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*Editorial note:* In this piece, Professor Horwitz comments on Professor Eggertsson. We hereby invite Professor Eggertsson to reply to Professor Horwitz, and would welcome such reply for publication in the next or any future issue of the journal.

# Great Apprehensions, Prolonged Depression: Gauti Eggertsson on the 1930s

Steven Horwitz<sup>1</sup>

## ABSTRACT

*[U.S. Treasury Secretary Henry Morgenthau, Jr.]:* No, gentlemen, we have tried spending money. We are spending more than we have ever spent before and it does not work. And I have just one interest, and if I am wrong, as far as I am concerned, somebody else can have my job. I want to see this country prosperous. I want to see people get a job. I want to see people get enough to eat. We have never made good on our promises...

But why not let's come to grips? And as I say, all I am interested in is to really see this country prosperous and this form of Government continue, because after eight years if we can't make a success somebody

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else is going to claim the right to make it and he's got the right to make the trial. I say after eight years of this Administration we have just as much unemployment as when we started.

*Mr. Doughton:* And an enormous debt to boot!

*HMJr.:* And an enormous debt to boot! We are just sitting here and fiddling and I am just wearing myself out and getting sick. Because why? I can't see any daylight. I want it for my people, for my children, and your children. I want to see some daylight and I don't see it...

—Transcript of private meeting at the Treasury Department, May 9, 1939, F.D. Roosevelt Presidential Library<sup>2</sup>

Gauti Eggertsson's article "Great Expectations and the End of the Great Depression" in the September 2008 issue of the *American Economic Review* offers an interpretation of the transition from the Hoover to the Roosevelt Administration that will provide ammunition for defenders of the New Deal and others who generally see activist government as the appropriate policy response to major economic declines. Eggertsson argues that the period 1933 to 1937 represented an end of the Great Depression, and that such recovery was driven by a regime change between the Hoover and Roosevelt administrations. The Hoover administration was defined by adherence to three "policy dogmas" that Roosevelt decisively rejected. Roosevelt took actions to make his commitment to rejecting those dogmas credible, and such moves shifted expectations in ways that led to recovery.

The "policy dogmas" in question are ones normally associated with significant limits on government intervention, while their rejection gave Roosevelt much more latitude to expand government activism. Eggertsson contends that the Hoover Administration was defined by its adherence to three "almost universally accepted policy dogmas of the time: (a) the gold standard, (b) the principle of the balanced budget, (c) the commitment to small government" (Eggertsson 2008, 1477). He portrays the Roosevelt Administration, by contrast, as not only rejecting these "dogmas" (and apparently having no "dogmas" of their own), but also as intending<sup>3</sup> to signal a regime change that would change the public's expectations about not just the future magnitudes of important macroeconomic aggregates, but also shift their expectations about the policy-making process itself. Those expectational shifts were, he argues, key to recovering from the Great Depression.

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2. This transcript is also cited in Folsom (2008, 2).

3. Eggertsson implies that the Administration intentionally managed public expectations, for example when he writes: "The key to the recovery was the successful management of expectations about *future policy*" (1476).

He provides a dynamic stochastic general equilibrium model that attempts to show how, if the supposed Hoover regime had continued, so would the slide: “In the absence of the regime change, however, the economy would have continued its free fall in 1933, and output would have been 30 percent lower in 1937 than in 1933...” (Eggertsson, 1506).

The model’s relevance and value stands or falls with the soundness of its representation or explanation of the most important factors in the actual history. I argue that Eggertsson’s research fails on several counts:

1. It is wrong to view the Great Depression as over by 1937. Just as recovery from an illness is a return to one’s normal state of health, economic recovery is a return to economic normalcy. Eggertsson’s recovery is illusory. Even the Roosevelt Administration recognized that there was little in the way of meaningful recovery as late as 1939 as the opening quote from Henry Morgenthau indicates. Also, it is inappropriate to separate the period 1933-1937 from the years that followed.

2. Eggertsson is accurate in his depiction of Hoover as committed to the policy of maintaining the public’s right to convert 20 dollars into an ounce of gold. But that view did not preclude the result that Eggertsson thinks needed to be obtained, namely significant monetary expansion. Moreover, if the traditional terms were too constraining, redefining the gold content of a dollar while maintaining normal convertibility was another “gold standard” option. It should be noted that Eggertsson, by treating the monetary regime during Hoover as simply “the gold standard,” follows the fashionable “gold standard” monolithism; he fails to recognize that a more classical kind of gold standard had already been abandoned.

3. As for the two other alleged “policy dogmas,” namely, balanced budgets and small government, they not only were not “almost universally accepted” but they greatly misrepresent what Hoover believed and what his administration did. Hoover’s own track-record and the policies he adopted after the stock market crash belie Eggertsson’s characterization of dogmatic attachment to balanced budgets and small government. Eggertsson’s depiction of the “Hoover” regime contradicts the historical record.

4. By using the phrase “policy dogmas” to describe Hoover’s views while giving Roosevelt’s no specific label, Eggertsson implies that no dogmas guided Roosevelt’s policies. Here too, the historical evidence suggests otherwise. Roosevelt too had his policy dogmas, and they often exacerbated problems, delayed recovery, and weakened the long-run dynamism of the U.S. economy.

5. Yes, the Roosevelt Administration rejected the three positions attributed to Hoover (though in 1932 Roosevelt did promise to balance the budget), and, yes, its bents were decidedly more statist. But it is misleading to view its policies as

anything close to a consistent strategy for recovery. At least in the first term, the Roosevelt administration was much more about long-run reform than about a coherent set of recovery policies, and there is no reason to believe that Roosevelt or his administration had a concerted plan about which specific policies to undertake to achieve long-run reform. Eggertsson (1476) suggests that the regime change was intentionally designed to shift expectations. But much of what Roosevelt did he made up as he went along. The particular policies adopted or proposed by Roosevelt often changed from year to year, season to season, and even month to month.

6. Eggertsson gives no discussion of Robert Higgs' (2006a) work on the debilitating effects of uncertainty regarding rules, which Higgs dubs *regime uncertainty*. Eggertsson flagrantly overlooks the ways in which Roosevelt's policies generated great uncertainty and great apprehension. The broad ideological dogmas of the Roosevelt Administration stepped up the assault on profits, property, and liberty, and involved virulently anti-business propaganda. As Higgs argues, the combination left private investors fearing for their property and the value of their long-term investments. By heightening apprehensiveness, Roosevelt's bold experimentation snuffed out private investment in long-term projects. An electronic search determines that "private investment" never appears in Eggertsson's article.

## **Will the Real Herbert Hoover Please Stand Up?**

If Eggertsson's paper were merely an exercise in modeling, asking what might happen if "an" economy transitioned from accepting to rejecting the three policy dogmas in the midst of a severe recession, that would be one thing. But Eggertsson is declaring an explanation of the Great Depression. He has a duty to portray accurately the policy positions of both Hoover and Roosevelt (as well as the evolving condition of the economy). In the case of Hoover, at least two of Eggertsson's three policy dogmas are severe distortions of Hoover's beliefs and practice.

Eggertsson's portrayal of Hoover as dogmatically committed to a balanced budget and small government is utterly at odds with Hoover's personal history and stated beliefs, as well as the actual policies he put into place while president. Hoover's first major role in government was as head of the Food Administration upon the US entry into World War I in 1917. He leapt into that job with great energy, having long believed that government can and should play a large role in the economy. In 1912, he had supported Teddy Roosevelt and the Progressives for the presidency, and was touted by many, including Franklin Roosevelt, as a potential Democratic presidential candidate in the 1920s. He was a registered

Republican, however, and in 1921 accepted a position as Secretary of Commerce under President Harding, a job that he retained through most of the 1920s.

Hoover vowed to turn what was one of the lowest-profile departments of the federal government into a more visible one, specifically by increased interaction with businesses and involvement in economic policy. Donald Stabile (1986) has characterized his views as a desire to “transform the structure of the US economy from one of laissez-faire to one of voluntary cooperation” (819). In her book *Herbert Hoover: Forgotten Progressive*, Joan Hoff Wilson (1975, 68) summarizes Hoover’s economic views this way:

Where the classical economists like Adam Smith had argued for uncontrolled competition between independent economic units guided only by the invisible hand of supply and demand, he talked about voluntary national economic planning arising from cooperation between business interests and the government... Instead of negative government action in times of depression, he advocated the expansion of public works, avoidance of wage cuts, increased rather than decreased production—measures that would expand rather than contract purchasing power.

When paired with his long-standing antipathy to free trade (65-66), this was hardly the program of a “limited government” or “laissez-faire” dogmatist. Other ideas he championed around this time included “increased inheritance taxes, public dams, and, significantly, government regulation of the stock market” (Rothbard 2008 [1963], 188).

As early as the 1920-21 recession, Hoover was becoming famous for convening conferences with business leaders as a way to use the power of government to generate what he saw as desirable “cooperation” as opposed to individualistic competition. In contrast to Harding’s much more genuine commitment to laissez-faire during that recession, Hoover quickly got busy organizing conferences and relief efforts and exhorting businessmen and the public to bring that spirit of “mobilization” and “spontaneous cooperation” experienced during the war to peacetime economic reconstruction. At one conference on unemployment in September of 1921, Harding opened with remarks committing him to keeping the federal government out of such issues, and yet Hoover followed by expounding the need to “do something.” The conference leaders, with Hoover’s approval, coalesced around a call for more “government planning to combat depressions and to bolster the idea of public works as a depression remedy” (Rothbard 2008 [1963], 192). Historian David Kennedy (1999, 48) describes Hoover’s activism this way: “No previous

administration had moved so purposefully and so creatively in the face of an economic downturn. Hoover had definitively made the point that government should not stand by idly when confronted with economic difficulty” (see also Vedder and Gallaway 1993, 67-68).

In the 1920s, Hoover used his position as Secretary of Commerce to call for a number of government interventions into the recession, all of which were rejected by Harding and the rest of his Administration. As Commerce Secretary, Hoover also stepped into a number of labor relations issues, trying here also to use the power of government to resolve various disputes. Thus, the ideal of small government was neither “Hoover’s” nor “almost universally accepted.” Powerful government actors along with many leaders of private industry boosted for American forays in fascism, which was often openly admired.

Eggertsson is largely correct about Hoover’s commitment to the gold standard. In numerous speeches at the time, Hoover asserted the importance of gold to a stable monetary system and healthy economy, even after 1929 as the depression began to unfold. The presupposition in Eggertsson’s take is that “eliminating the gold standard” (1504) was necessary for reflationary monetary policy. But, as Friedman and Schwartz (2008 [1963], 174-86) first pointed out and Bordo, Choudhri, and Schwartz (2002) have confirmed, the stock of “free gold” (i.e., that portion of the Fed’s gold holdings not needed for meeting its minimum reserve requirements or collateral against its various liabilities) during the early 1930s was more than sufficient to support the expansion of the money supply necessary to offset the fall in the velocity of money that was driving down total expenditures and severely exacerbating the problem of bank failures. Moreover, Timberlake (2005, 217) points out that the levels of gold reserves aside, the Fed Board had it within its power to simply “suspend gold reserve requirements entirely” if it had the will to do so. Faulting Hoover for his commitment to “the gold standard” overlooks the ways in which the desired remedy was in the hands of the Federal Reserve at the time without the need to abandon gold convertibility at 20 to 1, much less convertibility entirely. Richard Timberlake (2005) and Lawrence H. White (2008) argue that the more important “policy dogma” blocking reflationary monetary policy was not convertibility to gold nor the traditional rate of conversion, but the Fed’s commitment to a version<sup>4</sup> of the Real Bills Doctrine that unfortunately kept the Fed focused on credit conditions to the exclusion of money-supply conditions. Eggertsson’s diagnosis of monetary problems as caused by Hoover’s commitment to gold never mentions the ruling monetary doctrine at the Fed nor the ample stock of gold. It presupposes that

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4. For a defense of the Real-Bills Doctrine in the context of the Great Depression and otherwise, see Hortlund (2006).

sizeable expansion was incompatible with a gold standard of any sort—a presupposition that is probably false.

Hoover's lack of commitment to "small government" and "balanced budget" dogmas would be on display when faced with the 1929 crash. His first moves consisted mostly of calling more conferences of industrial leaders in Washington, the major outcome of which was a pledge by them not to lower wages in the face of the recession so as to maintain "purchasing power." Hoover also enlisted the Federal Farm Board, which was created under his watch in June of 1929, to enhance its role as a cartelizer of American agriculture (Kennedy 1999, 43-44; see also Rothbard 2008 [1963], 228 and Smiley 2002, 13). The Federal Farm Board lent hundreds of millions of tax dollars to farmers and established the Farmers' National Grain Corporation, which bought up wheat at the artificially high price created by the FFB. Kennedy (1999, 44) quotes Hoover as saying of the FFB that they had a "responsibility, authority and resources such as have never been conferred by our government in assistance to any industry." Similar cartel devices were created in livestock and other agricultural products.

By 1930, Hoover was pushing for the maintenance of wage rates even more firmly, and fighting for and signing the Smoot-Hawley Tariff. Viewing Hoover as dogmatically committed to small government will have to contend with his willingness to extend the power of government to regulate the prices and importation and exportation of a huge number of goods under Smoot-Hawley. If his commitment to small government was genuine, it is hard to imagine him fighting for that tariff, even as over 1000 economists objected (see *EJW* Editors 2007). Almost all historians of the Great Depression agree that the tariff was one reason that what was a nasty recession became a more severe depression, yet Eggertsson never acknowledges this example of Hoover's extension of government's power.

Federal government expenditures rose from \$4.2 billion in 1930 to \$5.5 billion in 1931, a one-year increase of 31 percent. The deficit for 1931 was \$2.2 billion, by far the largest peacetime deficit ever (Rothbard 2008 [1963], 263-264). In February of 1931, Hoover signed the Wagner Employment Stabilization Act, which increased expenditures for public works. Despite falling prices, Hoover continued to urge industrialists to maintain wages, a cause helped notably by the Davis-Bacon Act, which limited hours and promised "prevailing wages" in government construction projects. Hoover also tried to help keep wages up by urging Congress to dramatically restrict immigration, further evidence of his readiness to expand the powers of government. By the end of 1931, Hoover was moving to expand public works and government relief expenditures as well as threatening and/or engaging in intervention in the financial and housing markets.

Contrary to the impression that Eggertsson creates, Hoover was throughout this period rather unconcerned with the federal government's budget deficit. In fact, Eggertsson (2008, 1487) offers only two very thin pieces of evidence for Hoover's supposed dogmatic adherence to balanced budgets. The first is a quote from a December 1930 statement (which he misdates as July):

For the Government to finance by bond issues deprives industry and agriculture of just that much capital for its own use and for employment. Prosperity cannot be restored by raids on the public Treasury.

The context of that statement was Hoover complaining about bills being introduced that would have greatly increased government expenditures "beyond the sums which I have recommended for the present and next fiscal year by a total of nearly \$4,500 million." Furthermore, Hoover claims:

The gross sums which I have recommended to carry on the essential functions of the Government include the extreme sums which can be applied by the Federal Government in actual emergency employment or relief, and are the maximum which can be financed without increase in taxes.

So Hoover's complaint here is about expenditures *beyond* what he had already proposed for 1931, which eventually amounted to a 31 percent increase over those of 1930. In fact, Hoover explicitly rejected dogmatic commitment to a balanced budget in May of 1931. Kennedy (1999, 79) reports on Secretary of State Henry Stimson's diary note that Hoover argued "strenuously against the budget balancers in his own cabinet. 'The President likened it to war times...He said in war times no one dreamed of balancing the budget. Fortunately we can borrow.'" This calls into question Eggertsson's (1482) claim that Hoover's deficit "was not a deliberate policy," which he contrasts with Roosevelt's deliberate deficits. It also suggests the regime change that is central to Eggertsson's argument is mostly illusory.

There are similar problems with the second piece of textual evidence he presents for Hoover's commitment to a balanced budget, which comes from a speech from September of 1931. It is important to remember that this speech was given toward the end of a year in which Hoover was *already* running a \$2.2b deficit, the largest peacetime deficit in US history up to that point. Eggertsson (1487, his ellipses) quotes Hoover as follows:

Every additional expenditure placed up on our government in this emergency magnifies itself all out of proportion into intolerable pressures, whether it is by taxation or by loans. Either loans or taxes [...] will increase unemployment. [...] We can carry our present expenditures without jeopardy to national stability. We can carry no more without grave risks.

Examination of the speech raises further concerns about Eggertsson's scholarship. Eggertsson omits the opening clause of the sentence, immediately preceding the quoted material. Hoover (1931) said "Whatever the deficit may be and in whatever manner it may ultimately be met, every additional expenditure..." Thus, not only does Eggertsson misquote the speech by writing "Every" with a capital E, he has deliberately omitted Hoover's immediately proximate approval of an historically large budget deficit. Furthermore, earlier in the speech, Hoover (1930, my emphasis) refers to the "high and *necessary* extra burden of public works in aid to the unemployed, of aids to agriculture, and ...increased benefits and services to veterans" as elements of expenditure already being undertaken. The larger context of that speech is Hoover, again, saying that a deficit in and of itself is not necessarily a big problem, but that at that point he had gone as far as he was willing to go. Note too that this speech calls into question his supposed commitment to small government, given the number of programs he lists as part of the "high and necessary extra burden of public works."

The final nail in the coffin of Eggertsson's attempt to contrast so starkly the Hoover and Roosevelt Administration's policy regimes is the set of proposals Hoover offered in 1932, the year before the transition. Kennedy (1999, 83) refers to this set of programs as "Hoover's second program" and also notes that it "lay the groundwork for a broader restructuring of government's role in many other sectors of American life, a restructuring known as the New Deal." (See also Rothbard (2008 [1963], ch. 11, who terms this "The Hoover New Deal".) Hoover's policies consisted of several major new government programs and their associated expenditures, including a "Reconstruction Finance Corporation," making more banks eligible for discounting at the Fed, various programs to help mortgage holders, larger federal public works, more immigration restrictions, and loans to the states. Many of these measures anticipated precisely those that Roosevelt would put into place in the early days of his New Deal. Given all of this activity, describing the break between Hoover and Roosevelt as a "regime change" that rejected the dogma of small government is severely inaccurate. Eggertsson is right, however, that 1932, at least, saw Hoover raising the budget deficit as a bigger concern, leading to him pairing those expenditures with the Revenue Act of 1932, which raised taxes dramatically for many citizens. The higher tax rates yielded

lower tax revenues, with the result being a somewhat smaller but still historically large deficit of \$1.4b at the federal level. So despite his professed concern about the deficit, Hoover's actual policies did little to cause it to disappear—just as Roosevelt's campaign talk of 1932 did little to reduce deficits thereafter.

Hoover's own recapitulation of what his administration had done in response to the depression speaks for itself. In his August 1932 acceptance of the Republican presidential nomination, Hoover (1932) said:

We might have done nothing. That would have been utter ruin. Instead we met the situation with proposals to private business and to Congress of the most gigantic program of economic defense and counterattack ever evolved in the history of the Republic. We put that program in action... These programs, unparalleled in the history of depressions of any country and in any time, to care for distress, to provide employment, to aid agriculture, to maintain the financial stability of the country, to safeguard the savings of the people, to protect their homes, are not in the past tense—they are in action... No government in Washington has hitherto considered that it held so broad a responsibility for leadership in such time.

With just months to go in his term, Hoover clearly saw the work he had done over the prior three years as being anything but adherence to Eggertsson's "almost universally accepted policy dogmas." True to form, Hoover had approached the onset of the Great Depression with an aggressive expansion of government, including record budget deficits and an enormous increase in tariffs and income taxes, as well as an attempt to strong-arm businesses into not cutting wages in the face of a severe deflation, itself the result of mismanaged government intervention in the monetary system. The Roosevelt Administration was hardly a "regime change," as Kennedy (1999, 118) observes: "If Roosevelt had a plan in early 1933 to effect economic recovery, it was difficult to distinguish from many of the measures that Hoover, even if sometimes grudgingly, had already adopted." Eggertsson's portrayal of the Hoover presidency makes reckless mischief with the historical facts.

## **The Ideology of the New Deal**

Eggertsson is more accurate in characterizing the Roosevelt Administration, but there too we find serious problems. First, Roosevelt is said to have rejected the "policy dogmas" of Hoover and, by implication, had no policy dogmas of his own. Second, he strongly implies that Roosevelt's policies were part of a deliberate

strategy to turn around expectations and thereby generate recovery. He is wrong on both counts.

We may distinguish levels of policymaking. Higher up are guiding beliefs or ideological views held by policymakers, and, lower down are the particular policies adopted as means toward the broader goals, values, or visions. Thus, when Eggertsson refers to the supposed “policy dogmas” of Hoover, he is talking of guiding beliefs. If one is committed to “smaller government,” there are a number of ways to advance that goal. If one is ideologically committed to a balanced budget, one could pursue a variety of combinations of taxation, expenditure, and seigniorage. Eggertsson seems to suggest that Roosevelt had few “dogmas” of the first sort, rather just a generic pragmatic benevolence, but a package of specific policies to advance the generic goal of well-being. The reality was pretty much just the opposite: Roosevelt *did* have a guiding set of ideological beliefs and goals (dogmas, if you wish) but he was constantly improvising as to what sorts of concrete policies would best enable him to advance them.

The Roosevelt Administration had strong ideological commitments that guided its decision making. Central to Roosevelt’s rise to power and to the shape of his presidency was the group of intellectuals known as the “Brain Trust.” In early 1932 as he was putting together his campaign, Roosevelt decided to put less emphasis on the traditional advisory group of politicians and businessmen and instead looked for the best minds in the universities as a source of ideas. His various friends and political advisors put him in touch with a group of intellectuals that included Raymond Moley, Adolf Berle, Samuel Rosenman, Hugh Johnson and Rex Tugwell. This group of progressives had a long-standing relationship with one another, much of it centered around magazines such as *The Nation* and *The New Republic*. They reflected the collectivist values and statist ideas of the Progressive Era, notably that sufficiently wise and good-hearted people with the relevant data could use the power of government, especially economic planning, to improve on free-market coordination. Major Brain Trust figure Rex Tugwell, along with other intellectuals of the same kind of statist mindset, went to the Soviet Union in 1927 to explore alternatives to US-style capitalism (Shlaes 2007, ch. 2). Though not full-bore collectivists in the Soviet tradition, they all believed that scientific management could do the free market one better.

Among the dogmas Roosevelt’s advisors clung to was the belief that the depression was caused by underconsumption. The variants were many, but the general argument was that wealth in the 1920s had flowed to the rich and not the poor, leaving the latter without the means to consume all of the production that had characterized the era. This supposed mismatch between consumption and production was the result of a lack of coordination, and was inevitable under free enterprise. Other versions of the theory argued that overly intense competition

was part of the problem as it encouraged the expansion of production and the lowering of prices that cut into corporate profits. Also, members of the brain trust, and Roosevelt himself, strongly believed that greed, particularly by those who became wealthy in the stock market and other forms of speculation, was a root cause of the depression, a belief that led to a consistent pattern of scapegoating of wealthy and prominent businessmen. Such jealousy and hostility towards independent and possibly dissident islands of wealth and cultural power is typical of the mentality of progressivism, fascism, and socialism. Finally, it was a further article of faith among Roosevelt and many of his advisors that, with the closing of the western frontier, the possibilities for growth were limited, especially for the poorest, with the ethical implication that those who had made their fortunes would be made to “share” them with the rest of society. Many of these beliefs were reflected in portions of Roosevelt’s speech in July of 1932 accepting the Democrats’ nomination for the presidency (Folsom 2008, 37). In a speech two months later in San Francisco, Roosevelt reiterated many of these themes, adding that his administration would have to face “establishing markets for surplus production; of meeting the problem of underconsumption; distributing the wealth and products more equitably and adapting the economic organization to the service of the people” (Roosevelt quote at Folsom 2008, 41). The Roosevelt team was hardly dogma-free when it came to both diagnosing the Great Depression and planning rehabilitation.

Yet the diagnosis of Roosevelt’s brain trust was not that the US economy had caught a transient malady that emergency responses could cure. Rather they saw the Great Depression as the manifestation of chronic problems in the US economy and capitalism more generally. Shlaes (2007, ch. 9) argues that they also saw the election of Roosevelt as the opportunity to put into place a variety of structural and institutional reforms that would address those chronic problems. Instead of a set of policies focused on how to *recover* from the particular episode of the Great Depression, Roosevelt’s advisors were creating a set of *reforms* that would transform the very structure of the US economy for the long haul. In his autobiography treating his involvement in the New Deal, Ray Moley (1966, 292) wrote that during the NRA’s creation, “The concept of recovery as distinguished from reform was forgotten.” Putting aside the question of whether their diagnosis and prescriptions were accurate, there can be little doubt that they were driven by a set of ideas about the causes of the Great Depression and the structural cures required.

This matters for Eggertsson's argument because numerous observers of the New Deal have argued that it was much more about reform than recovery.<sup>5</sup> Viewing Roosevelt's assumption of power as a shift in tactics in the management of expectations is problematic because many of the actors in question did not see what they were doing as about short-run counter-cyclical policy (recovery) but as about long-run structural reform. Those reform proposals were guided by a set of deep-seated beliefs on the part of Roosevelt's advisors, not a "dogma-free" attempt to remedy the present economic crisis.

## Roosevelt the Experimenter

Within the broad set of structural changes thought desirable by the advisors, there was ample room for differences in policy specifics. Even with the broad contours of the problems and the solutions identified, how the administration chose to go about enacting policy could vary. Roosevelt himself referred to his preference for "bold, persistent experimentation" during the early days of his campaign in 1932 (Roosevelt 1932). His willingness to try things, see if they worked, and change courses if they failed certainly is to be preferred to stubbornly sticking to policies that continue to fail.<sup>6</sup> That experimentation poses two problems for Eggertsson's argument, however. First, it suggests that there was not the sort of coherent, organized regime that he suggests in the article. Second, Eggertsson ignores the degree to which that experimentation itself prolonged the depression and made it more severe than it would have been under a less experimentalist regime.

Examples of Roosevelt's ad hoc approach to policy are not hard to find. During the famed first 100 days, Roosevelt talked up almost every possible policy option as part of the reform agenda he wished to pursue. The list of programs is itself impressive in its sheer scope, which reflected an acceleration along the statist path trod by Hoover. The range of programs was full of contradictions, from slaughtering millions of baby pigs and tearing up acres of cotton under one program while other programs were trying to feed and clothe the impoverished, to the ways in which different programs affected prices and international trade. As Shlaes (2007, 149) argues, the key for Roosevelt was that "Americans must know

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5. This point is a running theme in Shlaes (2007) and Best (1991). Kennedy (1999) titles an entire chapter "A Season for Reform," and Smiley (2002, 97) writes: "There is little evidence that recovery was first on the agenda of any of these competing groups [of FDR advisors]; rather, all intended first to reform industry in one way or another, in order to move to the 'correct' path of recovery."

6. The "experimental" nature of the Roosevelt presidency is supported by Moley (1966, 224): "We were wide open to the influx of ideas—new ones and old. Anything seemed acceptable that appealed to our common sense and was worthy of a trial."

Washington was doing something. If there were contradictions between experiments and within them, well, that did not matter.”

Eggertsson focuses on Roosevelt’s decision to abandon “the gold standard,” so as to push up prices. This decision is part of the set of policies that Eggertsson suggests was a coordinated “regime change” on the part of Roosevelt and his advisors. But Roosevelt’s approach to the monetary regime was hardly systematic. Shlaes (2007, 147-148) describes how the administration was setting the price of gold in 1933:

Over the course of the autumn, at the breakfast meetings, Roosevelt and his new advisers experimented alone. One day he would move the price up several cents; another, a few more.

One morning, FDR told his group he was thinking of raising the gold price by twenty-one cents. Why that figure? his entourage asked. “It’s a lucky number,” Roosevelt said, “because it’s three times seven.” As [Treasury Secretary] Morgenthau later wrote, “If anybody knew how we really set the gold price through a combination of lucky numbers, etc., I think they would be frightened.”

Shlaes (2007, 160ff) also describes the way Roosevelt handled the delegation sent to an international monetary conference in June of 1933. He sent a number of different representatives, but could not decide on what outcome he wished them to reach in their negotiations. He sent contradictory telegrams to different members of the delegation, including directing them not to accept an agreement with France and the UK that they had earlier negotiated. After having promised them he wanted the gold standard maintained, Roosevelt switched views yet again, leaving his delegation in the lurch. One member asked Raymond Moley to ask Roosevelt “not to change his policies again, because his sudden turns had been exceedingly embarrassing” (as quoted in Shlaes 2007, 163). Meanwhile, from early June to mid-July, the stock market was on a 10 percent slide. August found Roosevelt issuing a variety of executive orders with respect to gold, with new ones coming days after previous ones, often directly contradicting them. These are hardly the actions of president using a consistent set of policies to engineer the public’s expectations—except in the sense that they would be made to expect a government full of caprice and presumption.<sup>7</sup>

One of the reasons for Roosevelt’s ongoing experimentation was that he was constantly listening to different voices among his advisors. Roosevelt was

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7. Again, see Moley (1966, 229): “But if the country could have known how unclear we were [about economics], the tide of confidence [from the banking system rescue] would have frozen in its course.”

notoriously fickle about who he listened to and trusted, and when experiments variously failed, he was highly likely to decide next that a different member of the brain trust had the right ideas. As a new voice captured his attention, new policy experiments came forth. Although the message coming from all of those advisors was broadly consistent in the penchant for greater statism, the particular policies enacted in his first term changed with the vicissitudes in Roosevelt's hunches and could hardly be treated as a systematic shift in regimes in the way that Eggertsson suggests.

The ad hoc nature of Roosevelt's economic policy became even clearer during his run for re-election in 1936. It was fairly clear by this point that the variety of policy experiments he had tried in the three previous years had failed to make more than a dent in the depression, as unemployment remained near 20 percent and key macroeconomic variables were nowhere near their pre-depression levels. Out of policy options rooted in any real framework that would address the problems, Roosevelt instead turned to coalition building and using new programs neither for *recovery* nor *reform*, but for the third "r": re-election. As the public continued to demand more from the federal government, Roosevelt shrewdly began to craft policies that benefitted a bloc of voters and interest groups that would define the Democratic coalition for decades to come. He ratcheted up his attacks on the "economic royalists," on whom he blamed the depression and the lack of recovery. The combination of the rhetoric of class warfare and policies (e.g. Social Security) that attracted large blocs of voters was sufficient to win him a landslide re-election. The policies in question, however, had little to nothing to do with recovery from the depression. What was once experimentation and structural reform had now morphed into a cruder political calculus. Roosevelt's bold, persistent experimentation is not just a refutation of describing his policies as a coherent recovery strategy, it also raises the question of whether that very experimentation was a cause of the remarkable prolongation of depression.

## Regime Change or Regime Uncertainty?

Eggertsson's paper, like many studies of the New Deal, takes a peculiar view on what constitutes recovery. There is admittedly a great deal of debate over these issues, but it is not unreasonable to argue that the two most important indicators of the economy's macroeconomic health, real GNP per capita and the unemployment rate, did not return to their 1929 levels until at least 1939 in the case of GNP and at least 1941 in the case of unemployment. The GNP date reflects "back to 1929" levels and not "back to where trend would have been in 1939." Back to trend would have taken several years more, depending on the data

one uses. Unemployment as conventionally measured remained above 14 percent through 1939. Even if one excludes those in government make-work programs from the unemployment rate, it remains above its 1929 figure until the early 1940s (Hughes and Cain 2007, 481).

Even thinking generously, recovery from the Great Depression did not occur until at least six years after Roosevelt assumed power, and perhaps as many as 15 years. It is true that annual real GNP figures bottomed out in 1932 and 33, with the subsequent years showing improvement. Unemployment was worst in those years as well, but saw little sustained progress back to normalcy in the mid and late 1930s. So the question is why one should even view the Roosevelt years as an example of a successful recovery process, given that it took anywhere from 6 to 15 years to get the economy back to pre-depression levels of the major macroeconomic indicators.

In the 1936 election, things were still bad enough that Roosevelt had ratcheted up his anti-business rhetoric, blaming the private sector for the ongoing depression. The economy indeed had put the worst in the past by the time the first full year of Roosevelt's first term was complete and the relative growth that characterized the next few years was significant, but in the larger context of the depression, it was not nearly enough to lead to meaningful recovery, especially with respect to unemployment. The Administration itself was aware of how little they had really accomplished on the latter issue. Like a mountain that rises from the floor of a deep ocean, even the growth that Eggertsson points to was still not enough to bring the economy back above the surface from which it had descended. The change from Hoover to Roosevelt hardly results in any sort of transition to economic normalcy.

It is true that by some measures the US economy had improved over the first several years of the Roosevelt Administration. Three points are worth making. First, it is always hard to disentangle how much of the improvement was due to any regime change under Roosevelt and how much was due to underlying market forces toward adjustment and correction.

Second, the Fed's monetary loosening and the recovery of the banking system were also sources of recovery, independent of the New Deal programs *per se*. Eggertsson rightly identifies importance of the monetary expansion that came under Roosevelt, but wrongly characterizes this as somehow an exclusive view of that administration. There was healthy debate among economists of the 1930s over monetary policy, with the reflationary position being very much in the mix.

Finally, measured aggregates do not well distinguish between productive and unproductive expenditures. The New Deal could have indeed paid people to dig holes in the ground and fill them up as a way of increasing GNP, but that would hardly have made the population better off. Employment aggregates can

even be quite misleading about employment. By measuring “hours worked” rather than “unemployment,” Higgs (2009) shows, we get a more accurate picture of the state of 1930s labor markets. He demonstrates that labor hours stayed flat from 1932 to 1934, then rose until 1937, dipping in 1938, before rising again. Despite that increase, he concludes that “even as late as 1940, total hours remained below the 1929 level by 6 percent, and only in 1941, with the population vigorously engaged in mobilization for war, did total hours exceed the 1929 value, by 3 percent” (Higgs 2009, 4). Looking at labor hours confirms the observation by Morgenthau in the opening quote: six or seven years of New Deal programs had not caused employment to recover to pre-Depression levels. Whatever the aggregates say, this was not much recovery at all.

One of the questions that any discussion of the effectiveness of the New Deal must confront is why the return to health was so drawn out. If New Deal policies are being claimed to serve as a model for recovery (rather than reform), it is reasonable to ask why we should adopt an approach that took anywhere from 6 to 15 years to get the private sector back to pre-depression levels, much less trend. Serious engagement with this question is absent from Eggertsson’s account.

He makes no effort to grapple with two other relevant arguments of Robert Higgs.<sup>8</sup> First, Higgs (2006a) has argued that it was the policies of the New Deal itself that prolonged recovery by creating what he has termed “regime uncertainty,” which refers to the way in which a barrage of innovation in policy, especially innovations inimical to freedom, create uncertainty about the rules of the game, uncertainty that in turn retards private investment, particularly in long-term assets, and hence retards economic growth. Second, Higgs (2006b) argues that true recovery did not take place until after World War II. He says that highly non-standard conditions during the war distorted the meaning of standard macroeconomic data in ways that made the economy look to naïve eyes more healthy than it was.

The regime-uncertainty argument takes its start from the aforementioned legal and cultural assaults on private enterprise and individual liberty. Higgs points out that one of the worst performing variables of the depression years was private investment. Gross private investment virtually disappeared from 1929 to 1933, and then began a very slow recovery that by 1941 was still not quite where it was in real terms in 1929. It is not until the conclusion of World War II that private investment exceeds pre-depression levels. Further, as Higgs (2006a, 7) explains:

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8. Although the Higgs papers were published in one volume in 2006, the core arguments of the individual papers cited were published in 1992 and 1997, making them very much available to someone writing in the last few years.

One appreciates even better the deficiency of investment in the 1930s by considering net, rather than gross, investment... In 1929, when [gross private investment] was \$16.2 billion, net investment was \$8.3 billion. Net investment fell precipitously to \$2.3 billion in 1930 and then became negative during each of the following five years. In the period 1931-35, net investment totaled *minus* \$18.3 billion... For the eleven years from 1930 to 1940, net private investment totaled *minus* \$3.1 billion. Only in 1941 did net private investment (\$9.7 billion) exceed the 1929 amount.

Conventional macroeconomic aggregates such as GDP or GNP tell a different story because they include the large increase in government expenditures that characterized this period. If the goal of depression policy is to increase GDP, then government spending programs will do so virtually by definition. However, if one is concerned with how to improve the performance of the private sector, then one needs to look at the relationship between that government spending and private sector performance. The wisdom that has come down to our own time is that government spending can “stimulate” an increased flow of private sector spending through various multiplier processes. Higgs turns this story on its head.

The interaction between the public and private sector is somewhat more complicated than  $C + I + G$  might indicate. Public policies can have a definite effect on the way in which the private sector behaves. Private investors are most willing to take on the long-term investments necessary for economic growth when they perceive that they operate in an environment in which their property and contracts will be respected, where the rules of the game are known and relatively stable, and where they expect relative stability or predictability in the value of money. Higgs argues that these elements were not only significantly weakened during the 1930s by a whole variety of policies associated with the New Deal, including the expansion of government spending and unshackling of the money supply from gold that Eggertsson celebrates, but that private actors knew not when the barrage of changes would end, or what the rules would be just a few years forward. Again, “private investment” does not appear in Eggertsson’s article.

The most ambitious of the early New Deal programs, such as the NRA and AAA, were particularly disconcerting, as they were perceived as establishing powers that would continually threaten the property rights of industrialists and farmers. In these programs the reform and planning mentality was on full display. Using public-opinion data Higgs shows that many Americans anticipated continual movement toward fascism. Hugh Johnson, who was in charge of the NRA, was particularly aggressive in his attacks on big businesses as well as his

decisions to target various smaller entrepreneurs who violated NRA codes. Johnson, as well as Roosevelt, used war rhetoric to drive support for various programs, especially the NRA (Shlaes 2007, 151). Johnson compared the way in which housewives expanding their spending would be as heroic as the troops at the Argonne in World War I. He also referred to those who refused to participate in the NRA's codes as "Judases" who have "betrayed the confidence of the President and the people of this nation" (Johnson 1935, 264). Such statist programs and propaganda continued to retard the recovery of private investment.<sup>9</sup>

Beyond the early New Deal, the later programs, along with Roosevelt's increasingly shrill anti-business rhetoric starting in the 1936 campaign, continued to make private investors very nervous about what was to come. They had also seen the more systematic destruction of private property rights in Russia, Italy, and Germany, and were well aware that the Roosevelt crew had spoken admirably about elements of the economic policies of all three societies (Shlaes 2007; Goldberg 2007).<sup>10</sup> The polling data along with data on the spread between short and long-run interest rates, masterfully assembled and integrated by Higgs, strongly support the view that aggressive and unrelenting reform and statist attitudes were the cause of the low levels of private investment throughout the 1930s. Private-sector actors were very fearful of what the future might hold and consistently unable to commit to long-term investments when policies changed from day to day and when crucial data such as the price of gold were being decided on mere whim. The very factors that Eggertsson points to as being virtues of Roosevelt's policies are among those that Higgs sees as their vices. Government activism, busting forward under Hoover and gaining momentum under Roosevelt, spelt regime uncertainty and it prolonged rather than cured the Great Depression. Eggertsson spins a tale about renewed expectations bringing recovery by 1937; Higgs suggests a much more learned and sensible story about expectations made murky and dreadful by surging statism, making real recovery impossible.<sup>11</sup>

Higgs' other argument suggests that true recovery from the depression did not occur until after the conclusion of World War II, casting further doubt on Eggertsson's model and its defense of Roosevelt's shift in policies. Higgs (2006b) offers a number of powerful arguments for his claim that a standard reading of the

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9. Cole and Ohanian (2004) make the argument that the New Deal programs might have extended the Great Depression by as much as seven years.

10. As both Shlaes and Goldberg note, admiration in the United States for the fascist experiments, especially Mussolini, was hardly unique to the political left, as a good number of conservatives and businessmen admired them as well.

11. In a study of the causes of the Great Depression published in 1937, Phillips, McManus, and Nelson (1937, 242) wrote: "[C]onditions in the investment market are still [early in 1937] such that extensive long-term investment is not being made." Contemporary observers saw the problem Higgs points to and recognized that neither private investment nor employment could be described as "recovered" by 1937.

typical macroeconomic measures vastly over-rated the health of the economy during World War II. I will not sketch all of the arguments, which were originally published in the *Journal of Economic History* in 1992 and have never been challenged by any scholar.

Higgs computes an alternative measure of real personal consumption per capita using Friedman and Schwartz's Net National Product deflator. That alternative measure tries to take into account the effects of price controls and other elements of wartime. Using that deflator, Higgs (2006b, 71) shows that real personal consumption per capita was essentially flat from 1939 to 1945, rising only a total of 6.8 percent over the 6 year period. Only in 1946, with the conclusion of the war, does it start to rise significantly. Combined with the continued low level of private investment during the war, it is hard to make the case that even if Roosevelt's New Deal policies did not end the depression that his war spending did. Neither seems plausible if Higgs is correct.

Higgs offers a war-adjusted concept of GNP that builds from Kuznets' "peacetime" GNP that deleted all war outlays and then further subtracts "gross war construction and durable munitions" (Higgs 2006b, 65). By this measure, GNP in 1945 was just 5 percent higher than in 1939, hardly evidence of true economic recovery. By 1949, GNP had improved 47.5 percent over 1939, as many of the wartime controls were removed, military spending slacked off, and returning soldiers added their production back to the economy. So even as World War II might have increased traditional measures such as GNP and employment, it is not at all clear that it led to a revitalization of the private sector in the process any more than did the New Deal spending. The ultimate recovery of the private sector in the US economy took place when the federal government stopped trying to cure the disease. The interventions circa 1939 were largely retained, but by the late 1940s people regained confidence. They knew what to expect in terms of the rules that would apply in the future. The Hoover-Roosevelt-wartime chaos seemed to have passed. Real prosperity finally returned.

In addition to rules uncertainty, another simple and powerful explanation for the protracted high unemployment rates of the 1930s is, as most fully developed by Vedder and Gallaway 1993, a work sometimes unfairly neglected, the various actions and policies that prevented wages from falling in the face of the monetary and price deflation. During his presidency, Hoover put into practice his 1920-21 idea of trying to forestall wage cuts during a recession. Now as president, he did so by gathering major industrialists at the White House and pressuring them to maintain wages and thereby purchasing power. One can also view the Smoot-Hawley Tariff as an attempt to protect labor in industries facing lower-wage competition from abroad. These Hoover-years policies certainly help to explain the quickly rising unemployment rates documented earlier.<sup>12</sup> Forward into the

Roosevelt years, the NRA policies as well as other New Deal legislation such as the Wagner Act, continued the Hoover tradition of government activism to support wages, with unemployment rates lingering in the 15 to 20 percent range for most of the 1930s.<sup>13</sup> When combined with Higgs' work, the wage/price-interventionism argument (which is also offered by Cole and Ohanian 2004) fits the judgment that there was very little actual recovery, and that the cause of the ongoing malaise was a combination of obstruction and rules uncertainty, created by a variety of government policies and the ideological tone of official speech and action.

Here, regarding the genuine recovery and the ideological shift coming out of the Second World War, I might add a historical speculation of my own, a speculation that surely is not both sound and original with me: Despite its uneasy and pragmatic alliance with the Soviets' communist regime, the war against Germany's national socialist regime, Italy's fascist regime, and Japan's totalitarian regime was mobilized and waged to a significant degree as a contest of freedom versus tyranny, despotism, and collectivism. It was not only the passing of Roosevelt in 1945, but also the ideological drama of the war that likely quieted American trends toward statism and rehabilitated rules certainty, confidence, private investment, and American economic vibrancy.

## Conclusion

Gauti Eggertsson characterizes the period 1933-1937 as an economic recovery, but such characterization does violence to the idea of recovery as return to economic normalcy. The construction of a sophisticated model and the apparent need to demonstrate the efficacy of Roosevelt's purportedly different-than-Hoover's policies eclipsed the duty to mind the historical truth of the matters in question. The paper is full of technical wizardry but terribly wrongheaded in its presuppositions, claims of relevance, economic analysis, and policy implications. Two of the "policy dogmas" that Eggertsson attributes to Hoover were neither apt descriptions of Hoover's own views and actions, nor were they "almost universally accepted." Hoover was no devotee of small government and was not afraid of budget deficits, at least not moderate ones. Yes, he was committed to

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12. In a paper forthcoming in the *Journal of Economic Theory*, Ohanian (2009) offers both extensive documentary evidence from Hoover's memoirs and the historical data to argue that Hoover's interventions in the manufacturing labor market "substantially depressed the economy, reducing aggregate output and hours worked by about 20 percent" (p. 3). He bluntly claims that Hoover was responsible for starting the Great Depression.

13. See the excellent discussion of the effects of New Deal labor laws in Powell (2003, ch. 14).

gold convertibility, but contrary to Eggertsson's presupposition, abandoning "the" gold standard was not necessary to avoiding the monetary collapse, nor to curing it. Roosevelt had dogmas of his own, but within them came a hodge-podge of ad hoc policies that could hardly be called a coherent regime and, in any case, was not even focused on recovery per se. Finally, the statism that ramped up under Hoover and grew to virulence under Roosevelt was the basic reason recovery never came during Roosevelt's lifetime. True recovery did not take place until after World War II when certainty of the rules, albeit now much more statist, was restored.

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# The Policy Views of American Economic Association Members: The Results of a New Survey

Robert Whaples<sup>1</sup>

## ABSTRACT

This article presents the results of a 2007 policy-views survey of a random sample of members of the American Economic Association. The new survey contains questions about many policy issues not treated by previous surveys. The questions treat issues such as trade restrictions, social insurance for those put out of work by international competition, genetically modified foods, curbside recycling, health insurance (several questions), medical malpractice, barriers to entering the medical profession, organ donations, unhealthy foods, mortgage deductions, taxing internet sales, Wal-Mart, casinos, ethanol subsidies, and inflation targeting. Additional questions treat the relationship between economic growth, happiness, and well-being, whether the typical American consumes too much, works too much, saves too little, and live in a house that is too large.

The results show disagreement on many issues but evidence of considerable agreement on others, including a consensus that the benefits of Wal-Mart stores typically outweigh their costs, that Americans save too little and that economic growth in developed countries increases well being. The survey finds a consensus in favor of eliminating trade barriers, eliminating or cutting ethanol subsidies, allowing payments to organ donors, and against requiring employers to provide health insurance.

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The article opens with some reflections about why we care whether economists reach a conclusion, and methods for determining whether a consensus has been reached.

## **Why We Care Whether Economists Reach a Conclusion**

Unlike this article, most of the articles published in the “Do Economists Reach a Conclusion?” section of this journal pin-point a policy issue, such as drug prohibition, sports subsidies, postal competition, occupational licensing, or Food and Drug Administration policy, and assess the published judgments of professional economists to see whether a policy-reform conclusion has been reached. This approach has been very fruitful, finding considerable consensus on many issues, which may be news to economists as well as non-economists.

The other approach, represented by this article, has been to tap “economists in general” on a range of issues by sending a questionnaire to a representative cross-section of economists, very often a sample of American Economic Association members from the United States.

Neither approach is foolproof in determining whether economists reach a conclusion. The survey of published judgments suffers from possible bias from either the surveyor or the attractiveness to researchers of particular research questions. Also, many research articles don’t explicitly state policy conclusions and many findings come with caveats and qualifications, making it difficult to assess consensus. Finally, the approach is hard work, requiring months of reading to assess the research on a particular issue.

The questionnaire technique can be criticized for relying on respondents who are insufficiently versed in a particular topic, tapping the profession’s gut feelings rather than the informed conclusions of acknowledged experts. There are also possible biases in sampling methods—Is AEA membership representative of U.S. economists as a whole?—and in response. Particular survey questions may also be criticized for being too simple to capture the nuances of the issue and the policy options available, or for using ambiguous or loaded wording.

Despite possible problems, both approaches have the advantage of cutting through the fog to provide a focused understanding of the bottom line—whether economists reach a conclusion on a particular issue. Identifying the existence or lack of consensus can be immensely important because doing so makes it harder for elected officials, partisans, and media to claim there is strong professional support for a position when there isn’t, or that there isn’t when there is.

Cultures often look for focal authorities—shamans, priests, prophets, seers, policy wonks, or experts. That is one reason that surveys of economists are eagerly read by the profession, the press and the public—as demonstrated by the fact that my 2006 article in *The Economists' Voice*, “Do Economists Agree on Anything?,” became the most downloaded article in the history of that journal, with 3105 downloads as of July 5, 2009.

Surveys of professional opinion have the potential to make democracy work better. They make it harder for politicians to perpetrate policies that the profession has rejected as flawed, and for the media to obfuscate about professional opinion. And economists do seem to be skeptical of the political process. Davis and Figgins (2009) find that economists of all political stripes lean toward disagreeing with the proposition that “the typical bill passed by the U.S. Congress and signed into law generates a positive net social benefit for society.” They also find considerable disagreement with the conclusion that “typically, most media outlets in the United States communicate economic issues accurately and truthfully to their viewers and readers.”

Whenever an economist expresses a conclusion that flies in the face of popular opinion and political orthodoxy, credibility is enhanced if it can be shown that the conclusion reflects a professional consensus.<sup>2</sup>

## The New AEA Survey

The survey was mailed to 325 randomly-selected, Ph.D.-bearing members of the American Economics Association (AEA) in Spring 2007. Because the survey is intended to reflect policies regarding the American economy, respondents all possessed undergraduate and graduate degrees from U.S. institutions. The response rate was 41.8 percent (136 surveys)—somewhat higher than the 40.0 percent response rate in Whaples (2006) and higher than reported in other surveys of general AEA membership.<sup>3</sup>

Among the survey's respondents, 13.2% are female<sup>4</sup>; 68.1% identify their main employer as academic, 8.1% as government, and 21.5% as “other.”<sup>5</sup> The

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2. Recent broad-range policy-view surveys of U.S. economists include Fuller and Geide-Stevenson (2003), Whaples (1996), Whaples and Heckelman, (2005), Whaples (2006), and Klein and Stern (2007).

3. Response rates among AEA member in recent surveys include 34.4% in Alston, Kearn and Vaughan (1992), 30.8% in Fuller and Geide-Stevenson (2003), 36.3% in Whaples and Heckelman (2005), and 26.6% in Klein and Stern (2005). This survey was a bit shorter than most, which may explain the higher reply rate.

4. Based on first names, women made up 16.3% of the sample to whom the surveys were mailed.

5. “Other” includes four who listed themselves as retired.

median year of birth is 1955, with one-quarter born before 1943, and one-quarter after 1967.

Tables 1 through 4 present the survey and the basic results.<sup>6</sup>

**Table 1: Agreement and Disagreement among AEA Members on Policy Questions**

PLEASE NOTE WHETHER YOU AGREE OR DISAGREE WITH THESE PROPOSTIONS USING THE 5-POINT SCALE BELOW. (If you have no opinion, leave the blank empty.)

	STRONGLY DISAGREE	DISAGREE	NEUTRAL	AGREE	STRONGLY AGREE	MEAN
	1	2	3	4	5	
1. The U.S. should eliminate remaining tariffs and other barriers to trade. (N = 132)	1.5%	8.3%	6.8%	46.2%	37.1%	4.09
2. The U.S. should increase benefits to workers who lose their jobs due to international competition. (N = 130)	5.4%	22.3%	20.8%	34.6%	16.9%	3.35
3. The U.S. should ban genetically modified crops. (N = 129)	48.8%	33.3%	10.9%	3.9%	3.1%	1.79
4. Laws mandating municipal curbside recycling should be eliminated. (N = 127)	26.8%	31.5%	16.5%	15.0%	10.2%	2.50
5. Employers in the U.S. should be required to provide health insurance to their full-time employees. (N = 129)	20.9%	41.1%	13.2%	17.8%	7.0%	2.49
6. Employers in the U.S. should be required to provide health insurance to ALL their employees. (N = 127)	31.5%	40.2%	7.9%	16.5%	3.9%	2.21
7. Employers in the U.S. should be taxed if their employee health insurance expenditures fall below a certain threshold. (N = 127)	33.9%	29.1%	20.5%	12.6%	3.9%	2.24
8. State governments in the U.S. should eliminate mandates about what health insurance must cover. (N = 125)	11.2%	34.4%	12.0%	27.2%	15.2%	3.01

6. Electoral polls often report the margin of error, because a plus/minus gap of a few percent can swing the likely outcome. Margin of error is less relevant in professional surveys where the issue is consensus versus lack of consensus. If about 80 percent of those surveyed agree with a proposition, the interpretation won't change much if the outcomes is, say, 87%, or 73%. With the sample sizes below of roughly 130, an estimate of 50 percent has a margin of error for a 95 percent confidence interval of plus/minus 8.6%; the margin is plus/minus 6.9% for an estimate of 80%.

POLICY VIEWS OF THE AEA

	<b>STRONGLY DISAGREE</b>	<b>DISAGREE</b>	<b>NEUTRAL</b>	<b>AGREE</b>	<b>STRONGLY AGREE</b>	<b>MEAN</b>
	<b>1</b>	<b>2</b>	<b>3</b>	<b>4</b>	<b>5</b>	
<b>9. The U.S. should place more stringent caps on medical malpractice awards. (N = 128)</b>	13.3%	18.8%	16.4%	39.1%	12.5%	3.19
<b>10. Barriers to entering the medical profession in the U.S. should be reduced. (N = 124)</b>	3.2%	21.8%	11.3%	38.7%	25.0%	3.60
<b>11. The U.S. should allow payments to organ donors and their families. (N = 128)</b>	4.7%	10.9%	14.1%	45.3%	25.0%	3.75
<b>12. The U.S. should impose taxes on unhealthy foods. (N = 130)</b>	27.7%	33.1%	14.6%	21.5%	3.1%	2.39
<b>13. The U.S. should change the income tax code so that health insurance benefits are taxed the same as income. (N = 126)</b>	15.1%	29.4%	13.5%	30.2%	11.9%	2.94
<b>14. The U.S. should change the income tax code to eliminate the mortgage interest deduction. (N = 129)</b>	12.4%	25.6%	16.3%	34.1%	11.6%	3.07
<b>15. The U.S. should exempt internet sales from taxation. (N = 129)</b>	17.8%	38.8%	18.6%	16.3%	8.5%	2.59
<b>16. The Public Company Accounting Reform and Investor Protection Act of 2002 (Sarbanes-Oxley) should be repealed. (N = 116)</b>	17.2%	30.2%	27.6%	19.8%	5.2%	2.66
<b>17. A Wal-Mart store typically generates more benefits to society than costs. (N = 130)</b>	2.3%	12.3%	13.1%	41.5%	30.8%	3.86
<b>18. A casino typically generates more benefits to society than costs. (N = 129)</b>	17.1%	35.7%	30.2%	13.2%	3.9%	2.51
<b>19. Economic growth in developed countries like the U.S. leads to greater levels of happiness. (N = 126)</b>	3.2%	15.9%	32.5%	38.1%	10.3%	3.37
<b>20. Economic growth in developed countries like the U.S. leads to greater levels of well-being. (N = 132)</b>	0%	2.3%	9.8%	49.2%	38.6%	4.24

**Table 2: Ethanol**

**Government subsidies on ethanol in the U.S. should be (N = 120)**

55.0%	a. eliminated
10.8%	b. reduced a lot
12.5%	c. reduced somewhat
11.7%	d. kept about the same
9.2%	e. increased somewhat
0.8%	f. increased a lot

**Table 3: The Fed’s Inflation Target**

**If the Fed were to target the inflation rate, its target rate should be \_\_\_\_\_ per year (give a number or a range). (N = 110)**

	Lower bound	Upper bound
<b>25th percentile</b>	1%	2%
<b>Median</b>	2%	2%
<b>75th percentile</b>	2%	3%
<b>Average</b>	1.69%	2.45%

**Table 4: Work, Consumption, Housing and Saving**

**The typical American works (N = 125)**

36.0%	a. too much
3.2%	b. too little
60.8%	c. neither too much nor too little

**The typical American consumes (N = 123)**

49.6%	a. too much
0%	b. too little
50.4%	c. neither too much nor too little

**The average size of houses in the U.S. is (N = 120)**

31.7%	a. too large
0%	b. too small
68.3%	c. neither too large nor too small

**The typical American saves (N = 128)**

0%	a. too much
69.5%	b. too little
30.5%	c. neither too much nor too little

## Some Highlights

**Trade.** The economics profession continues to show a consensus in favor of unfettered international trade, as 83 percent agree and only 10 percent disagree that the United States should eliminate remaining tariffs and other barriers.<sup>7</sup> This agreement need not imply a completely laissez-faire policy toward trade, however, as a majority (52 percent) favor the idea of increasing benefits to workers who lose their jobs due to international competition.

**Environmental Issues.** Economists are almost unanimous in agreeing that the U.S. should *not* ban genetically modified crops and are very skeptical of the economic and environmental benefits of the federal subsidy to ethanol (51 cents per gallon at the time of the survey). As Table 2 shows, the majority, 55 percent, favor *eliminating* government subsidies to ethanol production and another 23 percent favor reducing the subsidy, while only 12 percent endorse the current level and 10 percent favor a hike. The majority (58 percent) reject repealing laws mandating municipal curbside recycling, while a quarter favor the idea.

**Health Issues.** With nearly one-sixth of GDP devoted to health care and concerns that many Americans lack adequate access to health care, reining in the increase in health care costs and making insurance easier to obtain are high on the political agenda.

One oft-discussed proposal is to require employers to provide health insurance to *all* their employees. Economists roundly reject this idea, with 72 percent against and only 16 percent in favor—probably because such a law would add a fixed cost to employment thereby reducing the availability of part-time and low-wage jobs and would put downward pressure on the wages of these groups. Economists also reject the idea of requiring employers to provide health insurance to their *full-time* workers, with 61 percent opposed and 25 percent in favor. Another idea (recently enacted in Maryland, but struck down in court) is taxing employers if their employee health insurance expenditures fall below a certain threshold. Economists reject this idea too—with 63 percent opposed and only 17 percent in favor.

The next three questions concern proposals aimed at reducing health care costs across the board. Some argue that state government mandates about what insurance must cover are driven by special interests and extend the list of required coverage too broadly and thereby drive up costs. However, economists are evenly

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7. Whaples (2006) found slightly higher agreement with this proposition, the only question in this survey that is repeated from previous surveys.

split on the idea of having state governments *eliminate* mandates about what health insurance must cover. Economists lean in favor of placing more stringent caps on medical malpractice awards—with 52 percent in favor and 32 percent opposed. They strongly favor a policy that has received scant political or media consideration—reducing barriers to entering the medical profession—by roughly 2.5 to 1.

Previous surveys have demonstrated that economists often favor using market-based solutions to address a range of problems—such as giving parents educational vouchers to spend at competing schools (Whaples 2006). Another market-based idea is to tackle shortages of human body organs by allowing payments to organ donors and their families. As Table 1 reports, this idea is favored by over 70 percent of economists, with only 16 percent opposed. Finally, the majority (61 percent) of economists are skeptical of attacking health problems by imposing taxes on unhealthy foods.

**Taxes.** Health insurance receives preferential tax treatment. Should the U.S. change the income tax code so that health insurance benefits are taxed the same as income? Economists lean ever so slightly against this idea—44 percent oppose it, while 42 percent favor it. Should the U.S. amend the income tax code to eliminate the mortgage interest deduction? Again, economists are almost evenly split. Should internet sales be exempted from taxation? The majority of economists (57 percent) say “no,” while less than a quarter favor special treatment of internet-based sales.

**Sarbox:** In 2002, in the wake of corporate fraud at companies such as Enron and WorldCom, Congress enacted the Public Company Accounting Reform and Investor Protection Act—better known as the Sarbanes-Oxley Act. Proponents argue that the act adds important investor protections, but critics have called it an expensive, “intrusive, circulatory and duplicative grab-bag of rules” (Macey 2007). After five years of seeing the law in action, about one-quarter of the profession advocated repealing it but almost half rejected this idea, with the remainder sitting on the fence.

**Wal-Mart and Casinos:** With Wal-Mart’s annual revenue topping \$350 billion and sales exceeding those of the next five largest retailers combined, many grassroots critics and policy makers have become concerned about its labor market policies and overall economic impact. Fans point to its price reductions, which force competitors to respond in kind—yielding significant gains to consumers, especially those from lower income groups, and even to consumers who decline to shop there. Does a Wal-Mart store typically generate more benefits to society than costs? The vast majority of economists, it appears, are fans of Wal-Mart. Fully 72 percent conclude that the benefits of Wal-Mart stores generally outweigh the costs. Fewer than 15 percent disagree. The same cannot be said of

casinos, whose impacts on crime have recently been debated in the pages of this journal (see Walker 2008 and ensuing discussion). Only 17 percent of economists conclude that a casino typically generates more benefits to society than costs, while the majority (53 percent) disagrees.

**The Fed:** In recent decades, several central banks around the world have adopted explicit inflation targets. If the U.S. central bank, the Federal Reserve, were to target the inflation rate what range should it pick? As Table 3 reports, most economists would select a narrow range around 2 percent.<sup>8</sup>

**Individual Decisions and the Pursuit of Happiness:** Do Americans make wise choices? Economists are often slower than others to judge others' choices, realizing that tastes and aspirations vary considerably from person to person and that it is difficult for an outsider to assess the tradeoffs that someone else faces. Yet, individual shortcomings, cultural pressures, institutional arrangements, and governmental policies may conspire to yield sub-optimal outcomes for the most basic of economic choices. Table 4 reports economists' responses to a series of questions asking whether the typical American consumes, works and saves too much, too little or neither—and about the size of the typical American house.

Most economists (61 percent) conclude that Americans (who now work noticeably more than Europeans) work about the right amount, although more than a third believe that they work too much. By a razor-thin edge, the majority conclude that the typical American consumes about the right amount—but almost as many argue that consumption is too high. Housing ranks high on the list of consumption items and most economists (69 percent) find the average size of houses in the U.S. to be about right, with the rest contending that house sizes are too large (perhaps because of tax breaks, such as the mortgage interest deduction which 46 percent believe should be eliminated). If Americans' decisions *are* off the mark, many economists believe that the problem lies in saving behavior. Fully 70 percent conclude that the typical American saves too little, a conclusion that may be driven by the recent drop in the personal savings rate into single digits (and temporarily into negative territory) —a phenomenon which has been partially attributed to an array of problematic public policies.

Official statistics, such as average income per capita, suggest that from year to year and decade to decade the American economy has been delivering more and more to the typical American. But is more better? The survey closes with this

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8. The survey included a few questions that I have chosen to omit from this paper because of low response rates. One of these questions asked how accurately the Consumer Price Index measures inflation. The median and modal response is that the CPI overstates the true inflation rate by about 1 percentage point per year (N = 89).

fundamental question. Does economic growth in developed countries like the U.S. actually lead to greater levels of happiness? Broad social surveys show a roughly constant level of self-reported happiness among Americans from 1972 to 2005 (Pew Research Center 2006, 2) —a period in which average real per capita incomes roughly doubled. Despite this finding, almost half the economists surveyed conclude that economic growth in rich countries *does* lead to greater levels of happiness, with about a third neutral and almost one in six rejecting the idea. However, “happiness” does not seem to mean the same thing as “well being”—and the vast majority of economists (88 percent) are optimistic that continued economic growth in economies like the U.S. does yield ever greater levels of well being.

### **A Glass Half Full**

From a classical liberal viewpoint, one may regard the views of economists as a glass half full or half empty. The half-full attitude may be associated with Bryan Caplan (2006, 2008), David Henderson (2008), and others, who stress the fact that, relative to scholars in other disciplines and to the public at large, economists show greater support for liberalization. The half-empty attitude may be associated with Daniel Klein, who emphasizes that economists aren’t much different than others, that survey results often show a lack of single-peakedness in the distribution of economists’ policy positions, possibly reflecting ideological cleavages (Klein and Dompe 2007, 131f), and that economists show substantially less policy-view consensus than do sociologists, anthropologists, historians, political scientists, or philosophers (Klein and Stern 2005, 283f).

The respondents seem to reflect the broader viewpoint of modern Americans that more is better. And this is probably why so many in the public pay attention when economists offer their opinions. Much of the public believes that following the advice of the economics profession can continue to improve our collective well-being—so they’ll be interested to know that economists recommend achieving this goal by eliminating trade barriers, allowing Wal-Mart’s to open, rejecting mandates that employers provide health insurance to all their workers, allowing payments to organ donors, and slashing ethanol subsidies, for example.

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## Economic Notes from the Underground

*EJW editorial note:* The following four articles are introspective essays about what Timur Kuran has called “preference falsification.” We are grateful to Peter Gordon for his help in organizing the symposium and to Timur Kuran for his input.

# Confession of an Economist: Writing to Impress Rather than Inform

David R. Hakes<sup>1</sup>

### ABSTRACT

Think back to your first years in graduate school. The most mathematically complex papers required a great deal of time and effort to read. The papers were written as if to a private club, and we felt proud when we successfully entered the club. Although I copied the style of these overly complex and often poorly written papers in my first few research attempts, I grew out of it quite quickly. I didn't do so on my own. I was lucky to be surrounded by mature confident researchers at my first academic appointment. They taught me that if you are confident in your research you will write to include, not exclude. You will write to inform, not impress. It is with apologies to my research and writing mentors that I report the following events.

The preference falsification in which I engaged was to intentionally take a simple clear research paper and make it so complex and obscure that it successfully impressed referees. That is, I wrote a paper to impress rather than

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inform—a violation of my most closely held beliefs regarding the proper intent of research. I often suspected that many papers I read were intentionally complex and obscure, and now I am part of the conspiracy.

A colleague presented a fairly complex paper on how firms might use warranties to extract rent from certain users of their products. No one in the audience seemed to follow the argument. Because I found the argument to be perfectly clear, I repeatedly defended the author and I was able to bring the audience to an understanding of the paper. The author was so pleased that I was able to understand his work and explain it to others that he asked me if I was willing to coauthor the paper with him. I said I would be delighted.

I immersed myself in the literature for a few of months so that I could more precisely fit our contribution into the existing literature. We managed to reduce the equations in the paper to six. At this stage the paper was perfectly clear and was written at a level so that it could reach a broad audience. When we submitted the paper to risk, uncertainty, and insurance journals, the referees responded that the results were self-evident. After some degree of frustration, my coauthor suggested that the problem with the paper might be that we had made the argument too easy to follow, and thus referees and editors were not sufficiently impressed. He said that he could make the paper more impressive by generalizing the model. While making the same point as the original paper, the new paper would be more mathematically elegant, and it would become absolutely impenetrable to most readers. The resulting paper had fifteen equations, two propositions and proofs, dozens of additional mathematical expressions, and a mathematical appendix containing nineteen equations and even more mathematical expressions. I personally could no longer understand the paper and I could not possibly present the paper alone.

The paper was published in the first journal to which we submitted. It took two years to receive one referee report. The journal sent it out to a total of seven referees, but only one was able to write a report on it. Apparently he was sufficiently impressed. While the audience for the original version of the paper was broad, the audience for the published version of the paper has been reduced to a very narrow set of specialists and mathematicians. Even for mathematicians, the paper may no longer pass a cost-benefit test. That is, the time and effort necessary to read the paper may exceed the benefits received from reading it. I am now part of the conspiracy to intentionally make simple ideas obscure and complex.

The story does not end here. A year later at an economics conference I sat on a panel composed of editors of economics journals. The session was charged with instructing young professors on how to get published. Because I was involved in a number of other sessions, I paid little attention to the names and affiliations of my colleagues on the panel. When it was my turn to speak, along

with other advice, I told the story described above. When the next panelist was introduced, I was embarrassed to see that he was the editor of the journal that had published our incomprehensible paper. To reduce the level of embarrassment for both of us, I explained that our paper was handled through the U.S. editorial office of his journal, not the U.K. office which he manages. As an aside, to demonstrate just how small the world has become, we later discovered that my eldest daughter had studied in the editor's department while in the United Kingdom during the previous semester.

In conclusion, I wish I could promise that in the future I will always write to inform rather than to impress. But although confession may be good for the soul, it does not inoculate us from future sin. If in the future a referee or an editor suggests that I “generalize the model” or “make the model dynamic” when I feel that the change is an unnecessary complication which will likely cloud the issue rather than illuminate it, I will probably do as they requested rather than fight for clarity. That situation aside, I plan to redouble my efforts to write to inform rather than impress, to advise young researchers to do the same, and to be careful when criticizing referees and editors because they may be sitting next to me.

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## Economic Notes from the Underground

# Preference Falsification in Teaching

Stephen Kinsella<sup>1</sup>

### ABSTRACT

Writing a course description is hard. In Irish universities (and elsewhere), the individual lecturer must hew to a course description designed by either the head of department, or a committee of senior colleagues. The writer of this course description must contend with a host of issues; what concepts are to be included? What should be discarded? At what level should the material challenge the students? All the while, the writer must consider those who came before her in writing the course description, and those who will come after. The outcome of this process is, naturally, a course shaped around a textbook.

In economics, one finds young professors teaching from the textbooks they squinted over not so long ago, replaying elements of their favorite lectures. Young professors want to stick rigorously to the course description, given to them by the Head of Department, because they wish to be good colleagues, not make any waves, and not make any presumptions with the students, who might damage their teaching evaluations. In Irish universities, changing the content of the course by more than 25% requires approval at the faculty level. The young professors must hew to the textbooks for guidance, because the course description is deliberately light on specifics. Often only keywords are present for guidance.

Because most textbooks are written from similar methodological standpoints, (E.g. in macroeconomics: Barro and Sala-i Martin 2003, Blanchard

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2005, Abel et al. 2007, DeLong 2002), the course content is, perforce, of a similar orientation. The course in macroeconomics, say, begins from the production function, adds the marginal conditions for productivities of capital and labour, and continues on to build a micro-founded macroeconomic model, where all the individual markets are in equilibrium, all the consumers maximize their consumption and saving profiles over time, and business cycles are simply the resultant outcomes of stochastic shocks to technology, labour, capital, and so forth.

Teaching exclusively from a neoclassical perspective is distressing for one who tends toward less conventional thought. Say one is a post-Keynesian. If one believes in the inability of unregulated markets to satisfy effective demand, if one does not believe the production function is a reasonable way of modeling economic activity, if one endorses the influences of fundamental uncertainty, asymmetric information, and psychological biases as underpinning economic decisions, then to teach a theory which assumes away these features of reality is contradictory. Here I'm assuming a polar position for the sake of argument: of course one can alloy the textbook with supplementary readings, spend time critiquing the model, or simply disregard the course description, and teach what one wants to. But let's say for the moment the young professor has to choose between rigorously upholding her personal views, or teaching verbatim out of a standard textbook. I should confess at this stage that my personal orientation would be post-Keynesian.

In this world, if one's preference is for a non-equilibrium macroeconomics, and one teaches equilibrium macroeconomics, then this activity can be referred to as preference falsification, in the sense of Kuran (1987, 54).

Preference falsification can occur in teaching when a personal intellectual compromise takes place with respect to the content of a class being taught. The professor publicly espouses a theory or mode of storytelling whose policy prescriptions are, in some cases, orthogonal to their private and professional views or attitudes.

## **Theory of Preference Falsification**

The theory of preference falsification develops a framework in which individuals sometimes refrain from displaying privately held preferences or beliefs in order to appease, or curry favor with, a power group (Kuran 1987, 1990). Say the society in question is one's fellow economists, and the power group in question is the academic establishment.

Kuran's theory in its simple form posits a choice function, where the individual chooses between several options. In Kuran's original 1987 exposition, these options were different policies to be enacted by a government. Here, the individual must choose which content to teach her students. Kuran formulates a choice involving three factors.

**Direct benefit:**

The direct benefit accruing to the individual of espousing a certain view. In public policy as in economic culture, the rub is that one's choice of espousal has very little effect on overall outcomes. That's when falsification becomes common. One cannot remake the culture that students and colleagues will otherwise swim in. Meanwhile, the direct benefits to the individual from teaching from the standard textbooks are obvious: the learning curve is flatter for the students, the level of preparation required for the instructor is lessened, the coverage of material is at the same level throughout, the exposition has been tested on many audiences and is therefore more polished, and the very existence of a textbook will reduce student uncertainty as to the content of the course.

**Reputation:**

The individual's reputation will suffer by not adopting the viewpoint of the powerful group. The influence of one's reputation changes the choice function of our agent. The individual must decide whether to adopt a public preference. Several authors point to the marginalization of economists holding unconventional viewpoints in different teaching and research contexts (Kaul 2008, 134-138; Fine 1999, 182), so it may be assumed that teaching unconventional economics will impact one's reputation.

**Integrity:**

Integrity is defined as the distance between one's private preferences and one's public preferences. The closer one's private and public preferences, the more integrity one retains, and correspondingly more utility from this factor. But how much value does integrity have to the individual?

Kuran explains that individual must make his or her decision comparing the expected consequences of public support for an option which may be far from his or her private view.

What has Kuran's model to do with teaching? Say an individual holds beliefs that the economy is not well characterised by a production function approach, or a growth-model approach, which mainstream textbooks by and large adopt. Then to profess that view in public is to falsify one's preferences, as the difference between the privately held view, and the publicly held view are sufficiently large as to cause some dissonance for the individual. This was my experience.

## My experience of preference falsification

A confessional piece such as this requires the enumeration of sins. It should be obvious by now that I am not a mainstream economist, or the situation would not have presented itself. My first impulse when I was told to teach a course on International Monetary Economics was to reach straight for the standard text in the area, namely, Mishkin (2004). I quickly found that this text, although clearly written, and accompanied by Powerpoint slides, notes, and other supporting materials, did not represent the type of content I wanted to teach the students. I chose a much more difficult and pedagogically challenging book by Godley & Lavoie (2007), written from the stock-flow consistent, post-Keynesian perspective. The choice of textbook, which created the skeleton of the course and decided much of its content, created several problems right away.

**Direct Benefit:** The first problem was the absence of a direct benefit to either myself or the students from an instructional point of view. From an intellectual point of view, the benefits of stock-flow consistent modeling were that these models do not rely on the neoclassical production function, which avers an economy's output is the result of a convex combination of labour and capital to produce efficient markets which clear through the equalisation of the marginal productivities of each input.

Using the production function approach, production is assumed to be instantaneous—there is no time in the neoclassical model. There can be no involuntary unemployment in this schema, and national income will be distributed amongst the workers and capitalists according to their share in the productive process, which delineates the division between wages and profits. 'Sticky' price neoclassical models can accommodate less than optimal outcomes, but even these simply result in a lower employment equilibrium, mainly because of search frictions and menu costs.

The other distinguishing feature of stock-flow consistent modeling is its conception of the firm as a separate actor in the economy, rather than a collection of individuals with capital and labour to rent. Faced with high levels of uncertainty regarding their future decisions, and based on expectations of future sales and profits, firms choose production, employment, and investment schedules in an environment that requires credit in the form of loans, stocks, and bonds. Each stock-flow consistent model assumes production and consumption unfold over time, thus, in every model, the need for finance, or an endogenous description of money, is both systemic and foundational. Firms, the government, and households cannot exist in this schema without a banking sector. The Godley and Lavoie conception of the economy is characterised by a movement of stocks and flows of economic variable through time (Godley & Lavoie 2006, 6). This incorporation of historical time and its characterization requires modeling by difference and differential equations, and does not, perforce, lend itself to analytic solutions. The methodology must be based upon simulation of economic fundamentals, 'stylised facts', and behavioural assumptions.

Despite the intellectual benefits I gained from teaching material I happened to believe in, from an instructional point of view, formalizing a series of stock-flow consistent models of ever greater complexity was a cumbersome process involving difference equations, which the students had to be taught before any macroeconomic facts could be presented to them.

The learning curve was quite steep for this type of instruction, and there are no learning resources accompanying the text, unlike Mishkin (2004). In a post-Keynesian class, one must spend time explaining the rationale for the choice of material as well as showing how it relates to other coursework done in the students' previous courses, which eats into time. The direct benefit does seem to slip away as all these costs are considered and incurred, week after week. The confusion many students felt was reflected in a below average evaluation of the course. The qualitative comments given by the students were concerned mainly with the uncertainty of examination questions, and impracticality of the modeling methodology.

**Reputation:** My 'reputation', so far as I can tell, has not been overly affected or damaged by the use of these non-standard models, though my colleagues openly questioned why I was teaching this course in a heterodox manner, and because I am at the start of my career, I don't have a reputation to damage. But then there is an opportunity cost in the alternate, conventional reputation I might otherwise cultivate. The one area where the choice of material has mattered is the scrutiny my course outlines are now given. The individual and highly detailed course *outline*, written by the individual lecturer, must 'match' with

the course description and the course's learning outcomes written by the department Head or committee.

**Integrity:** Have I shown integrity in terms of equating my personal beliefs and private actions? I don't think so. At every stage I was keen to show the students stock-flow consistent models in relation to more standard open economy models like IS-LM-BP and dynamic stochastic general equilibrium (DSGE) models, which served only to confuse the students. I confess that fully representing my own beliefs would mean disregarding standard approaches to the study of international macroeconomics, and so in this manner, I did not show integrity as a professional economist.

There, I said it. I feel better.

The key is the difference between the privately held belief of the individual and his or her publicly revealed belief. Kuran's model suggests that the distance between the two beliefs must be relatively large to outweigh the reputation and direct-benefit effects, which come from teaching in a neoclassical framework. The only route out of this impasse is the intelligent creation of a toothless course description.

## What to Do?

Let's return to our course description writer, who is writing a general layout of content for other junior members of faculty to teach from, trying to balance academic freedom with reasonable quality assurance as to academic content. As mentioned above, any course on monetary economics should contain a description of, say, theories of interest rate determination and international monetary institutions. Few card-carrying economists would dispute this inclusion. However, not so much agreement will be found on *which* theories of interest rate determination are to be taught, and which readings to describe those monetary institutions are to be assigned.

The judicious semantic easing of course description requirements allows the heterodox economist to reduce the distance between his or her privately held (and perhaps heterodox) views and the more public statements of economic 'truth' a professor must impart to his or her students. A simple nod to other schools of thought through secondary references in the course description allows some opening to teach different approaches. The professor can also explain to the student the difficulties involved with teaching alternative approaches, to increase student buy-in to the idea of the course. The power is in the course description writer's hands. At a stroke, she can open the door to alternative approaches, and with the same stroke, she can close them. Meanwhile, whatever the course

description writer may do, the actual professor can attempt to make her students aware of the difficulties she faces with regard to the delivery of the content, and in so doing, make partners of her students in this intellectual enterprise.

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## Economic Notes from the Underground

# Confessions of a College Non-Economizer

William Patrick Leonard<sup>1</sup>

### ABSTRACT

Lacking degrees in the discipline, I am at best a backdoor economist, having cobbled some coursework, self study and service as an associate editor of a journal. By academic training I am a higher education administrator, receiving my PhD in Educational Communication from the University of Pittsburgh in 1969. I have been a college officer for most of my career on commuter campuses. In a complementary attempt to stay in touch with students and grounded to the institution's primary purpose, I have taught principles and other lower division economics courses at three institutions over the last twenty plus years. But my confession is not really as an economist, but a college administrator. It speaks, not to the underbelly of economics, but to the inner working of many American colleges. While economics is taught in their classrooms, it is not necessarily practiced in their administrative offices.

As a dean, vice president and president, I have been constantly reminded of the fundamental relationship between revenue and institutional welfare. Except for the research university, most institutions rely on tuition and fees as primary sources of revenue. As an economics instructor, I have tried to prompt my students to consider some basic concepts—scarcity, efficiency and effectiveness—in addressing a range of textbook problems. Unconsciously, I have

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compartmentalized my administrative and my classroom priorities. I can not say that my role as an economics instructor has informed my administrative decisions. Operating costs, salaries, benefits, services and supplies increase predictably each year. As an academic administrator I have unconsciously relied upon two of the least internally controversial, least combative means of bringing revenue in-line with probable operating costs. I have consistently taken operating costs as a given. More often than not, I have sought to increase enrollment or tuition, usually both, rather than rigorously controlling expenses. With the few exceptions of painfully easing out moribund services and programs, I have ignored the academy's traditional production function—the mix of faculty, curriculum and infrastructure—that drives our operating cost increases each year. Yet as an instructor, I have consistently demanded my students demonstrate in their responses to trivial case problems their ability to recognize inefficient and ineffective use of scarce resources and to propose alternatives.

I suggest that increasing enrollment and tuition are less offensive to internal stakeholders—board, faculty and staff—than making better use of existing resources. As an administrator the economist lurking in my subconscious should prompt me to recognize underutilized resources manifested in twelve-hour teaching loads, the agrarian calendar, services duplicating those readily available in the community and underutilized classrooms and laboratories. I am keenly aware that altering faculty teaching loads, streamlining curriculum or making better use of infrastructure can be akin to moving a cemetery. The dead have many friends. While I sense sources of savings that could benefit the college's students and external stakeholders, I know that moves to promote efficiency and effectiveness will likely be seen as an assault on the values and livelihood of friends and colleagues. Faculty have devoted the majority of their adult lives to academic disciplines and industry traditions that are the foundations of a unique lifestyle. For example, try convincing a faculty to accept a larger teaching assignment. Larger teaching loads could be accommodated by eliminating faculty governance and reducing scholarship expectations. Students would be better served and the need for additional part or full time faculty reduced. Costs increases would be restrained. Or, try convincing a unit in the Student Affairs division at a commuter campus that many of their services and activities are not only underutilized and a financial drag on the whole institution, they are duplicated in the surrounding community. Both faculty and staff likely rejoin with carefully crafted rationales why their unit is vital to maintaining institutional quality. They are products of an academic tradition. Since it worked so well for them, it is the college's obligation to provide the same opportunities that produced their successes in perpetuity. To do otherwise would denigrate quality. Furthermore, spoken or not, anyone who proposes cost containment measures that undermine time honored academic

traditions effectively abandons her roots. She succumbs to the "dark side" and does so at some risk.

College faculties enjoy institutional primacy not found in other contemporary industries. Increasing teaching loads and eliminating faculty roles in institutional governance will attack deep-rooted traditions. With the bulk of institutional leaders the products of the faculty ranks, and many expressing a desire to return to the classroom, it is easy to see why there is little motivation from that quarter to upset the status quo. The familiar proclamation that "the faculty is the institution" expresses more than arrogance. This characterization fits many post-secondary institutions. Faculty participation in institutional governance or management by consensus often gives this constituency near veto power. Each year the *Chronicle of Higher Education* reports that a faculty vote of no confidence—or else public pressure from faculty—has brought an abrupt end to the tenure of presidents and lesser administrators. While these notices are infrequent, they are read often enough to remind senior administrators of the power of the faculty.

Enrollment and tuition has been a far less dangerous path. Pushing for higher enrollments gives the institution as a whole a sense of positive momentum. Increasing tuition has been received by students, parents, elected officials and the public at large as inevitable. There are objections from our external constituents but we remind them that quality must be maintained if not enhanced. The strategy has worked to date and our traditions remain largely unchanged.

In recent decades the higher education industry has received little, if any, countervailing pressure to the inefficient use of scarce college resources. The boards I have encountered tended to approve mission statements, strategic plans and operating budgets with minimal modification. Whether public or private, governing boards must respond to numerous internal and external constituencies. Too often, they defer to the institution's administration, with the faculty's underlying support, at the expense of their students and many external constituencies. Boards regularly review and approve operating budgets with a tuition increase embedded. In announcing the budget and tuition for the coming year, their press releases express regret. They cite a litany of price increases. But quality however vaguely defined must be maintained if not enhanced. Without strong external pressures for internal reform, my peers and I have been free to pursue far less controversial remedies—increased enrollment and tuition.

In reality my experience has not supported the validity of either tactic, yet I have continued to employ them. Increased enrollment merely strains existing resources, while providing justification for the next budget increase. Charging more tuition to merely continue to do things in the same way has not maintained or enhanced the quality of our graduates. I fear that "bigger" and "more" have

come to be ends in themselves and we have lost sight of the ultimate nature of institutional quality—student learning.

To date my peers and I have been able to ward off any serious external pressure for internal cost containment with a limited repertoire of convincing rationales. We have repeatedly and successfully reminded students and external constituencies that college graduates earn much more income in a lifetime than those without degree. Therefore, they should look at tuition and fees as an investment and not a cost. Quality is costly, and these annual increases are only meant to maintain if not enhance the value of the full collegiate experience. The pitch is perhaps no sleazier than those of other industries. But it is ironic how often the academic folk knock commercial promotionalism as though they are unsullied by such graft. This quality-maintenance argument has been remarkably successful over the last few decades. With the uncertainties of a long recession its success may not continue.

The corporate world may provide a hint of what is ultimately in store for colleges and their leaders who continue to resist taking control of their operating costs by more effectively and efficiently putting their scarce resources to work. Periodically, corporations and their executives have been forcefully reminded of the iron law of responsibility—when entities fail to use their power in a socially responsible way they will lose it. I am not optimistic that our resistance will end before popular backlash propels legislation that will force the requisite change and beyond. A few years ago the industry warned off U.S. Congressman Buck McKeon's (R-California) threat of a higher education price control measure. If No Child Left Behind legislation serves as a model for higher education reform, then we have much more to worry about than preserving traditions and lifestyle.

## About the Author



**William Patrick Leonard** earned his PhD from the University of Pittsburgh, an MBA from Loyola University, Chicago with a BS in Ed and MS in Ed from Indiana University. His administrative skills have been developed with participation in Carnegie-Mellon University, College Management Program, Educational Testing Service, Institutional Assessment Program, League for Innovation, Executive Leadership Institute and Council of Independent Colleges, Vocation and Mission Program. He has served as Associate Editor, *Business Library Review*, 1985 to 2001. He has published in *Economics of Education Review*, *Journal of Higher Education Administration*, *Journal of Academic Librarianship*, *Review of Education* and other

professional journals. He has served as a college officer at a number of public and private four year institutions. In March 2009, he assumed the position of Director of Academic Personnel and Student Services and now serves as Acting Dean at Solbridge International School of Business, Daejeon, Republic of Korea. His email is wpatleon@gmail.com.

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## Economic Notes from the Underground

# Economic Dissociative Identity Disorder: The Math Gamer, the Anti-Policy Econometrician and the Narrative Political Economist

Bruce L. Benson<sup>1</sup>

### ABSTRACT

According to Wikipedia, dissociative identity disorder (DID), as defined by the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders, is a psychiatric diagnosis that describes a condition in which a single person displays multiple identities or personalities (known as alter egos or alters), each with its own pattern of perceiving and interacting with the environment. One of the individuals who wrote an outside letter for my promotion to full professor several years ago stated that, until he saw my vita, he did not realize that the Bruce Benson who wrote *The Enterprise of Law* (1990) was the same Bruce Benson who wrote a number of spatial-price-theory papers. I obviously have had multiple economic personalities in my career, but I believe that my struggle with this disorder has been resolved. One of my colleagues recently sent me comments on a job candidate who had presented a macro general equilibrium model, stating that the candidate “had simplified ... [the] model so much as to strip out most of anything that was really interesting and important. It

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seemed to me that a good narrative economist (e.g., you or Doug North or Bill Fischel) could have developed a much better, less technical and less restricted, theory that was also much richer and generated far more and more interesting testable hypotheses.” I was very pleased when I saw myself described as a “good narrative economist,” even though I started my career dominated by a very different alter ego, seeking recognition among mainstream economists as a micro “theorist”. Much of the theory I produced was really just mathematical game playing, however, and while this work was being produced, another personality, a narrative political economist, was also trying to emerge. After the mathematical-game-player personality got sufficiently bored and disillusioned with the games and began to recede, yet another new personality emerged as I turned to econometrics. Still focusing on a desire to gain recognition through publication in mainstream journals, I continued to have two personalities. In recent years, however, treatments and medication have suppressed the variants of my “mainstream” personality. I am now pretty much of a single mind.

My mentor during my PhD program at Texas A&M University was Melvin Greenhut. I was learning that transportation costs, so often neglected, are very significant in the real world. Mel did some very valuable work during his career, although he also suffered some from DID. When he was working on his dissertation there were two main theories about location choice: a cost-minimization theory and a spatial competition theory. Mel combined the two, producing a richer, more fruitful theoretical framework. By the time I was working with him, he had become a leading contributor to the literatures on “spatial price theory.” The Samuelson/Takyama-Judge spatial version of perfect competition model had all production in spatially separated markets taking place at the same point in the markets, on the head of a pin, before being shipped. Mel’s demonstrations that competition in real world markets of spatially-linked firms does not look anything like Samuelson/Takyama-Judge or more familiar models of “perfect competition” had very serious implications for anti-trust and regulatory policy. Some of my early work focused on the policy implications of spatial-price-theory, but my most successful efforts, in the sense of prestige publications, were essentially mathematical game playing.

As spatial price theory developed, various contributors to the literature discovered that, by changing assumptions about functional forms or interdependent behavior, surprising results could be “predicted.” Whether the predictions or the assumptions had any real-world relevance mattered not, as these kinds of models were being accepted by the most prestigious journals. In fact, the first paper I submitted to a journal was accepted at the *American Economic Review*. Two spatial price theorists had demonstrated that two of the most widely used behavioral assumptions implied that price should rise with entry, but I explained that their results arose from their demand function assumption, and that a different demand

function with the same behavioral assumptions predicts falling prices with entry! The *AER* publication was exciting, of course, so my math-gaming personality emerged to dominate my other personality. I engaged in more math games as I strived for recognition among mainstream economists. For instance, I demonstrated that, theoretically, an import tariff can lead to falling prices in the domestic economy, depending on the assumptions about demand functions and behavioral strategies. This, as far as I can tell, is totally irrelevant from an economic perspective, but that did not matter, as it was published in the *Journal of International Economics*. Between 1980 and 1985 such game playing produced 15 “pure theory” journal articles, at least a dozen of which appeared in highly regarded field journals, including the *Journal of International Economics*, the *Journal of Industrial Economics*, the *Journal of Urban Economics*, the *American Journal of Agricultural Economics*, the *International Journal of Industrial Organization*, *Regional Science and Urban Economics*, as well as general journals including *Economic Inquiry* twice, the *Southern Economic Journal* three times, plus the *AER*. I also produced three “applied” theory articles, but they ended up in less prestigious journals.

I guess I was playing the model building game pretty well, but it was boring and by the mid-1980s, getting another math-game article accepted did not generate the rush that it had earlier. I also quickly became aware of the fact that mathematical theories can be manipulated to predict practically anything that the theorist wants to predict, simply by changing some assumptions. While I knew I was playing a game, many other theorists seemed to take their models much more seriously. Knowing the ease with which predictions can be altered, I found it disturbing when such “theorists” claimed that their mathematical models carried policy implications. I recall one theorist telling me that good policy analysis requires the “rigor” generated by mathematical modeling. When I pointed out that his model assumed away absolutely vital considerations for policy analysis, his answer was something like: “well, we can build those things into the theory but we need to start out with a more streamlined model.” Unfortunately, such things never seem to be built into models, and yet, policy recommendations based on such mathematically-complex but economically-simplistic model-building abound.

My math-gaming personality had made a large investment in learning the games it was playing, so despite the boredom and the growing disenchantment with “applied theory,” I continued to produce spatial-price-theory papers for a few more years, including two more publications in the *American Economic Review*, as well as others in journals similar to and including those listed above. Nonetheless, the flow of such papers diminished over time as dissatisfaction with mathematical gamesmanship mounted. I began to feel that I was wasting my time and my mind. The last gasp of my mathematical game player personality was published in the *Economic Journal* in 1991.

As the flow of mathematical papers declined I began to consider moving into empirical analysis of spatial models, in hope of exploring the relevance of these models. In fact, for a few years, multiple personalities fought for dominance. I published four spatial-pricing papers using experimental techniques in 1987 and 1988, for instance, culminating with a *Journal of Economic Behavior and Organization* article. At least a half dozen empirical papers testing spatial models were also published between 1990 and 1995, some in well-regarded mainstream places like *Economic Geography* and the *American Journal of Agricultural Economics*. These papers tested fundamental implications of spatial price theory (implications from theoretical work Mel Greenhut had produced in the 1950s), but these personalities were not strong enough to maintain themselves for very long.

Tests of spatial pricing models were not my only forays into applied econometric analysis. My disillusionment with theoretical policy analysis encouraged another personality to emerge and grow in strength: one dedicated to empirical policy analysis. I did not feel that my understanding of econometrics was sufficient to jump into empirical work, but fortunately, I got an opportunity to work with Ron Johnson on a couple of empirical papers. While Ron is not easy to work with, the results were very satisfying. At the time, the empirical literature on interstate tax competition was almost unanimous in the conclusion that tax competition had no significant impact on location choice and economic performance across states. Ron and I did not believe the result, and simply by considering lagged impacts of relative changes in state taxes, we were able to contradict this widely held conclusion. Showing that implications drawn from other studies are not robust was very exciting. Essentially, this was *anti-policy* analysis, in that the objective was to show that policy conclusions drawn by many applied econometrics studies were suspect.

The emerging empirical policy personality morphed into an anti-policy empirical analyst, which dominated my being by the early 1990s. Projects joint with David Rasmussen and other coauthors took on the drugs-cause-crime literature, as well as the drunk-driving literature that, at the time, unanimously contended that the best way to reduce alcohol-related traffic fatalities is to raise taxes on beer. All of my personalities like beer, so this was a direct attack on me. But the literature was also finding relationships that were much too large to be credible. We demonstrated that the large and significant negative relationship between beer taxes and traffic fatalities found in so many empirical studies is not robust, as these models suffer from serious missing variable bias.

Similarly, we produced significant empirical results implying that the drug-war reallocation of criminal-justice resources actually causes crime, since fewer resources are available for the control of non-drug crime. Simultaneously we cast significant doubt on the existence of a strong drug-use-causes-crime relationship.

We also offered evidence implying that drug enforcement efforts do not appear to respond to changes in crime rates, as they should if police really believe that drug enforcement is a powerful tool in fighting crime. Instead, our evidence indicates that police were reallocating their resources to drug control in order to take advantage of changes in asset-seizure law that allowed police agencies to keep what they confiscated.

While this anti-policy research was quite interesting, it also was very frustrating. First, it revealed that econometric analysis is just as easy to manipulate as mathematical model building. Results depend crucially on the variables included and the empirical model tested—OLS or simultaneous equations, cross-section or panel, log-log or log-linear, etc. Clearly, policy advocacy based on statistical analysis is just as suspect as policy implications drawn from math models.

Another source of frustration was the discovery of how much empirical policy analysis seems to be driven by a search for the “right” answers. As a result, it is extremely difficult to get empirical research published if it challenges the conventional wisdom in a literature. Editors send a submission to referees whose work is being challenged, and many look for ways to justify rejection of the paper. These justifications often lack any validity, but editors routinely reject the papers anyway. In mathematical game playing, on the other hand, I find, authors often see challenges as sources of increased citations and openings for additional papers. Model-builders who advocate policy based on their mathematical manipulations are similar to empirical policy analysts, however, in that they generally do not want their normatively-driven policy prescriptions to be attacked. The implications for my chosen profession are depressing: If you play economically irrelevant mathematical games you can publish in the high-prestige journals, but if you do applied analysis that questions the soundness of previous findings (i.e., anti-policy analysis), you are relegated to less prestigious journals—though, I’ve found that once published these studies tend to attract a lot of attention and citations, and before long other researchers start reaching the same conclusions. So, while I continued doing some of this work until the early part of the new century (my last empirical paper was published in 2007), the frustration with the publication process ultimately brought my empirical anti-policy personality to the same fate as my mathematical-gamer personality.

During the entire time that the mathematical-game-player and anti-policy statistical-manipulator personalities were prominent, another personality also existed: a narrative political economist. Many people who associate me with spatial price theory, or perhaps with anti-policy econometrics, may not even realize that I have this other alter ego.

My thesis advisors at the master’s and PhD levels both told me that I did not write well, but this did not defeat the narrative economist in me. While my friend,

Bob Higgs, the editor of *The Independent Review*, still complains about my long run-on sentences, Bob and a few other editors such as Murray Rothbard and Tom Borcherding, who actually believe that one of an editor's responsibility is to edit, have pushed me to become a better writer. It is probably a good thing since the narrative economist in me has finally risen to dominance, with a focus on political economy in the Adam Smith sense (as well as F. A. Hayek, James Buchanan, Douglas North, Ronald Coase, Armen Alchian, Gordon Tullock, etc.): essentially, comparative institutions analysis.

Actually, as the mathematical game player was developing, at least a part-time narrative-political-economist personality was also kicking in the womb of my scholarly being. Luckily, as part of my PhD program, I took public-choice courses from Randy Holcombe and Steve Pejovich, along with a property-rights workshop with Eric Furubotn. I actually began writing in these areas even before I finished graduate school. Some of this work was mathematical and later on, some was empirical, but for the most part, it has been narrative economics. Such work is difficult to publish in high-prestige journals, of course, but the success of my spatial price theorist personality meant that my department chairs and, for the most part, my colleagues, did not complain too much about the time I spent developing my narrative skills and working on topics that I find meaningful. I have certainly heard some complaints, of course, often alleging a "quality-quantity tradeoff" and that "quality" research is research published in prestigious journals—e.g., work I did in spatial price theory or anti-policy analysis. Actually, of course, many of the complaints simply reflect differences in perceptions of quality academic work. Several years ago a friend asked me to apply for a chair in his department. When he asked his colleagues on the search committee about the status of my application he was told that I had some very good publications but that my vita would be even stronger if I did not have all the papers outside the mainstream, most of which were political economy narratives with strong libertarian messages.

For the most part, my narratives are attempts to explain observed relationships between institutions and behavior, often as part of a comparative institutions project. The observations generally are provided by others. I consider, for instance, the evolution of institutions as reported by historians, attempting to explain both the causes and consequences of the observed changes. Similarly, I explore and draw on anthropologist's descriptions, current research, popular press reports, government reports, court and police documents, etc. Such sources may speak to international institutions, internet institutions, regulatory processes and outcomes, taxes and transfers, informal norms and practices, private alternatives to public institutions, and so on. As one alter ego was developing spatial-price-theory models during my early career, another wrote narratives about political

corruption, logrolling, bureaucratic behavior, interest group politics, regulatory transfers, private security markets, contracting out, arbitration, stateless law, and other topics that interested me, even if they did not interest mainstream editors. Many of my early papers of this kind were published in places like the *Journal of Libertarian Studies*, the *Cato Journal*, the *Journal of Contemporary Studies*, and other places outside the mainstream. Had those independent outlier journals not existed, the narrative political economist may have emerged much more slowly, so I am ever grateful to those who support and edit these journals.

By the mid-1980s I also started occasionally getting narrative papers accepted by mainstream economics journals, such as the *Southern Economic Journal*, the *Journal of Legal Studies*, and *Economic Inquiry*. This was encouraging, and the narrative effort grew as my mathematical side receded. 1988 saw narrative publications in *Public Choice*, the *International Review of Law and Economics*, and the *Journal of Institutional and Theoretical Economics*, and after that, efforts to produce mathematical papers dropped sharply.

The 1988 publications were some of the first to spinoff from my work associated with my 1990 book, *The Enterprise of Law: Justice Without the State*. Sometime in 1981 or 1982, I got a call from David Theroux, then President of the Pacific Research Institute. David, who later founded the Independent Institute, explained that he was putting together a book on gun control that Don Kates was editing for him. Kates, a modern-liberal lawyer who also happened to be anti-gun control, had gathered together other anti-gun control modern liberals to write papers, and David was looking for a classical liberal perspective to add into the mix. He told me that I was recommended by Murray Rothbard, who had just published a couple of my papers in the *Journal of Libertarian Studies*. David offered me \$1,000 to write a paper and I jumped at the opportunity, even though I did not know much about gun control. \$1,000 was a lot of money for someone not long out of graduate school whose wife was a student—she had worked to support us while I went through both my Master’s and Ph.D. programs and now it was my turn to support her while she pursued her degree. The plan was to attack the frequently made claim that “increases in guns cause increases in crime” by suggesting that increases in crime cause increases in guns—that is, growing levels of crime induce people to obtain guns to protect themselves. To support this argument, the narrator in me wanted to stress the failures of the criminal justice system and point out that increasing gun ownership was occurring simultaneously with increasing investments in all sorts of other private-sector crime control and protection activities. I found so much evidence of private sector crime prevention and protection, and of police misbehavior, that I had to call David to ask what I should do. I remember telling him that I already had about 100 pages typed, and suggesting that I could do one of two things: cut the 100 pages down so it could fit as a book

chapter or add even more material to it, and write a book. David replied, “Do both” and sent me a contract for a book on private sector involvement in legal processes and security services.

I soon learned that I actually knew very little about the topic that I was to address, and that many people including Murray Rothbard had already given the subject a great deal of thought. Nonetheless, several drafts and years later *The Enterprise of Law* was published in 1990. In the process, my narrative economist personality was introduced to Hayek, then Mises, Kirzner, and other Austrian economists. This alter ego also developed a new understanding and appreciation of Coase, Alchian, Demsetz, North, Williamson, Cheung, and a number of other brilliant narrative scholars. Several papers on the topics addressed in the book were also written and published in journals over this period. All of this work is narrative political economics.

While the narrative work related to *The Enterprise of Law* has not been published in high-prestige journals as frequently as my spatial price theory papers were, it has attracted much more attention, inside and outside economics. The book itself has been used in law-school, criminology, and economics classes, translated into Spanish and soon Italian, and generated large numbers of citations in both economics and law journals. One of the many papers from this project, “The Evolution of Commercial Law,” was rejected by several journals where people continue to tell me it should have been published. Finally, it was accepted by the *Southern Economic Journal*. Now it probably is my most cited article. It was awarded the Georgescu-Roegen Prize as the best paper published in the journal in 1989-90, and it has been reprinted in at least seven books, some following translation. Subsequent work expanding on the *The Enterprise of Law* and “Evolution of Commercial Law” themes has been published in such places as *Economic Inquiry*, *Constitutional Political Economy*, *Public Choice*, *The Journal of Law, Economics, and Organization*, *The European Journal of Law and Economics*, and *The Journal of Law and Economics* (an empirical paper about private security). A follow-up, award-winning book, *To Protect and Serve: Privatization and Community in Criminal Justice* also was published by the NYU press in 1998. While these publications do not stack up to the *AER* in the eyes of most economists, I am convinced that any one of them present better and more relevant economic insights than all of my old mathematical-game-playing publications put together.

Economic narratives also can be significantly influenced by the normative views of their authors, of course, so even though I find this kind of work to be very interesting and satisfying, I am not contending that it avoids the problems of manipulation that can characterize mathematical and econometric research. Results from math models, statistical models, and narratives can all be manipulated by formulation. However, the assumptions made in a narrative

analysis tend to be relatively simple and transparent, so readers can understand what is going on. Crucial assumptions in mathematical models can be very opaque, and often obscure rather than clarify. The assumptions my narrative personality makes about institutions and behavior are generally explicitly stated, and quite basic. They start with scarcity and reasonable conjectures of human purposes, conjectures that make sense from our vicarious position as students of the actors' contexts. These fundamental assumptions are brought to bear in the context of an uncertain world where transactions costs impede human pursuits. Institutions tend to evolve to reduce such costs, and facilitate voluntary interactions. I also generally assume that individuals and groups have two principal ways of augmenting their wealth, voluntary cooperation in joint production and trade, and taking wealth from others by force or guile. Some individuals develop comparative advantages in violence and predation, so institutions also may coordinate joint production of plunder or extortion. When extortion is institutionalized, a set of rules regarding wealth transfers is developed by one group and imposed on others. In most polities, there is an attempt to legitimize physical coercion monopolized by what we call government, and its apparatus is then solicited or suborned by various interests. My formulations suggest that recognition of both kinds of incentives, to cooperate voluntarily and to transfer through coercion, enhance our understanding of institutions and their behavioral implications. These assumptions and formulations often explain institutions and their evolution quite well, once one does the research to provide a rich and well-informed narrative. I feel this narrative political economy work has real value, not just for me but for the community of genuine scholars pursuing the great conversation of liberal civilization. By contrast, the work I did as a mathematical model-builder was purely self-serving, as it enhanced my standing in the mainstream of economics without adding to the body of useful economic understanding. While the anti-policy econometric work may have had some value, encouraging others to question policy pronouncements derived from statistical modeling, such benefits are, at best, fleeting.

It took more than 25 years for me to suppress my destructive economic personalities so that the narrative political economist could finally be free. I suppose I could relapse if I fail to take my meds (anti-depressants supplemented with beer and Jack Daniels), miss too many sessions with my therapists (Jim Gwartney, participants in Liberty Fund Colloquia), or lose touch with my support groups (APEE members, *EJW* editors and supporters, my wife and daughters). For now, I continue to write, sending my papers to places like *The Independent Review*, the *Review of Austrian Economics*, and a few other journals where editors want readable papers that have real-world relevance, with an occasional submission to

mainstream outlets. Two or three more books also will hopefully be produced before this narrative personality passes from the scene.

## About the Author



**Bruce L. Benson** grew up in a small town in North Central Montana. In 1969, he was drafted and assigned to a combat infantry unit in Vietnam. Before he returned to the “world” 12 months later, the roughly 350 men who cycled through his company experienced the deaths of more than 40 young Americans, almost 100 more Americans physically wounded, and God knows how many Vietnamese killed and wounded.

The totally unnecessary and unjustified carnage he witnessed and contributed to would shake any faith he had had in political and bureaucratic rhetoric. Afterward, he returned to Montana and attended the University of Montana, obtained his BA (1973) and MA (1975) in economics, and met Terrie now his wife of 36 years. He obtained a PhD in economics at Texas A&M University (1978) and took a position at Penn State University (1978-82), where his first daughter, Lacey, was born in 1981, before, once again, returning to Montana by accepting a position at Montana State University (1982-85). Florida State University made Professor Benson an offer he could not refuse, however, and he has been there since 1985. His second daughter, Katie, was born in 1991. In 1993 he was named as a University Distinguished Research Professor in 1993, and in 1997 received a DeVoe Moore endowed Professorship. He also was designated in 2006, as a Courtesy Professor in Law by FSU’s College of Law. An administrative personality tried to emerge in 2006, and while his research personalities quickly challenged and suppressed this new alter ego, this was not done quickly enough. He is now about half way through his six-year sentence as Department Chair, the punishment he received for allowing this evil personality to tentatively show its head, and he has no hope of early release with parole. Professor Benson’s e-mail addresses are [bbenson@fsu.edu](mailto:bbenson@fsu.edu) and [jumpstrt2@yahoo.com](mailto:jumpstrt2@yahoo.com).

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*Editor's note on style:* This article follows Professor Kennedy's British style in spelling and quotation marks. Italics are used to distinguish Adam Smith's words in the text, though not in block quotations.

# A Reply to Daniel Klein on Adam Smith and the Invisible Hand

Gavin Kennedy<sup>1</sup>

## ABSTRACT

Daniel Klein generously invited me to 'rejoin and conclude' our exchanges on "Adam Smith and the Invisible Hand" (Kennedy 2009a; Klein 2009b). Of all the debates I have had with disputants since I began publishing on the mythology of the invisible hand (Kennedy 2005a, 165-68; 2005b, *passim*), Daniel's is by far the most thoughtful and constructive, and I treat his views accordingly.

## The Debate

Let us be clear what I think we are debating: Is the invisible-hand, a popular literary 17<sup>th</sup>-18<sup>th</sup>-century metaphor of no significance to Adam Smith as I contend, or was it for him something with a deeper, yet unspecified, substance, as suggested by Daniel and many others today?

Debate on these propositions, legitimately in my view, must examine exactly what Adam Smith said about the invisible hand, as the main indicator of the role it may have played in his thinking. Modern, post-mid-20<sup>th</sup>-century assertions and

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expansions of the metaphor's meaning, while interesting, carry less weight in assessing Adam Smith's 18<sup>th</sup>-century views.

Daniel adds two more debateable issues: 'whether the three occurrences [of Smith's use of the metaphor] can be reconciled' and 'whether the phrase may properly serve as a tag for an important idea in natural jurisprudence' (264). The last issue is entwined in interpretations from the late 19<sup>th</sup>-century onward (Maitland 1875; Young 1997; Minowitz 2004; Klein 2009a, etc.) and, therefore, I only comment briefly on them.

There are at least two other possible issues. First, its alleged role as a 'concept', 'theory', or 'paradigm' of Adam Smith's (Arrow and Hahn 1971, 1; Arrow 1987, 71; Tobin 1992, 117; Stiglitz 2000, 1448, 1457; Stiglitz 2002, 460, 477), which is the common treatment of the metaphor by most modern economists, and second, the large and still growing literature on theological interpretations of Smith's thinking (specifically the invisible hand metaphor), which I did not discuss directly with Daniel, though I have joined that debate elsewhere (Kennedy, 2009b).

My reply to Daniel concentrates on finding a mutual understanding by clarifying what Adam Smith actually said about 'an invisible hand', which is at the root of our present differences. This approach is separated from the debate on the mutually supportive myths nurtured about Smith's use of the invisible-hand metaphor in a few late-19<sup>th</sup>-century references, but mainly from the second-half of the 20<sup>th</sup> century onwards. The myths are largely responsible for the current ubiquitous status of the invisible hand in academic literature and in daily media usage. At root, I regard these myths as a kind of rhetorical fiction, especially when they are linked directly to Adam Smith, and, as ingenious and creative as some of their literary constructions are, they remain imaginative and fictional, in my humble view.

Again (Kennedy 2009b, 240-41), not much notice was taken of Adam Smith's use of the metaphor except for a few isolated references, with limited, if any, cross dialogue among them, that appeared in the late 19<sup>th</sup> century (Maitland 1875; Onken 1874; Leslie 1879; Buckle 1885; Bonar 1892, 1893; Smart 1899).<sup>2</sup>

However, I think it is ambitious of Daniel to include Dugald Stewart (1753 - 1828) as evidence of an early use "of Smith's phrase prior to the twentieth century". Stewart's reference to Adam Smith's use of the invisible hand metaphor is a lonely footnote in his *Lectures on Political Economy*, given in the winter of 1800-01 at the University of Edinburgh and reproduced in an 11-volume edition

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2. I am grateful to Daniel for identifying four additional late-19th-century references to the invisible hand; I have recently seen a list of over 30 references to the metaphor from the 17th-18th centuries, in addition to the 17 references collected in Kennedy, 2009a.

of his collected works over 50 years later (Stewart 1854, 1856). Significantly, Stewart's *Account of the Life and Writings of Adam Smith* (Stewart 1794/5), the first biography of Smith, was reproduced in almost every edition of *Wealth of Nations* and *Moral Sentiments* published throughout the 19<sup>th</sup> century, but it did not mention the invisible hand.

By any standards, a period from the 18<sup>th</sup> to the 20<sup>th</sup> century is a long gestation period for an allegedly key principle of Adam Smith's oeuvre. Therefore, we can agree that the metaphor did not achieve mainstream textbook and journal recognition prior to the early decades of the twentieth century.

In short, I agree with Mark Blaug's entry on the invisible hand in the *New Palgrave* (2008):

It was only in the last quarter of the 19th century (as a result of German critics of Smith) that the phrase 'invisible hand', which after all occurs only once in the *Wealth of Nations*, was elevated to a proposition of profound significance. Rothschild deals expertly with the subject and concludes that 'the image of the invisible hand is best interpreted as a mildly ironic joke' (Rothschild 2001, 116). This may be going a little too far in the opposite direction to the now prevailing interpretation, but there is no doubt that Smith himself did not attach great importance to the idea of an invisible agency channelling the behaviour of self-interested individuals and instead regarded the metaphor of the invisible hand as a sardonic, if not ironic, comment on the self-deception of all of us, including moral philosophers. (Blaug 2008)

However, the exact number of citations is a minor issue amidst the relatively limited interest in the invisible hand prior to the mid-20<sup>th</sup> century. Each week, *Google Alerts* sends to me scores of references to the 'invisible hand' from today's academia and media, a few of which I comment upon daily.<sup>3</sup> Nothing remotely like today's "invisible hand" currency existed from Smith's death to the late-19<sup>th</sup> century. My statement broadly stands: The invisible hand metaphor has become a 20<sup>th</sup>-21<sup>st</sup>-century icon, to the embarrassment of many modern economists, who encouraged investors and governments to bet their banks on the wondrous safety of what they assured them were Adam Smith's invisible hand—hence the spate of anti-invisible hand outbursts and renunciations recently.

I have been waiting for a new paper on the invisible hand by the distinguished scholar, Warren Samuels to be published in October (to appear in Young 2009) before developing my considered response to the metamorphosis of

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3. See: [www.adamsmithslostlegacy.com](http://www.adamsmithslostlegacy.com)

the metaphor into a modern icon. However, I was much encouraged by a comment made by Jeffrey Young at the June 2009 meeting of the History of Economics Society at the University of Colorado, Denver, when he asked a speaker why Adam Smith did not mention the metaphor despite several passages in *WN* Book I and II, *Wealth of Nations* where Smith would have done so if the invisible hand had the attributes credited to Smith today.<sup>4</sup>

## The Three Occurrences

Daniel makes a strong case for the links among the three occurrences of the invisible hand metaphor to support the weight of his contentions about the significance of the phrase to Adam Smith. If they are not linked (as I contend), or are only loosely so, then Daniel's appeal for their significance in Smith's oeuvre is fatally weakened. Daniel calls on supporting evidence of their linkage from Professor Alec L. Macfie (1898-1980), (Macfie 1971), one of the Glasgow University senior scholars who made a major editorial contribution to the publication of the Glasgow Edition of the *Works and Correspondence of Adam Smith* (1976-82). However, such support as modern scholars give for their interpretations may be less substantial than their reputation warrants. After all, they wrote almost entirely from within a consensus that was, and remains, wholly committed to Daniel's broad assessments, which are, in my view, problematical, bearing in mind that we are discussing matters of fact—what exactly did Smith write between 1744 and 1790? But when we examine modern interpretations of Smith's works over two-hundred years later, we confront disputable opinions of what he may have meant over two hundred years earlier.

My contention is that the closer we get to Smith's indisputable writings (facts) and the further we get from modern interpretations (opinions), the closer we get to resolving disputes, such as whether the three occurrences were linked in Smith's thinking, as evidenced by what he actually wrote and when he wrote it, and not from what modern authors claim him to have meant by looking through the prism of their own modern agenda (Professor Macfie included). Also, it is not clear to me that Macfie is a steady witness, as Daniel (266) is unsure as to what Macfie actually meant in his assessment of the invisible hand: 'rather paradoxically...'; '...might refer to...'; '...perhaps Macfie means...' or '...and means to suggest...'; and '...may engender...'.

Meanwhile, Daniel (265) gently chides me for implying that Macfie's considered views on the invisible hand contain 'nothing with which [I] need

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4. Beside its absence in Books I and II, see also Book IV: *WN* IV.vii.c.88: 630.

contend', just because I did not go on to discuss them. If I had discussed them I would have mentioned that Macfie writes that "the invisible hand appears only once in the *Wealth of Nations* in a rather slight way" and that the reference to the invisible hand in the *Astronomy* is also slight, and that even the passage in the *Theory of Moral Sentiments* is not especially emphatic (Macfie 1967, 103). In 1976, in a mixture of fact and opinion, Alec Macfie and D. D. Raphael assert (in their introduction to the *Theory of Moral Sentiments*) that "commentators have laid too much stress on the 'invisible hand', which appears only once in each of Smith's two books. On both occasions the context is the Stoic idea of a harmonious system, seen in the working of society" (Macfie and Raphael 1976, 7).

In summary, I am less impressed by Macfie's interpretation of Smith's views than I am with Smith's views as he expressed them.

Therefore, I stand by my comments on the lack of significance of the invisible hand phrase in Smith's writings. The 'invisible hand of Jupiter' is a classical literary reference by a 21 year-old, Balliol classical scholar, Adam Smith, to the pagan/heathen beliefs of credulous Roman citizens that their god, Jupiter (Jove), cast lightning bolts at the enemies of Rome. Jupiter's statue, placed prominently on the Capitoline Hill of ancient Rome, and replicated by statuettes in many homes and public buildings, and sometimes on their coins, was not in any way connected to Smith's other two uses, despite Daniel's masterly attempts to show a connection.

The time-line of Smith's authorship of his *Astronomy* essay shows that it was his first literary product (began in 1744 and completed sometime before 1758). Moreover, Smith kept the manuscript of his 'juvenile essay', *Astronomy*, in his bureau,<sup>5</sup> unpublished until he told his Literary Executors, Joseph Black and James Hutton, on his deathbed of its existence and asked them to ensure it was published posthumously, which suggests a long-term deliberate intention to have it published, unlike the rest of his private papers and manuscripts which were burned on his orders. To suggest, as Daniel does, in his penultimate paragraph that there is significance in the invisible hand phrase appearing near the 'dead center' of both of Smith's major books, *WN* and *TMS*, in their last editions, may also be an exhibition of the tenuous debating equivalent of clutching at straws. We don't know where the metaphor may have appeared in Smith's lost manuscript of his third unpublished Book, *Jurisprudence*, though we do know that it does not appear in the student notes of his jurisprudence lectures (Smith 1982 [1762-3]).

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5. Twenty-one years after meeting, Smith informed David Hume of the essay's existence in April, 1773, and asked him to publish it if he died while away in London seeing to the publication of *Wealth of Nations* (Smith Corr. p. 168). That Smith had kept it secret from his intimate friend suggests it had special significance.

In searching for other links among the three instances of Smith's use of the metaphor, Daniel makes heavy play with what other authorities have attributed to Smith (Maitland 1875; Hirst 1904; Minowitz 1993, 2004; Young 1997; Buchanan 1999; Griswold 1999; Grampp 2000; Cropsey 2001; Rothschild 2001; and Craig 2006, and Hayek is also mentioned). Interesting as such undoubted authorities are as contributors to modern thinking, and dozens more could be quoted too—Daniel by no means ploughs a lonely furrow—including Noble Prize winners and other distinguished senior colleagues, they are not necessarily decisive for our debate. None of them is immune to the charge that in this case the 'emperor is naked'.

The absence of discussion of the actual instances where Smith refers to the invisible hand metaphor (though it was not a metaphor in *Astronomy*; for Roman citizens, including Emperors, Jupiter was all too real) is a remarkable feature of our debate. Daniel, almost uniquely, does attend to the context of the invisible hand in *Astronomy*, and I am grateful for his exposition and impressed with his skilful weaving of his evident scholarship, unfortunately on this occasion without agreeing with his conclusions. But most authorities seldom go further than truncated quotations from *Moral Sentiments* and *Wealth of Nations* without any context, nor apparently are they familiar with the surrounding paragraphs.

Why is this? For some, their conclusions are pre-formed from their accepting uncritically the assertions of others—Smith, apparently, was, variously, a Christian, a Stoic, a Deist, a Providentialist, a proponent of Natural Religion, or any combination of these. A few others have read the relevant chapters, made up their minds and expect readers to take their assertions on trust. I recommend that everybody read the relevant chapters for themselves.

I report Smith's actual words, without importing pre-conceptions. Like most students, I too accepted what I was taught about Adam Smith's 'invisible hand', until I read closely what he had written in the context of *Astronomy* and in his two major books. I read the first 20 pages of *Astronomy* as being a critique of the 'pusillanimous superstition' and ignorance, and the last 52 pages as an (outdated) history of astronomy. Thus, in my paper (Kennedy 2009b), I treat the first part of *Astronomy* as Smith's disguised critique of religious beliefs, and as an interesting philosophical exposition of the sentiments of 'Wonder, Surprise, and Admiration'.

Parenthetically, I am surprised that Daniel suggests that I believe that '*philosophy saps the wonder out of the matter*' (Klein 2009b, 267) when I wrote that 'Smith illustrates the pathway to wonder' (245). Smith noted the example of the loadstone that moved the bits of iron along the table, causing the observer who did not '*behold it*' '*extreme Surprise*', and he would '*still wonder how it came to be enjoined to an event so little expected to have any connection*', after which, with understanding, comes '*admiration*'. Wonder is '*the effect of novelty*' (*Astronomy*, II.1-12), following Surprise,

and both lead to admiration. It is philosophical knowledge, says Smith, that ensures that *‘our Wonder accordingly is entirely at an end’* (Astronomy II.9: 43).

Daniel partly disagrees with my assessment, though I found some comfort in his comment that “Maybe Smith held back the *Astronomy*, spared it from the flames, and authorized its posthumous publication as another gentle giveaway that he wasn’t all that serious about the God business” (266), but it seems to me we should be able to agree on what Smith actually says prior to any interpretations we give to what Smith actually says.

## The Two Related Occurrences

The two other references to the invisible hand are closely related but not in the manner that Daniel and others think they are, and I shall discuss why we arrive at different conclusions as to their significance in Adam Smith’s thinking.

In *Moral Sentiments*, Smith discusses the *“beauty which the appearance of UTILITY bestows upon all the productions of art, and of the extensive influence of this species of Beauty”* (TMS, 179). Smith states, if somewhat awkwardly, his theme’s central and original principle: *‘that the exact adjustment of the means of attaining any conveniency or pleasure, should frequently be more regarded, than that very conveniency or pleasure, in the attainment of which their whole merit would seem to consist, has not, so far as I know, been yet taken notice of by any body’* (TMS, 179-80).

His parable of the *‘poor man’s son’* (TMS, 181-83), who seeks *‘power and riches’* only to find that at the end of it he acquired *‘a few trifling conveniences’* that *‘leave him always as much, and sometimes more exposed than before, to anxiety, to fear, and to sorrow; to diseases, to danger, and to death’* is meant to illustrate his central principle, specifically that *‘the pleasures of wealth and greatness’* also *‘strike the imagination as something grand and beautiful and noble, of which the attainment is well worth all the toil and anxiety which we are so apt to bestow upon it’* (TMS, 183) and *‘it is well that nature imposes on us in this manner’*, because *‘this deception’* keeps *‘in continual motion the industry of mankind’* and enables *‘a greater multitude of inhabitants’* to be maintained (TMS, 183-84).

Smith introduces the *‘invisible hand’* metaphor by using the example of the *‘proud and unfeeling landlord’* who *‘views his extensive fields, and without a thought for the wants of his brethren [and in his] imagination consumes himself the whole harvest that grows upon them’*. This is nature’s *‘deception’* at work on the landlord. Smith removes the deception and immediately switches attention to the reality of what happens. The landlord’s stomach could not consume but a portion of the total produce of his fields, albeit a portion that is *‘most precious and agreeable’*. Critically, *‘[t]he rest he is obliged to distribute’* (note the word ‘obliged’) to his family, guests, retainers, and servants, so that in actuality (with poetic licence) the deluded landlord *‘receives no*

*more than that of the meanest peasant*. The dependents receive their (albeit less *precious and agreeable*) portions of the *necessaries of life*, not from the landlord's *humanity or his justice*, but from his delusionary drive for *luxury and caprice* (TMS 184).

The landlords' *obligatory* distribution includes the subsistence of *'all the thousands whom they employ'* (farm labourers and their families, and tradesmen who supply *'all the different baubles and trinkets, which are employed in the oeconomy of greatness'*, TMS, 184), not forgetting the seed-stock for next season's planting, all of which must be taken from the landlords' total harvests each season. This is undertaken by the *'rich and unfeeling landlord'* because he can do no other: he is not driven by a mystical and invisible force or by disembodied hands.

Without extensive distributions beyond the personal consumption of landlords, *'the continual motion of the industry'* would cease and the *'thousands who they employ'* would not last the winter. Nothing more need be added to complete Smith's explanation for the socially fortuitous and delusive behaviour of the *'proud and unfeeling landlord'*. Yet Daniel, and others, are adamant that there is something more to be added and claim, sincerely, that Adam Smith says so.

The famous 'invisible hand' paragraph follows, and is often quoted without comment as if its claimed hidden meaning is obvious. Daniel admits to regarding 'the invisible hand passage in TMS as a terrible muddle' and I appreciate his honest judgement, but I believe the muddle comes from imputing far too much to the passage than it deserves or Smith intended.

They are led by an invisible hand to make nearly the same distribution of the necessaries of life, which would have been made, had the earth been divided into equal portions among all its inhabitants, and thus without intending it, without knowing it, advance the interests of society, and afford means to the multiplication of the species. When Providence divided the earth among a few lordly masters, it neither forgot nor abandoned those who seemed to be left out in the partition. These last too enjoy their share of all that it produces. In what constitutes the real happiness of human life, they are in no respect inferior to those who would seem so much above them. In ease of body and peace of mind, all the different ranks of life are nearly upon a level, and the beggar, who suns himself by the side of a highway, possesses that security which kings are fighting for. (TMS, 184-85)

Smith floats two ideas in this passage. The first is that rich landlords, because of the necessities of their treadmill-like obligation, are bound *'to make nearly the same distribution of the necessaries of life, which would have been made, had the earth been divided into equal portions among all its inhabitants'*. But the land was never divided equally; it's a thought experiment only. There certainly is no archaeological,

anthropological, historical or literary evidence for equal distribution that happened on any scale, though it may have been tried locally without lasting, for reasons similar to those Smith outlines (WN 556-57).

The second idea is Smith's (fairly typical) palliative comment: the existing unequal division of the land made no real difference in practice to the diets of the poor. The division of the produce gives the poor roughly enough to live on and richer landlords can do no other whether '*led by an invisible hand*' or not, because they must feed the '*thousands whom they employ*' or their '*greatness*' would collapse. Who would till the land next Spring without winter food? Neat!

Clearly, it does not require the (highly) imaginative fiction of Providence, under which rich landlords '*without intending it, without knowing it*' do what they must do anyway. They had been doing it for millennia before anybody imagined the rhetorical fiction of Providence, which in Smith's time he used, perhaps, to mollify readers among the families of, or those in political service to, landlords (the dominant class in the British Parliament) whom Smith hoped to influence.

Smith did likewise in *Wealth of Nations* when explaining the sequence of events leading to his invocation of the invisible hand metaphor. Daniel finds the example in *Wealth of Nations* 'much less muddled' (271). I agree; it is fairly straightforward when read within the chapter it is contained in and not read just as a simple, short quotation, to which I suspect most readers restrict themselves. Therefore, I find Daniel's charge that I read the passage 'rather narrowly', perplexing. However, I only paraphrased what Smith wrote. For Daniel, the metaphor has a 'broader reading' (as it needs to have if Daniel's readings are to sustain the heavy implications that he places upon them). For Smith it had a specific meaning, hence his use of the popular 18<sup>th</sup>-century metaphor!

To help settle this matter, I encourage readers to do what most modern proponents of 'theories' of Smith's use of the invisible hand seldom, if ever, do, which is to consider for themselves paragraphs 1-9 in the relevant chapter 2 in Book IV which is headed '*Of the Restraints upon the Importation from foreign countries of such Goods as can be produced at home*'.

The first nine paragraphs leading to Smith's invocation of the invisible hand metaphor fully explain the circumstances in which some, but clearly not all,<sup>6</sup> merchants act to protect their capital stock, and how commercial society permits them to do so in a fuss-free manner, with consequential beneficial effects on domestic output and employment.

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6. Foreign trade—and the carrying trade—were major economic activities with Europe from the 15th century, and even more significant to and from the British colonies in North America, the Caribbean and India in the 17<sup>th</sup> and 18<sup>th</sup> century.

First, he sets out the effects of duties and prohibitions on domestic industry (paragraph 1, 452); then he explains how this diverts domestic resources, not necessarily for the benefit of society (paragraph 2), and he identifies the restrictions on the capital a country can employ when it diverts it in an ‘*artificial direction*’ (paragraph 3). Crucially, Smith turns to the individual who seeks the best employment of his capital in pursuit of his own advantage, not society’s (paragraph 4) and shows why some merchants prefer their capital to be invested locally (paragraph 5). This observation is constrained by the profit motive—as long as profit is ‘*equal or nearly equal*’ to that of foreign trade he will indulge his fears about the insecurity of foreign trade (paragraph 6). Smith gives reasons for the preferences among some, but not all merchants: ‘*his capital is never so long out of his sight*’; ‘*he can know better the character and situation of the persons whom he trusts*’; and should he ‘*happen to be deceived, he knows better the laws of the country from which he must seek redress*’. In the carrying trade—the trade he least prefers—his capital is ‘*divided between two foreign countries*’, and ‘*no part*’ of it ‘*is ever necessarily brought home, or placed under his own immediate view and command*’ (paragraph 6).

It follows that by employing his capital locally he endeavours to produce output of the greatest possible value to himself (paragraph 7), to which paragraph 8 supports with a further, clinching, elaboration: ‘*it is only for the sake of profit that any man employs a capital in support of industry; and he will always, therefore, endeavour to employ it in the support of that industry of which the produce is likely to be of the greatest value*’ (WN, 455).

Adam Smith set out his entire case in paragraphs 1-8. Therefore, when I summarised from *Wealth of Nations*, if I am guilty of “narrowness”, so is Adam Smith! Having read the 8 paragraphs for yourself, ask what difference would it have made to Smith’s preceding analysis if the famous invisible hand metaphor in paragraph 9 (WN, 455-6), had not been added? Because Smith had already fully and credibly explained the circumstances leading to the outcome (as he also did in *Moral Sentiments* with the rich landlord’s ‘delusion’) I suggest, respectfully, it would have made no difference at all!

The invisible hand metaphor adds nothing, and as a metaphor it was not intended to add anything, to the explanation of the merchants’ behaviours (protecting their own ‘security’) which is precisely what a metaphor is supposed to do. According to Smith, in his *Lectures on Rhetoric and Belles Lettres* (1983 [1762]): a metaphor is a ‘*figure of speech*’ in which ‘*there must be an allusion betwixt one object and other*’ and it ‘*can have beauty*’ if the metaphor ‘*is so adapted that it gives due strength of expression to the object to be described and at the same time does this in a more striking and interesting manner*’ (29-32). The metaphor of ‘*an invisible hand*’ certainly has ‘*beauty*’ and Smith deploys it in ‘*a more striking and interesting manner*’ than the nine paragraphs leading up to his use of it.

Those of his readers, who found the argument in the nine paragraphs a tax on their understanding of the consequences of the preferences of some, but not all, merchants, would have found the well-known 17<sup>th</sup>-18<sup>th</sup>-century metaphor a helpful guide to Smith's meaning. Always remember that Smith wrote *Wealth of Nations* to persuade legislators and those who influenced them to change the interventionist policies of mercantile political economy that were followed by all British governments since the 1600s.

Only much later did a few readers (at first) attach a deeper, and wider, or if you like, even religious, significance to the metaphor, which may explain why so many economists have lost sight of what Smith actually wrote and, perhaps, why Daniel thinks that self-interest is not the 'referent behavior' that Smith uses in his account of the way a commercial society operated. Indeed, we can go to another chapter in *Wealth of Nations* to note his summary of exactly the same principle discussed in the 'invisible hand' passage, on this occasion applied to British trade with its North American colonies. Open *Wealth of Nations* and compare the passages from Book IV (WN IV.ii.1-9: 452-456) with Smith's remarkably similar language describing the same case, without mentioning 'an invisible hand', which appears later in Book IV (WN IV.viii.85-88: 629-30).

Daniel, however, seems to downplay the fact that Smith anchors his passages in self-interest in both examples, and raises instead the 'centrality in Smith of natural liberty', supported by an assertion that 'The behavior of the investor is significant not merely because it promotes wider utility, but because it works within rules of comparative laissez-faire' (272). I confess to being perplexed by this drift of Daniel's argument, which he supports by a quotation from Jeffery Young (1997, 168). Also, I notice immediately that 'laissez-faire' is not a phrase that Smith ever used—like so much else in modern commentaries, laissez-faire has been regularly imputed to him by economists (and politicians) since the 19<sup>th</sup> century.

On liberty, however, Daniel is on firmer ground but may be stretching it a little by making it central to Smith's use of the metaphor in either *Moral Sentiments* or *Wealth of Nations*, or for that matter crediting it as a 'proper tag for an important idea in natural jurisprudence' (271). The three pages (273-75) Daniel devotes to the views of Frederic Maitland (1850-1906) while interesting historically, are not really germane to the issue of the significance of the metaphor to Adam Smith. Maitland was a distinguished Cambridge academic jurist and the author of several books on jurisprudence, and he held standard 19<sup>th</sup>-century English views on laissez-faire, as opposed to its interventionist alternatives, but I fail to see how they justify Daniel's assertion that, "*In freedom, people tend to act in ways tolerably in line with what a knowing, benevolent being would like to see happen*" (275, italics added) or, indeed, what exactly this has to do with our debate on 'metaphor or myth'. Quoting from

sources a century after *Wealth of Nations* may not illuminate what Smith held to be significant.

I do not for one moment suggest that for Smith, liberty was unimportant. For Smith, who did not have a vote under the existing franchise—and, for me from observation of the pretence at being democracies by tyrannies around the world, such as Iran, North Korea, Zimbabwe, etc. —liberty is more important than democracy. Of course liberty was an important part of Smith’s legacy, but to imply or assert that modern constructions on Smith’s use of the invisible hand tagged to liberty is, in my humble view, tenuously stretching the connection between Smith’s use of the invisible hand metaphor and what some, but not Smith, called *laissez-faire*, a concept which Smith never used. It was first demanded, after all, by M. Le Gendre, a 17<sup>th</sup> century French shopkeeper for his, not his customers’, freedom from ministerial regulations.

I appreciate Daniel’s genuine convictions, as I can see how the implied association of the metaphor to such wider meanings genuinely appeals to modern economists (including such authorities as Hayek), who do not have an agenda other than what they know as accomplished scholars. They genuinely appreciate the neatness of the metaphor for their modern interpretations of how economies evolve (spontaneous order, unintended consequences). Let me be clear, it is not their integrity that I challenge; it’s their associating their modern usage with that of Adam Smith’s.

Therefore, I cannot agree with Daniel (277) that it ‘does not much matter whether Smith *intended* the phrase to serve as a tag for the comparative merit of freedom’ (emphasis added). Yes it does! That is precisely what this debate is about: the significance for Adam Smith of his use of the popular 18<sup>th</sup>-century metaphor of an invisible hand. Its significance as a wider concept—even ‘theory’—as interpreted by modern economists, is another debate entirely, and one I shall join in due course, and see if Daniel and I can reach an agreement.

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## About the Author



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## Character Issues: Classic Reprints

*EJW editorial note:* This lecture of March 1955 to the Scottish Economic Society has not previously been made available in any electronic form. The present version results from manually typing up the essay as found in Macfie's *The Individual in Society: Papers on Adam Smith* (London: Allen & Unwin, 1967): 19-41. But the lecture first appeared in the *Scottish Journal of Political Economy* 2(2): 81-103. We gratefully acknowledge the *Scottish Journal's* permission to reprint the piece. At the end, we have added "About the Author," but everything else that follows, including "thole," "kenspeckle," and the occasional brackets are, to our knowledge, faithful to the 1967 appearance.

# The Scottish Tradition in Economic Thought<sup>1</sup>

Alec Lawrence Macfie<sup>2</sup>

### ABSTRACT

This essay must start with a confession. In undertaking, some months ago, to submit an article on some such subject as 'The Scottish Tradition in Economic Thought', I was, it is now clear, in a state of not very creditable ignorance. I had then a rather vague idea that one could in an article say something directly significant on this subject. I had, of course, at various times read the Scots classics in a rather haphazard way; but the effect of reading them all straight through in their proper sequence, in the hope of tracing the individual Scottish thread running through them—the effect of this has been radical. For it has forced the

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1. Delivered at the Annual General Meeting of the Scottish Economic Society on March 24, 1955. Reprinted from the *Scottish Journal of Political Economy*, vol. II, pp. 81-103.

2. A.L. Macfie (1898-1980) held the Adam Smith Chair of Political Economy at the University of Glasgow, Glasgow, United Kingdom G12 8QQ.

conviction that there is a quite specific doctrine and method in Scots economic thinking, especially clear and influential between roughly 1730 and 1870, and still alive, if not on top. As it shows some of the greatest names—Hume, Smith, the Mills—and also some considerable satellites—Hutcheson, Lauderdale, Rae, McCulloch—it at once appears that a mere article will not do. If there is a Scottish line inspiring these great writings, then only a volume could do it justice. The kind of thesis one would like to examine is the view that between Hutcheson and John Stuart Mill it was that Scottish mode of approach that formed the atmosphere of British economic thought. But this starts many other hares, and the most we can do here is to chase some of them conscientiously.

Our general theme must then be that there is a characteristic Scottish attitude and method which is important in the history of economic thought. It may be called the philosophical approach, though many of us may prefer to call it, equally aptly, the social approach. This is not the dominant approach today in academic teaching—the scientific or analytical method holds that place everywhere—but in Scotland the traditional approach is still alive and influential.

It should then be our business to show that this Scottish method and interpretation grew in a definitely historical setting. It grew out of the Scottish soil and reflected truly the Scottish atmosphere. To establish this one would need to describe at least four influences which nourished it. First, there is a place of Scots social thinking in the stream of European culture and history. The Scots gave it their own typical turn but their thought is in the broad Stoic stream. It was brought to its highest pitch by Hutcheson, Hume and Smith. Rather more speculatively, it was alive and dominant in the Mills. Then this Scottish method has its simple everyday reflection in the teaching and curricula of the Scots universities, especially Glasgow in the period between Carmichael and Smith, and later Edinburgh. This we need to prove the persistence, the natural roots of the method. It has formed the seed-bed of Scots students and teaching—all of them in our Scottish universities—right back to link up interests of the Scottish people, and they again have their special contacts and friendships with the Continent, practical contacts shaping the educations of the people who provided the leaders in Scotland till after the '45.

It will then be necessary to show how this approach and method were at work in the classical sequence up to and including the Mills. It was indeed working in Marshall, though more as a climate than as the guiding line. Is this not the way in which the faithfulness to the classic line, on which he so insisted, took effect? Certainly Jevons was the more typical English exact scientist. But we must not digress. An equally strong claimant as a fact is the decline of the philosophic method from its dominant position, or from being a pervading atmosphere, after about 1870. Along with this we have to note the relative decline from eminence of

Scots economic thinkers. After the Mills no Scot reaches the heights in economic theory, and after Marshall there are no more ‘three-deckers’, no comprehensive philosophic surveys of economic theory. Whether this is because the Scot can be great only when he may also philosophize is a matter for speculation.

Then finally one ‘lone star’ influence that demands notice is the fact of Smith’s genius. Genius can have strange indirect effects. Its brilliance often extinguishes other valuable, though lesser lights. This is not due to any lack of sources in Smith’s work; they are all there. But successors cannot continue on the level of genius. One aspect of the great man, probably that which suits later conditions, is chosen, much of the rest probably rejected. The section of Smith’s work which was so chosen and developed till it became supreme was the first two books of the *Wealth of Nations*. The theory of static equilibrium there so carefully sketched has grown into an analytic system and method which has for long dominated English-speaking universities, and our universities today control our theory as never before in modern times. It is a paradox of history that the analytics of Book I, in which Smith took his own line, should have eclipsed the philosophic and historical methods in which he so reveled and which showed his Scots character. Book I of the *Wealth of Nations* is now a part of world thought, as is the *Origin of Species*, or *Principia Mathematica*. But even so we cannot speak for the future. The positive analytic method is a mere stripling. An immense future stretches ahead; and the past certainly shows that each phase of social development produces its own philosophy and method in all the social disciplines. Do these signs of today which spell out the future show any special call for the sociological method? If so, a Scottish revival is due. An opportunity is opening for Scottish thinkers.

## II

We begin then with the thesis that there is a unique Scots method or approach, or interpretation of social issues, as individual as any personal approach, and that it can be described as philosophic or sociological. It rises to maturity in the eighteenth century, especially through the work of Hutcheson, Hume and Adam Smith. They gave it its unique bite of flavour, though the flavour depends on the Scottish soil and cultivation in the propensities and institutions of the people. This is seen in the course of writings taken as their normal education by all the members of this close-knit group—between Edinburgh and Glasgow. The course began with Natural Theology (including some Natural Philosophy), went on to Moral Philosophy, and thence to Justice and Law. It was under the law of contract and private property, with its social aspects, that the broad descriptive

and critical comments on political economy arose (the kind of discursive comment *ambulando* which the philosophic method naturally and richly inspires). This accepted sequence is found in all the writers—in Gershom Carmichael, Adam Ferguson and Dugald Stewart as much as in the great trinity. It also dominates the method and point of view of Lauderdale and Rae, though after Smith the more specialized treatment of economics inevitably begins. Each writer may give his work a special slant towards his own special interest: Hutcheson towards morals, Hume towards metaphysical skepticism and also history, Ferguson towards sociology and Smith towards economics. Smith, however, like Hume, adds the crown of genius, and genius is apt to confuse the inevitabilities of logic. It also shapes history in a way which may obscure the roots and setting of the writer. Thus, just because Smith is a world figure, we are apt to ignore his completely Scottish character. We cannot begin to understand him, especially what are often thought of as his weaknesses, if we thus ignore his roots, for the Scottish method was more concerned with giving a broad well balanced comprehensive picture seen from different points of view than with logical rigour. In fact, Smith was, like the others a philosophic writer, modeled on Hutcheson, whom W.R. Scott truly called the ‘preacher-philosopher’.<sup>3</sup> His aim was to present all the relevant facts critically. Modern writers start from a totally different angle. They found on the law of non-contradiction. They aim at isolating one aspect of experience and breaking it down by analysis into its logical components. Thus the older type of writer is often accused of ‘inconsistencies’, and certainly these are to be found, especially in Hutcheson, whose pupil here Smith certainly was. To the analyst such inconsistencies are anathema. To the modern method they represent failure. But to the philosopher they reflect the facts of our experience. It is part of wisdom to recognize, accept and be able to carry such inconsistencies. While we should of course try to reduce them, we should not insist on avoiding them in our critical descriptions, for then we omit the crux of our fate, and also the practical human problems.

This attitude is still very much alive in the Mills. James is the most sociological of the Benthamites and his valuable historical and psychological expansions are in the true synthetic tradition. The son of course is both the jack and master of all trades. If one reads through the *Logic*, the *Political Economy*, and the main ethical and political writings in one gulp, the sheer wide power, range and status of this mind cannot be missed. But the ‘improvement’ bacillus is always at work in it, inspiring and driving—the optimistic though individual practical belief in wide human improvement reflected down from Hutcheson and Smith. The Utilitarian movement is itself, of course, a philosophy. But as a philosophy it is

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3. W.R. Scott, *Francis Hutcheson*, p. 70.

English rather than Scottish, especially in its positivism and its willingness to be dominated by facts. Bentham gave it these main characters; by contrast the Scottish qualities in the Mill writings are unmistakable.

But is the attitude and tradition still alive? Well, it has to be confessed, only in a rather negative way. Scots teachers and writers are today certainly primarily interested in the historical or the social and critical system of Marshall's *Principles*. But that is a world movement. They were themselves brought up in it. The writings of Scots economists are, however, still coloured by the traditional Scottish point of view. One hint, significant if slight, anyone immersing himself in this literature can hardly miss. In the Scots writings up to recent times there is hardly, so far as I can find, one serious example of the use of mathematics to develop analysis. (The Hutcheson example is well known. He removed the mathematical passage in the second edition of the *Inquiry concerning Beauty and Virtue*.) Some may contemplate this fact with relief. The whole Scots sequence cleaves to actual events, to historical and institutional relations growing through them, and to the individual experiences that support and develop the argument. But such individual factors do not lend themselves to mathematical or purely deductive logical treatment. It is not the case that there is any relative weakness in the Scot in mathematics. The facts certainly prove the opposite in the eighteenth and nineteenth centuries, in Glasgow and Edinburgh.<sup>4</sup> The obvious explanation is that the Scottish philosophical and the mathematical methods do not blend. The assumptions in the first are normative, in the second exact. For this very reason, mathematical processes are especially grateful to the English exact positive scientific approach in economics. It is no doubt a useful contrast of methods within our small island. Ideally, the one should stimulate the other.

It should be noticed here that this approach is reflected in the curricula of the Scots universities, and always has been. It has been bred into Scots thinking over all the generations, and is now steadily acquired as well as inherited. This can be illustrated by the curricula of the eighteenth century. The course was in Humanity and Greek Philosophy (metaphysical and natural), Logic and Moral Philosophy, and, less securely though it was always there, Mathematics.<sup>5</sup> It is here interesting that in August 1695 'the Faculty at Glasgow appointed "Mr. John Tran...to compose the Ethics, Oeconomics and Politics" for the general course in philosophy which the Parliamentary Commission of 1695 was seeking to arrange by agreement between the four Scots universities'. This was only a tidying

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4. For the seventeenth century see H.W. Meikle, *Some Aspects of the later seventeenth century in Scotland*, p. 25.

5. The Chair of Mathematics in Glasgow dates from 1691 but it was not until 1826 that the subject was formally admitted to the curriculum for the degree of M.A. This seems to have kept down the number taking the subject. But the record is creditable, especially in the great period. Cf. Mackie, *The University of Glasgow*, p. 216.

operation, for the disciplines were already established in all the universities. Lively individual preferences, as might be expected, prevented any common course from ever reaching print, so Mr. Tran's course is not available; but the fact stands out that as early as 1695 'Oeconomics and Politics' were established studies for the graduands.

We should never forget that Adam Smith grew from these roots; that his work is as influenced by them as any writer's could be; and that it could not have grown from any but just such an exactly and richly cultivated soil. This fact has been obscured by the unfortunate inadequacy of Scottish histories of the seventeenth century. Yet, as H.W. Meikle insisted in his Murray Lecture, the foundations for the future cultural growth were in fact largely laid in Scotland in the seventeenth century. Adam Smith was taught in a burgh school in Kirkcaldy, and later in the normal courses of a Scots university he built up the knowledge, methods and aims of his own thinking. He was not building on air or mere personal talent, as popular records are apt to suggest. We should remember such strategic events as these: The Advocates' Library began its modern career in 1682, and, as Dr. Meikle insists, it was one of the vital vehicles of Scottish culture. Again, Stair's *Institutions of the Law of Scotland* appeared in 1681. This is the scientific genesis and inspiration of Scottish jurisprudence, and is itself a unique work by any standard. It was from such stems that Smith's genius was bred. The foundations remain to this day. No Scottish student in Arts can take a pass degree without a philosophy, and the Honours courses are based on the width of the pass degree, as knowledge of at least four subjects is required in it (though modern specialization has made some inroads). This training has no doubt encouraged a people more interested in a philosophic argument (and in persuading other people that he, the teacher, is right) than even in getting what one wants. (Does not the Englishman typically make exactly the opposite choice?) The Scots theological interests and relations should also be mentioned, but cross illustrations will grow as we proceed.

We note next that this local movement of thought was itself also a reflection, though as an original facet; a reflection of what was perhaps the major European stream. It is the great flow from Stoicism. But the Scottish inspiration came rather through the Roman glosses than from the Greek sources. Cicero, Marcus Aurelius, Seneca, Epictetus, these were Hutcheson's mentors (though Leechman mentions the way he encouraged Greek studies). Hume's references are to the same texts, and so it is for all of them. They did not—they were not trying to—dig so deeps as the mighty Greeks. They were painting a broad practical sketch of society, expressing all the important balances, not exposing the roots. For these slightly superficial surveys the Roman texts were invaluable models. As an example, take Hutcheson dealing in their typical way, later followed by Hume and Smith, with the advantages of social life. He quotes Cicero (*De Finibus*) on the

advantages of population sufficient to allow of division of labour. The passage has his usual optimistic flavour. It does not appear in his treatment of economy, and it is interesting as showing the social origin of the Scottish treatment of division of labour rather than the more individualistic source described by Locke. But this so fruitful treatment of division of labour is still embedded in an ethical, political, legal argument.<sup>6</sup> It was Smith who was inspired to effect its transference, to form the premiss of a purely economic argument and so give a new science its task and direction.

The Stoic influence in Scottish thought is most apparent in the constant study of Law. Especially on the practical side of this philosophizing, the argument was carried by discussions of property rights and origins, or by questions on contract, particularly in relation to land. This appears in the first teacher of the school, Gershom Carmichael, under whom Hutcheson studied and whose major work was the editing of the text of Puffendorf. So we are back to the broadest river in European culture, the Stoic-inspired Roman jurisprudence, carried practically throughout Europe on the broad currents of Roman and Canon Law. The richest contacts for Scotland came from France and Holland. (The French, like the Scots were more deeply versed and skilled in Latin than in Greek.) In our Glasgow school this comes through directly in the study of Puffendorf, and especially of Grotius, whom Smith read under Hutcheson. This tradition pervades the *Wealth of Nations*, but in a Scottish dress. It is optimistic, tolerant, always eager for social benefits between as well as within nations, very different from the nervous, individualist, critical, touchy excursions of the revolutionary French writers. This good temper the school owes at least as much to Hutcheson as to Smith. Leechman, who was a pupil of Hutcheson and later became Principal in Glasgow, has described Hutcheson's personal attitude in words which apply equally to Smith, and indeed to Hume and all the Edinburgh writers. In spirit, aim and conduct they were citizens of the world, and they behaved as such. The world was smaller then and more harmonious, but these sympathies and contacts still struggle to live in Scotland today.

These scrappy remarks are all that space allows here. The place of Scottish thought in the main stream of European jurisprudence is a subject that grows in subtlety and scope the more one learns about it. It would need a long chapter in any volume dealing with our subject. Its literature is as massive and difficult as any Europe's thought can show. But there is a more popular level of interpretation,

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6. This passage occurs in vol. I, p. 289, of *A System of Moral Philosophy*, in a discussion of 'the necessity of a social life'. It is quite separate, in place and subject, from his treatment of economic values which occurs in vol. II, Ch. 12, embedded in his consideration of contracts.

and it may be useful to suggest in a footnote some simple writings that at least serve as introductions or summaries.<sup>7</sup>

The main faith which the Law of Nature and Stoicism inspired in Scotland was a faith in natural liberty in a natural society. Here certainly Smith was Hutcheson's faithful disciple. Of Hutcheson, Leechman tells us: 'As he had occasion every year in the course of his lectures to explain the origin of government, and compare the different forms of it, he took peculiar care, while on that subject, to inculcate the importance of civil and religious liberty to the happiness of mankind: as a warm love of liberty, and manly zeal for promoting it, were ruling principles in his own breast; he always insisted on it at great length, and with the greatest strength of argument and earnestness of persuasion: and he had such success on this important point, that few, if any, of his pupils, whatever contrary prejudices they might bring along with them, ever left him without favourable notions of that side of the questions which he espoused and defended'. Smith certainly was so influenced with the others. But our view of natural liberty in the *Wealth of Nations* has received a deceptive twist from history, from the individualism of the industrial developments, and the interpreters, apologists and critics, of the Industrial Revolution. This twist does not do balanced justice to Smith's own feeling about natural liberty: here he followed his teacher, and was in a broad way, at one with his fellow writers. The arguments were thrashed out in relation to American colonists. On this, though earlier, Hutcheson was at least as liberal and decisive as Smith.<sup>8</sup> And the extent to which Smith's imagination could range is seen in his suggestions for federal government in a unified British family. Here again we have to make an effort of historical imagination to see what our classic fathers wanted and appreciated. We are so apt to read our own wants, especially in terms of some index number of the standard of living, into their more closely-knit social and cultural aspirations. Perhaps this quotation from Hume expresses their hopes as tersely and vividly as we have space for: 'The more these refined arts advance, the more sociable do men become; nor is it possible that, when enriched with science and possessed of a fund of conversation, they should be contented to remain in solitude, or live with their fellow-citizens in that distant manner which is peculiar to ignorant and barbarous nations. They flock into cities; love to receive and communicate knowledge; to show their wit or their breeding; their taste in conversation or living, in clothes or furniture. Curiosity allures the wise; vanity the foolish; and pleasure both. Particular clubs and societies are

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7. *Legacy of the Middle Ages* (Meynial on Roman Law); *Cambridge Medieval History*, vol. V (Hazeltine, Roman and Canon Law); Vinogradoff, *Roman Law in Medieval Europe*; Bryce, *Studies in History and Jurisprudence, Holy Roman Empire*; James Macintosh, *Roman Law in Modern Practice*; and J.N. Figgis, *From Gerson to Grotius*.

8. Cf. *The William and Mary Quarterly*, vol. XI, No. 2 (April 1954). For Hutcheson the article by Professor Caroline Robbins (a grateful name) is specially interesting. The whole number is very relevant.

everywhere formed, both sexes meet in an easy and sociable manner, and the tempers of men, as well as their behaviour refine apace. So that, besides the improvements which they receive from knowledge and the liberal arts, it is impossible but they must feel an increase of humanity from the very habit of conversing together and contributing to each other's pleasure and entertainment.' Natural liberty is a very different sentiment when inspired by the aim of 'an increase in humanity' from that pervading business specialization. But it was this society as a glorified Athenaeum that these eighteenth century Scotsmen desired, and indeed to a creditable, if limited, extent achieved.

It might here be convenient very briefly to recall some of the more influential factors in these Scottish contacts; especially with France and Holland. The natural enemy was England, and this lingered, so far as influences went, into the early eighteenth century. So the inevitable friends were found in the Low Countries and France. Professor Mackie remarks: "There was much coming and going between Scotland and France, where until about 1670, the government accorded to the Huguenots the privileges promised by the Edict of Nantes in 1598, and it is obvious that in the world of western scholarship the man from Glasgow could fully hold his own.' That he did so can be gathered, on a popular level, from such a study as *The Scot Abroad* by John Hill Burton. The direct evidence is scattered over the memoirs of scholars, statesmen and fighting men in all literatures of Europe. The Reformation paved the way for a possible partnership with England but the balance swung decisively only in the eighteenth century. We need merely mention the economic ties with the Low Countries. Only 1707 with its gradual swing towards the west in commercial expansion, displaced these predominant links with the Continent. Of equal influence, especially in the sixteenth and seventeenth centuries, were the religious contacts. Scottish churchmen in their many see-saw evasions tended towards Holland, Geneva, even France, rather than towards England.

The evidence here is well known. What was important was the personal and social impact of these contacts. It is best realized through the diaries—again a formidable library. But four famous later ones can be recalled—those of 'Jupiter' Carlyle, Ramsay of Ochertyre and Cockburn, and, most intriguing of all, the recent Boswell volumes. There is also a lesser known one, from which a passage seems worth quotation because it reflects vividly a swing in Scottish social habit; it comes from *John Fergusson 1727-50 (An Ayrshire Family in the Forty-Five)* by James Fergusson. Lord Kilkerran, a Court of Session judge, is concerned about the education of his son, the subject of the biography. He wishes a legal training such as will suit him to care for the Ayrshire estate. 'John's education', we are told<sup>9</sup> 'had

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9. Op. cit., p. 25.

advanced to the university stage when, in the autumn of 1743, his father decided that it should be completed in England. It was an unusual decision for those days. For generations past Scottish boys, especially students of law, had gone to the continent to seek such instruction as they could not get at home. This was partly because of the long wars with England and the traditions of continental alliances and friendships they left behind. Moreover, English universities were not open to Presbyterians, nor, if they had been, could the Roman Law, on which the Scottish legal system is founded, be studied there so well as in the Netherlands, where Lord Kilkerran himself had gone as a young man, and many of his contemporaries.' The boy was placed at Dr. Dodderidge's (dissenting) Academy at Northampton. In a letter to the reverend doctor the father says, 'The boy is seventeen since July last, and after being taught Latin and Greek at the publick school<sup>10</sup> with the assistance of a tutor, has been one year at the University with Mr. Maclairn, Professor of Mathematicks, whose name will not be unknown to you. What his proficiency has been in the languages I shall not anticipate your judgment, and as they are of great use in life, especially the Latin, for the study of Roman law, to which I intend he shall apply himself, I hope it will not be out of your way to improve him in the knowledge of the language. The Greek I know you are more fond of in England than we are here, and for gentleman educated for the church it is absolutely necessary, but otherwise I consider it only as a part of the belles lettres'.

This no doubt marks a social watershed. Thereafter, in growing volume the contacts are English. In the earlier diaries, however, we feel the strength of the personal influences. They are mainly centred round the Law and the Church. The leaders of Scotland depended on the management of their estates, or aimed to acquire estates. The law they must know was feudal and Roman, and the pre-eminent schools of such law were in Holland and France. Every man of property, intent on the education of his heirs, inevitably sent them on their Continental travels. In this regard, it is one of the smaller compensations of Boswell's swing to good behaviour that he turned (with such agonizing) to Holland and the traditional road for Scottish finish. For one could desire no more complete short account of the type of studies young Scotsmen followed than that in *Boswell in Holland*; and the spice of genius is added, as ever with Boswell. Part of the genius brings us to the heart of the personal life for the many Scots who share in Boswell's gaieties. But they are all very serious also, as one would expect of the breed. We certainly feel their determination to master any knowledge which will help them to manage their Scottish affairs. The same practical spirit and thoughtful comparative study of foreign institutions also flavours Sir James Steuart's volumes.

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10. The High School of Edinburgh. The mathematician is Colin Maclaurin, of the *Treatise on Fluxions*.

In their social gatherings the Church and the men-at-arms are the other main contributors. From the diaries we can sense the strength of the eclectic tradition. They are typically curious about people, men at work, about comparative institutions and what we could call 'social statistics'. They are not concerned with logical processes or sequences, or the framing of abstract hypotheses and their analysis to their utmost limits. They wish to build a truly balanced picture of social life as they found it and the forces which controlled it. Here it must be insisted Adam Smith is a true Scot of the eighteenth century. He had the common Latin Continental scholarly basis (how strange to find his library so full of Stoic texts, so almost entirely devoid of technical economic texts). He is not consciously concerned with building a logical model, or even with arguing in the merely logical mode. We should note his inconsistencies, but we should remember that they are the reflections of the method. Each aspect, the analytical, the historical, the contemporary-comparative (or sociological), is dealt with in turn. The inconsistencies arise out of these different aspects, and so out of real conditions. In Book I we have the analytics, and they bore such great issue that Book I has almost obliterated the rest in the world's estimate; but the rest is at least as characteristic of Smith as Book I. It is the *whole* book which represents his meaning.

Next in this family history come that unique father and son, James and John Stuart Mill. That this is fact some may contest; especially outside Scotland they may not be recognized as full-blooded Scots. Their effective lives were spent in the metropolitan circles of British culture, and they spent their lives in advocating a social theory and policy which are generally regarded as English, though they are more truly British. Certainly, we ought to accept the son's own words that *philosophically* he was no Utilitarian. One could spend an article on each of these issues; here we can only summarize. About James Mill there is really no question. To the end of his days he was as Scots as his native Angus (and the breed there is strong). Of his type he is really a very Scots Scotsman, so Scots that the parishioners who heard his early 'tasting' sermons could not thole him. No doubt a church is one of the few places in which a Scotsman submits to being told, but evidently the burning ardour to teach that was James Mill's enduring dynamic in life took intensities which even they were not prepared willingly to stomach.

His story is as typical a Scots success story as one could find. He had the special qualities and virtues of the Scottish approach, and he brought them from Scotland via Edinburgh University. He was interested in all social forces and structures. There are the usual exercises in comparative sociology in both the *Elements* and the *History of British India*. (In the latter there is an interesting reference to the work of John Millar in Glasgow; a direct link between the tradition's source and the modern classical sequence.)<sup>11</sup> And of course he shows the eclectic

interests that are so Scottish, especially in his wide psychological insights. The simple clarity of his thought and style reflect his great Scottish predecessors (of the east rather than the west coast). Indeed his very clarity may explain why he has been so constantly underestimated. Obscurity is one way to temporary reputation. Ricardo's *Principles* is, to most non-specialists at least, unintelligible if they try to consider the book as a coherent system.<sup>12</sup> So it is probably Mill's translation of it in his *Elements* that in the earlier period carried the original work into the central consciousness of ordinary British thinking. For Mill's little book is entirely clear, though it is probably as near to what Ricardo meant as anyone is likely to get, which may not be very near. But further, here, and most distinctly, the economic theory is placed in its proper relation to the other social sciences, in the true Scottish manner. 'It is also,' he writes, 'in a peculiar manner, the business of those whose object it is to ascertain the means of raising human happiness to its greatest height, to consider, what is that class of men by whom the greatest happiness is enjoyed. It will not probably be disputed, that they who are raised above solicitude for the means of subsistence and respectability, without being exposed to the vices and follies of the great riches, the men of middling fortunes, in short, the men to whom society is generally indebted for its greatest improvements, are the men, who, having their time at their own disposal, freed from the necessity of manual labour, subject to no man's authority, and engaged in the most delightful occupations, obtain, as a class, the greatest sum of human enjoyment. For the happiness, therefore, as well as the ornament of our nature, it is peculiarly desirable that a class of this description should form as large a proportion of each community as possible.' So even James Mill saw visions, and here what a prescient vision! For he foresaw the welfare state, in which we are all to be comfortably middle-class. There is, further, no doubt that on its personal side Mill was the driving power of the whole movement. The tributes of George Grote<sup>13</sup> and of his own son, and his letters with Ricardo leave no doubt about this. Through him the Scottish tradition flows through the veins of the whole Utilitarian movement.

For the son, on the merely factual side, the case is less obviously strong. His mother was English, though she had little, if any, influence on his intellectual life.

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11. Mill quotes Millar as an authority on three occasions: in important footnotes on the English constitution, slavery in primitive times and the position of women in North American tribes. Calling him 'that sagacious contemplator of the progress of society', he later remarks that 'the writings of Mr Millar remain about the only source from which the slightest information on the subject can be drawn', the subject being civilization among the Hindus. I am indebted to Dr R.L. Meek for this reference.

12. Need it be said that no criticism of Ricardo is here intended? His contribution to economics is shining and accepted. But many will agree with Schumpeter's summary: 'Ricardo's *Principles* are the most difficult book on Economics ever written. It is difficult enough even to understand it, more difficult to interpret it, and most difficult to estimate it properly' (*Economic Doctrine and Method*, p. 80).

13. G. Grote, *Minor Works*, p. 284.

He lived in England through his formative and most active years. But was it England? Could anyone live under the direct guidance of James Mill without living rather intensely in Scotland? The famous, in some respects infamous, education was his father, and it is just a supreme example or excess of a peculiarly Scottish propensity. It formed Mill through and through. It produced a mind of unique power, speed and range. Reading over his major works—and one has to do this to sense his special power, for it is extensive rather than intensive—one cannot miss the recognition that this is a weapon specially forged, made possible only by the conscious persistence of its maker. Evidence of a type which others may find inconclusive is the simple fact that, in these two, any knowledgeable Scot will recognize his brothers; not in capacity, but rather in their fanaticisms and weak spots. Especially Scottish is their consuming desire to teach everyone they meet. If one were asked to find outstanding examples of this fanaticism, few could stand beside them. Perhaps the Ancient Mariner could. With that goes the typical elimination of the humorous point of view which is so generally accepted in the south as Scottish. The teaching Scot, like the ravaging wolf, is too busy to be humorous. If his mind had been disengaged he could no doubt have seen the joke, perhaps as quickly as a Frenchman or a missionary. But he is not interested in the joke. He has to get his message across. It is not argued that this is necessarily a proper attitude. It is merely insisted that it is a Scots attitude, and that the Mills illustrate it pre-eminently. Then again, Mill's 'inconsistencies' are even more famous than Smith's and they arise out of the same method for the same reasons. There are other questions that arise with John Stuart Mill, especially perhaps in regard to his wife's responsibility for the social aspects of his writing, but these we must ignore. They do not alter the broad conclusion as to Mill's responsibility for the sociological emphasis throughout the *Principles*, and especially in the theory of distribution. Yet this is entirely what one would expect of one brought up by his father, who was in turn nourished by the Scots method and interests.

This brings us to about 1870, or indeed later, if we include Marshall. Marshall is the equivocal figure. If the Mills are typically Scots, Marshall is the proper English contrast. The *Principles* of Marshall is in the shape and spirit of the *Principles* of Mill, as Marshall delighted to insist. One wonders at times if he does not protest too much. Certainly the tone and wide sympathies, especially of Book III and VI, are in the tradition, but the footnotes and Books IV and V introduce the modern monographic abstractly analytic method. The history passes down to the Appendices. Some gremlins have slipped into the cupboards and cellars of the venerable building. As we know, the footnotes and the appendices have grown to separate commands. They have sunk the 'three-deckers', which sail the seas of realistic speculation no more. Perhaps it was Jevons who launched the decisive attack, for he began the long line of modern positive scientific monographs, but

the contributions of Marshall towards their ultimate victory are as subtle and deep as was Marshall himself.<sup>14</sup>

However this may be, the fact which stands out for us is clear. The specific Scots tradition ceases to dominate about the middle of the century (sharing rule with Ricardo). There are no great, or even highly placed, Scottish writers in economic theory after Mill, and after Marshall the tradition that Scots thinkers did so much to form, the eclectic, comparative, widely sociological tradition, has faded out. What is still very much alive is the analytic method, the technique of static equilibrium which finds its source, or a main source, in Book I of the *Wealth of Nations*. But this is no longer specifically Scottish—it never was. It is absorbed into the stream of world thought as truly as is the central theory of Darwinian evolution. In its modern, dominantly Marshallian perfection it has captured the Universities. All academic economists have spent their lives teaching it as the core of their subject. And the Universities today control the spirit of social theory far more exclusively than in the eighteenth or even the nineteenth century. Only Smith of our greater Scots economists was an academic, and he was so much more. This (perhaps temporary) eclipse of the Scottish method is then the last fact we must, rather dismally, record.

### III

We turn from fact to fiction—here admittedly a distinction of degree. The effect of Smith's genius on the course of the Scottish tradition is a difficult speculation. Yet one cannot ignore it, for it was also Smith's genius which started the modern analytic method on its conquering course. Anyone who studies any pre-Smith Scottish economist (Sir James Steuart, say), then Ricardo, the Mills and Marshall in sequence, will accept this. Here then is the novel individual force that Smith himself contributed. But in his use of history and of broad sociological facts and comparisons to develop his argument and to demonstrate the need for considering all the influences together as seen in actual institutions—in this Smith was not original, he was simply Scottish. His aim was the Scottish, not the modern aim. If here also he shows genius, as he did, it lies in the richness, the apt relevance of his illustrations. It is a mistake here merely to lament the past. The old sociological thinkers of the Scottish school had weaknesses and gaps that to us are glaring. Especially, their view of what can pass as a fact fully deserved Dugald

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14. This paragraph may seem to suggest that Marshall would have sympathized with the predominance of analytics and the specialized monograph. I believe this is the opposite of the truth. Marshall was essentially a perfectionist, seeking to reform the institutions and members of the *society* he knew, and wished us all to know. If so, he was nearer the early classics than the economic theorists of today.

Stewart's inspired description, 'conjectural history' (he did not mean this as a criticism). With their equipment, based as it was on little more than travellers' tales, facts could hardly be more than conjecture. If the older method is ever to revive in economics, we must realize that the task will be much more difficult than it was in the eighteenth century. Then one mind could reasonably absorb all the writings on the social sciences, as a glance through Adam Smith's library will show. Today this is impossible. We are all so specialized that when we stray from our own disciplines the sense that we may be talking weak superficialities is an inadequacy that we must accept and face. May it not be mere arrogance for any of us to expect to survey all the knowledge? Or has the delusion of Faust in reality captured our spirits? A due humility seems to require that we take the risk of these recognized inadequacies. The eighteenth century thinkers were not accustomed to speak with the assured dogmatism of our modern analysts. As we are, with our present one-line specialisms, we run the risk that nowhere will a balanced picture of the whole social adventure, or even sections of it, be drawn. Yet this should always be the crown of our endeavour. Are our trained thinkers then to leave this valuation to journalists and politicians, for it will inevitably be made by someone? It is neither fair nor right that they should be alone in making it. And it seems immensely dangerous to allow them to be alone.

It should not be laid to Smith's account that Benthamite Utilitarianism became the basis of orthodox economic thought. The opposite is the truth, on the ethical side; Smith there went much deeper than Utilitarianism. It is in fact just an accident of history—one of the many which underline the inadequacy of mechanistic interpretations—that the method of static equilibrium originated in his Book I. The central assumptions of Benthamite Utilitarianism are themselves antithetic to the whole spirit of the Scottish social school. The main philosophic contrast is between a mechanistic psychology, which inevitably eliminates any truly moral theory, and the optimistic forward-looking assumptions of the Scottish school; or again it is seen in the fact that the Scots saw the central fact as a *growing* society, a creature quite different from any single individual, whereas to Bentham any society was merely an aggregate of individuals. This broad contrast is of central importance for modern economics simply because Marshall accepted as the basis of his positive economic theory the mechanistic 'ethical' or psychological assumptions of Bentham.<sup>15</sup> The static equilibrium theory of 'normal' value is therefore itself inevitably mechanistic. It traces the run down, after disturbance, to

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15. *Principles*, p. 17, n. [One must remember this is volume I. If Marshall had written volume II, he would certainly have dealt with dynamic theory. As to the association psychology, the eighteenth century Scottish school also accepted it but this was *fante de mieux*. This psychology prejudged the emotion *versus* reason argument in favour of the former. It therefore laid thinkers like Smith open to the charge of inconsistency, when they appeared to give reason any determining power.]

a position of stable equilibrium. It has great heuristic value. But its practical inadequacy stands out; it is not equipped to deal with changes *away* from equilibrium. Yet these changes seem to dominate our economic fates.

In this context a historical speculation will perhaps be allowed. Suppose Benthamism had not captured the dynastic succession. Is it possible that then the spirit and outlook of Lauderdale and Rae might have gained command? We can at least imagine it. They were both critics of Smith, inevitably though admiringly, but they were both completely in the Scots manner and method. It is, however, the line of their criticism that is significant. They thought Smith's theory should give more weight to the importance of invention, novelty, new arrangements in history. Smith, of course, did much here, but to Lauderdale and Rae invention is picked on as the core of economic growth, and this is suggested as the central issue in theory and practice.<sup>16</sup> One cannot say this of the *Wealth of Nations*. If their interpretation had developed, it would have had to do so through the Scottish type of procedure, by comparative and historical excursions. The analytic equilibrium theory in fact misses change. It cannot cope with the individual causes of change, just because it is analytical. This means that the method deals with laws and characters that are common on different economic situations. But change and innovation cannot be dealt with by such a method, simply because an innovation, an economic novelty or change away from equilibrium, is by definition a fact. It is therefore in the major sense unique, not common. If it is thus unique as history, it must then be dealt with, it can only be dealt with finally, by methods which are proper to the particular, to qualities as well as quantities. Such methods as the historical, the philosophical or the sociological, are in their turn complementary. The Bentham-Marshall analysis has given us keen cutting power where regularities can be traced, and this is invaluable, but the older method has faltered or been absorbed in the sands of specialization. Yet the strong basis for its use, as Lauderdale and Rae saw it, still remains. Enterprise is its most positive pole. It is the individual improving or creative element that finds some place in *every* worker in his degree. This is the drive behind economic growth. There are ample sources for this, as for most lines of theory, in the *Wealth of Nations*. Had it grown, it is possible to imagine the type of theory that Schumpeter has so richly developed in our day, working in

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16. For emphasis on the vital importance of what we can summarize as 'know-how', and the suggestion that Smith did not give it sufficient weight, see Lauderdale, *Inquiry into the Nature and Origin of Public Wealth*, pp. 159-61, 176-7, 184-5, where Smith is criticized for missing the special productivity of capital, its own productivity as distinct from that of labour; cf. also p. 287. For Rae this productivity of capital is the central theme. It inspires the whole of his remarkable *Sociological Theory of Capital*. Mill with his usual wisdom quoted fully from the outstanding passage (Mill's *Principles*, pp. 165-72), but Rae's theme just is that economic progress depends on enterprise, so in turn on the existence of those social conditions which call it forth. Chapters IX, X, and XIV are especially relevant.

broadening circles through the whole range of economic experience over the last two centuries. If this had happened, our economic theory would certainly be a better balanced, a more realistic, more practical equipment than it is today. We might also be nearer control of our main economic evil, the problem of unpredictable growth or decay, inevitably away from economic equilibrium.

#### IV

If we try to account for the decline in the Scottish influence, we become even more speculative, and because, apart from its influence on the future (on which this article will end), the subject is of no special practical importance, only brief comment is attempted. We should perhaps first remember that the very brilliance of our eighteenth century throws our judgment out of balance. One would not expect the splendour of classical Athens from its modern representatives. The power of the Scottish influence on European thought is indeed surprising as coming from such a small and then rather remote people. That they realized their position is often evident in their remarks. It is taken for granted quite naturally in this letter from David Hume to Gilbert Elliot (1757): 'Is it not strange,' he writes, 'that at a time when we have lost our Princes, our Parliaments, our independent Governments, even the presence of our chief nobility, are unhappy in our Accent or Pronunciation, speak a very corrupt Dialect of the tongue, which we make use of: is it not strange, I say, that in these circumstances, we should really be the people most distinguished for Literature in Europe?' It is the strangeness, not the fact that excites his wonder. The fact was accepted, and Hume was certainly no boaster, and was in a position to judge. To ask why it happened is as hopeless of answer as questions about any broad movement of history.

On the more practical side, the cradle being mainly in Glasgow, there is real occasion for research into the economic setting there over the century, for it showed a vigorous outburst of energy, and opportunities made and taken, such as would stimulate and direct wider social enquires. The eighteenth century rapidity at least must be rather unique. Later, Glasgow slowed down to the tempo and form of British industrial development in general. But the answers we can offer to 'Why the decline in Scots economists?' are at best negative. The one positive factor we have noted is that an antithetic movement of thought captured the leadership of social thinking, and Scotsmen, like Welshmen, or even Irishmen, have been absorbed into it. We in the Scottish Universities have done our due and proper part in teaching economic theory in the Bentham-Marshall tradition to a rich flow of students. The present article may at times have seemed critical of that

tradition, but this is not intended. Any fair estimate must recognize the invaluable services to accuracy of definition, objective consistency and cutting edge that the more positive methods have built and sustained. When we remember the vague guesses of previous speculation we must insist that these disciplines of the experimental and scientific methods are at least an essential stage in the longer journey of knowledge. But are we sufficiently satisfied with their results to regard them as more than a stage, tool-making rather than directly ore-bearing?

However that may be, there is little doubt that Scotsmen, while acquiescent, have not been entirely comfortable and luxuriant in modern economic theorizing, and it may well be that men so placed do not tend to excel there. The Scottish mood remains critical. The trend has been to teach the orthodox line, but to do one's special work rather in historical, social or semi-philosophical research. The two obvious Scots rebels should at least be mentioned—Ruskin and Carlyle. That their crusade was largely emotional is not in itself to its discredit. But here we deal with scientific thought, so we need only remark that their protests were typical of the Scottish tradition in that they were broadly social and moralistic. A more closely reasoned academic reaction is to be found in one who taught Marshall's system all his academic life yet came in the end to feel it was not enough. William Smart's *Second Thoughts of an Economist* puts the equilibrium theory of distribution entirely fairly (on the lines of his book on Distribution). He quietly insists that if you are teaching mere economics, you must in logic and common sense distribute according to 'economic worth'. Where he reacts, as he reflects on his work towards its close, is in the conviction that economic theory should itself be based on assumptions which are at once more realistic and more moralistic than those accepted in Marshall's footnote (on page 17). He agrees that the static equilibrium theory of prices follows logically from the assumption that a rising standard of living is a sufficient aim for economic science—but only, he correctly points out, if we think of the standard of living in merely quantitative terms. If exact logic is maintained, Benthamite assumptions can lead only to such conclusions. (Strict logic is not in fact maintained.) Yet, to this experienced business man who later turned to teaching and economic history, it was clear that the quantitative measure was incapable of dealing with the real emotions and forces which inspire business and working men. If pushed to the application we wish to reach, it gives false results. We wish, for workmen also, the economics of free enterprise in a growing society rather than what we have been given, the economics of free competition in a static society. This is the Scottish reaction still at work.

It is difficult to find a kenspeckle link in the tradition between Smart and John Stuart Mill, simply because there are then no kenspeckle Scots economists. In Glasgow the man who bridged these years was Edward Caird. He taught some economic theory in his Moral Philosophy course, but in the only set of notes

available (made by a student in his class) the economic theory is slight;<sup>17</sup> it is typically illustrated from classical and Stoic sources, interlaced into their moral theories. He certainly regarded it as part of his duty, though a small part, to teach Political Economy, but when he felt the subject grow beyond his energy he gladly passed it on (in 1887) to William Smart. The following passage,<sup>18</sup> which concludes a public lecture by Caird, shows, however, how conscious he was of the wider social forms in economic life, and how typically Scots was his reaction to them: 'I do not think it will be possible henceforth to separate political economy from general study of politics, or to discuss the laws of the production and distribution of wealth apart from the consideration of the relation of the distribution of wealth and the modes of distributing it to the other elements of social well-being. The abstraction of science will always be necessary for thorough knowledge of economy, as of everything else; but when we isolate part of *human* existence, it is more important than in relation to any other subject to remember that we *are* abstracting—*i.e.* that we are dealing with fragments of a whole, of which no final account can be given by anatomy. The practical value of the social science of the future will depend not only on the way in which we break up the complete problem of our existence into manageable parts, but as much and even more upon the way in which we are able to gather the elements together again, and to see how they act and react upon each other in the living movement of the social body.'

One might well ask, where has the Scots energy gone, if it has lacked the highest achievement in economics? Some questions merely raise others. After all, the abilities of a people can be spread over all its interests. To academic minds, Theology, Philosophy, the Law, Science, Medicine have always opened wide doors, and there the generations of Scottish scholars have found at once satisfaction and distinction. But more specifically one may expect a people to follow its aptitudes. If so, Scots interested in sociological or normative processes would not find these very directly in modern analytical economics. Here it may be fair to note the numbers of distinguished Scots in the history of social anthropology. In the eighteenth century they shared almost oligopoly with the French, but later we have Maine, J.F. McLennan, W. Robertson Smith, Sir Alexander Gray and Professor Hamilton in their contributions to economic history. And, in general, anyone who knows the Scots universities from the inside realizes that the most natural approach of Scotsmen is either philosophical or historical, or severely applied in the sense that it confines itself to interpretation based on scientifically established fact. So it is with the students also. No one who

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17. There may be further sets of his notes in private hands. If so they would be welcomed by the Library of his University, if legible.

18. Edward Caird, *The Moral Aspect of the Economic Problem* (1888).

has taught Scots students can miss the special response when the philosophic aspect is raised. We cannot explain such tastes. It is just 'the nature of the beast'.

Finally, as to the future, will it be likely to offer new opportunities to Scotsmen to follow their bent? If it does, we may expect a more positive Scottish contribution. Whether Scots today maintain the qualities so impressive in their scholarly past is endlessly debatable because the limits of the argument cannot be fixed. Their interest in the speculative and in the normative type of knowledge is certainly as intense. It appears that the influence of the Church is not dominant as it certainly was in the eighteenth century. It was the Presbyterian structure that then most closely expressed the familial relations of the relatively small society which led the country.<sup>19</sup> This may have diminished, but mainly in form. If one were to add the intellectual impacts of the Church, the Universities, the Law, Medicine, the more scientific side of business, the Scots are certainly as intellectually inclined today as ever they have been. Then again, one might point to some watering of the warm family emotion which formed our earlier loyalties and institutions. Certainly the power of the larger kinship group has passed. But we remain a small tightly knit nation, keenly interested in each other as persons living together. That this still lives is expressed in our poetry, essentially a folk, a family literature. The talented editors of *A Scots Anthology* (stretching from the thirteenth to the twentieth century) remark: "The main body of Scottish poetry is not in this heroic vein but springs from the normal day to day life of the people, a people who, until the end of the eighteenth century, lived wholly in the country or in small towns where you had only to go down a wynd off the main street to reach the country. It was, too, a remarkably homogeneous people. Rich and poor lived pretty close together and the gap between the great folk and the common folk was not an unbridgeable gap; divisions between the classes did not obscure their common humanity. And so we find that a court poet like Dunbar could leave the lofty, artificial, allegoric strain and be not only familiar, but vulgar; that some of the best of our songs of peasant life and feeling are by writers of the landed and professional classes; that Scott—Sheriff of Selkirk; Laird of Abbotsford, and Baronet—could write of the life and feelings of common folk with an understanding and reverence equalled only by Wordsworth; and that, in our own time, *Fisher Jamie*—that delightful serio-comic elegy on a Tweedside poacher—was composed by a highly cultured Scot who ended his life as Governor-General of Canada and Chancellor of the University of Edinburgh." If one reads through these five hundred or so pages of living emotion, the continuing sense of personal individual life in a free community is manifest. We

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19. Cf. Mackie, *The University of Glasgow*, pp. 186, 212.

may still then feel assured that whether or not we are the size of our fathers, we remain the same kind.

The future, however, will open to us only if we give it what it wants. Are there any signs that there may be a renewed need for the Scottish approach in the social sciences? Here we can only consult our broad impressions. Are we satisfied that the methods of analytic economics are sufficient, or indeed alone suitable, as the theoretical approach to the economic issues now typical in industry? In the nineteenth century the practical effort was specialized as never before. It appeared reasonable to treat by a linear science what was then practiced as a rather linear way of life in private business. Even within their own minds, it was customary for business men to keep somewhat separate the standards of business from their more speculative ethical and religious ideals, and a simple code of the economic man could have sufficient appearance of practical relevance, as well as afford a firm intellectual basis. Between them social biology, Freud, the joint-stock company, modern nationalism and wars, the inevitable state direction they foster, have altered the slant of our thought about man in society. The importance of groups as the setting of the individual, perhaps the central awareness of the eighteenth century in Scotland, this awareness is today again dominant; dominant in fact, and therefore to be dealt with by scientists; dominant in the kind of problem which practice proposes to thought, as in all the mixed issues of welfare and defence which make up the traffic of our political life. In this new world, the effort to see the different aspects together in their proper relations again becomes at least as important as the exact definition and analysis of each aspect. The two traditional methods, the analytic and synoptic, are then seen to be complementary, mutually supporting and nourishing each other. We may well have to be patient with sociology. Its difficulties are immense. But if there is a future for sociological economics and politics—and if there is not we shall have to make it—then there is an opportunity for the resurgence of the Scottish tradition and the Scottish genius. May the opportunity make the men!

## About the Author



**Alec Lawrence Macfie** (1898-1980) was born in Glasgow, served in the First World War and then studied at the University of Glasgow. In 1930 he became lecturer in Political Economy and then held the Adam Smith Chair of Political Economy from 1945 until he retired in 1958. He was Dean of Faculties, 1974-1978. His works include *Theories of the Trade Cycle* (1934), *An Essay on Economy and Value* (1936), *Economic Efficiency and*

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*Social Welfare* (1943), and *The Individual in Society: Papers on Adam Smith* (1967). In 1971 he published in the *Journal of the History of Ideas* a paper on the expression “the invisible hand” in Adam Smith’s works. Macfie was coeditor with D.D. Raphael of the Glasgow edition of Smith’s *The Theory of Moral Sentiments*, published in 1976 and now available from Liberty Fund. Macfie’s papers are held by the University of Glasgow ([link](#) to the Macfie collection). In *The Individual in Society*, Macfie wrote: “To Smith [passions] are natural, but to be duly restrained. And the positive agent in restraint he finds, not in any revulsion from due self-love and developing wealth, but in the growth of moral rules and social institutions to control them appropriately, through the slow workings of informed sympathy” (81).

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