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Comments

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The Limits of Free Trade

Protectionism has long enjoyed populist appeal, but has not achieved intellectual respectability. Quite simply, it lacked a leading economist guru to champion its cause. Therefore, protectionists the world over must be jumping for joy at having found an illustrious one, namely Paul Samuelson. Gurus and champions don't come any bigger than that.

But what has Professor Samuelson actually proved in his recent article, "Where Ricardo and Mill Rebut and Confirm Arguments of Mainstream Economist Supporting Globalization" (Summer, 2004, pp. 135–146)? He compared two situations: 1) The United States has a free trade policy, and China has low productivity in the sectors where the United States is an exporter. 2) The United States has a free trade policy, and China has high productivity in these same sectors. The U.S. terms of trade are worse, and the aggregate U.S. economic welfare is lower, in situation 2 than in situation 1. (Professor Samuelson focuses on a special numerical example where the Chinese productivity rises exactly to the point where U.S. exports are reduced to zero, but that is irrelevant for the argument. It is just a kind of worst-case scenario.)

Have U.S. terms of trade deteriorated in recent years? From the beginning of 1990 through the end of 2003, the U.S. Bureau of Labor Statistics index of import prices rose by 5.4 percent, while export prices rose by 5.9 percent. If we exclude petroleum products (presumably not the point of Professor Samuelson's arguments), import prices rose by only 1.7 percent. Thus, U.S. terms of trade have been steady, or perhaps even improving, since 1990.

Even if we ignore the empirical counterevidence, Professor Samuelson's theoretical proposition remains valid as a logical possibility. But what are its policy implications? Alas, none. Neither trade policy, nor any other available policy, will take us back to the desirable situation 1. The only action that can restore 1 is to "bomb China back into the stone age" of their older lower productivity. We sincerely hope Professor Samuelson is not proposing that. The only practical policy question facing the United States now is this: Given that we are now in situation 2, do we go on trading freely or not? Should we switch to the protectionist alternative 3, namely, Fortress America?

If our objective is to maximize aggregate U.S. economic welfare, the answer is unequivocally no. All the standard arguments apply to this comparison and say that 2 is better for aggregate U.S. economic welfare than 3. Only in Professor Samuelson's worst-case scenario is there no difference between 2 and 3, because if even with a free trade policy the comparative advantage configuration is such that our trade is exactly zero, then there is no gain from trade. However, there is no loss from keeping our trade free either, so situations 2 and 3 are equivalent in that case. In all other cases, 2 is positively superior to 3 from the aggregate U.S. perspective. Therefore protectionists should take no comfort from Professor Samuelson's intervention on their behalf; they must still go on looking for their guru.

Of course aggregate economic gains are not enough to establish either the ethical desirability

or political practicality of the policy of free trade on its own, unless mechanisms whereby winners actually compensate losers are in place. Therefore the analysis recommends a policy package—keep trade free, but compensate the losers it creates. We believe that Professor Samuelson and we are in agreement on this.

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Response from Paul A. Samuelson

Leo Tolstoy wrote to his admired younger friend, Anton Chekhov, "Stop writing plays. They are as bad as Shakespeare's."

Apparently, I perpetrated a major indiscretion when explicating an old truth about comparative advantage in the Summer 2004 *Journal of Economic Perspectives.* Outnumbering the letters I received from illiterates in economics incorrectly welcoming me as a new champion of protectionism were the letters from sophisticated friends reproaching me for muddling the case for free trade.

I write briefly for two purposes.

First, the correct arguments by Paul A. Samuelson in the *JEP* do not (repeat, not) persuade me to advocate abandoning free trade policies by advanced industrial countries like the United States, by successfully developing economies like Japan or India, or by still-floundering basket-economy cases in Africa or the Middle East. The reason is simple and nonequivocal: Economic history and best economic theory together persuade me that leaving or compromising free trade policies will most likely reduce future growth in well being in both the advanced and less productive regions of the world. Protectionism breeds monopoly, crony capitalism and sloth. It does not achieve a happy and serene egalitarian society.

My second purpose is to explore what ought to be the credo of a scholar who is at the same time a humanitarian person. In this connection a relevant case study is David Ricardo's famous late-in-life recantation against his earlier belief that an industrial revolution invention of machinery would necessarily increase the well being of everyone—of capitalists, of landowners and of workers. He had even brainwashed his laissez-faire followers into a similar belief. Imagine then the consternation and resentment of a John R. McCulloch when the third and final edition of Ricardo's *Principles* contained a new chapter "On Machinery" purporting to prove that some inventions could reduce the wage demand for labor (and could even reduce the ["Kuznets"] real GDP) (see Ricardo, 1817, 1820, as edited by Sraffa).

Truth-telling brought Ricardo few kudos but much reproach. Even the best modern commentators—Wicksell, Schumpeter, Kaldor, Stigler—thought the old boy had lost his marbles, had abandoned Say's Law and (in modern lingo) had alleged that the Invisible Hand of competition could systematically lead to "non-Pareto optimality" and deadweight loss. David Ricardo was a notoriously poor expositor, and much of his overblown reputation traces to his gratuitous obscurities. Therefore, in two idle moments in the 1980s, Samuelson (1988, 1989) had to vindicate Ricardo as being right about what was a banality: Yes, technical change can either raise or lower the market-clearing real wage. Wicksell never doubted that. But the modern commentators forgot that Ricardo was a Malthusian who believed that a lowered demand for labor would so reduce population as to reduce total GNP and its wage share. No Pareto nonoptimality in that.

To connect up with my decision to publish in the *JEP,* Ricardo was never led by his correct chapter "On Machinery" to become a Luddite who hankered to throw wooden shoes into new real life machines. He pragmatically believed that workers would end up better off after win-win inventions and win-lose inventions both took place. No $2 + 2 = 4$ argument could prove that. But the sage economist must muster best available knowledge about history and theory in giving plausible pragmatic advice.

Still, in this discussion of credo about publication, tolerate some autobiographical palaver. With Wolfgang Stolper as senior author, Samuelson submitted "Protection and Real Wages" to the 1941 *American Economic Review* editor. The manuscript had developed, developed cogently, that Ohlin-Heckscher trade theory could deduce that free trade globalization could permanently lower the U.S. real wage if America's labor/land endowment ratio was below that ratio abroad. As peer reviewer, the late Howard Ellis of Berkeley persuaded the *AER* editor to turn down the Stolper-Samuelson submission. Reason: "It will weaken the case for free trade." So instead of our pearl of wisdom reaching 5,000 captive members

of the AEA, it reached a few hundred avant-garde readers of the new-ish *Review of Economic Studies*. Fifty years later, at Ann Arbor, Michigan, a conference celebrated the article for its new graphical tools and high citations score.

That was only one anecdote. Earlier, Marion Crawford (soon to become Marion Crawford Samuelson) as a Radcliffe Masters degree student in Gottfried Haberler's Harvard class, who adjudicated in a term paper the current debate about "the Australian tariff." Her published *QJE* conclusion (1939) demonstrated that her teacher, at the height of his long, distinguished career, was mistaken in his belief that ". . . so important and versatile a factor of production as labor could hardly be hurt by free trade." What is important about the incident is not that a virtual beginner should score so clear a victory. Rather it was remarkable that, five years after the 1933 publication of Ohlin's fat magisterial *International and Interregional Trade*, any trade expert was still wedded to Bastiat's belief that free trade does help everyone.

Careful scholars will perceive that my 2004 point is analytically distinct from the point about self-immiserizing trade made cogently by Bhagwati (1958) and Johnson (1955) several decades ago: the 2004 lose-win effect depends not at all on inelasticity of any demand, instead being able to occur under favorable J. S. Mill demand elasticities. Also, 2004 broke new ground in providing a novel money-metric-utility that measured precisely "gains of winners" and "losses of losers," which did properly net out both consumer gains and producer losses. Moreover, its proof deliberately stuck completely to the canonical Ricardo-Mill labor-only model.

My final comment is substantive. Those orthodox pals who complained that my apostasy that would surely be misunderstood and be muddling, seemed to be mostly of the following belief: "Yes, there will be both win-win and win-lose inventions during the next 50 years. Any innuendo that win-lose inventions will probably outweigh win-win inventions is paranoid and implausible. Ergo, it's a lay-down hand for free trade."

Fair enough. However, if the next 50 years are like the last 50 years, there are plausible reasons why the advanced nations can expect to encounter in a biased way some possible preponderance of win-lose inventions from abroad. (Toyota overtaking General Motors and Ford falls into the win-lose taxonomy.) The basic explanation for this presumptive bias is that the Wagnerian leitmotiv of 1950–2050 will be the continuing and ineluctable spread of imitative knowledge from the metropolitan heartland to the sleep-walking peripheries where low-paid, educatable people and businesses can most easily learn by imitation. The result is not a Spenglerian "Decline of the West," but rather a trend toward (but not necessarily all the way) to geographical equalization. Bet for the future half century to be a repetition of what took place historically in the first half: continued growth in the advanced world but arguably at a lower rate ceteris paribus due partly to competing imitative inventions abroad.

It may be of interest that none of my chastening pals expressed concern about globalization's effects on greater inequality in a modern age when transfers from winners to losers do trend politically downward in present-day democracies.

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Chairing the Federal Reserve

In their article "Choosing the Federal Reserve Chair: Lessons from History," Christina D. Romer and David H. Romer (Winter 2005, pp. 129–162) emphasize the link between the ecomomic ideas that successive Federal Reserve chairmen brought with them and the subsequent policy outcomes. They identify William McChesney Martin Jr., chairman from 1951 from 1970, as a successful chairman with realistic economic beliefs who oversaw a long period of low inflation and mild real fluctuations in the 1950s and early 1960s, but find that "Martin's public statements and writings were relatively few before he joined the Federal Reserve" (p. 152), so their sources on his prior economic beliefs are limited to his 1951 Senate confirmation hearing and statements that he made in 1950 while Assistant Secretary of the Treasury.

There is, however, another source relevant to Martin's involvement with economics during the Depression, a source to which Martin did not draw attention while he was at the Treasury and Federal Reserve: his founding editorship in 1932–1933 of *The Ecomomic Forum*. That unusual quarterly journal brought together contributors including the more adventurous of leading economic theorists from John Maynard Keynes and Irving Fisher (advocating 100 percent reserve requirements on bank deposits) to Bertil Ohlin (on Knut Wicksell) and Erik Lindahl (on Sweden's monetary experiment), but also decidedly nonmainstream amateur economists such as Major C. H. Douglas and the poet Ezra Pound, whose ecomomic views Romer and Romer would assuredly not regard as sensible (Dimand, 1991). The inaugural issue (Winter 1932–1933) opened with articles by Alvin Johnson, Director of the New School for Social Research, and by Keynes, but also included "Is Fiat Money Any Worse Than Fiat Poverty?" by William T. Foster of the monetary heretics Foster and Catchings. The second issue (Spring 1933) carried articles on creating employment by Princeton trade theorist Frank Graham and on how to restore prices to their 1929 level by Columbia finance professor Benjamin Graham and by James Harvey Rogers, Sterling Professor of Economics at Martin's alma mater, Yale, and monetary advisior to the Roosevelt Administration, but also included Major C. H. Douglas expounding "The Premises of Social Credit." The Summer 1933 featured A. A. Berle Jr. on the economics of organization, Ezra Pound on Social Credit, Oxford chemistry professor and Nobel laureate Frederick Soddy summarizing his heterodox monetay theories, and "Pareto: The Karl Marx of Fascism" by Richard Worthington, a Yale student of Rogers (who had studied with Pareto in Lausanne). At the end of 1933, Martin's place as one of the two editors was taken by Frank Vanderlip, a retired financer (Assistant Secretary of the Treasury 1897–1900, President of the National City Bank 1909–1919), who had (like William McChesney Martin Sr.) been involved in drafting the Federal Reserve Act and who had, in retirement, become involved in Irving Fisher's campaign for "stable money" (a price level rule for monetary policy).

Martin's early economic views, and those of his fellow editor Joseph Mead, were expressed in the twenty-page introduction to the first issue of the *Economic Forum*: "The need for better planning is shown by the faulty interplay between our monetary mechanism and the system of production; by the maldistribution of the means of payment and the inability of the people to consume the products actually produced; by the waste and inefficiency, resulting from plant duplication and overly optimistic expansion of capital equipment in various industries; by the effects on employment of the mechanization of economic activity where labor-saving devices can be applied. . . . However, the greatest impetus to the growing conviction that the monetary system can and must be so controlled and regulated so as to stabilize the price level within reasonable limits, has come from the brilliant discussions of John Maynard Keynes" (pp. 15, 19). The *Economic Forum* ceased publication in 1937, and was largely forgotten, along with Martin's involvement. The first biography of Martin is only now forthcoming (Bremner, 2004) and will undoubtedly shed new light on the intellectual developement of that valuable public servant.

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In their article "Choosing the Federal Reserve Chair: Lessons from History" (Winter 2004, pp. 129–162), Christina D. Romer and David H. Romer present much valuable information.

However, because macroeconomic outcomes are affected by many factors, to evaluate a Fed chair—and from that to deduce what characteristics make for a good chair—requires agreeing upon how much the Fed is responsible for the events during the chair's tenure. Unlike Romer and Romer, I would blame the two inflation spikes of the 1970s mostly on OPEC rather than upon the Fed and would give the Fed chair most of the blame for the unempolyment spike of the early 1980s. Thus, Arthur Burns—and his qualifications—get better marks than Romer and Romer would give them, while Paul Volcker and his characteristics get a poorer grade.

Also, the article seems to me to have a something of a tone of "We fortunate inhabitants of the current era who know the natural rate hypothesis are able to conduct sound monetay policy, and so were an outstanding few persons in the past who managed to intuit this hypothesis. But back before this vital truth was known, successful monetary policy was generally elusive." Given how much understanding of economics change over the decades, it would be wise not to claim such unique wisdom for the perspective of our generation. Moreover, the natural rate hypothesis has very limited operational utility because the NAIRU changes in ways we don't know how to predict. Thus, Greenspan's success in the 1990s should be credited to his being a flexible empiricist who was willing not to raise interest rates during the second half of the decade when inflation remained low but unemployment was far below what previously had been the NAIRU. Had Greenspan been a true believer in the natural rate of unemplyment, he might well have choked off the expansion in mid-decade. Indeed, with hindsight clearly showing a large decline in the NAIRU took place, there probably are Young Turk economists now writing less-than-politely about how their elders could ever have been so mistaken as to believe that monetary policy could be mechanically derived from the natural rate of unemployment.

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The views expressed here are the personal views of the author, not positions of the institutions with which he is affiliated.

Correction

An error appeared in "Rational Choice Theory and the Paradox of Not Voting" by Timothy J. Feddersen, which appeared in Winter 2004, volume 18, number 1, pp. 99–112. The first equation in footnote 1 on page 101 should be

$$\frac{e^{n(2\sqrt{\sigma_1\sigma_2}-\sigma_1-\sigma_2)}}{4\sqrt{\pi n\sqrt{\sigma_1\sigma_2}}}\left(\frac{\sqrt{\sigma_1}+\sqrt{\sigma_2}}{\sqrt{\sigma_j}}\right).$$

The editors thank Nick Shryane of the University of Manchester for bringing the error to our attention. This change has also been noted in the archived version of this article at ⟨http://www.aeaweb.org/jep/⟩.