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A Reply to Steven Horwitz's Commentary on "Great Expectations and the End of the Depression"

Gauti B. Eggertsson¹

ABSTRACT

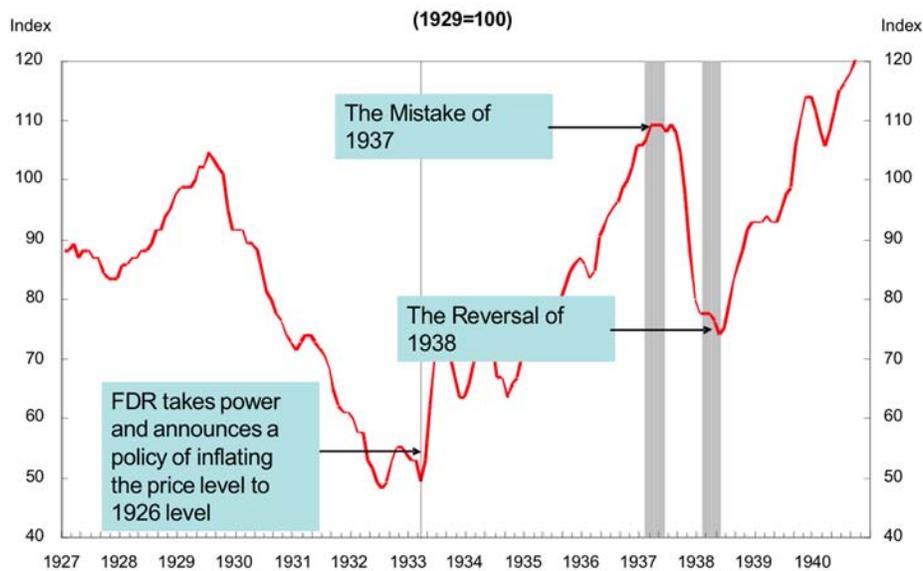
Steven Horwitz's commentary on my article "Great Expectations and the End of the Depression" (*AER*, September 2008) appeared in the September 2009 issue of *Econ Journal Watch* (Horwitz 2009). I thank him for drawing attention to the article. I also thank the editors for providing this forum for discussing the paper. This note responds to some of the issues raised.

To start with, let me summarize the paper Horwitz is commenting on. The *AER* article (Eggertsson 2008) proposed that the recovery from the Great Depression in 1933-37 was driven by a shift in expectations. Expectations about prices turned from deflationary to inflationary, while expectations about output turned from contractionary to expansionary. This shift was triggered by a policy regime change characterized by deliberate policy actions by Franklin Delano Roosevelt (FDR). The policy actions included abolishing the gold standard and explicitly aiming for reflating the price level to pre-Depression levels. Furthermore the policy actions included a vigorous fiscal expansion, driven by an increase in government spending and budget deficits. The main contribution of the article is

1. Research Department, Federal Reserve Bank of New York, New York, NY 10045-0001. The views expressed in the paper are those of the author and are not necessarily reflective of views at the Federal Reserve Bank of New York or the Federal Reserve System. Any errors or omissions are the responsibility of the author. The author thanks Michael Bordo, Marco Del Negro, Andrea Ferrero, Valerie Laporte, Charles Steindel, and Jón Steinsson for comments.

to formalize this hypothesis, which is not entirely new (see e.g. Temin and Wigmore 1990), within a dynamic stochastic general equilibrium model and to model explicitly the policy choices of FDR in an infinitely repeated game where the government cannot commit to future policy. In this game the regime change is modeled as “exogenous” abandonment of certain policy “dogmas” or “rules” which constrained Hoover but not FDR (these rules were the gold standard dogma, the small government dogma and the balanced budget dogma). The paper shows how an exogenous elimination of the dogmas can endogenously explain the policy actions (within the context of a Markov Perfect Equilibrium of the policy game) and quantitatively account for the recovery in prices and output. The paper offers both narrative and quantitative evidence to support this reading of history.

Figure 1: Industrial Production



Source: Federal Reserve Board

While there are many interesting issues raised in Horwitz I have chosen here to focus only on the main themes. Horwitz presents six major criticisms of the *AER* article which are summarized in his introduction. I address each in turn; quoting parts of his claims (while referring readers to his article for more detail) and then provide some brief responses.

1. “It is wrong to view the Great Depression as over in 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.” (315)

I agree with Horwitz that the Great Depression was not over in 1937. This, however, does not contradict the paper. Instead, it supports it. Let me explain. The *AER* article is about the first part of the recovery, that is, the period 1933-37. In particular, the article focuses on the turning point in 1933 when the largest downturn in U.S. economic history (1929-33) turned into the largest non-wartime expansion in U.S. economic history (1933-37)—a point that coincided with FDR’s inauguration in March 1933. There was a second contractionary phase of the Great Depression in 1937-38. The “Mistake of 1937”—the action that triggered the second phase—was to abandon the reflationary regime that FDR embraced in 1933 (The ‘Mistake of 1937’ is the title of an earlier paper, Eggertsson and Pugsley 2006). It was only when the administration recommitted to a reflationary regime in 1938 that the recovery resumed.² Thus, the sequence of events in the second phase of the Great Depression, shown in the figure above and hence the slowness of the full recovery, provides *additional support* for the *AER* article, rather than contradicting it.

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.” (315)

This appears to reflect a misunderstanding. Section V (pp. 1504-06) in the *AER* article explicitly incorporates the gold standard, modeling it as an *inequality constraint*: The money supply had to be *less than or equal to* gold reserves held by the U.S. government in certain proportions. There was nothing that precluded the government from holding more gold than corresponded to the outstanding monetary base. As the paper points out, this inequality was not binding in 1929-33. The U.S. government, in other words, had more gold than it needed to support the outstanding monetary base and was thus free to expand money supply. This is also

2. This about-face on reflation is marked as the “reversal of 1938” in the figure. The two shaded areas signify the turning points and are derived from newspaper accounts; see Eggertsson and Pugsley (2006) for details. I use industrial production, since that allows me a month-to-month reading. While I discuss these developments in detail in the article “The Mistake of 1937,” I give the gist of the argument in footnote 11, page 1480, of the *AER* article.

pointed out in Horwitz (2009, see pp. 317-18), citing Friedman and Schwartz. I see no disagreement here. What was strictly binding in 1933 when FDR took power was not the gold standard—and this is a central point of the paper—but the *zero bound on short-term nominal interest rates*.

All this is discussed in quite extensive detail in the paper, but toward the end, in Section V. That section also stresses that even if the gold standard was not binding in 1933, abandoning it was important for expectations. Why? Because even if the inequality constraint was not binding in 1933, people may have believed that it *could* be binding in the future. Thus, the paper argues, eliminating the gold standard was important to shift *expectations* from being deflationary to being inflationary, as the gold standard put an upper bound on the possible monetary expansion *in the future*.

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.” (315)

The *AER* article is about the regime change that happened as FDR took office from Herbert Hoover and the sharp distinction between their policies. It is the change in policy that is of principle importance for the argument. Horwitz, however, spends a surprising amount of energy to convince us that some other politicians at the time, such as e.g. President Harding and others wanted even smaller government than Herbert Hoover and hence that Hoover was a “big government” supporter relative to Harding. The characterization of Hoover as subscriber of “small government” is therefore misleading, the argument goes, since there were people that believed even more fervently in the principle of small government. This seems to me to be a little bit beside the point.

The argument in the paper only requires that FDR wanted *more* government spending than Hoover and that FDR was willing to maintain *higher* deficits than Hoover, i.e., that there was a regime change in 1933. What someone else wanted, e.g., President Harding or somebody else, and how Hoover’s view contrasted with those views, has limited bearing on the argument. It seems to me that the narrative record, the data, and the model speak clearly to this: There was a clear regime change when FDR took power, and there was a sharp contrast between FDR and Hoover’s views on the size of the government, deficits and the gold standard.

Consider the data cited in the paper: Government consumption and investment was *90 percent higher* in FDR’s first full calendar year in office than in Hoover’s last year in office. This seems suggestive of a regime change. Deficit

spending, by some measures, was as much as 66 percent higher in FDR's first year, standing at 9 percent of GDP. This, also, is suggestive of a regime change. Further, the Annual Reports of the Treasury show a change in the basic budget strategy in 1933 (and this, to my best knowledge, has not been emphasized much before in the literature and I find this narrative evidence rather compelling). While Hoover's Secretary of the Treasury planned to balance the budget—and failed—FDR's Treasury Secretary planned to do deficit spending—and succeeded spectacularly (See the discussion in Section I of the *AER* article).

The *AER* article uses a relatively standard general equilibrium model (similar to what is now in use in most policy institutions and in academic papers on monetary policy) that shows that the regime change proposed can quantitatively account for a large part of the recovery in 1933-37. In summary, I think both the model based exercise and the data are quite supportive of my own reading of the narrative account.

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." (315)

The *AER* article is not a novel reading of the historical record except to a very limited extent. It strikes me as conventional wisdom that Hoover and FDR differed in their views on balanced budgets, big government, and the gold standard (see, for example, Temin and Wigmore [1990] and references therein, as well as references in the *AER* article). What is new in the article is formalizing those particular policy views as explicit constraints, or "dogmas", in a relatively standard dynamic stochastic general equilibrium model and formalizing this in a dynamic policy game and matching the results to the data.

Debating whether the belief in small government or balanced budgets should be called a "dogma" or something else seems to me to be somewhat redundant. I am perfectly happy to call the "dogmas" in the paper something like "rules" or just refer to them as they are described in the model: One equation says that the government will try to keep spending fixed at some particular (low) level (relative to FDR), another equation says that the government aims at balancing the budget (see equations 16 and 17 in the paper), and finally an inequality constraint for the gold standard (see inequality 53 in the paper). Whatever we call them, I think all these mathematical relationships have reasonable grounds in history.

Were there other equations that perhaps restricted the government's choices during FDR's administration (for example, a belief in the importance of redistributing wealth, or hostility towards capitalists)? Yes, perhaps, and this

would be very interesting to explore. Horwitz, however, does not suggest particular policy constraints that might be introduced in the model to approximate the narrative record and/or some salient feature of the data more closely. That seems to me to be the most interesting avenue for exploration going forward and I hope that the literature will develop in that direction. It seems to me that the most sensible measure of success in this regard is: Can these additional constraints give a better quantitative account of some particular feature of the data and/or the narrative record?

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.” (315-16)

Horwitz is certainly right that there was a great deal of confusion during the initial days of FDR’s administration and—indeed—the whole recovery period. But as I argue in the paper, FDR’s seemingly confusing and contradictory policies all pointed in one direction: Everything was designed to curb deflation and inflate the economy. The hypothesis that this relentless pursuit of inflation triggered the recovery is central to my *AER* paper, to “The Mistake of 1937” (Eggertsson and Pugsley 2006), as well as to “Was the New Deal Contractionary?” (Eggertsson 2006), which addresses the National Industrial Recovery Act.

Perhaps it is useful to think of this body of work as formalizing and quantifying the consequence of a general policy strategy elegantly summarized by the historian David Kennedy (1999) in *Freedom from Fear: The American People in Depression and War, 1929-1945*: “And yet, amid the chaos of the Hundred Days, and indeed through the tense stand-off of the interregnum that preceded it, one threat flashed and dove like a scarlet skein shot through brocade: inflation.”

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.” (316)

It is true that I do not cite Higgs’ work on uncertainty regarding rules, which I am unfortunately unfamiliar with, or any work suggesting that regime uncertainty suppressed investment and output suddenly when FDR took power. The problem with a hypothesis that FDR “created uncertainty”—relative to Hoover—when he took office and that this curbed output and investment seems to me to be the data (see e.g. the figure above). It seems to me quite difficult to generate this story in a reasonably calibrated quantitative model. The period 1929-33 marks the deepest depression in U.S. economic history while the period 1933-37 marks the sharpest recovery in U.S. economic history outside of wartime. The turning point was March 1933, exactly the month FDR took office, as seen in the figure above. Was this just a striking coincidence? And given this striking coincidence should we instead be surprised that the recovery was not even faster? My own prior is that the turning point was not a coincidence and my sense is that we should focus on models that can explain it. (In the paper I actually discuss the hypothesis that the recovery was a coincidence and show that this hypothesis has counterfactual implications for the movements in the short-term nominal interest rate.)

Regarding investment: Investment, as it happens, also started improving, right around March 1933. This is shown in figure 1, panel A, p. 1478, of the *AER* article’s introduction, using investment data from Temin and Wigmore (1990). Admittedly, the baseline model abstracts from investment. The appendix, however, extends it to include investment. I am uncertain as to why an “electronic search” through the paper did not turn up the discussion about investment and the associated appendix. Perhaps one should caution future readers not to rely too heavily on electronic searchers.

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A Life among the Econ, Particularly at UCLA

William R. Allen¹

Interviewed by Daniel B. Klein

ABSTRACT

Where did you grow up, where did you go to school, and how did you first get interested in economics?

By some imperative—physiological, psychological, philosophical—fish gotta swim, birds gotta fly. Nothing quite so definitive and inevitable specified that I would pursue a university career. But I was never much tempted to explore any other course.

I am a child of the Midwest, the Great Plains, the Heartland of America, in particular, the (Adopted) Land of Lincoln, where men are men and the women tend to look like men. After winning World War II, finishing college, and nearing completion of the doctorate, I looked at a geography book and moved to California and UCLA.

But let me back up.

My family would have been shocked and perhaps chagrined if I had headed for a life in business or government. True, in high school, I flirted with the glamorous idea of leaving the Midwest for Hollywood in order to be an animator for Walt Disney, and I still insist that “Snow White and the Seven Dwarfs” is the

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greatest of movies. However, it soon became accepted by me and the other Allens that I was to be a professor. But profess what?

Thinking, reading, writing, and talking about significant things in some contexts were pretty much required by family conceptions of civilized behavior. My father was an erudite Methodist minister; my mother published several hundred poems, essays, and stories in church outlets; one of my older brothers, Clark Lee Allen, achieved a doctorate in economics, and was perhaps the most effectual classroom instructor I have known; and the other brother, Francis Allen, was among the eminent legal scholars of his time. My sister, Olive Allen—a free spirit—did not entirely conform: She had a fine career as a Registered Nurse, so one of the Allens did things tangibly useful.

With regard to field of specialization, history was my first attraction. I checked out my high school library's copy of Charles and Mary Beard's, *The Rise of American Civilization* almost every day for the several weeks it took me to read the heavy volumes. My brothers once suggested that some day we all might collaborate on a project conspicuously intertwining the three disciplines of economics, law, and history. That never materialized, although my economist brother and I much later did co-edit a rather elaborate book of readings in *Foreign Trade and Finance* (Allen and Allen 1959).

While working on my A.B. at Cornell College—a small liberal arts school in the wilds of Iowa, not to be confused with the much younger university in New York—I lived with my economist brother and his tolerant wife in the summer of 1947, when he was teaching at Northwestern. I audited his two-term Principles course and a monetary course given by an extraordinarily able family friend, Robert Strotz. I was intrigued by the pursuit of a straight-forward line of analytics with empirical application and seeming verification to real-world issues. My brother suggested that I might find it more satisfying to wrestle rigorously with today's problems than to rehash the debates of the distant past. And he added that I could continue to investigate previous experience by work in the field of economic history.

I completed my undergraduate work with a dual major in history and political science in 1948. That fall, I entered the economics doctoral program at Duke University, where my brother was then teaching.

As it developed, I did little work in formal economic history, but, with guidance from my major instructor at Duke, Joseph J. Spengler, I dipped quite heavily into the area of dead economists, a.k.a. the history of economic thought.

My lawyer brother never evinced to me disappointment with my pursuit of economics rather than law. He probably suspected that it took at least two economists to equal (in some sense) one lawyer. But late in his life he requested my assistance in preparing a section on the history of the post-war Bretton Woods

agreements in writing a brief biography of Chief Justice Fred M. Vinson, for whom he had clerked for two years. And following his period as Professor of the University at Chicago, where the study of law, economics, and business tended to meld, he moved to the University of Michigan as the dean of the law school, and there he recruited an economist into the ranks of the law faculty.

Are there lessons or morals to be drawn from this experience? It would not do to base firm conclusions on a single life experience, but I make two tentative generalizations.

First, it should not be ridiculous for a would-be economist to major in something other than economics or mathematics as an undergraduate. Whatever we do, there are always alternative costs of not having devoted that time, attention, and energy to some other activity. As a graduate student, I suffered a bit from beginning advanced work with a relatively skimpy background in my newly adopted field of economics. But such economics instruction as I had from my brother and Strotz, along with assiduous study of textbooks by Stigler, Boulding, Meade and Hitch, and Bowman and Bach, enabled me to catch up and survive with adequate grace. And along with surviving, there surely were gains, maybe even net gains, from obtaining a semblance of liberal arts education to leaven the technical training. Of all the courses I took, my favorite was a small upper-division class at Cornell devoted entirely to the poet John Milton, taught by a sensitive, meditative, erudite specialist in the subject, Howard C. Lane. It has been comforting to be assured that “they also serve who only stand and wait.”

Second, especially for one who is headed for graduate work, there is much in favor of first attending a small liberal arts college. At Cornell, there was a great deal which could be learned about the various aspects of the world and its evolution in the mandatory year-long freshman courses in English, history, and the social sciences. The learning was facilitated by classes of small size taught by non-T.A.s, and by much interaction with fellow students in the dorms and dining halls. And one can be captain of the tennis team without being a professional jock. One can even find someone of beauty and grace who becomes one’s spouse soon after being graduated.

Perhaps a third generalization should be added: It is helpful to have two uncommonly able older brothers who are both demanding and supportive.

I'd like to ask about the political views in your family. Did you discuss politics much? What were the political views of your parents, your siblings? What were your politics at the time of your graduate training at Duke?

The Allens and Politics

The Allen family could hardly be apolitical. With parents preaching the “social gospel” of doing good works and with one son becoming a legal scholar and two becoming university economists, all with much interest in aspects of the history of the human experience, we inevitably were concerned with political institutions and processes. But the interest was generally quite impersonal, with the view of politics being from some distance. Certainly, none of us was tempted in the slightest to run for an office or to seek a governmental position (except that my lawyer brother, who clerked two years for the Chief Justice of the United States, very likely would have been gratified by appointment to a high bench).

I recall no animated Allen dinner-table political conversations. Indeed, I remember only a single instance of such talk. At a large family event in late 1963, I remarked that John F. Kennedy’s presidency was not happily distinguished and would be remembered primarily in relation to his assassination and the end of Camelot. My conservative brother-in-law was gratified by my suggestion, but everyone else was silent.

I understand that my parents voted for Hoover in 1932, but I suspect that they were Democratic from then on. My economist brother seemed to have said little on politics, which he doubtless considered to be grubby business, but he was a good Friedmaniatic in economics. My more liberal lawyer brother was not an active participant in the political game, but he worked hard and effectively to make the legal system less dreadful. So the Allens commonly faced up to political realities, but they did so with considerable aloofness, reflecting insouciance, bemusement, despair, and only rarely with hopeful enthusiasm.

Still, the three brothers had some immediate contact with public policy and its implementation. Francis, the lawyer, chaired the committee that drafted the heralded Illinois Criminal Code of 1961, and he chaired Attorney General Robert Kennedy’s commission on Poverty and the Administration of Criminal Justice, which led to the Criminal Justice Act of 1964 and the Bail Reform Act of 1966. The volume of his Oliver Wendell Holmes lectures at Harvard in 1973 is entitled, “The Crimes of Politics.” Clark Lee, the first of the family economists, on the day of being awarded his doctorate at Duke, went with the eminent Joseph J. Spengler to become the core of the Atlanta price control office in 1942—a wartime assignment they both found repugnant. Later, while in the army, he participated in instructing officers who were to administer economic aspects of the post-war occupation of Germany. My own government work (other than winning World War II single-handed) was as a summer consultant in 1962 to the Balance of

Payments Division of the Department of Commerce, where I wrote several memoranda on momentous issues and conducted an obligatory after-hours mini course on macroeconomics for the sizable office staff.

Only once have I been significantly active in a political campaign, viz., in advocacy of the effort of Adlai Stevenson in 1960 for a third Democratic presidential nomination. I was not old enough to vote in 1944, I was too occupied and pre-occupied with graduate work to vote in 1948, and I was moving from Washington University in St. Louis to UCLA in 1952. I began voting in 1956, and, like many sensitive souls in 1952 and 1956, I was intrigued by Stevenson and doubtful of the competence of Dwight Eisenhower. (I shifted my voter registration—to which I attach little significance—from Democratic to Republican in the 1960s.) Adlai was appealing to me despite some of his economics and his being a politician. It was my feeling and hope, perhaps leavened by a touch of naiveté, that he was sufficiently civilized and sophisticated, and bright and erudite, to be approachable and willing to listen to and learn from sound guidance. In 1960, I published a *The New Republic* piece supporting him, and represented him in a debate before a Los Angeles business luncheon.



Jan Sterling

Several days before the 1960 convention, I was in a meeting of perhaps a dozen in the offices of a Beverly Hills senior lawyer to consider Stevensonian strategy. It was a presumably worldly-wise group, including Dore Schary, who had recently produced the movie, *Sunrise at Campobello*, and the actress Jan Sterling. (Jan and I sat next to each other at the bend of an L-shaped couch, and her left knee and my right knee became well acquainted.) But at one point the question was raised if we had time to place a full-page statement in *The Los Angeles Times* welcoming the delegates at the beginning of the convention. After considerable uncertain discussion of the

possibility, I gained respectful notoriety by suggesting that the lawyer have his secretary telephone the *Times* to learn the deadline for submitting the material.

One of Stevenson's law partners, Willard Wirtz, had been a Northwestern law instructor of my brother, which likely played a role in my being asked by telephone late one evening to be in charge of preparing the seconding speeches for Adlai next day at the convention in Los Angeles. I took with me to Stevenson's Sheraton West hotel a bright, literate UCLA philosophy professor. We were told that the prominent seconding speakers, including Eleanor Roosevelt and the governor of Kentucky, were entirely capable of writing their own remarks for the convention session later that day, but time was running out. My colleague and I did not try to compose full speeches, but some of our suggestions—single sentences and paragraphs—were used. (The brilliant nominating address, stirringly presented by Eugene McCarthy, was being prepared in a side room by a writer from *Look* magazine when we arrived at the Stevenson suite in late morning.) Shortly after our arrival, Adlai, looking harried and preoccupied, stepped into the large living room, but quickly left, and, to my disappointment, we did not meet.

My associate and I were rewarded with passes to the balcony of the Sports Arena, the site of the convention. Stevenson people, including Dore Schary, had managed to steal several hundred passes, and the gallery was packed with Adlaites. Following the rousing nomination address for Stevenson, there was the usual floor demonstration—but it would not stop. After half an hour of much noise and parading, the chair of the convention, in frustration, threatened to clear the cheering gallery. The tone of the crowd abruptly changed from celebration to defiance. I do not doubt that there would have been a riot if the police had tried to clamp down, but the fun and the noise quickly ended. The next day I received an invitation to meet Stevenson at a hotel reception, but I was depressed and presumed that he would not wish to deal with outsiders, so I confined myself to sending a telegram of appreciation and regret. Over several months Stevenson and I exchanged two or three notes.

About a month after the convention, I was scheduled for a research visit to the Roosevelt Library at Hyde Park. I wrote a note to Mrs. Roosevelt indicating that I would be honored to meet her. Not to my surprise, there was no reply. But one day a historian friend and I returned to our Hyde Park rooms after a morning spent in the archives, and the landlady was atwitter: Mrs. Roosevelt was driving around town looking for me! As we spoke, the grand lady herself drove up in her Plymouth, and invited me to go to her home on the edge of town (not the family mansion) for lunch. When I gently noted that a friend was with me, she invited him, too. She spoke much and without inhibition, often waving her hands and seemingly not much interested in the road. She remarked that the recent convention was more completely sewed up by the winner before the affair began

than in any other convention she had seen. (I did not mention the 1936 convention.) The house was attractive in a rustic design, not unusually large, and some fifteen of us ate lunch at two tables (I was at her table) on a screened-in porch. A distant relative of hers at the second table remarked, “Eleanor, your problem is that you have never understood rich people,” and she replied, “That’s right—I never have.” Afterward, she sat on the lower steps of a stairway and invited questions. I asked something about Harry Dexter White, but it was apparent that she did not want to pursue that topic, so I did not press the matter. Mrs. Roosevelt was highly interesting, alert, and confidently gracious. She was attractive even in appearance, with correction of her previous overbite and with soft rose-petal complexion.



Distasteful as I find most politicians and their usual ways, we can hardly ignore the political world and its inhabitants, expecting it all to go away. This is a world dominated by scarcity, sin, sorrow, and lack of sophistication. We have to confront many unattractive things and try to minimize losses. A good many years ago, armed with a tape recorder, I interviewed several dozen economists who were, or had been, in the federal government. Who were your audience, and what influence did you have? How autonomous were you in choosing topics for analysis? What were the pressures to adhere to a party line? At what level of analysis could you perform? The interviewees generally were forthcoming and candid, and the resulting long essay created a substantial stir (Allen 1977; reprinted in the present issue of this Journal). I then approached a laissez-faire foundation with the suggestion of holding a conference on how economic policy is made and how it might be done better. But the foundation officer was unreceptive, saying that his objective was to get rid of government, not to make it more efficient!

What entices people to strive for and retain high political office? All are over-eager for authority and community stature; typically, they have an instinct for the jugular and for jungle survival; a few are clever and articulate; most are impervious to manifestations of sophisticated priorities and immune to constraints and guidance of dispassionate analysis. Columnist Peggy Noonan, in the *Wall Street Journal*, suggests that presidential hopefuls are simply “barking mad.” Few who have been conspicuous contenders for major office in my time would be welcomed by me as dinner partners or next-door neighbors. But the leaders of the band are elected, and bush-league third-raters may be the best the public deserves. After being on the losing side of an election, Alexander Hamilton exclaimed, “The people, sir, is a great beast.” But the noble American experiment continues after

more than two centuries. The experiment often falters, and the condition of the republic becomes increasing precarious. Still, Adam Smith remarked that “there is much ruin in a nation,” and we lurch, stagger, and stumble along.

After completing your PhD at Duke, did you enter the academic job market? Where did you land?

Fateful Tides: The Beginning of a Faculty Career

We have been told by the highest poetic authority that “there is a tide in the affairs of men, which, taken at the flood, leads on to fortune . . .” But the tide falls as well as rises, and that can leave us “bound in shallows and in miseries.”

The winter and spring of 1951 saw a miserable tide in the university faculty market. By then, I had completed doctoral course work, passed qualifying examinations, and largely completed a dissertation during a traveling fellowship year briefly at Harvard, with two productive quarters at Chicago, and finally in Washington, D.C., mainly in the Library of Congress. I was ready to start professing in the classroom.

For several years following the unpleasant upheaval of the early 1940s, the academic job market boomed, with college enrollments swollen with returning veterans and beneficiaries of the G.I. Bill. But by 1951, faculty rosters had been well expanded, and few additional bodies were sought. An anticipated position at Florida State, where my brother was chairman of the economics department, now was not available.

The fates had not singled me out for beastly treatment. A good friend a year ahead of me at Cornell College was a striking victim. A fellow history major, he had straight-A records as an undergraduate and then in graduate work at the University of Wisconsin, a stronghold in history education. At Wisconsin, he became a protégé of a senior, prize-winning scholar, co-authoring with him a journal article and a book. But in the fall of 1950, the only employment he found was a one-year appointment at Berkeley to cover for a regular faculty member on leave. At the end of the year, and after much travel at his own expense seeking a position, he ended in the New York state university system. The position was respectable, and he became a stalwart of his campus, but it was essentially an institution confined to teaching. My highly capable friend, who had splendidly invested to establish a conspicuous career, was in a poor position to research and write, and remained in professional obscurity outside his local domain. (One assignment in his first year in the New York school was to teach a course in the

Principles of Economics. He wrote to me for suggestions of how to learn quickly some rudiments of the subject. I recommended the encyclopedic Bowman and Bach (1st ed. 1943.)

In summer 1951, presumably on recommendation by my distinguished and much-honored mentor at Duke, Joseph J. Spengler, the Central Intelligence Agency took some interest in me. I was already in Washington, D.C., and I was interviewed there by at least two high C.I.A. officials after I answered a questionnaire with the approximate thickness of the New York City telephone book. My wife's mother had been born in Czechoslovakia, as were the parents of her father. One of the questions put to me drew on that background. I was asked what I would do if, while on the C.I.A. staff, I was approached on the street by someone who told me that a relative in Czechoslovakia would be very harshly treated unless I provided some sensitive information. I replied to the interviewers that I would be vastly offended by such *gaucherie* and would be genuinely remorseful for the innocent victim—but I did not know the victim and, having recently won a war which had millions of victims, I was sufficiently inured to such suffering and was so committed to winning the ongoing cold war that I would not hesitate to tell the subversive bastard to begin immediately to pursue the shortest route to the innermost circle of Hades. The answer seemed to be received well.

I was told that the C.I.A. would want to consider me for a new unit, a group of (as I recall) perhaps fifteen young, imaginative people to brainstorm on big questions. The example given me: What would be the overall consequences and repercussions for the Soviet economy if, overnight, it happened to lose a fourth of its steel-making capacity? A favored mode of thought would be input/output analysis, which was then in vogue. I pointed out that, while I was familiar with the basic nature of input/output doodling, largely from some writings of Leontief, I had no detailed, operational expertise and probably was not mathematician enough to become proficient. I was assured that my technical innocence was not the issue, for in a “back room” there would be mathematicians and econometricians to do the grinding. My committee would provide the ideas for the technicians to work out and apply in detail.

Apparently, I was in line to be offered the C.I.A. job, but first an exhaustive background check by the F.B.I. was to be completed. Some time went by, and no one could tell me if the investigation would require two more weeks or two more months. (The owner of my Virginia apartment, a distant family friend, told me that a couple of agents had visited her and asked if she had ever seen any radical-looking literature in my place.) September was approaching when Washington University (St. Louis) contacted me. I do not remember how the contact came to pass. An academic position in the hand appealed to me more than a mysterious possible role in the C.I.A., so I accepted the offer of \$4,000 to be a Lecturer.

(Some months earlier, I was invited to visit the economics chairman at the University of Maryland, but—to his admitted embarrassment—the proffered salary was only \$3,000, as I recall.) Our side eventually won the Cold War, but maybe that war would have been shorter if I had waited to join the C.I.A.

Fran and I drove to St. Louis two or three weeks before classes began. Although I won the war in part by instructing bombardiering cadets and combat crew members, I had never taught a college course and had no lectures prepared. My chairman, Werner Hockwald, assigned me to only Principles in the fall semester and Principles plus U.S. economic history in the spring. The customers and I got along well (I was less than ten years older than their average age), but it is my suspicion that my teaching effectiveness was less than I then supposed it to be.

The department was small, with limited experience, and interesting. There were seven of us; only two had completed the doctorate, and the rest of us were candidates at schools including Duke, Harvard, Yale, Chicago, and Johns Hopkins; and it was a competent and generally congenial group, with each of the young men eager to get on with his career.

In winter 1951-52, UCLA was looking for an assistant professor. Probably again through the good offices of Joe Spengler, the UCLA chairman, Paul Homan, learned of me. He visited the Allens in St. Louis, and an invitation followed, with salary of \$4,860. Washington University bid \$4,500, but California climate and glamour were more important than salary differential in leading Fran and me to opt for another move. So, in August 1952, we headed west in our non-air conditioned Plymouth. Except for several visiting appointments over the following decades, the Allens settled permanently in the Hills of Westwood.

The UCLA department was larger and better housed than the one we left. And it had more mature talent, although it had only limited standing and, in effect, was essentially a post-war development. A few members of the staff—Warren Scoville, George Hildebrand, and Dudley Pegrum—were already well recognized, but Armen Alchian and Karl Brunner were just coming conspicuously on stage, and John McGee, like me, was at the beginning of his career. The coming twenty-five years would see the department's Golden Age, which now is history that has grown ancient.

Such experiences in 1950-52 made it clear to me that professional advancement and fulfillment is not a function solely of much investment and fine endeavor. There is, to be sure, a considerable element of meritocracy, with some appreciable correlation between performance and reward. But the correspondence is far from perfect. The tide flows out as well as in, and one's fortune can be heavily shaped by happenstance and accident and luck.

How did things unfold during the 1950s and early 1960s? What were notable developments during that time?

Did you or any colleagues have a conscious sense of direction in economic thinking, a sense of a UCLA distinctiveness?

Can you tell us about the department's group dynamics? Were certain individuals emerging as leaders (intellectually or institutionally)? Were there important conflicts within the department?

The Alchian Department

There was the glory of Greece and the grandeur of Rome, the school of Aquinas and scholasticism, the Renaissance and the Age of Shakespeare, the classical music period of Mozart, Beethoven, Chopin, Brahms, and Puccini, the Founding Fathers of Washington, Adams, Jefferson, Franklin, Hamilton, and Madison—and there was the UCLA Economics Department of Alchian.

When was the departmental Golden Age, what was the nature of the phenomenon, how did it happen, and what became of it? The story has various complex aspects, ingredients, and dimensions. The following brief reminiscences and interpretations can be only selective, suggestive, and illustrative, not definitive and exhaustive. (Readers may wish to compare my remarks to those of Daniel K. Benjamin and of Ronald H. Coase in *The Collected Works of Armen A. Alchian*, 2006).

Armen A. Alchian did not burst upon the professional stage at a precocious age. By the 1950s, he had done significant work, not widely disseminated at the time, in the Army Air Forces and at the Rand Corporation, where he long continued as a consultant. But he became widely conspicuous only at age thirty-six, with publication of “Uncertainty, Evolution and Economic Theory,” *Journal of Political Economy*, June 1950.

In the early 1950s, the UCLA department included relatively senior scholars George Hildebrand and Warren Scoville; several old-timers, including Dudley Pegrum; Alchian contemporaries Karl Brunner and Wytze Gorter; and several innocent youths, including John McGee and me. Paul Homan, widely known as the long-time editor of the *American Economic Review*, was chairman. (Homan and I had rather different degrees of enthusiasm for the work of J. K. Galbraith, but in my first semester, fall 1952, he steadfastly supported me in an investigation by a regent. A student—a former Marine—in my money-and-banking class had protested my favorable assessment of the 100 percent reserve proposal, and Paul vigorously defended me, contending that the proposal was a conservative, not radical, suggestion and a reputable idea.) The group included some splendid talent, along with lesser lights, but the department then was more promising for the

future than presently accomplished. The next two decades or so saw the blossoming of the Era of Alchian, an era which is conveniently book-ended for me by my joining the department in 1952 and my taking an absence in 1971 for a two-year stint at Texas A&M.

Despite my arrival, the department did not immediately metamorphose into one of the generally acknowledged nation's dozen finest. And it did not attain eminence and influence through deliberate and systematic implementation of a finely honed dogma and deliberate strategy. It began to evolve with several very able people performing strikingly well while slowly adding fresh troops (some of whom contributed valuable talent, skills, and attitudes), and building a distinctive broad orientation in a defining core of the group. That critical Core was a minority of the staff, but by the mid-1960s people like Alchian, Brunner, Scoville, Hildebrand, Jack Hirshleifer, Harold Demsetz, George Hilton, Axel Leijonhufvud, and Earl Thompson became the face and soul of the department in assessments by the profession generally. They were joined by the young Sam Peltzman and soon also by the established John McCall. I found much gratification in being associated with such innovative scholars and teachers—along with several, including Harold Somers and Werner Hirsch, who cannot be fully fitted into the Core.

The members of the Core did not lose their individualities. Perhaps to a greater degree than in more conventional departments, each retained his own characteristics in temperament, techniques of analysis and instruction, philosophical orientation, and idiosyncrasies. And each could be irritating to me and to each other. Alchian acted as department head one year in the late 1950s when chairman Gorter was on leave. He upset Scoville and Hildebrand by quite autonomously trying to lure Milton Friedman to our lotus-eating enclave.

But substantive debates were generally contests of those of basically equal standing and with mutual respect, and there was no serious attempt to impose party line or fealty on personnel of junior rank. In my first couple of years, there was discussion of some acrimony concerning proposed tenure for two assistant professors, but McGee and I were little touched by the conflict. On various matters, Brunner tended to be impatient and opinionated, but I soon learned that he respected a young colleague who would snap back at him. Alchian was personally less intense, but could have frisky fun. He reported to me that in a recent meeting of full professors considering me for advancement, he had expressed wonderment about my publications touching on an unusually large variety of topics. I replied that, as a student of the wide-ranging Spengler, catholicity of interests seemed to me a virtue. He agreed, but said that he felt it in order—to promote modesty among the young?—that there should be some kind

of indicated concern on the record of every candidate and that, in my case, he thought of nothing else to criticize!

The imprecisely defined departmental Core was not a socially exclusive club. For many years after my arrival, it was customary for each faculty couple to present a series of dinner parties, annually trying to invite each of the other couples in groups of three or so—five or six affairs during the year. So every couple would be in a party at almost all of the other faculty homes. One would be a host or a guest every two or three weeks. Invariably, these were pleasant and spirited evenings—but, especially in the early period of debate over tenure for the two assistant professors, appropriate attention was given to the guest list for a given party.

For the most part, the composition of the Core was quite stable, but over time some members were lost and other acquired. Turnover was probably greater among those outside the Core. Some additions to the faculty were destined by professional qualities and characteristics to be of the Core; some were not so clearly oriented and were seemingly content to remain on the group periphery.

The Core was blessed by the arrival of Demsetz, Hirshleifer, Hilton, Thompson, and Leijonhufvud in the early 1960s. Intriligator and Baldwin, who joined in the same period, were well-trained and active, but in their formal manner of work and intellectual heritage and orientation, in their techniques and philosophical inclinations, they seemed to be quite outside the Alchian Core. Among other able young men, Norman Breckner, Clay LaForce, and Fred Barron slipped comfortably into the Core province; George Murphy, Larry Miller, and Don Stout remained more conventional; Lee Hansen could have become a Core member if he had stayed, but he, along with Baldwin, Miller, Stout, Breckner and, alas, Brunner left by the mid-1960s.

What defined and distinguished the Core is a question of considerable subtlety and nuance. The leader of the impressive band clearly was Alchian. His position of prominence evolved and developed, not by his intention or machination or the extroverted personality of a self-conscious and self-serving field marshal. (Much later, when as chairman I was recruiting the eminent Jim Buchanan, I apologized to Alchian for being obliged to offer Jim a salary greater than Alchian's. Armen firmly put me at ease—after all, he had some understanding of how markets work.) Almost always soft-spoken, unaggressive, and seemingly bemused, he was genuinely curious about certain workings of the world, and he was imaginatively and innovatively bold in seeking explanations—and he was remarkably generous in helping other curious analysts. He was confident that much of previously unaccountable behavior and phenomena could be explicated by fundamental and often quite simple (when adroitly utilized) analytic propositions and techniques. The tools of Econ 1 and 2 can be powerful in

masterly hands. Larry Miller observed, with some appreciation, that Alchian “found economics behind every rock.” (Miller, a product of Harvard who was not a member of the Core, published a short piece in the *Journal of Political Economy* in 1962 indicating wonderment about the phenomenon of “the Chicago School.” Later, in a department meeting, another young man who had not risen above his Boston-area beginnings, asserted that many of the senior faculty of the northeast doubted that Buchanan was really an economist.)

The work of Alchian and of the Core generally was focused on “pricing and allocation.” (In a faculty group meditation, Pegrum once relieved himself of many words on the topic of macroeconomics. He was abruptly taken aback when Alchian finally remarked. “There is no such thing as macroeconomics.”) For years, Alchian and Hirshleifer presented a remarkable three-quarter graduate sequence, Econ 201A, B, C, with Armen giving the essence of pricing-and-allocation in the first two terms and Jack following with study of capital theory. But Alchian, with Reuben Kessel and Ben Klein, did significant work also on inflation, and Leijonhufvud and Thompson have made major contributions to aggregative discussion. And Leijonhufvud of course wrote the famous satire from which the present memories take their title (Leijonhufvud 1973).

The department in its brief Golden Age was aware of being out of step with most of the profession both in the purpose and nature of the work of the Core and in how the work was conducted. For members of the Core, Economics was to be dedicated to genuine, bone fide, real-worldly, enlightening and useful empirical problem-solving. Who is to gain what from the efforts of economists? What is the relevance and worth of their meditations and exercises? How great and widespread would be the net calamity if all the economists suddenly departed for their esoteric Nirvana? In a 1985 memorandum to me, Leijonhufvud wrote:

[Alchian’s] unique brand of price theory is what gave UCLA Economics its own intellectual profile and achieved for us international recognition as an independent school of some importance—as a group of scholars who did not always take their leads from MIT, Chicago or wherever. When I came here (in 1964) the Department had Armen’s intellectual stamp on it (and he remained the obvious leader until just a couple of years ago ...). Even people outside Armen’s fields, like myself, learned to do Armen’s brand of economic analysis and a strong esprit de corps among both faculty and graduate students sprang from the consciousness that this ‘New Institutional Economics’ was one of the waves of the future and that we, at UCLA, were surfing it way ahead of the rest. But Armen’s true importance to the UCLA school did not stem just from the new ideas he taught or the outwardly recognized ‘brandname’ that he created for us. For many of his young colleagues he embodied qualities of

mind and character that seemed the more important to seek to emulate the more closely you got to know him.

All this is not to suggest that mathematics, so prized by the Economics Establishment, was to be denigrated in the mansion of economics—but mathematics was not to be allowed to evict economics and make itself sole, or even major, inhabitant of the mansion. Mathematics, with its econometric handmaiden, was to be an aide and a tool in the service of pertinent economics—the technical medium was not to be the substantive message. It has long been accepted, even embraced, as such by sophisticated economists. Around 1954, Brunner offered an informal short course for some three of us junior people (attendance was effectively mandatory) in which we worked through portions of Paul Samuelson's *Foundations of Economic Analysis* (1947). In the early 1960s, the department deliberately sought a specialist in mathematics to round out the team, and it acquired Mike Intriligator fresh out of MIT. By a generation after Cournot, such notables as Walras, Marshall, Edgeworth, and Fisher were among those who championed mathematical techniques as an essential tool in the economist's toolbox. But used unwisely and naively, it turns economists into pretentious parasites on the rest of the community. Alchian once commented to me on a recent article he had read. It was an interesting piece, embodying a good idea, he said—but the exposition was diverted and diluted by some wholly unnecessary and distracting mathematical doodling. I once sent a manuscript on Irving Fisher to Milton Friedman, asking if he thought it appropriate to submit to the *Journal of Political Economy*. He replied that he would like to see it in print, but that it would not receive a fair assessment there because it was non-mathematical. (He recommended sending it to the *Journal of Law and Economics*, which did publish it.) On two occasions circa 1990, first-year graduate students stopped by my office to lament that they had come to UCLA to learn the sort of economics in the Alchian-and-Allen textbook, but what they had been presented was largely the flapdoodle of abstract model-building. A recipient of a UCLA doctorate had a piece rejected by the *American Economic Review*, despite the editor's approval of content, because it was not sufficiently mathematical. Even world-class Core-types can allow themselves to slip into mechanical rigor largely for its own sake. One of our graduate students a generation ago began his graduate work at Chicago with a theory course given by a budding super-star; but it was a course, he said later, of nothing other than formal manipulation, which could have been offered at any of the glamorous schools—so he transferred to UCLA. And one of our splendid staff bewildered his graduate customers by giving a fine course in applied, empirical economics—and then devoting the final exam exclusively to pure theory.

Alchian was less prolific in publishing than were the most glamorous superstars of the profession, and he eschewed ambitious self-promotion and personal empire-building. When Gerald Ford became president in 1973, he called a large conference to Whip Inflation Now. Alchian was invited to participate, but declined, indicating to some of us that staying home to play golf would be more productive than anything likely to be said in Washington.

While he may sometimes have been over-reticent to testify in support of colleagues on administrative matters, he was pleased to advise and to co-author on issues of analysis. It is my understanding that he devoted many hours to Leijonhufvud when Axel was laboring to make sense of Keynes and Keynesianism; and he is credited with much direction to William Sharpe's dissertation project which was instrumental in Sharpe's later Nobel award. Over many years, Alchian was co-author with numerous colleagues and associates, including Demsetz, Ben Klein, Susan Woodward, Henry Manne, Harold Somers, Kessel, William Meckling, Thompson, Wesley Liebeler, Kenneth Arrow, and me. And all the while the department turned out a goodly stream of personally mature, professionally sophisticated, productive and influential economics graduate students, who not only knew well the words, but could also carry the essential tune, of analysis and its useful applications. Considerations of genteel propriety preclude a sample listing of these fine young men and women, but the department, especially the Core, was a valuable educational organization at all levels of instruction, beginning with the Principles course (taught by Alchian, Scoville, Bernie Campbell, and me, among others). Obviously, "The Alchian department" was not a monolithic, all-encompassing community. There were always more numerous aliens, some of them very able people, not well assimilated into the Core. Within the Core, there was much mutual confidence in the professional orientation and personal integrity among the members. This is hardly to say that the Core was a band of brothers each with infinite patience and appreciation of all activities of all colleagues all the time. Some of the frustrations from impatience and inconsistency within the Core, along with dissension from the aliens, were eventually felt by me.

The rise of the Alchian department was mainly a happy serendipitous phenomenon, the era taking shape rapidly, gathering momentum in the late 1950s, and reaching something of a high plateau prior to Brunner's departure in 1966. It was sustained for a while by remaining troops and some fine acquisitions, including McCall, Finis Welch, Michael Darby, Klein, Joseph Ostroy, Thomas Sowell, Rol McKean, and Arnold Harberger. But the founding generation and some of the later acquisitions began to fade away. Too soon, Scoville, Brunner, McCall, Welch, Darby, Klein, Hilton, McKean, and Sowell left the department by moving, retiring, or dying. Where does one now go to find replacements for such

people? By the mid-1970s, UCLA, while still boasting much new talent, seemed to have lost most of its uniqueness—and the new generation had little knowledge of, or interest in, the history of the department and what it had been. I sensed a new and less attractive atmosphere when I rejoined the department in 1973 after two years at Texas A&M—“you can’t go home again.” In recent years, Somers, Hirshleifer, Hirsch, and Thompson have died, Alchian has become inactive, Leijonhufvud first sampled the fleshpots of Italy and is now living elsewhere in California, Demsetz is largely retired, as I am, with only Harberger, Intriligator, and Murphy, actively representing my generation.

Several years ago, a UCLA basketball star observed, “History is what happened before you were born—and who cares?” Sic transit gloria.

It is in order to revert to Armen Alchian. It is not provincialism to relate the history of the department from the early 1950s into the ’70s in Alchian terms and perspective. He was one of the superb economic analysts and teachers of the second half of the twentieth century. Other supreme luminaries of the fraternity enthusiastically acknowledged that he was one of them. In 1977, the department sponsored a two-day conference in his honor, with more than 150 attending. Among the many stars, four presented formal papers. Three of the four had properly received the Nobel Prize, as did the fourth shortly thereafter: Kenneth Arrow, Milton Friedman, Paul Samuelson, and George Stigler. In 1985, I nominated Armen for an annual University Service Award. My letter was attached to thirty-five solicited testimonials, several from UCLA colleagues and former students, and most from the world’s leading economists—the four speakers at the 1977 conference and such as James Buchanan, Ronald Coase, F. A. Hayek, Hendrik Houthakker, Leland Yeager, Arthur Burns, Gary Becker, Gordon Tullock, Roland McKean, Philip Cagan, Michael Jensen, and others. The letters were magnificent, unstinting in their praise and appreciation, couched in superlatives, and several expressed astonishment that Armen had not already received every honor the university could bestow. The following year, I nominated Armen for the Nobel, and attached these testimonials. My nomination letter included the following:

Economics is a broad discipline in methodology, as the Committee is fully aware, ranging from detailed historical, institutional, legalistic description to totally abstract, arcane theory. All such approaches, techniques, and emphases are appropriate. But there is much specialization among the members of the

fraternity. And, increasingly, the profession has dealt in rigorous, elegant manipulation, even when the work is purportedly empirical—and even when the substantive results hardly warranted such virtuoso flair. Professor Alchian is a splendid technician, and he has contributed significantly and conspicuously to general “theory.” But, in contrast to many, he has always appreciated that the final payoff of Economics is elucidation of the real workings and phenomena of the world. I know of no one at any time who has had a finer sense of how to use economic analytics to explain the world. Sometimes the explanation requires involved, complex analysis, and Professor Alchian does not fear to use the tools which are required; what is uncommon is his lack of fear in using the MINIMUM tools which are required. In large part, his peculiar genius (the word is used advisedly) is to make extraordinarily effective use of elemental, and often elementary, techniques of analysis. And a host of people—many of whom are now in strategic positions in universities, in government, in the legal system, in the world of business and finance—have enormously benefited from the tutelage of Professor Alchian. ... I present Armen Alchian as a giant—a giant who, because of his lack of pretension, is easily overlooked by laymen and even by some supposed professionals—who has greatly honored his profession and uniquely contributed to its usefulness. He would grace the distinguished fraternity of Nobel Laureates.

This story should end in triumph. But neither the UCLA Alumni Association nor the Nobel committee met its clear obligation. I wrote a two-page letter to the chairman of the campus group asking if the major problem of the Association was lack of competence or lack of integrity. The Swedes deserved a similar inquiry.

The Roman Empire Syndrome

A few of us continue through the life span to grow in beauty, grace, and charm; most tend to stagnate and then to deteriorate. Modes of automobiles and styles of architecture wax and wane, ultimately consigned to oblivion. Concerns, ambitions, and mores change from season to season and generation to generation. Nations, empires, and whole civilizations flourish for only finite periods. Fads and fashions come and go in intellectual endeavor and orientation as well as in the sphere of culture and commerce.

A sobered old member of the UCLA departmental Core can believe that he has seen the early surge of uncommon creativity devolve into comfortable

conformity. The economics faculty still includes people of intelligence, training, and technical versatility; they have made useful contributions, perhaps especially to issues of monetary policy; but they are not conspicuously differentiated from the other fifteen or so leading departments. The decline and fall of the Roman Empire was spread over at least three centuries; the UCLA Department of Economics has not fallen—and the current power group, with dubious interpretation and limited appreciation of its heritage, likely would deny that it has declined—but the Era of Alchian and the Core has concluded its tenure and most of its influence.

The disappointing and discouraging story of the departmental devolution over the past thirty to forty years pertains to many people and incidents, so it can be related and interpreted in various ways. The following selective sketch, using personal experience as my pivot, touches on teaching, both on the campus at different levels and in the general community; losses, retentions, and acquisitions of personnel; the Time of Troubles on the campus in the late 1960s and early '70s; and the ultimate disenfranchisement of the remaining Core in the mid 1990s.

Teaching

In the 1950s and '60s and into the '70s, the department was recognized as a strong teaching organization in intent and performance. Fred Barron was one of three campus faculty to receive the first annual Alumni Association teaching awards in the late 1950s; I was granted the elite campus Eby teaching prize in 1978, along with awards from the Western Economic Association and the Freedoms Foundation at Valley Forge; and others, including Alchian, Demsetz, Hirshleifer, and Leijonhufvud, clearly would have been appropriate teaching honorees.

The department put several of its most effective classroom instructors—Alchian, Scoville, Barron, Burnham Campbell (who was an able economist and useful colleague but did not publish enough to gain tenure), and Allen—into regular offering of the Principles courses. A UCLA Nobel chemist, Donald Crum, taught every year a section of his department's introductory course, for, he said, (a) it was useful to him to leaven his specialized work by frequently reviewing the wide range of his entire field, and (b) he found gratification in providing large numbers of students, most of whom would never take advanced courses, with a sound layman's exposure to his area. But newer additions to the economics department evinced no apparent enthusiasm for introductory surveys—and little appreciation of those who taught them. In a department faculty meeting, probably in the 1980s, one of the brightest of the younger people suggested that the Principles courses were mainly a waste of resources and that the

world would be more efficient by having the departmental curriculum begin with intermediate theory. I responded that a more appropriate streamlining might be to cut out much of the upper-division offerings, for they did little other than to mathematicize the economics presented in the first courses.

The department still offers Econ 1 and 2, but for many years they have been given by visitors, not by teaching stalwarts of the regular faculty. Trudy Cameron, not a member of the Core, was the last full-time member of the faculty regularly to teach Principles—and she moved away half a dozen years ago. Recently, in a seemingly aberrant gesture, a senior member of the New Geniuses volunteered once to teach a section of Principles.

For many years, Teaching Assistants assigned to Econ 1 and 2 classes have been obliged to attend training sessions. But, despite my annually teaching Principles for more than half a century, my several teaching awards, and my co-authorship with Alchian of *University Economics*, I was never asked to participate in the TA orientation program.

During the Golden Era the relations among the excellent graduate student coterie and between them and the faculty were healthy, civilized, productive. Students and teachers exhibited much individuality, and there was much variety in teaching and consulting styles. Many now look back fondly and wistfully on the personal camaraderie and professional association of those interesting and fruitful days.

As the department was finely developing in the early 1960s, Alchian honored me with an invitation to join him in preparing a textbook. He had signed a contract with Wadsworth Publishing, and it was felt that I might supplement Armen's great powers of analysis and exposition. He was a genius in utilizing price-and-allocation analysis; my teaching had included aggregative and international areas. The book was initially intended to be compact, basically putting down on paper our Principles courses. The finished product, first published in 1964 (with later editions in 1967 and 1972), was not short, and it never became as stylistically elegant as I had wished, but it was very well received by the main part of the profession (Berkeley and Minnesota being exceptions). Still, the history of publication supports Joe Spengler's warning to me that a really good textbook is not likely to sell well. *University Economics* has received much praise—including informal assurances from many people that they found in it masses of fine material for their classroom lectures while they adopted for their customers other books, more easily managed by both teacher and students. The substantive success of the book—at all levels of instruction, from freshmen to graduate students—is to be imputed overwhelmingly to Alchian, although contributions by me included monetary analysis learned from Karl Brunner, and the long section on foreign trade and finance drew heavily on lessons in the

classroom of Lloyd Metzler, audited at the University of Chicago in 1950-51. *University Economics*—and its delimited spinoff, *Exchange and Production* (1969, 1977, 1983)—became part of the public face of the department.

In the general period beginning in the late 1960s, Alchian and Clay LaForce, with moral support from me, established the Foundation for Research in Economics and Education (FREE), remnants of which are still in operation. (Clay succeeded me as departmental chairman in 1969. He held the position for an extraordinary nine years before taking the deanship of the Management School.) It is my memory that the initiating rationale was to have an organization legally separate from the department and the university, active in raising independently controlled funds to carry out activities to promote the ideals and purposes of our version of useful economics. Some of the funds were devoted to supporting members of the department and recruiting new members. FREE also produced a series of some half-dozen brief videos, *The People of Market Street*. Armen played a central role in shaping the rudimentary analytics in the vignettes; Clay surely was the major promoter and manager of the complex enterprise. The little movies were professionally produced, and they, too, added to the growing conspicuousness of the department.

In the mid-1970s, a second organization was formed, the International Institute for Economic Research (IIER). Clay and a lawyer neighbor of his did the legal paperwork, and for some years IIER and FREE shared an office suite a few blocks from campus. I was given the presidency of the outfit.

An early venture of IIER was arranging and hosting a sizable two-day conference in 1977, funded by the United States Treasury through the good offices of Thomas Willett and Richard Sweeney, the latter being a splendid former colleague of mine at UCLA and then Texas A&M. IIER activities over the following decade centered on publications, broadcasts, and radio and television interviews of the president. Several dozen pamphlets and flyers were presented in both original and reprint series, with distinguished authors, in addition to printing hundreds of radio and television scripts in various formats, including semi-annual booklets and three volumes. (The first of the *Midnight Economist* volumes was published by Playboy Press in 1981, and doubtless it would have had greater sales with embrace of my advice to include a centerfold of me reclining on a bearskin rug.)

IIER had a distinguished board of corporate leaders in addition to Alchian and LaForce, along with an international economics Who's Who of associates and advisors; it had a distribution list of some 15,000 people; it produced the *Midnight Economist* radio program (and some television) heard daily on several hundred stations; it received awards from the Freedoms Foundation at Valley Forge, and it was featured in profiles, in *The New York Times*, *Fortune*, and many other outlets.

All such activities as FREE and its People of Market Street and IIER and its publications and the Midnight Economist are here reviewed under the general heading of Teaching. For at least some members of the departmental Core, teaching was not confined to classrooms—and certainly not to the graduate and upper-division classrooms. There was a strong sense of “community service” involved, spreading Truth to as wide an audience as possible, and many letters to me over the years, inspired by Midnight Economist activity, indicate that people of myriad backgrounds can appreciate three-minute capsules of genuine economics, presented unpretentiously, civilly, and with relevance to real people struggling in our world of scarcity and sin. Many radio and television station managers and program directors are excessively fearful that economics is too difficult or dull or disputatious for their audience.

Teaching and Community Service are two of the criteria in assessing UCLA candidates for faculty advancements and promotions, along with Campus Service and Research. It is to be expected that the last of those measures, research publications, is given more weight than the other three combined.

Personnel

Through planning, effort, shrewdness, naiveté, happenstance, and the crumbling of cookies, you win some. And some you don't . . .

In my nearly six decades at UCLA, I have had about one hundred tenure-track departmental colleagues. There will be no attempt here even to mention all members of that multitude—able though many of them were or promised to become notably accomplished—much less to characterize each or assess individual contributions. This limited story pertains essentially to the minority Core—its development from the early 1950s to the mid-'60s, its continuance, with additions to and losses from its composition into the late 1970s, and then its dilution and decline in activity and influence, until its virtual extinction in the 1990s.

From the beginning of the 1950s, building on an initial contingent of Alchian, Brunner, Scoville, Hildebrand, and later myself, the faculty approximately doubled over the next dozen years; some of the additions were splendid. People like Clay LaForce, Harold Demsetz, Jack Hirshleifer, Lee Hansen, George Hilton, Earl Thompson, Sam Peltzman, Axel Leijonhufvud, and Roland McKean joined the department as full-blown or potential Core members between 1958 and 1966. (Demsetz was with us in 1960-63, then went to Chicago either for more seasoning or as a missionary, and then was induced by chairman LaForce to return in 1971.) In about the same period, the group added, among others, Bob Baldwin, Harold

Somers, George Murphy, Mike Intriligator, and Werner Hirsch—non-Core sorts, in my view, who were, in important respects, competent and productive colleagues, even if not entirely comfortable in the company and the context of the professionally-nonconformist Core.

It can be reasonably contended that the department reached something of a peak just before Brunner left in 1966, a peak which was perhaps a plateau maintained well into the 1970s. But over several years, along with the loss of Brunner, Scoville died prematurely, and Hansen, Peltzman, and McKean moved away. These losses were countered by the arrivals of Ben Klein, John McCall, Tom Sowell, Bob Clower, Mike Darby, Finis Welch, and Earl Thompson, but Clower, Leijonhufvud, McCall, and Thompson were the only ones of that group to stay with the department for a prolonged productive time. Clower added to the visibility of the department by editing the Western Economic Association's *Economic Inquiry* (1973-80) and the *American Economic Review* (1980-85). Earlier, *Economic Inquiry* had been co-edited by Somers and Alice Vandermeulen.

By the early 1980s, the roster of the Core included Alchian, Allen, Demsetz, Hirshleifer, Klein, Leijonhufvud, McCall and Thompson. This was a group of impressive competence and accomplishment. But it was a minority in a faculty of well over thirty, it was aging, and three of the members—Klein, Leijonhufvud, and McCall—were destined to leave the department over the next few years. Sebastian Edwards, Edward Leamer, and Arnold Harberger joined the team—the first two, along with Darby, eventually following LaForce to the Management School—but by then the rose was losing much of its bloom. Harberger, a late addition to the Core, is still active. It is sobering to lament how it might have been—if senior people like Brunner, Scoville, Sowell, McKean, and Buchanan and younger ones like Peltzman, Klein, and Leijonhufvud had not left us ... And if Chicago still had Friedman, Stigler, Lewis, and Coase, and if Virginia still had Buchanan, Tullock, Nutter, and Yeager

Economics departments continue to grind out numerous bright Young Scientists who are close substitutes for each other, but the ongoing supply of Core-type economists is small and sporadic. Few new grads seem to have a strong sense of, or feel for, or interest in the substance and use of traditional Economics at its best. Few see that there is to be much art, instinct, imagination, and broad erudition in significantly fruitful endeavor, that achievements of high sophistication are resultants of more than learned mechanics and convoluted techniques.

Some such wonderments and misgivings seem reflected in a long statement to the departmental faculty by Leamer in late 1988, followed by a similar and supportive message the next day by Demsetz.

Leamer recalled that when he joined UCLA in 1975, “the Department had a commitment to further strengthening of the applied fields The fields of

econometrics and economic theory would play an important role in the Department, but their function was to serve the applied fields, not to have a separate life of their own. ... The UCLA Department has a reputation ... in applied economics, particularly the Alchian/Demsetz tradition in micro economics and industrial organization. If we lose this brand name capital we will lose one of our most attractive features By carving out a distinct intellectual niche, we could proudly claim that we were 'best,' by our own standards at least. The banner of applied economics is especially well suited to this role because it can elicit an emotional commitment, even a righteous fervor."

Indeed. The economics fraternity overall is not doing well in adding Core-type analysts who—by innate talent, acquired skill, and critical masses of professional camaraderie—"find economics behind every rock." Demsetz, in a recent, brief autobiographical sketch (Demsetz 2008), recounts that his work has "focused on empirical and policy problems and on the logic of the theory that bears on these. Cold logic, imagination, and exposition by way of words, simple geometry, and basic statistics are the tools on which I have mainly relied" Economics is a big tent which should accommodate various interests and methodologies. But "heavy attention" to "technical tooling" can "offer a substitute" for genuine economic perception and dissection. "Quality of economic thinking is more likely to be revealed by the way the underlying problem to which the tools are applied is conceived and analyzed. Economists in training seem to seek a body of data that is appropriate for the exploitation of technical tools rather than one which offers intrinsic economic interest."

Albert Einstein supposedly remarked: "Not everything that can be counted counts, and not everything that counts can be counted."

The Chairmanship

I was departmental chairman in 1967-69. It was my World War III. The history of that episode is both exhilarating and devastating, and it is not widely and always accurately remembered. Indeed, upon occasion the story has been shunned and even deliberately suppressed.

There were two conspicuous aspects of those years. First, there was a recruitment binge: In 1967-68, ten assistant professors and one senior prize were hired, and the next year another assistant professor. That triumph was followed by the local Time of Troubles, sharing in various campus disruptions across the country from the mid-1960s into the early '70s—or the latter part of the department's Golden Age.

Hiring ten young people in the same recruiting season was a massive operation. Several hundred individual folders were compiled, each with resumes and several letters of recommendation. I was gratified by the extraordinary efforts of my colleagues in reviewing and assessing the mountain of accumulated information, mainly in the folders and also through a number of interviews of candidates. We knew that one of the ten assistant professors we hired in 1967 could stay for only one year—and we were suckered by his eminent department into supposing that he really was a genius. Of the other nine, some turned out to be more impressive than others, and today Bryan Ellickson and Joe Ostroy are still with us. Ben Klein was another of those garnered in the binge.

While heavily committed to filling the junior positions, we worked to seduce the eminent Jim Buchanan to join us. He wanted to leave Virginia, as Gordon Tullock had done the previous year, and he would be another jewel of the Core. (Years earlier, my economist brother added Buchanan to the Florida State faculty.) The hiring process at UCLA is cumbersome, involving the satisfying of committees and a coterie of administrators. Franklin Murphy, one UCLA chancellor in my time to earn my substantial respect, graciously and sensibly short-cut the procedure a bit, and we added Jim to the team as of July 1968.

Shortly later, in October, the Black Student Union uprising hit the department. Five BSU representatives visited me and demanded that I hire some black faculty, although they had no candidate in mind or intention of helping to find candidates with credentials other than the desired skin pigmentation. I made it forcefully explicit that the department would not recruit on the basis of race. The confrontation immediately spilled into the campus newspaper and the Los Angeles print and broadcast media. After a threatening telephone call at home, I bought a shotgun, and strung fine wire around the lawn to trip anyone storming the house. The Scovilles invited the Allens to move temporarily into the furnished apartment above their garage, but my stalwart wife, Fran, refused to be “run out of my home.” I was interviewed and discussed in various forums. The Academic Senate twice nearly censored me, and Chancellor Charles Young, who had succeeded Murphy, referred to me in supercilious manner in public. Soon after the war began, a bomb was placed at an entrance leading to the departmental offices, but it did not explode. I remarked in a television interview that the most conspicuous difference between my enemies and the Nazi hooligans of the 1930s was that the latter could make a usable bomb. Perhaps I came to appreciate in some small degree how Churchill felt in May 1940.

I was not alone, but supporters were vastly outnumbered. Clay LaForce, my vice chairman, and Warren Scoville, then a dean, were with me. Buchanan reamed out the chancellor in a luncheon meeting attended by our full professors (as I had earlier done in a telephone confrontation). It was clear that Foster Sherwood, the

chief associate chancellor, and Franklin Rolfe, dean of Letters and Science, were sympathetic, but they were constrained by their being members of the campus administrative team. Glenn Campbell, head of the Hoover Institution, visited UCLA in his capacity as a regent of the university, and we met with Young in the chancellor's office. On campus, the school paper was displeased with me, but published several statements of support from a few faculty and students. I had no direct skirmish with students other than the BSU delegation. The baseball coach called to offer immediate protective manpower, armed with bats, if the departmental office was occupied. The office staff were on edge, of course, but stayed at their posts, led by the redoubtable Janet Lipscomb.

Such support was limited, and it sometimes came from wimpish summer patriots. One faint-hearted professor in another department sent a note of appreciation and encouragement—but he asked that I not leave his message in my office, for it might be discovered by ransacking students. Alchian was on leave at Chicago in fall 1968—but the dean of the Chicago law school and Earl Hamilton sent supportive notes.

In a department meeting after Alchian's return, the session was breaking up when Armen turned to a full professor who, it was known, had lectured his seminar on how badly I had handled the BSU challenge. From perhaps ten feet away, I heard Armen say to him, "Why did you undercut the chairman?" The character was so startled as to be speechless, and he nearly ran from the room.

One of my senior departmental colleagues actively sought to work with the campus administration to resolve issues, but he was naive and worsened the situation. It was remarkable and vastly discouraging to see established scholars across the campus—especially in History, Linguistics, and, heaven help us, Law—either enthusiastically join the mob, or supinely succumb, or resist the mob wholly ineffectually. The ten young recruits in economics must have been highly unsettled by the noise and confusion, and I determined it to be inappropriate for me to try to pressure them into battle. Two other assistant professors did join the battle—in support of the mob. One of them was a product of Berkeley, whose father was the Iranian ambassador to the United Nations; the other found it gratifying to try too hard to do good.

So the campus atmosphere was poisoned, and ill effects are evident to this day. The forward momentum of the department was staggered and diverted, with camaraderie strained and dispelled.

The testiness was aggravated in considering senior faculty appointments. In fall 1968, there appeared to be an opportunity to seduce Gregg Lewis from Chicago. I wrote to my tenured colleagues: "... as far as I am concerned ... we would be blithering idiots to pass by a chance to get him. I simply am not going to have an ounce of patience with any scholar in our midst who raises the 'Chicago

school' issue. ... I am fully prepared to accept the resignation of anyone who opposes trying for Lewis for anything resembling 'ideological' reasons. ... Let this also be understood. I have some reason to believe that I can take care of myself (and even support others) in reasonably open fight. But I have had my fill of having a shiv stuck in my back. It has happened, I think, only a 'small' number of times, perpetrated by only a very 'small' number of people. But the quota of acceptable times also is 'small.' If it happens again, I guarantee a crucifixion of the villain." One member of the department—a mid-level non-member of the Core—complained in a note to me that my message "is not couched in the proper language which should be used to address members of the stature of the faculty of UCLA"—and he was "deeply opposed" to hiring Lewis.

Another case involved Warren Nutter. I had become acquainted with Nutter, a fine and courageous economist of diverse experience, and I let it be known that he would be a splendid acquisition. Not only did one of my senior non-Core colleagues warn me—"a shot across the bow," he wrote—not to pursue the matter in any unilateral manner, but Charles Hitch, the university president, wrote that, having snagged Buchanan from Virginia, it would be heavy-handed to get a second senior man from the same small department. Hitch and Nutter were on opposite sides of the debate on the prospects of the Soviet economy—and it eventually became apparent that Nutter won the debate. Shortly later, after leaving the chairmanship in July 1969, I wrote a letter of considerable vigor to Hitch indicating that he had intervened unwisely and inappropriately. Hitch did not reply directly, but he asked Alchian, who had been best man at Hitch's wedding, to try to smooth ruffled feathers.

And there was the incident of the grumbling full bulls—Scoville, Pegrum, Hirsch, and Somers. In important respects, Scoville was a member of the Core, but he shared with the others a prickly sensitivity to the claimed perquisites of seniority. The first fifteen months of my chairmanship were a period of frenetic activity, centering on the recruitment of eleven faculty. But the senior quartet let it be known—just before the BSU war, as it turned out—that they had misgivings about the appropriateness, propriety, and legitimacy of vice chairman LaForce teaching only two courses and my giving only one in the current academic year. My written reply was that their only proper concern was their own teaching load and that what was assigned to the vice chairman and me "is, properly and actually, a decision of the chairman alone." (Oddly, during that period I was, unilaterally, reducing the teaching load of full professors to four courses a year. Buchanan had requested a schedule no fuller than that. I made it explicit to other professors that I would give Jim no unique favors and arrangements.)

I well understand the feelings of marshal Will Kane at the end of the movie, *High Noon*, when he disdainfully cast his badge to the ground before the

townspeople who had given so little support as he faced and defeated ruthless enemies. In June 1969, shortly before I discarded the chairmanship, someone inserted a statement into *The Daily Bruin* that I would not be missed. As I fought to defend the faith (according to Allen theology), not everyone in the profession was offended and repelled, and several outside parties even approached me with overtures to chair reputable departments and consider other positions of administrative leadership. At the close of my chairmanship, Alchian, Buchanan, Somers, and LaForce took me to dinner.

Disenfranchisement

By the 1990s, the numbers, the activity, and the influence of the Core—as it had evolved through the 1950s, '60s, and '70s—were much diminished. The new ruling class might have gracefully tolerated the eccentric old fogies. But it was deemed more efficient to retire them quite completely.

I was the first to be disposed. There was a promotion review, of a highly able assistant professor, a potential colleague of the Core. The hard-nosed scientific technicians held him in little regard. After becoming emeritus in 1991, I had not always attended department séances, but older faculty prevailed upon me to come to a meeting of preliminary review of the candidate, a review which became an indelicate, supercilious attack. The following day, I was informed in a phone call from the chairman that a majority of tenured faculty found it inappropriate for me to participate in future meetings. The official rationale of the decision was that I—a forty-year member of the team, a scarred former chairman, a public and professional figure of some visibility, a recipient of teaching awards (and director of an AEA prize-winning doctoral dissertation—of John Pippenger)—had not given a graduate course for several years.

I am informed by an eminent member of the Core that in the mid-1990s the disenfranchisement of the emeriti was completed. Another review involved consideration of a potential invitee to the faculty. The emeriti voted unanimously against appointment; all of the rest of the group, who constituted a very large majority, voted in favor. Within a day or two, the emeriti were informed that they would no longer have the privilege of voting.

Some old-fashioned folk might deem such treatment of the emeriti as something less than refined propriety and sophisticated judgment. More events could be here recounted, with deleterious administrative and social behavior extending beyond the department, encompassing executive indiscretion and vulgarity at all levels of the university governance. For me, UCLA and its department

of economics has become little more than a place of some business, not always well conducted, and a house is not invariably a home.

In 1884, Oliver Wendell Holmes, Jr., spoke of fortunate youth having hearts “touched with fire.” He added that “... as life is action and passion, it is required of a man that he should share the passion and action of his time at peril of being judged not to have lived.” The Core, in its abbreviated time, did some living, much of it satisfying, but ending with its proffered legacy being largely rejected.

It is hardly possible to take very seriously any of the professoriate all of the time or most of them most of the time. They commonly are fearful, hesitant, and foolish when confronted by complex real issues and aggressive enemies, but they tend to screech, snarl, and spit when they perceive their territory, reputation, and perquisites to be threatened. They can pose as being valiant and principled, but they are inclined to disperse and camouflage themselves upon hearing the first volleys of significant battle.

With respect to Economics specifically, the professional arena now is largely one of gamesmanship in technique. My advice to my grandchildren and great grandchildren approaching college will be to expect little from the economics department at their chosen school, it probably being best to take only the Principles sequence, and yet to read and discuss the kind of economics that inspired the Core at UCLA and that we ventured to enhance and impart to others.

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About the Authors



William R. Allen has labored primarily in the scholarly fields of international economics, monetary economics, and the history of economic theory. He has educated the public through his popular writings and radio work as “the Midnight Economist.” After serving in the Army Air Corps, he obtained his A.B. (1948) from Cornell College and Ph.D. (1953) from Duke University. He taught at Washington University prior to joining the UCLA faculty in 1952. Like the economists interviewed in the present paper, he has worked for government as consultant to the Balance of Payments Division of the Department of Commerce. He has received teaching awards from the UCLA Alumni Association, the Western Economic Association, and the Freedoms Foundation at Valley Forge. He has been Vice President and President of the Western Economic Association, Vice President of the History of Economics Society, and Vice President and a member of the Executive Committee of the Southern Economic Association. Among his many publications is his *University Economics*, coauthored with Armen Alchian. From 1978 to 1992 more than 200 radio stations carried daily broadcasts of “**the Midnight Economist**” written and delivered by Allen. He has been Chair of the UCLA Department of Economics, President of the International Institute for Economic Research, Vice President of the Institute for Contemporary Studies, and Associate of the Reason Foundation.

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Character Issues: Classic Reprints

This is a reprint of William R. Allen, "Economics, Economists, and Economic Policy: Modern American Experiences," in History of Political Economy, Volume 9, no. 1, pp. 48-88. Copyright, 1977, Duke University Press. (www.dukeupress.edu). All rights reserved. Used by permission of the publisher. A couple of very minor changes have been made to the text, with the author's approval. We have preserved the reference style (footnotes) of the original. At the end, we have added an up-to-date "About the Author." We thank Jill Scharl for typing up the piece.

Economics, Economists, and Economic Policy: Modern American Experiences¹

William R. Allen²

ABSTRACT

Economics has come of age in the 1960's. ...the Federal government has an overarching responsibility for the nation's economic stability and growth. And we have at last unleashed fiscal and monetary policy for the aggressive pursuit of those objectives. ...Interwoven with the growing Presidential reliance on economists has been a growing political and popular belief that modern economics can, after all, deliver the goods.

—WALTER W. HELLER³

1. The Ford Foundation generously provided a grant which made possible the interviews on which this paper is based; the final draft was prepared during the tenure of an Earhart Foundation grant.

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3. *New Dimensions of Political Economy* (Cambridge, Mass., 1966), pp. 1, 2, 3.

A major problem of our time is that people have come to expect policies to produce results that they are incapable of producing. ...we economists in recent years have done vast harm—to society at large and to our profession in particular—by claiming more than we can deliver. We have thereby encouraged politicians to make extravagant promises, inculcate unrealistic expectations in the public at large, and promote discontent with reasonably satisfactory results because they fall short of the economists' promised land.
—MILTON FRIEDMAN⁴

I

It is comforting, even if not wholly realistic, to suppose that we study economics most basically in order better to understand the world. For some, a largely abstract comprehension, which subsumes conditional prediction, is ambition enough. Others aspire to empirical, operational prediction. And for a more limited group this prediction in turn appears to make feasible some degree of discretionary control—including, for net good or ill, control by governmental authorities. Economics has had much of “political economy” in its history. The community seems to suppose that it can obtain useful policy guidance and administration from the economists it subsidizes—and rarely has the economics fraternity discouraged such supposition. Economists have, in various ways, tried to supply what has been demanded.

We have an enormous literature on economic policy targets and on policy tools and their proper assignment and utilization; in addition, we have been blessed with a few attempts to formulate a general “theory of economic policy.” But the policy literature, which purports even in its most abstract statements and most esoteric modes to resolve actual problems, typically aggregates selected variables too grandly but encompasses the world too narrowly, categorizes too inaccurately, and specifies too simplistically to provide real guidance to real economists in real bureaucracies. And from time to time a member of the fraternity relieves himself of a bit of Olympian poetry about the universalistic requirements of the Master Economist in directing mankind past the pitfalls of the valley of scarcity. But what do working economists—not all of whom well resemble Renaissance men—actually do in the government?

It has been my interesting fortune to interview—with permitted use of a highly visible tape recorder—a number of economists who have been active at

4. “Have Monetary Policies Failed?” *American Economic Review* 62 (May 1972): 12, 17-18.

some organizational level or other in the determination or the implementation of national economic policy, mainly in the United States. Necessarily, my sample is modest in relation to the very large and varied universe; almost as many economists have been in government for at least a short term as have written Principles texts—and it is to be hoped that economists have done less harm as civil servants than as textbook authors. My group of some sixty interviewees differ in age and experience and governmental rank. Most of them basically were or are academicians on temporary government assignment, but several are in permanent government careers, with almost all literally in government, but a few being outside consultants of some sort.⁵ Seemingly in considerable candor, they talked about *what* is done by the economists in government, under what *circumstances*, for what *audiences*, subject to what *constraints*, with what *analytics*, with what evident *impact*—and about how governmental decisionmaking or the advising of decisionmakers might be done better.

II

It would be exceedingly innocent to suppose that most or even many economists in government devote their major energies to quiet, imaginative meditation on the massive problems of the universe or to the construction and utilization of elaborate, elegant models, either of theory or of econometrics. Government life typically is not the contemplative life of the logical and independent mind, with scholars of maturity and continuing accomplishment systematically

5. There are uncounted hundreds of “economists” and “economic researchers” in the federal government. Probably most and perhaps all of them fully earn their daily bread. But a large proportion are essentially clerks and research assistants. The interviewees in this project are relatively *senior analysts*—up to ambassadorial level, cabinet members, and immediate Presidential advisers. Further, the focus here is heavily on *policy advising*. References to “government economists” pertain to those policy-making and policy-advising economists who were interviewed, along with those to whom the interviewees themselves refer. The Council of Economic Advisers and the Federal Reserve are each represented by about a dozen interviewees; the Departments of State and Commerce each provide about half a dozen; in all, more than fifteen departments and agencies are included. Some two-thirds of the interviewees were known to the author by only name and reputation prior to the interview; perhaps fifteen of the total group can be described as close acquaintances. Each individual is an obviously appropriate participant in the project, but the group can hardly be representative in a formal statistical sampling sense of the uncertainly delimited universe of “government economists.” Rather, it is a numerically nontrivial set of commonly conspicuous people who have participated in policy-advising activities at various responsible levels and who graciously submitted to the inquiries of an amateur but systematic interviewer. It may be added that for all of the diversity of government agencies, dates of government work, nature of responsibilities, and philosophic orientation represented here, these people provide a great degree of unanimity of view on the issues of this study. The interviews took place between October 1972 and April 1976.

bringing their accumulated wisdom and developed expertise freely to bear on the policy issues of today and on the shrewdly anticipated problems of tomorrow.

In the seemingly typical situation, government economists are quick-draw specialists, shooting from the hip: gather a few data and prepare a memo by tomorrow morning—if not by this afternoon. In this commonly hectic and sometimes frantic scramble, the topics can be significant and complex, and the audience can be high in the table of organization. Paul W. McCracken, former member and later chairman of the Council of Economic Advisers, observed that

one of the problems for an academic...is to shift gears in terms of the time dimension and recognize that we may get a problem today on which we must have a memorandum...this evening, where the academic's inclination would be, well, I ought to have at least a semester to do the work. ... Time pressures were severe.⁶

G. Warren Nutter, while Assistant Secretary of Defense, found that

under the pressure of the need to make immediate decisions, there is never an opportunity to study a problem in the sense that an academic wants to study a problem. ... The decision has to be made quite often within a matter of minutes. [By the time you try to] gather all the facts and apply a sophisticated model...it's a new problem—the momentum of affairs will have already changed the nature of the situation. ... In the academic world, you think now and decide never; and in the government, it's just exactly the other way around.

In the government experience of Lawrence A. Fox, formerly Deputy Assistant Secretary of Commerce for International Economic Policy, “there is absolutely no time for the policy people to contemplate...there isn't any time really to think through a position.” It follows that “a constant flow of analytical reports coming along from staff” is required for informed decisionmaking—but “in the Commerce Department...we really have a minimal kind of research operation,” and “characteristically, in my position, there isn't sufficient time for reflection and consultation with the people who have done the staff work.” (More recently, in spring 1976, Fox has noted that while the political appointees still do not have time to think, the research function has been significantly strengthened by a restructuring of the former International Economic Policy Staff into an enlarged Bureau of International Economic Policy and Research.)

6. A transcript of this interview is included in a pamphlet, Paul W. McCracken, *Reflections on Economic Advising* (Los Angeles: International Institute for Economic Research, March 1976).

With commendable versatility, the government economist not only does much shooting from the hip, he also puts out fires: crises, big and small, must be dealt with right now. For Gardner Ackley, while a member and then chairman of the Council of Economic Advisers,

the standard practice was responding to emergencies and dealing with problems between now and tomorrow morning, sometimes meeting all night. The typical deadline for things that were requested was a few hours. ... There were opportunities from time to time to do some preparation, but it was the exception rather than the rule.

Burton A. Weisbrod, when a member of the Council's staff, was instructed at 11:00 one morning to prepare an analysis by 4:00 that afternoon of "the economic payoff from the Peace Corps." (Weisbrod found the experience disconcerting but instructive: "you can always do *something* under any kind of constraint." Further, he took consolation in the thought that with "rapidly diminishing returns," having four days or even four weeks might not have greatly improved the research product—"now, if you had four years, you could probably do something more." W. Lee Hansen, another former CEA staff member, gained the same impression: "Maybe if you had a year for a project, you could do it right. But often, I think, eight hours or one week was probably adequate for what you could really do with it.") Robert J. Lampman recalls remonstrating to the CEA chairman that "you can't put anything down in an hour or a weekend" on a large, complex topic. But the chairman replied that "if we don't someone will." And he argued that economists should not defer to non-economists on economic questions. Still another CEA staff member, J. Carter Murphy, in his first week on the job was instructed by the Council chairman to prepare a memorandum on the balance of payments impact of a West Coast dock strike. Murphy mused that that should be an interesting project of perhaps four to six weeks; he was informed that he had six hours—including typing time. (The memo was to be provided to government attorneys, who were to seek a Taft-Hartley injunction in court the next morning. The legally specified basis for the injunction was demonstration of existence of a national emergency. So, first the Administration made the decision to claim a national emergency, in order to obtain the injunction, and then economists were to provide supportive data and analysis.)

This latter incident was mentioned to the director of an "analysis" office in one of the major departments, who responded:

We have that all the time. It's kind of silly. This is almost the order of the day, really. Time tables are very short. ... [The current Secretary] never gets

around to anything until Monday for Tuesday morning, and so people work frantically all day and literally often all night to have it ready for Tuesday morning. ... So we had to pull this thing together, rewrite some, edit it, get it typed, reproduced in umpteen copies, because everything has to be in umpteen copies. We had about 95 pages in about six hours ... I had the whole staff working on it. Well, we did it, but you're not very proud of it. ...I can cite frantic things...papers fell all over the floor, and we were all crawling on the floor getting our particular bits. ... It is really incredible, you just can't believe it...it was laughable...really wild...its is appalling, and...sometimes these papers and things that are put together are the basis for policies and decisions which are made.

Inevitably, in this vale of scarcity, uncertainty, expensive information, accident, and nefariousness, the government official will be confronted by something of a stream of sticky surprises calling for immediate responses. But some are convinced that the size and rush of the stream could be substantially reduced by quite unremarkable foresight and initiative. A senior economist in the Federal Reserve, Samuel Pizer, complains:

I think it is amazing, considering the number of economists employed in the U.S. government, how little there is that seems to me to be the product of having taken a decent amount of time to prepare lucid, well constructed documents and how much time instead goes into scrambling to produce a position paper on order, even though the fact that these position papers will be needed is readily foreseeable a long time before. It seems to me, over and over again, we get awfully close to the point of position-taking before there is staff input. There are many problems which have been kicked around for years on which there are still not, in my opinion, decent position papers. There is too much scurrying around.

Neil H. Jacoby, former member of the Council, holds that "Sam Pizer is exactly right on this" and recommends "a separate office...reporting directly to the President" on contingency economic plans,...just as the Pentagon has contingency military plans." Problems of effective anticipation are not confined to economists. John H. Jackson, while General Counsel in the Office of the Special Representative for Trade, found it difficult to "do things that have an impact for, say, a year from now or two years or ten years from now—things tend to always be too immediate, next week's meeting or next month's meeting..." While it is possible to do a better job of looking ahead and minimizing surprises,

a lot depends on the very high level people and what they encourage. ... What it takes is a man who has some vision longer than next week's affairs and who is willing to deprive some of the resources that theoretically are needed for day-to-day activities in order to man or staff some resource base for the longer range and then to give him some head. ... That's why I happen to think universities are so important, because they can step back from that, and they can allocate resources on the basis of non-bureaucratic judgments.

While the CEA has had only limited success in identifying "the problems...that were coming along...and...laying the groundwork in advance," Ackley warns that

Responding to problems with only five-hour deadlines should [not] be overstressed, because many of the problems were continuing problems...even though the deadlines with respect to a particular decision may have been short, often the problems were of serial nature, and there were opportunities to sort of build cumulatively the education of the President and the development of the policy.

In similar vein, J.J. Polak, Economic Counsellor and head of the Research Department of the International Monetary Fund, finds that

continuous work on a subject enables you to have a background from which you can derive judgments right away. [Commonly,] problems that come for immediate action are really new versions of problems we have met five or ten or fifteen years ago, to which you have been addressing yourself for a long time.

Nutter, rather less sanguine, has "no doubt...at all" that people in government can do better than they have done in foreseeing difficulties and preparing for them, and "any governmental policy that allows itself just to be carried along, drifting with the course of events, is going to be a catastrophe." Still,

operations always tend to overwhelm plans. Once you are in the midst of a role in process, it always seems that you are so preoccupied with having to put a quick fix here or take care of this problem there that nobody really can be insulated to sit down and think and solely think. In any event, it's a kind of futile enterprise. I think in the political operation to imagine, to conceive, that what you ought to have is two groups of people, one group over here that are running things and meeting the day-to-day problems and another

group over there that are just thinking and planning ahead, is a mistaken conception. You can't do that, because you can't make sensible plans unless you have your fingers involved in some of these problems, too.

Even in think tanks, operational pressures can detract from considerations of broad and long-term possibilities, says Arthur S. DeVany, formerly with the Center for Naval Analysis. A group at CNA was "designed...to look at...a global picture...be a world futurist." But the director of the group, even with "his charter to be absolutely free of short-run work...found himself wrapped up in the day-to-day staff work and the immediate...pressing decisions—do we put the fleet in the Indian Ocean."

Along with *ad hoc* fast shooting and conflagration dampening, there are the regularly scheduled reports to prepare—annually, quarterly, weekly, daily, perhaps hourly. Even if one finds no reason to doubt the gross worth of all this busyness, their preparation absorbs a huge proportion of the time and energies of many people. A former CEA staff member, Richard N. Cooper, notes that the Council's Annual Report is

essentially a book written in about six weeks...constantly revised as you go on. ... Sometimes the Report had to discuss in some analytical detail decisions that were not finally taken until a week before Christmas. ... It was really a continuous, rolling process of drafting, cutting and pasting, revising, redrafting...these drafts were all sent around the government for comment by the relevant agencies. Then we would have to collate those comments, decide which suggestions to accept, which ones to reject, check them all out by phone. If the agency thought very strongly about some suggestions we were rejecting, they would escalate a bit in the hierarchy.

It is commonly claimed by office and agency directors that intellectual initiative on the part of staff members is not suppressed and, indeed, is welcomed. Preparation of papers for submittal to professional journals (after clearance) is sometimes encouraged. But where time and energy are quite thoroughly absorbed in nitty-gritty activities of the moment, research initiative will not often be conspicuous. In one "analysis" office, there is not a single full-time "pure" researcher, according to the director, and

there is really no research which you can call research. Now, sometimes we are asked to fill out forms on what research is being done. ... But most of it is pretty practical, short-term work; it's not research in the university sense. ... Universities still provide a much more...comfortable haven for people who want to do research. ... Once in a while one of our bosses will sort of

encourage us a little bit in this. But they forget about it next week when they need something—it all goes out the window, and this is the way it goes on through the year, so in the end you don't really get anywhere.

Similarly, in the experience of Evelyn M. Parrish, Chief of the Balance of Payments Division of the Commerce Department, “analysts get more and more absorbed in the problem of collecting...and preparing statistics. ... So, even though from time to time we hire people who are only to be analysts, sooner or later they are absorbed into the system.”

Robert E. Baldwin, one-time Chief Economist in the Office of the Special Representative for Trade, found his staff

too small to undertake any elaborate analysis, and there are too many day-to-day problems. ... Where I think this should be done, of course, is on the outside in the academic places. I don't think you can do very good research of a real academic quality in the government. As soon as the agency head finds out he's got a smart guy, that guy not only can do a good research job, usually he is able to write the best memo in one day, and he will be usurped for that purpose, and he just won't be left alone.

Weisbrod agrees:

...don't fool yourself in thinking that you can get any serious research done within government. I think that's virtually impossible, for there are too many pressing demands on people, and long-term payoffs—where long-term means even six months, let alone six years—are viewed as too futuristic: we've got problems today to deal with, we can't spare you to work on something for the next six months. ... I think that long-range research of the type we associate with the academy cannot be done inside the government, both because people cannot be shaken loose for a long period of time, and, second, because there are too many policy constraints that get put on them, so that researchers are not allowed to look into certain kinds of things.

III

In performing their chores—the large and the trivial, the regular and the *ad hoc*, for audiences of different sizes and natures—government economists are subject to various constraints. We all must act within limitations of many sorts and

in the light of a variety of incentives, but the world of the government economist can be, and typically is, very different in important respects from the world of the academic economist.

Even the visitor to government, if he wishes to be effective or at least to avoid total frustration, will find it expedient to try to be accepted by and to work with the permanent government personnel. And often acceptance does not come easily. The academic economist evidently is commonly looked upon as an overinnocent and overpaid interloper, gamboling in on a brief quasi-vacation junket, who proceeds to grind out more or less elegant but quite useless answers to questions which are, at best, only marginally germane to the purported real work of government. "We were something like the 90-day wonders," Weisbrod recalls:

we came from the outside, and we were suddenly in the position of sort of looking over somebody's shoulder where that somebody had been there for twenty years, knew all the ins-and-outs of the things, and we were...saying, well, why don't you do it some other way? ... So I think that there were varying degrees of hostility or resentment. How great those degrees were was probably largely a matter of personalities. ... One reason why there wasn't more hostility than there might have been...[was that] they also knew we weren't going to be around very long.

But even the brevity of visitors' appointments may engender suspicion, Hansen suggests, for it was felt that "these young punks...won't have to live with the consequences of whatever recommendations they make." While "we, as outsiders, could come in and raise" certain elemental and analytical questions which bureaucrats and their own "somewhat captive" economists often found it psychologically and tactically inconvenient to raise,

you have to show some humility about it all. ...you have really got to work with them and gain their confidence, at least to some degree, so they will listen. ... On the other hand, if you work too closely with them for too long..., then you become an extension of them, and then you have lost your effectiveness.

A warning against alienation comes also from Thomas D. Willett, former CEA staff member and current Director of Research and Senior Adviser for International Monetary Affairs in the Treasury Department:

If a smart-aleck economist comes in too brashly and scares the bureaucrats too much, it can be very counterproductive in the sense of building up their

hostility toward more “reasonable” economists and will reinforce biases they have against academic types.

How far one goes in differentiating his mode of behavior, in specifically adapting to the particular environment, may turn heavily on whether one plans to stay in the world for a prolonged period. Believing oneself to be only a short-term visitor in government, with an avenue of return to academe or some other alternative, apparently can do wonders for the maintenance of a free spirit and of a manner of independence. Most of the CEA staff, recalls Weisbrod,

consisted of people like myself, who were there for one or two years, and that gave us an independence that I thought was very important. ...we could say what we thought—sometimes it was foolish but other times it wasn't —...because we didn't have to worry about the long-term consequences. ... I didn't have to worry about whether somebody in another agency got angry with me.

This view is supported by Willett: not only can temporary Council appointees “call them as they see them without worrying about what that's going to do to the advancement of their career,” but “one of the functions is to come in and ask silly questions a lot of times—maybe two-thirds of the time they may be silly, and another third they may be something the people haven't thought of. I think...it's essential to have a healthy component of in-and-outers.”

In the view of Cooper, the members of the CEA staff, most of whom were academics,

did not have the usual concerns that civil servants have about their own career in government...they were more forthright in dealing with other agencies. They spoke out more frankly, perhaps, than civil servants would on issues on which CEA had a position. Secondly, I dare say they spoke out more forthrightly with respect to their own personal positions, even when CEA did not have an agency position or even when there were shadings of disagreement within CEA—making clear that my own view is thus-and-so although in this area I can't count on my boss to hold the same view. I think there was more of both of those things at CEA than at the typical government agency, where people stay in line or are kept in line.

While largely agreeing, elaboration is provided by Don E. Roper, of the International Finance Special Studies Section of the Federal Reserve Board: visitors and consultants, too, are not immune to the temptations of designing their remarks to enhance the probability of being invited to return. C. Fred Bergsten,

formerly Assistant for International Economic Affairs in the National Security Council, agrees that “in any job...the more options you have in your own life, the more forthright you are just bound to be.” And from the viewpoint of the advisees, “that cuts both ways”: on the one hand, “his superiors should view it as beneficial, because it means they are going to get better advice from him; on the other hand, his superiors can, therefore, not count on...unquestioning loyalty from him.”

None of the interviewees held that he had been so bound to the support of a “party line” that there had been a clear (or at least fatal) violation of his “professional integrity.” But here we are in a subtle area, and unconscious refuge may be taken in semantic niceties and delusions. One may sternly refuse overtly to falsify—and yet, if he stays with the organization, tailor his substantive activities and choice of emphases in a manner intended to minimize his conflict with the rest of “the team.” In the CEA, “generally there was no party line,” Weisbrod says, for “very often we were in issues at an early stage where there was no policy.” But “where there were policies..., there surely was what you would call a party line, and it was definitely constraining.” In a hierarchical arrangement, with communications directed strictly through channels, one’s immediate administrative superior can inhibit the researcher and mold the substance of his work. In an effort to be heard, a sense of tactics may shape professional activity. Walther Lederer, formerly head of the Balance of Payments Division of the Commerce Department and then with the Treasury, observes that not only do “people like to be liked,” but they “like to feel that they have some contact with the higher level.” Roper suggests that agency heads “don’t have to say anything for people to build in important constraints on what they do.” One may well “shy away from attacking areas...or conclusions that the [heads] wouldn’t like. So that does influence...research.” Thomas G. Gies, while with the Federal Reserve, observed that

success as an administrator required that you would have to be not too deviant from the party lines of the strong people within the organization, and this, then, is transmitted back to the people in the junior staff positions. If I were to write a memorandum...which represented a departure and a disagreement with established position, my boss would file it, he would not carry it forward. ... You just don’t like to butt your head against a wall, you do want your memoranda to get...read, and if you know that your boss is never going to include that memorandum of a particular kind, I think you would tend not to write that sort.

Analogous considerations are present at all organizational levels. As Director of the Treasury's Office of Tax Analysis, Harvey I. Brazer found that "our job was to prepare convincing cases, in a kind of advocacy role, once we had decided," in conjunction with the highest Department officials, "what our policy position was going to be." And "every once in a while there was a small—perhaps not so small, perhaps I only rationalized it as small—compromise with integrity." As a Treasury consultant to the CEA, Brazer argued against inclusion of a statement on the alleged benefits to the United States balance-of-payments position from a tax cut, a statement which was proposed for the Annual Report.

But it was felt that we had to say it, so we said it—and I even drafted the paragraphs, on orders, so to speak. ... I took the attitude that I didn't mind losing some small ones if I could win the big ones, and I suppose I felt that, well, if Walter Heller and Gardner Ackley and the others could take that kind of statement, I could, too, particularly since they were identified with the Report; I wasn't...there were certain kinds of rhetoric—and I guess there always is in government—that some people feel it necessary to pursue to gain support for a given position. ...we'll say almost anything...that Mr. Mills thinks is necessary for us to get 219 votes in the House, because without 219 votes in the House, there's nothing you can do. So you just sort of swallow it. ... It's a kind of political tradeoff, I suppose. And I think there's a danger in people staying in that kind of job too long—they begin to accept these tradeoffs with too much complacency—...then the integrity of your position is pretty much bound to disappear.

Lampman recalls that, in preparing the CEA Annual Report,

staff members...would have to supply language for an idea that they found repulsive, for example, language that would appear to support the farm price support program without at the same time appearing to endorse it fully. And so you have a lot of waffling and weasel-wording of various sorts—and this was a recognized art in government that outsiders had a hard time dealing with. On occasion, it seemed to us most ludicrous, and the outside or academic types around the Council would almost get hysterical at times, and I think that some of us were glad that we didn't have to sign the Report.

Lampman provides another illustration of the adjusting of economic argumentation to political dictates:

Kennedy expressed interest in having something about poverty as a theme of his 1964 campaign for re-election. Now, you can't possibly then say that you are going to treat it as a purely academic or technical question. It starts off with this issue of political emphasis, so the question all along was how do you marry a certain kind of commitment to a President...with some notions of feasible and believable and credible theories of causes and consequences.

Prolonged adjusting to Administration positions and defending of policies which one has helped to implement, whether or not with conviction, all in a situation which tends to deteriorate intellectual capital, may preclude much imagination and innovation. In the opinion of Harry G. Johnson,

people who go into government on a career basis become...stale in their ideas, looking for ideas which really serve the Administration or what they think the Administration wants. ... It is very difficult to resist the temptation to try to take credit for policy, and the pressures on the economist then are to try to shape his thinking so that he comes up with a policy which is saleable. [In addition,] they lose touch with the subject. ... The people in government typically aren't all that good economists by the time they get somewhere. And what they're trying to persuade you is that bad economics is good politics and that they are better at the politics, therefore, you should accept the bad economics. [Since] by the time you get to the position of responsibility, the ideas you formed twenty-five years ago when you were a fresh and eager student are pretty well rigidified, ... I wouldn't expect innovation from that crowd, at all. ... First, you never get any time to think about fundamentals. Second, you make your career by implementing a policy. It's pretty difficult to say the policy you've spent your life implementing is the wrong one, let's change over.

Bergsten indicates a similar view with a much shorter time horizon. One goes into government "with a bank of intellectual capital," but within two years "you tend to get stale and perhaps most importantly, start spending most of your time defending what you have done rather than doing new things."

An economist who was long with the RAND Corporation, Roland N. McKean, feels that

it's very important to have options in the private sector of the economy or you do start calling them as you think your superiors would like to have them called, unless you are a very courageous guy—in which case, you don't survive there, either. ... It's futile to say that government economists should

not pay attention to what their bosses want. They simply won't survive...and they will be replaced with non-economists who will carry out these functions.

Lederer suggests that

in any kind of bureaucracy...the research worker feels under an obligation to supply the boss with an answer to any kind of question that the boss may want to ask. And they don't dare to tell the boss that sometimes you can't have an answer. ... [Further,] certain types of answers have found more favor with the higher level than other types of answers.

The pressures tending to mold a common staff orientation need not be (and evidently rarely are) calculated and overt. McKean suggests that

it manifests itself mostly...in a natural selection process within almost any organization. Most people find it uncomfortable to be consistently going against the party line or to have the disapproval, however subtly or mildly it is expressed, of their colleagues if they keep going against the generally accepted belief, if they keep swimming upstream, so to speak. And as they find this uncomfortable, nobody kicks them out, but a large percentage of such people, the mavericks, will leave.

And this process of coalescence of, and institutional domination by, similar-thinking individuals can take place without people "losing their integrity...either in the management end of it or in the worker end."

Others agree that there need be no deliberate design in order to achieve a comfortable, confident commonality of perspective. Probably a few people, Gies thinks, can maintain the ability, intellectual and psychological, to "shift gears" back and forth between the "isolated purity" of the independent analyst and the "current involvement" of "political affiliation." And most "at the Board of Governors level" tend to "become completely immersed" in policy; they cannot "disengage and do sort of non-associated thinking about financial matters." The immersion tends to result in captivity. William P. Yohe, who has served as a Federal Reserve consultant and resident scholar, observed a "complete turn-around" within "a few months" of a person who joined the staff of the Board, with

overt disavowal of much of what he had previously thought and said...he became a captive of the thinking of the Board...an intellectual captive. ...these people somehow come to think that they and they alone are

capable of rendering judgments on proper conduct of monetary policy and proper evaluation of past actions.

Few would hold that the bureaucratic natural selection process alluded to by McKean is invariably and entirely unfortunate, much less dastardly and damnable. The point at issue goes far beyond mere personal compatibility with and genial acceptance by associates—it has to do, more basically, with influence, with acceptable contributions to the joint effort, with avoidance of diluting and distracting the overall program. For a team member, “shape up or ship out” is not inherently a monstrous rule. But there can be the temptation to shape up by suppressing one’s own best judgment when it would be the better contribution to ship out. And to stay without shaping up (in the consensus view) can result in distress which does not clearly yield gain to anyone. There may be no satisfying solution to dilemmas of when to fight and when (and how far) to acquiesce—and when to resign. Hansen found that

one ended up often doing things he thought were unimportant, were not being correctly specified, or where there might have been some answer that was desired. [But] you more or less make a commitment, and you come down here for a year, and you have to hold your nose part of the time. ...while you could object and you could resign, the costs were rather high from a personal point of view.

Johnson suggests that such dilemmas are much more fretful to some than others:

It’s partly a matter of temperament. ... Some people are quite happy compromising, others aren’t. I think that there is a self-selection process involved, that people who know that they won’t compromise and feel that it would be a sacrifice of their integrity to compromise won’t take the job in the first place.

But there are those for whom “this problem of accommodating one’s self to a policy position with which one does not personally agree” can be extremely difficult, McCracken assures us, as in “before a Congressional committee having to defend a policy which I had opposed within the Administration.” Should one then resign?

Not necessarily. ...you don’t quit the team the minute the quarterback calls a play [you consider a mistake. But] there does come a time when the thing...to do is to get out. Probably anyone on the spot there has a little tendency to rationalize staying on too long.

Jacoby agrees and adds that anyone “who keeps his job at *all* costs is not only damaging his own integrity, but he is sabotaging the democratic process.”

Potential dilemmas in this context may be partially resolved by distinguishing those circumstances in which dissenting argumentation is tolerable and even to be encouraged from other circumstances in which an apparent unanimity is in order. Debate within the family; but close ranks in public support of whatever decisions have been made. Ackley reports:

we lost lots of arguments and, having made our case and having had it rejected for what were at least plausible reasons, not casually or capriciously, we had to avoid indicating disapproval. ... Obviously, when an issue was decided, you couldn't very well go around making speeches saying the decision was wrong, at least if you expected to remain on the job and be useful. ... [And] once a question had been really seriously considered. ..., it was useless to keep coming back and asking for that decision to be re-opened, at least too often.

It is a different situation for the staff economist when a decision is announced from rarefied heights of administration *without* calling for preliminary debate. There may be even a placing of certain potential policy options off-limits. During the Kennedy and early Johnson Administrations, the possibility of promoting and adopting an international financial arrangement of freely fluctuating exchange rates was not to be considered. Indeed, even the term “dollar devaluation” was never to be used. According to Weisbrod,

the issue of floating exchange rates was regarded as simply verboten, that is, you did not talk about it. ... apparently...there had actually been a decision that the term would not be used, that it would simply not be discussed. This was an issue which was closed; the President didn't want to talk about it, he didn't want to hear about it. So there were these policy constraints, and one simply had to accept them if he was going to stay on the job.

If one went ahead and prepared a memorandum on the matter, “it would have been regarded as, at the least, foolish—a foolish use of time and energy.” “Within the range of normal agenda,” James Tobin, when a member of the Council of Economic Advisers, did not find “anything that we thought was out of bounds for us to suggest or talk about—not any sacred cows. ...” Still, “at that time” in the early 1960's, “it would have been quixotic, and on a tactical basis it would not have helped,” to push the case for fluctuating rates in discussions with the Treasury. Helen B. Junz, Adviser in the Federal Reserve Board Division of International Finance, thinks that “it would not require enormous amounts of

courage” to propose suggestions that are not “terribly acceptable”—at least, if the suggestions are “within the realm of realistic policy options.” It would have taken a boldness a few years ago to advocate freely fluctuating rates because “it wasn’t a viable policy option at the time.” To be “very much on the fringe” of policy advocacy is to risk being “considered sort of a little bit nutty.” And “there are quite a lot of people who do not like being on the fringe, and who therefore would prefer to work in areas that were much less controversial.” Hansen found that not only was the idea of unfixing exchange rates “regarded as silly,” but inflation analysis “was all aggregate expenditures type of approach. The fact that the money supply might be important was taboo.”

One could hardly avoid the question, to what extent should the economist try systematically to incorporate, into his diagnosis and prescription, considerations of political feasibility and tactics? Few of the interviewees took a wholly “pure” stance. It is commonly acknowledged that the typical economist is something of an amateur in politics and that his comparative advantage lies in economic analysis quite narrowly circumscribed. And the economist’s partial advice may be properly swamped by other considerations. McCracken felt that “*we* had a responsibility to make sure that the President understood the *economic* aspect...; *he* had the problem of taking into account all kinds of dimensions of the problem.” Ackley agrees: “economists shouldn’t expect to win all the arguments...their role is to give what they think they know that’s relevant, but ...basically the decisions are almost invariably a compound of political and economic and broader policy aspects.” While some final policy decisions are clear-cut and easily handled, Francis M. Bator, while Deputy Special Assistant to the President for National Security Affairs, found generally that “things get to the President in the end because they do involve trying to make judgments which involve incommensurables and uncertainty about the consequences of choosing this or that.”

Even the economist utterly ignores only at considerable peril the program of the Administration, the fact that some topics and options are more or less clearly proscribed, legislative and administrative history, institutional mechanism and process, the seeming temper of the public, the chairmen and the compositions of key Congressional committees, and other “political” factors. “...the academic who has never had experience at a senior level in government,” McCracken suggests, “sometimes has trouble understanding that it isn’t just a simple question of a pure advice, namely, economics, being adulterated by other considerations.” Stubbornly consistent displays of what are generally deemed to be naïve un- or otherworldliness are not a shrewd tactic if one has hope of his analyses and recommendations finding a receptive audience: “there’s a limited number of spears your can afford to shatter” in causes doomed by conditions of political

reality, McCracken warns. The dilemmas of mixing “first-best” economics, philosophic orientation, and “realistic” politics can be painful—and costly to all involved. Bator recalls

one very good young economist in AID who had such a moral commitment to the kind of AID program...that the United States ought to be engaged in...but which was so totally out of scale with anything that Congress would conceivably consider that twice-weekly outrage and pounding of the table and taking the time of senior officials about why they didn't repeal the law of gravity really wasn't very helpful. At the other extreme, there is the danger of the man who gets so housebroken and so unused to pushing at boundaries, breaking the rules, who so comfortably accepts the particular prejudices of the minute,...the particular tactical “necessities” of the situation that he becomes useless, because, in effect, he stops building into his analysis an attempt at a longer view, at a wider set of values, at a greater range of choices or a sharper look at the real tradeoffs.

On balance, George P. Shultz, former Director of the Office of Management and Budget and Secretary of Labor and of the Treasury, regrets the tendency of economists entering government to “become instant political experts, and they are too prone to start talking about what is politically a good idea or feasible.” While they should be aware of “the fact that they are operating in...a political entity,” it is important, also, for them “to remember that their contribution is primarily as an economist. We want to hear what is the economics of this all about. There are plenty of people around...who are very alert to the political nuances of things.”

In the view of Charles P. Kindleberger, formerly of the Federal Reserve, the Bank for International Settlements, and, in various capacities, the Department of State, although government economists do well to avoid such involvement in “the political process” as might induce “putting body English on their forecasts in order to...achieve political effect,” they “have no choice” in taking “into account political facts of life.” Indeed, they may—with misgivings but in a presumably good cause—be active tacticians. In arranging the Marshall Plan,

once we got to a 5.2 billion figure for the first year, I defended it to the death. And any time I had to make a change in anything, I changed something else to keep it, simply because if we changed that number every day, we'd know that the whole thing would come unstuck. Somebody made up a joke that those computers...produced 5.2 billion on every problem for a long time after...it was the only number they could come up with. I'm

ashamed of that. On the other hand, that was acting in a political way—we all believed in the Marshall Plan, we all understood the political process. ...you oversell these things terribly. You really don't quite dare, in the face of a lot of opposition, say that we really don't know about how this will work.

While a senior economist with the Department of Transportation, James C. Miller III found it sometimes unsettling to be “an intermediary” between a policymaking office and outside economists, “trying to...get something accomplished and at the same time trying to...keep my intellectual credibility.” And there was still a third group to contend with, viz., lobbyists, which called for putting aside dispassionate objectivity and becoming an advocate. “By default, had I not been an advocate, I know which way the decision would have gone.”

The consulting economist several steps removed from the point and the person of actual decisionmaking may well describe what he judges to be a best solution, even if he realizes that he must provide also second-or third-best alternatives. But the government economist participating quite immediately in the decisionmaking process may feel either smothered or exhilarated by the intrusions and the parameters of political circumstance. Weisbrod concedes that

we were not expert political strategists;...even if we had been, we were not in a position to actually play that role. On the other hand, I think it's an exciting and intriguing kind of role. ... I think it was, as a practical matter, hard for us not to try to play amateur political strategist and think about how could we get this thing through that group, who would accept what, and what kind of friends and allies you needed to do what. So I guess we all played that in varying degrees, and I think some staff people did quite a lot of it...tried to get through to the right people and sway their views.

And perhaps those on the staff who were deemed by the Council members as “doing the best jobs were the ones who did get most politically involved...in the sense of sort of getting through to the top people.” In Murphy's view, the nature of the CEA's work requires concern with political aspects:

at CEA, every question is a policy question, and one deals with it only in policy terms, for the most part. ...one is not simply reporting data. The task there is, as data flows in, to try to assess its implications for...policy. ...the evaluation process is part and parcel of the measurement process and the analytical process.

Miller concludes that at the outset, “you might as well tailor even the specifics” of a proposed program “in the light of the political ramifications,” for “you were eventually going to have to answer those questions, in any event.”

The problem of preserving an academic purity, eschewing political considerations, and holding to analytically best solutions is exacerbated—or made quite impossible—for high-level officers. Such people, Nutter says,

go into a political process. [They do well to tell themselves] “I’m now entering into a political occupation, I’m going to have to say a lot of things in support of decisions that I wouldn’t necessarily be in favor of, but, because they have been the result of the process, I’m going to have to support them...I must face that openly and not try to delude myself that I can stand above this and somehow be strictly objective.”

For himself, Nutter would avoid government assignments which “would put me in an awkward position of trying to masquerade as a professional economist at the same time that what I was saying was strictly political.” Occasionally, Shultz would attempt to make explicit whether or not he was speaking as an economist; “but people get pretty impatient with you if you get too prissy about all that, and I think justifiably so. You have got to be a man among men and say what you think.” At the beginning of his work, Miller would try to avoid the making of “a value judgment,” but “typically, the policy-maker would ask for recommendations,” along with analyses, “and, after a while,...I simply gave up on the caveat and just forthrightly made recommendations, based on efficiency as a criterion.”

McKean adds that academic economists, too, might avoid some massive errors by taking into account

what is really realistic about the political process. ... I’ve seen many economists say, well, there are externalities in the private sector, we should turn to government. Well, if you ignore all of the political administrative difficulties, you might say government will handle it perfectly. I know they won’t. Now, I think it’s very important to take into account real organizational costs and difficulties in government, because otherwise you may be driven to such simplistic solutions as saying, there’s some imperfection in the private sector, move in the public sector—we’ll ignore political feasibility, we’ll ignore administrative difficulties, we’ll ignore incentive problems, we’ll just put it in the public sector. And that’s nonsense, because the public sector does have difficulties that stem from the political process.

IV

The problems and difficulties and constraints we have noted—organizational, psychological, tactical—are real and significant. There remains a different sort of question which is even more fundamental: what, in substance, does the economist have to offer to the policymaker?

The question can be broken into two aspects. First, how good is economic theory in general—how much does the economist know about the nature of the world and how much can he do to elucidate and to explicate it? And, second, how far can he usefully go with his elegant theory, how much of his magnificent analytics can he effectively utilize, in providing real, immediate guidance to the policymaker?

None of my partners in conversation contended that economists actually do more harm than good as policy advisers. Indeed, typically, it was stoutly maintained that the economist's basic orientation and general framework of analysis can inspire the asking of pertinent questions and the applying of appropriate criteria. It was felt that more extensive and systematic employment of elemental economic analytics could still further reduce losses from badly conceived and badly implemented policies. But there was a very high degree of unanimity in emphasizing that we here speak essentially of only "the economist's basic orientation and general framework"—this is, indeed, elemental economics, which typically is elementary.

Ackley recalls that in the work of CEA, "so many of the questions on which I thought that we had some useful input are questions involving fairly simple matters—which sort of orders of magnitude rather than very precise point estimates, or even just sort of signs of derivatives." McKean points to

just the basic tools and reasoning plus a few efforts that have been made to test hypotheses. ... I think we have a good deal to offer there, though it sometimes seems to me almost nobody in officialdom listens much to it. ... I think the economist's framework again is the right one to weigh the advantages and disadvantages as best you can see them. But when economists sit down and prepare models to try to trace out those consequences in any sophisticated fashion, I think it's just about as apt to be misleading as it is to be helpful. I'm not sure those analyses really deserve much weight in the official's eye.

Shultz fears that

oftentimes the person who seems to be the most skilled in the use of elaborate techniques himself tends to forget the basic, fundamental framework of price theory, say, which all of the elaborate techniques are to build on. The simple things are still pretty much true, and I think they do give fundamental guidance.

Weisbrod agrees that the economist's potential contribution is genuine but limited:

that which we have to offer is fundamentally very simple and second nature to economists but not to others, and I think it's essentially the notion of opportunity costs.

In Nutter's view,

you have to end up relying on the most fundamental, simple tools of analysis—they are the ones that are the most useful, anyhow, when you have to face these immediate decisions. ...if people really have a good grasp, for example, of supply and demand, they are in a very good position to deal with 95 per cent of the important issues...from the point of view of the government.

What is required, Hansen found, is

just good common-sense economics,...the kind of basic analytical framework that we all sort of got in Econ 101. ... Simple supply and demand and benefit-cost...if you can just keep these things in your mind, plus if you are open to seeing how they might have to be modified in the light of the institutional constraints and considerations, then, I think, that's the game, really.

Willett is

extremely impressed with the power of Ec 1 economics. ...in a lot of areas there are cases where understanding a little bit of simple economics can lead to an improvement in policy...if you know it, and if you are willing to follow it.

In the CEA, "a lot of the stuff was done just on the back of an envelope," says Lampman, who is

generally skeptical about how much you improve the quality of advice by referring to very elaborate studies of quantitative character...economics in many critical situations is a matter of judgment rather than of science, of art rather than specific, well-established causal relationships.

McCracken makes explicit a dimension

which...seems occasionally not to be well understood by our brethren in the profession. While I was in the Council, I found myself spending a far larger proportion of my time than most people realize on what might be called "micro" economics. And while microeconomics during much of my lifetime has tended to be a little bit the Cinderella, as it were—it was macroeconomics which was the hot area—I must say that here the discipline of economics has certainly earned its keep.

And Andrew M. Kamarck, Director of the Economic Development Institute of the International Bank for Reconstruction and Development, has found not only that "the most useful part of economics is knowledge of what is fairly elementary" but that of special importance is the "relationship between price and supply and demand." "The use of prices as a technique of policy" is "extremely important"—but many "still haven't learned that most of the time it is much more effective to control the economy through price incentives than it is through highly complicated administrative mechanisms."

"Career" economists in government may critically ascribe various characteristics to academic economists. For some, the academician naïvely plays too simple an analytical game to be useful; for others, the best campus work is not distinguishable from government work; and in still another view, government analytics are superior, not only in operational realism and relevance but in formal innovation. Lederer finds that academicians commonly have "an oversimplified view of the world and the problems"; "academic economists tend to neglect" complications stemming from market structure, inventory accumulations, adjustment costs and time, substitutions for price changes in market corrections, substitution of international negotiations for competitive actions:

...so what they are advocating and...saying is sometimes so far removed from what can be realistically considered by a person who has to take into account market realities and political repercussions that they can become pretty useless. ... Many in the academic profession have gotten to be too specialized in little, abstract problems.

Actually, it is doubtful that “analysts in government have a better economic theory,” but “they play better by ear,” with a finer regard for “the political and other realities.” Anthony M. Solomon, former Assistant Secretary of State for Economic Affairs, also gets “a little disgusted with academicians who come in and give us clichés out of textbook economics which have little relevance to the practical world.” But good economics is good economics, whether on the campus or in the government. And

the best work in analytical economics is work that leads squarely to possible solutions. ...most of the leading economists on the campuses are men who have much to offer in the way of large policy solutions, and that kind of analysis is always welcome here. ...there are many top economists on campuses who are also concerned with [our real problems of policy] and they are not working on them strictly on the basis of slide rules and econometric supply and demand projections, because if they are they are pretty damn silly and irrelevant. They are looking at exactly the same factors, or should be, that we are looking at.

Polak tells us that “the level at which the Fund has to operate is very close to the academics.” Indeed, on the most fundamental problems,

we are quite often ahead of the academic community. ...the Fund operates continuously at the margins of subjects, where the universities haven’t reached yet. ...we are the provider of some of the most interesting literature, and this has been so right from the start of the Fund.

Most of the interviewees were asked to comment on the description of governmental economists and economics which was presented in 1962 to the American Economic Association by Alain Enthoven, then in the Department of Defense. In describing the situation in the Pentagon, Enthoven summarized:

the tools of analysis that we...use are the simplest, most fundamental concepts of economic theory, combined with the simplest quantitative methods. The requirements for success in this line of work are a thorough understanding of and, if you like, belief in the relevance of such concepts as marginal products and costs in complex situations, combined with a good quantitative sense. The economic theory we are using is the theory most of us learned as sophomores.⁷

7. “Economic Analysis in the Department of Defense,” *American Economic Review* 53 (May 1963): 422.

No one seriously disputed the essence of this characterization of governmental economics work as done in the early 1960's. And few believed that the level of rigor and technical sophistication in economics (rather in contrast to the rapidly expanding—if not always satisfying—use of econometric techniques) had increased strikingly during the following decade. “I certainly agree that the most important thing that economists can contribute are these simple notions,” said McKean. And McCracken added:

the Council of Economic Advisers will certainly always want strong competence..., but it also will want people who understand that for the vast array of problems the actual analytical work, as Enthoven was pointing out there, will be at a fairly basic level.

A rather different view was provided by Cooper. While he largely agreed with Enthoven with respect to the early 1960's,

CEA under [Walter] Heller substantially upgraded the type of analysis that was introduced into government policy, e.g., potential output of economy, full employment budget. ... Heller put together a group that introduced a systematic, modern applied economic analysis into government decision-making. [And since then] things have changed a lot. All you have to do is look at the *Survey of Current Business* today or the *Federal Reserve Bulletin* and compare it to ten years ago to see the increase in sophistication of tools, in published analysis. There is just no question that the sophistication of the tools that were used was upgraded substantially during this period.

However, Andrew F. Brimmer while a Federal Reserve Board Governor found “lack of an explicit theory or conscious application of a theory” in the Board and in the Open Market Committee (nor has the research staff developed or adopted “an explicit theory,” although it has done elaborate model-building and useful computerizing):

The Committee has never been willing to give a systematic exposition of the theoretical basis of its work. Moreover, given the diversity of view on the Committee, I don't think it could agree to. So people can agree to act who cannot agree to an explanation.

This is not fundamentally a situation in which competing theories are confronted and weighed: there can be little “systematic debate” when a majority, “including some of the most strategically placed members on the Committee,” believe that an adequate theory does not exist.

Two related notes of caution were sounded several times. *First*, there are not many who have “a thorough understanding of and...belief in the relevance” of basic theory. Only a sophisticate is likely to make very productive use “in complex situations” of elemental analytics; “you have to believe in it,” says Johnson:

You’ve got to have confidence in it in the sense that you’ll stick to it. And you also have to be able to show that you understand it. ... Well, the people I have known who have been successful with this kind of advising were by no means just anybody off the street of economics, so to speak...they stuck to their line.

McCracken worries about the “tendency” among economists “to homogenize our personal value preferences and our analyses.” There are economists, Johnson agrees, who take policy stances primarily on grounds of ideology and political tactics. But

if people will take that attitude, that they won’t express elementary economics for fear that it will comfort...one political party rather than another, you really sold the game. So it’s not all that easy to find the people who can do economics at that level and really believe it. ...if you have a fairly sophisticated knowledge of the world, then you can use common sense.

And Cooper, despite seeing during the past ten years much increase in the sophistication of analytic techniques employed in government, basically concurs:

At the conceptual level, the basic tools with which we deal, not much is introduced between freshman economics and second-year graduate economics. Put that way, I would agree entirely that what is needed is a really thorough grasp of sophomore economics. But being thoroughly comfortable with using that particular set of tools is what’s really important. ...what is useful from a policy point of view is just clear thinking. ... It’s not terribly sophisticated stuff...once you’ve grasped what the concepts of economics are, they are simple. It’s just that most people haven’t mastered them.

Second, while the exigencies of the government job generally preclude systematic and prolonged thought, modeling, and testing, and while much of the immediate work consists of hurried and harried scribbling on the back of the proverbial envelope, the scribbling draws on intellectual capital accumulated over the preceding years. Little explicit use is made of the literature, but the content of

the quick answer reflects previous study; time does not permit much current innovative investigation, but yesterday's careful work guides today's judgment. Theodore Morgan, former CEA staff member and adviser to the government of Thailand, warns that

you ought to know more than you use...because you ought not to feel that you are at the edge of your resources and there might be something over that edge that you ought to know but you don't know.

Life with CEA, Weisbrod agrees,

was very chopped up, very diluted, never much time to think about anything, shifting gears from topic to topic, person to person many, many, many times a day...no time to think, no time to read, no time to talk about ideas. ...after a while, I discovered that I was simply saying the same things over and over again. ...the depletion of the mind goes on pretty rapidly, but...if there's enough there to begin with, then you can still be quite effective, I think...giving gut reactions...making snap judgments...but if you have enough capital to fall back on, you can make some sensible decisions, write sensible memos in a great hurry.

Ultimately, however, distractions and frustrations are joined by deterioration and exhaustion. Previously accumulated intellectual capital "you drew down very fast," Hansen reports, "because you did not have time to develop any more while you were there." Jackson had the same kind of experience:

I was using my intellectual capital up without time to reflect and think and read and keep current. I was using what I knew and had put together before I went there, and to me that spelled that one ought not to stay too long.

"You get obsolete very fast in government," Kindleberger agrees, "there isn't any doubt of that. You read telegrams instead of articles, and the wastage of obsolescence and depreciation of economists in government is a very serious problem." Bergsten

couldn't even begin to keep up reading in the literature in my own most narrowly conceived field, international monetary affairs,...let alone the whole of foreign economic policy for which I was responsible. [And that was the case with] all the other operational guys, too, some of whom had a bent, because of academic or quasi-academic backgrounds, to do long-range

thinking if time permitted, but they were on the same boat I was, just overwhelmed with operational stuff.

After a person has spent four years as a would-be analyst on the fly, Nutter testifies

whatever intellectual capital he had is drained, he is then reduced to the amorphous state of writing memos off the top of his head to other people who are replying...off the top of their head...—the echo effect—and so you have lost touch with reality altogether in the sense that you have had no time to keep abreast, you've had no time to study in depth, you've had no time to think clearly because of the constant pressure. The best contribution you could make at that point is to move out of there and let somebody else come in who is fresh.

Bator agrees that

after four years...one has had it. Fatigue is something that political scientists...have not really taken sufficiently into account—what happens to people when they are actually dead beat and don't yet know it and what then happens to governments.

After a year and half as a CEA member, Jacoby

was completely exhausted. The pressures are inconceivable. The President called on the Council for a procession of opinions on highly complex matters. ... And, of course, there never was time to do it as thoroughly as you would wish. You did have to do a certain amount of shooting from the hip, although when you are advising the President, you don't like to do that—it's dangerous.

Spanning both of the noted cautionary elaborations is the suggestion by Nutter that the economist may be advantaged in approaching a wide variety of policymaking problems—

not so much because of his sophisticated theory, but because of the process of thinking and of analysis to which he has become accustomed in his profession and the discipline associated with it, he has a tendency to be considerably more hard-headed in analyzing a problem, being able to get to the roots of it sometimes much more quickly than other people, and, in particular, he has a kind of conceptual framework into which he can place

problems that many other professions don't have. ...the primary usefulness is the mode of thinking, the frame of mind, the tough-mindedness, the concentration on the objective conditions that affect a problem, and the understanding that there is a system at work in which all these things are related and that you think of the whole.

Shultz agrees that

the person who has training as an economist, who is sensible, and who will talk in a language that is understandable to others has a big advantage in the government and can have a big impact. Because on the broad range of issues, the economist has a way of going about a problem and of organizing information and of identifying what is likely to be important. ...the person who has been in political life, perhaps he has this sort of instinct—sometimes he's right, sometimes wrong—that is good politics or it isn't good politics or something, but hardly ever can they explain why. Whereas the economist has a theory, and it can be helpful, particularly if you don't say you have a theory.

A central implication of that theory is the conception of mutual benefit to transactors and of social net gain. "One of the differences between economists and politicians," Tobin suggests, "is that politicians instinctively think that the economy is a zero-sum game, and maybe they feel more comfortable in a kind of brokerage function, whereas economists are at pains to explain that it's usually not a zero-sum game." Bator injects a warning into his generalization that economists can be unusually "well suited" for government work:

There are things that economics training and economic modes of thought bring that really are very, very helpful. There are some situations in which it can be debilitating...you become overimpressed by the machinery or never think yourself out of the particular definition of the problem that comes naturally to you, because you think in economizing terms and therefore may ignore a very important part of the problem that's just as real. ... By and large, the record of economists in the government, the senior jobs, is a very good one.

But does the judgment of the economist in government greatly matter? The nominal audience may reach as high as the President, but in actuality is anyone seriously listening? Aside from satisfying the analyst's own pride of workmanship, does it make much difference whether or not he does a good job? Evidently, no simple generalization will here suffice. The importance and the impact of the

economist's work vary considerably—or seem to the economists themselves to vary—from situation to situation.

Baldwin felt

this tremendous responsibility. You found the big guys just couldn't think through carefully every issue, and that once something came up, if you had what seemed to be a sensible bit of analysis on it and little memos showing some proof of something—maybe two or three pages, but kind of supporting your line of reasoning with the arguments and maybe a few little data—then they would accept this. ...suddenly these people are confronted with decisions, and they are dying to have advice, and they don't want to be made a jackass of, and if you have something to give them, they'll read it through. ...it does have an effect.

Roper offers a different interpretation. With few exceptions, no “single person can have a significant impact on major policy decisions,” for “one is not likely to reverse the opinion of the head of an agency who has had years of experience that reinforced his confidence in his judgment.” The staff economist is to “develop the best possible arguments” for his administrative superior, and most economists “will offer some counterargument” when the superior's position is deemed to be wrong. But this will not generally have much “impact...in changing policies on important matters.” Roper adds that the (few) research economists in government do earn their keep by bringing more “relevance” than do many academic workers “into the mainstream of economic research by virtue of being in an environment where they are forced to address real world problems.” But whether or not the work of a particular individual or even of a branch bank research staff in the Federal Reserve has a discernible feedback is typically hard to determine—a question of some importance with respect to staff morale and, therefore, to productivity. The staff economist in a Federal Reserve bank provides material to his research director, who briefs his president, who may be a member of the Open Market Committee. But the final decisionmaking is remote from the individual economist, he is unlikely to learn that his contribution carried weight and was recognized, and the rest of the world will never applaud. “Nobody, for example, at the *New York Times*...would be aware of who it was who caused whatever slight modification of existing policy occurred,” says Gies. “It was...decided by the Open Market Committee, and who knows what the hell it was that caused them to decide. So you rapidly lose identification as you proceed outside your department into the bank and then beyond.”

It would seem that there is room for contributions of economists to the improvement of governmental economic decisionmaking. Miller found that

“incredibly important decisions were being made with incredibly insufficient information by incredibly unanalytical people.” This is consistent with the suspicion of some senior economists that their contributions typically are swamped by other policy considerations. Consider Edward L. Allen, formerly with the Department of Defense and the Central Intelligence Agency, and then Deputy Assistant Secretary of Commerce for International Economic Research and Analysis:

It has always seemed to me that when we get engaged in [international] negotiations...that the kinds of factors considered are pragmatic. There is no one really concerned about the niceties of international trade adjustment. ... There is great attention paid to political and security considerations which, rightly or wrongly, have proved to be overriding in almost all instances up to date. I'm not sure that these negotiators would understand the subtleties of sophisticated economic analysis, since they don't seem to understand the basic numbers of the balance of payments, to begin with.

Notably in the case of the CEA, deliberate efforts have been made to enhance the status of economic advisers in government. Lampman points out that the CEA

was a much less established institution in 1961 than we think of it now. ...[it] was communicated to us by Walter Heller that we were on the frontiers of developing important political relationships for the professional economist at the level of the Presidency. [But in Lampman's view] approaching the close of the Kennedy Administration, the morale in the Council was quite low. The feeling was that we hadn't really made any impact either with the President or, more importantly, through him on the country...we'd had impact on some speeches; we had improved the quality of economics in the Presidency in some ways, but it was a very marginal impact.

To the extent that the influence of the economist in government has fallen short of either what was desired or what was anticipated, there are different explanations of the disappointment or the surprise. In the area of economic development, Solomon alleges limited policy relevance of the literature. Beyond quite obvious economic considerations, “the whole job” of devising and implementing development policy “falls more into...diplomacy and common sense and rudimentary concepts of fairness and quid pro quos, and the amount of advice you can get from the economists...on the campuses is very, very minimal.” And Kamarck doubts “very much if anybody in the [World] Bank reads most of

the stuff in the journals. ... Most of it deals with unreal situations—situations that never existed and never will exist. ...an awful lot of it is really scholasticism.” (Kamarck complains, too, about “giving more sophisticated treatment to the data than the data really deserve.” Many economists, some in the Bank, indulge in the “bad technique” of “pseudo-precision” and “fake precision.” Especially “on the macro level,” complex models are developed which “aren’t used for policy decisions, because the data just aren’t that good.”) Gordon Tullock, at one time a Foreign Service officer, finds a disconcerting lack of correspondence in recent years between, on the one hand, a substantial number of highly able and distinguished economists available to government and, on the other hand, the disappointing quality of policy. Poor policy stems in part, to be sure, from ignorance: “...on many of the specific questions on which economists are asked to advise, I don’t think they know the right answer.” But bad policy can stem also from failure of the advisee to follow guidance. And there is failure, too, on the part of the economics fraternity:

Economics is rapidly moving into...a highly escapist phase, in which the average economist works on some subject which either has no relevance to the real world at all—and therefore doesn’t lead him into squabbles with his friends in the Department of English—or, if it has relevance to the real world, it is a very narrow area, and outside that very narrow area where he’s working, he simply agrees with his friends in the Department of English as to what is good policy.

But Robert D. Tollison, after observing governmental regulatory policies from the vantage point of the CEA staff, laments the bureaucratic reluctance “to apply...even simple price theory”:

We were told...you can’t make policy based on economic theory. ... It was clear they weren’t willing to listen to an analytical argument. ... You can go all the way through the regulatory aspects of the government, and you just see that the basic choices are getting filtered out by some other mechanisms than what I would call economic sense. And the role of the economist there is a stopgap—keep them from doing something completely dumb, just completely dumb.

Beyond the avoidance of doing something completely dumb, the objective seems to be to please the regulated industry and thereby to please congressmen and cabinet members. To be sure, Nutter agrees, “decisions are not made by economists, they are made by the political authorities.” And those economists who, through esoteric technique, try “to add this supposed sophistication...are

just putting a façade on a structure that is built on other considerations.” Those considerations stem from “the very process of the bureaucracy...the whole interplay of our governmental process.” From similar perspective, McKean is

pessimistic in general about the economist’s role or the role of economics in government. ... Wherever you find somebody having made an economic analysis which seems to result in implementation of a government policy, you rake around among the leaves, and I think you find usually that the government agencies, or officials who implemented it, had reached that conclusion for other reasons—it was in their interest. ... The things that shape decisions in government are the pressures emanating from the bargaining amongst officials, given the nature of the political process, and if you want to change the way those decisions are made, I don’t think just sheer brilliant economic analyses are likely to do very much by themselves.

Support is provided by W. Michael Blumenthal, former Deputy Assistant Secretary of State, who was chief U.S. negotiator during the Kennedy Round as ambassador in Geneva. The only economists likely to have significant impact in governmental decisionmaking, he contends, are “the ones who understand who makes the decisions, what determines how a decision is made, and how you influence the decisionmaking process in this jungle.” The analytic specialist can make a limited, background contribution, but “the one who really has an impact also has the capacity to have a sense of power and to understand how in the real world you apply this in a compromising way, in a less than perfect way.”

V

One might suppose, and certainly hope, that these economists are prepared to draw fruitfully on their experience in proposing ways to do a better job of policy advising. Most of them are very humble about the effectiveness and the worth of economic analyses in past and current policymaking, and doubtless all would like to have the job done better. But how? The answers were few, often either too delimited or too grandiose in scope to be immediately interesting, and generally given with little confidence.

Several explicitly rejected the idea of allotting more money to the hiring of more economists, and there were only a few calls for larger budgets. The view of Allen is widely representative: “I don’t think that there is any enormous need for more people. There are plenty of people around in the research business.” Specifically, McCracken “would insist very strongly on the importance of keeping the Council small. ...you want a small, high-powered, fairly high-paid staff, and it

will tend inevitably to be all chiefs and few Indians.” If it were made appreciably larger, “then you’ve got to have department heads and the whole structure, and it would add enormously to its viscosity.” In much the same language, Ackley agrees that the CEA staff is sufficiently small that it “really required no organization...nobody supervised anybody else. Once you get much larger..., you would have to set up some kind of hierarchy and people reporting to people who report to other people.” From the viewpoint of the CEA staff member, Willett found it attractive to have no intermediaries between the staff and the three-man Council, partly because of resulting status. In the outside bureaucracy, “there was a degree of respect accorded to the senior staff economist because of this direct access.”

Jacoby emphasizes that the effective capability of the CEA is not adequately indicated by its own staff alone, for “we had at our beck and call the entire economic resources of the government.” But while manpower, therefore, did not generally pose a problem, there were “terrible gaps in the data base...basic deficiencies in the whole economic information system.” (Wilson E. Schmidt, at one point Deputy Assistant Secretary of the Treasury, believes that “in the international field, the greatest area for investment...in terms of social return is improving the quality of the data. ... The econometric techniques, the computer facilities have gotten way ahead of the data.” Intergovernmental disputes often are “reduced to debates over the numbers.” But adequate budgetary support for the long process of improving the quality of data is unlikely, for “the policy-planning horizon in the U.S. government is much shorter than the feasibility horizon for getting something done.”) Even those few, in several agencies, calling for additional staff were pessimistic that any increased manpower would be long permitted to provide general, basic, long-run analyses. Evidently, grubby, technical, short-term work generally expands to absorb available research resources.

Another considered improvement is more general adoption of the procedure of the Council of Economic Advisers, in which the bulk of the research personnel is on the staff for only one or two years. This short-term, in-and-out turnover calls for a given individual to engage in a burst of relatively uninhibited activity and then to leave before enthusiasm and energy have greatly lessened and intellectual capital has obsolesced or atrophied. This conception of personnel strategy does have its attractiveness, but there are also costs. Most notably, the costs are of virginal innocence, for it takes time—often, it seems a good many months—for most people to learn of data sources, routines, institutional parameters, and “skeletons in the closet” (Theodore R. Gates) and in general to reach maximum effectiveness in the new situation. Gates while in the Office of Special Trade Representative drew heavily on people with the Council and believes that their usefulness stems, in part, from “not having to worry about

future career complications from having broken up too much china”—but, in many cases, it was six months or perhaps even longer before they became really effective.” Shultz finds that the short-term academic visitor to government

has two adjustments, in a sense. First, he is working as part of an organization, and he is unaccustomed to doing that, where what you work on today is what needs to get done and not what you decide you are going to work on. Then, second, there is a lot to know about the flow of information and institutional arrangements in the government that takes a while for anybody to catch on to. So a person spends some time just getting up to speed, and, if it’s only a year, he is really just at his most useful when he leaves.

By contrast, Miller feels that “picking up the institutional information is comparatively easy,” and he regrets the common “prejudice” in government that “if you don’t know the institutional matters, then you can’t know anything.”

A possible way to gain some of the advantages of the quick in-and-out process while minimizing some of the costs is to make greater use of outside individual consultants and of specialized research organizations. According to Johnson,

one of the great strengths of economic policy in this country has been institutions like the Brookings Institution plus the willingness of other foundations to support research on policy-oriented issues without having the commitment to make the policy. One of the major drawbacks in other countries is there is no other support outside government for research on policy issues, and this means that the research is essentially problem-oriented and biased toward the politically acceptable solution. ...there should be some sort of production of documents in thought which is not devoted to trying to prove a point...informed research without immediate policy limitations.

But there always remain the problems of relevance and applicability to government policy:

It’s like industrial research labs are always after a basic research lab somewhere else where they can do real science. And this is a myth, in a way, as to how far you can do that: if you get too basic, you are not any use to the company; and, the same way, if government research got to be too basic, it wouldn’t be much use to governments.

The general feeling among the interviewees apparently is that additional utilization of such specialized research organizations is likely to be significantly valuable only up to a point soon, even if not yet, reached. At any rate, a specific organization may have a finite, even quite brief optimal life, McKean speculates. For a time, the agency has a freshness and eagerness and a sense of independence. But it may be “almost inevitable in the political process that if someone is funding a research organization...it is very difficult for the officials who monitor the organization to continue indefinitely to let it have a free hand.” The officials become restless and begin to press for more immediately applicable research and for supportive analyses yielding desired answers—so we probably should “deliberately kill off the organization after a while and start a new one.”

Support for both Johnson and McKean can be found in the view of Nutter, who is enthusiastic about the possibilities in research institutes, “where there is an opportunity for thinking ahead and talking in terms of broad policy...away from the pressures,” but who also warns against “the captive institute,” for “to imagine that a kept kind of institute, which depended almost solely for its existence on your largesse, was going to be a source of independent, objective, candid advice is a little ridiculous.” The warning is applicable, also, to individual consultants, Jackson suggests. With “too much attention to the bureaucratic maneuverings by everyone,...there tends to be a selection of outside advisers and economists partly with a view to whether they are supporting your particular objectives in the maneuvering.” DeVany finds two related problems in dealing with research consultants. Not only is it often the case that “the contractor’s wants are revealed in the selection of the group itself to do the work”—“it’s known which” shops tend to give which kinds of results—but in addition the individual researcher’s sense of convenience tends to yield overcautious efforts; “research is risky,” and commonly analysts will “bite off just a big enough piece to make it interesting but not so big that they really have to come up with novel methods or ways of finding the answers...and they know what they could do within a certain period of time, and...that is always going to inhibit creative or risky or path-breaking research.”

Another controversial suggestion for improvement of governmental analytics is to increase appropriate competition among the analysts. Some competition has consisted of interagency “snow jobs” for the intimidation of others in debate instead of the elucidations of issues and the facilitation of thought. It has been suggested, for example that the Treasury has, upon occasion, sent economic and financial specialists to deal in conference with the historical and political generalists of the State Department, with the technical experts then blowing the opposition out of the water. (Solomon, of State, doubts that Treasury is “silly enough” to pursue “a very obvious...policy of sabotage.” Still, “monetary matters are clearly in the action area of the Treasury; they have the ball. You need a

very brilliant man in the field to have sufficient weight to keep Treasury from going down roads that...are inimical to our foreign policy in the broader sense.”) But this is not necessarily or inevitably the nature of competition. In the world of thought and analysis, as in the general marketplace, the consumer is likely to be helped by more rather than fewer options and sources, provided by suppliers who are obliged to approximate their best efforts in order to prosper. Cooper interprets some of the interagency conflict as

improving U.S. policy overall by factoring in a number of different points of view, rather than letting the line agency whose responsibility a given area is to dominate the whole of that policy. Line agencies develop blinders, often very confining blinders, which partly arise from their own limited perspective, but partly arise, it must be said, from the longevity of their own personnel.

DeVany notes:

one group of analysts will come up with the answer that somebody wanted, but there will always be someone out there chipping away...offering results on the other side...you have maybe even a genuine debate. ... It's that process, maybe the only process, that keeps people honest or helps to have better answers or the truth emerge.

Thus, Tullock would never award “a research contract; you should always have at the very least two” independent studies—and “offer to each of them the possibility of criticizing the other.” And Norman V. Breckner, formerly of the Center for Naval Analysis and now with the Federal Energy Administration, finds “no substitute for competition”—have a research job undertaken by “a number of different sorts of groups, individual firms, if you like—just like the textbook says.” McKean, too, hopes that, with rival agencies and rival claims and criticisms of one another, a more complete and more accurate picture may evolve. “You get more truth out of having three different biased ‘lies’ coming out than you do out of having one ‘lie’ which you no longer have any criticism of.”

VI

In speaking with economists who are or have been in government, one obtains a picture and gains an impression which are sobering. The government economist typically is not a highly independent researcher and analyst, free first to pick many of his subjects and entirely free then to broadcast generally the results

of his labors. He is a member of an organization, commonly devoting the bulk of his time to topics specified from on high—the specification often being enunciated only a few days (or, indeed, hours) before the deadline; conscious of a prevailing orientation and purpose on the part of those administrative superiors who constitute his main audience; conscious, also, that the decisionmakers he is more or less directly advising are themselves subject to constraints of worldly realism and political feasibility—along with innocence in the area of economic analysis; bringing to his task an accumulated intellectual capital which, even if impressive at the outset of his government work, may not thereafter be greatly enlarged or even well maintained; having more or less available a corpus of theory and an arsenal of techniques which, for all their elegance, refinement, and academic glamour, are often too time-consuming for purposes of shooting from the hip and too esoteric for the data, the colleagues, and the audience; and having little reason to suppose that his work has significant impact in the making of policy, being largely confined to support of programs and procedures determined earlier and by others and for which he may have only modest sympathy.

A small suggestion of a moral from this may be permissible. If the activity and the contribution of the economist in government is at all well represented in this survey, might we suspect that social welfare could be enhanced by “minimizing” the number and the scope of governmental activities which normally call for the discretionary policymaking services of economists? The economist knows so little, relative to the complexities of the world, and is in a position to contribute so marginally in policymaking practice, that it strikes some of us as unrefreshingly simple to ask him to do more than—if, indeed, as, much as—he is now doing in government. (But such inadequacies are not unique to economists. And, of course, to minimize and to circumscribe the role of economists in government is not the same as delimiting the role of government in economic activity, for economic policy can be made—and made as badly as it has been—without economists.)

It is alleged that there is a persistent “disequilibrium” in the sense that as economists grow more competent in dealing with policy problems, the questions asked are made broader in scope and more complex. Whether the economists are good or bad, courageous and independent or cowardly and malleable, effectively or poorly utilized at the moment, any community, Johnson suggests, tends strongly to press its government—and the government, in turn, presses its economists—to levels of incompetence. Frustratingly and disconcertingly, more is demanded at any given time than can be delivered—or could be delivered even under circumstances which the frail and beleaguered government economist might consider optimal. “The political art always is a question of working within an environment and then trying to do better with it than in fact can be done”

—“attempt to squeeze more out of the tools and the people than is in them.” Those in government “are trying to *use* more than we [economists] can supply,” he adds. “One has to distinguish between the attempt to *use* economists and an attempt to *absorb* what economists have to say.” It would appear, then, that “the government is always going to be a disheartening game for the honest economist.”

About the Author



William R. Allen has labored primarily in the scholarly fields of international economics, monetary economics, and the history of economic theory. He has educated the public through his popular writings and radio work as “the Midnight Economist.” After serving in the Army Air Corps, he obtained his A.B. (1948) from Cornell College and Ph.D. (1953) from Duke University. He taught at Washington University prior to joining the UCLA faculty in 1952. Like the economists interviewed in the present paper, he has worked for government as consultant to the Balance of Payments Division of the Department of Commerce. He has received teaching awards from the UCLA Alumni Association, the Western Economic Association, and the Freedoms Foundation at Valley Forge. He has been Vice President and President of the Western Economic Association, Vice President of the History of Economics Society, and Vice President and a member of the Executive Committee of the Southern Economic Association. Among his many publications is his *University Economics*, coauthored with Armen Alchian. From 1978 to 1992 more than 200 radio stations carried daily broadcasts of “**the Midnight Economist**” written and delivered by Allen. He has been Chair of the UCLA Department of Economics, President of the International Institute for Economic Research, Vice President of the Institute for Contemporary Studies, and Associate of the Reason Foundation.

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Czech Economists on Economic Policy: A Survey

Daniel Stastny¹

ABSTRACT

In December 2008 and January 2009, in cooperation with the Czech Economic Association, I conducted a survey of economists in my native country of the Czech Republic.² It was officially administered by Peter Bolcha, a junior member of the Department of Economics, University of Economics, Prague. The identity of the chief investigator (me) remained anonymous until afterward. The survey was administered through a web-based system that allowed participation only by invited respondents.³

The list of invited economists included all members, past and present, of the Czech Economic Association whose email addresses were available from the CEA, and all economics instructors at Czech colleges and universities whose email

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2. The survey was motivated by—and is an extension of—what is by now a fairly long research tradition. The credit for pioneering this area goes presumably to Samuel Brittan (Brittan 1973) and to a group of authors around James Kearl (Kearl et al. 1979). Subsequent surveys cover Austria, France, Germany, and Switzerland, summarized Frey et al. (1984). A survey in the UK was undertaken by Ricketts and Shoemith (1990), in Canada by Block and Walker (1988), and in South Africa by Geach and Reekie (1991). The original survey was replicated for the US: again by Alston, Kearl, and Vaughan (1992) and by Fuller and Geide-Stevenson (2003). Several other surveys have since been conducted.

3. The responses remained anonymous even to us: I generated the login codes, and representatives of the Czech Economic Association compiled and provided access to the email list. The respective parties kept the lists separate at all times. When sending out invitations, the system matched an economist on the list with a login code. No one had access to both the email list and the login-code list.

addresses were posted on their institutions' web sites (compiled by Peter Bolcha and his assistants). A total of 708 economists were sent the invitation, of which 182 participated, making a response rate of 25.7 percent. Table 1 provides breakdowns by age, gender, employment sector, income, and political party. Regarding age, note that the median age was 35, much younger than in American surveys (Klein and Stern 2006; Whaples 2006, 2009), probably for two reasons: my survey was web-based, and standard economics classes in the Czech Republic got started only after 1990.

Table 1: The respondents by age, gender, employment sector, income, and political party

<i>Age</i>	<i>Gender</i>		<i>What sort of economist do you consider yourself?</i>		<i>Gross income per year (in thousands, CZK)</i>		<i>Which political party's program is closest to your vision of economic policy?</i>		
25 or below	2.20% (4)	male	75.82% (138)	academic	56.04% (102)	250 or less	9.89% (18)	ČSSD (social democratic)	12.64% (23)
26 to 35	48.35% (88)	female	22.53% (41)	private	20.33% (37)	250 to 500	24.18% (44)	KDU-ČSL (Christian conservative)	3.85% (7)
36 to 45	18.13% (33)	declined to answer	1.65% (3)	government	8.79% (16)	500 to 750	20.88% (18)	KSČM (communist)	2.20% (4)
46 to 55	9.89% (18)	-	-	avocation	3.85% (7)	over 750	26.37% (48)	ODS (civic conservative)	43.96% (80)
56 to 65	15.93% (29)	-	-	other	3.85% (7)	declined to answer	18.13% (33)	SZ (environmental)	2.75% (5)
66 or more	4.40% (8)	-	-	declined to answer	6.59% (12)	-	-	other	4.40% (8)
declined to answer	1.10% (2)	-	-	-	-	-	-	declined to answer	27.47% (50)
blank response	0.00% (0)	blank response	0.00% (0)	blank response	0.55% (1)	blank response	0.55% (1)	blank response	2.75% (5)

21 Policy Judgment Questions

The survey consisted of a total of 27 questions: 5 background questions, one general question relating to economic policy, and 21 policy-issue questions formulated in terms of reform *direction*, as in: The level of the minimum wage rate should be: increased, kept unchanged, or reduced? The directional dimension works from the idea of liberalization. Thus the formulation anchors things in the status quo and solicits judgments about whether policy should be liberalized, kept the same, or made more restrictive—although the liberalization interpretation is not always as clear as with the minimum-wage question.^{4,5} The set of issues covered by the question overlaps with other policy surveys, perhaps with one exception being the proposition #21 on university education cost sharing by students, which is peculiar to the Czech Republic.⁶

The responses to the 21 policy issues were coded with 1 representing favoring tighter restriction, 2 favoring the status quo, and 3 favoring liberalization.⁷ In Figure 1, the horizontal line at 2 represents the benchmark of the status-quo. The numbers are in Table 2.

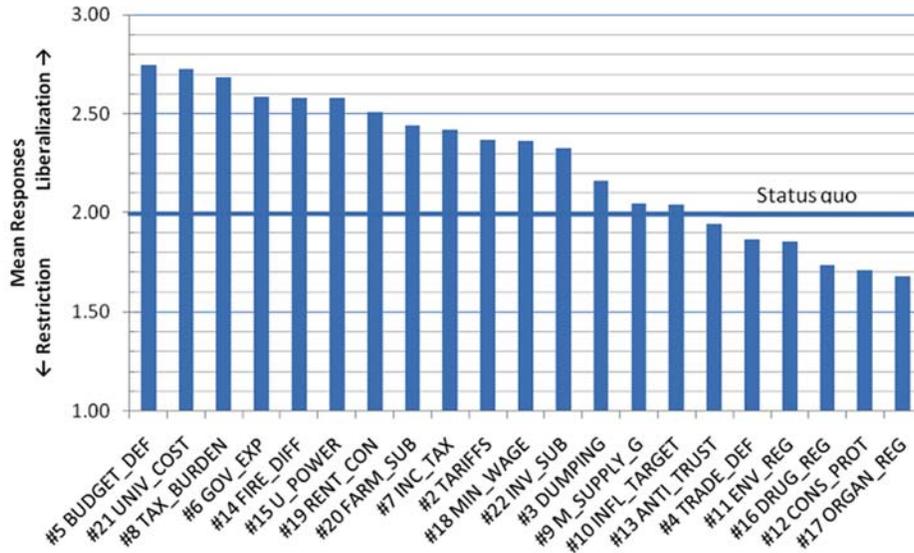
4. Three propositions appear to be of particular concern: #4 “The extent to which policy-makers pay attention to the balance of trade deficit should be:”, #9 “The rate of the money supply growth should be:” and #10 “The level of the inflation target set by the central bank should be:” Regarding the first one (#4), as implied by the context, the question relates to international affairs and protectionism; it is precisely the balance of trade deficit that in popular thinking is associated with the need for protectionism. Economists who responded that “attention” should be higher are assumed to reveal an interventionist mindset. Regarding the monetary issues (#9 and 10), arguably both lie outside the liberalization dimension, strictly speaking. But both propositions suggest a situation where opting for higher levels (of money supply or of inflation targeting) is indicative of skepticism towards the ability of markets to stabilize themselves, and perhaps of support for the monetization of government finance. In what follows I count responses for higher levels in those three issues as interventionist. Dropping the three questions would leave the findings unchanged.

5. A few of the policy measures such as rent-control may be of a local nature. Because the survey was presented as a nationwide survey of Czech economists, respondents were asked to understand the issue in terms of all areas of the Czech Republic—on the whole, should rent-controls be relaxed or tightened?, as it were.

6. Education at public-sector university, conducted in the Czech language, is supplied free of charge to all students (Czech or foreign) of public universities. Introducing some sort of students’ participation in covering the cost has been a subject of public debate ever since the beginning of the transformation in the early 1990s.

7. For all 21 questions except #19 and #21, favoring a tighter restriction corresponds to a survey response of “higher,” while favoring liberalization corresponds to a survey response of “lower.” For #19 and #21, it is the reverse.

Figure 1: The Mean Responses to 21 Policy Issues



Support for liberalization, on the whole

It seems that Czechs learned something from the terrible experience of socialism and the exceptional governmentalization of society. Only 5 out of 21 propositions have a mean response that is below 1.9 (with 2.0 corresponding to the status quo), while 13 of the 21 propositions have a mean above 2.1, leaving three propositions between 1.9 and 2.1.⁸ Moreover, many of the means are much above 2.1, while none are much below 1.9. Czech economists favor greater freedom more frequently than greater restriction.⁹

8. There are four cases (propositions #9, #10, #11 and #13) where, if the true population mean were 2.0, the likelihood of obtaining a sample mean as far above/below as the actual sample mean exceeds 1 percent, in three of these cases (propositions #9, #10 and #13) such likelihood actually exceeds 5 percent. For the other 17 cases it is less than 1 percent.

9. Naturally, for such comparisons, the selection of the 21 propositions is decisive and one can then object that the above 21 are not a fair representation of the entire policy arena. While it may be complicated to select such a fair sample, it should be noted that the propositions in this survey reflect the same areas as previous surveys of economists.

Table 2: Survey propositions and response statistics

#	Proposition	Mean	Response distribution			
			1	2	3	Blank
2	The extent to which trade barriers (tariffs, quotas etc.) are used should be:	2.370	12.6%	37.4%	49.5%	0.5%
3	The extent to which antidumping and similar trade-political proceedings against foreign producers are used should be:	2.162	25.3%	31.9%	41.2%	1.6%
4	The extent to which policy-makers pay attention to the balance of trade deficit should be:	1.867	31.9%	48.4%	18.7%	1.1%
5	The size of the government budget deficit should be:	2.749	6.0%	12.6%	79.7%	1.6%
6	The size of the government expenditures should be:	2.584	11.5%	17.6%	68.7%	2.2%
7	The marginal income tax rate should be:	2.421	11.5%	33.5%	52.7%	2.2%
8	The size of the overall tax burden should be:	2.683	6.0%	19.2%	73.6%	1.1%
9	The rate of the money supply growth should be:	2.045	9.3%	74.2%	13.7%	2.7%
10	The level of the inflation target set by the central bank should be:	2.039	9.9%	74.2%	13.7%	2.2%
11	The extent of regulation aimed at environmental protection should be:	1.852	39.6%	31.9%	25.3%	3.3%
12	The extent of regulation aimed at consumer protection should be:	1.709	44.5%	37.9%	15.9%	1.6%
13	The extent to which the anti-trust authority interferes with the economy should be:	1.944	28.6%	47.3%	23.1%	1.1%
14	The level of difficulty the labor legislation imposes on laying off employees should be:	2.581	5.5%	30.2%	62.6%	1.6%
15	The legislated labor union power should be:	2.580	4.4%	33.0%	62.1%	0.5%
16	The extent of regulation of trade in illicit drugs (addictive substances) should be:	1.737	47.8%	28.6%	22.0%	1.6%
17	The extent of regulation of trade in human organs should be:	1.680	45.6%	37.9%	14.3%	2.2%
18	The level of the legislated minimum wage should be:	2.361	14.3%	34.6%	50.0%	1.1%
19	The level of the legislated maximum rent that can be charged for apartments should be:	2.506	12.1%	24.2%	61.5%	2.2%
20	The extent of government support of agriculture should be:	2.440	13.7%	28.6%	57.7%	0.0%
21	The extent to which university students share the cost of university education should be:	2.725	4.4%	18.7%	76.9%	0.0%
22	The extent of investment perks offered by government should be:	2.326	17.6%	31.9%	50.0%	0.5%

Comparisons between Czech and American Economists

Support for intervention beyond the Czech status quo is significant in three areas: trade in human organs, trade in illicit drugs, and regulation aimed at consumer protection (though, as Caplan (2007) emphasizes, it is probable that their interventionism is milder than that of the Czech population at large). Comparing to their American counterparts, I would say that the viewpoints on

consumer protection are perhaps similar. But I think there is a notable difference on illicit drugs and organs.

Surveys of AEA members conducted by Robert Whaples report that more than 60 percent of respondents agreed or strongly agreed with legalization of marijuana (Whaples 2006), and more than 70 percent agreed or strongly agreed with allowing payments to donors of organs (Whaples 2009).¹⁰ Although there are wording differences, on these two issues the American economists seem to show more liberalism than do the Czechs. On the drug issue, one may speculate that some of the difference arises from differences between the respective status quos.¹¹ But on the organ trade issue the current Czech policy is a ban on trade and compensation, so there is not much scope for policies to be more interventionist, save perhaps stricter enforcement. Yet, this is what Czech economists on average are calling for. Therefore, Czech economists certainly seem to be less open to liberalization on the issue.

The drugs and organ issues involve a repulsion on a personal level, in a way that labor-market or international-trade issues do not. My own speculation is that, while the debacle of socialism in Central Europe has made the Czech people relatively open to market reasoning, in critically discussing policy issues involving “repugnant” personal choices the Czech *public* discussion is simply far behind that of the United States. Precious few Czech institutions, professors, or public intellectuals have critically examined the prevailing prejudices.¹² Maybe economists in the United States are more liberal on drugs and organs simply because the public culture is.

My survey shows strong support for liberalization in taxation and spending (propositions #5, #6, #7, and #8) and labor markets (#14, #15 and #18). Comparison with American economists on such issues is difficult because of differences in questions and survey design, but these would be the areas in which Czech-economists’ liberalism seems to be strongest in relation to their American counterparts.

10. On the economists’ support for legalization of organ trade, see also Diesel (2010).

11. It is a well-known fact that the US war on drugs represents a generally more interventionist approach than the European policies, including the Czech one (both in terms of the scope of restrictions and in terms of penalties for breaking them). Consumption of marijuana, while not legal, is widely tolerated. American economists could then lean towards less restriction and the Czech economists could opt for more restriction while both agreeing on some kind of compromise in-between.

12. In Stastny 2001, I offer a liberal perspective on many social issues, including drugs and organ trade.

Single-peakedness versus U-shapedness

This matter concerns the sample's distribution of opinion for a single issue. It speaks to whether there is ideological cleavage or polarization in the sample. Indeed, it is somewhat embarrassing for a scientific discipline to exhibit U-shapedness—one would expect members of a scientific community to bunch themselves around the enlightened answer, yielding single-peakedness.

In my survey, response distributions exhibited single-peaked *on every one of the* 21 issues. On not a single proposition of the 21 propositions did the response distribution exhibit any degree of U-shapedness. This contrasts noticeably with surveys among American economists. Asking 18 policy-issue questions, Klein and Stern (2006) received 6 distributions that were not single-peaked (that is, that were U-shaped to some degree). For Whaples (2006) survey, 14 out of the 24 propositions indicate such cleavage of opinion, and for Whaples (2009) it is 10 out of 20. Thus, in my survey I found no U-shaped response distributions, whereas in these American surveys it appeared in from one-third to one-half of the response distributions.

Now, my survey questions had just three response options, whereas the Whaples and Klein and Stern surveys had five, perhaps giving more space to a degree of U-shapedness. But, as plausible re-groupings of the American data show,¹³ it is likely that the striking difference is not fully or even significantly accounted for by the number of response options.

A more significant factor, I think, arises from differences in survey design. The Americans were asked, essentially, whether the minimum wage law is good or not, while the Czechs were asked to state whether the minimum wage should be higher or lower. The likelihood of a divergence between two economists' ideal level is always greater than the likelihood of a difference in their favored directions from the status quo. A supporter of minimum wage laws may nonetheless feel that the current level should be reduced. This difference in survey design might explain the absence of U-shapedness in the Czech responses.

Another possible factor may be that policy views in the United States are somewhat more rigidly aligned according to the two-party system. In the Czech politics there is "left and right," but Czech parliament is a proportional system and

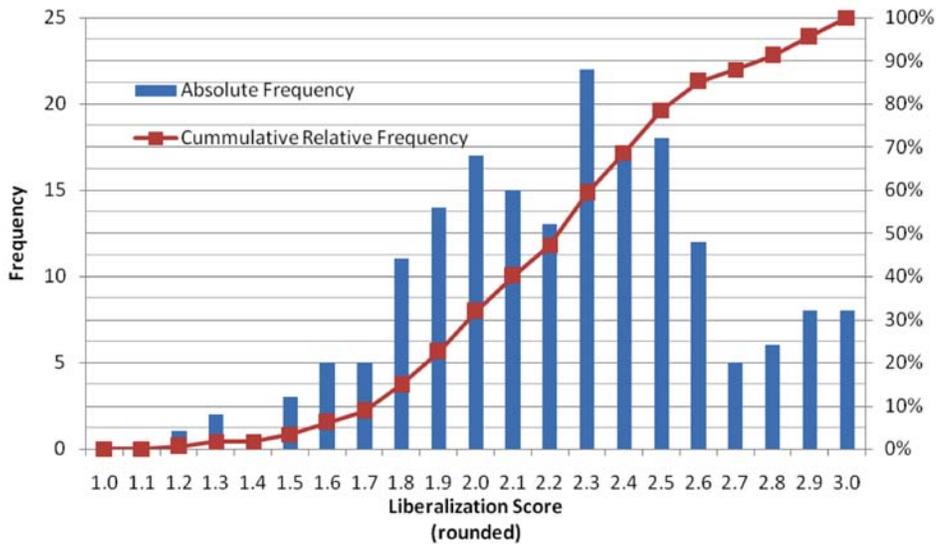
13. In a 3-class response system used in the Czech survey, each of the tail classes ("higher" and "lower") is likely to be so wide as to practically include the two classes on each tail used in the 5-class American survey (e.g. "strongly agree" and "agree" on the one end, and "disagree" and "strongly disagree" on the other). Thus, if the American surveys used 3-class response design (grouping the two classes on each tail into one), we could witness an even higher number of propositions with U-shapedness than under 5-class response design. For example, Whaples (2006) survey would under this assumption report 16 cases of U-shapedness (as opposed to 14 under the actual 5-class response system).

currently there are five parties. Even sympathizers of a party that is either clearly “left” or clearly “right” might feel less rigidly committed to that party’s views. In the United States, there may be more orientation toward two-party partisanship, giving rise to more cleavage in responses.

Respondents’ Liberalization Scores

For each respondent, I took his or her mean response to the 21 policy propositions to be his or her *liberalization score*. With a range from 1 to 3, the higher the score, the more the respondent favored liberalization. The distribution of liberalization scores is shown in Figure 2.

Figure 2: Liberalization scores distribution for all survey respondents



The rightward skew of the distribution confirms the bent of the Czech economics profession toward liberalization. As seen from the red line showing the cumulative distribution, around 30 percent of the respondents have scores of 2.0 or below, which leaves a good majority in the “leaning toward liberalization” range of scores. We can also see that at the upper end the histogram does not flatten out, as at the lower end, but rather that there is a contingent of highly consistent liberalizers with scores above 2.8.

Liberalization Scores by Personal Traits

The survey asked about age, income, employment type, political party, and gender. For age, liberalization scores tended to go down by age.¹⁴ For income, liberalization scores tended to go up by income.¹⁵ For employment type, the differences were minor.¹⁶ We now turn to the other two: political party and gender.

Political party

The survey asked: “Which political party’s program is closest to your vision of economic policy?” Figure 5 shows the average liberalization scores economists of each of the five parties in the Czech parliament. The color-coding in the figure is faithful to the official colors of the parties, or to a color somehow associated with its program.

As shown in Figure 3, the number of economists identifying with three of the parties is hazardously small ($n = 4, 5,$ and 7), so be cautious with the associated results. That said, the average liberalization scores by party are ordered in a predictable way.

The most interventionist set of economists were those identifying with the Communist Party (KSČM, colored red), effectively a successor of the ruling party of the communist regime in the former Czechoslovakian federation. Those identifying with the Czech Social Democratic Party’s (ČSSD, colored orange) were somewhat less interventionist; it is a Czech version of a typical European social-democratic party. There are two parties for which the average liberalization score is mildly in favor of more freedom, both sharing some of the social democratic values while each of them focusing on a particular area of interest, the Green Party (SZ) and the Christian Democratic Party (KDU-ČSL, colored black). Lastly, the Civic Democratic Party (ODS, colored blue) is by most standards the

14. Here are the average liberalization scores by age groups: 25 or below ($n=4$): 2.12; 26 to 35 ($n=88$): 2.37; 36 to 45 ($n=33$): 2.24; 46 to 55 ($n=18$): 2.15; 56 to 65 ($n=29$): 2.12; 66 or above ($n=8$): 1.90; declined to answer ($n=2$): 1.95.

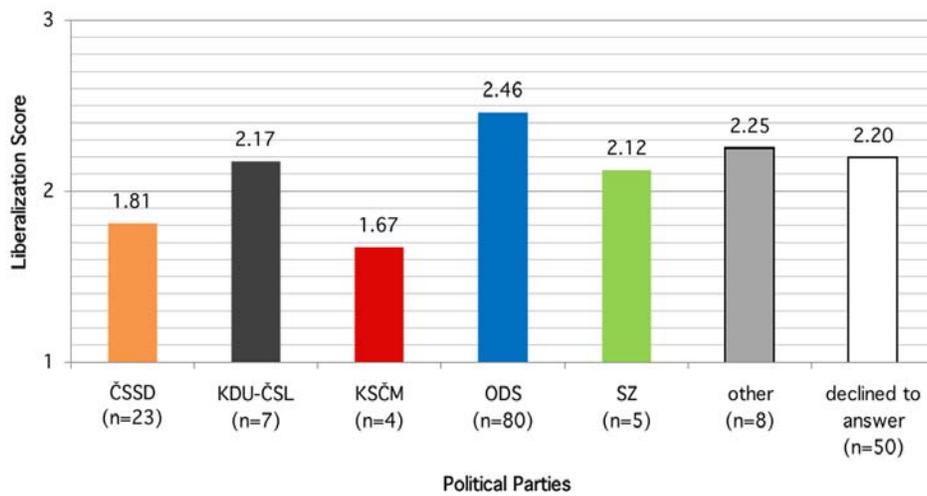
15. Here are the average liberalization scores by income groups: 250 thousand CZK or less ($n=18$): 2.08; 250 to 500 ($n=4$): 2.10; 500 to 750 ($n=38$): 2.32; 750 or above ($n=48$): 2.36; declined to answer ($n=33$): 2.33.

16. Here are the average liberalization scores by employment type: academic ($n=102$): 2.27; private sector ($n=37$): 2.33; government ($n=16$): 2.18; economics as an avocation ($n=7$): 2.06; other ($n=7$): 2.36; declined to answer ($n=12$): 2.06.

least interventionist party, loosely analogous to the British Conservatives or American Republicans.

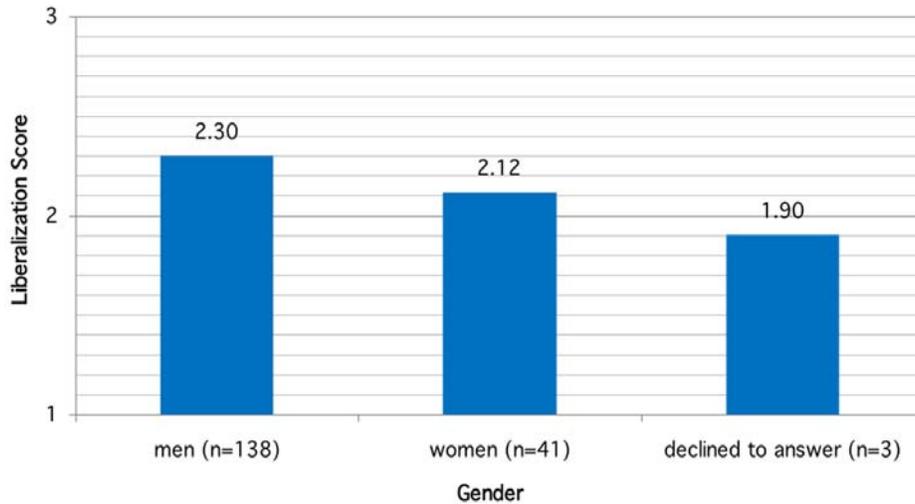
There was also the group that explicitly chose not to report any party (colored white). It was quite large (50 out of 177) which perhaps reflects apprehension and distrust regarding the secrecy of data. Respondents were more willing to report their incomes than their party affiliation. History dies hard.

Figure 3: Liberalization scores by political party



Gender

It appears that women economists in the United States tend to be more interventionist than men economists (May and Whaples 2010; Hedengren et al. 2010). As seen in Figure 4, the same seems to be true in the Czech Republic.

Figure 4: Liberalization scores by gender

Concluding Remarks

On most issues treated, most Czech economists would prefer less governmental involvement or restriction. On the whole, they tend to favor liberalization.

We have noted some contrasts between Czech and American economists. On the issues of illicit drugs and organ markets, the Americans seem to be more disposed toward liberalization. Also, we have noted that the American survey responses show much more cleavage in their distributions, but that could be a result of differences in survey design.

The appendices supply the survey data, which may be used in further research. In fact, I designed and conducted the survey to investigate the degree of consensus among Czech economists on particular policy issues, as well as the degree of correspondence between any such consensus and actual policy (see Stastny 2011). The appendices also provide the instrument in Czech and English translation.

Appendices

Appendix 1: The original survey instrument (in Czech) (pdf): [Link](#)

Appendix 2: The survey instrument in English translation (pdf): [Link](#)

Appendix 3: The data (Excel): [Link](#)

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Editorial suggestion for viewing: This article contains many color figures, some in landscape. Figures 2 through 17 are placed after the conclusion. We suggest that you print out the first 9 pages of this article and read them while viewing Figures 2 through 17 onscreen. For the landscape figures, click in Adobe Reader on "View," "Rotate View," and "Clockwise."

Economist Petitions: Ideology Revealed

David Hedengren¹, Daniel B. Klein², and Carrie Milton³

ABSTRACT

Petitions are a way for a group of signatories to formulate a statement on an important issue and declare themselves publicly. Because the statements are open-ended, issue-specific, and not anonymous, petitioning is a meaningful mode of expression.

Petitioning by economists has a considerable history, including a 1903 UK petition against protectionism (Edgeworth et al 2010), a 1930 US petition against protectionism (Editors 2007), a 1933 US petition to raise the general price level (Brown 1933), a 1981 UK petition against Thatcher's macroeconomic policy (Wood 2006, Booth 2007), and a 1991 New Zealand petition against deficit cutting (Kerr 1994).

The internet has advanced the practice, and the inventory of economist petitions has become substantial. Being public in nature, petitions are not hard to track down. We posted a working list ([link](#)), solicited further tips, and arrived at a set of 35 petitions for analysis, dated between 1994 and 2009.

We have counted as "economist petition" any published statement signed principally by economists to express and endorse a position on a public issue. We

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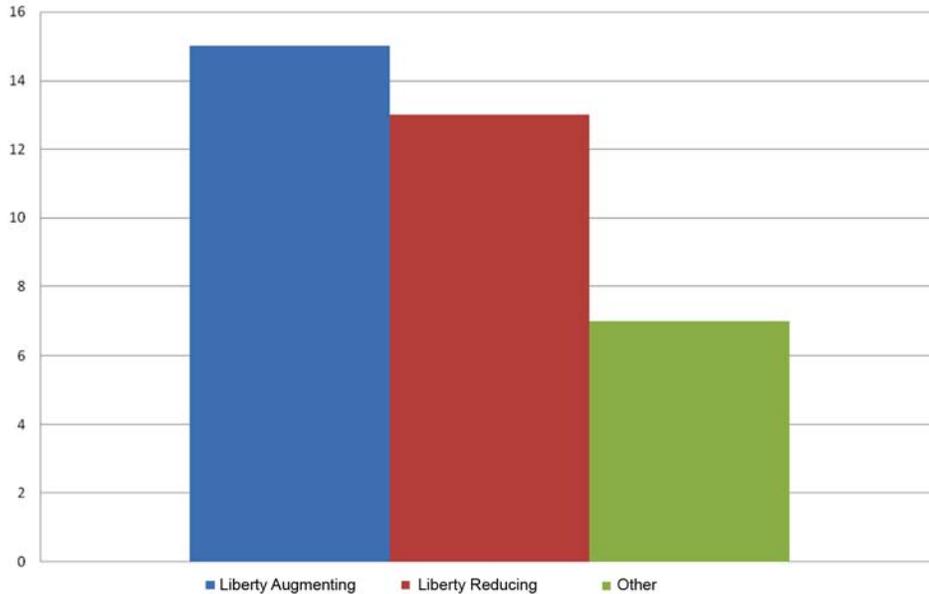
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included all signatories, *not just the economists*. In fact, we did nothing to determine a signatory’s status as an “economist.” Of the 35 petitions, the lowest number of signatories is 10. The mean number of signatures on a petition is 308.

We categorize the 35 petitions based on, not the argumentation contained in the petition, but the nature of the proposed reform. In our treatment, the reform’s “nature” turns on the *liberty principle*. We separated the 35 petitions into three categories: liberty augmenting, liberty reducing, and other. The number of petitions in each category is shown in Figure 1.

Figure 1: Number of petitions by petition type



Our framework works from the classical-liberal/libertarian idea of liberty. It works from the liberty principle, an issue-oriented formulation anchored in the status-quo saying: *If Reform₁ rates higher in liberty than Reform₂, then support Reform₁*. For example, the petition to raise the minimum wage proposes to initiate coercion against employers for certain erstwhile-legal actions (paying someone \$6/hr) that are themselves non-coercive according to the classical-liberal configuration of ownership. Note that “reform” may entail no change at all, or simply represent the status quo—and, indeed, that is usually the relevant comparison in classifying a petition.

Classical-liberal semantics carry a presumption of liberty, but we do not mean to imply any 100-percent ethical commitment to the liberty principle. Principles vie with and compromise one another. With Adam Smith, we believe

that exceptions to the liberty principle have a place, and are to be treated as exceptional. Exceptions do not preclude a notion from principle-hood. We confess our liberal sensibilities and the role they play in our formulations, but, strictly speaking, we do not pass judgment on any on the petitions treated in this paper.

The idea of ranking Reform₁ and Reform₂ (which, again, may be no reform at all) in terms of classical-liberal liberty is, admittedly, sometimes vague and indeterminate, but for most of the petitions—as with the minimum wage petition—it is reasonably straightforward. Some of the petitions are harder to classify using the liberty principle. Some petitions lead into problems that Klein and Clark (2010) discuss in terms of possible disagreement between “direct liberty” and “overall liberty,” a certain kind of ambiguity sometimes plaguing the liberty principle.⁴ Also, some petitions address issues of public administration—government rules governing the use of government property—where the liberty principle simply does not apply, at least not directly.

The petitions classified as “other” are of three sorts: those that deal principally with public administration, those that address party politics (John Kerry, John McCain), and those of which we simply did not feel sufficiently confident in classifying in terms of overall liberty.

We have collected the text of the 35 petitions into a single pdf ([link](#)). The signature data is compiled in an Excel file ([link](#)). Table 1 lists the 35 petitions and shows how we classified each. In a couple of instances, it would, in our view, have been reasonable to classify the petition other than as we have, but, again, in the vast majority of cases the classification is straight-forward based on the stated criteria.⁵

4. While Klein and Clark (2010) focuses on the ambiguity of the liberty principle arising from possible disagreement between direct and overall liberty, Klein (2004) offers a fuller taxonomy of the limitations of the principle.

5. We have classified John Cochrane’s 24 Sept. 2008 petition about bailouts—which concludes: “we ask Congress not to rush, to hold appropriate hearings, and to carefully consider the right course of action, and to wisely determine the future of the financial industry and the U.S. economy for years to come”—as *other*, but it also would be reasonable to classify it as *liberty augmenting*. Of our judgments in classifying a petition, the most controversial would probably be our classifying the 4 March 2004 petition about cap-and-trade as liberty reducing.

Table 1: The 35 Petitions, 1994-2009

Label	Date	Organizer/ Sponsor	Category	Signatures ⁶	Affiliations	Universities
Support Market Oriented Health Care Reform 1994	03/16/94	The Independent Institute	Augm	637	233	216
Oppose Antitrust Protectionism	06/02/99	The Independent Institute	Augm	240	137	129
Support Market Oriented Health Care Reform 2000	03/01/00	The Independent Institute	Augm	538	254	236
Economists for Sweatshops	07/29/00	Academic Consortium on International Trade	Augm	252	117	102
Oppose Death Tax	05/21/01	National Taxpayers Union	Augm	279	172	148
Scholars Against Sweatshop Labor	10/22/01	Political Economy Research Institute	Reduc	435	196	182
Oppose Bush Tax Cuts	02/01/03	Economic Policy Institute	Reduc	464	197	156
Oppose Tax Increase	01/14/04	National Taxpayers Union	Augm	116	96	73
Endorse John Kerry for President	08/25/04	John Kerry Campaign (Not Sure)	Other	10	7	6
Oppose John Kerry for President	10/13/04	George W. Bush Campaign (Not Sure)	Other	367	229	187
Warning Future of Social Security	05/11/05	Cato Institute	Augm	454	274	191
Increase Immigration	06/19/06	The Independent Institute	Augm	523	270	232
Support Raising the Minimum Wage	09/27/06	The Economic Policy Institute	Reduc	659	288	237
Oppose Marijuana Prohibition	11/30/06	Marijuana Policy Project	Augm	554	351	344
Oppose Government Regulation of Internet ("Network Neutrality")	03/28/07	AEI-Brookings Joint Center	Augm	17	13	9
Statement on Prediction Markets	05/01/07	AEI-Brookings Joint Center	Augm	25	16	12
Economists Against Protectionism	08/01/07	The Club for Growth	Augm	1028	426	367

6. There are some petitions where we found fewer names than the publisher suggests. However the difference is less than 0.05% of signatures.

ECONOMIST PETITIONS

Oppose "Windfall Taxes"	10/17/07	National Taxpayers Union	Augm	234	178	143
Support John McCain Economic Plan	05/11/08	John McCain Campaign (Not Sure)	Other	326	213	158
Raising Some Concerns about Government Bail Out for Mortgages	09/24/08	John Cochrane	Other	230	61	57
Support Government Bail Out for Mortgages	10/01/08	Unknown	Reduc	76	34	16
Concerned about Climate Change	10/07/08	Nancy Olewiler	Reduc	254	43	40
Support Federal Recovery Act	11/19/08	Center for Economic and Policy Research	Reduc	387	193	157
Oppose Federal Recovery Act	01/27/09	Cato Institute	Augm	203	130	128
Oppose Budget Reduction in Washington State	02/19/09	Washington State Budget & Policy Center	Reduc	22	11	7
Support Employee Free Choice Act	02/24/09	The Economic Policy Institute	Reduc	40	23	18
Support Cap and Trade	03/04/09	Southern Alliance for Clean Energy	Reduc	601	341	271
Replace Federal Income Tax with FairTax	03/29/09	FairTax.org	Other	80	75	71
Support Using Procurement Auctions Over Grant Submissions	04/13/09	Paul Milgrom	Other	64	30	25
Support Government Intervention to Promote Biofuels	04/21/09	Union of Concerned Scientists	Reduc	16	14	12
Oppose Green Protectionism	05/08/09	Atlas Global Initiative for Free Trade Peace and Prosperity	Augm	1215	648	349
Fed Independence Petition	07/15/09	Wall Street Journal	Other	183	52	29
Support Tax Increase on Corporations and High Income Persons	10/07/09	Oregon Center for Public Policy	Reduc	36	16	10
Government Oriented Health Care Reform 2009	11/17/09	Unknown	Reduc	23	10	7
Support for a Financial Transactions Tax	12/03/09	Center for Economic and Policy Research	Reduc	204	122	103

All of the remaining figures of this paper, that is, Figures 2 through 17 appear after the conclusion.

Figure 2 displays the number and type of petitions by the organizer/sponsor. Organizers canvass for signatures using their networks and email lists. It is not as though every economist is perfectly informed of every opportunity to sign a petition and reveal his or her preferences accordingly.

Figure 3 shows the number of signatures by petition type, with a grand total of 10,792 signatures.⁷

There are nearly twice as many liberty-augmenting signatures as liberty-reducing signatures.

Figure 4 displays the number of unique signatories by petition type. In this breakdown, an individual who, for example, signs three liberty-augmenting petitions and one liberty-reducing petition will appear once as a liberal signatory and once as an interventionist signatory. Thus, the sum of unique liberty-augmenting, liberty-reducing, and other signatories is *greater than* the 6,030 unique signatories (shown in gray). Considerably more individuals have signed a liberty-augmenting petition than have signed a liberty-reducing petition.

Figure 5 shows individuals who signed at least nine petitions. The most activist economists support liberty-augmenting petitions.

Figure 6 shows individuals who signed at least eight liberty-augmenting petitions signed (the set overlaps greatly with that of Figure 5). The most remarkable thing about the figure is its scarcity of red: The 63 individuals shown there lend 564 signatures to liberty-augmenting petitions, and only 5 signatures to liberty-reducing petitions—and, by the way, all five of those signatures are on the cap-and-trade petition. One of the most striking findings of our study is that there is very little crossover between pro- and anti-liberty activities. Almost all active petition signers lean heavily toward either liberalism or interventionism. Our study indicates how fundamental ideology and worldview really are.

Figure 7 shows individuals who signed at least four liberty-reducing petitions. Notice the scarcity of blue in the figure.

Figure 8 shows Nobel laureates. Although there is a liberal bent among the group overall, it is weaker than the entire population of signatures. Many of the Nobel laureates since 1990 have zero signatures, namely, Robert Aumann, Ronald H. Coase, Robert F. Engle III, Robert W. Fogel, John C. Harsanyi, Leonid Hurwicz, Paul R. Krugman, James A. Mirrlees, Roger B. Myerson, John F. Nash

7. We recorded all signatures (US and foreign) from all petitions with the exception of the petition opposing green protectionism. That petition (as of 8 May 2009) had “approximately 3300” signatures ([Link](#)). Because of resource constraints we were only able to extract signatures from the 1,216 US signatories.

Jr., Elinor Ostrom, Edmund S. Phelps, Myron S. Scholes, Reinhard Selten, Amartya Sen, Michael Spence, William Vickrey, and Oliver E. Williamson.

Figure 9 includes selected economists not included in the preceding figures.

Figure 10 shows the difference by gender. As a proportion of each gender's liberty-reducing signatures, men's liberty-augmenting signatures are 2.25 and women's are 0.83—a striking result that extends findings that women economists are more interventionist than men economists (May and Whaples 2010; Stastny 2010).

Figure 11 shows the 25 institutions by most appearances with a signature. (If the Hoover Institution were folded into the Stanford University total, Stanford would then be second in the ranking.)

Figure 12 shows the 25 institutions by most appearances with a liberty-augmenting signature. (If the Hoover Institution were folded into the Stanford University total, Stanford would then be second in the ranking.)

Figure 13 shows the 25 institutions by most appearances with a liberty-reducing signature. Many of the institutions which have the most liberty-reducing signatures also have a goodly number of liberty-augmenting signatures. In fact, for 8 of the 25 there are more liberty-augmenting than liberty-reducing signatures. Of particular interest are those universities which appear in both Figure 11 and Figure 12, namely, Harvard, University of Michigan, Columbia, Stanford, and UCLA: each has more liberty-augmenting than liberty-reducing signatures.

Figure 14 plots institutions based on the quotient: liberty-augmenting signatures/(liberty-augmenting + liberty-reducing signatures). (This set is limited to institutions with 30 or more signatures.) Of the 25 highest institutions, 22 have a quotient greater than 90 percent. (Inside each bar is the number of liberty-augmenting signatures.)

Figure 15 plots institutions based on the quotient: liberty-reducing signatures/(liberty-augmenting + liberty-reducing signatures). (Inside each bar is the number of liberty-reducing signatures.) For the 25 highest institutions, the quotient quickly falls under 90 percent and is as low as about 40 percent. This is quite different than Figure 14 showing the liberty-augmenting quotients.

For the institutions at the upper end of Figure 15, the results are quite striking. Since pro-liberty sentiment in academe is usually found especially among economists, and since the data comes from petitions consisting principally of economists, the data strongly suggests that the institutions leading this quotient are particularly unfriendly to liberty.

Several of the top 25 economics departments as ranked by *US News and World Report* (2009) (see Figure 16) are among the highest 25 in liberty-reducing quotient, whereas none except the University of Rochester and the Hoover Institution (at Stanford) are among the highest 25 in liberty-augmenting quotient.

However, among institutions with at least 40 liberty-assessed signatures, the University of Chicago, Duke University, and Stanford University (if Hoover were folded into it) are among the top 25 institutions ranked by *US News and World Report*, and they have liberty-augmenting quotients that are high, though not among the top 25 listed in Figure 14.

Figure 16 shows the signatures of the top 25 economics departments as ranked by *US News and World Report* (2009). Again, the signatures of, say, Harvard University are from any signatory from Harvard; the signatures are not confined to members of the economics departments.

Figure 17 shows a nationwide map where darker shades of blue denote a higher liberty-augmenting quotient, lighter a lower. The figure suggests a relatively interventionist bent in the Northeast, especially Maine, Vermont, and Massachusetts. On the West coast, Oregon stands out for interventionism. Meanwhile, relatively high quotients are found in some of the states of the Southeast, the Midwest, and the Southwest.

Ideology Revealed

The most notable finding of this investigation is that virtually every single economist who is active in signing petitions leans heavily in one direction or the other. The pictures tell the story better than words can, but here are some facts put into words:

- The 63 economists who signed at least eight liberal petitions lent a grand total of 564 signatures to liberal petitions, but their signatures on interventionist petitions amounted to just five!
- The 102 economists who signed at least four interventionist petitions lent a grand total of 461 signatures to interventionist petitions, but their signatures on liberal petitions amounted to just sixteen!
- There were 589 economists who signed at least three liberal petitions, but only one also signed at least three interventionist petitions (and that individual, Malcolm Robinson, signed only three of each kind). Meanwhile, there were 230 economists who signed at least three interventionist petitions.
- In fact, among the 589 who signed at least three liberal petitions, there were, besides the aforementioned Malcolm Robinson, only two individuals who also signed at least two interventionist petitions, Carl F. Christ and Peter Crampton. But these two each leaned heavily in the liberal direction, signing five liberal and just two interventionist petitions. It is fair to say that Malcolm Robinson is the *only* exception

to the finding that economists who are active in signing petitions lean heavily one way or the other.

- Twenty-five Nobel-prize economists were among the set of signatories. Five of them signed at least three liberal petitions—Vernon Smith, Milton Friedman, Edward Prescott, Thomas Schelling, and Robert E. Lucas Jr.—and among those five economists there was not a single interventionist signature.
- Six other Nobel economists signed at least two interventionist petitions—Kenneth Arrow, Joseph Stiglitz, Robert Solow, George Akerlof, Lawrence Klein, and Daniel McFadden—and among those six economists there were just three liberal signatures.

Our investigation shows just how fundamental ideas about liberty and government intervention really are in the thinking of economists—or at least those who like to sign petitions.

Concluding Remark

Judgment about the most important things—that is, the most important issues, the most important positions on the issues, and the most important arguments for and against a position—is part of the scientific discipline of political economy. Petitions are a good way to formulate and express such judgment, both professionally and in public discourse generally. We may frown on particular formulations and signatures in support thereof, but we should not frown on the practice *per se*.

Figure 2: Number of petitions by organizer/sponsor

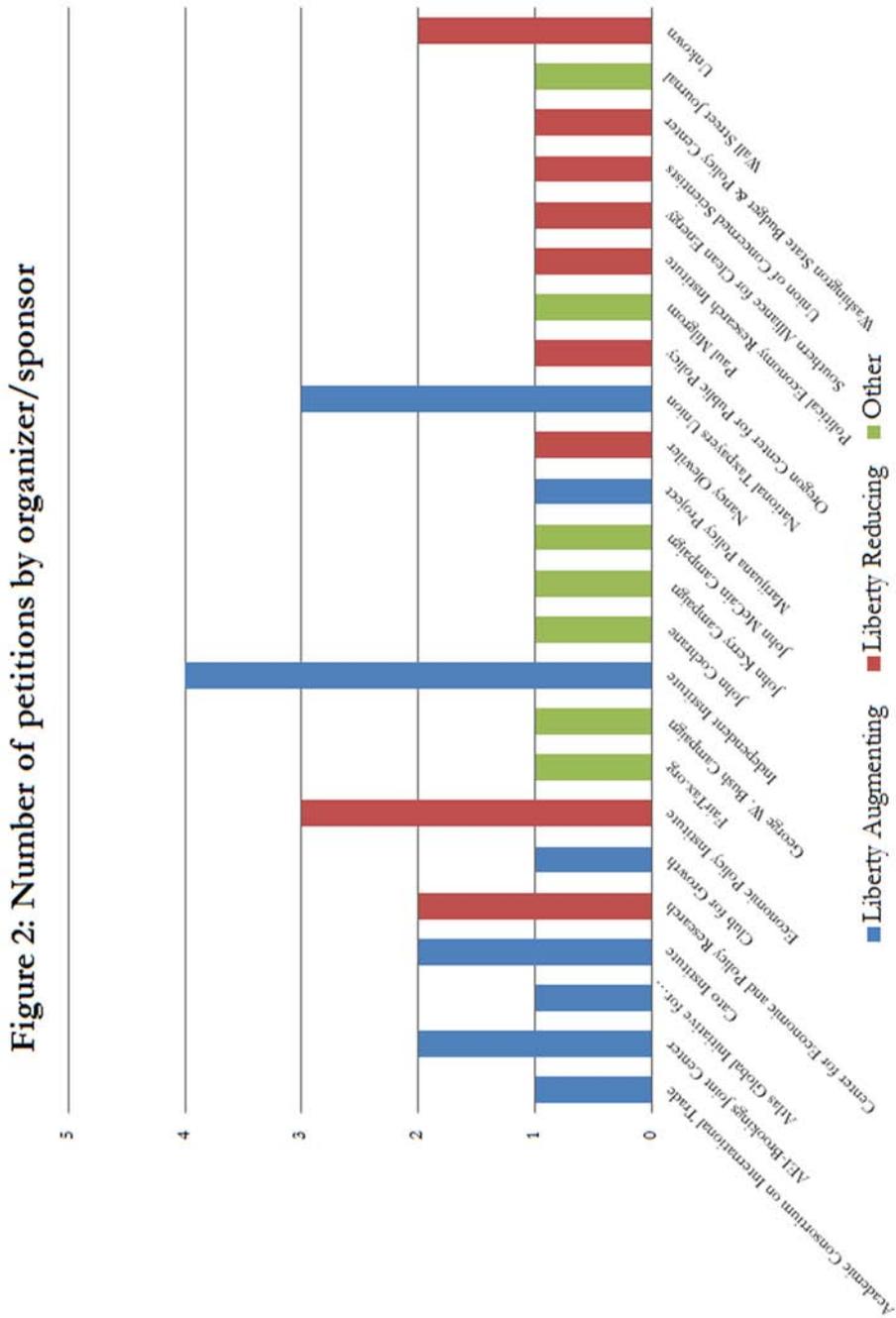


Figure 3: Number of signatures by petition type

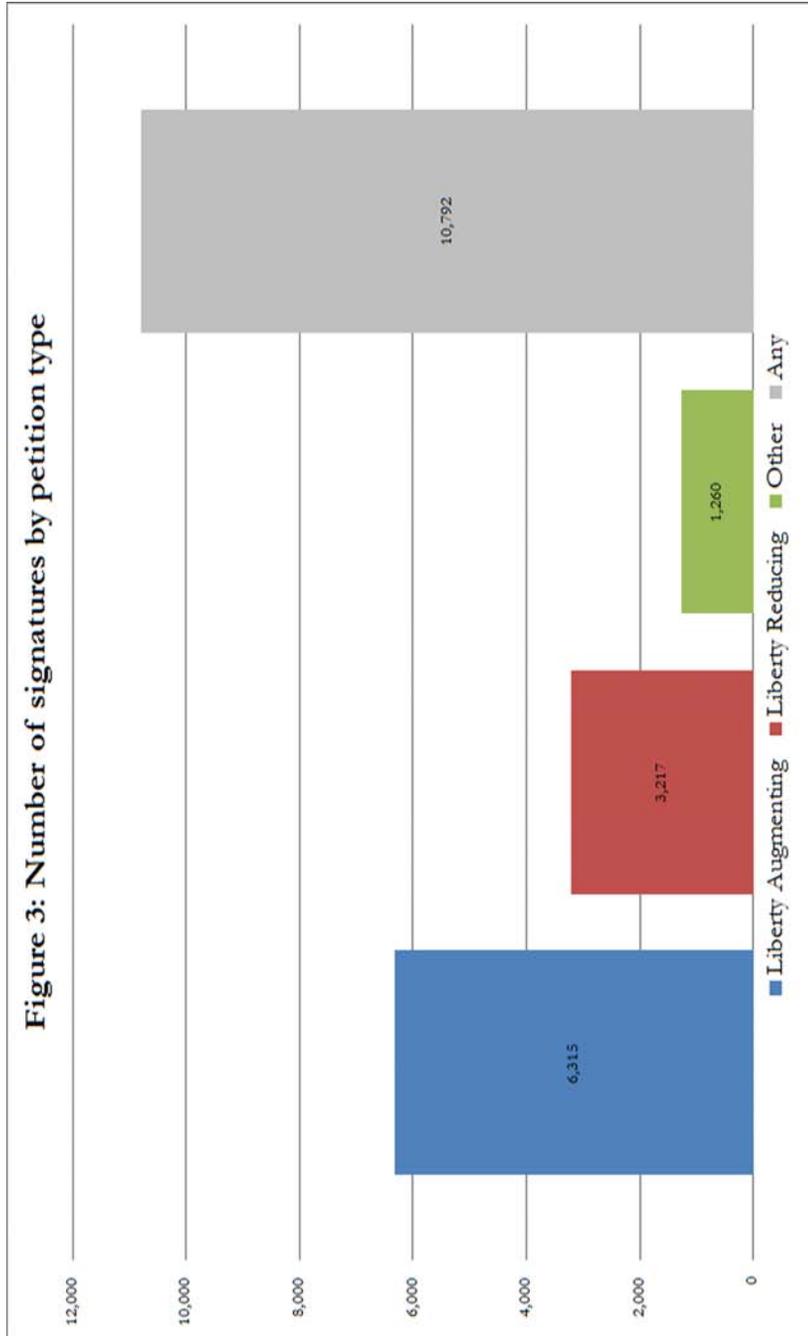


Figure 4: Number of unique signatories by petition type

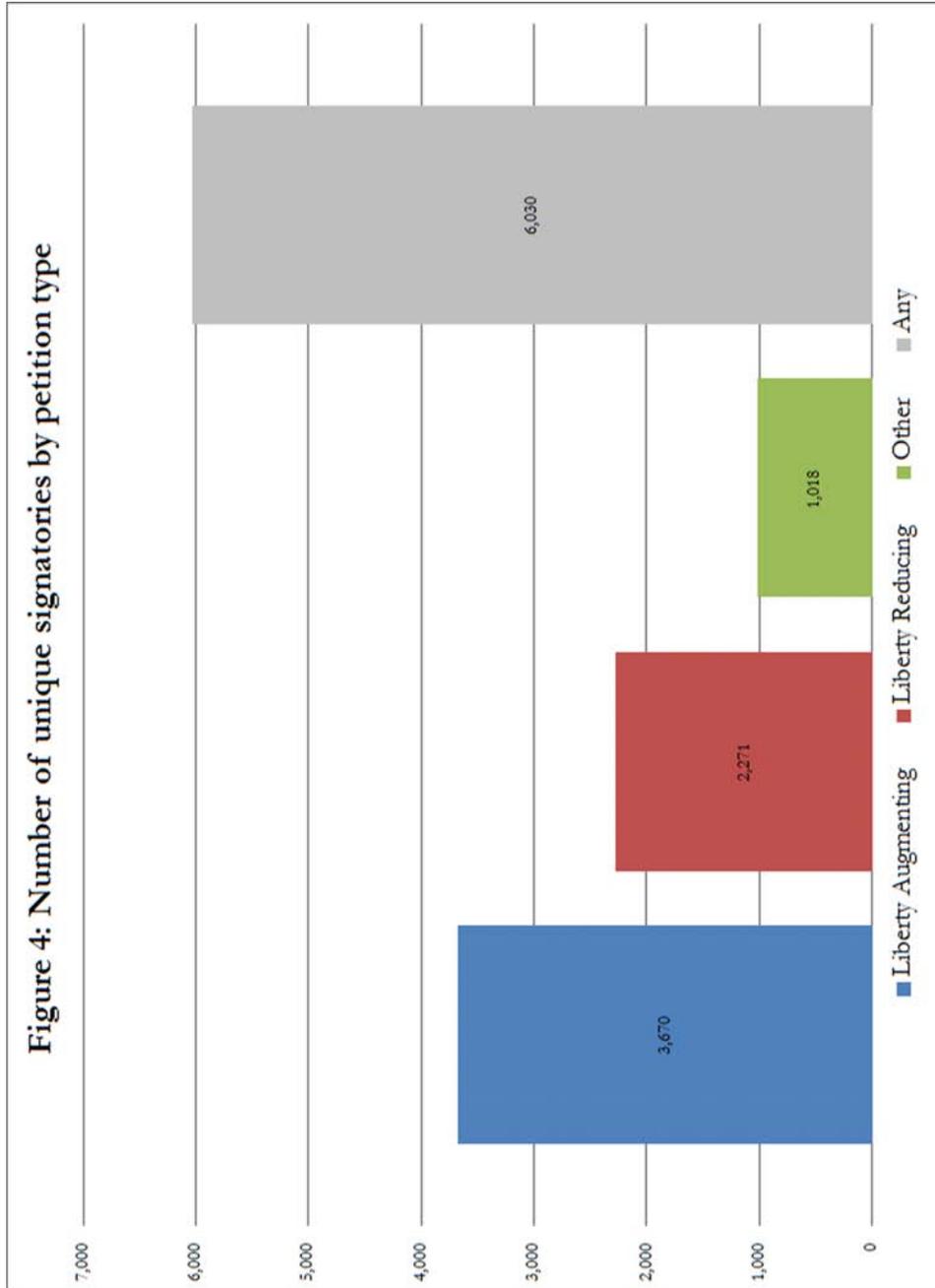


Figure 5 (1st of 2 panels): Individuals who signed at least 9 petitions

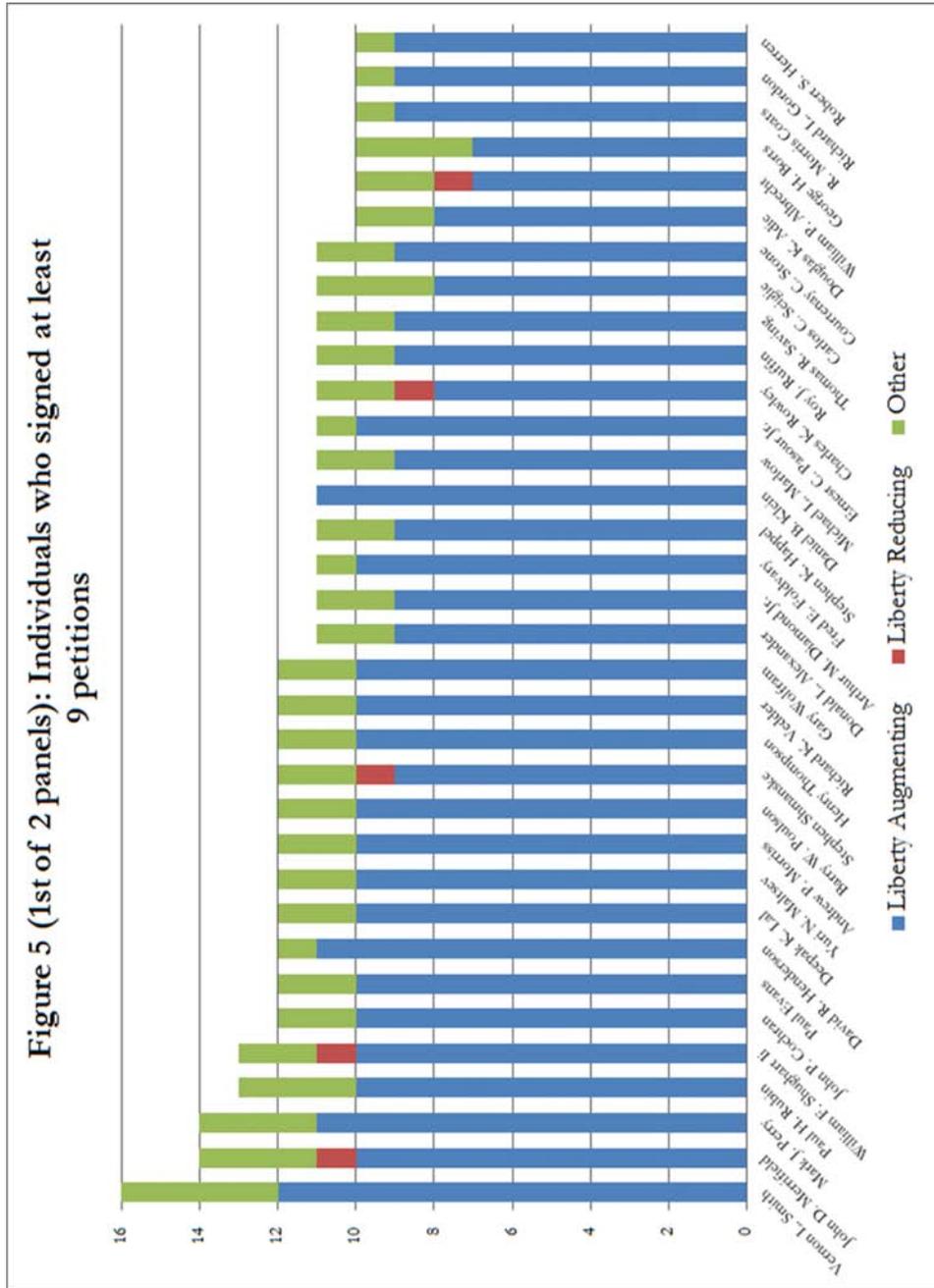


Figure 5 (2nd of 2 panels): Individuals who signed at least 9 petitions

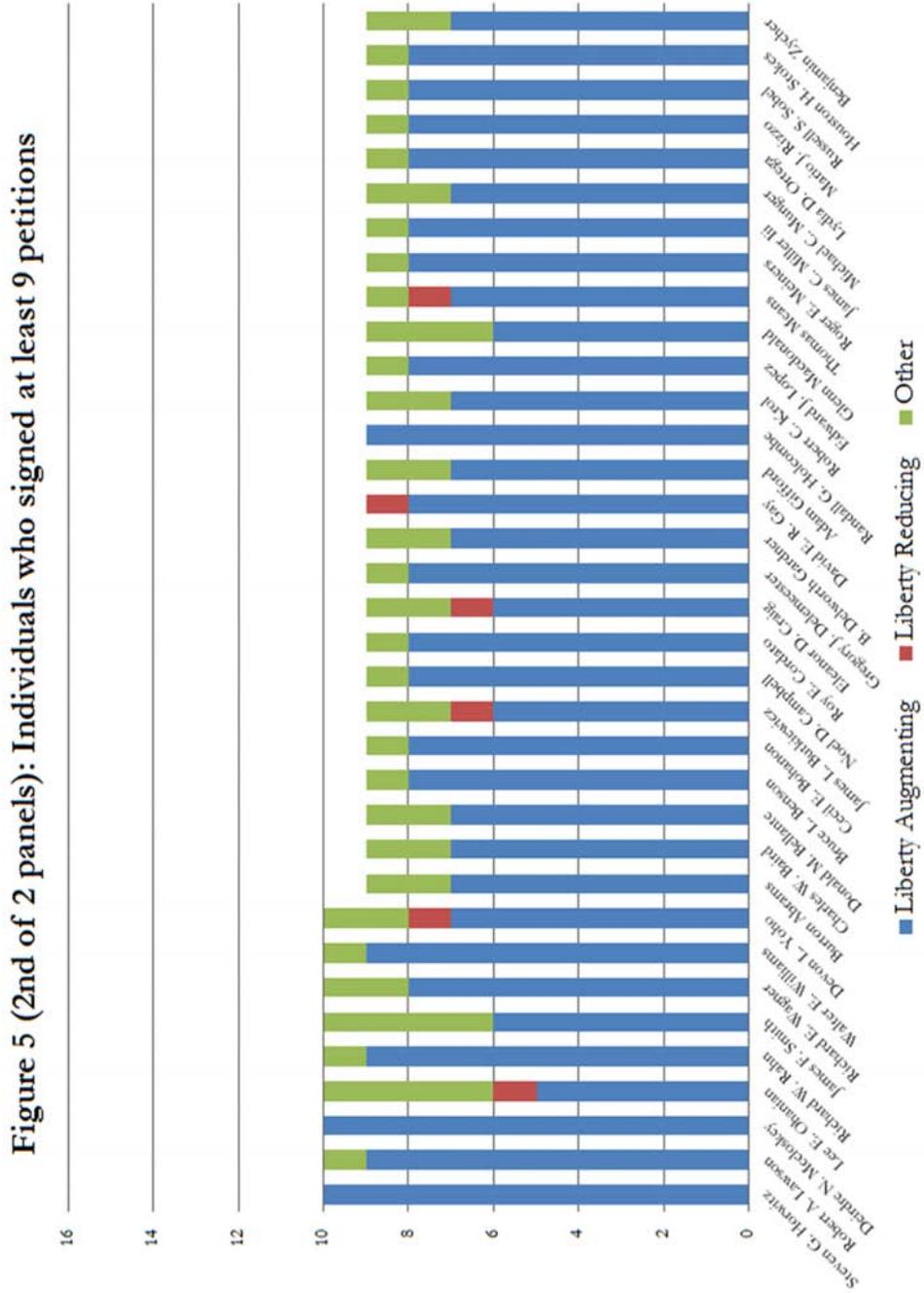


Figure 6 (1st of 2 panels): Individuals who signed at least 8 liberty-augmenting petitions

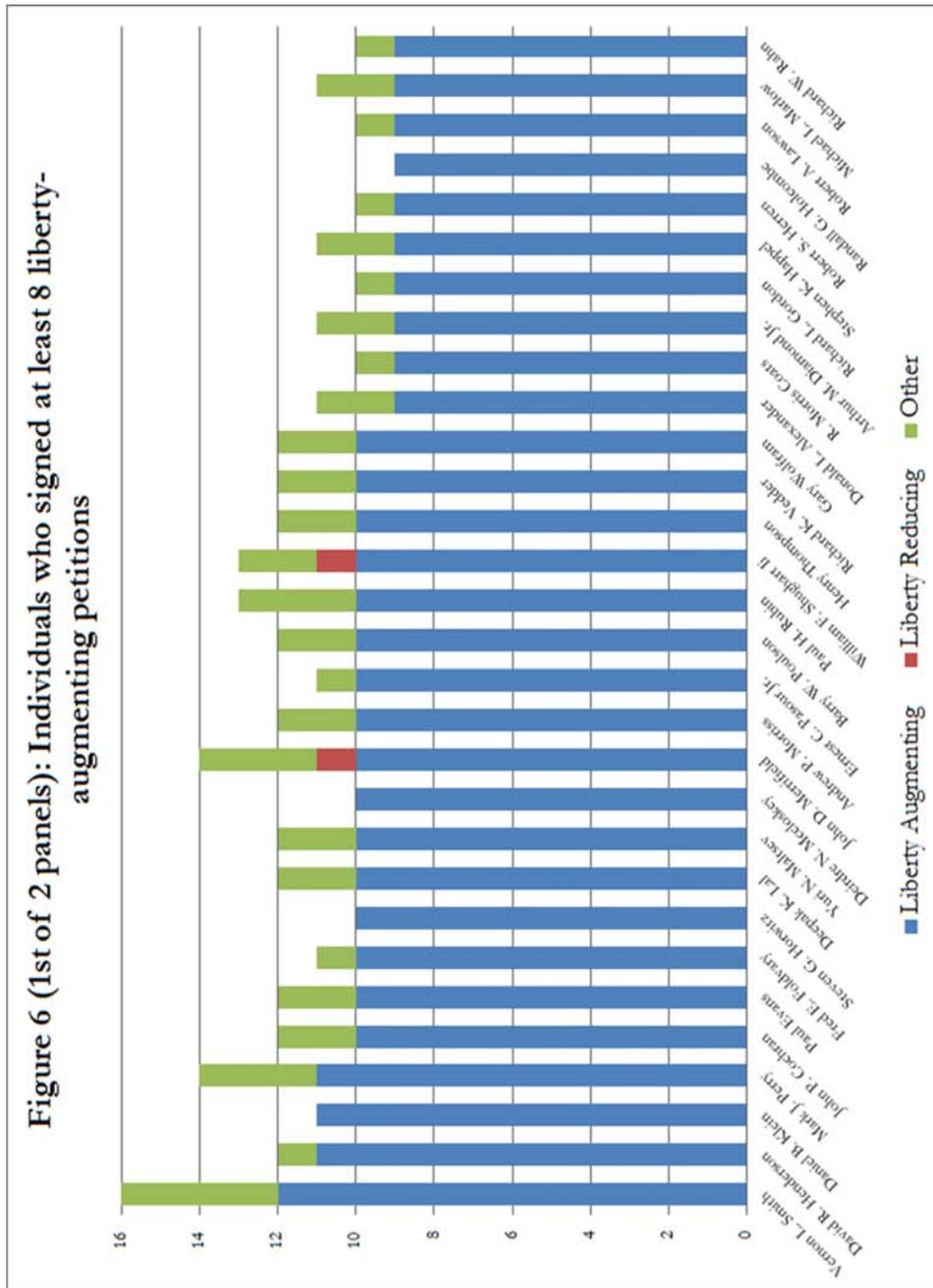


Figure 6 (2nd of 2 panels): Individuals who signed at least 8 liberty-augmenting petitions

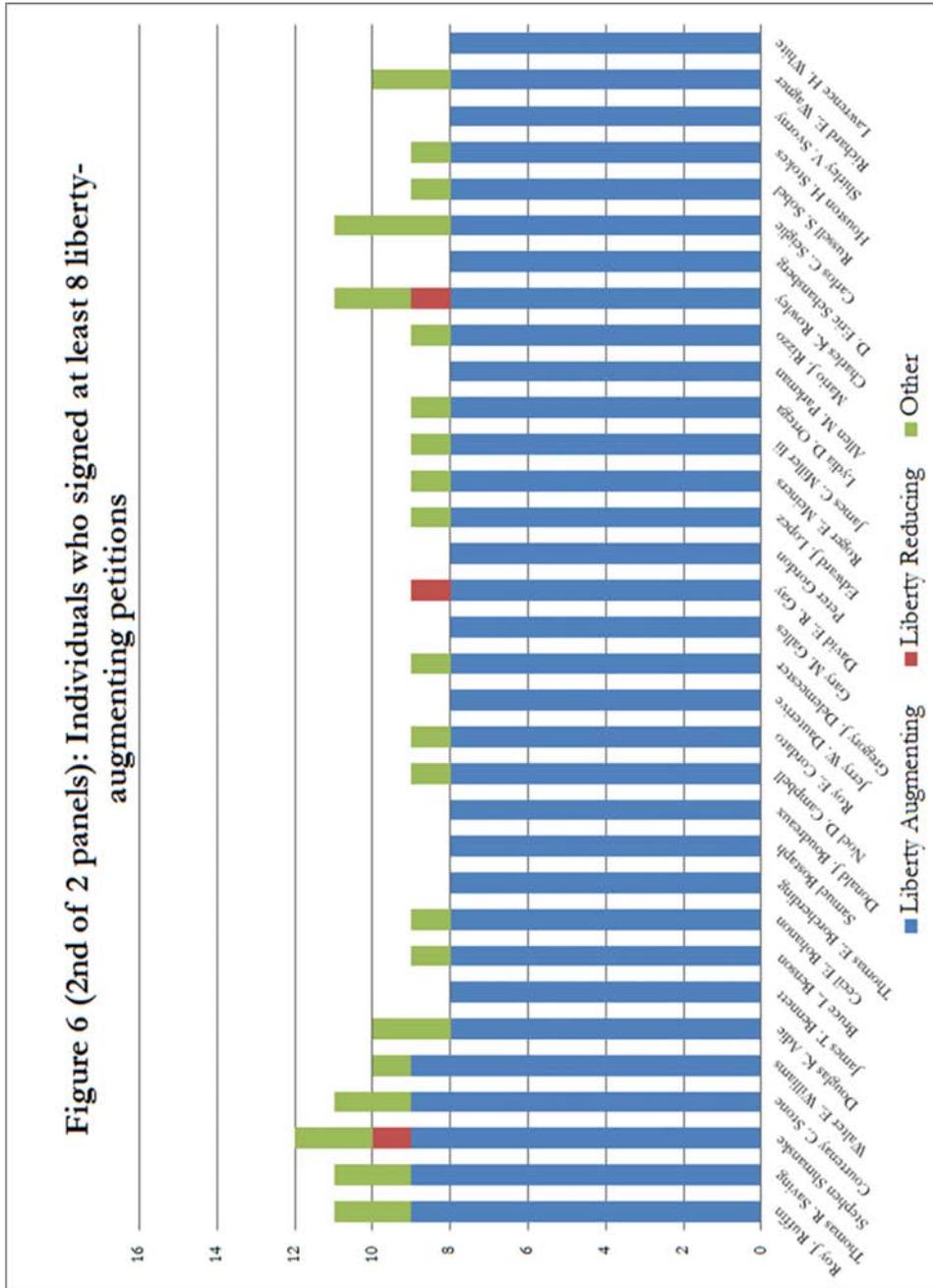


Figure 7 (2nd of 3 panels): Individuals who signed at least 4 liberty-reducing petitions

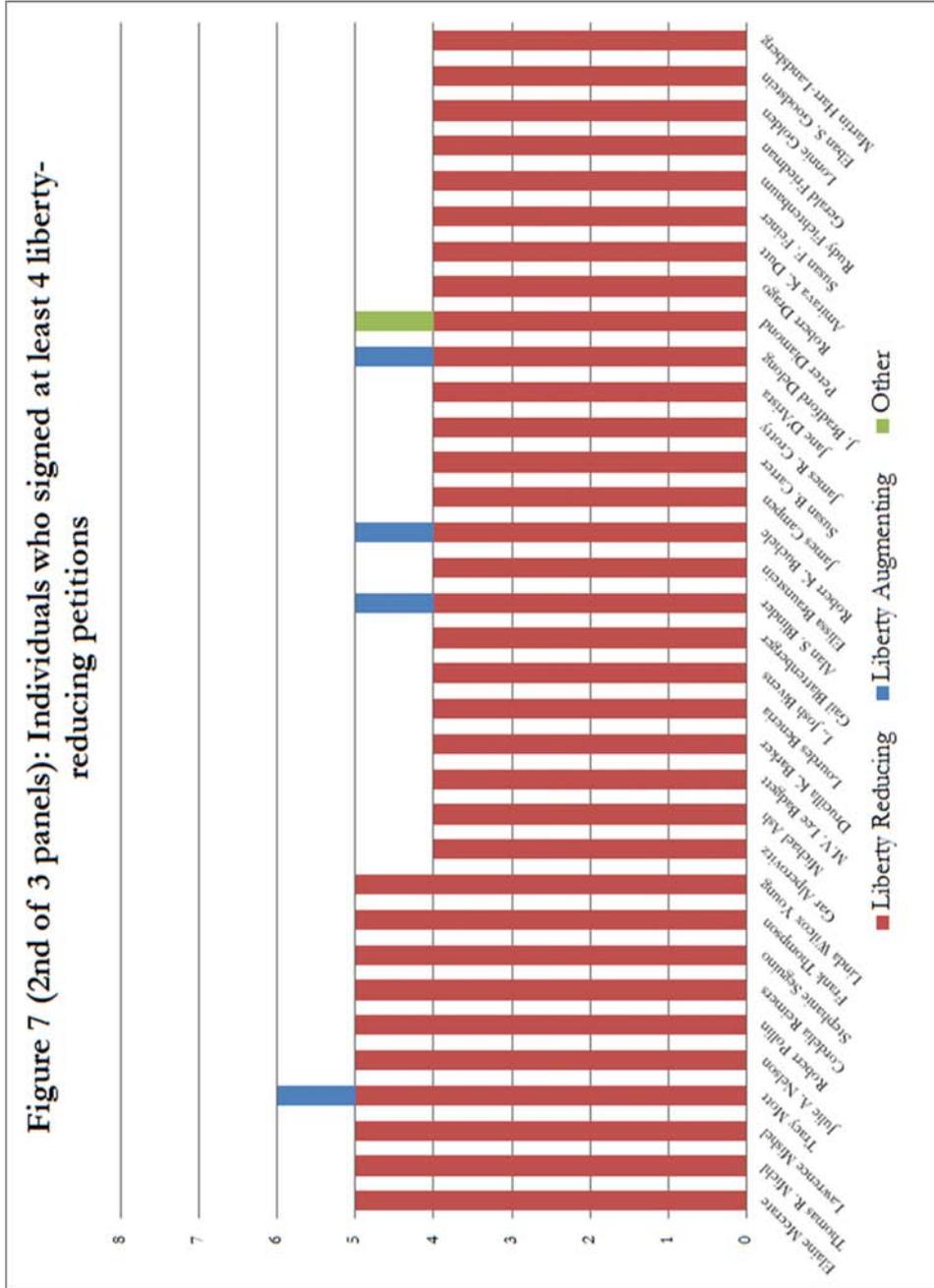


Figure 8: Nobel laureates

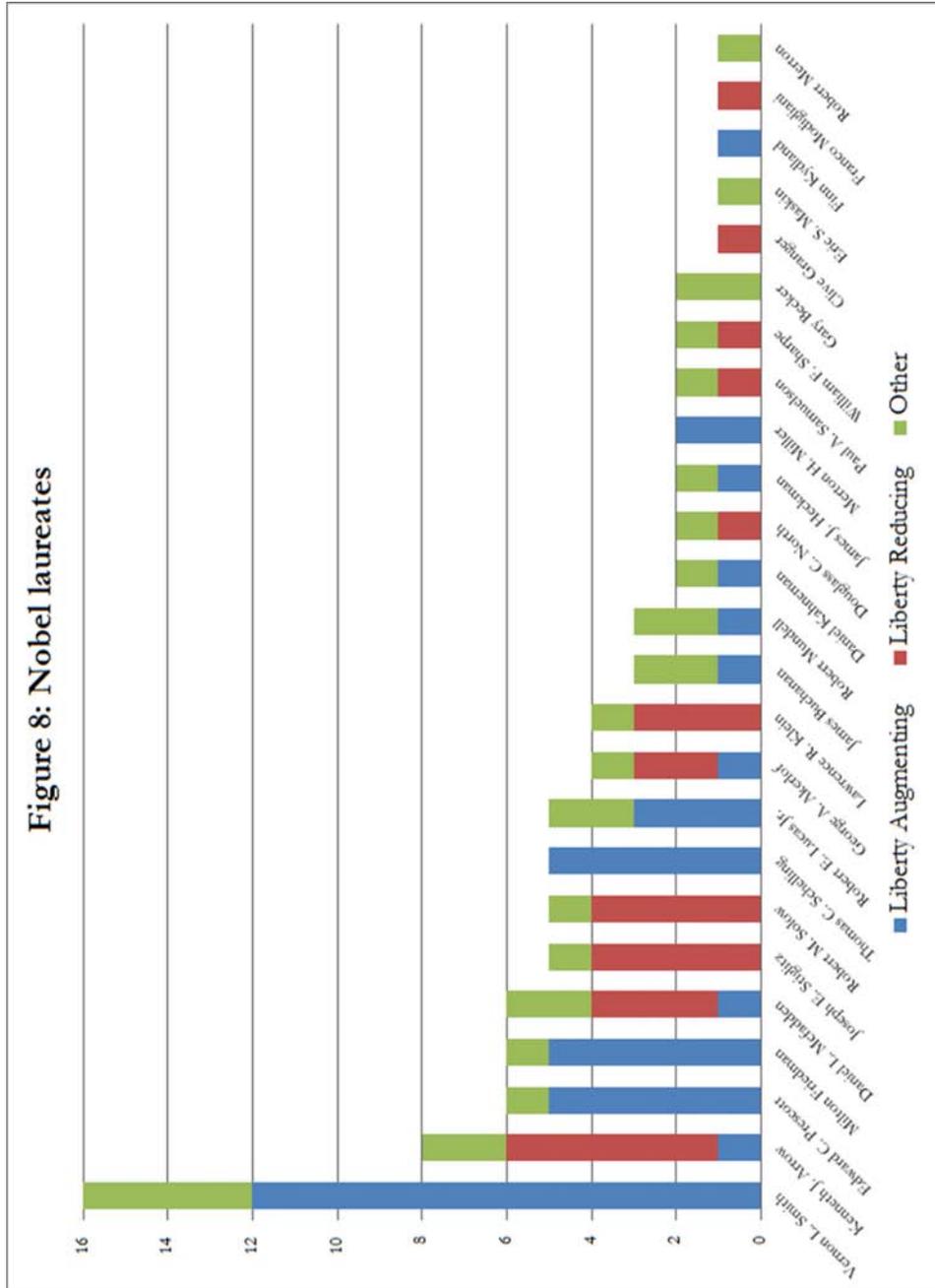


Figure 9 (1st of 2 panels): Notable economists

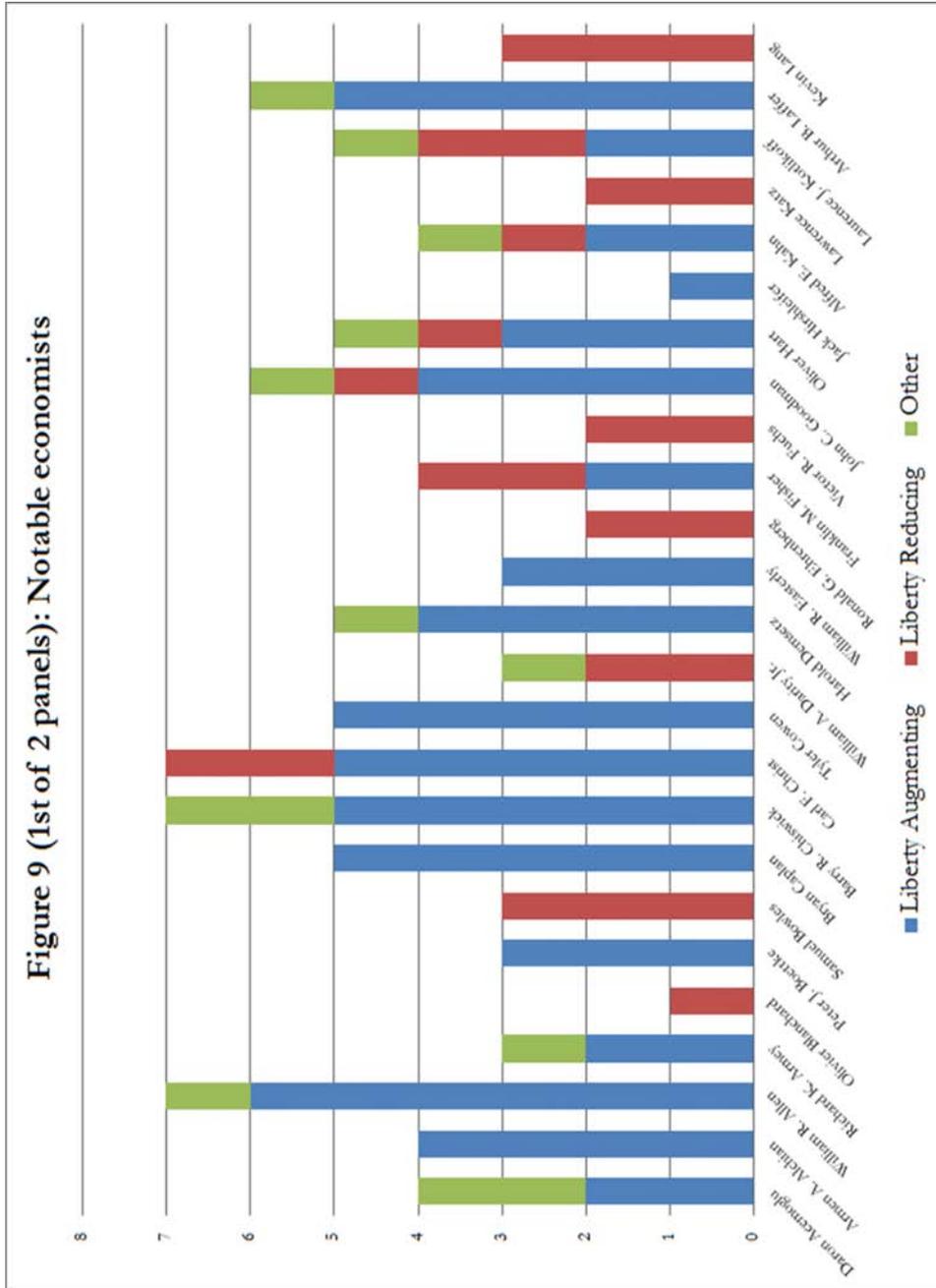


Figure 9 (2nd of 2 panels): Notable economists

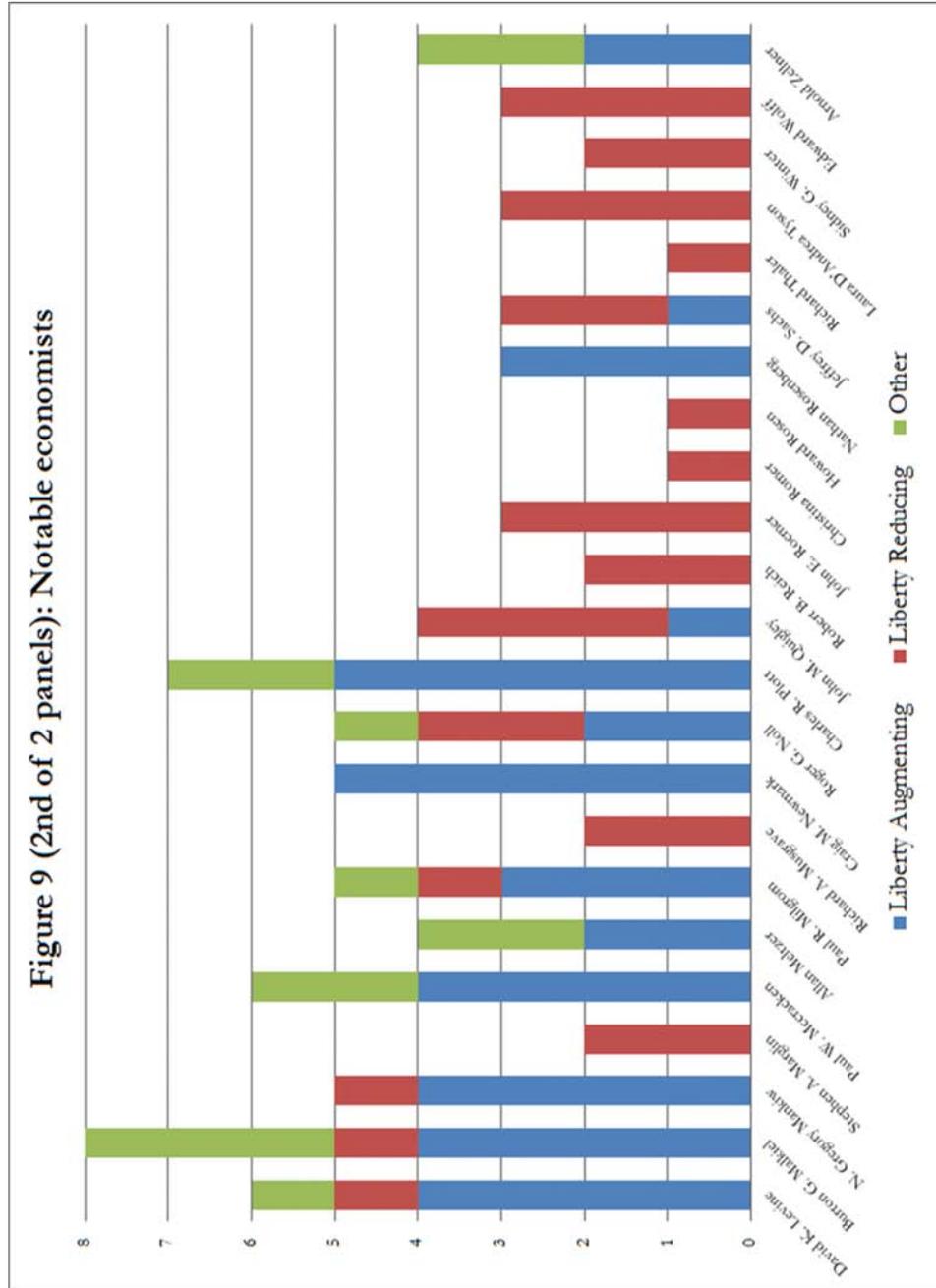


Figure 10: Number of signatures by gender

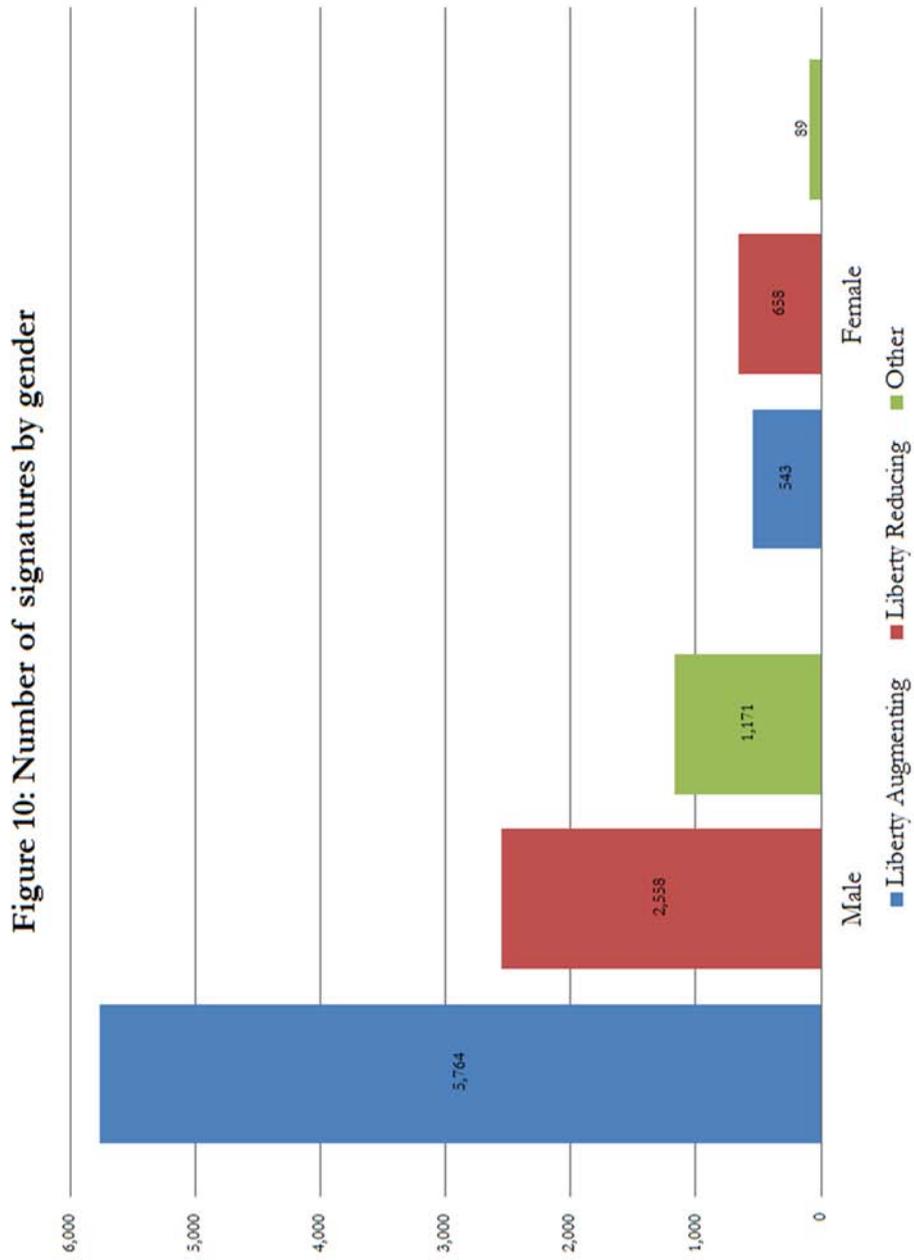


Figure 11: 25 institutions by most signatures

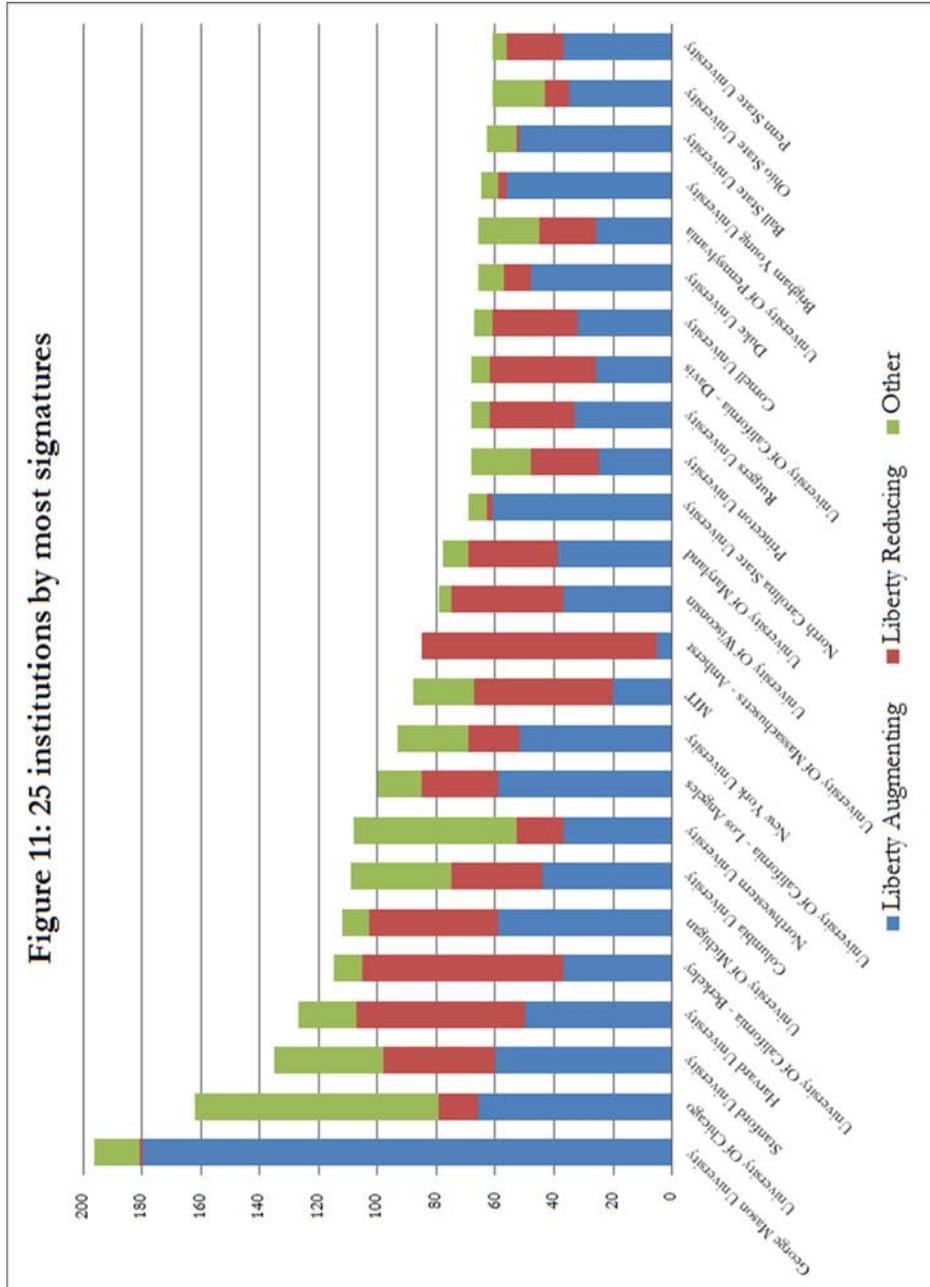


Figure 12: 25 institutions by most liberty-augmenting signatures

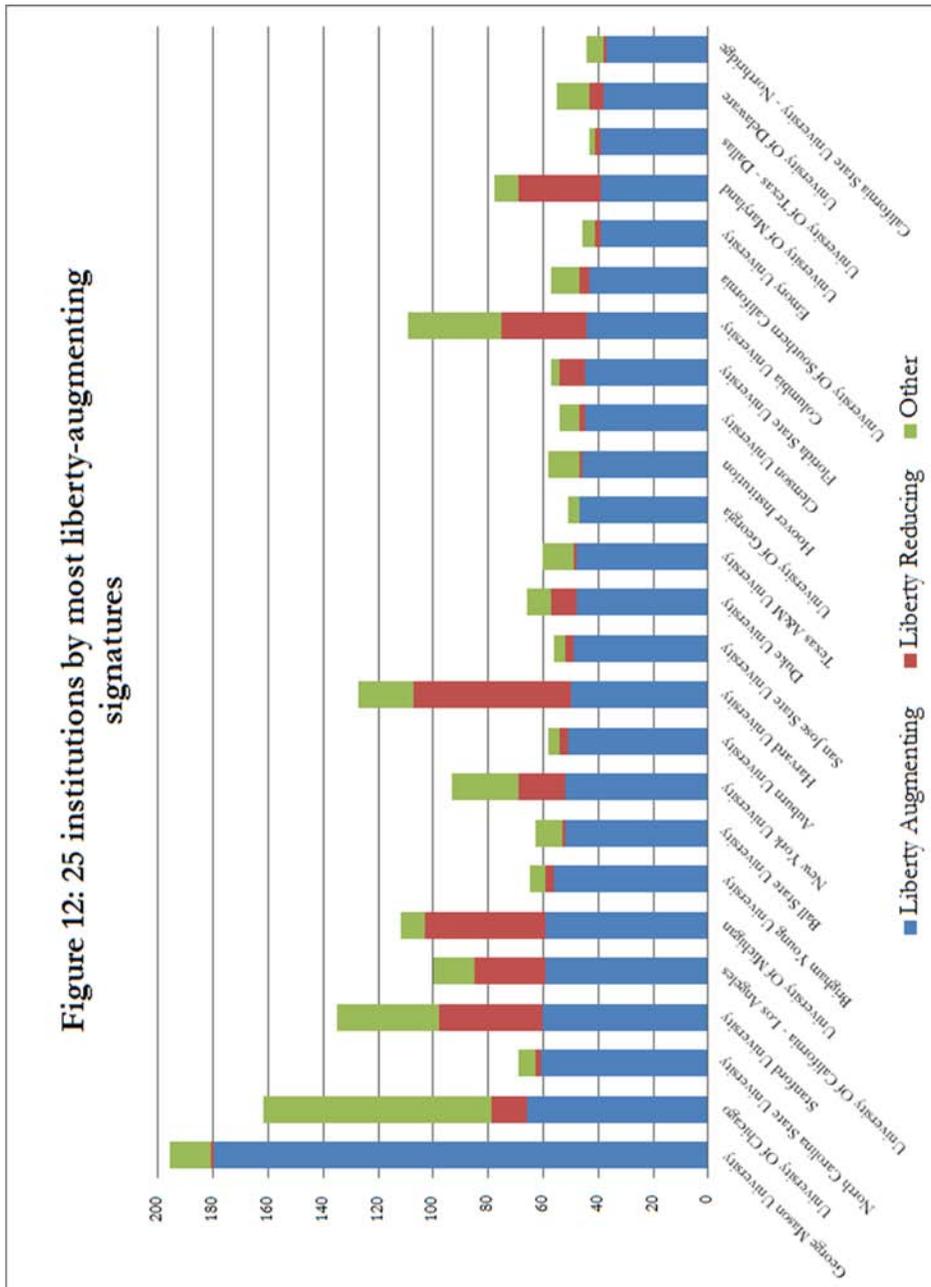


Figure 13: 25 institutions by most liberty-reducing signatures

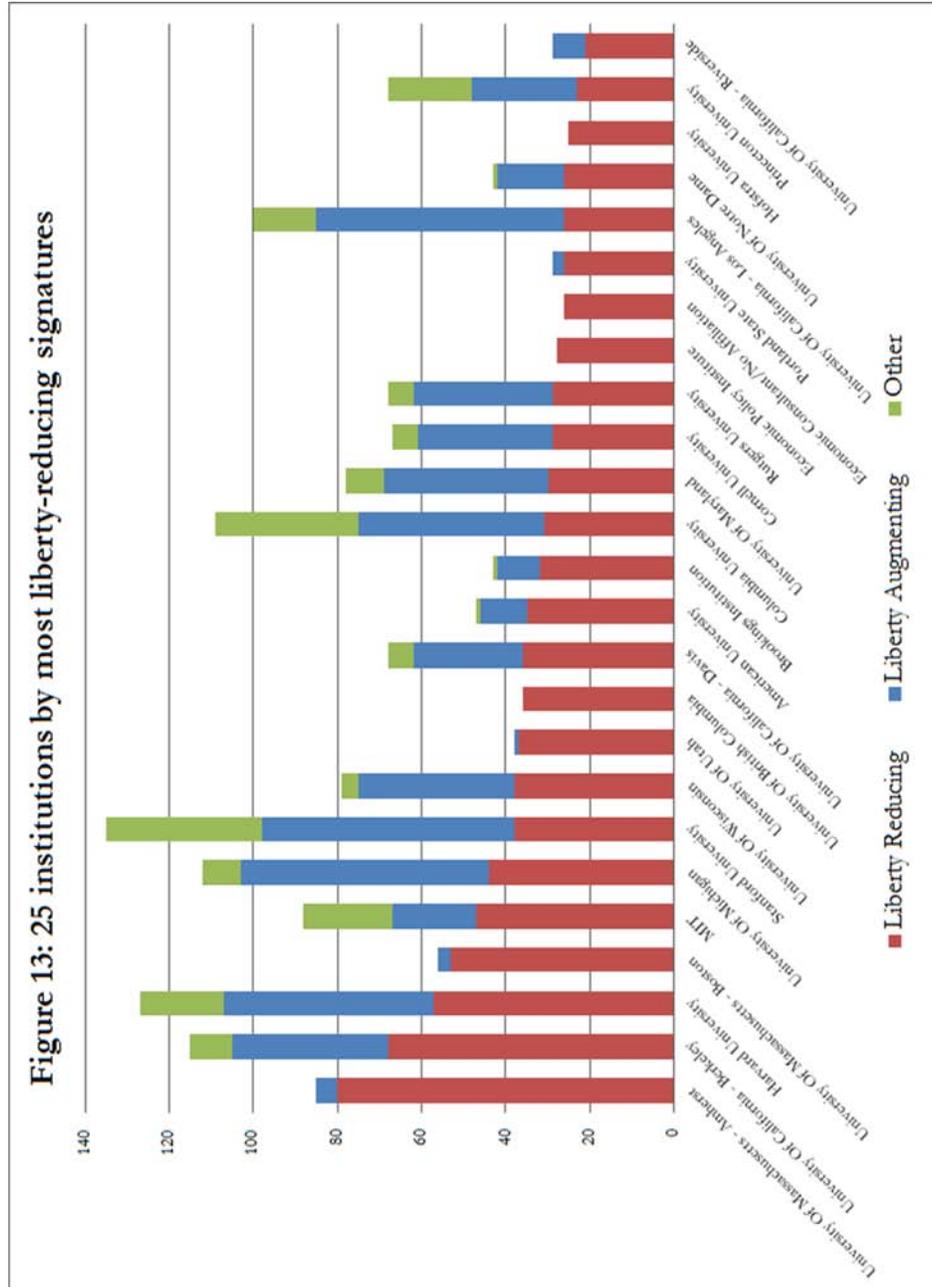


Figure 14: 25 institutions by highest liberty-augmenting quotient

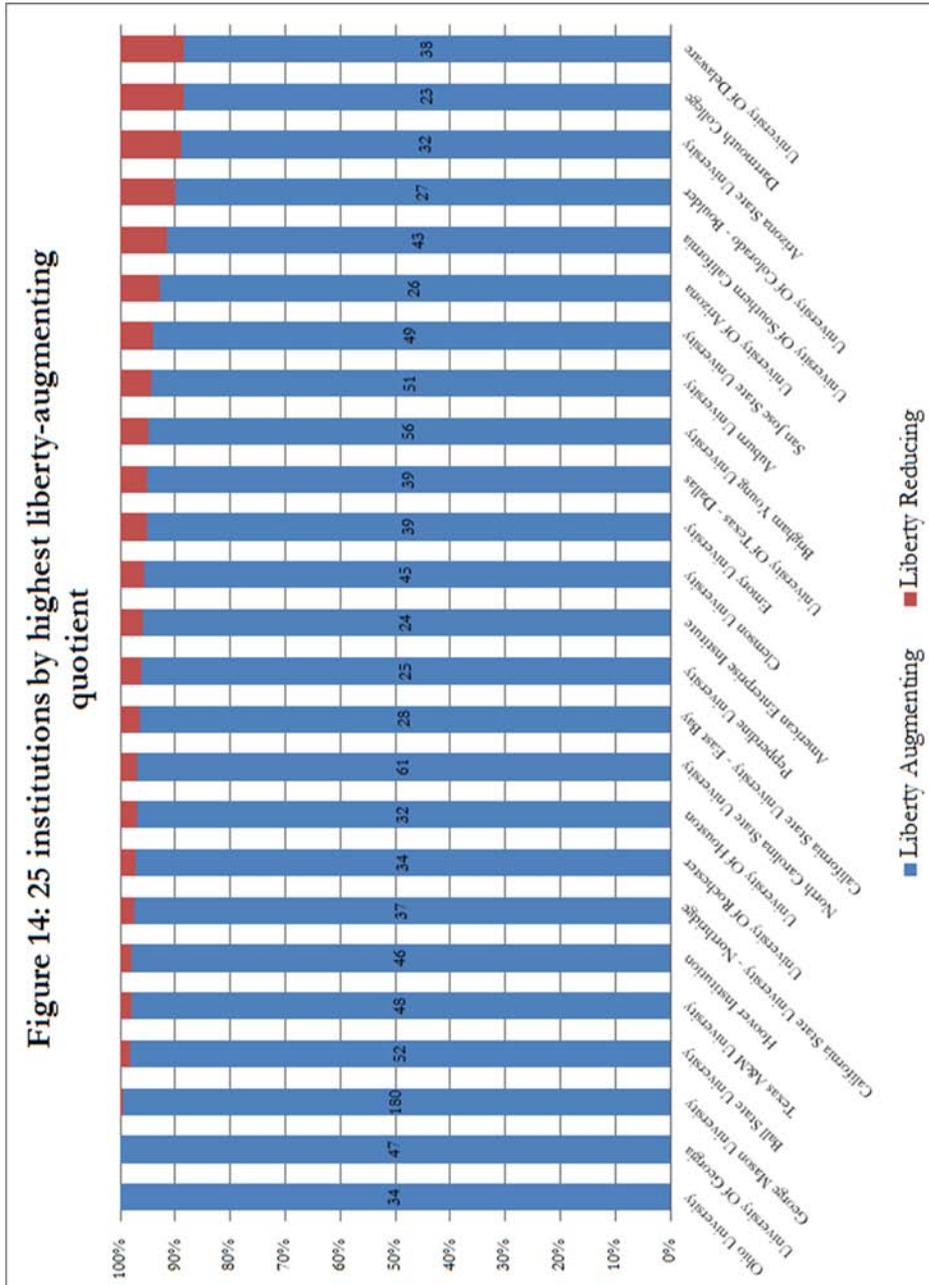


Figure 15: 25 institutions by highest liberty-reducing quotient

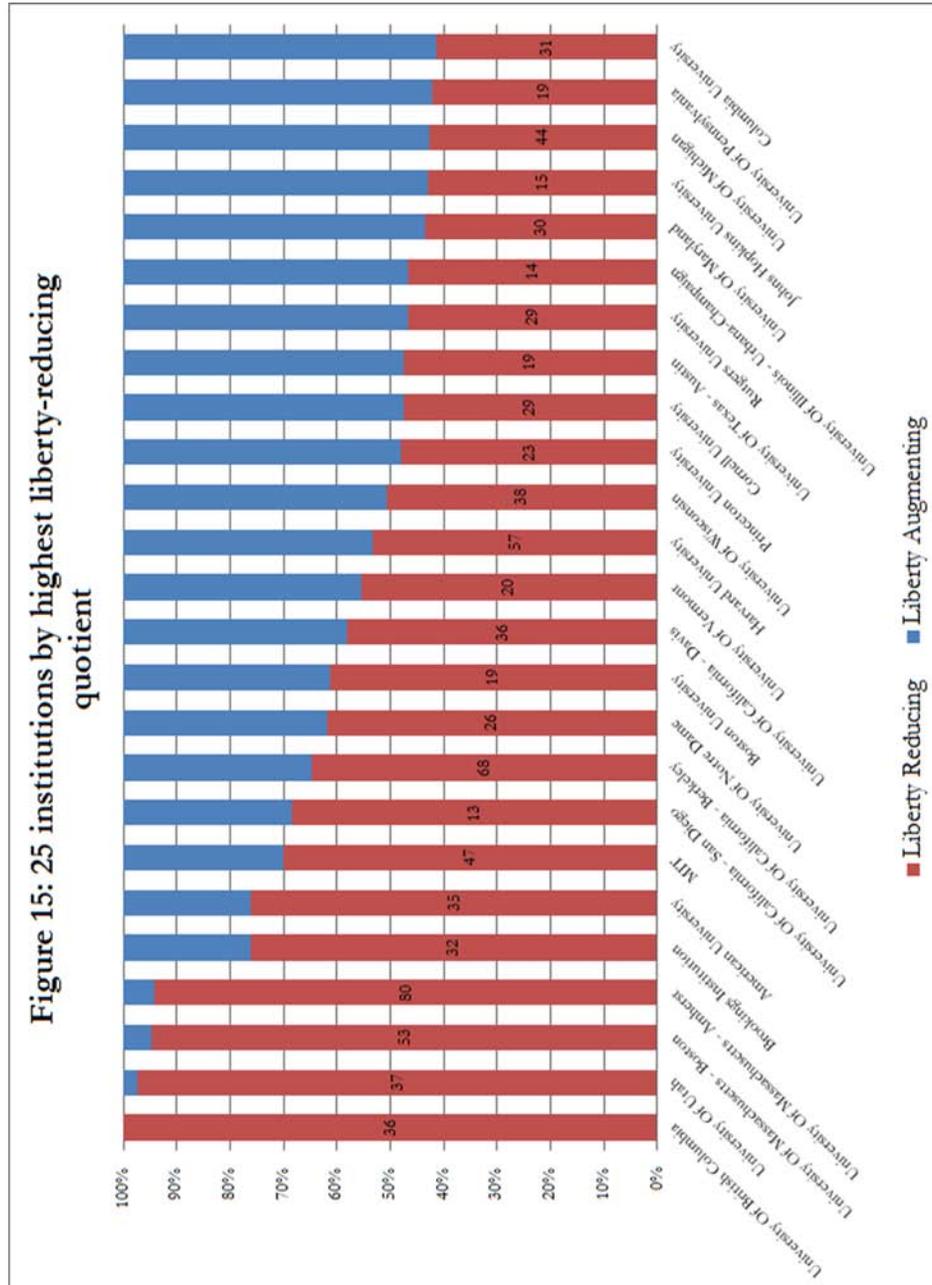


Figure 16: Signatures by top 25 economics departments (US News and World Report ranking)

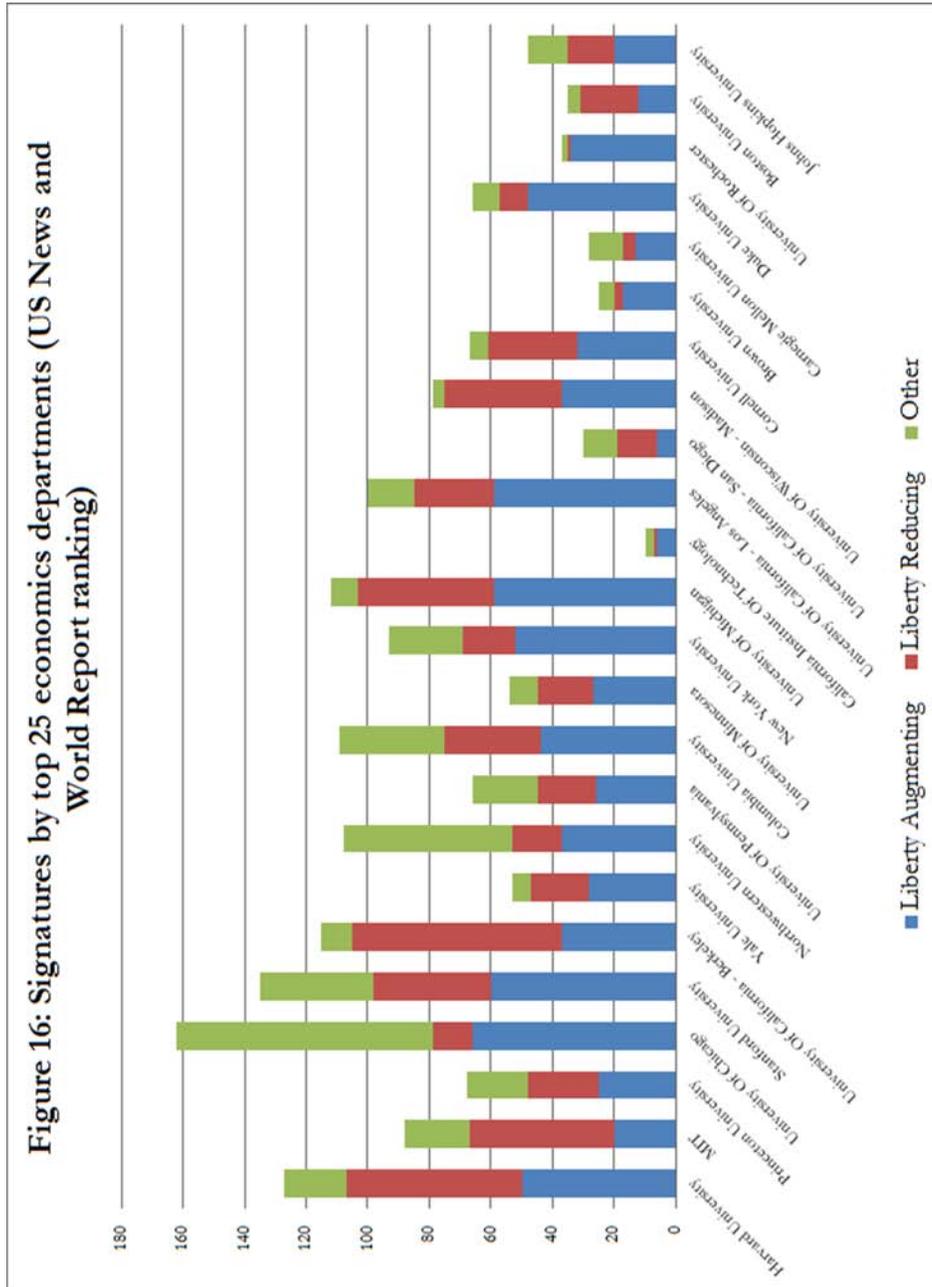
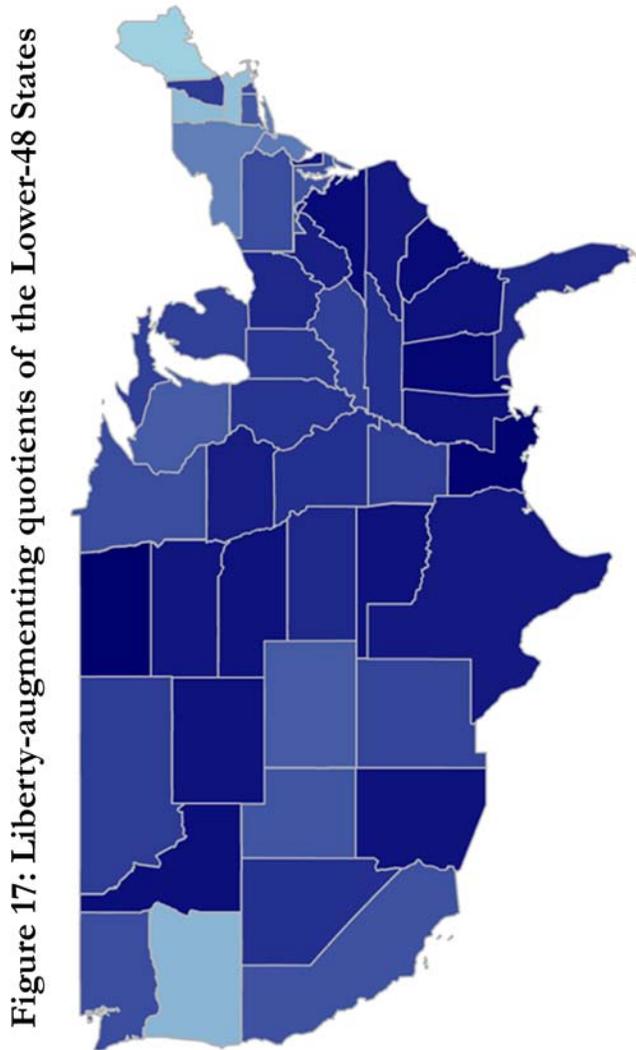


Figure 17: Liberty-augmenting quotients of the Lower 48 States



Appendices

Appendix 1: Excel file containing all signature data: [Link](#)

Appendix 2: PDF containing text of all 35 petitions: [Link](#)

Appendix 3: SAS codes for generating the figures contained in this paper: [Link](#)

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Do Economists Reach a Conclusion on Organ Liberalization?

Jon Diesel¹

ABSTRACT

The first successful human kidney transplant was performed in 1954. The advent of immunosuppressant medications greatly expanded the possibilities in matching donors and recipients. Cadavers (the recently deceased) could now serve as a source for organs and tissues. Improved surgical techniques have also increased the quality of life and survival rate for transplant recipients (Kaserman and Barnett 2002, 1-2).

In 1968 the United States began regulating the market to establish a system of altruistic giving under the Uniform Anatomical Gift Act (UAGA). Subsequent events led to fears of peddling organs for profit, resulting in the National Organ Transplant Act (NOTA) of 1984. NOTA bans payment for bodily organs intended for transplantation. It also establishes systems of organ procurement and distribution. Beard, Kaserman, and Saba describe the cadaveric organ procurement system as, “a set of nonprofit monopsonists that are constrained to pay a zero price for this essential input” (2006, 14). Cadaveric organs are harvested by Organ Procurement Organizations, which are paid to harvest organs from recently deceased individuals. But the families of the deceased cannot be compensated (Beard, Kaserman, and Saba 2006, 14). The organs then go into the Organ Procurement and Transplantation Network, the system mandated by NOTA to facilitate the conveyance of organs to recipients. It is administered by

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the United Network of Organ Sharing, which runs a national database designed to find matches between organs and donors. Organs are allocated using a combination of medical and non-medical criteria (Weimer 2007, 18-19). Examples of non-medical characteristics are time on the waiting list and whether the recipient had previously donated an organ (Tabarrok 2002, 109-110).

The 1984 legislation was enacted prior to transplantation becoming common. There is a growing list of patients in need (Kaserman and Barnett 2002, 9). The shortage drew the attention of economists. Since the 1980s economists have been offering policy analysis, but they debate the role that markets should play. Do economists reach a consensus on organ liberalization? I perform a survey of published judgments to find out. An Excel file ([link](#)) contains passages to justify my treatment.

The Current System

The issues surrounding cadaverous organs involve multiple parties—the person recently deceased, that person’s heirs or next of kin, governmental authorities, health organizations, and possibly designated beneficiaries. Actual practice in the United States varies considerably by state and on the spot often without clear legal demarcations and boundaries.

The current systems for organ donation and allocation are governed by UAGA and NOTA, which prohibit compensation, but they do not prohibit donation or gifting to a designated beneficiary² (Cohen 1989, 6-8). Some economists describe the situation as a fixed price of zero resulting in a shortage, reduced quantity transacted, and a significant loss in life and health (see Barney and Reynolds 1989, 16; Schwindt and Vining 1986, 491; Kaserman and Barnett 2002, 93; Cohen 1989, 1).

The legislation sets rules that create confusion, making contract and execution difficult. The rights to disposition of one’s body rests with the individual; upon death the rights go to the next of kin (Cohen 1989, 7). The confusion lies in the many steps involved in procuring an organ. If the deceased’s intentions are known, then the procurement agency may execute those wishes with or without the families consent. Procurement agencies often defer to the wishes of the family to avoid situations that would portray the agency negatively (Howard 2007, 26). Also, there have been examples of certain types of organs, such as corneas, that were harvested regardless of the deceased or the family’s wishes (Tabarrok 2010b). In 1987 the federal government passed into law a form

2. Donors may specify an individual, but cannot specify or exclude a group of individuals.

of required request, meaning that hospital funding of Medicare and Medicaid is tied to a mandatory request to the next of kin for donations (Cohen 1989, 21). In this way, the next of kin have been empowered to veto the donor intent of the deceased.

Lloyd Cohen describes the system as gravely suboptimal. The donor, or donor's family, receive no positive monetary incentives for donating. The hospital and doctors face negative incentives, in the form of monetary punishment, for not requesting donation from the family. The negative incentives also impose the burden of making requests for donation to grieving families onto medical professionals, potentially reducing the likelihood of donating (1989, 23).

The tight requirements for donor candidacy reduce the pool of likely donors to about one percent of all deaths. The person must have been healthy, free of diseases or infections, within a preferable age range, and have a cause of death that does not eliminate their candidacy (Kaserman and Barnett 2002, 9). The growing waiting list for cadaveric organs has led to an expansion of organs considered viable for transplant. The use of expanded criteria organs has made little progress in providing the desired amount of organs (Tabarrok 2010b). The process required to elicit donations further reduces the number of cadaveric organs donated. Insincerity or ineptitude at such a traumatic time reduces the likelihood of families consenting to donation. It is estimated that of the medically viable *donors* only 25-50 percent are harvested (Kaserman and Barnett 2002, 10-11).

Organs are entered into the national database to search for matches. Local recipients are given priority due to the small window of opportunity for transplant. Patients are ranked using a points system. The highest points are awarded to patient most likely to have a successful transplant. The doctor of that patient is contacted, and the organ is offered to him. If the physician refuses, then the organ is offered to any other local recipients first, then the organ becomes available to matches in a wider region. The criteria differ by organ type. Some require blood type matches or other idiosyncrasies that limit availability (Kuznik 2004, 7-11).

What Should Be Done?

Some economists focus on improving the workings of the current system. Exhortation activities are directed at potential donors, surviving family members, and medical professionals (Thorne 1998, 249). The proponents of more exhortation believe that procurement agencies under-invest in procurement activities because they fail to capture all of the returns on their investments (Thorne 1998, 253). Some proponents of exhortation feel the increased effort in

securing more donations would be cheaper than implementing a market oriented system (Thorne 1998, 256).

Barter famously suffers the “double-coincidence of wants” problem. The government’s prohibiting of the mediating of wants by resort to the medium of exchange—money—has spurred the pursuit of workarounds. People strive to mitigate the transaction costs of government-imposed barter.

One method for overcoming the current legal constraints is donor pools. As Tabarrok puts it, “the policy of the United Network for Organ Sharing (UNOS) is that organs are a ‘national resource’” (2002, 108). Donor pools restrict access to organs to individuals whom have shown a commitment to donate their own organs, which solves the commons problems (Tabarrok 2002, 109). Donor pools can and do operate under the current system; Lifesharers is an example of a private organ sharing club (Tabarrok 2010b). Donor pools provide a signal of willingness to participate, and a credible commitment that ameliorates the commons problems (Tietzel 2001, 169). Willing donors are given preference on recipient lists or organs are restricted to members of the group (Tabarrok 2010b). Donor pools have been criticized as discriminating against those unaware that access to organs would be contingent on membership. According to David Howard, donor pools may introduce non-medical variables into the allocation process that break with egalitarian principles (2007, 34).

For living donation of kidneys, Alvin Roth, Tayfun Sonmez, and M. Utku Unver tackle the concept of a clearinghouse to facilitate swaps among four individuals, two donors and two recipients. An inoperative pair does not have the needed matches in biological characteristics, but each may be operative when crisscrossed with another inoperative pair (Roth, Sonmez, and Unver 2004, 459). There is a national database for donors waiting for cadaveric kidneys, but to date, there is not a national database for patients having a willing, live donor that does not match the recipient (Roth, Sonmez, and Unver 2004, 460). Roth et al’s clearinghouse is an example of reducing transaction costs in a barter system: a necessity given the current legislation.

If liberalization were to repeal the government prohibition on using money, markets would develop. Economists have proposed a litany of market-oriented reforms ranging from minor compensation to donors all the way to an open market for organs. One of the first reforms proposed by economists was a futures market that targeted the supplier’s (donor’s) incentives to induce greater organ supply. Compensation in futures markets can take many forms, and involve a wide array of groups as procurement agencies. Organs could be sold by the individual and procured by private parties (Brams 1986, 13), insurance companies (Hansmann 1989 64), or the government as a monopsonist (Schwindt and Vining 1986, 489). The payment could be made when the contract is created or when the

individual dies. Suggested payments have varied from cash payments to the individual (Adams, Barnett, and Kaserman 1999, 154), cash payments to a designated beneficiary (Cohen 1989, 2), or non-liquid reimbursement such as covering funeral services (Hansmann 1989, 62) or tuition assistance (Schwindt and Vining 1986, 496). The individual would knowingly, and freely, enter into a contract while having property rights over his body. The individual passes those rights to the procurement agency. Futures markets would reduce the confusion caused by the next of kin being involved (Cohen 1989, 2).

The method of procurement and compensation within traditional markets can vary as much as in futures markets. Markets would consist of buyers and sellers. There are only two organs that can be sold while living. A person can survive after donating a kidney or a portion of the liver (Kaserman and Barnett 2002, 6). Becker and Elias estimate the compensation for a kidney in the US would be in the ballpark of \$15,000 (2007, 14). Others have proposed a regulated market in which organs are procured from individuals by the government and allocated using something like the current system (Matas and Schnitzler 2004, 216). Proponents of markets suggest that living donors are preferable to cadaveric donors, because cadaveric kidneys have a short window for use. Living donors provide opportunities to find better matches reducing the likelihood of the organ being rejected (Becker and Elias 2007, 16-17). But for organs such as hearts, lungs, intestines, or corneas, the only source is cadavers (Kaserman and Barnett 2002, 1-2).

Markets are criticized for potentially crowding out altruistic donors. Most economists cite Bruno Frey (1993) on altruism and pricing. He shows that in some cases pricing can have a negative effect on participation rates when participants perceive that accepting payment degrades their sense of virtue (Frey 1993, 654). Some worry that financial incentives will reduce the total number of donations, because the altruists will no longer donate. Richard Epstein³ offers a rebuttal. An individual could preserve his self-image of “altruist” by declining payment or donating the payment to a charity of his choosing. And even if some “altruists” are inhibited, they may be replaced by individuals seeking compensation. Moreover, a high price signals the humane significance of the act of supplying the organ. Epstein contends that markets with crowding out could do no worse than the current system (Epstein 2008, 475-77). The issue is addressed extensively in Taylor (2005), *Stakes and Kidneys: Why Markets in Human Body Parts Are Morally Imperative*.

3. I opted to include Epstein as an economist. Although he does not have a degree in economics, such activities as being the director of the Law and Economics Program at the University of Chicago Law School, editor of the *Journal of Law and Economics*, and so on, would seem to qualify him as “economist.”

Presumed Consent and Mandated Choice

I categorize each economist by the reforms he or she seems to favor. The categorization is rooted in the status quo, and then asks the primary question: Does the economist seem to be favoring liberalization, greater restriction, or simply the status quo? The concept of “liberalizing” from the status quo is synonymous with the liberty principle: Does the endorsed reform represent an augmentation or a reduction in liberty?

Two kinds of reforms are tricky to parse. One is called *presumed consent* (or implicit consent), which would change the default to universal organ donation at death (Abadie and Gay 2006, 600). Under the reform, a person who objects to donating his organs must explicitly state his objections during life or his organs will be subject to harvesting (Cohen 1989, 14). Abadie and Gay investigate the impact of presumed consent and conclude that, “once other determinants of organ donation are accounted for, cadaveric donation rates are 25-30% higher on average in presumed consent countries” (Abadie and Gay 2006, 613). Becker and Elias argue that presumed consent would not completely solve the shortage of organs (2007, 16).

Cohen suggests that presumed consent laws confiscate property (body parts) upon death (1989, 14-15). There is no evidence that the wishes of families are ignored within countries that have presumed consent rules. The body is still considered the property of the deceased’s family. Many argue that presumed consent orients the status quo in a way that facilitates more donations (Tabarrok 2010a). Those arguing in favor of presumed consent have not addressed the implications of the policy on the presumption of liberty. Presumed consent is a contravention of the liberty principle, because Smithian natural liberty, it seems to me, would hold that upon death one’s things become the property of one’s heirs or next of kin, not the government. Indeed, the very labels “presumed consent” and “implicit consent” tell of taking people’s stuff without their consent. Imagine a law that confiscated half an individual’s income upon their death unless he or she filled out a set of paperwork. Regardless of the ease in filling out said paperwork, I argue that the law would be a liberty violation. That organ harvesting is a much better cause is beside the point, in terms of parsing the liberty principle.

Presumed consent’s incursion on liberty might seem minor, for one need only to opt out of organ donation. Becker and Elias, however, caution against the precedent set by presumed consent (2007, 16). If collectivists became more emboldened in their claims that the government owns all organs upon death, they

might start charging individuals for opting out of organ donation—although, to my knowledge, this has never occurred in presumed-consent countries.

Another tricky reform is called *mandated choice*. It would hold that individuals must decide whether to release their organs. By mandating choice, it would eliminate any default position. Supporters say it creates a binding contractual obligation between the individual and the state well before imminent death. The universal establishment of a decision during life would, it is hoped, eliminate the need to deal with family members during the emotional period just after death (Byrne and Thompson 2004, 23). Again, I am inclined to count this as a contravention of the liberty principle, although it depends somewhat on how the “mandate” is implemented and enforced. It would not seem to be a large incursion on liberty, and it would not seem to pose some of the indirect threats to liberty that presumed consent does.

Econlit and Beyond: Canvassing for Published Judgments by Economists

I began with multiple keyword searches on EconLit using *organ(s)*, *kidney(s)*, and *transplant* as keywords. (A screen capture of each search is available upon request.) I reviewed the abstracts of articles for relevance to organ policy. For articles found relevant, I documented the title, author(s), source, date searched, and the number coinciding with the EconLit search.

The first column of Table 1 contains the keyword used to search for articles; the second column contains the total number of articles found by EconLit; the third column is the number of articles deemed relevant to organ liberalization. If an article appears in multiple searches it will be captured in the spreadsheet for the first appearance only.

Table 1: Results of Keyword Search of EconLit

Keyword	Total Number	Relevant Articles (not in a preceding search)
Organs	143	66
Organ	155	41
Kidneys	21	6
Kidney	94	5
Transplant	138	0

I fanned out from Econlit by examining the works cited by a work from the primary Econlit searches. The same keyword process was performed using

Google to check for any non-traditional forms of media such as blogs, associated press publications, interviews, or webpages. A final method of canvassing was to search in books (notably textbooks)—I inquired with textbook authors⁴ and set up a webpage (link) where leads could be entered as comments. The search for judgments, then, started systematically with Econlit and fanned out in less systematic ways. The logic behind this study is that an economist who publishes a judgment makes him- or herself accountable to colleagues and the reading public for the judgments affirmed. Ultimately, I incorporated any published judgment by an economist, regardless of the media or of how I came by it. The media include popular publications and blog entries, for they too make the economist accountable to public scrutiny. My goal has been to make the survey as comprehensive as possible. This journal welcomes letters about sources I overlooked.

I verified an author's qualifications as "economist" using any of the following as sufficient: (1) a graduate degree in economics; (2) a teaching position in an economics department of higher education; (3) a professional position with the title "economist."⁵

72 Economists Categorized, Based on Their Judgments

All of the reform judgments that I found can be classified with the following seven categories:

1. Favoring presumed consent.
2. Favoring mandated choice.
3. Favoring the status quo while frowning on liberalization.
4. Favoring the status quo without addressing liberalization.
5. Favoring the status quo while entertaining mild liberalization.
6. Favoring some but not dramatic liberalization.
7. Favoring dramatic liberalization.

I also assess how clearly the economist makes his or her reform position: A) vaguely, B) fairly clearly, or C) clearly. The appended spreadsheet includes quot-

4. I used *The Economics Network* (link) as my source for economics textbooks. Under the "Introductory" classification, I recorded all textbooks titles and authors within the categories Economic Principles and Applied Economics for the USA. Under the "Intermediate" classification, I did the same for microeconomics (USA), Health Economics (General and selected specifics), and Law and Economics (General Texts). I located author's credentials and contact information in the same manner as my other searches.

5. I recorded the first met qualification, captured in the appended Excel file, and moved on. If I could not verify a person's credentials as an economist I omitted him/her.

ations that help to justify my assessment. My goal is to provide an accurate assessment of an author's views.

There are a number of authors who have written multiple pieces on the topic. Some offered different methods for increasing the supply of organs. But I did not find any author who made highly contradictory statements. Table 2 provides the last name of each economist sorted into a cell.

Table 2: Categorization of 72 Economists by Their Published Judgments on Organ Policy

Judgment	Is the Judgement conveyed vaguely or clearly?		
	<i>Vaguely</i>	<i>Fairly Clearly</i>	<i>Clearly</i>
Favors presumed consent		Abadie Breyer Gay Kliemt	
Favors mandated choice		Byrne Thaler Thompson	
Affirms the status quo while frowning on liberalization		Mocan Tekin Thorne Wellington	Howard Munshower Steiner Tietzel
Affirms the status quo without addressing liberalization	Dewar Nicoló	Roth	
Stays close to the status quo while entertaining mild liberalization	Álvarez	Gottheil Guell Sönmez Ünver	
Favors significant but not dramatic liberalization			Schnitzler
Favors dramatic liberalization		Levitt Posner	Adams III Altinanahatar Anderson Arnold Bailey Barnett Barnett II Barney Beard Becker Benjamin Blair Boudreaux Brams Brooks Caplan Cohen Cowen Boyes Crampton Crespi Crooker Epstein Elias Friedman Hamermesh Henderson Kaserman Kruse Mankiw Melvin Miller North O'Sullivan Reynolds Rottenberg Saba Saliba Schiller Schwindt Sheffrin Spurr Stonebraker Tabarrok Walker

Discussion of the Results

Rows 6 and 7 favor liberalization and account for 68 percent of the 72 economists included. As for opponents of liberalization, if we count those merely espousing presumed consent and mandated choice among them (not necessarily an appropriate thing to do), then we would say that those listed in rows 1, 2, and 3 account for 21 percent. So, by any figuring, the liberalizers greatly outnumber the opponents of liberalization, leaving 11 percent in rows 4 and 5 who seem to affirm the status quo. Given that the status quo usually carries something of presumption of rectitude, and given that presumed-consent and mandated-choice viewpoints do not necessarily imply opposition to other reforms in the direction of liberalization, it is fair to say that organ policy is one issue on which economists who are vocal take exception to status-quo restrictions in an exceptional way.⁶

Table 3: AEA members on allowing payments for organs

The U.S. should allow payments to organ donors and their families	Strongly disagree	Disagree	Neutral	Agree	Strongly Agree
	4.7%	10.9%	14.1%	45.3%	25.0%

Source: Whaples (2009)

How does the set of economists who publish judgments compare to the set of professional economists at large? While the two sets of economists have been found to differ on some issues, with the former more supportive of liberalization, on others they seem to be alike in their distributions of views.⁷ In 2007 a sample of members of the American Economic Association were surveyed by Robert Whaples (2009) on whether organ donors or their families should be allowed to receive payments. Responses from 128 respondents are summarized in Table 3. The allowing of payments—a significant liberalization—was supported by 70

6. The following authors discuss organ policy in their textbooks without expressing an opinion or providing much discussion of the issue: Sean Flynn, Campbell R. McConnell, Stanley L. Brue, David C. Colander, James D. Gwartney, Richard L. Stroup, Russell S. Sobel, David MacPherson, Neva Goodwin, Julie A. Nelson, Frank Ackerman, and Thomas Weisskopf.

7. Rough likeness between the two sets of economics seems to hold also for sports subsidies (Coates and Humphreys 2008), rent-control (Jenkins 2009) and most likely agricultural subsidies (Pasour 2004, Whaples 2006), but on the U.S. Postal Services’s monopoly (Geddes 2004; Whaples 2006) and the Food and Drug Administration (Klein 2008), as well as most likely occupational licensing (Svorny 2004) and rail-transit projects (Balaker and Kim 2006), the issue-expressive economists seem to be more liberal on the issue than are economists at large.

percent, and opposed by 16 percent. When it comes to organ policy, the two sets of economists seem to be rather alike.

Repugnance

Daniel Hamermesh (2009) says that, although he believes donors should be compensated, he does not approve of a person being able to purchase an organ in a market system. Alvin Roth (2007) discusses acts that are considered repugnant when involving money. He uses as examples life insurance, prostitution, addictive substances, and dwarf tossing (Roth 2007, 39). The acceptance of some repugnant activities has changed over time. Life insurance is considered a norm today, but it was considered unconscionable a hundred years ago (Roth 2007, 41). Levitt suggests that people are becoming increasingly open to markets in organs as the inadequacy of the current system becomes apparent. The number of patients dying while on transplant waiting lists continues to grow (Dubner and Levitt 2006).

Some argue that denying the right to sell an organ robs people of an opportunity to increase their income (Barnett, Saliba, and Walker 2001, 380-81). Others point out the inequity and hypocrisy of a system in which every party involved in organ transplantation is paid except the donor (Dubner and Levitt 2006). Others point out the contradiction in one's being able to give his body to science in exchange for a paid cremation, but parts of the body cannot be sold to save lives (Tabarrok 2009). Government-sanctioned "commodification" already exists in the form of surrogate mothers and the payment schedule used for soldiers in combat (Becker and Elias 2007, 21).

Some economists argue that denying individuals the right to engage in mutually beneficial exchange is unethical⁸ (Boudreaux 2006b). In my view, supporting the prohibition on certain consensual life-saving activities, apparently from certain political prejudices or the impulse to signal one's allegiance to certain political communities, is repugnant.

Appendix

Supporting quotations and economist credential check ([Link to Excel file](#))

8. The argument is not limited to economists. See notably James Stacey Taylor (2005) *Stakes and Kidneys: Why Markets in Human Body Parts Are Morally Imperative*.

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Editorial note: *The May 2010 issue included, “Economic Enlightenment in Relation to College-going, Ideology, and Other Variables: A Zogby Survey of Americans,” by Zeljka Buturovic and Daniel Klein ([link](#)). That article and a related Wall Street Journal piece engendered much critical commentary. We posted an invitation to submit criticism to EJW, and specifically invited eleven individuals who had published critical remarks on the Internet ([link to the post](#)). We received the four comments that follow. A reply from Buturovic and Klein will appear in the next issue. —Kevin D. Rollins*

The Unenlightening “Economic Enlightenment in Relation to College-going, Ideology and Other Variables”

Roderick Hill¹

ABSTRACT

Buturovic and Klein (2010) attempt to assess the level of “economic enlightenment” among an unrepresentative sample of Americans according to their views on eight statements. They compare the responses of the college-educated to those with less education. They also ask whether “economic enlightenment” is related to other things, like political ideology. Their findings are unconvincing, in part because of the nature of the survey questions.

One could ask factual questions to test economic knowledge, as opposed to “economic enlightenment”. Only two of the statements attempt this: “*A company with the largest market share is a monopoly*” and “*Overall, the standard of living is higher today than it was 30 years ago*”.

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Instead, most of Buturovic and Klein's statements require analysis. While this sets a higher standard, it raises greater difficulties in determining the "enlightened" answer. Applying economic analysis to a policy question often involves a choice among alternative theoretical models. Different models can give qualitatively different answers, a problem Buturovic and Klein don't mention.

The details of policy matter too. Buturovic and Klein (177) explain how to interpret the statements: "Unless a statement in a questionnaire explicitly makes it a matter of 100%, by using 'every', 'all', 'always', 'none', or 'never', it is natural to understand the statement as a by-and-large statement about overall consequences." Potential critics who might interpret a statement as applying to every conceivable situation are called "tendentious and churlish". Thus, "*Restrictions on housing development make housing less affordable*" does not apply to every conceivable restriction.

So how should one interpret "*Minimum wage laws raise unemployment*"? Does it refer to the consequences of actually-existing minimum wage laws? Given pervasive monopsony power (Manning 2003) and the unclear empirical findings now widely acknowledged in the principles texts, an answer of "somewhat disagree" is surely defensible, particularly if the respondent thought the statement referred to any significant change in unemployment.

But Buturovic and Klein write (178): "We think that the basic logic asked by the question is revealed by carrying it to a minimum wage of, say, \$20. Unemployment would go up a lot." Apparently then the statement is to be understood as "*No conceivable minimum wage law could raise unemployment*", contradicting their earlier claim about how these statements should be interpreted.

At least Buturovic and Klein explain their "enlightened response" to the minimum wage statement. To explain the statement: "*Free trade leads to unemployment*", they simply write: "Does free trade lead, overall, to greater unemployment? No, it does not." Apparently, evidence is not required.

It is trivially true that free trade leads to unemployment for workers displaced from import-competing industries. The point is to move some resources, including labour, from protected import-competing activities to higher-value uses. The resulting unemployment and the large costs borne by those displaced are what make trade liberalization controversial. Anyone aware of this issue, including free traders, should strongly agree that "free trade leads to unemployment".

Yet Buturovic and Klein claim the statement means something else entirely: "*Free trade leads, overall, to greater unemployment*". (If this is what they meant, why didn't they phrase the statement this way?) In other words, does employment expansion in export and non-tradeables industries at least offset employment declines in what are typically labour-intensive import-competing industries, leaving overall unemployment no greater than before trade liberalization, all else

equal? There is no theoretical reason why this should be the case. What empirical evidence is there? For example, Baldwin, Mutti, and Richardson (1980, 417), in a rare study of this question, find a net increase in unemployment when they simulate the effects on the United States of a multilateral tariff reduction.

How about “*Third world workers working for American companies overseas are being exploited*”? One’s response depends on an ethical judgement about what constitutes “exploitation”, telling us nothing about “economic enlightenment”. This normative statement is out of place with the other seven positive statements.

Given these problems with 3 of the 8 statements (themselves selected on unclear criteria from 16 statements in the poll), whatever results Buturovic and Klein report with their unrepresentative sample is of little interest.

Finally, none of the poll statements challenge conservative or libertarian sensibilities, as Buturovic and Klein themselves point out. What are the possible consequences, if we accept their ideas of what constitutes “economic enlightenment”?

Suppose many respondents know nothing about economic analysis, but have clear ideological views. Although none of the positive statements imply anything about the (un)desirability of any policy, suppose the ignorant don’t know enough to know that and think that policies that they favor must lead to good things, and policies they dislike lead to bad things.

The ignorant libertarian could agree that “*Rent control leads to housing shortages*”, believing that rent control is bad and thinking that housing shortages are bad too, so one bad thing leads to another. The ignorant progressive likes rent control and will disagree that rent control leads to housing shortages. (She hasn’t read Arnott and Igarashi (2000), who show that while rent controls increase search time for housing, this could improve efficiency in an imperfectly competitive market.) Seven of the eight statements are structured so that ignorant respondents answering in this way would produce just the results Buturovic and Klein claim: that conservatives and libertarians are more “economically enlightened” than liberals and progressives.

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Economic Enlightenment Poll Creates More Heat Than Light

E. D. Kain¹

ABSTRACT

Zeljka Buturovic and Daniel B. Klein's survey (2010) of "economic enlightenment" based on political ideology (and Klein's subsequent *Wall Street Journal* op-ed ([link](#))) creates more heat than light. This is unfortunate, especially since the economic assumptions Buturovic and Klein use to frame their poll are ones most economists and advocates of free markets should agree with. Nevertheless, too many of the questions remain far too open-ended and subjective to be of much practical use. Take, for example, this question on standard of living: "Overall, the standard of living is higher today than it was 30 years ago."

The "unenlightened" answer is "disagree". But this is a very broad question, and even from a market-friendly perspective, it could reasonably be answered with "it depends". After all, for a number of people, standards of living may have in fact plummeted, as a number of industries from steel to lumber to the newspaper business have all had the proverbial rug pulled out from beneath them.

Ultimately, Buturovic and Klein are polling not on right or wrong answers to economic questions, but on whether people agree or disagree with their own conclusions. Not surprisingly, we discover that conservatives and libertarians agree with Buturovic and Klein far more than progressives and liberals. It seems, if nothing else, an enormous waste of time and energy to show that proponents of markets and advocates of government intervention into markets disagree on basic economics.

I write all of this well aware that on many if not all of these questions I actually agree with the results favored by Buturovic and Klein. I'm sure that my

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own economic and political leanings would have ensured a near-perfect score had I been surveyed—not because I’m an economist or even terribly well-versed in complex economic questions, but because I share the same basic assumptions about free markets and government intervention into the economy as Buturovic and Klein.

Rent control does lead to housing shortages; having the largest market share is not enough of an indicator that a company is monopolistic; overall, despite increases in economic disparities, most people in this country have a much higher standard of living than they did 30 years ago. Nor do I believe global markets necessarily exploit foreign workers—though in some instances that has certainly happened.

Whether or not you agree or disagree with this survey’s economic leaping-off points, it’s important to note that this is bad polling done for strictly partisan reasons. The questions remain far too open-ended and the presentation of the results as “enlightened” vs. “unenlightened” misleadingly suggest that economic questions are all but answered. Progressives may be wrong on a lot of economic questions, but conservatives shouldn’t take too much comfort in the results of a clearly slanted poll either.

Understanding Econ 101 is one thing; understanding the much more complex economic systems of the real world is something else entirely. In that regard, Americans of all political stripes are woefully lacking.

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Identification Problems in Economic Enlightenment Surveys: A Comment on Buturovic and Klein

Daniel P. Kuehn¹

ABSTRACT

Buturovic and Klein (2010) argue that “economic enlightenment” is strongly related to political ideology, with conservatives and libertarians registering as more “enlightened” than moderates and liberals. The authors cite Hayek’s claim to be able to see into the “ethos” of the left, but one could easily furnish equally unelaborated insights from other economists about how conservatism “leads nowhere... satisfies no ideal... conforms to no intellectual standard.”² Such politically motivated salvos are galvanizing for the like-minded, but do not offer much in the way of a falsifiable hypothesis.

This weak theoretical backdrop for the author’s findings is even less satisfying because it is complemented with such problematic empirical analysis. The paper is burdened by a severe identification problem that should have precluded any firm conclusions from being drawn, not to mention later claims in the press that those findings were “unequivocal”. Essentially, the authors present one equation in three unknowns. Three factors can contribute to the “accuracy” of a response to the Zogby survey: a respondent’s (1.) “economic enlightenment”, (2.) likelihood of responding normatively to a positive question, and (3.) the compatibility of the positive “answer” and the respondent’s normative position on

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2. John Maynard Keynes, *Collected Writings*, Volume IX, pg. 296.

the issue. With no clear identification strategy, the authors assert that observed differences are attributable to the first factor.

Previous survey instruments designed to deal with precisely this identification problem were eschewed by the authors as too “dry”. This claim is rather subjective, but it is a legitimate reason for drafting a new survey. Nevertheless, more interesting content is useless if the survey is not designed to address basic identification problems. Before reading the paper, my assumption would have been that:

1. “Economic enlightenment” is relatively *unrelated* to political ideology,
2. Propensity to respond normatively to a positive question is relatively *unrelated* to political ideology, and
3. Normative answers to most economic questions by more conservative or libertarian respondents would be more “accurate” by virtue of the general compatibility between the normative positions of conservative and libertarian ideology and many positive findings in economics.

These assumptions of mine are corroborated (although not proven) by Buturovic and Klein’s results. They are further confirmed by the fact that the greatest observed difference in “accuracy” on questions occurs precisely where one would expect the greatest difference in normative positions. For example, liberals were more likely to respond “inaccurately” on questions that considered the needs of low income households than on questions with less ideological cogency. The differential between these more and less ideologically cogent questions is not especially obvious if you assume, with Buturovic and Klein, that real differences in “economic enlightenment” are driving the result. It would not make sense for someone to understand supply and demand in the market for medical services but not in the market for apartments, but it would make sense for them to have divergent normative positions on these two markets.

It is important to recognize that this concern is quite different from Buturovic and Klein’s second caveat. It is not simply an issue of whether some questions are so charged that they cause certain people to respond with pre-fabricated positions³; my point is that we would expect to see these differentials *even if each ideological group had an identical distribution of likelihoods of responding normatively*. The argument is not simply that some “enlightened” liberals may respond normatively to a question and be counted as a false negative. It is equally

3. Although if this was perceived as the heart of the issue by the authors, the reader justifiably wonders why such charged questions were even included in a survey ostensibly designed to identify real differences in “economic enlightenment”. Such charged question would seem to militate against confidence that the findings were truly reflecting “economic enlightenment”, so why were they chosen?

likely that unenlightened conservatives and libertarians may respond normatively as well, thus introducing a false positive.

My response so far has deliberately avoided any discussion of the questions themselves, primarily because my critique of the fundamental identification problem would still stand, even if the survey instrument was unimpeachable (I do not think it is). Without a way of isolating which of the three factors outlined above contributed to a respondent's answer, greater clarity in the questions themselves, greater fidelity to the literature in identifying specific answers as "enlightened", or more questions that challenge conservative and libertarian ideology would not, on their own, provide a reasonable way of arbitrating between Buturovic and Klein's interpretation of the results and my interpretation (which was also confirmed by their results). Nevertheless, the conspicuous lack of specificity in almost all the questions does make untangling the identification problem even more difficult than it might have been with a survey instrument more conscientious of these empirical wrinkles. Survey design cannot simply be an exercise in assuming that respondents will adopt the authors' preferred interpretation of the question. Survey designers need to *anticipate* divergent interpretations, or seek out better existing data. Instead of getting a satisfactory explanation for the vagueness of the survey instrument, critical readers are accused by the authors of being "tendentious and churlish".

In addition to Hayek's insights, Buturovic and Klein favorably cite Bastiat's critique of the left. They would have done better to adhere to Bastiat's maxim that we should consider both the "seen and the unseen". When respondents answer ostensibly positive questions about economics, the easiest approach is to assume a real difference in "enlightenment." But what remains "unseen" (and empirically unidentifiable with this data) is whether other differences between respondents are driving the results. Unless the authors can provide a sound way of arbitrating between alternative explanations (such as mine) and their interpretation, it's difficult to see what their analysis contributes. We are still left with divergent understandings of the relationship between political ideology and "economic enlightenment" which are both seemingly supported by the data. Addressing this identification problem is essential for any future work on this issue.

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Daniel Kuehn is a Research Associate in The Urban Institute's Center on Labor, Human Services, and Population, where he has worked for four years. His research focuses on statistical evaluations of federal employment programs, unemployment, unemployment insurance, youth employment, and the high-skill labor market. He has also contributed to work on youth policy and asset building in low income families. Daniel earned a master's degree in public policy from The George Washington University, specializing in labor market policy, and he has a bachelor's degree from the College of William and Mary in economics and sociology. In the fall of 2010 he will be applying to doctoral programs in economics, where he hopes to study labor market dynamics and the macroeconomics of labor. His email is dan.p.kuehn@gmail.com.

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A Rigged Test: A Critical Look at Buturovic and Klein’s Conception of “Economic Enlightenment”

David F. Ruccio¹

ABSTRACT

My first reaction to Zeljka Buturovic and Daniel B. Klein’s “Economic Enlightenment in Relation to College-going, Ideology, and Other Variables: A Zogby Survey of Americans” (2010) was, as I wrote on my blog (Ruccio 2010), that the test was rigged. After exchanging views on-line with Klein, and rereading the article, I stand behind my original reaction: the authors’ study is based on a methodology—a particular set of statements and responses, along with a conception of “economic enlightenment”—that was biased from the start.²

My principal critique of the study is that it is based on a set of statements for which the authors state the incorrect, which they interpret as “unenlightened,” responses. The statements themselves are the sort one will find in any mainstream, neoclassical textbook of introductory economics.³ Here are the eight statements (and, according to the authors, the “unenlightened” responses) used in the study:

1. Restrictions on housing development make housing less affordable.
 - Unenlightened: Disagree

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Acknowledgment: I want to thank Kevin Rollins and Daniel B. Klein for inviting me to submit a comment.

2. My exchange (Ruccio 2010) with Klein can be found here (accessed: 27 July 2010): [Link](#)

3. I teach from such a book on a regular basis: *Microeconomics*, by Paul A. Samuelson and William D. Nordhaus (2009). Just as I was composing this response, my desk copy of Samuelson and Nordhaus arrived for my Principles of Microeconomics course this fall.

2. Mandatory licensing of professional services increases the prices of those services.
 - Unenlightened: Disagree
3. Overall, the standard of living is higher today than it was 30 years ago.
 - Unenlightened: Disagree
4. Rent control leads to housing shortages.
 - Unenlightened: Disagree
5. A company with the largest market share is a monopoly.
 - Unenlightened: Agree
6. Third-world workers working for American companies overseas are being exploited.
 - Unenlightened: Agree
7. Free trade leads to unemployment.
 - Unenlightened: Agree
8. Minimum wage laws raise unemployment.
 - Unenlightened: Disagree

The problem of this and all such tests of correct and incorrect economic reasoning (such as the pre-testing and post-testing utilized by the mainstream economics education movement) is that the statements and correct responses are rigged. They represent the view of neoclassical economists, and respondents are considered unenlightened if they don't give the correct, neoclassical answers.

One can easily imagine a different set of policy-oriented questions and answers. For example, a different survey might include statements like "The reserve army of labor keeps wages low" or "Exploitation occurs even when workers are paid a living wage." Similarly, the statements chosen by Buturovic and Klein might have different "enlightened" or "unenlightened" responses. Take number 6: from a Marxian perspective, Third World workers are being exploited, as are First World workers—as are, for that matter, all productive workers in capitalist enterprises. Therefore, a specifically "Marxian enlightened" answer would be "agree."

But that's not my main point, since I don't want to defend either what the authors of the study consider to be the correct answers, or the "incorrect" answers given by many of the respondents. My point, rather, is that there are different economic representations—among academic economists and everyday economists, inside academic economics as well as academic disciplines other than economics and outside the academy. They literally use different economic discourses, through which they view such issues as rent control and minimum wages, and of course come up with different answers.

This is an area I explore in *Economic Representations* (Ruccio 2008) as well as (with my coauthor, Jack Amariglio) in “Academic and Everyday Economic Knowledges” (chapter 7 of Ruccio and Amariglio 2003). Our general point is that a particular group of academic economists—call them neoclassical or neoliberal or classical liberal—do not hold a monopoly on the production and dissemination of economic knowledges. Economic issues and themes are thought about and discussed by many people other than academic neoclassical economists—within the discipline of economics, elsewhere in the academy, and throughout society. The groups include, inside the discipline of economics, economists who utilize a wide variety of non-neoclassical perspectives, from Keynesian and institutionalist to feminist and Marxian theories. Inside the academy, but outside the discipline of economics, scholars “do economics” in disciplines as diverse as cultural studies, literary theory, anthropology, geography, and political science. And outside the academy, economics is practiced by a wide variety of nonacademic writers and activists, including people who work in think tanks, human rights organizations, trade unions, and so on. All of these economists, whether or not they actually have a doctorate (or, for that matter, any academic training) in economics, produce knowledges about and participate in debates concerning economic concepts, issues, and policies.

These academic and everyday economists often—but certainly not always—arrive at conclusions that differ from those of neoclassical economists. To dismiss those conclusions as “unenlightened,” as do Buturovic and Klein, is to invoke a troubling position of scientific authority and of the disciplinary self. It means that one set of responses—those of the neoclassical academic economist—are considered to be the enlightened, correct ones and everything else is, well, incorrect and unenlightened.

In fact, the authors immediately move on to analyze correlations with various identifiers (such as level of education and ideology) without making any attempt to justify their designation of responses as enlightened or unenlightened. They presume, from within their own economic theory, what the correct and incorrect responses are, thereby invoking a singular notion of economic science and a privileged notion of the disciplinary self. They (and those who agree with them) are declared enlightened, and anyone who gives a different response is not.

An interesting question to ask of such surveys is, what are the discursive protocols that lead to such different responses to particular statements about economic policies? Attempting to answer that question would mean investigating not only the different entry points and methodologies of the theories people use to respond to such statements but also the different interests of the people who contribute their responses. In other words, instead of presuming that economic science is singular, and that economic scientists have no interests (aside from

objectivity), it is possible to start from the position that economic theories are always plural—there are different, incommensurable economic theories that people use to make sense of the economic dimensions of their lives and of the society in which they live—and that interests matter when it comes to responding to statements—in the sense that responses depend on a whole range of social identities, including whether or not one represents and performs the hegemonic theory within the discipline of economics.

As it turns out, Buturovic and Klein betray their interests in the final section of the paper. First, they call for more economics education, seemingly guided by the idea that the problem consists of economic illiteracy, and that “more emphasis on economics instruction” would give people more economic enlightenment. Their outlook appears to be that people are either a blank slate (on which correct economic knowledge can be written) or are misguided by an “ersatz” economics (which needs to be destroyed, after which correct economic knowledge can be instilled). Second, and even more important, they express their concern with the “kind of economics instruction” students receive, and advise parents and students to avoid economics courses that are “hostile to classical liberal thinking.”

This is the most troubling dimension, which calls into question the entire study. Buturovic and Klein already know what an enlightened economics is—that which instills “classical liberal thinking”—and everything else contributes to unenlightenment. Economics education, for them, is not for teaching critical thinking about economic issues, exposing students to a variety of theories, or examining the changing vicissitudes of theories and methods across the history of economic thought. No, it’s to instill what they consider to be the correct ideas, the “enlightened” responses to contemporary policy questions.

This is indoctrination, which in a capitalist democracy is not a particularly enlightened position.

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