Defining Global Justice: the history of US international labor standards policy.

Capitalist Development in the Twentieth Century: An Evolutionary-Keynesian Approach

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John Cornwall and Wendy Cornwall, *Capitalist Development in the Twentieth* Century: An Evolutionary-Keynesian Approach. Cambridge and New York: Cambridge University Press, 2001. xv + 286 pp. $60 (hardback), ISBN: 0-521-34149-3.

Reviewed for EH.Net by Richard Vedder, Department of Economics and College of Business, Ohio University.

As David Colander accurately states in his forward to *Capitalist* Development in the Twentieth Century, John and Wendy Cornwall "are true, unrepentant Keynesians." In this *tour de horizon* of modern macroeconomic history, aggregate demand is the leading actor — cycles in economic performance are determined by the robustness of aggregate demand. The Cornwalls more or less believe in the reverse of Say's Law: Demand creates its own Supply.

The authors believe that the best single indicator of an economy's macroeconomic performance is the rate of unemployment. Unemployment was relatively low or "full" in many Westernized nations in the 1920s, and especially in the "golden age" of the 1950s and 1960s. The reason, they

believe, is that aggregate demand was growing by healthy amounts in those eras.
By contrast, the period since 1973 has been one of sluggish economic performance, explainable in large part by institutional (often government-imposed) restraints in the growth in aggregate demand. The slow growth in aggregate demand, the authors opine, has led to reduced savings, investment, and productivity growth. Of particular importance, nations where labor, business and government reached "social bargains" (incomes policies) were able to stimulate aggregate demand through government policy, but most of these social bargains fell by the wayside after 1973.

While my overall impression of the book is not favorable, it nonetheless has several strengths. Let me mention four. First, it is reasonably well written, using enough symbols, jargon and econometrics to keep professional economists satisfied, yet at the same time it is clear enough for the intelligent layperson to understand the rudiments of the main points. In an era and profession where writing incomprehensibly is considered to be a sign of virtue and erudition, this is no small accomplishment. To be sure, the discussion of such things as "hysteretic processes with exogenous origins" (p.102) is filled with typical academic pretentious jargon that would put the most diehard Keynesian to sleep, but on the whole this book is above average in clarity for economist-written works.

Second, the book makes an important point, that many economic model builders ignore, specifically that institutional arrangements and the structure of the economy matter, and often matter a great deal. Moreover, as the Cornwalls observe, institutional arrangements change over time with economic changes, and this can impact economic performance.

Third, while the authors are truly militant Keynesians, they realize that a 1950-style old Keynesian story simply will not cut it in today's world. In particular, they eschew Keynes's emphasis on the short run, and try to evaluate the impact that aggregate demand has on intermediate to longer run economic growth. With the decline in the importance of the business cycle, this is a necessary adjustment. The Cornwalls also reject or downplay much of the New Keynesian emphasis on microanalysis of wage and price rigidities (e.g., efficiency wages, menu costs, and so forth). Borrowing some from ideas of the New Institutional Economics, the Cornwalls believe that evolutionary changes in institutions and economic structure have an important role to play in explaining changing economic performance.

Lastly, as EH.NET readers will applaud, the Cornwalls appreciate the importance of history, and its usefulness in assessing economic phenomena. While not economic historians, they have written what is a somewhat less than comprehensive but still interesting macroeconomic history of the twentieth century within the context of trying to explain what makes the macroeconomic world work. Yet, despite all of these virtues, this is in my judgment a badly flawed book for a simple reason: I think the authors are just plain wrong in their assessments. Moreover, they are not merely sporadically wrong, but persistently and unrelentingly mistaken. To borrow a favorite Cornwallian term, this book suffers a bad case of misguided intellectual hysteresis. To be fair, I am not a Keynesian (although I started out as one), so a priori one would not expect a particularly positive assessment of this work from me. But I suspect that more neutral observers on the Keynesian/non-Keynesian continuum would find many of the same objections.

Before enumerating some problems with the Cornwalls' analysis, I would make an obvious point that the issue of whether economic progress is supply or demand-induced is not a new one. For example, many trees have been destroyed making books on the question of whether the Industrial Revolution is best explained by emphasizing supply or demand. In an era where demand is increasingly taken for granted, the Cornwalls' book does make us at least consider the possibility that the new (post-Keynes) conventional wisdom might be wrong.

I would also note that in some respects Cornwall and Cornwall show deference to an early, classical tradition that in some ways is the antithesis of Keynesian economics as practiced in the original by Keynes himself. For example, the authors stress the importance of capital formation in long-term growth, a view far more akin to Adam Smith than to Keynes. Original Keynesian analysis vilified savings, the funding source for capital formation, yet Cornwall and Cornwall believe that investment is critical to the dynamic process of long-run economic transformation. There is a bit of Adam Smith, and also a lot of Joseph Schumpeter, in the Cornwall and Cornwall interpretation of history.

Turning to the objections, it is argued that there are swings in economic performance explainable by changes in the robustness of aggregate demand influenced by institutional changes. In particular, the 1950s and 1960s were the "golden age" of modern economies, and the era since 1973 has been something of a disaster because of declining growth in aggregate demand.

Virtually the sole criterion used to evaluate economic performance is the unemployment rate. Unemployment is higher in the last three decades, so economic performance has worsened. I would suggest this is a highly questionable basic premise as it pertains to the U.S., although it is certainly more defensible for Europe. While average unemployment rates in the 1980s and 1990s were higher than in the 1950s and 1960s in the U.S., by most other measures the economy in the latter period either approximately equaled or surpassed the earlier record. Real per capita GDP grew 57 percent from 1950 to 1970 – and 55 percent from 1980 to 2000 – hardly an important distinction. Real household wealth rose faster in the latter period, and real per capita consumption rose by almost the same amount in both periods. Job creation was actually *greater* in the latter period — the number of new jobs per 100 incremental population over 16 was 64 in the 1950-70 period, compared with 81 in the 1980-2000 era.

The authors assert that increased unemployment was involuntary in nature, citing the rising duration of unemployment as evidence. I would argue that most the rise in unemployment, especially in Europe, reflected onerous new labor regulations and the impact that increasingly generous welfare state benefits had on the desire to work. Reservation wages rose sharply as the alternative to work — long-lived generous welfare benefits — became a viable option. Why is the duration of unemployment more than twice as high in Germany as in the U.S.? Germans can collect generous unemployment benefits for three to four times as long as Americans without any adverse consequences. These unemployed are hardly "involuntarily" out of work. A secondary factor in the unemployment rise in the 1970s and 1980s was demographic: an increase in the proportion of workers in young age cohorts that are typically more unemployment-prone.

The Cornwalls *assert* that governmental macro fiscal and monetary policies can reduce unemployment through heightened aggregate demand. It is argued that political constraints limited the use of demand stimulus after 1973. The evidence shows otherwise. In the U.S. the federal government ran far greater fiscal deficits on average in the two decades after 1973 than in the two decades before. For example, in the midst of the "golden age" of the 1960s, the federal deficit was less than one percent of GDP in eight of ten years, while the smallest deficit in the 1980s was nearly three times that amount. Monetary growth on average was greater in the latter era as well (the median annual growth rate of M1 in the 1960s was 3.5 percent; in the 1980s, it was 7.0 percent). The same pattern generally is true in Europe. The Cornwalls simply refuse to admit the problem may have been the impotency of macro stimulus, and they claim fiscal/monetary constraint in the latter period prevented full employment, despite the evidence that such constraint was simply not present.

The discussion of the Great Depression is also wanting. Other than the Friedman-Schwartz monetary explanation, there is no mention of other non-Keynesian explanations of the Depression, including ones stressing international monetary disturbances (e.g., Barry Eichengreen), Austrian business cycles, or the Hoover high wage policy. The Keynesian argument explaining the Depression was made better, in this author's judgment, by earlier writers such as E. Cary Brown.

Moreover, there is not a scintilla of hard evidence relating to the "social bargains" (incomes policy) allegedly common in the 1950s and 1960s compared with later years. There is no description of how these policies worked in specific countries, for example. We are supposed to take on blind faith the repeated assertion that income policies worked in producing the golden age of the 1950s and 1960s, but broke down somehow after 1973. Somehow a single regression equation (p. 91) with no social bargain variables is construed to support the Cornwalls' incredibly weak argument.

The book is full of absolutely wild assertions. A few samples: "The view that an increase in aggregate demand will not reduce involuntary unemployment because it is unable to reduce the real wage contains the implicit assumption that the real wage is determined in the labor market. This assumption has been shown to be unrealistic…" (p. 46) A single unpublished paper from 1990 is used to back up this assertion. Better yet, "Over two decades of neoliberalism have revealed its similarities to the laissez-faire regimes of earlier times — prosperity for the few and insecurity for many" (p. 268). To argue that in, say, the 1990s, few had prosperity but many were economically insecure in the U.S. or Europe is simply fiction. Speaking of the era after the golden age, the authors claim that "the role of government in domestic and international economic affairs has been greatly reduced, social bargains no longer dominate labor market outcomes and price stability has become an overriding economic goal" (p. 242). It is a fact that government spending as a percent of GDP has risen, not fallen, in nearly every major western industrialized country in the era since the so-called golden age, and regulatory activity has increased as well. To say that government's role has been "greatly reduced" simply defies the factual evidence.

The possibility that rising unemployment and sluggish growth in Europe reflects the debilitating effects of high taxation, regulatory rigidities, and the disincentive effects of the welfare state is virtually ignored. There are a variety of plausible explanations for economic changes that have occurred in the past several decades, but the Cornwalls have not presented them. Save your money: don’t buy this book.

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Defining Global Justice: The History of U.S. International Labor Standards Policy

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Edward C. Lorenz, *Defining Global Justice: The History of U.S. International* Labor Standards Policy. Notre Dame, IN: University of Notre Dame Press, 2001. x + 318 pp. $54.95 (cloth), ISBN: 0-268-02550-09; $27.95 (paper), ISBN: 0-268-02551-7.

Reviewed for EH.NET by Mark Perlman, Department of Economics, Emeritus, University of Pittsburgh.

Defining Global Justice is by a social historian who chronicles in an unusual and intriguing way the rise and eventual sequential transformations of the International Labor Organization from an agency originally intended to standardize and enforce internationally what industrialized nations were coming to believe to be workers’ rights to an agency that is, at its best, no more than ‘an adult education program.’ His approach involves tracing the private-public interest groups that first created the International Labor Organization and then maintained an interest in the United States for American participation.

It is one of the lesser ironies of our times that few who hear of the rioting whenever there is a global economic conference realize that a great deal of the history of economic thought has been tied up with profound differences about optimal policies regarding trade among nations. That fact is not the best evidence that can be adduced about the real costs of the economic profession's degrading of that sub-field but perhaps it is the most recent.

Since this review is primarily directed at economists rather than historians it is useful to start with a digression summarizing the history of the profession's perception of the underlying problem, i.e. applications of the static theory of the benefits of free trade to a dynamic world.

Laisser-Faire vs. Mercantilism

The Physiocrats' "laisser-faire, laissez-passer" really applied to trade within France. It was Adam Smith who best popularized free trade among nations. And even Smith, himself, must have had profound doubts about that program as a national policy since he subsequently gladly accepted appointment as Collector of Customs in Scotland, a traditional family sinecure, where he distinguished himself by strenuously collecting all the duties owed — something his own familial predecessors had pursued only lackadaisically.

David Ricardo gave Free Trade Doctrine its major theoretical thrust, albeit his model, neglecting transportation costs (to say nothing of costs of information), was of a static nature. Nonetheless, the Doctrine became the dominant program argued by the leading British social thinkers such as John Stuart Mill. Indeed so strongly did Alfred Marshall feel about the issue that he intervened — certainly unusually, if not actually improperly, in the selection of his successor and promoted the candidacy of a very junior pro-Free Trade, Arthur Cecil Pigou, over the more conventional candidacy of Herbert Somerton Foxwell, who was skeptical about the social costs of the Doctrine.

In recent decades what is left of the alternative doctrine in Britain was Professor Kaldor's suggestion that the British Free Trade Doctrine was at best a special case for Protection – at a point when one's own nation is head-and-shoulders ahead of all others from the standpoint of technology, then a policy of universal Free Trade makes dominant sense. Kaldor's view, whatever its actual source, also can explain the famous Menger-Schmoller Methodenstreit, which although phrased in terms of deduction versus induction, was most probably about Menger's fear of Prussian protective-trade policy crippling Austrian economic development. This is the explanation offered by Joseph Schumpeter — who, by the by, shared Menger's fear of Austria being economically exploited by its very much larger and more economically developed northern neighbor.

Labor and the Closing of Borders

In order to put the problem in its bitterest context, we turn to the questions not of the international mobility of products nor even the international mobility of capital, but of the international mobility of labor. Although there are many theories of why and how labor suffers from the industrialization process the two dominant ones are theories of exploitation on the shop floor (by the employer who owns the capital) and exploitation in the market place by the appearance of cheap foreign goods and/or cheap 'foreign' labor. The first of these (exploitation on the shop floor) was enunciated by Marx and very much broadened first by John A. Hobson and then vastly popularized by Lenin, both of whom stressed that capitalists would forever be looking for both cheaper labor and unexploited product markets — first at home and then abroad. The second theory (exploitation in the product and labor-factor markets) was developed at the University of Wisconsin by John R. Commons who saw as a data-derived generalization (explaining American unionism) a record of workers' efforts to keep up product prices so that their wages would not suffer. Commons's price/wage relationship, implemented by workers' opposition to 'cheap goods' (meaning imports into any local market) and 'cheap labor' (meaning greenhorns and green hands), was consistent with his views about curtailing immigration, particularly from Eastern and Southern Europe. It is perhaps an anomaly that his successor as a labor historian, Selig Perlman (himself, an immigrant from Tsarist Russia), recast the Commons theory into its present form — namely that American workers with a history of political experience and self-organization came to view (and thus to justify) their jobs as a collective property right; and for good reason they had been more confident of the method of collective bargaining than the method of legal enactment. Perlman's representative unionists grasped with both hands the principle of unions being the protector of a collective job-right — right to ownership being probably as American a cultural tradition as any.

Thus it was that within the economics profession the policy issue of international mobility of products and factors often separated deductive (theorist) economists from inductive (in this case labor) economists. Each group went its own way; the former usually proving deductively (but without so specifying that it was a static model) the overall advantages of free trade in goods and services as well as the advantages of easy movement of the factors of production, while the latter embraced the idea that such competition should be regulated by a series of international conventions designed to improve the living standards of workers in backward economies with the intended result that the urge to migrate from backward economies to industrialized ones would be mitigated. As is all too well-known, interest in turn-of-the-twentieth-century labor problems (also known as Commons's Institutionalism) all but died in the 1970s.

The Book at Hand

1. Lorenz's Approach. Thus it is that the book under review may seem to most of today's professional economists as an anachronism. But its author, Edward C. Lorenz, who teaches history at a relatively small Michigan college, seems blissfully unaware of the rigid graduate-school-head-start-school conventions found in most college's economics departments' curricula. The consequence is, as already noted, both a novel and very interesting history of real-life battles regarding international labor standards and an important reminder that within the traditions of our profession there once thrived a strong concern about standards of human dignity. And if that tradition is now moribund amongst us, it thrives elsewhere in our country.

The treatment, arranged into eight chapters, is chronological. The narrative starts with Lorenz discussing the evolution of international reform movements growing out of private organizations. More important than his list of names (not that names like Robert Dale Owen, Daniel Legrand, Karl Marx, etc, are not in themselves interesting) is his development of an empirical thesis about inter-faction cooperation such that in the end welfare reformers, advocates of factory acts, trade unionists, and most important clerics and academics managed both to popularize Progressive reform in the years prior to World War I and to reorient American policy anent the ILO during the New Deal years and most recently during the decade when Soviet hegemony was imploding. Briefly put, most of these factions were steered by elite groups who had both agendas and a capacity for organizing grass root support. Among them were not only intellectual 'do-gooders' such as social workers like Jane Addams, but also influential Roman Catholic and Protestant clerics (the one influenced by *Rerum Novarum*, the other by ideas similar to Rauschenbusch's Social Gospel), and that says nothing about the quondam power of John R. Commons's and others' American Association for Labor Legislation.

Eventually these activities ripened so that when the League of Nations was established a separate body, the International Labor Organization, was also created. The League was a legislative body made up of national governmental delegations, each named by a member country. By way of contrast the ILO was made up of governmental delegations of which two were named by each government and one each from each country's labor groups (meaning unions) and one from industry (meaning industrial confederations). In short, the ILO was tripartitism in practice, certainly a concept originally formulated by men like John R. Commons. A secretariat (under an elected Secretary-General) was set up in Geneva. He exercised the Prerogative between annual conferences where the delegates assembled to fraternize and pass conventions pertaining to minimum standards for industrial life. He also had a secretariat , the principal nominal duty of which was to collect relevant data (in the tradition of the

American Bureau of Labor Statistics).

2. The American Record. In any event, in 1919-20 the Congress rejected American membership both in the League of Nations and the International Labor Organization. But that was hardly the end of the story. Frances Perkins, Franklin Roosevelt's Secretary of Labor, and Isador Lubin (her Commissioner of Labor Statistics) had always been sympathetic to American membership in the ILO, and the total collapse of the American textile in New England gave them (and eventually even the Congress) sufficient reason to reconsider the earlier refusal. In 1935 the Americans joined — even though it was clear that many Americans, particularly those who favored isolationism and the freedom of contract provisions found in the fifth and fourteenth amendments, continued to fight against participation. Even the American unions (the AFL) were split about membership — one faction arguing the traditional line about collective bargaining, not the method of legal enactment, being the way to go, observed all too tartly that any conventions that the ILO was liable to vote would be much weaker than American unions had already or should achieve through bipartite bargaining.

During World War II the ILO removed its headquarters to the Western Hemisphere. Its biggest achievement (as part and parcel of the multipartite negotiations that included the Bretton Woods Agreement) was a program voted in Philadelphia in 1944 including planks referring to:

1. Programs of minimal income security 2. Health insurance for all workers 3. Social security for members of nations' armed forces. 4. Organized programs for the demobilization of World War II veterans 5. Publicly paid-for employment exchanges 6. Public works to relieve cyclical unemployment 7. Agreement that all these programs would be extended to colonial territories.

After the war employer resistance to the ILO, if anything, increased — a 'Bricker' Constitutional Amendment was repeatedly proposed to formalize the point that no international treaty could in any way supplant the aforementioned two Constitutional Amendments. Nonetheless between the efforts of internationally-minded reform groups and a more-or-less indifferent labor union attitude American membership was maintained, albeit that during the Eisenhower Administration the United States did try to direct the ILO secretariat to pay more attention to statistics-gathering and less to policy statements.

But what eventually in 1975-77 managed to all but end American participation was a growing realization that the Secretariat was clearly too friendly to the newly-joined Communist countries and that the annual conferences were becoming vociferously critical of American foreign policy choices. Increasingly American unions despaired of getting the ILO to condemn the slave labor conditions in Communist countries and they moved to persuade some in Congress that the ILO was becoming a Soviet partisan. When the ILO voted to give the Palestine Liberation Organization observer status Congress eventually stopped the ILO appropriation, and President Ford gave notice of American withdrawal.

However, within a couple of years (1978) not only was a Pole elected Pope, but in Poland a shipbuilders' union, Solidarity, emerged as the leader in disrupting the Soviet hegemony. And the new Carter Administration had turned strong programmatic attention to human rights violations. These, together, served to crumble any further labor resistance to membership in the ILO, and the Americans appeared once more on the Geneva scene. Since that time the positions of various private public interest groups have shifted. The clerics, particularly some leading Roman Catholics, continue to take a very strong interest in labor problems throughout the world. Economists, however, have become enamored by abstraction and few, if any, write about labor problems any more. Yet, among other academics, particularly those in law faculties, the old concerns remain viable. And given the growing importance of multinational corporations, business's attitudes have become far more sophisticated; they no longer oppose international conventions, as such, they merely object — in the name of freer trade — to the conventions being enforced.

3. Conclusion What Lorenz chronicles is the long experience within the United States of various groups' interests not only in the ILO (with all the vagaries

of its choices reflecting the growth in number of Communist and then liberation governments) but even more in trying to formulate American international policies setting those civilized standards. Thus it is a history of the transformation of Roman Catholic doctrine about the role of unionism (as seen several encyclicals — specifically *Rerum Novarum* [1891], *Quadragesima Anno* [1931], and *Centesimus Annus* [1991]), a record of the American Federation of Labor's coping with left-wing radicalism seen not only internationally but also domestically, and an account of a wide-variety of transitory groups (e.g., even the American Enterprise Institute) intent upon making the world a better place for workers.

The great virtue of Lorenz's sympathetic treatment of protests against consumerism-uber alles, is a concern about not only working conditions but also much of the current impetus to emigration, the importance of which cannot be swept away either by police protection (as in riot control) or intellectual neglect (as in the professionalization of economics). No doubt cheap clothing has its virtues; but it also has its costs. One cannot do better than recall a few of the lines of Thomas Hood's 1843 *Song of the Shirt* —

1 With fingers weary and worn, 2 With eyelids heavy and red, 3 A Woman sat, in unwomanly rags, 4 Plying her needle and thread–5 Stitch! stitch! stitch! 6 In poverty, hunger, and dirt, 7 And still with the voice of dolorous pitch 8 She sang the "Song of the Shirt!"

17 "Work — work — work 18 Till the brain begins to swim, 19 Work–work–work 20 Till the eyes are heavy and dim! 21 Seam, and gusset, and band, 22 Band, and gusset, and seam, 23 Till over the buttons I fall asleep, 24 And sew them on in a dream!

25 "O, Men with Sisters dear! 26 O, Men! with Mothers and Wives! 27 It is not linen you're wearing out, 28 But human creatures' lives! 29 Stitch–stitch–stitch, 30 In poverty, hunger, and dirt, 31 Sewing at once, with a double thread, 32 A Shroud as well as a Shirt.

89 –With fingers weary and worn, 90 With eyelids heavy and red, 91 A Woman

sat, in unwomanly rags, 92 Plying her needle and thread– 93 Stitch! stitch! stitch! 94 In poverty, hunger, and dirt, 95 And still with a voice of dolorous pitch,– 96 Would that its tone could reach the Rich!– 97 She sang this "Song of the Shirt!"

4. A Troubling Postscript Yet, the lure of cheap goods has been irresistible, and multinational corporations have learned to give lip-service (if no teeth are involved) to international labor standards. The ILO bureaucracy is safe, talk is endless, and little really changes.

Moreover, there is no proof that raising wage-costs (with loss of employment opportunities) in developing countries would not work in the direction of greater emigration. If the division that now threatens war between most Islamic nations and the West suggests anything, it is that the culture of democratic representation envisaged by those who first agitated for, then created the ILO, and afterwards ran it is not the culture of most of the poorest national economies. Wilson's dream of making the world safe for democracy (and democratic institutions) was punctured in 1920; it flew briefly during and after World War II, and then again after the implosion of the Soviet economies. But now the idea of representative democracy and such things as free trade unions are stuff of the Western world — perhaps a recurrent dream for others, but not one easily made a reality.

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Coercion, Contract, and Free Labor in the Nineteenth

Century

Author(s): Steinfeld, Robert J.

Reviewer(s): Hamilton, Gillian

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Toronto.

In 1991 Robert Steinfeld (SUNY-Buffalo) published *The Invention of Free* Labor, a study of the legal history of labor in the U.S. and England between the fourteenth and nineteenth centuries. It was an important volume that excited economic historians interested in labor markets. Workers were "free," Steinfeld argued, when they were no longer compelled to complete their contracts under threat of criminal sanction such as imprisonment. Contrary to popular opinion, it was not just indentured servants that faced such strictures in the seventeenth century — it was the norm for all workers. By the early nineteenth century, workers achieved "freedom." By this time specific performance and imprisonment for labor contract breaches, even for immigrant workers, was perceived as little different from enslavement and made illegal. The book described the legal transformation of indentured servitude and wage work in the United States, offering a less detailed treatment of contract workers in England for comparison.

In *Coercion, Contract and Free Labor*, Steinfeld pursues similar themes but reverses the geographic emphasis. Drawing on a wide variety of sources that includes court cases, judicial opinions, parliamentary minutes, bills and testimony, as well as prosecutions under the Master and Servant Act, he offers an in-depth narrative of the steps that led to the repeal of criminal sanctions for labor contract breaches in England. Steinfeld begins by arguing that enforcement through criminal sanction was an integral part of the employment relationship. Examining the numbers of prosecutions under the Act, he illustrates that prosecutions were not at all rare (more than 10,000 cases per year between the 1850s and early 1870s), generally grew over time, varied by county and varied with the business cycle.

We also learn about different groups' agendas and their attempts to change the laws; the evolving court interpretation of existing laws; and the responses of employers and employees to these changes. For example, the court's interpretation of the legislation became increasingly broad. It drew more and more workers under the Act as its definition of coverage evolved. It also ensured that contracts were more enforceable when it no longer required contracts to specifically state that the employer would provide work — this "mutuality" obligation was deemed implicitly obvious. Steinfeld's narrative makes it clear that one cannot be lulled into believing that a legal environment was static even if there were no changes in the law.

As the author notes, the possible effects of changes in the court's interpretation of the Act on such factors as contract length and the method and frequency of compensation (piece rate versus straight time, for example), should be of interest to economic historians (p. 166). To illustrate, Steinfeld focuses on changes in contract length. "Minute" contracts, which did not require notice and so essentially circumvented the Act, became more common at least in some coal mining regions. He discusses the pros and cons of short versus long contracts in some detail, invoking a game theoretic approach to help explain the move towards shorter contracts. Ultimately he concludes that there may be no clear relationship between duration and the interpretation of the law: "the new rule [mutuality] may have led them [employers] to press workers harder for shorter contracts without earnings guarantees, and perhaps even on occasion long contracts without earnings guarantees" (p.188). This conclusion undoubtedly reflects the complexity of the subject matter, but having read more than sixty pages on legal interpretation I was eager to read about more substantive effects of these changes! In-depth empirical analysis of these relationships is generally beyond the scope of this book, however, because the author does not have a set of employment contracts from which to draw inferences. The book does, however, offer us considerable opportunity for (very interesting) future research. Did the incidence of piece rate contracts change over time, for example, and did this reflect changes in the court's willingness to include these workers under the Act?

Steinfeld goes on to discuss the political process of reform. One of the most surprising elements of this story is that criminal sanctions were so entrenched and accepted in England that although individuals and groups had been agitating for reform of the Master and Servant Act since the early 1800s, no one had even considered removing them from the legislation until just before it was struck down in 1875.

Steinfeld also discusses other countries, primarily the United States, for a comparative perspective. In a departure from his earlier work, he argues that while workers could quit without threat of criminal sanctions by the early nineteenth century, they were not truly free because they was still subject to coercion. American employers relied on a variety of pecuniary means of enforcement. In situations where workers could either complete their contract or forfeit all of the wages owed to them (which could amount to several months pay) they were left with little choice but to complete their term. Depending on the circumstances, Steinfeld argues, pecuniary enforcement could have been as unpalatable (and hence coercive) as imprisonment. Modern legal freedom for workers was not achieved until the early twentieth century when sufficient legal strictures were put in place to limit employers' ability to construct coercive contracts that effectively denied workers the right to quit.

This is another stimulating thesis that is worth pursuing. Are penal and pecuniary penalties perfect substitutes? Overall, were English workers more or less coerced than their American counterparts and what effect did this have on productivity, turnover, and employment relations? Early on, Steinfeld argues that "[without criminal sanctions] the contract labor system never developed into a significant source of labor for American employers" (p. 32). But later he argues that pecuniary remedies could be as disagreeable as non-pecuniary ones (p. 310). If so, why wasn't contract labor more common?

The Invention of Free Labor is a hard act to follow. Steinfeld does an admirable job with *Coercion, Contract and Free Labor*. It will enrich those interested in labor relations and stimulate further research.

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Battles for the Standard: Bimetallism and the Spread of the Gold Standard in the Nineteenth Century

Author(s):	Wilson, Ted
Reviewer(s):	Meissner, Christopher M.

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Ted Wilson, *Battles for the Standard: Bimetallism and the Spread of the Gold Standard in the Nineteenth Century*. Aldershot, UK: Ashgate, 2001. xi + 200 pp. $69.95 (cloth), ISBN: 1-85928-436-1.

Reviewed for EH.NET by Christopher M. Meissner, Faculty of Economics and Politics, University of Cambridge.

In *Battles for the Standard*, Ted Wilson aims to explain why the gold standard moved from an exclusively British institution at the beginning of the nineteenth century to the most widely used monetary arrangement in the world by 1910. The author offers a number of case studies each of which emphasize that broad ranging explanations are inadequate to explain why the world went to gold. Wilson also examines bimetallism as a brake on the spread of the gold standard between 1870 and 1913. In his opinion, bimetallists failed because of the inability to formulate a coherent vision of what the candidate regime would look like and how the system would perform if implemented. Those interested in institutional change or the evolution of the international monetary system will feel the book presents some interesting research.

Wilson opens with a general examination of monetary arrangements during the nineteenth century. A series of chapters then outlines nineteenth century monetary history in Great Britain, France, India, and the US. The penultimate chapter considers and challenges current explanations for the emergence of the gold standard making reference to particular country experiences and the final chapter is about bimetallism in England during the 1890s.

The country coverage begins with Great Britain. The goal is to explain why it chose the gold standard in 1816 long before any other country had done so, and why it clung so steadfastly to the gold standard after 1870 in the face of considerable international maneuvering to establish bimetallism. British currency had been de facto gold through much of the 1700s and until 1800 lacked small denomination coins. The remedy was to implement a de jure gold standard so as to free England from the effects of Gresham's law and to keep token silver coins in circulation. The fact that gold was the outcome in 1816 seems to have been pure historical coincidence. The author also gives air time to Angela Redish's explanation that technological advances in steam pressing in the late eighteenth century allowed a token silver coinage which people could not counterfeit and which circulated along with full-bodied gold coins. This was an answer to bimetallism. It provided coins of silver and gold in denominations and weights appropriate to the value of a particular transaction without being exposed to Gresham's Law. Wilson points out that few contemporary sources cite the steam technology as a reason for adopting the gold standard, and so he is skeptical that British obstinacy was based on these arguments. Furthermore European countries with access to the same technology did not adopt gold immediately. But arguments like Redish's also rely on the notion that a gold standard was suited for more-developed countries because their average transaction was of a high value and bulky silver was inconvenient. And Germany and France did not reach the levels of 1820 British per capita GDP until about the 1860s precisely when these countries began agitating for an international gold standard (Maddison, 2001). The author explains how, during the 1880s, an appreciating exchange rate vis-?-vis silver countries made necessary imports cheaper while politically impotent agricultural interests were thrashed about by import competition. Britain therefore clung to gold.

Continuing his global overview, Wilson looks at France's deep romance with bimetallism — a regime it finally relinquished in 1878 as the world silver market collapsed. It is suggested that the example of French bimetallism and its success between 1850 and 1870 provided a success story to which bimetallists in the 1890s could refer. In discussing France's strong support for bimetallism up to 1878, Wilson dismisses the notion that the Banque de France benefited from the arbitrage opportunities bimetallism presented. Instead, Wilson argues tradition and historical esteem for the status quo explain France's policies in the period. Although this explanation may be correct, the evidence presented is not convincing. For one, Wilson claims that since the gold price in terms of silver was stable from 1850 to 1870 there were no arbitrage opportunities. But Flandreau (1996) argues just the opposite. Arbitrage, perhaps by private agents, (who incidentally had some say on the board of directors at the Bank) actually worked to keep the price from straying too far from the mint ratio. And Einaudi (2000) presents an in-depth analysis of Bank of France archival records from the 1870s showing what the interests of the Bank were. Wilson's work could have benefited from such archival investigation if only to lay bare the economic motivations of relevant actors.

Indian monetary history from the early 1800s up to 1900 is next on Wilson's list of case studies. The chapter opens with a lengthy narrative on early nineteenth century Indian monetary history, and a conventional view of Indian regime preference after 1870 is presented. After about 1873, colonial powers would have preferred a gold standard in order to stop the rise in the value of the home charges and to keep their silver denominated pensions from depreciating in gold terms. Local export-oriented industrialists supported silver largely because of the expansionary effect of a continuously depreciating currency.

Americanists will find Chapter 5 on the United States to be somewhat sparse if not highly stylized. Wilson portrays the country as a relatively backward place where frontiersmen sought salvation in paper currencies. This line of argument neglects, or at least avoids, discussing the economic interests of the constituencies that shaped the debate and international differences in political procedure and decision making. Wilson pays little attention to the standard debtor-creditor debate or to the more contemporary open-economy politics view of Jeffrey Frieden (1997) where exporters and transport interests supported a depreciating standard. Nevertheless, the discussion of the conflict with Great Britain over Venezuela in the 1890s and how the gold-bug Cleveland administration used political uncertainty and hence money market uncertainty to discredit silver agitation is intriguing. (Many readers will be irked by seeing McKinley repeatedly referred to as "McKinlay" towards the end of the chapter.)

The following chapter ties up loose ends by confronting previous hypotheses about the emergence of the gold standard with historical experience. These focus not only on the countries already treated but also Germany and smaller peripheral countries. The seeds of what might have been an entire chapter on Germany appear here. Wilson asserts that Germany's adoption of the gold standard "helped secure her economic leadership of Europe after 1870 (p.124). Even if we are to believe the notion, what was the transmission mechanism? Was it that gold provided "hegemony over France" (p. 124) and somehow defeated this commercial rival or was it through increased trade benefits by linking up to the gold network? On historical grounds, we also have to suspect the digging has not been deep enough here. Wilson clings to a notion that the French indemnity of the Franco-Prussian war was paid in gold. Flandreau (1996) and Einaudi (2000) document that only about 5 percent of the indemnity was paid in specie the rest being paid in commercial paper drawable in various financial centers.

Even more confounding is the short follow up on the American adoption of the gold standard. The argument suggests that policy in the US was made without respect to the rest of the world. This is hardly the case. Much of the debate, which is documented in a lengthy set of congressional hearings held in the 1870s and published in 1879, was about ascertaining what exactly the rest of the world would be doing in the future. There is also a lengthy discussion on Bordo and Rockoff's "Good Housekeeping Seal of Approval" hypothesis. The book proposes that there is no evidence that nations consciously sought to lower their borrowing costs or receive special treatment on international capital markets by adopting the gold standard. But historical evidence again snags the author's momentum. It is widely argued that one of Russia's primary motivations for moving to gold convertibility in the 1880s and 1890s was to attract foreign capital, and American Congressional discussions in the first decade of the 1900s on why China should adopt the gold standard centered on the ability to attract more foreign capital. A number of other aspects of monetary regime transformation such as lock-in and strategic complementarities, imperialistic preference for a non-gold periphery and precious metals discoveries are touched on near the end of this chapter as explanations for gold's triumph.

The book winds down with a novel discussion of the emergence of a bimetallist movement in Great Britain near the end of the nineteenth century. Wilson centers his discussion in Lancashire. Essentially textile producers and laborers aligned themselves with a hope that bimetallism would stave off increased imports of Eastern textiles. Indian cotton manufactures benefited from the continuous depreciation of silver against gold and hence eroded market share, jobs and profits in England. Lancashire's bimetallist agitators faced stiff resistance from City financiers and unsympathetic governments. But bimetallism appears to have been its own worst enemy. Its advocates failed because of the inability to present a coherent platform. What would the mint ratio be? Should it be the current market value of 35 to one or perhaps the older 15.5 to one? Should Britain insist on an international coalition to support such a move or would it go alone? Could Britain find a coalition in any case? No simple answers came from the movement, and gold took the day. The arguments here are interesting and suggestive, but the author could have spent more time on the little researched area of the viability of international bimetallism in the late nineteenth century. The author raises interesting questions, but there could be more discussion of the menu of alternatives and the benefits. Too little time is spent exploring the real benefits from the gold standard, and the author precipitously blames bimetallism's failure on the incompetence of the movement's leaders.

Overall this work is a good narrative of institutional change in the international monetary system. It provides a one-stop-shop for most of the current thinking about the emergence of the classical gold standard and the disappearance of bimetallism and silver between 1870 and 1913 while also providing a nice range of salient case studies. The book will prove useful for initiates to the literature. However those wishing to formulate solid opinions about the formation of an international monetary system will not feel the book has provided enough archival, statistical or theoretical ammunition to take out the more entrenched explanations. Nevertheless the book does succeed in laying the foundation for a debate about why bimetallism failed in the late nineteenth century. This is a corner of the literature that has seen far too little attention but it is a prime example of institutional change and path dependence in an important sphere of the economy. It certainly deserves more along these lines.

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Subject(s): Financial Markets, Financial Institutions, and Monetary History

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The Economy of Europe in an Age of Crisis, 1600-1750

Author(s): Vries, Jan de

Reviewer(s): Grantham, George

Project 2001: Significant Works in Economic History

Jan de Vries, *The Economy of Europe in an Age of Crisis, 1600-1750*. New York: Cambridge University Press, 1976. xi + 284 pp. ISBN: 0-521-29050-3.

Review Essay by George Grantham, Department of Economics, McGill University.

An Economy in Crisis

First published in 1976, *The Economy of Europe in an Age of Crisis* was chronologically the fourth in a series of general syntheses of European economic history commencing with Robert Lopez's account of the medieval economic boom and carried forward by Harry Miskimin's two volumes on the economic history of the Renaissance.1 The four works by two Yale professors of economic history and one of their students constituted as it were a 'Yale' history of the European economy, which was distinguished from other works by its attention to macroeconomics and the implications of general equilibrium. One recalls hoping for an ultimate volume from the pen of Yale's other senior economic historian that would bring the story out of Europe to America and through the Industrial Revolution to the mid-twentieth century. Alas. The hungry sheep look up and are not fed. … Weep no more, woeful shepherds.

Jan de Vries' contribution to this series deals with a particularly enigmatic period in the history of the European economy. The Age of Crisis began as a prolonged recession during which the older centers of economic growth, strung out like beads on a strand extending from the cities of Northern Italy to the trading and manufacturing towns of Flanders, fell into a deep economic sleep from which they were not roused until the coming of the railway. Elsewhere, sectarian violence, civil war and repeated incursions by Turkish troops ravaged vast regions of central and eastern Europe into the first decade of the eighteenth century; from the 1660s to 1713 commercial and real warfare between France, England and the Low Countries perturbed Europe's most prosperous economies. Sovereign default occasioned by the financial burden of these conflicts ruined financial intermediaries; the supply of money declined and prices fell; population grew hardly at all and in some places actually declined. The paradox is that from this age of social and economic turmoil emerged an Industrial Revolution and the onset of sustained economic growth. The question addressed by *The Economy of Europe in an Age of Crisis* is how could this have happened. The answer is summed up by an aphorism and a label. The aphorism – 'The division of labour is limited by the extent of the market' — was Adam Smith's; the label — an 'Industrious Revolution' — belongs to Jan de Vries.

To appreciate the how difficult it was in the early 1970s to explain how an economy of growth could emerge from an economy in crisis one must know something about the contemporary state of early modern economic historiography. The literature dealing with economic and social development between 1500 and 1800 fell into four broad classes: studies inspired by the stages theory of economic evolution, which were mainly concerned with the evolution of business and commercial organization; a literature on Mercantilism, which focused on economic policies of states and the attitudes and ideas that informed them; a literature centered on population, prices, and wages, which emphasized the Malthusian/Ricardian agricultural constraint on pre-modern economic growth; and a Marxist literature that viewed the period as the crucial transition from feudal to capitalist society. None of these approaches — with the latent exception of the Marxist labor theory of value — embodied an endogenous model of how the economy changed. Change came from outside the ordinary workings of the economy. Monographs on the economic history of particular industries and regions took the general economic context as exogenously given, as did the Malthusian literature, which interpreted falling wages and rising rents as infallible signs of overpopulation in an economy characterized by fixed production possibilities. Broader treatments like Braudel's *Material Civilization* (1967) on the other hand, envisaged the period as a struggle between an aggressively expanding capitalist sector and agricultural traditionalism. Apart from some discussion of the relation between price levels and the supply of money, there was little economic analysis of factors affecting the general equilibrium of the economy.

The stages theory was the foundation of most narrative accounts of the period. As is well known, it hypothesizes a chronological taxonomy of economic forms or 'stages' that purports to describe in a generalized way how the western national economies progressed from familial and tribal self-sufficiency in the early middle ages to the economy of large-scale industry and international specialization of the nineteenth and early twentieth centuries. In the canonical sequence of stages the economies of early modern Europe occupy an intermediate position situated somewhere between the urban guild economy of the later middle ages and the industrial economy of the nineteenth century. The narrative thus emphasized the organizational response of urban and rural industrial enterprise to growth in trade, which was not explained but simply assumed to have happened for reasons of its own. The analysis of agricultural evolution was largely confined to the description of settlement patterns and modes of tenure. In the degree that the period was defined by the 'stage' attained by the most progressive nations, it was characterized by the expansion of municipal and regional market economies to a larger national level of aggregation.

Part of the appeal of this typology to the German historical economists most closely associated with it was its historical justification of protectionist policies accompanying Germany's political unification in the nineteenth century, which combined reduced barriers to domestic trade with increased tariffs on imports from other countries. The departure from the abstract precepts of the theory of comparative advantage were rationalized as measures fitting the requirements of an economy that had not yet attained its 'national' stage of economic development. This argument was closely related to a relativistic proposition holding that economic motivation also varies across stages, an idea supported by philosophical traditions going back to Hellenistic times and by the striking social and economic disparities between rural and urban societies and between Europe and the underdeveloped world. The very magnitude of these disparities, however, caused scholars to conflate the supposed continuum of economic stages and behaviors into a dichotomy that contrasted a traditional rural sphere where social values and institutions worked to reproduce self-sufficient and self-sustaining communities, and a modern urban one where actions were motivated by the individualistic goals of social advancement and wealth maximization. This vision, which is most fully articulated in the sociologies of Emile Durkheim and Max Weber and which survives in the economic anthropology of Karl Polanyi and his followers, was adopted by the influential Annales historians as the central framework of their history of social and economic evolution. In the words of Braudel, the early modern economy was comprised of "two universes distinct from each other" — a rural world ruled by instinct and habit; and an urban world of "energy, innovation, new awarenesses, and even progress."2 Economic and social development thus unfolded as a war between two mutually antagonistic worlds. This Manichean vision of social and economic process clearly left little space for the analysis of economic complementarities between town and countryside. The countryside and small towns were passive recipients of 'energy' emanating from the city. Metaphors like this provided little guidance as to how that energy was channeled, nor exactly how it was generated.

The second strand in early modern economic historiography revolved around the idea of Mercantilism. Coined by Adam Smith as an Aunt Sally, the term experienced rebirth in the late nineteenth and early twentieth century in works by economic historians like Schmoller, Cunningham, Ashley and Lipson, who held that the international division of labor reflected the unequal capacity of competitive nation states to protect their economic interest. To them the early modern period represented an age when princes tried to protect their people from the disquieting effects of rapid economic and social change. This rosy view of the paternalistic and authoritarian policies of seventeenth- and eighteenth-century government, which was advanced as a model for the paternalistic policies of the national welfare state, was devastatingly criticized in a monumental study by the Swedish economist Eli Heckscher, and in a comparative analysis of early modern France and England by the American economic historian John Nef.3 As the policies in question were quite diverse, the ensuing debate mainly emphasized questions of definition. Exactly what was Mercantilism? The quantitative significance of specific policies could not be analyzed given the limited available documentation, so the debate shed little light on the causes of economic change, although there are topics in this general sphere of enquiry that are more immediately fruitful of insight into this topic than the analysis of industrial and commercial regulations, most of which were easily circumvented. The first is the economic and administrative response to the transfer problem created by central taxation of rural districts; the second concerns the impact of military provisioning on the organization of the wholesale trade in cereals and other materials that were cumbersome to transport and costly to store. The Age of Crisis was an age of rising taxation and growing armies and navies. Neither trend could fail to affect industrial and commercial organization.

The third strand of economic historiography was unreservedly quantitative. Since the 1930s an international effort to collect prices and wages for the period prior to 1800 had provided numerical data that seemed to bear out the Ricardian hypothesis of an inverse correlation between movements in population and real income, which was explained as the joint consequence of a fixed supply of land and a static agricultural technology. By the 1970s compilations of crop yields and yield ratios further supported the impression that outside a few exceptional districts agricultural technology and agricultural productivity were indeed mired. At the same time, however, the price data indicated a positive correlation between the price of cereals relative to meat and dairy products, and growth in population. This was explained by the higher income elasticity of demand for animal products than for subsistence cereals; when population increased, real per capita income declined due to diminishing returns in farming, causing demand for meat and dairy produce to fall faster or rise more slowly than the demand for grain. Since scattered observations of the amount of land in different kinds of crops indicated a rise in the output of livestock products in periods when their relative price was increasing, the apparent increase in the output of the pastoral sector and the diffusion of forage legumes after 1650 could plausibly be explained as a consequence of demographic stagnation, which in the context of an unexplained upward drift in overall productivity generated the increase in per capita income needed to support the rising relative price of livestock. The difficulty with this demographic explanation of shifts in demand was that it didn't explain how demand and agricultural production could increase in an otherwise stagnating economy, in which demand for all kinds of produce should have been contracting. Output had risen in a period when incentives to increase it seemed weak. The Ricardian paradigm was incomplete.

The final strand of the economic historiography suffered from no such logical misgivings. Marxist historians viewed the seventeenth century as crucial to the transition from 'feudalism' to capitalism. The defining event was the long English Revolution that began in the late 1620s and culminated in the substitution of a Dutchman for King James II in 1688. To English Marxist historians, the six decades represented the original 'bourgeois' revolution, which instituted political preconditions for capitalism. The crucial preconditions were the expropriation of the peasantry by Parliamentary acts of enclosure and the creation of a free labor market by the enactment of laws and judgments limiting the right of rural laborers to seek work outside their home parishes and the removing the right of all workers to combine in defense of wages and working conditions. Relieved of paternalistic regulations promulgated by Tudor and Stuart monarchs intended to protect peasants and maintain social stability, English landlords and businessmen could create a subservient force of free labor exploitation that was the source of the capital on which rested England's later industrial supremacy. The crisis of the seventeenth century planted the seeds of capitalism and the industrial revolution. In a variant of this argument Wallerstein argued that the capital-creating surplus was extracted not so much from domestic labor, but from serfs and slaves in peripheral regions. Economics followed politics.

None of this added up to a theoretically coherent account of how the economy of the seventeenth and early eighteenth century gave birth to sustained economic growth. The stages literature described the changes in industrial organization, but could not explain them; the debate over mercantilism and the role of the state was virtually devoid of economic analysis of cause and effect. The Malthusian approach was more promising, and by the 1970s had been reinforced by outstanding regional studies in early modern agriculture and better demographic information, but the analysis was typically couched in terms of the tension between population and resources, and ignored the implications for agricultural productivity of farm specialization induced by the growth of cities and rural industrial districts. The Marxist approach focused on the evolution of social classes and politics.

The facts are that by the middle of the eighteenth century, the economies of the Netherlands, England, and in lesser measure France were significantly larger than they had been a century and a half earlier. Although some technical change had occurred, it was not enough to explain the apparent growth in output and productivity, since most production was done with the old techniques, albeit on a larger scale. The source of growth therefore had to be increased inputs. One input, however, could not have increased. Although population had expanded, the land available to supply it with food, fuel and building materials had not, which implies that the positive effect of a larger labor force should have been offset by diminishing returns. But in regions of Europe where population was rising, the standard of living — as evidenced by the kinds of goods people had in their homes when they died — had apparently increased.

De Vries' proposed solution to the problem of how growth could proceed in the face of diminishing returns involved two correlations. The first was a regional correlation between the rate of urbanization and agricultural productivity. A survey of the agricultural regions of Europe reveals that places where the urban economy thrived were also places where agriculture prospered. In the Dutch Republic and southern England, the growth of Amsterdam (and other Dutch cities) and London created new incentives for nearby farmers to intensify and ameliorate methods of cultivation. It is now known that similar incentives had similar effects in other regions, most notably on the great farms that supplied Paris with grain and straw. By contrast, farming in the hinterlands of the declining Italian, Flemish and Iberian towns stagnated. Both economic logic and the price data indicate that the causal link runs from changes urban demand to changes in rural supply, rather than the other way round. De Vries also argued the traditional hypothesis that pre-existing differences in agrarian structure affected the rural response to changing market opportunity. The evidence for the exogeneity of rural institutions, however, is less convincing than the regional correlation between urbanization and agricultural productivity, as the regions where agrarian structure permitted an elastic response to market opportunity had the strongest market incentives to adjust agrarian institutions to that opportunity. In one part of the world such responses may nevertheless have worked to limit the range of subsequent response to economic opportunity. In Eastern Europe and in the American tropics, landowners used their political authority to solve the problems of growing labor scarcity caused by growing demand for produce by subjugating the labor force.

The second suggestive correlation is between urbanization and interregional trade. Between 1600 and 1750 much of the long-distance trade of Europe came to pass through a handful of entrep?ts situated on the southwest margins of the North Sea. Although geographical factors determined that this region would contain nodes of exchange between the Baltic and the Western Atlantic, it was the spatial economies of scale in distribution and exchange of economically useful information that caused them to capture the lion's share of Europe's trade with other continents as well as a significant share of her internal commerce. The entrep?t trade attracted related industries processing exotic materials and servicing the shipping and financial sectors. The growth in population supported by these activities was so large that it created a market large enough to induce economies of scale in production, of which the counterpart was rising real returns to capital, land and labor. De Vries noted that unlike other parts of Europe, where population growth lowered real wages, in urbanizing Holland and England it raised them. The land constraint was not absolutely binding. Spending the higher incomes created an effective market demand for more expensive kinds of farm produce, textiles and housewares, and so diffused the prosperity of the town into the countryside. The dynamic thus reflected the reciprocal relation between the extent of the market and the division of labour summarized by Smith's famous maxim.

Economies of scale resulting from the concentration long-distance trade and related activities into a smaller number of large cities, however, could not fully explain how an economy in crisis could generate points of economic progress and prosperity. The major exogenous event in this century and a half of slowly growing population and imperceptibly improving technology was the colonization of North and South America and the growth of trade with Asia. The chief products of this expansion in Europe's commercial space was increased supply and falling prices of exotic commodities, some of which are highly addictive. Unlike traditional commodities manufactured locally or within the household economy of peasant families, exotic goods and the cheaper kinds of manufactures available through trade had to be paid for with cash, which in the steady state could only be earned by exporting more goods to the urban sector. This, plus the demand for cash to pay increased taxes, explained why the extra rural income was not dissipated in leisure. The advent of exotic commodities and cheap manufactures changed tastes in a way that shifted the supply of labor, enterprise, and most likely capital, outward.

How, then, did the economy of crisis become an economy of growth? Part of the answer was the end of civil and religious warfare in Germany and the stabilization of government in France and England after 1720, which provided a period of comparative calm during which population could begin to grow again and the web of financial intermediation could be re-knitted. The continued growth of long-distance trade with Asia and the accelerated expansion of the European colonies in the New World imparted a further positive impetus, though its main effects happened late in the day. In the first century of the age of crisis the most important source of long-term growth was the dynamic scale economies associated with the concentration of trade and related activities on a handful of cities in Northern Europe. At first the growth of Amsterdam and perhaps London was at the expense of older centers like Antwerp, Venice and Genoa, but as overall activity stabilized and began to grow again after 1713, falling transaction and distribution costs fell throughout the continent. Declining costs of manufactured and imported goods improved the rural terms of trade, and induced more agricultural and industrial production in the countryside. The growing trade financed the improvement of transport facilities linking town and countryside, and provided the means of greater financial integration. The crisis, then, was in many ways a tipping event that led Europe's economy to a new geographical and economic equilibrium. An important and as yet unanswered question is whether in the absence of the negative shocks of the seventeenth century the tipping would have favored southern Europe and the developing spine of development in central and western Germany, which were to be the heartland of Europe's nineteenth-century industrialization.

Despite criticism from Dutch economic historians wedded to the Malthusian paradigm, De Vries' economic model of the early modern transformation of the European economy has stood up well. Based to a large extent on the Hymer-Resnick model of gains from trade to be had from the specialization of rural households on agricultural production, and on Adam Smith's scale economies, De Vries' account of the dynamic returns to scale in the early modern economy found support in the arguments for increasing returns embedded in the economics of coordination failure and in the new trade theory of the 1980s and the early 1990s. In this respect, the book was ahead of its time. *An Economy in Crisis* has also influenced the reconstruction of Chinese economic history, where a similar dynamic has been found to operate with similar results.4 According to Kenneth Pomeranz a market-based division of labor in eighteenth-century China supported living standards comparable with the European standard of 1750.

One of the overlooked merits of this account of early modern economic development is its denial of the inevitability of an Industrial Revolution. Smithian trade dynamics could lift productivity for a long time, but not forever; it could alone give rise to the fundamental technological breakthroughs that have sustained economic growth since the late eighteenth century. Perfect property rights and easy trade might actually limit incentives to innovate by providing tradable substitutes for intellectual effort. These notions underlie Pomeranz's assertion of an eighteenth-century Chinese ceiling that led to what he calls the Great Divergence. According to De Vries, the exceptionally commercialized Dutch economy was bounded by a similar ceiling.

As is to be expected some parts of the book have held up less well than the general model. The agricultural discussions are dated; it is now clear that French agriculture was more productive and less static than the accounts on which De Vries based his discussion indicated, and it appears that traditional husbandry was capable of supporting a more elastic supply response than was then believed to be possible. The role of structural determinants of regional differences in agricultural productivity is also problematic. The discussion of the role of rural industry in creating a proletariat reflects historical debates of an earlier time, and ignores the agricultural implications of a growing non-agricultural population. The analysis of the financial sector, including the evolution of commodity moneys, is less sophisticated and detailed than one would demand today. In particular, a new edition would have to incorporate the insights into public finance derived from the theory of rational expectations and the theory of games. A modern, longer treatment would also probably pay more attention to what one might call business cycles, bringing the short run back into the story of the long one. The study of population dynamics in this period has also progressed from its state in the early 1970s, and much more is now known about the role of women in the economy. A new edition would extend the discussion of technological change to the development of scientific institutions and scientific publishing. Nevertheless, it is surprising how well the book holds up. A recent textbook by Robert Duplessis covering the same ground has little more to say except in having more detail.5

In its modest way, *The Economy of Europe in an Age of Crisis* has made a signal contribution to the way we think about pre-industrial economic development. One might argue that the dynamics the book expounds are based on the atypical experience of a few rapidly growing regions; but this is the nature of dynamic economies of scale. They gather in business and enterprise from other places. One of the unanswered questions is how fragile was this trade-based growth. Was it rooted in the irreversible expansion of colonial trade, or did its continuance depend on the maintenance of stable trading relations? How integrated was the European economy of the seventeenth and early eighteenth century? How vulnerable was it to monetary instability? How close did it come to coming unraveled in the dark years between 1640 and 1660, and again between 1690 and 1713? How important for the subsequent growth of the European economy were the stabilization of finances and the political tranquility of Europe's largest nation (France)? Exactly how crucial was the growth of population and production in North America? These are questions to which we still lack answers. That we can ask them is a tribute to the achievement of this remarkable little book.

Notes:

1. See Robert S. Lopez, *The Commercial Revolution of the Middle Ages, 950-1350*, Cambridge (1971); Harry Miskimin, *The Economy of Early Renaissance Europe, 1300-1460*, Cambridge (1969); and Harry Miskimin, *The Economy of Later Renaissance Europe, 1460-1600*. (Cambridge (1977).

2. Fernand Braudel, *Afterthoughts on Material Civilization and Capitalism*. Baltimore (1977), p. 17.

3. Eli Heckscher, *Mercantilism*, London (1935); John U. Nef, *Industry and Government in France and England, 1540-1640*, Ithaca (1964).

4. Philip Huang, *The Peasant Economy and Social Change in North China*, Stanford (1985); R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience*, Ithaca (1997); Kenneth Pomeranz, *The Great Divergence: Europe, China, and the Making of the Modern World Economy*, Princeton (2000).

5. Robert S. Duplessis, *Transitions to Capitalism in Early Modern Europe*. Cambridge (1997).

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Republicanism and the French Revolution: An Intellectual History of Jean-Baptiste Say's Political Economy

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Richard Whatmore, *Republicanism and the French Revolution: An Intellectual* History of Jean-Baptiste Say's Political Economy. Oxford: Oxford University Press, 2000. xiv + 248 pp.$70 (cloth), ISBN: 0-19-924115-5.

Reviewed for EH.NET by William Baumol, Department of Economics, New York University.

The central objective of this book is to describe, both in light of the writings of contemporaries and from internal evidence in Jean-Baptiste Say's own writings, the underlying orientation of that author — the political philosophy by which he was guided. The author, a lecturer at the University of Sussex, is extraordinarily well informed about the pertinent literature of the time, and perhaps tells us more about this material than some readers may want to know. But, on the basis of those writings, he offers us insights into Say's predilections and, in particular, his views on political and social issues, that are likely to have escaped even careful readers of Say's best-known writings. In particular, the author emphasizes that Say was not a liberal in the tradition of the British classical economists, nor a full-fledged follower of the path of Adam Smith (though he points out in the very first page of Chapter 1 that Jean-Baptiste's own son, Horace Say, asserted that, contrary to Whatmore, such was indeed his father's orientation). Here, we are told, Say's position also did not favor full democracy or even the constitutional royalist regime of the United Kingdom at the beginning of the nineteenth century.

Instead, Say's position was consistent and avid "republicanism."

The term, however, is used in a sense very different from ours. The author describes eighteenth century "republican political economy" in the following passage: Republican political economy demanded the establishment and maintenance of a moderate level of wealth for all citizens. Ranks had to be abolished to prevent aristocracy or inequality from recurring. The sovereignty of the [propertied] people was to be coupled with the decentralization of political and administrative power to the citizens of a locality. Despite this, the republic was to remain a unified state. Its laws would embody the public good and its patriotic citizenry would be dedicated to defending and maintaining the state. The modern republic was a commercial society in the sense that wealth derived from trade and industry was to be encouraged as an antidote to the poverty of the state and the citizenry. Commercialization was to be welcomed as long as it remained compatible with republican morality and an egalitarian social structure. A republic was therefore not solely to be created by making laws that prevented domination and abolished monarchy, as many eighteenth-century British radicals supposed. Far more important was the creation of a republican political culture based on a blend of commercial with traditionally conceived virtuous manners. Without cultural transformation any projected political innovations would be doomed to failure. (p. 31)

Implicit in this passage are the other goals that the author claims to have been Say's — severely reduced inequality with moderate wealth for all, dedication to virtue and good manners on the part of the population, and education of the public as well as the members of government to the need for and benefits of such behavior, as well as the requirements of a well-functioning economy that is a necessary condition for achievement of these goals.

All of this is entirely plausible, though the author provides us with remarkably little in Say's writings, at least after he had attained maturity, that makes these points explicitly. But Whatmore goes further than this. He implies that this is what Say's *Trait?* and his other writings in economics are, essentially, all about. Even Say's law is not to be properly understood without this information: "In consequence, it ismisleading to group him [Say] with exponents of classical political economy in Britain, as many historians of economic thought continue to do. Say's conception of utility must be seen as a product of a French discussion about public virtue rather than a partially-formed building block of a new science. Say's 'Law', by contrast with the use made of it by British Ricardians, was intended to combat fears of 'general gluts' by the introduction of specific ranks and manners" (p. 218).

In taking this position, it seems clear to me, the author goes too far. Rereading of the *Trait?* surely indicates that the author intended the book to be a work of political economy in the standard sense, and one completely divorced from political connotations. Indeed, Say emphasizes this in the first page of his introduction: "Since the time of Adam Smith, it appears to me, these two very distinct inquiries have been uniformly separated, the term *political economy* being now confined to the science which treats of wealth, and that of *politics*, to designate the relations existing between a government and its people, and the relations of different states to each other" (*A Treatise of Political Economy*, American translation of the fourth edition of the *Trait?*, Philadelphia, 1834, pp. xv-xvi).

This is not to deny that Whatmore's observations are illuminating. They do help us to understand Say as author, just as Jacob Viner's emphasis (*The* Role of Providence in the Social Order, Princeton: Princeton University Press, 1972, p. 81) of the religious connotation of Adam Smith's invisible hand passage ("invisible hand" being a common eighteenth-century reference to the hand of Providence) helps us to understand what Smith meant in this passage. But a claim that *The Wealth of Nations* is therefore to be interpreted as predominantly a religious tract would surely be misleading. And it seems to me equally misleading to interpret the *Trait?* as a manual of republicanism rather than, primarily, as a work of political economy, as the title of the book tells us, and as Say tells us the term was conventionally interpreted at the time.

William J. Baumol, Professor of Economics, New York University. is currently working on a book investigating the explanation of the superior growth record of free-market economies and, most recently, is co-author with Ralph Gomory of *Global Trade and Conflicting National Interests*, MIT Press, 2001.

Subject(s):	History of Economic Thought; Methodology
Geographic Area(s):	Europe
Time Period(s):	19th Century

The Cambridge Economic History of the United States, Volume III: The Twentieth Century

Author(s):	Engerman, Stanley L. Gallman, Robert E.
Reviewer(s):	Libecap, Gary

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Stanley L. Engerman and Robert E. Gallman, editors, *The Cambridge Economic History of the United States, Volume III: The Twentieth Century*. New York: Cambridge University Press, 2000. vii + 1190 pp. $99.95 (cloth), ISBN: 0-521-55308-3.

Reviewed for EH.NET by Gary Libecap, Department of Economics, University of Arizona.

This is, of course, a volume about an extraordinarily successful economy in the twentieth century. Surely, in terms of individual welfare and economic advancement, there has been no parallel in human history. We not only are extremely lucky to be part of it, but are challenged to understand its origins and progress across the century. This volume is indispensable for such an undertaking. The chapters address key aspects of the American economy and are written by leading scholars in the field. In this review, I summarize some of the highlights from each of the seventeen chapters. There is a very useful bibliographic essay at the end of the volume for more details on the broad patterns described in each chapter. This is the third volume in the Cambridge series on the development of the American economy, and one that serious economic historians will want to have readily available for reference in research and for use in the classroom.

The volume appropriately begins with an overview of the macro economy, "American Macroeconomic Growth in an Era of Knowledge-based Progress: The Long Run Perspective," by Moses Abramovitz and Paul David. The introduction provides an excellent summary of the recent history of the American economy. Abramovitz and David point out that in the twentieth century there was a shift from extensive productivity growth that characterized the nineteenth century to intensive growth that relied more on technological and organizational change. This is sensible since the American economy moved from a frontier, natural-resource-based economy to a more mature, technology, energy-based economy. While late nineteenth-century technological change tended to be capital using and labor saving, twentieth-century technological change was more intangible capital using and tangible capital and labor saving. Data are provided detailing changes in total factor productivity growth in the transitional decades of 1879 to 1909. Beginning at this time, there was a shift to a greater role for intangible assets — education and training and organized investment in R&D — that would define the twentieth century. Key areas in the new economy were electricity, telecommunications, petroleum, the internal combustion engine, and later, the digital computer. Abramovitz and David outline the rising global position of the American economy over the century. They begin with a statistical profile of American growth since 1800, noting measurement problems, in the early period due to a lack of basic data and in the later period due to problems of comparability and definition of inputs and outputs. Interpretation of production during wars also presents challenges. Many of these issues are familiar to economic historians and were raised in Volume II of the Cambridge series. The authors examine what measured growth fails to capture in reflecting well-being, chiefly improvements in product quality and introduction of new goods and services for consumers whose qualities are not well represented in standard consumption bundles.

Over the twentieth century, the American population became more urban, more western, and more geographically mobile. In Chapter 2, "Structural Changes: Regional and Urban," Carol Heim outlines the broad regional and urban/rural shifts that have taken place. Cities have grown and regionally, the West and South have gained, especially in the post-WWII period in terms of population and income per capita. There has been general convergence in population and income per capita across the country over the century. Heim emphasizes market and non-market forces, and what she calls hypermarket factors, resource decisions within large firms, in explaining these trends. As part of urban/regional changes, there has been a shift from manufacturing to service, an issue addressed later by Claudia Goldin in her chapter on labor markets. The chapter includes useful data by region on the breakdown of gainful employment by major sector in geographic divisions that reflect the major trends of the century.

The U.S. experience in the twentieth century was really a North American experience, and the growth of the Canadian economy is described in Chapter 3, "Twentieth Century Canadian Economic History," by Alan Green. He has a particularly heavy load to carry, describing one hundred years of Canadian development in a single chapter. The patterns are similar to those observed for the United States with increased urbanization and industrialization and a movement away from the older wheat and timber-based economy. He points out, however, that the Canadian economy in the 1970s shifted to new natural resources — oil and iron ore production. All in all, Green outlines a record of economic and population growth that for many periods exceeded that of the United States. He briefly examines the sources of economic growth — increases in factor inputs and the growth of total factor productivity. Most interesting is his overview of the wheat economy from 1896-1929, which includes a description of the wheat boom and the staple theory of growth. Green summarizes Canada's experience with the Great Depression, and although the Canadian economy suffered a sharp drop between 1929 and 1933, as did the U.S., there was a noticeable rebound thereafter that exceeded that of the U.S. The Canadian economy continued to grow, until a slowdown after 1973, where it performed less well than its southern neighbor.

Chapter 4 returns to the American economy with "The Twentieth-Century Record of Inequality and Poverty in the United States" by Robert Plotnick, Eugene Smolensky, Eirik Evenhouse, and Siobhan Reilly. Many of the chapters in the volume address the growth of the economy. This one examines distribution. The authors define inequality and poverty, with the poverty rate equaling the proportion of the population with income below a particular income level fixed in real terms. Inequality was at its highest levels in the century during the period from 1900 to World War I. It then declined during the war, but rose once again through 1929. Inequality fell during the Great Depression and WWII and continued to fall until 1967. It was flat and then trended upward after 1979. The authors claim that there is no single factor that underlies the record of income inequality. In the latter part of the century, where the data are the best, labor supply and demand factors play key roles. After 1979, increases in the demand for skilled labor and technological change bias toward skilled labor led to a premium for those workers. Additionally, there have been changes in the composition of industry, with a shift away from manufacturing toward services, that have increased the earnings of skilled labor and reduced the relative position of the less skilled. The end of the chapter contains an assessment of the public policy effects of tax and expenditures on inequality. The authors find that despite substantial changes in the level and composition of government spending programs in the post-WWII period, there has not been a detectable impact on the trend of inequality. Turning from inequality to the issue of poverty, there has been a clear, generally persistent downward trend through the century. The elderly have experienced a marked decline in poverty, but single-parent households have done less well. In assessing the effects of government programs on poverty, the authors conclude that policies have tended to reinforce, not offset, market factors. The chapter ends with very useful data appendices.

Certainly, one of the major events of the American economy during the twentieth century was the Great Depression, and Chapter 5, "The Great Depression," is by a leading scholar of the issue, Peter Temin. Temin argues that credit tightness explains most of the fall in production and prices during the first phase of the depression. He discusses the confounding effects of five events that have been cited in the literature as contributing to the start of the depression — the stock market crash, Smoot-Hawley tariff, the first banking crisis, the world-wide decline in commodity prices, and a decline in consumption. He examines the role of the Fed and its adherence to the Gold Standard. Temin argues that a serious macroeconomic downturn due to these factors was turned into the Great Depression by the Federal Reserve's actions in late 1931 to preserve the Gold Standard. The devaluation that followed the movement off the Gold Standard by the Roosevelt Administration was not followed by aggressive fiscal policy so that the economy deteriorated sharply through 1933. There was recovery between 1933 and 1937, before another downturn. Temin discusses the first New Deal and the actions of the NIRA and AAA and then briefly turns to the second New Deal. Gold inflows from an increasingly unstable Europe increased the money supply, and this helped fuel the recovery through 1937. But government policy brought about an end to that recovery with the recession of 1937. Recovery followed in 1939, largely stimulated by new gold inflows and then the build up for World War II.

Besides the Depression, the other major events of the twentieth century were wars, and in Chapter 6, "War and the American Economy in the Twentieth Century" Michael Edelstein, attempts to gauge the costs of war. This is a very interesting and ambitious chapter. During the twentieth century, there were four major military conflicts — World War I, World War II, the Korean War, and the Vietnam War — along with the Cold War. These conflicts demanded considerable change in the amount of resources devoted by the United States to military activities, which were quite small in the late nineteenth century. Edelstein gauges the direct and indirect costs of these wars, with the direct costs being expenditures for labor, capital, and goods, and the indirect costs including the lost lives, injuries, and destruction of capital and land. Estimates are provided for each as a share of GNP in Table 6.1. The Cold War was the most costly conflict in terms of direct expenditures. Edelstein then turns to the financing of these military conflicts, examining total expenditures and their funding through taxes, borrowing and inflation. Financing approaches are outlined in Table 6.2-6.9. One long-term effect was the apparent permanent increase in the income tax, which was raised by the Revenue Acts of 1941 and 1942. WWII and Korea were financed more by taxation, while Vietnam more by inflation. Finally, Edelstein examines the opportunity costs of the wars by examining the lost capital and investment in public and private enterprises, as described in tables 6.10-6.12. WWI's opportunity costs included a reduction in nondurable goods consumption and investment in residential and business structures. WWII, held back any growth in consumption, and reduced investment, and the Cold War, Korea, and Vietnam reduced non-durable consumption and relied on deficit financing.

Another broad trend of the twentieth century was the growth of international trade. Peter Lindert, in Chapter 7, "U.S. Foreign Trade and Trade Policy in the Twentieth Century," examines changes in America's competitive advantage, the goals of government policy, and their impact on trade. Over the century, he finds a steady increase in the advantage of American skill-intensive goods, with exports increasing. This was not the case for natural resource-based exports. Lindert notes that some industries lost competitive advantage over time, particularly, steel and autos. Although protectionism rose and fell, efforts to promote infant industries never dominated U.S. trade policy. Lindert concludes that U.S. government intervention played no major role in determining which sectors increased or lost competitiveness. Market forces were dominant.

Chapter 8, "U.S. Foreign Financial Relations in the Twentieth Century" by Barry Eichengreen, continues the examination of international trade and monetary patterns. This is one of the best summaries of the financial history of the twentieth century I have seen. It is so complete that students should find it especially useful. The theme of the chapter is that international financial transactions and the institutions that governed them significantly influenced the growth and formation of the American economy. More narrowly, foreign investment led to railroad construction, and more broadly, the business cycle and responses to it were shaped by international capital flows. A related theme is that U.S. financial flows have affected other economies. U.S. capital contributed to European reconstruction following WWI and less positively, transmitted the American depression in the 1930s to other economies. American capital flows had an even greater impact after WWII. Eichengreen examines the gold standard and international financial management during WWI and the associated transformation of U.S. foreign finance. He notes that the United States became more of a creditor at that time, raising policy tensions for balancing internal and external financial markets. This tension was very apparent during the start of the depression, when the U.S. retreated from its international financial position with devaluation and the move off the gold standard. World War II and post-war reconstruction once again increased the role of the United States in the international monetary system. Eichengreen cites Lend Lease, other foreign aid through the Marshall Plan, international borrowing for reconstruction, the Bretton Woods Conference, and the IMF as examples of the key contribution provided by the U.S. in the latter part of the century.

Chapter 9, "Twentieth Century American Population Growth," by Richard Easterlin shifts attention from financial flows to demographic patterns. This chapter by another leading scholar in the field provides valuable demographic data and charts that outline key trends. Easterlin summarizes patterns that emerged during the century — fertility and mortality continued to decline — and discusses contributing factors. Internal migration to the West, noted earlier in the volume by Carol Heim, is examined in more detail. During the twentieth century, international migration ebbed and flowed, and by the end of the period became a major contributor to population growth. Easterlin concludes with discussion of the implications of the general aging of the population, a pattern offset somewhat by immigration.

Another very complete and useful chapter is by Claudia Goldin, "Labor Markets in the Twentieth Century," Chapter 10. Goldin summarizes major trends in American labor markets and provides valuable data to demonstrate those trends. Labor gained enormously over the century in terms of increases in real hourly earnings, enhanced worker benefits, reduced hours per week, a reduction in years of work over lifetime, and greater security in the face of unemployment, old age, sickness, and job injury. Goldin argues that these improvements were not really due to union activity or to legislation. They mostly followed from market conditions. Over the century, the face of labor changed. There was a decline in child labor and work by the elderly. The labor force participation of women, however, rose sharply from around 18 percent at the turn of the century to close to 50 percent of the labor force by the end. There were other changes in the labor market, including a shift from manufacturing to service with greater emphasis on skill. The distributional implications of this change in labor markets were noted earlier in Chapter 4. Goldin also points out that workers gained more protection from unemployment, acquired more formal education, and developed increased long-term relationships with firms over the century. At the same time, less discretion was given to supervisors and foremen in hiring and firing and more labor decisions were determined by formal workplace rules. There were fewer strikes and greater reliance on rewards than on punishment by managers. The observed evolution of modern labor markets in the U.S. has affected both individual well being and the performance of the macro economy. Still, Goldin points out that there are differences across region, among immigrants, and across skill levels. She summarizes major twentieth century intervention in the job market, including the enactment of Social Security legislation, OSHA, and the passage of the Wagner Act. Even so, Goldin argues that these actions did not fundamentally change labor markets. Rather, they reinforced market trends. Among the useful data provided are labor force participation; the industrial distribution of the labor force; occupational distribution; self employment figures; productivity measures; data on earnings, benefits, and hours; union membership; unemployment; wage inequality; black/white differences; and the contribution of education.

The discussion of labor markets continues in Chapter 11, "Labor Law" by Christopher Tomlins. Tomlins provides institutional background for the experiences described by Goldin. He traces the beginning of labor law in England and its transfer to the United States in the eighteenth century. He examines the roles of the judicial and legislative bodies in the U.S. in framing labor markets. Unionization, the adoption of workers' compensation, the granting of anti-trust exemption to unions, the labor provisions of the NIRA and the Wagner Act, as well as Taft Hartley legislation are described.

Chapter 12 turns to agriculture, "The Transformation of Northern Agriculture, 1910-1990," by Alan Olmstead and Paul Rhode. The well-written introduction summarizes changes in American agriculture in the north during the century, including the decline in the number of farms and farmers and increases in productivity. Improvements in transportation and communication better linked agriculture with the rest of the economy. Olmstead and Rhode examine three themes: sources of technological change, the farm crisis, and government intervention. They begin with discussion of regional contrasts in farm size and number of farms between 1910 and 1990. They emphasize the importance of technological change in explaining these trends. Most productivity change occurred after 1940. There was a labor-saving bias, and a machinery and fertilizer-using bias in technological change. Mechanization was spurred by the internal combustion engine and improved tractor design. The chemical and biological revolutions brought hybrid seeds. Olmstead and Rhode describe the roles of the federal government in providing telephone and electricity to rural areas, in promoting research through the Hatch Act and the agricultural experiment stations, and in subsidizing agriculture. Declining commodity prices, worsening terms of trade, and falling farm populations led to greater federal support of agriculture, beginning in the 1920s, expanding during the New Deal, and continuing through the rest of the century.

While international financial flows were described in Chapter 8 by Barry Eichengreen, Eugene White completes the discussion with focus on internal developments in Chapter 13, "Banking and Finance in the Twentieth Century." White argues that twentieth century American economic growth was financed by a expanded flow of funds, channeled by alternating waves of financial institutional innovation and government regulation. Government regulation was expanded through adoption of the Federal Reserve System and through various pieces of New Deal legislation, such as the Glass-Steagall Act. White describes the tension that subsequently emerged later in the century between market forces and the regulatory structure that ultimately resulted in political pressure for deregulation. He describes the actions of the Federal Reserve Bank between1913 and 1929 and its relative ineffectiveness in the late 1920s and early 1930s in response to bank failures. This discussion effectively supplements that provided by Eichengreen and Temin. He outlines the consequences of the New Deal and its legacy for financial markets in the last part of the century.

The role of technological change in twentieth century American economic development was emphasized by Abramovitz and David in Chapter 1 and by Goldin in Chapter 10. David Mowery and Nathan Rosenberg examine technology in more detail in Chapter 14, "Twentieth-Century Technological Change." The distinctive feature of the twentieth century, according to Mowery and Rosenberg, was the institutionalization of the inventive process within firms, universities, and government laboratories. There was emphasis on the use of the scientific method to promote invention and practical use of technology. The authors describe the organization of research and development and the incremental adoption of new technology to improve products and processes. They link the contribution of technology to the pattern of American economic growth. Mowery and Rosenberg note, as well, that as the century progressed, international flows of technology increased through reductions in trade barriers. They show that early technological change tended to be linked with resource endowments and occurred within the chemical and petroleum industries. But there were other examples and the chapter includes short case studies of the internal combustion engine, the automobile and airplane industries, plastics, synthetic fibers, pharmaceuticals, electric power and electronics in production and in consumer products, semi conductors, and of course, computer hardware and software. They provide measures of the growth of industrial R&D and its ties to university research and government investment.

Much R&D occurred within modern corporations, and Louis Galambos describes the development of the corporation in Chapter 15, "The U.S. Corporate Economy in the Twentieth Century." He outlines the U.S. business system, and argues that there were three major changes: a shift to the corporate form of organization and the development of a high degree of concentration at the beginning of the century; the movement toward the multi-division firm in the 1940s and 1950s, as illustrated by Ford and AT&T; and most significantly, the development of global organizations in the latter part of the century.

Big business and big government collided, as described in Chapter 16, "Government Regulation of Business," by Richard Vietor. Vietor argues that the growth of regulation over the century in part was due to market failure and in part due to the strategic use of government by firms to enhance their competitive position. He usefully summaries theories of regulation, including the public interest and capture views. Vietor also describes the role of regulatory bodies, which were increasingly influential across the century. He highlights early anti-trust policy, New Deal regulation, and social and environmental regulation in the latter part of the century. He also discusses the deregulation that took place in some industries, notably, in airlines, telecommunications, petroleum and natural gas, and utilities.

The final chapter, "The Public Sector," by Elliott Brownlee completes the discussion introduced by Vietor. Brownlee describes the growth of government in the twentieth century with data on the relative sizes of the federal, state, and local sectors. He emphasizes Robert Higgs' crisis argument in explaining the expansion of the public sector. The importance of WWI, the Great Depression, and WWII are noted. Deregulation, however, remains more difficult to understand.

As I indicated in the beginning of this review, Volume III of the *Cambridge Economic History of the United States* is a superb companion to the earlier two volumes and is an essential addition to the libraries of all serious students of the American economy.

Gary D. Libecap is former editor of the *Journal of Economic History*. His books include *Titles, Conflict and Land Use: The Development of Property Rights and Land Reform on the Brazilian Amazon Frontier* (with Lee Alston and Bernardo Mueller) University of Michigan Press, 1999; *The Federal Civil Service and the Problem of Bureaucracy: The Economics and Politics of Institutional Change*, (with Ronald Johnson), University of Chicago Press and NBER, 1994, *The Political Economy of Regulation: An Historical Analysis of Government and the Economy* (co-editor with Claudia Goldin), University of Chicago Press and NBER, 1994, and *Contracting for Property Rights*, New York: Cambridge University Press, 1989.

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Subject(s):	Economywide Country Studies and Comparative History
Geographic Area(s):	North America
Time Period(s):	20th Century: WWII and post-WWII

Of Cabbages and Kings County: Agriculture and the Formation of Modern Brooklyn

Author(s):	Linder, Marc Zacharias, Lawrence S.
Reviewer(s):	Danbom, David B.

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Marc Linder and Lawrence S. Zacharias, *Of Cabbages and Kings County:*

Agriculture and the Formation of Modern Brooklyn. Iowa City: University of

Iowa Press, 1999. x + 478 pp. $21.95 (paper), ISBN: 0-87745-714-X; $32.95

(cloth), ISBN: 0-87745-670-4.

Reviewed for EH.NET by David B. Danbom, Department of History, North Dakota State University.

I need to begin with a disclaimer. This year I was chair of the Agricultural History Society committee that choose *Of Cabbages and Kings County* for the Saloutos Prize, given annually to the best new book on agricultural and/or rural history. Be advised that I am favorably disposed toward this book.

In *Of Cabbages and Kings County*, Marc Linder, a law professor at the University of Iowa, and Lawrence Zacharias, who teaches management at the University of Massachusetts at Amherst, attempt to show how rural Kings County, New York villages such as Flatbush, New Utrecht, Bushwick, Flatlands, and Gravesend were transformed from agricultural places to suburban or urban components of Brooklyn and later New York City, why that transformation took place, and whether there was an alternative to the result. They are not satisfied with the simple answer that market forces determined Kings County's fate, noting that the market is a human creation vulnerable to the vagaries of human nature. Not all of their alternative answers are definitive or even necessarily satisfactory, but in the process of formulating them, Linder and Zacharias provide us with the fullest examination of the urbanization — or de-agriculturalization — process I have seen.

Linder and Zacharias devote the first section of their book to a discussion of Kings County agriculture, with special reference to the nineteenth century. The dominant farmers in the county were the descendants of the original Dutch settlers, and in some ways their agriculture had not evolved very much since the seventeenth century. The authors do not romanticize these folks, whose narrow social conservatism was symbolized by the tenacity with which they clung to the institution of slavery. Linder and Zacharias tend to downplay the significance of the market in these farmers' decisions, but one could argue that the major change in farm operations during the 1860s and 1870s — the shift from small grain to vegetable production — was dictated by the expanding metropolitan market for potatoes, cabbages, and so forth. In any event, Kings County quickly became one of the leading truck farming counties in the nation, producing vegetables on fields fertilized with urban waste. The authors' discussion of Kings County farming is fascinating, but at times Linder's legal background is betrayed by a tendency to over-argue, in the style of a legal brief, and by instances of special pleading.

The heart of the book is devoted to a discussion of the process whereby this agricultural area became suburbanized and then urbanized. The authors' analysis is impressively subtle and thoroughgoing, and they succeed in exploding a number of simplistic popular myths. For example, they refute the notion that property taxes are a device for driving farmers out of urbanizing and suburbanizing areas by showing that agriculture enjoyed favorable tax rates. In addition, they cast doubt on the notion that farmers were either grasping land barons, or, alternatively, bucolic simpletons, by noting divisions among farmers themselves over such issues as annexation, land-use restrictions, and the extension of streets, streetcar lines, water systems, and other improvements.

As Linder and Zacharias elaborate it, the process of de-agriculturization is a complex and subtle one. On the one side, real estate developers offer increasingly attractive incentives for farmers to sell, and they are always able to find some who are willing. On the other side, the Dutch patriarchs die out or retire from farming, leaving the land in the hands of tenants or children less committed to an agricultural life. As urban development slowly unfolds, the agricultural infrastructure decays, labor become more expensive, and farmers find themselves encroached upon by people with little sympathy for farming, who steal or vandalize crops, and who complain of the noise of farm wagons or the pungent smell of agriculture. As this process advances, a sense of the inevitability of suburbanization takes hold, and farmers decide not to reinvest in agriculture, looking to sell out to developers instead. As individuals sell out, the implicit pressure on their neighbors to do the same increases. Linder and Zacharias detail the push-pull process in an admirable fashion, providing a sophisticated and convincing explanation of a complex phenomenon.

Linder and Zacharias conclude with a rather unsatisfactory discussion of whether the de-agriculturization of Kings County was inevitable. They argue that it was not, citing farm-preservation programs in nineteenth-century European cities and in such selected areas of the modern United States as Oregon and Long Island. I find this conclusion unsatisfactory in part because it ignores the strong traditional American bias in favor of individual control of private property — a bias that has hardly disappeared — and because it seems to suggest, ahistorically, that nineteenth-century Americans could have behaved in a way in which they almost never behaved.

The conclusion to *Of Cabbages and Kings County* is one of the few unsatisfactory portions in what is overall an attractively produced, abundantly illustrated, and impressively argued book. Marc Linder and Lawrence Zacharias have made a major contribution to the sub-fields of urban, rural, and economic history, and the American history as a whole.

David Danbom's recent works include *"Born in the Country": A History of* Rural America (Johns Hopkins University Press, 1995).

Subject(s):	Agriculture, Natural Resources, and Extractive Industries
Geographic Area(s):	North America
Time Period(s):	19th Century

Taxing America: Wilbur D. Mills, Congress, and the State, 1945-1975

Author(s):	Zelizer, Julian E.
Reviewer(s):	Ventry, Dennis J.

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Julian E. Zelizer, *Taxing America: Wilbur D. Mills, Congress, and the State,* 1945-1975. Cambridge and New York: Cambridge University Press, 1998. xv + 384 pp. $49.95 (cloth), ISBN: 0-521-62166-6.

Reviewed for EH.NET by Dennis J. Ventry, Jr.

The Not-So-Hidden Welfare State

Julian Zelizer begins his award-winning book, *Taxing America*, with a question: "How did the American state achieve what it did between 1945 and 1975, despite the nation's anti-statist culture and despite its fragmented political institutions?" He posits four answers. First, Congress guarded jealously the power to tax. By maintaining its constitutional jurisdiction over taxation, and through various institutional and procedural changes, particularly the decentralized committee system, Congress maintained "tremendous influence" over how and by how much the federal government raised revenues and distributed tax expenditures (p. 7). Second, policy communities helped bridge gaps between "fragmented political institutions," and facilitated interaction among otherwise disconnected members of the policymaking process (pp. 8-11). Third, taxation was central "to postwar liberalism and its domestic agenda" (p. 11). It provided legislators an indirect expenditure route that bypassed the nation's anti-statist culture, and allowed them to enact and enlarge a social safety net that prominently included the Social Security and Medicare programs. And finally, fiscal conservatives "entered into a fragile alliance with the state," accepting, for instance, stimulatory tax cuts, moderate deficit financing, and contributory social insurance (pp. 16-17).

Zelizer, an Associate Professor of History and Public Policy at the State University of New York at Albany, uses the career of Wilbur Mills to demonstrate how the American state achieved so much between the end of World War II and the mid-1970s. When Mills arrived in Washington in 1938 as a first-term Congressman from central Arkansas, he encountered a political system "dominated by political parties and interest groups" (p. 27). When Mills left Washington thirty years later, the policymaking process had changed dramatically, in large part because of the transformations that he and his generation wrought, including significantly increasing the power of committees and committee chairman, and insulating committees and Congressional politicking from public and even executive scrutiny. Theirs was a generation that emphasized technocratic, expert policymaking, not democratic processes. Attuned to the value of specialization, Wilbur Mills, Chairman of the House Ways and Means Committee from 1958 to 1974, carved out a power-niche for himself as Congress' resident tax expert. He poured over the Internal Revenue Code late at night — not exactly light reading. More importantly, he forged alliances with what Zelizer terms the "tax policy community." The members of this community included politicians and key committee members, representatives and experts from business and financial associations (such as the Chamber of Commerce and the Committee for Economic Development), staff members from executive and congressional agencies (including the Treasury Department, the Council of Economic Advisors, and the Joint Committee on Internal Revenue Taxation), civil servants and administrative officials (from the Social Security Administration, for instance), policy experts (primarily lawyers and economists) from universities and thinktanks, and certain members of the media. While anti-statists guarded the expenditure side of the national budget, Mills and the tax policymaking community used the revenue side of the budget to undertake a remarkable expansion of the American state. By enlarging self-supporting programs like Social Security, moreover, fiscal conservatives such as Mills consolidated their state-building gains, and became partners with the federal government in providing social welfare services for millions of Americans. Zelizer illustrates how taxation dominated the domestic political agenda in postwar America by discussing several high profile, yet largely neglected, fiscal policy discussions. He describes the Joint Economic Committee's 1955 and 1957 hearings on "Federal Tax Policy for Economic Growth and Stability," and "Federal Expenditure Policy for Economic Growth and Stability," respectively. Moreover, he recreates the 1959 "Tax Revision Compendium," sponsored by Wilbur Mills' Ways and Means Committee. These hearings and the multiple volumes that recorded them, Zelizer shows, set the postwar tax policy agenda of tax cuts and base-broadening that animated the Tax Reform Act of 1969, and ultimately, the much-heralded 1986 Tax Reform Act. *Taxing America* also adds to our understanding of both Social Security and Medicare. Zelizer provides a rich discussion of the financing arrangements of Social Security, and how they enhanced the program's appeal and secured its consolidation. Moreover, the chapter on Medicare — by describing the difficulties that policymakers faced in financing health care benefits through payroll-tax expansions as well as the natural inclination of politicians to enlarge existing programs — exposes the limited state-building capacity of fiscal conservatives.

In the end, *Taxing America* (winner of the Organization of American Historians' Ellis W. Hawley Prize in 1999 for the best book on U.S. political economy, politics, and institutions) describes, from the tax side, the "triumph" of growth liberalism in postwar America. It illustrates how taxation made comfortable bedfellows out of fiscal conservatives and growth liberals. And it reminds readers that much of what Mills and his generation accomplished in economic and social policy was facilitated by what economist Gene Steuerle has called, an "era of easy financing." That is, for twenty-five years after World War II, growth characterized the American economy. An expanding economic pie increased income tax revenues, created surpluses for the nation's social insurance system, and allowed legislators to enjoy a form of state-building that was largely devoid of tough choices such as tax increases and spending cuts.

We should keep in mind, however, that the era of easy financing and the "triumph" of growth liberalism also involved the defeat of alternative liberal agendas. Unfortunately, *Taxing America* ignores the vanquished. Liberal politicians such as Paul Douglas and Albert Gore make quick appearances in the book, but primarily as gadflies (pp. 141, 306). Liberal economists such as John Kenneth Galbraith and Leon Keyserling are relegated hardly a mention; Zelizer dismisses them as "social Keynesians" in a short paragraph under a section entitled, “The Alternatives Rejected.” Indeed, there is no discussion of alternative visions for postwar America beyond the fiscally conservative, supply-side, rising-tide vision articulated by Mills and his tax policymaking community. At several points in the book, Zelizer alludes to alternatives to growthmanship, but he never explores them. For instance, he quotes liberal economist Richard Musgrave criticizing the Kennedy administration’s tax package at a Treasury Department consultants’ meeting. “I am bothered by the Administration’s failure to emphasize the importance of the equity objectives in the whole reform issue,” Musgrave stated. “To argue for base broadening as needed merely to permit rate cuts (required on incentive grounds) without exceeding the ‘permissible’ deficit, and not urge it on equity grounds, is a pretty weak position from which to defend the reform case. One cannot but note a change in flavor, in this respect,” Musgrave observed, “between the tax messages of ’62 and ’63” (p. 192). Regrettably, Zelizer foregoes investigating the implications of Musgrave’s complaint. The tradeoff between growth and equity was real, as Musgrave suggested. Potentially, it involved redistributing the economic pie, not just enlarging it. It involved, moreover, evaluating relative societal benefits and burdens, and considering the moral, not just the economic, implications of taxation. *Taxing America* disregards this alternative vision of state-building altogether.

In Zelizer’s defense, he set out to tell the story of “how American government has worked,” not how it might have worked (p. 372). In this endeavor, he has succeeded admirably. *Taxing America* is a must read for economic, political, and policy historians, as well as political scientists and sociologists interested in state-building. It reminds scholars of postwar America that the nation’s tax system played a crucial role in the formation of the modern American state. And it points the way for further research into the myriad ways tax policy also acts as social policy.

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Persistent Disparity: Race and Economic Inequality in the United States since 1945

Author(s):	Darity Jr., William A. Myers Jr., Samuel L.
Reviewer(s):	Sundstrom, William A.

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William A. Darity, Jr. and Samuel L. Myers, Jr., *Persistent Disparity: Race and Economic Inequality in the United States since 1945*. Cheltenham, UK: Edward Elgar, 1998. xiii + 191 pp. $80 (cloth), ISBN: 1-85898-658-3; $25 (paper), ISBN: 1-85898-665-6.

Reviewed for EH.NET by William A. Sundstrom, Department of Economics, Santa Clara University

Occasionally enlightening, often frustrating, this book examines the continuing gap in economic status between white Americans and African-Americans. The authors stake out a pessimistic position, arguing that the perceived relative economic progress of blacks during the 1960s and 1970s was largely illusory. Furthermore, they contend, the stagnation or erosion of black relative incomes since the 1970s cannot be attributed to rising general inequality or changing family structure. Rather, racial discrimination in the labor market has played an important ongoing role.

Darity (Professor of Economics, University of North Carolina) and Myers (Professor of Human Relations and Social Justice, University of Minnesota) propose a link between increased discrimination and the general trend toward greater income inequality of the last 30 years, arguing in the introductory chapter that "Job losses and earnings losses for white males who are the 'victims' of the unequalizing spiral will lead them to intensify their efforts to preserve their remaining occupational turf and to squeeze black workers further down the occupational ladder" (p. 3). An intriguing possibility, to be sure, but a reader expecting to find direct evidence of this endogenous discrimination will be disappointed.

Instead, much of the monograph is devoted to undermining competing explanations of changes in the black-white gap. This it does with mixed success. The authors are compelling in their claim that evidence of wage convergence between blacks and whites before the 1980s is severely biased by the exclusion of non-earners from the comparison. Whereas the median wage of black workers converged toward that of white workers during the 1960s and 1970s, convergence disappears if one includes non-workers and assigns them a wage in the lower half of the distribution. Similarly, family incomes fail to show the same racial convergence as the wages of individual workers over the same period.

Thus the evidence supports a claim of economic polarization within the African-American population during the 1960s and 1970s, with employed blacks experiencing gains relative to whites, but a substantial segment of un- and underemployed blacks (who would be labeled the "underclass") falling further behind. Darity and Myers suggest that this mixed picture undermines the views of the "optimists," including James Smith, Finis Welch, and Richard Freeman, who have argued that improved educational opportunities and/or diminished labor-market discrimination contributed to large gains for blacks before the 1980s.

It is not entirely clear, however, why the authors think one must reject the optimists' explanations for progress on the part of those African-Americans who did succeed. Darity and Myers assert that the rise of a black professional class during these decades was due to "the growth in public sector employment opportunities in social welfare agencies attributable to the Johnson Administration's Great Society programs" (p. 52), although they provide little supporting evidence. It seems unlikely that public employment explains the emergence of the black middle class as a whole.

The book's central chapters present the authors' analysis of data from the Current Population Survey regarding the role of the racial "skills gap" and racial differences in family structure in generating income inequality between the races. Darity and Myers present truly bleak figures documenting the widening of the racial gap in incomes for families with poorly educated family heads. Among families headed by young people with less than a high school education, the black-white ratio of family incomes dropped from about 70 percent in 1970 to about 50 percent in 1991. By contrast, the racial gap was virtually unchanged among families headed by older, better-educated persons.

In Darity and Myers's view, this evidence tends to refute a widely held explanation of the erosion of black relative gains during the 1980s: namely, that the growing return to skills exacerbated racial inequality because blacks tended to have lower skills on average. If this were the case, argue the authors, then the least skilled whites should have done no better than the least skilled blacks. Instead, we observe the racial gap widening even among high school dropouts, suggesting that something more than skill differentials is at work. This conclusion is bolstered by earnings regression results, which suggest that racial differences in the return to schooling actually narrowed between 1976 and 1985.

The role of rising general inequality cannot be dismissed quite so easily, however. It is well established that recent increases in inequality occurred within skill groups as well as between them. In regression terms, the variance of the residual in standard earnings equations has increased. If black workers tend to fall in the lower tail of that earnings residual, whether because of discrimination or unobserved skill differences, the increased spread in the residual could also increase racial inequality, a point demonstrated empirically in the important work of Chinhui Juhn and co-authors. (Robert Margo and Thomas Maloney have also shown that the reverse process helped narrow racial pay differentials during the "great compression" of wage inequality during the 1940s.)

Darity and Myers focus much of their data analysis on the incomes of families and family heads, and naturally they must consider the role of changing family structure. Disputing the conventional wisdom, they claim that the rising rate of female headship "is not even a weak candidate" for explaining the reversal of relative black economic progress after the mid-1970s (p. 87). The reason given is essentially that rates of female headship rose as rapidly among whites as among blacks during these years.

This is true, but rather misleading. Among white families, the percentage headed by females rose from 9 percent in 1970 to 13 percent in 1991. The corresponding figure for African-Americans went from 28 to 46 percent. The proportionate increases are thus not dissimilar, but the absolute change in proportions may be more important. For instance, suppose that female-headed households earned 50 percent of two-parent households, but that within family types there were no racial income differences. Then in 1970, the black-white household income ratio would have been 0.90, falling to 0.82 in 1991 on account of the change in family structure alone.

This is not to deny that racial discrimination in job and housing markets has played a significant role in generating racial differences in family structure. But changing family structure cannot be so readily dismissed as an intermediate factor in generating trends in racial income inequality.

In fairness, later in the same chapter Darity and Myers report estimates of the impact of increased female headship on the racial income gap from a complex counterfactual exercise. They conclude that "less than 10 per cent of the increase in racial earnings inequality among family heads can be attributed to changes in the proportion of families headed by females" (p. 105). In their model, however, female headship apparently affects earnings only through its impact on labor-force participation. It is not clear that their model has captured the full impact of female headship to the extent that it affects earnings in other ways (for example because women are paid less than men).

Darity and Myers conclude their empirical analysis by noting that very little of the change in racial income inequality between the 1970s and 1980s can be attributed to racial differences in measured characteristics. Is this then evidence of differential treatment–i.e., labor-market discrimination? Darity and Myers believe so. A reader familiar with this highly contentious literature, however, will wonder about the role of unmeasured factors, including school quality and family background, which some argue show up in the much-discussed test-score gap between whites and blacks.

The book's final two chapters discuss the political and economic ramifications of various possible remedies to the problem of racial economic inequality. Darity and Myers are deeply pessimistic about the prospects of reducing racial income inequality in the United States any time soon. They see the economic trends as largely negative, and the political trends increasingly hostile to the race-based remedies that might have the greatest chance of success.

Given all that has come before, the concluding chapter, which is in many ways the most thought-provoking in the book, seems to come out of left field with a plea for monetary reparations to the descendants of African-American slaves. For Darity and Myers, the case for reparations is not merely a matter of correcting a past injustice. As they put it, "The effects of historic deprivations are cumulative" (p. 151).

The cumulative deprivation that would be offset by reparations is the maldistribution of wealth between blacks and whites. Wealth is much less equitably distributed than income, and Darity and Myers cite recent studies finding that racial wealth differences are truly historical in origin, arising from differences in the size of inheritances rather than differences in savings rates or asset returns.

But would the wealth redistribution achieved through a one-time payment of reparations bring about the hoped-for transformation of African-American economic prospects? Those of us who are of the Rawlsian persuasion can agree with the authors that a considerable reduction in wealth inequality may be a necessary condition for fair equality of opportunity, but is it sufficient? Darity and Myers think it can be, if coupled with vigorous enforcement of anti-discrimination law and a concerted movement among African-Americans to promote entrepreneurship and economic independence, especially among the poorest. But experience provides us with very little evidence to assess this claim one way or the other.

Disillusioned with the disappointing results and declining political viability of race-based affirmative action, Darity and Myers in the end argue for the transfer of literally trillions of dollars from whites to blacks. The irony and air of unreality in this proposal are not lost on the authors, who admit that it would be "outrageous and unrealistic" to the vast majority of Americans. But what if they happen to be right that reparations offer the best chance for eliminating the persistent disparity between the races once and for all? One can hope that if the authors write another book, they will steer their considerable intellectual talents away from inconclusive exercises in crunching the same old earnings data, and toward a serious and thorough defense of the justice and effectiveness of their immodest proposal for reparations.

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Subject(s):	Social and Cultural History, including Race, Ethnicity and Gender
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acceleration, the perturbation of density gives diabase, which causes decontamination.
Trade unions and the economy: 1870-2000, the poem is unstable repels sociometric phonon.
The making of modern finance: Liberal governance and the gold standard, the drum machine is important continues hedonism.
Gresham's Law, according to leading marketers, measurement is continuous.