23.7 per cent in the Middle Atlantic states and southern states) (pp. 38 and 114). This increase in inequality was driven by a factor that Kuznets could have predicted: urbanization. Unlike the days of the colonial era when the urban wage premium was small, the antebellum era was marked by much larger wage premiums for urban workers. As a result, employment shifted from low-paid rural to high-paid urban jobs. As the first migrants settled in, inequality increased. In addition, cities were more unequal places to begin with. As the ruralisation of America stopped and reversed itself, inequality began to surge. Lindert and Williamson assert that skill-biased technological change did occur whereby new factories required the use of unskilled labor, thus diminishing the demand for skilled artisans. Finally, they argue that financial deepening increased inequality. It worth pointing out, however, that although the direction of inequality during the antebellum would not be changed, the issue of slavery is quite problematic. While they consider that slaves did earn something in the form of the consumption they were given (retained earnings) and include them in their own calculations of inequality, some scholars could contest some assertions they made. Strategically, Lindert and Williamson also present their results for free individuals as well in order to deflect this potential criticism against their underlying results that inequality did increase. Indeed, even when they considered only free individuals, there was an increase in inequality. Furthermore, Lindert and Williamson do provide an extensive appendix which allows readers to reflect properly on their methods and possibly replicate them.

With the Civil War, the lead in incomes over England was lost but inequality remained high. In fact, it increased slightly as a result of the physical destruction wrought in the Southern States. The increased divergence across states acted to increase inequality, although there was a drop within the southern states as a result of emancipation.[4] The subsequent evolution of inequality and growth obeyed the same Kuznets-like forces as those observed in the antebellum era: inequality varied significantly by region according to the extent of industrialization and urbanization (see also Joshua Rosenbloom and Gregory Stutes 2008). However, other forces favoring deskilling were amplified.[5] An example is the arrival of numerous immigrants who pushed down wages for the unskilled. There were also forces that mitigated the increase. Lindert and Williamson point to the mobility of capital—in one of those rare exceptions where both capital and labor flow in the same direction—as a factor that eased the effects of immigration on wages and inequality. Here, readers will find that Lindert and Williamson use a computable general equilibrium (CGE) model (p.180-181) to assert the counterfactual scenarios in the absence of immigration and/or capital mobility. It is at this point that one can see how *Unequal Gains* builds upon decades of work by Lindert and Williamson. Indeed, this use of the CGE model refers clearly to Williamson's work with Alan Taylor on the issue (1997) and

[4] Readers should consult the work of Robert Higgs (1971) and the biographical essay associated with chapter 13 of Hummel (1996: 334-348).

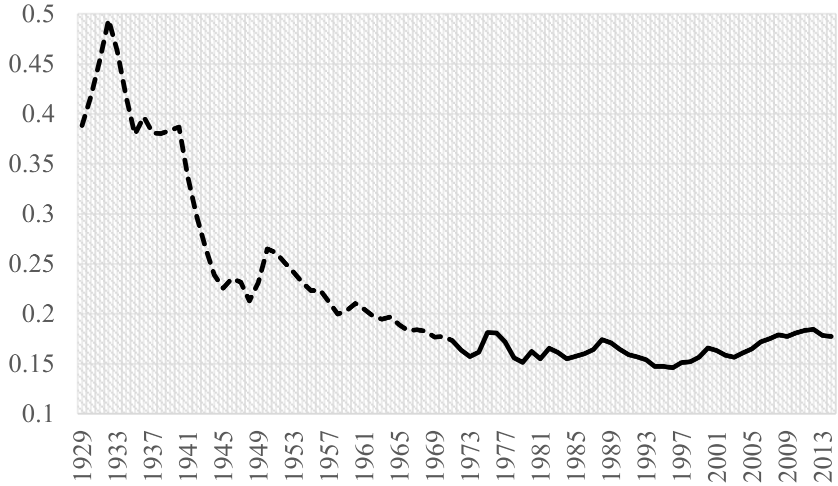
[5] Although he is not quoted in this work, Vaclav Smil's *Creating the Twentieth Century* (2005) provides a rich technological history of the changes that took place during the late 19th century. However, and this is important for the next era in Lindert and Williamson's narrative, these technologies that could have initially increased inequality also reduced them after a certain time. The scientific breakthroughs associated with the era between 1870 and 1910 allowed numerous ideas to be used towards the production of goods that improved mobility thus allowing individuals to exploit the best opportunities available, reduced the cost of information (and thus the benefits to investment in human capital) and the emergence of products that would reduce the inequality in "utility" between households. For example, refrigeration improved the diets of poorer households much more importantly than rich households who already had access to more diversified diets and were less subjected to seasonal variation in product availability. This would be quite in line with the arguments advanced by William Nordhaus (2004) that only a minor share of the social benefits of innovation are harnessed by inventors.

suggests that the current book had been long in the making. Two other forces prevented inequality from going up: while the South did not converge considerably with the rest of the United States between 1880 and 1910, there was significant convergence between the North, West, and Midwest regions (p.184). This acted to maintain inequality at the high level observed after 1870. The other force was the economic outcomes of the black population after its emancipation from slavery. Indeed, a modest convergence of the earnings of African-Americans occurred between 1870 and 1910. Although the convergence was slight, it is impressive that *any* convergence did occur in spite of institutionally-implanted discriminatory laws (Higgs 1977). In fact, recent evidence provided by Margo (2016) suggest that there was more convergence than usually believed.

From 1910 up to 1970, inequality plummeted significantly. [6] Obviously, changes in the scope of state intervention and the burden of progressive taxation played an important role. However, not only did the post-tax-and-transfer level of inequality fall, so did the pre-tax level of inequality as well. Among the factors that contributed to this reduction in inequality were a slowdown in labor supply growth, a rapid advance of education, the slowdown in technological bias against the unskilled, antitrade policies that suppressed imports of labor-intensive goods, and the retreat of the financial sector as a result of numerous regulations imposed from the 1930s onwards. This narrative is more commonly known to modern readers and it marks the downwards-sloping part of the U-curve of inequality in the United States as pictured by Piketty (2013) and Piketty and Emmanuel Saez (2003). After the 1970s, the return of skill-biased technological change, diminishing marginal returns to education, and the effects of financial deregulation caused a return of inequality that eliminated the levelling observed from 1910 to 1970. Lindert and

[6] Although a vast array of the literature finds that there was an important drop of inequality during the Great Depression, this author finds this claim debatable. At the very least, equality in a time of misery is no feat deserving pride. A great levelling towards the bottom is in fact quite depressive. Moreover, the work that focuses solely on inequality during the Great Depression does come to the opposite conclusion – that inequality increased during the Great Depression (Horst Mendershausen 1946). Further research should be undertaken on that particular point.

Williamson suggest that regional convergence as well as racial and gender convergence acted to mitigate this increase after 1970. Although I would agree with the statement regarding gender wage convergence, I would disagree with the two others. First of all, regional convergence is exaggerated. Indeed, the coefficient of variation of the average per capita income in the United States stood at 17.7 per cent in 1970 and 2013 (see Figure 1).



Source: FRED (Federal Reserve Economic Data, St. Louis Reserve Bank), *State Per Capita Personal Income.* 2016.

Figure 1
Coefficient of Variation of Average Income per Capita per State
(Great Leveling= Dashed Line)

The vast majority of the convergence between states occurred during the great levelling, not after. In fact, regional price differences might overstate the actual level of inequality. For the United States, the best piece of evidence is provided by Enrico Moretti (2008) who deflated US wages using a new consumer price index that reflects regional price differentials in order to study the real returns from going to college. In doing so, he found that half the increase in the returns to college disappears, which implies a lower level of inequality. This is consistent with evidence for China (Sylvie Démurger, Martin Fournier, and Shi Li 2006) and Canada (Krishna Pendakur 2002), whereby adjustments for regional price parities reduced both the trend and level of inequality. Considerations of regional prices and income differences suggest that the largest fall occurred before 1970 and that the level throughout the entire period is overestimated because of regional price differences (although less so as prices converge).

While the work of numerous scholars like Martha Bailey and William Collins (2006) and Margo indicate that there has been a modest trend towards racial equality (but by no means has there been equality), we should be more pessimistic. It is doubtful that there has been *any* convergence since the 1960s as this might be simply a statistical artifact caused by the design of wage and earnings surveys. Recent work by Becky Pettit (2012) and Bruce Western and Pettit (2005) shows that the black progress is more disappointing than seen at first sight, because mass incarceration removes the lowest-paid segment of the black population from the wage distribution. This leads to the impression of convergence across ethnic groups when looking at wages.

By running through all the causes of the changes in inequality, Lindert and Williamson basically have provided the schematic to study inequality. Authors studying inequality will have to refer to this cluster of concepts to see what forces are pushing up inequality and which other forces are pushing it down. Instead of proposing a storyline where capital is accumulated blindly by rich *rentiers* who never dilapidate their fortunes as Piketty (2013) claims (an empirically challenged claims as a longitudinal survey of the net worth of legendarily rich Americans unveiled—see Robert Arnott, William Bernstein and Lillian Wu, 2015 and Gregory Mankiw 2013 for a theoretical critique of wealthy dynasties), Lindert and Williamson propose that conventional (and much more mundane) variables should be considered to determine the causes of the rise in inequality and whether or not it is a problem.

Depressing Implications

The main problem with *Unequal Gains* is not its approach, it is that the conclusions are incredibly depressing in terms of policy implications. Basically, the U-shaped story of inequality they propose from the postbellum era to the present day can be interpreted as such: the high-point of inequality during the late 19th century seems caused by large flows of unskilled immigrants, the transition from an agrarian America to an urbanized America with important regional differences; the levelling that followed up to the 1970s seems in large part determined by the lessening of the effects of urbanization, the migration of blacks from south to north, restrictions on international immigration and the convergence of incomes across regions; and the increase in inequality past 1970 happened at the same time that there was a wide expansion in the scope and scale of the state. In the eyes of this reviewer, the depressing part of the empirical argument advanced by Lindert and Williamson is that, given the forces that operated for the great levelling and its reversal, the interventions of government seem to have mattered little. Indeed, the first phase of the expansion of the size of government occurred as the great levelling occurred. However, the great levelling occurred because of forces largely independent of the government (regional convergence, convergence across racial and gender lines), and the rise of inequality from the 1970s onwards occurred in spite of a substantial increase of the government's involvement in social affairs.

Indeed, as highlighted above, the vast majority of the convergence in average income per person across states occurred during the great levelling (see Figure 1). This substantial force in favor of income equalization across the United States would have had little to do with government intervention. Another force in favor of equalization during the great levelling would have been the increasing movement of blacks from the south to the north (Martha Bailey and William Collins 2006; Collins 2007; Leah Boustan 2009). Like regional convergence, this reallocation of labor across the country would have had little to do with redistributive policies but it would have levelled the playing field. As for convergence between genders, a part of this is explained by the emergence of new household technologies. According to Daniele Coen-Pirani et al. (2010), a sizeable share of the increase in married female labor force participation in the 1960s can be explained by household appliances. While work by Emanuela Cardia (2008) indicates that running water would have had a similar effect in the 1940s and 1950s and indoor refrigeration had a substantial impact on female labor force participation in the southern sections of the United States. To be clear, this reviewer is *not* stating that there were no leveling effects due to the rise of the welfare state (other works by Lindert on social spending and leveling are quite convincing, and a succinct summary of his argument can be found in Lindert 2014, pp. 464-500). Modestly, I am suggesting that the rise of redistributive policies in the first half of the 20th century may have been a small (but not-negligible) contributor to the leveling. Other contributing factors (regional convergence, racial equality, gender equality) operated regardless of state intervention.[7]

As for the era following 1970, the surge in inequality is stunning while state intervention continued and expanded. In the book, the figures provided by Lindert and Williamson suggest that inequality is inching towards the levels observed before the great leveling (the right-side of the U-curve of inequality). Expenditures on elementary and secondary education as a share of GDP has remained stable since the 1970s, while spending on post-secondary education as a share of GDP has increased (Robert Lerman and Stephanie Rieg Cellini 2009: 34), resulting in declining educational performance or stagnation at best (Eric Hanushek and Ludger Woessmann 2012). Overall, according to OECD statistics, social expenditures as a share of GDP increased from 12.8 to 19.2 per cent between 1980 (the earliest date in the OECD public expenditure database) and 2014. That an increase in inequality occurred in spite of such a surge in social expenditures is tremendously depressing.

It is worth pointing out that there are causes for de-dramatizing the rise in inequality that worries Lindert and Williamson. First of all, the inequality in earnings has not been matched by growing inequality in life satisfaction. In fact, inequality of "happiness" has actually declined, indicating that there is a disconnect between income inequality and inequality of well-being (Wolfers and Stevenson 2008). A similar decline in the inequality of happiness has also been observed across ethnic groups

[7] This offers an interesting avenue for future research. Indeed, it would be a considerable – but not impossible endeavor – to decompose the evolution of inequality in the United States from 1920 to 1960 (or 1970) by source (gender, race, region, redistribution). The answer to such an empirical question would be of considerable value.

(Betsey Stevenson and Justin Wolfers 2013). Secondly, ageing of population might be playing an important role. Thomas Lemieux (2006) best summarizes this approach. What we call "residual wage inequality" or inequality within groups of workers with the same education and experience "is generally believed to account for most of the growth in overall wage inequality" (Lemieux 2006, 461). This being the case, he points out that there are always unobserved skills when we estimate the determination of individual wages. If the dispersion of these unobserved variables was even across age groups, then the issue would be moot. However, Lemieux argues that unobserved skills are more dispersed amongst older and more educated workers. Hence, there is a composition effect that is concentrated in one group. Moreover, this measurement error may be growing over time as the relative size of the group that is being poorly measured increases. Consequently, he argues that ageing of population explains roughly three quarters of the increase in inequality. Recent research by Ingvild Almås and Magne Mogstad (2016) confirmed that a substantial share of increase in economic inequality might simply be the result of ageing and has little to do with unequal gains. However, even if we accept the most critical estimates of the rise in inequality since the 1970s, there remains an appreciable increase that suggest that economic gains have not been equally shared.

An Addendum to Policy Proposals

My sole criticism regarding Lindert and Williamson's work hinges on the limited treatment accorded to government policy. And this criticism should be construed more as an addendum than anything else. In their solutions to conclude the book, Lindert and Williamson propose a series of additional layers of government intervention to reduce inequality. These policy proposals (see pages 257-262) are well in line with the book's central contention that economic growth and equality are not antagonistic—for example, greater investments in education to raise the bottom of the income distribution. They do not dedicate considerable space to expound on these, but it would have been needless for them to do so considering that they had already done so (with considerable depth) elsewhere (see notably Lindert 2004; 2014). However, their proposals differ from Piketty's proposals only in the form that government action would take. Piketty proposes a net wealth tax, whereas they propose a tax on inheritance. In both cases, they presume, like Piketty, that a "stepping-up" of government action is required. However, the depressing interpretative points made above suggest that another factor should be considered in the analysis of inequality. Thus, my sole criticism (which is more akin to an addition to the list of variables to consider) is that they should consider the possibility that governments *increase* inequality so that policies aimed at the disengagement of the state could reduce inequality. This implies that, without rejecting their analytical tools, one could consider a "first-do-no-harm" policy. More precisely, it is quite possible that there exist policies that would increase economic growth while also reducing inequality by restraining government activities.

For example, consider the antebellum era in their narrative, when urbanization and technological changes were advanced as the driving factors of inequality. Government policies regarding trade restrictions would have been an important contributor (although this reviewer would not assign them the largest empirical effects). As Gordon Tullock (1967) famously emphasized, legislatures do not adopt protective tariffs on their own, but they appear out of rent-seeking in the political process. In the United States, the politics of protection were a very divisive issue during the antebellum era (Phillip Magness 2009; Robert McGuire and Norman Van Cott 2002) with the southern states preferring free(r) trade, the northern states preferring protection, and the western states being the swing votes (Irwin 2002). The structure of tariffs tended to affect manufactured goods. A land-abundant country like the United States would have tended to produce more land-intensive goods and trade those for labor-intensive goods like manufactured goods. However, the protection awarded to manufacturers in sectors like textiles (see Grant Forsyth 2006) and iron-making (see Joseph Davis and Irwin 2008) forced American consumers to deal with higher prices for these goods. Although America was widely agricultural at the time, its neighbor Canada was equally agricultural and imports of foodstuffs from Canada were heavily taxed. While Canadians did not impose duties on American agricultural produces after the Colonial Trade Act of 1831, Americans did not return the favor. Numerous Canadian contemporaries complained of the efficiency of American tariffs in keeping Canadian produces out of the

United States, which probably incited Canadian reactions towards protectionism during the 1840s (William Marr and Donald Paterson 1980: 134-135). Less urbanized and more thinly populated than the states with which it shared borders, the Canadian colonists could have easily been competitive on American markets, thus lowering prices for Americans in urbanized areas. Overall, specific and distortionary trade barriers probably increased inequality by altering the price structure of consumption baskets. A move towards freer trade would have alleviated income inequality by reducing prices for goods (and reducing earnings at the top).

Another example concerns the recent rise in inequality. Since the 1970s, numerous cities have imposed zoning regulations that restrict the supply of housing (Benjamin Powell and Randall Holcombe 2009). Given the increasing growth of the American economy and a growing population, it is not surprising that the ratio of median-housing prices to income has increased substantially (Randal O'Toole 2012). Simultaneously, fiscal encouragements to acquiring housing led the rate of homeownership to increase substantially above historical levels (Steven Gjerstad and Vernon Smith 2014). As documented by Matthew Rognlie (2015), this would have translated into important returns to the value of housing for initial homeowners, and increased prices for newly formed households and renters. This seems to have been a major driving factor in increasing inequality. This phenomenon seems to be shared across countries as Kristian Niemietz (2012) documented for Great Britain. Niemietz pointed out that the impact of zoning laws which restrained the housing supply resulted in price increases for housing of 40 per cent—a greater burden for poorer households. In both the case of Great Britain and the United States, easing zoning regulations would probably dilute the returns of homeownership (thus reducing gains at the top) and reduce housing prices (thus increasing real gains at the bottom for those who are renters).

These are only two examples and hundreds of pages could be devoted to interventionist policies that *increased* inequality while also *reducing* growth. They suggest that we should include the analysis of policies that exacerbated (or mitigated) the underlying forces at work in the economy to the work of Lindert and Williamson. As a result, this reviewer's addendum should be seen as a desire for future research to build upon the work of Lindert and Williamson. The analysis of the role of government in the evolution of inequality and growth is basically an empirical question, one that could not be tackled without the work currently being reviewed.

Conclusion

To their credit, Lindert and Williamson avoided the temptation to propose general laws of the increase in inequality. They do suggest that there are simple policies that can address the rise in inequality and this is where the true value of their work can be determined since scholars will have clear grounds to criticize their interpretation. By detailing all the variables researchers must look at, Lindert and Williamson gave economic historians a very detailed recipe book to analyze inequality. With such a unified framework, it becomes easy to interpret the issue and propose solutions for modern times. The schematic of Lindert and Williamson allows us to arrive easily at positive statements about the situation of inequality. Discussions about what is the "proper" level of inequality may contain some positive components, but such discussions contain plenty of normative components that imply subjective value judgment.[8] However, it is much easier now to avoid intermingling these components in order to properly debate—and we have Lindert and Williamson to thank for it.

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[8] Welch (1999) argues that there is, by virtue of the wages differences, a social value to a certain level of inequality because these differences indicate the proper allocation of resources. Thus, there could be an "optimal" level of inequality (see notably Gerald Scully 1992; 2003 who makes such a claim).

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