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Econ Journal Watch

Foreword

All economists are familiar with the notion that actual social processes leave much scope for improvement. Scholars have developed many theories about social mal-performance.¹ Every failure theory applies to some extent to the intellectual practices called Economics. We believe Economics can be improved significantly.

Economics is an agglomeration of cultures, and culture is inherently self-referential. The mechanisms of positive feedback, network externalities, and path dependence apply in force. Cultures often steer themselves down paths of error.

Throughout history, criticism has checked culture, and new technologies have brought new forms of criticism. The Internet makes it possible for us to develop pointed criticism, draw authors into dialogue, and reach readers worldwide. New media enable criticism to move from generalities—so

¹ Some of the conditions and mechanisms that make for social problems include: natural monopoly, free riding, concentrated interest/diffused costs, rational ignorance or the not-worth-knowing-better problem, rent-seeking, corruption, collusion, cronyism, cynicism, politicization, agenda setting and manipulation, asymmetric knowledge, deference to authority, groupthink, herd behavior and “truths-are-us,” bandwagon effects, informational cascades, vetting and expulsion, self-sorting, controlled opposition, status quo bias, availability bias, intentionality bias, preference falsification, belief plasticity and cognitive dissonance, self-exaltation, self-deception, indoctrination, taboo and greed.

FOREWORD

often thought to be toothless and unsatisfying—to specifics. *Econ Journal Watch* is a determined effort to use these capabilities to develop an encompassing critique of Economics and consider some of the paths available for improvement.

Established society has often received incisive criticism as impertinent. For all its progressiveness and freethinking, academic Economics is structured as echelons by field, stratified by publishing in the top journals and getting hired at the top departments. The culture remains that of genteel urban society. Occasionally one of its members pursues pointed criticism in the scholarly journals, but the process is plodding and at the top journals the circle remains overly insular. Without the tension of independent criticism, an intellectual culture tends to become clubby and scholastic.

The Internet promises to deliver criticism that is lively, incisive, and scholarly. New media used by earnest scholars can only better the cultures that concern us all.

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Overlooking the Obvious in Africa

JANE S. SHAW*

A COMMENT ON: COLLIER, PAUL, AND JAN WILLEM GUNNING. 1999. WHY HAS AFRICA GROWN SLOWLY? *JOURNAL OF ECONOMIC PERSPECTIVES* 12(3): 3-22.

[ABSTRACT, KEYWORDS, JEL CODES](#)

READING THE 1999 *JOURNAL OF ECONOMIC PERSPECTIVES* ARTICLE by Paul Collier and Jan Willem Gunning, “Why Has Africa Grown Slowly?” one gets the impression that mainstream development economists are floundering for explanations of poor growth in Africa.

Collier and Gunning survey a laundry list of explanations for poor growth, most of which appear to be taken seriously in the profession today. They also offer some extremely tentative conclusions for why Africa’s economic conditions are so poor. I find the paper frustrating for its failure to embrace vigorously the explanation that outshines the rest. The authors do not explicitly reject the obvious explanation; they even mildly embrace it. But, on the whole, it is downplayed and neglected.

Paul Collier is director of the Development Research Group at the World Bank and Jan Willem Gunning is director of the Center for the Study of African Economies at Oxford University, and surely they are highly learned in African economic issues. I admit that I am no authority on Africa, yet I cannot help but feel that Collier and Gunning suffer from some kind of intellectual blinders, that the roots of Africa’s economic problems are not all that difficult to uncover. Since Adam Smith,

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economists have developed theories about how prosperity depends on liberty. These ideas are borne out by new indices of economic freedom. Even a thorough look at sub-Saharan Africa's one growth success story, Botswana, would have offered more insight than Collier and Gunning provide.

Collier and Gunning start out by saying that in the 1960s Africa's economies had a promising future, but beginning in the 1970s, "both political and economic matters in Africa deteriorated" (3). The goal of their article is to explain why. Indeed, the title, "Why Has Africa Grown Slowly?" is an odd way to describe the dire situation in sub-Saharan Africa, where per capita GDP has fallen by 1 percent per year since 1980. The title is technically right only if the 1960-73 period of rapid growth is averaged in. Recent indicators are nothing less than grim. Between 1990 and 1998 the number of people in sub-Saharan Africa living in poverty increased from 242 million to 291 million. During that period life expectancy in 17 sub-Saharan countries actually decreased, even while increasing by two years in the rest of the developing world (Wolfensohn 2001, xii).

Collier and Gunning examine a 2x2 scheme of explanations for the continuing misery in Africa: "Destiny" vs. "Policy" and "External" vs. "Domestic." They reveal some frustration with the failure of conventional econometric models to shed much light on Africa's stagnation.

Sorting out the policy effects from the destiny effects is a difficult econometric problem. In the ordinary least squares regressions common in the analysis of African growth, the dependent variable is typically the average growth rate over a long period, and a variety of policy and destiny variables enter as the explanatory variables. Depending upon the specification, either policy or destiny can appear important (16).

Collier and Gunning explore the four combinations of the scheme. The first is "Domestic-Destiny"—that is, geographic and demographic characteristics that "may predispose [sub-Saharan Africa] to slow growth" (7). These include fast population growth, low population density, poor soil quality, and the prevalence of the tropics with its many diseases. Although fast population growth and low population density are not contradictory, their juxtaposition shows the "everything but the kitchen sink" nature of Collier and Gunning's survey. It is admirable to pursue a scheme exhaustively, but we immediately start to doubt that the 2x2 scheme itself

reflects good scientific judgment.

One wonders especially because some of the “Destiny” factors are quite speculative. Collier and Gunning refer (although not by name) to what is sometimes known as “the curse of natural resources.” This is the correlation between high natural resource endowments per capita and slow growth, a correlation that has shown up in a number of studies (see, for example, Sachs and Warner 2001), and spawned tortuous analysis. Collier and Gunning propose a chain of impacts to explain the connection. They offer this reasoning: Valuable natural resources may lead to high levels of exported natural resources, which “may lead to an appreciation of the exchange rate, which in turn makes manufacturing less competitive”(9), and this could be a problem because manufacturing may have positive externalities that natural resource exploitation does not—such as more widely applicable learning. Collier and Gunning also propose the idea that “dependence on natural resources strongly increases the risk of civil war”(9). (The source for this claim is Collier and Hoeffler 1998.) But this explanation seems lame. The Collier-Gunning paper does not explore the causes of Africa’s civil dissension or its impact on growth. For instance, it does not discuss conflicts stemming from having different tribes within artificial national boundaries and under artificial structures of centralized power.

The second explanatory category is “Domestic-Policy”—domestic factors caused by policy choices rather than natural endowments. Here the authors present a variety of examples showing expanded government involvement in the economy, and this is the best part of the paper. For example, Uganda’s government at one time required that coffee be transported by rail (when the requirement ended, hauling rates dropped by half). Kenya’s government prohibited manufacturing firms from starting up unless they could obtain “no objection” letters from existing firms. The examples are a rich collection—but Collier and Gunning do not draw many conclusions from them. Their strongest statement is that an expansion of the public sector resulted in the “paradox of poor public services despite relatively high public expenditure”(10). This is an inadequate description of the economic damage caused by government contraventions of what Adam Smith called natural liberty.

Indeed, the fact that Collier and Gunning speak of a “paradox of poor public services despite relatively high public expenditure” indicates that they are not familiar with the public choice literature, which explains why high levels of public expenditures are often, and predictably, linked to poor public service, because of bureaucratic and political incentives

(Tullock 1971, Stroup 2000). But public choice authors are not to be found in the Collier and Gunning article.

Collier and Gunning's third category, "External-Destiny," covers factors outside the control of Africans. For example, Africa has relatively few navigable rivers and transportation costs are high. Also, Africa's exports are "concentrated in a narrow range of commodities, with volatile prices that have declined since the 1960s" (13). Such factors matter, of course, but the underdevelopment of transportation infrastructure and the narrowness of Africa's export commodities should themselves call out for explanation, rather than be regarded as a matter of destiny. One gets the feeling that the investigation is correlating illness with symptoms, rather than causes.

Finally, there is "External-Policy," which refers to deliberate choices on the part of African governments that affect African countries' relationships with other nations. These factors include African government decisions leading to "higher trade barriers and more misaligned exchange rates than other regions" as well as "quantitative restrictions" on imports (14). Not surprisingly, like many domestic policies, these decisions have hurt Africa's economies and encouraged corruption, and the authors say so.

Having laid out the categories of explanation, and emphasizing that the distinctions are oversimplified, Collier and Gunning offer an assessment. They weigh in on the side of policy and domestic factors: "we believe that domestic policies largely unrelated to trade may now be the main obstacles to growth in much of Africa" (18). The suggestion is promising, but it is more of an aside than a conclusion.

Although Collier and Gunning recognize the importance of policies, they don't go very far in identifying what makes a policy a bad one. What is largely missing is an analysis of the institutional conditions—the laws, traditions, customs, and habits—of countries and their populations. The characteristics summed up by the term "freedom" or "natural liberty" figured prominently in the explanations that Adam Smith gave for the wealth of nations. Many postwar economists have revived Smith's theory of growth. Collier and Gunning might be half-heartedly in agreement with this position, but this view gets lost amid the forty or so possible factors they offer.

A particularly useful source of information about countries' institutions, first published in 1996 (and thus perhaps too recent for Collier and Gunning to have utilized), is the Economic Freedom of the World Index (Gwartney and Lawson 2003). Developed over nearly a decade, the index measures the relative role of markets vs. government control in a

country. The 20 percent of countries with the highest economic freedom have average incomes of almost \$20,000 per year while the 20 percent with the lowest economic freedom average incomes of only about \$2,000. Undeniably, as economic freedom increases, so does prosperity. Adam Smith was right. Collier and Gunning note the low level of *political* freedom in sub-Saharan Africa (citing the Gastil scale measuring political rights) but economic freedom is different and is directly relevant to economic growth.

Collier and Gunning mention the “main exception to African economic collapse” (17). Between 1965 and 1997, Botswana grew at a 7.7 percent annual growth rate, which they say is the fastest in the world. Yet they mention it almost as a curiosity and do not examine it further to see what might be different about it. In the Economic Freedom of the World index based on 1995 data (Gwartney and Lawson 1997), Botswana (# 48) was one of only three sub-Saharan Africa nations in the top 50 countries ranked by level of economic freedom—the other two were Mauritius (#5) and South Africa (#50). If Collier and Gunning had looked at the institutional quality of African countries, they would have seen a red flag alerting them to the absence of economic freedom. They might have constructively explored why Africa does not have much economic freedom.

One cause of economic backwardness in Africa could be the excessive reliance of many African governments on foreign aid. Collier and Gunning discuss aid as an “external-destiny” factor and note that Africa “has attracted much more aid per capita than other regions” (12). They acknowledge that there has been a “long debate as to whether aid has been detrimental or beneficial for the growth process.” Some critics have damned foreign aid harshly, but Collier and Gunning’s attitude toward the criticism is dismissive. “Early critics claimed that aid reduced the incentive for good governance (for example, Bauer, 1982),” they write (12). Collier and Gunning contend that econometric work has found no effect of aid on policy. Their explanation is that “to the extent that aid encourages or discourages policy changes, the two effects apparently offset each other”(13). Furthermore, they argue that foreign aid increases economic growth when government policies are “good,” but not when policies are “poor” (13). In support of this position, they cite econometric evidence (two World Bank working papers, one of which was coauthored by Collier). Thus, they appear to dismiss the argument that aid might perpetuate a government’s poor policies.

Yet some of Collier’s World Bank colleagues acknowledge that when policies are poor, aid has hampered reform. The World Bank book *Aid and Reform in Africa* states in its “Overview” that “aid in the poor policy period”

may have “perverse incentive effects. That is, finance may deter reform, and the absence of finance may encourage reform since it removes one easy way out of macroeconomic problems for the government. This argument is particularly applicable to large-scale budget or balance of payments support, which in a bad policy environment may reduce the urgency of reform”(Devarajan et al. 2001, 27).

The studies that Collier and Gunning cite to support the view that foreign aid can increase growth have recently been challenged in a working paper by William Easterly and two colleagues. This group used the same methodology as one of the studies cited by Collier and Gunning (Burnside and Dollar 2000), but measured the effects over a longer period and included some data that were not available for the original paper. Easterly, Levine, and Roodman (2003, 6) report that adding these data “raises new doubts about the effectiveness of aid and suggests that economists and policymakers should be less sanguine about concluding that foreign aid will boost growth in countries with good policies.” These findings appeared subsequent to Collier and Gunning’s paper, but they speak to the questionable judgment of dismissing the long-standing criticism of foreign aid.

Given Collier and Gunning’s frustration with the limitations of econometrics, it is regrettable that they pay so little attention to Peter Bauer. Collier and Gunning cite a 1982 article by Bauer in *Encounter* magazine. They have the title right, but they have the date wrong (they cite November instead of March) and the authorship wrong (it is by Bauer and Basil Yamey, not by Bauer alone), and, in any case, this article is just two pages long. Bauer wrote twelve books, at least eight of them on development.

In his book *Reality and Rhetoric*, Bauer argued that the only beneficial impact of foreign aid is the avoided cost (that is, interest) of private capital. Against this he placed the harmful effects of aid, starting with its tendency to “increase the resources and power of recipient governments compared with the rest of the society” (Bauer 1984, 46). Thus, Bauer’s criticism of foreign aid comports nicely with Smith’s theory of growth, which holds that markets, institutions, and production levels develop spontaneously in a regime of natural liberty and secure private property.

Foreign aid, wrote Bauer, often “helps or even enables governments to pursue policies which patently retard growth and exacerbate poverty” (Bauer 1984, 46). Bauer cited as examples of such policies persecution of minorities (including traders and others who are productive), restrictions on trade, and more. He also observed that foreign aid encourages wasteful, highly politicized expenditures on industrial plants that would not be built

through private capital. Although the total amount of aid is small compared with a country's GDP, the amounts "are often a significant part of government revenues and of foreign exchange earnings" (Bauer 1984, 47). Bauer did not do an econometric study of the influence of aid on government policy, but all development economists should nonetheless take seriously the grave hazards of politicizing society and governmentalizing resources—hazards that would seem to be inherent in practices that channel funds to national governments.

In the period since World War II, development economics has picked up and dropped a variety of prescriptions for growth in less-developed countries, such as protection of infant industries, Paul Rosenstein-Rodan's emphasis on social overhead capital, and the theory of "balanced growth." But these prescriptions have not stood up well over time (Shaw 1999). Sadly, the information gathered in the Collier-Gunning article also offers little guidance for addressing Africa's poor economic conditions. Recognizing that recent econometric work has not offered much insight into Africa's economic problems, Collier and Gunning make their own judgments, which include a recognition of the devastating role of government policies. But they bury these assessments. Indeed, the article's concluding paragraphs avoid answering the title question, "Why Has Africa Grown Slowly?" Instead, the conclusion is titled "Will Africa Grow?" Their peroration drifts from one idea to another, even to the point of suggesting that one reason for low foreign investment may be investors' erroneous perceptions about African countries. The last two paragraphs of the article follow:

Our own interpretation lies between these extremes. We suggest that while the binding constraints upon Africa's growth may have been externally-oriented policies in the past, those policies have now been softened. Today, the chief problem is those policies which are ostensibly domestically-oriented, notably poor delivery of public services. These problems are much more difficult to correct than exchange rate and trade policies, and so the policy reform effort needs to be intensified. However, even widespread policy reforms in this area might not be sufficient to induce a recovery in private investment, since recent economic reforms are never fully credible. Investment rating services list Africa as the riskiest region in the world. Indeed, there is some evidence that Africa

suffers from being perceived by investors as a “bad neighborhood.” Analysis of the global risk ratings shows that while they are largely explicable in terms of economic fundamentals, Africa as a whole is rated as significantly more risky than is warranted by these fundamentals (Haque et al., 1999). Similarly, private investment appears to be significantly lower in Africa than is explicable in terms of economic fundamentals (Jaspersen et al., 1999). “Africa” thus seems to be treated as a meaningful category by investors.

The perception of high risk for investing in Africa may partly be corrected by the passage of time, but reforming African governments can also take certain steps to commit themselves to defend economic reforms. Internationally, governments may increasingly make use of rules within the World Trade Organization, and shift their economic relations with the European Union from unreciprocated trade preferences to a wider range of reciprocated commitments. Domestically, there is a trend to freedom of the press, and the creation of independent centers of authority in central banks and revenue authorities, all of which should generally help to reinforce a climate of openness and democracy, which is likely to be supportive of economic reform (Collier and Gunning 1999, 20).

There’s not a lot of guidance here. Development economists need to wake up and smell the coffee. The best answers to the question “Why Has Africa Grown Slowly?” are still those of Adam Smith and his latter-day intellectual progeny like Peter Bauer. An understanding built on that foundation might actually help Africa rediscover the path to growth.

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How to Get Real About Organs

ALEXANDER TABARROK*

A COMMENT ON: BYRNE, M. M., AND P. THOMPSON. 2001. A POSITIVE ANALYSIS OF FINANCIAL INCENTIVES FOR CADAVERIC ORGAN DONATION. *JOURNAL OF HEALTH ECONOMICS* 20: 69-83.

[ABSTRACT, KEYWORDS, JEL CODES](#)

MORE THAN 80,000 AMERICANS ARE WAITING FOR ORGAN DONATIONS. Due to lack of available organs at least 6000 of them will die this year.¹ These deaths could be prevented if more cadaveric organs were conferred. Efforts to encourage organ donation have included a prominent advertising campaign featuring Michael Jordan, support for workplace programs, new driver's ed programs, and organ registries.² Economists and others have contributed to the cause by pointing to the price control on human organs. Given the prohibition of financial compensation for organ donation, the maximum price allowed by law is zero. At a price of zero, quantity demanded exceeds quantity supplied. Many have argued that by lifting the control, or by finding other ways to increase the incentive to sign

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¹ As of March 7, 2003 there were 80,602 people on the waiting list for a transplant. In 2002, when there were a similar number on the waiting list, 6,386 died while waiting for a transplant and another 1,866 became too sick for an operation to be possible. The most recent data can be obtained from the United Network for Organ Sharing, online at <http://www.unos.org/data/>.

² Tommy Thompson, the secretary of the U.S. Department of Health and Human Services, has launched a series of initiatives to increase organ donation. For more details see: <http://www.hhs.gov/news/press/2001pres/20010417.html>.

an organ donor card, more organs would become available (Cohen 1989, Epstein 1983, Becker 1997, Barnett, Blair, and Kaserman 1996, Tabarrok 2002).

The typical counter-argument to the market approach maintains that, for one reason or another, it is wrong to “traffic” in human organs. The debate has foundered on whether it is more wrong to traffic in human organs or to let people die from the shortage.³ Byrne and Thompson (2001) take the debate in a different direction by suggesting that financial incentives to sign a donor card might actually *decrease* the supply of organs. Byrne and Thompson point out that, even if a person has signed his organ donor card, i.e., registered as a donor, it is his next of kin who decides whether to permit the harvesting of organs. (As I describe later, this is not accurate in every state.) They explain that current practice gives the relatives the authority to decided ‘yes’ or ‘no’ to donation even though the letter of the law would make the signer’s signing an authoritative ‘yes’ (Byrne and Thompson 2001: 72-73). In practice then, signing an organ donor card simply signals to relatives the donor’s intent.

The central idea in Byrne and Thompson's model is that a donor has two kinds of preferences: ‘authentic’ or ‘true’ preferences and preferences for the incentive good (such as cash payments). According to their model, when the donor's relatives make their donation decision they try to estimate the donor’s ‘true’ preference for donation and factor out whatever role financial or other incentives played in the decision to donate. Under the right assumptions, money-for-signing or money-for-donation will lead to fewer actualized donations. Imagine that very few people currently sign their organ donor card. Bill Gates then offers \$500 to anyone who signs and, as a result, virtually everyone signs. When asked to follow through on the donation, the relatives reason that without the incentive the donor would not have signed, and they refuse. Even the relatives of people who would have signed without any incentive might refuse because they aren’t sure

³ Most of the plans offered by economists use pecuniary incentives. Tabarrok (2002) argues that a “no-give no-take plan” (signers of organ donor cards would receive priority in receiving organ) establishes appropriate incentives without commodifying the body. See also Schwindt and Vining (1998). Recently, Lifesharers.com has begun to implement a “club” approach similar to that suggested by Tabarrok. Members of Lifesharers agree that in the event of their death any usable organs are conferred first to other Lifesharers members. As more members join the Lifesharers club the benefits of being a member increase. Since being a member means being a potential organ donor, Lifesharers increases the incentive to be an organ donor.

whether the deceased donated out of the ‘good of his heart’ or because his decision was ‘distorted’ by the incentive.

Unfortunately, Byrne and Thompson spend an inordinate amount of time deducing conclusions from their premises and very little time arguing for the relevance or plausibility of those premises.⁴ They do not provide empirical evidence for their assumptions or develop an intuitive argument. On examination, their key premises appear doubtful.

Byrne and Thompson's assumptions are questionable in respect to both donors and donor relatives. Absent extraordinary circumstances, it is unlikely that the relatives would readily override the actual evidence of donor intent (a signed organ donor card) in favor of a theory about ‘true’ intentions. Donor intent is currently one of the best predictors of family choice. Siminoff et al. (2001) find that when the deceased's family knew that the deceased had a donor card 89.3% agreed to donate.⁵ Although this does not rule out lower donation rates when financial incentives are put into play, it does suggest that relatives take donor intent seriously. Moreover, it is not true that family consent is always required for organ donation. As of January 2003, twenty-seven states had passed “donor-designation”/“first person consent” laws that state clearly that the intent of the donor is legally binding, even absent family agreement.⁶ Naturally, no organ procurement organization wants to override family wishes during a difficult time, and, as a result, in some states family wishes can still override donor intent regardless of the law. In recent years, however, a number of organ

⁴ Milton Friedman (1953) argued that what matters is not premises but predictions. Thus, a theory with unrealistic, even absurd, premises is okay so long as it leads to good predictions. Byrne and Thompson, however, do not attempt to test their model. Indeed, the central question here is precisely whether we *should* test a system of financial incentives for organ donation. To the extent that Byrne and Thompson's model convinces readers that financial compensation is a bad idea it precludes the test that is supposed to justify the model. Realism and appropriateness of assumptions are valid criteria for judging an argument whenever testing is expensive or otherwise unlikely to occur and is especially vital whenever testing is the very question at debate.

⁵ This compares with 65% who agreed to donate if they knew that the deceased did not have a card and 44.4% who agreed to donate if they knew that the deceased had not signed an organ donor card.

⁶ States with first-person consent laws are Alaska, Arkansas, Arizona, California, Colorado, Delaware, Idaho, Indiana, Iowa, Louisiana, Maryland, Minnesota, Missouri, Montana, Nevada, New Jersey, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, South Dakota, Tennessee, Virginia, Washington, Wisconsin, and West Virginia. Florida is currently considering such a bill. The list is from a UNOS provided table “Donor Designation (First Person Consent) Status by State as of January 2003.” A number of these laws passed after publication of Byrne and Thompson.

procurement organizations have become more aggressive in seeing that the wishes of the donor are followed. In Maryland, for example, the approach has shifted from asking the family about donation to informing the family that the person will be an organ donor. As an empirical matter, therefore, the assumptions of the Byrne and Thompson model are likely to be false both as they regard the preferences of donor-families and their rights. Thus, the signaling problem that Byrne and Thompson identify is unlikely to be significant. In any case, Byrne and Thompson do not offer any evidence, direct or indirect, that financial compensation would reduce family agreement to donate.

With regard to donor preferences, Byrne and Thompson's model is unrealistic because it ignores meta-preferences for honesty and personal autonomy, and, most importantly, it misunderstands what is being exchanged when someone signs an organ donor card in response to an incentive. The state of Georgia, for example, gives an \$8 discount on driver's licenses for those who sign their organ donor cards. Byrne and Thompson require that many of the people who sign their donor cards in response to such an incentive actually think themselves fortunate when they realize that their families will refuse harvesting should that contingency arise. But most people have a preference for following through on their agreements. Few people like to think of themselves as the sort of person who makes a bargain only to go back on that bargain at the first opportunity (especially when someone's life depends on it). Even fewer people want their loved ones to think of themselves this way. By overriding the choices of their loved ones, even (perhaps especially) if done on paternalistic grounds, donor families could well be insulting their loved one's perceptions of personal autonomy.

Byrne and Thompson model the decision to sign an organ donor card as if the exchange were a conventional contingent contract—the donor gets a payment today in return for giving up something of value in the future. Yet, the contingency is the donor's death, and what the donor gives up at that time isn't worth anything to the donor. Certainly, the donor could have strong preferences about what happens to his organs in the future in which he is dead. But in modern, Western society most people don't worry about needing their organs in the after-life—all the major religions support organ donation. It is more plausible that what the incentive buys is the time

and attention that it takes to sign one's organ donor card *today*—and the unpleasantness of pondering one's own death.⁸

If the costs of signing the donor card are incurred when the card is signed, and not when the organs are harvested, then the relatives cannot make the donor better-off (even ex-ante) by refusing donation. Indeed, the reverse would seem to be the case. Once the organ donor card is signed the costs are sunk, but the benefits of giving life to someone else remain in the future. In short, Byrne and Thompson assume that giving up your organs when you are dead is a cost of signing your organ donor card—but it seems more plausible to view this as the benefit of having signed your donor card. Indeed, this is precisely why people sign their organ donor cards today.

To appreciate the potential benefits of financial inducements to signing, consider the preferences of people today who have not signed their donor card. Given the costs of signing, and low expected benefits, many don't bother to sign. With greater reflection, however, surely a number of these people would experience regret at not having taken the opportunity to save someone else's life. Had these people known of and already faced their death (thus having incurred the costs of signing) many of them would have had their utility *increased* by the knowledge that their gift would give life to another person. If this is a plausible description of many people's preferences then it is clear that Byrne and Thompson have modeled donor decisions incorrectly. Moreover, their assumptions have artificially eliminated the scope for trading partners to use cash incentives to prompt people to reflect on the matter. In Byrne and Thompson's model, people's initial attitudes are their eternal attitudes. But an individual's attitude about such a decision clearly depends on social influences, the promises he makes, and the depth of his reflection about the matter—factors all of which can be affected by the financial compensation that induces initial signing.

In some settings the Byrne and Thompson assumptions could be valid. Consider a country in which there are strong, perhaps religion-based, fears concerning organ donation. Suppose foreign firms enter the market and offer a financial incentive to sign an organ donor card. Despite misgivings, some of the destitute sign donor cards. It is possible that, when asked to permit harvesting, many families refuse. It is also possible that such refusal might decrease the total number of donations—although this is less likely, as by hypothesis there would have been few donations irrespective of financial incentives.

⁸ On the view of various religions see: Cooper-Hammon and Taylor (2000).

In the United States and other Western countries, however, the plausibility of the above scenario is much decreased. A 1993 Gallup survey representative of the U.S. adult population found that most people in the United States [78%] do not think it important that they be literally buried or burned with all their organs (Gallup 1993).⁹ Support for organ donation was overwhelming, with 85% in support and only 6% opposed. Despite large measures of support, it is estimated that only 14% to 28% of adults have signed organ donor cards (Gallup 1993, Manninen and Evans 1985). What prevents organ donation in the United States is probably indifference and institutional incompetence. When those people who previously indicated that they were unlikely to donate an organ were asked why, 47% said “No reason/don't know/haven't given much thought.” This was by far the most common response—the next highest response, at 13%, was “medical reasons” (i.e., they thought the organs would not be wanted).¹⁰

What the Gallup Poll results indicate is that a large majority of people support organ donation, but because of indifference, or perhaps a superficial reluctance, they do not sign their organ donor cards. As a matter of mathematics, it follows that any large increase in organ donor card signatories brought about by financial incentives must come predominantly from organ donation *supporters*. Thus, there is little possibility of the perverse response showcased in Byrne and Thompson's article. Financial or other incentives are ideally suited to enable interested parties to overcome the sort of indifference or reluctance that precludes most supporters of organ donation from signing their organ donor cards.

⁹ The poll can be accessed online at:

http://www.transweb.org/reference/articles/gallup_survey/gallup_index.html.

¹⁰ The possible responses to this question and answers were: Medical reasons (13%), Too old (10%), Don't want body cut up/want to be buried as whole person (9%), Don't feel right about it (6%), Against religion (5%), Other (10%), No reason/don't know/haven't given much thought (47%) (Gallup 1993).

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Response to Tabarrok

MARGARET M. BYRNE* AND PETER THOMPSON**

“Think you’re an organ donor? Not if your family doesn’t know.”
—Advertising billboard 2003.

EVERY GRADUATE OF PRINCIPLES OF ECONOMICS LEARNS THAT supply curves slope upwards. But most of them quickly forget the assumptions that make this so. Some may recall that a perfectly competitive market of identical firms yields a horizontal long-run supply curve. Others may recall that monopolists don’t have one at all. Relatively few will recall the obvious, but usually implicit, assumption that payment for goods and services supplied are actually made to those who create the supply. In Byrne and Thompson (2001), we made this trivial idea the cornerstone of an analysis of financial incentives for cadaveric organ donation.

We constructed a model in which property rights over the organs of the deceased are assigned to their surviving relatives and analyzed the consequences for the supply of cadaveric organs of paying for organ donation. We showed that in a world in which individuals are imperfectly informed about the preferences of their family members, in which survivors try to balance their own preferences about donation against the preferences of the deceased, and in which some members of the population consider posthumous organ donation a psychic cost, payments may induce a perverse

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supply response. The mechanism behind the perverse supply response is straightforward. Registering, or failing to register, as an organ donor provides a noisy signal to one's family members about the strength of one's preferences for being a donor.¹ Adding financial payments into the mix simply increases the noise-to-signal ratio of the registration decision in such a way that Bayesian surviving family members reduce their estimate of the strength of preferences for organ donation of the deceased.

Our analysis led us to two central policy conclusions.² First, property rights should be assigned unambiguously to the registered organ donor, eliminating the family from the decision-making process. Second, all individuals should be required to register explicitly either as a donor or non-donor. We concluded with the speculation that, under these institutional arrangements, the financial rewards necessary to induce an adequate increase in the supply of cadaveric organs will be modest.

In his commentary on our work, Tabarrok (2004) raises no objections to our policy recommendations. But he does object to how we got there. He complains that our preference structure is implausible, that most people view being an organ donor as a psychic benefit rather than a cost, and that what compensation buys is the effort of signing a donor card. Moreover, he argues, our analysis is irrelevant because a majority of states have already passed laws stating clearly that the intent of the donor is legally binding.

We do not doubt that in many states becoming a donor may be unnecessarily costly to the individual, and that financial incentives may help overcome this. This issue is fully addressed by our second policy conclusion, but it is not the central concern of our paper.³ Similarly, wider issues such as meta preferences for honesty, intrinsic motivation, altruism, and the evolution of social norms, have been put to one side in our attempt to explain the possible consequences of introducing financial incentives in the

¹ The signal is imperfect because there is a continuum of strengths of preferences, but the signal is binary.

² A third policy recommendation was that financial incentives should be limited to payments made posthumously. This was not central to the theme of the paper, but is rather based on ethical considerations. We showed in our analysis that payments made at the time of registering for organ donation, such as the discount on driver license renewals offered to registrants by the state of Georgia (a bill emulating this policy was introduced during the 2000 legislative session in Connecticut [HB 5461]), can induce time-inconsistent decisions that could lead in some instances to the unethical harvesting of organs from non-donors. Byrne and Thompson (2000) were the first to make a link between time-consistency and medical ethics.

³ Tabarrok sidesteps the time consistency problem raised by financial payments made for registration that we discuss at length in the paper.

current institutional environment. Simplification is, of course, the core of economic modeling and we make no apologies for it.

Tabarrok's dismissal of our concerns about the psychic costs of being a donor and the signaling problem is premature. Contrary to his claim, we make no assumption about the distribution of preferences over organ donation, and only a fraction of the population need view organ donation as costly for our results to hold. Survey evidence unambiguously shows that a significant fraction of the population thinks this way.⁴ Survey evidence equally clearly shows that individuals are imperfectly informed about the preferences of their family members,⁵ and that in making donation decisions they try to take into account these unknown preferences.⁶ Finally, survey evidence also indicates that these problems affect the donation decision (Siminoff and Lawrence 2002, Morgan 2004, and Morgan and Miller 2001).

But showing that the assumptions behind our model are factually relevant does not mean that their mathematical consequences are empirically important. Tabarrok rightly notes that we fail to present "any evidence . . . that financial compensation would reduce family agreement to donate" and that we "do not attempt to test [our] model." Is the signaling problem sufficiently important, relative to complicating features excluded from the model, that we would actually observe a perverse supply response? Of

⁴ In a well-known study, Gallup (1993) conducted a telephone survey of over 6,000 individuals to elicit attitudes toward organ donation. The study is frequently casually cited to show overwhelming support for organ donation, in much the way that Tabarrok cites it. Table 1 of the report shows that 85 percent of respondents to the survey question: "In general, do you support or oppose the donation of organs for transplants?" chose "support," but the practical import of this question is far from clear. After all, only 37 percent of these same respondents indicated that it was very likely that they themselves would become organ donors (Table 2), only 55 percent claimed they would be willing to be an organ donor (Table 10), and the non-survey evidence says that the number who actually will is even smaller.

⁵ Further findings from the Gallup poll are as follows. Among respondents who had indicated a wish to become an organ donor, only 52 percent had told family members (Table 14); among those who did not wish to become an organ donor, only 32 percent claimed to have told family members (Table 15); and 27 percent did not know whether most of their family members supported the "idea" of organ donation (Table 13). This limited communication is in part because thinking about one's death makes a significant minority (36 percent, Table 9) uncomfortable. It also appears to affect decision-making: while 42 percent of respondents had already made a decision about their own organs, only 25 percent had made a decision about their family members' organs (Table 10). See Guadagnoli et al. (1999) for a statistical analysis of the Gallup survey results. In interviews with family members of 360 dead patients in Pennsylvania trauma centers, Siminoff and Lawrence (2002) report that over 52 percent of the families had to guess the patient's preferences.

⁶ Siminoff and Lawrence (2002) report that when making the donation decision, 82 percent of families considered how the deceased patient might have felt about organ donation.

course, we do not know. Until such time as extensive use of financial incentives has been made, it is not apparent to us what data we could use to test the model. But we do explain what institutional changes could be made so as to sidestep the potential problem entirely.

As proponents of a wider use of financial incentives in health care,⁷ we would be very happy to be responding today to a commentary that had tested our model and found it wanting. But, instead, we find ourselves responding to an armchair critic. Tabarrok does not test the model, he does not suggest how we, or others, may test it, and he presents no new evidence. Instead, he selectively reports and interprets just three pieces of previously available data. First, he cites a finding in Siminoff *et al.* (2001) that 89.3 percent of families who knew that the deceased had a donor card agreed to donate as evidence that relatives take donor intent seriously, missing both the relevant evidence in Siminoff and Lawrence (2002) and the fact that in our model the strength of the perverse supply response is *increasing* in the weight family members put on donor intent. Second, he cites the evidence in Table 1 of the Gallup poll, the relevance of which is questionable, while ignoring the remainder of the study. To Tabarrok, this evidence is enough to condemn us with an allusion to methodological positivism.^{8,9}

Tabarrok's dismissal of our paper as irrelevant is equally premature. He argues that our work is just not relevant because as of January 2003 twenty-seven states had on their books "first-person consent" legislation clearly assigning property rights to the donor rather than the donor's family.¹⁰ He notes approvingly a wider and more aggressive movement toward increased personal autonomy for organ donors. We are also encouraged

⁷ In Byrne and Thompson (2000) we argue for cash payments to be made to encourage terminally ill patients to reject futile treatment.

⁸ While Tabarrok finds our model sufficiently unconvincing that no testing is necessary to dismiss our work, he worries that our model might sufficiently convince others that "financial compensation is a bad idea [precluding] the test that is supposed to justify the model" (Tabarrok 2004, 3 n4).

⁹ We did not cite Gallup (1993) in our original study for the simple reasons that (i) economists, as a rule, do not put much stock in survey results, and (ii) the Gallup poll is, in our view, utterly untrustworthy as scientific evidence. Tabarrok's use of the poll forces us to put that view to one side here. But we urge the reader not to take these telephone polls too seriously. Gallup reports in their survey that less than 2 percent of all respondents indicated that they did not intend to donate their organs because they wanted to be buried as a whole person (Tables 2 and 3), but 17 percent of them agreed with the statement that "it is important for a person's body to have all of its parts when it is buried." (Table 27).

¹⁰ Tabarrok acknowledges that some of these states implemented legislation after our paper went to press (in June 2000). In fact, over half of them did.

by the direction policy is taking as it is in exactly the direction we recommended. But we shouldn't get too complacent. First, as we made clear in our paper, first-person consent laws do not resolve the signaling problem when individuals are not required to declare themselves to be non-donors. Second, there remain 23 states without first-person consent laws, and an enormous gulf between law and practice continues to exist among those states that do have first person consent laws. Pennsylvania, for example, enacted first-person consent in 1994. The first of its three organ procurement organizations (OPOs) to enforce the law, the Center for Organ Recovery and Education, only did so in the face of much controversy in 1999. Many states are further behind in following their own laws. Texas, for example, has first-person consent, but none of its OPOs are prepared to harvest organs against the family's wishes.

We believe strongly that pilot studies are needed to determine whether financial incentives can improve organ donation rates. If the policy changes we advocate are adopted and followed, we believe that financial incentives *will* be successful. However, without mandated choice and proper assignment of property rights over organs to the donors, financial incentives may have perverse effects.

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Reply to Byrne and Thompson

ALEXANDER TABARROK*

IN THEIR RESPONSE, AS IN THEIR PAPER, BYRNE AND THOMPSON elide the implausible assumptions that drive their model. Consider their statement: “Only a fraction of the population need view organ donation as costly for our results to hold.” Since I agree that survey evidence “unambiguously shows” that some people do not want to be organ donors it would seem they have proven their case. Yet, they have omitted the most important aspects of their model. For their perverse supply result to occur, not only must some people who truly find the prospect of organ donation distasteful nevertheless sign their organ donor cards in response to a monetary incentive, but, also, a significant portion of families must, in an attempt to discern the donor’s “true” intentions, decide not to allow organ harvesting, reversing the express intent indicated on the card. Because of their love for the potential donor, the families will try to have their cake and eat it too—that is, receive the reward for signing the organ donor card and also receive the reward of *not* donating the organs.¹

In my Comment, I pointed out that these assumptions are highly implausible, both with respect to donors and their families. Do potential donors really want their families to back them out of their promises? Again, for most people the cost of signing an organ donor card is not the contingent possibility of donating an organ should you die, but rather the momentary mental anguish at having to ponder one’s death. Once the card is signed the latter costs are sunk, and the prospect of saving a life in the

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¹ More precisely, every family is moved in the direction of refusing to donate on the above grounds.

unfortunate contingency of your own early death becomes a *benefit* of having signed.

Byrne and Thompson ignore this line of reasoning, arguing, “simplification is of course the core of economic modeling and we make no apologies for it.” The issue, however, is not simplification—the assumptions that I make about preferences are equally simple—but whether the assumptions that they have chosen are remotely correct.

Contra Byrne and Thompson, I do object to their policy recommendations. Byrne and Thompson wish to eliminate the family from the decision making process, outlaw financial payments for organs prior to death (but allow posthumous payments), and create a national donor registry in which everyone would have to register as either a donor or a non-donor.

Since there is no evidence that perverse supply responses are a serious possibility I think that, on those grounds, the mandatory registration would be costly, with little promise of benefit.

Byrne and Thompson and I, of course, agree that a donor’s action, such as signing an organ donor card, ought to be invulnerable to posthumous reversal by relatives. However, if the government does not require everyone to register as a donor, or a non-donor, then there will continue to be cases in which the deceased individual has not acted either way. Eliminating the family entirely from the process, such that organs could be harvested only from registered donors, as Byrne and Thompson seem to propose, would tragically reduce the number of harvestable organs.

Finally, outlawing financial payments for organs prior to death would outlaw proposals like Lloyd Cohen’s (1989, 1995) “future’s market in organs” or my own “no give, no take” policy (Tabarrok 2002), some variant of which I see as the best hope for saving the 6,000 people who will die this year because of the policy-induced shortage of human organs.²

² Byrne and Thompson suggest that I “sidestep” the “time-consistency” problem. But, this so-called problem is just a variant of their main idea and, as such, is an artifact of their implausible premises. In their main model, the donor’s family takes into account that the donor wants to receive the reward and may also have “true” preferences not to donate. In the “time-consistency” model the donor himself reneges. Calling this a time-consistency problem is an abuse of terminology—the problem is no more a time-consistency problem than is involved in any contract. Byrne and Thompson think that if you allow contracts before death then it is possible that some people will change their minds about donation and it would be wrong to force non-donors to be donors. But, contract law rarely imposes specific performance. If someone wants out of their contract the law will let them out with the payment of damages—here repayment of the initial reward plus interest ought alone to suffice to eliminate cheats and the “unethical harvesting of organs” that Byrne and Thompson warn of.

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Stock Market Efficiency Withstands another Challenge: Solving the “Sell in May/Buy after Halloween” Puzzle

EDWIN D. MABERLY* AND RAYLENE M. PIERCE**

A COMMENT ON: BOUMAN, SVEN AND BEN JACOBSEN. 2002. THE HALLOWEEN INDICATOR, ‘SELL IN MAY AND GO AWAY’: ANOTHER PUZZLE. *AMERICAN ECONOMIC REVIEW* 92(5): 1618-1635.

[ABSTRACT, KEYWORDS, JEL CODES](#)

OVER THE PAST TWENTY YEARS FINANCIAL ECONOMISTS HAVE documented numerous stock return patterns related to calendar time. The list includes patterns related to the month-of-the-year (January effect), day-of-the-week (Monday effect), day-of-the-month (turn-of-the-month effect), and market closures due to exchange holidays (the holiday effect) to name just a few.¹ This research is cited as evidence of market inefficiencies (see,

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This paper benefited from editorial comments by Professor Tom Saving of Texas A&M University and encouraging comments by Professor Burton Malkiel of Princeton University. Additionally, constructive comments were provided by an anonymous referee.

¹ The January effect is frequently misinterpreted as implying that stock returns, irrespective of market size, are unusually large in January. From Fama (1991, 1586-1587), the January effect refers to the phenomenon that “stock returns, especially returns on small stocks, are on average higher in January than in other months. Moreover, much of the higher January return on small stocks comes on the last trading day in December and the first 5 trading days in January.”

for example, Schleifer (2000)). As a counter argument, Jensen (1978) highlights the importance of trading profitability when assessing market efficiency. If a trading rule is not strong enough to outperform a buy and hold strategy on a risk-adjusted basis then it is not economically significant.

In a *Wall Street Journal* commentary, Malkiel (2000) argues that “these attacks on the efficient market theory are far from convincing.” In the same commentary, Professor Richard Roll (principal of the portfolio management firm, Roll and Ross Asset Management) is quoted as saying,

If calendar time anomalies represent evidence of market inefficiencies, then they ought to represent an exploitable opportunity. I have personally tried to invest money, my client’s and my own money, in every single anomaly and predictive result that academics have dreamed up. And I have yet to make a nickel on any of these supposed market inefficiencies. Real money investment strategies don’t produce the results that academic papers say they should. If calendar time anomalies are evidence of market inefficiency, then there ought to be an exploitable opportunity (Malkiel 2000).

In a recent issue of the *American Economic Review*, Bouman and Jacobsen (2002) document yet another calendar time anomaly in stock prices, which they claim many Americans tend to be unfamiliar with. They label this anomaly the Halloween effect, as October 31 marks the end of the “scary period” for investors.² In particular, Bouman and Jacobsen conclude that stock returns are significantly lower during the May–October periods versus the November–April periods, and they propose a trading strategy to exploit this anomaly. The Halloween effect amounts to a “Sell in May and go away” strategy. The strategy is described as investing in a value-weighted index like the S&P 500 index during the November–April periods and in a risk-free investment like U.S. Treasury bills during the May–October periods. Bouman and Jacobsen remark that,

² Halloween is celebrated every October 31. Over a century ago, the American humorist Mark Twain (1894) remarked, “October, this is one of the peculiarly dangerous months to speculate in stocks.” But then Twain knew that other dangerous months “are July, January, September, April, November, May, March, June, December, August, and February.”

Surprisingly, we find the Sell in May effect is present in 36 of the 37 countries in our sample. The effect tends to be particularly strong and highly significant in European countries, and also proves to be robust over time. Sample evidence shows that in a number of countries it has been noticeable for a very long time, and in the U.K. stock market, for instance, we have found evidence of a Sell in May effect as far back as 1694. We find no evidence that the effect can be explained by factors like risk, cross correlation between markets, or the January effect. We also try some alternative explanations . . . but none of them seems to provide an explanation for the puzzle (Bouman and Jacobsen 2002, 1618).

The U.S. equity market is the world's largest in terms of market capitalization, and the value-weighted S&P 500 index is used worldwide as a benchmark for U.S. stock market performance. If the Halloween strategy is economically significant as suggested by Bouman and Jacobsen, then this phenomenon should carry over to U.S. based index futures, in particular to the S&P 500 futures contract. Since transaction costs are lower for index futures versus cash market transactions of similar size, the S&P 500 futures contract constitutes fertile ground for testing the trading rule "Sell in May and go away" versus the Buy and Hold strategy—a benchmark for market efficiency. Our objective is to re-examine the evidence presented by Bouman and Jacobsen documenting a Halloween effect for the U.S. stock market. A second objective is to examine the S&P 500 futures contract for evidence of a Halloween effect and to compare the Halloween strategy with the Buy and Hold strategy.

According to Fama (1998), empirical studies documenting long-term return anomalies are sensitive to methodology. Most long-term return anomalies tend to disappear with reasonable changes in technique; our results support Fama's argument. Possible explanations for apparent discrepancies between the results presented in the current study and those of Bouman and Jacobsen are model misspecification and data snooping. Bouman and Jacobsen's documentation of significant Halloween effects for U.S. equity returns appears to be driven by two outliers. The first outlier, October 1987, is associated with the 1987 crash in world equity prices. The second outlier, August 1998, is associated with the collapse of the hedge fund Long-Term Capital Management. (Incidentally, it is our casual observation that a preponderance of major economic and/or political

events that negatively impacted world equity prices have occurred during the May-October periods. Another example is the 1990 invasion of Kuwait by Saddam Hussein in August and the attendant increase in world oil prices.)

The S&P 500 futures contract debuted April 1982 on the Chicago Mercantile Exchange (CME), and, therefore, the futures' data set covers the period April 1982–April 2003. The empirical evidence supports the hypothesis that there is no economically exploitable opportunity in the S&P 500 futures market associated with the Halloween effect. For S&P 500 index futures, the Halloween strategy of “Sell in May and go away” underperforms the Buy and Hold strategy by a wide margin, at least through April 2000. Thereafter, U.S. equity prices entered a bear market, and any strategy that includes short positions in S&P 500 futures yields superior results.

REVIEW OF HALLOWEEN EFFECT EVIDENCE

Bouman and Jacobsen investigate monthly returns across world stock markets for the period January 1970–August 1998 and conclude that monthly returns are unusually large during the November–April periods. Their study reports that, “A simple strategy based on the saying would outperform a buy and hold portfolio in many countries . . . and would also be a lot less risky” (1619). At first glance, the results reported by Bouman and Jacobsen appear to be at odds with the efficient market hypothesis. The closing line of their article notes that, “we are faced with the following problem: History and practice tells us that the old saying [Sell in May and go away] is right, while stock market logic tells us it is wrong. It seems that we have not yet solved this new puzzle” (1630).

Halloween Strategy: Previously Known by Wall Street Professionals

A widely known practitioner oriented investment book is Hirsch's *Stock Trader's Almanac*, an annual publication since 1968. In the 1986 edition and thereafter, Hirsch makes reference to a Six-Month Switching strategy that is identical to the Halloween strategy (In 1993, Ned David Research, Inc. published a similar study, and Bouman and Jacobsen cite this study). In particular, in the 1997 edition, Hirsch presents a spreadsheet of annual

returns for the Six-Month Switching strategy over the period 1950-1996 (Hirsch 1997, 54). Results are reported for the Dow Jones Industrial Average (DJIA). Hirsch shows that a \$10,000 investment in the DJIA beginning in 1950 grew to \$206,762 conditional on the proceeds being invested exclusively over the November-April periods. In contrast, by investing the proceeds exclusively over the May–October periods the investment grew to only \$17,272. The difference in the two investment strategies is striking, and in response Hirsch remarks, “Don’t tell the big boys about this! Let’s keep this one to ourselves (Hirsch 1997, 54).” Hirsch’s Six-Month Switching strategy has been in the public domain since the late 1980s. The concept of efficient markets suggests that once information becomes widely known, then excess risk-adjusted returns are arbitrated away. The January effect received much publicity in the financial press in the early 1980s, and as Fama (1991, 1587-1588) demonstrates, the January effect became statistically insignificant over the period 1982–1991. In particular, the difference in January returns between small and large firms was economically exploitable over the period 1940-1981, but this phenomenon disappears after 1981. Finance theory predicts a similar fate for the Halloween anomaly, especially in well-developed capital markets like the United States.

Halloween Strategy: Robustness to Alternative Model Specifications

To test for the existence of a Halloween effect, Bouman and Jacobsen apply the usual dummy regression technique, which is equivalent to a simple means test—are monthly mean returns over the November-April periods significantly different from the May–October periods? To maintain consistency with Bouman and Jacobsen, in this study, this is represented as:

$$R_t = \mu + \alpha_1 S_t + \varepsilon_t \quad (1)$$

The dependent variable R_t represents continuously compounded monthly index returns for a value-weighted index. Thus, R_t is defined as the natural logarithm of the price relative.

The dummy variable S_t takes on the value 1 if month t falls within the November-April periods and 0 otherwise. The constant term μ represents the monthly mean return over the May-October periods while $\mu + \alpha_1$ represents the monthly mean return over the November-April

periods. A positive and significant α_1 indicates that monthly mean returns are larger over the November–April periods, and Bouman and Jacobsen take this as evidence of a significant Halloween effect.

As confirmation of Bouman and Jacobsen’s results for the U.S. stock market, equation (1) coefficients are estimated over the period January 1970–August 1998 based on value-weighted Center for Research in Security Prices (CRSP) index returns with dividends.³ The results, as reported in Panel A, Table 1, are virtually identical to those reported by Bouman and Jacobsen.⁴ In particular, the monthly mean return over the May–October periods ($\mu = 0.4235$ percent) is not significantly different from zero at a meaningful level. However, the coefficient of interest is α_1 , positive at 1.0349 percent and significantly different from zero at the 5 percent level. A statistically significant and positive α_1 is confirming evidence of a Halloween effect in the U.S. stock market. However, differences between the Halloween strategy and the Buy and Hold strategy are not economically significant, especially after making adjustments for transaction costs and short-term capital gains taxes.

Impact of outliers on results

The October 1987 stock market crash was a worldwide phenomenon impacting all world stock markets. In October 1987, U.S. stocks fell on average by over 20 percent. As noted previously, Bouman and Jacobsen document unusually low U.S. monthly returns over the May–October periods, but their finding is potentially driven by the fact that the Crash of 1987 occurs in October. Irrefutably, the October 1987 stock market crash is an outlier, and this is verified by a within sample z-score of -6.234 and corresponding p-value of 0.3×10^{-9} . From time series estimation procedures,

³ Bouman and Jacobsen examine value-weighted indices with dividends reinvested. To be consistent with Bouman and Jacobsen’s results, this study examines a value-weighted index with dividends reinvested. A preferred choice is the S&P 500 index, but this index does not reflect the impact of dividends on holding period returns. However, the S&P 500 index yields almost identical results versus the value-weighted CRSP index with dividends. The correlation between monthly CRSP with dividends and S&P 500 index returns over the period January 1970–August 1998 equals 0.987. CRSP is associated with the University of Chicago’s School of Business.

⁴ Table 2 presents results for S&P 500 index futures. However, results reported in the two tables are not directly comparable due to differences in the time interval examined. The reader is referred to the explanatory notes for Table 2.

it is well known that estimation of equation (1) coefficients and their significance via ordinary least squares is highly sensitive to outliers.

Bouman and Jacobsen's data set contains 344 monthly returns with the most recent month being August 1998. On August 17, 1998, the Russian government unexpectedly announced a moratorium on debt repayment, and this event threw world financial markets into a tailspin. This event and others led to the collapse of the hedge fund Long-Term Capital Management in August 1998, a month in which U.S. stocks fell on average by over 15 percent. Thus, August 1998 is a potential outlier, and this is verified by a within sample z-score of -4.270 and corresponding p-value of 0.9×10^{-5} . The decision by Bouman and Jacobsen to include August 1998 in their sample period increases the probability of rejecting the null hypothesis. Thus, two months are identified that potentially drive the findings of a statistically significant Halloween effect over the period January 1970–August 1998.

Controlling for the impact of outliers, equation (1) is modified by inserting a second dummy variable D_t , which is set equal to 1 for October 1987, 1 for August 1998, and 0 otherwise:

$$R_t = \mu + \alpha_1 S_t + \alpha_2 D_t + \varepsilon_t \quad (2)$$

The estimated coefficients for equation (2) are reported in Panel B, Table 1, but the results are reversed from those reported for equation (1). In particular, the Halloween effect is represented by $\alpha_1 = 0.7784$ percent, but given a p-value of 0.092, this coefficient is no longer statistically significant at a meaningful level. Furthermore, monthly returns over the May–October periods are represented by $\mu = 0.6800$ percent, and this coefficient is now statistically different from zero at a p-value of 0.038. In equation (2), μ represents the monthly opportunity cost of being in Treasury bills over the May–October periods relative to the Buy and Hold strategy after adjusting for the impact of outliers, and this result is economically significant. The impact of the two outliers is represented by α_2 , which is negative in sign and highly significant. It appears that documentation of a statistically significant Halloween effect in the U.S. stock market over the period January 1970–August 1998 is being driven by the large negative returns observed during the months of October 1987 and August 1998.

Table 1
The Halloween Effect: Review of Evidence for U.S. Equity Prices
January 1970 through August 1998

$$R_t = \mu + \alpha_1 S_t + \alpha_2 D_t + \alpha_3 J_t + \varepsilon_t$$

Panel A	Coefficient	t-value	p-value
μ	0.4235	1.21	0.226
α_1	1.0349	2.10	0.037
α_2	N/A	N/A	N/A
α_3	N/A	N/A	N/A
Panel B			
μ	0.6800	2.08	0.038
α_1	0.7784	1.69	0.092
α_2	-22.0560	-7.27	0.000
α_3	N/A	N/A	N/A
Panel C			
μ	0.6800	2.08	0.038
α_1	0.6205	1.28	0.200
α_2	-22.0560	-7.27	0.000
α_3	0.9363	1.08	0.282

R_t represents monthly continuously compounded returns for the value-weighted Center for Research in Security Prices index with dividends. The constant term μ represents the monthly mean return over the May-October periods. The monthly mean return over the November-April periods is represented by $\mu + \alpha_1$. The impact of the two identified outliers October 1987 and August 1998 is represented by α_2 . The impact of January returns is represented by α_3 . Panel A corresponds to Equation (1); Panel B corresponds to Equation (2); and Panel C corresponds to Equation (3).

Impact of January returns on results

Studies by Haugen and Lakonishok (1988) and others suggest that stock returns are unusually large in January and label this phenomenon the January effect.⁵ The unusually large monthly returns documented by Bouman and Jacobsen during the November–April periods could be a manifestation of the January effect, and Bouman and Jacobsen test for this possibility by including a January dummy in their regression analysis. To duplicate Bouman and Jacobsen’s analysis, equation (1) is modified by

⁵ The reader is referred to footnote 1 for clarification of the January effect.

inserting a third dummy variable J_t , which is set equal to 1 whenever month t is January and 0 otherwise:

$$R_t = \mu + \alpha_1 S_t + \alpha_2 D_t + \alpha_3 J_t + \varepsilon_t \quad (3)$$

The estimated coefficients for equation (3) are reported in Panel C, Table 1, and are similar to those reported for equation (2). As before, the Halloween effect is represented by $\alpha_1 = 0.6205$ percent, but given a p-value of 0.200, the statistical significance of the Halloween effect is reduced further by inserting the January dummy.

INDEX FUTURES AND THE HALLOWEEN TRADING STRATEGY

Description of Data Set

On the CME, S&P 500 futures trade in four contract months—March, June, September and December—with the last trading day the Thursday preceding the third Friday of the contract month. In April 1982, the contract multiplier was set at \$500, but after the close of business on October 31, 1997 the multiplier was halved to \$250. The data set consists of daily S&P 500 futures settlement prices over the period April 30, 1982–April 30, 2003. Observations are selected from the contract closest to maturity with one minor modification related to contract expiration. On the last business day of the month prior to the contract month, observations are switched to the next most distant contract. For example, in January, observations correspond to the March contract, but then on the last business day of February, observations are switched to the June contract and so forth for the other contract months.

The Halloween Effect: S&P 500 Futures

In this section, the analysis is conducted in terms of rates of return using the natural logarithm of the price relative. Another return metric for futures contracts is the price change. As noted by Chance and Rich (2001), a forward (futures) contract is a zero investment strategy. Dusak (1973)

argues that margin deposits do not represent capital invested in futures contracts, and her analysis is conducted in terms of price changes. The margin associated with futures is a performance bond, and U.S. investors have the option of satisfying the initial margin with Treasury bills.

To maintain consistency with the return metric used in equation (1) through equation (3) for spot prices, monthly S&P 500 futures returns are defined as the natural logarithm of the price relative. Monthly returns are calculated for each S&P 500 futures contract over the period April 1982–April 2003 based on the switching rule established earlier. The usual dummy regression technique is applied to test for the existence of a Halloween effect in the market for index futures. Equation (3) is replicated, but the dependent variable FR_t now represents monthly S&P 500 futures returns. This is represented as:

$$FR_t = \mu + \alpha_1 S_t + \alpha_2 D_t + \alpha_3 J_t + \varepsilon_t \quad (4)$$

As before, the dummy variable S_t takes on the value 1 if month t falls within the November–April periods and 0 otherwise. The previous section examines monthly returns to the value-weighted CRSP index with dividends, and October 1987 and August 1998 are identified as outliers. After adjusting for the impact of these two outliers, the Halloween effect disappears. October 1987 and August 1998 are identified as outliers for S&P 500 futures, and the dummy variable D_t in equation (4) is inserted to adjust for the impact of these two outliers on returns. D_t takes on the value 1 for October 1987, 1 for August 1998 and 0 otherwise. In equation (4) the dummy variable J_t , which is set equal to 1 whenever month t is January and 0 otherwise, is an adjustment for the January effect.

Table 2
The Halloween Effect: Review of Evidence for S&P 500 Futures
April 1982 through April 2003

$$FR_t = \mu + \alpha_1 S_t + \alpha_2 D_t + \alpha_3 J_t + \varepsilon_t$$

Panel A	April 1982-April 2000*		
	Coefficient	t-value	p-value
μ	0.7928	20.7	0.039
α_1	0.3737	0.66	0.508
α_2	-20.3610	-2.80	0.000
α_3	0.9540	0.94	0.349
Panel B	April 1982-April 2003		
	Coefficient	t-value	p-value
μ	0.3369	0.88	0.378
α_1	0.5687	1.01	0.314
α_2	-19.9050	-3.02	0.000
α_3	0.8340	0.82	0.413
Panel C	April 1982-April 2000		
	Coefficient	t-value	p-value
μ	0.3042	0.7	0.486
α_1	0.7171	1.10	0.275
α_2	N/A	N/A	N/A
α_3	1.5470	1.29	0.197
Panel D	April 1982-April 2003		
	Coefficient	t-value	p-value
μ	0.0209	0.05	0.959
α_1	0.8847	1.46	0.147
α_2	N/A	N/A	N/A
α_3	1.8340	0.76	0.449

*Using the Center for Research in Security Prices (CRSP) value-weighted index and examining the period April 1982-April 2000, the respective coefficients are: $\mu = 1.200$ (p-value = 0.001), $\alpha_1 = 0.6021$ (p-value = 0.267), $\alpha_2 = -22.576$ (p-value = 0.000) and $\alpha_3 = 0.7943$ (p-value = 0.417).

FR_t represents monthly returns for the S&P 500 futures contract. The constant term μ represents the monthly mean return over the May-October periods. The return metric is the natural logarithm of the price relative. The monthly mean return over the November-April periods is represented by $\mu + \alpha_1$. The impact of the two identified outliers October 1987 and August 1998 is represented by α_2 . The impact of January returns is represented by α_3 . Panel A and Panel B correspond to Equation (4); Panel C and Panel D correspond to Equation (4) eliminating $\alpha_2 D_t$.

PRESENTATION OF EMPIRICAL RESULTS: S&P 500 FUTURES

Equation (4) coefficients are first estimated for the subperiod April 1982–April 2000, and these results are presented in Panel A, Table 2. April 2000 is identified as the end of an 18-year bull market that began in August 1982. The period after April 2000 marks the beginning of a major bear market. In particular, the Halloween effect is represented by $\alpha_1 = 0.3737$ percent, but this coefficient is insignificant with a p-value of 0.508. Excluding the two identified outliers, monthly S&P 500 futures returns over the period May–October are positive ($\mu = 0.7928$ percent) and significantly different from zero at a p-value of 0.039.⁶

In Panel B, Table 2, equation (4) coefficients are estimated for the period April 1982–April 2003, which includes the three-year bear market that commenced in April 2000. All of the coefficients are insignificant at a meaningful level except α_2 , which reflects the impact of the two identified outliers.

Based on the evidence presented in Table 2, the hypothesis that there exists a significant Halloween effect for the S&P 500 futures contract is rejected. The Halloween effect coefficient α_1 remains insignificant at a meaningful level even after removing the outlier dummy from equation (4), and these results are presented in panels C and D, Table 2. Therefore, the lack of supporting evidence for a Halloween effect for S&P 500 futures is not dependent on inclusion of the outlier dummy variable.

The Halloween Effect: S&P 500 Futures Trading Strategies

Two S&P 500 futures trading strategies referred to as Strategy-I and Strategy-II are identified to exploit the Halloween effect, and both of these strategies are compared against the Buy and Hold strategy. These three trading strategies are defined as follows: (1) The Buy and Hold strategy: Long one S&P 500 futures contract over the investment horizon April 30, 1982–April 30, 2003; (2) Strategy-I: Long one S&P 500 futures contract over the November–April periods and short one S&P 500 futures contract over the May–October periods; (3) Strategy-II: Long one S&P 500 futures

⁶ From the explanatory notes found in Table 2, similar results are reported for spot prices.

over the November–April periods, and no S&P 500 futures position over the May–October periods.

Profits (losses) for each S&P 500 futures trading strategy are calculated as \$500 times the change in index points with all realized profits (losses) invested in Treasury bills.⁷ As noted previously, the S&P 500 futures contract multiplier was split in half after October 1997, and thus the number of contracts identified with each strategy increases from 1 to 2 after this date. Treasury bill rates are taken from Ibbotson & Associates (2003) *Valuation Edition*.

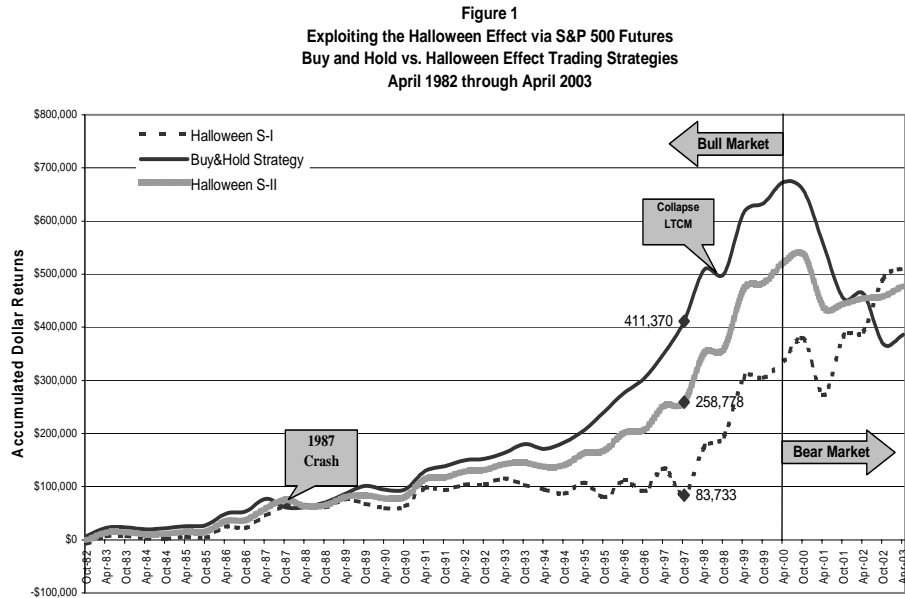
The accumulated dollar profits from each of the three strategies over the investment horizon April 1982–April 2003 are depicted in Figure 1. The Buy and Hold strategy initially outperforms the other two strategies but loses ground momentarily around the October 1987 stock market crash. This observation is not unexpected, as both Strategy-I and Strategy-II benefited from either being short S&P 500 futures or out of the market in October of 1987. Thereafter, the Buy and Hold strategy outperforms the other two strategies by a wide margin through April 2000, which marks the beginning of a major bear market. For example, over the period April 1982–October 1997, the accumulated dollar profits to the Buy and Hold strategy equals \$411,370 versus \$83,733 for Strategy-I and \$258,778 for Strategy-II.

A bear market commenced in April 2000 and thereafter the Buy and Hold strategy performs poorly relative to the other two strategies.⁸ This observation is not unexpected, as both Strategy-I and Strategy-II are either short S&P 500 futures or out of the market entirely over the period May–October. Mark Hulbert, financial journalists and editor of *Financial Digest*, recently stated, “In bull markets, timers rarely beat their nemesis—a buy-and-hold. It’s only in bear markets that they stand a chance of coming out ahead”(Hulbert 2003). Hulbert conjectures that market-timing strategies like the Halloween strategy outperform the Buy-and-Hold strategy only during bear market years, and this paper reports similar results. An interesting casual observation is that in bear-market years like 2000, 2001, and 2002 most of the decline in stock prices occurred during the May–October

⁷ In Figure 1, the return metric is the price change. The dollar difference between the Buy and Hold strategy and the other two strategies is the opportunity cost associated with the Halloween strategy.

⁸ There is no precise definition for a bear market, but intuitively it represents a year when equity prices decline or a decline from peak to trough by more than 20 percent.

periods.⁹ In summary, this paper rejects the hypothesis that the Halloween effect presents an exploitable trading rule for the S&P 500 futures contract.



Strategy-I: Long one S&P 500 futures contract over the November-April periods and short one S&P 500 futures contract over the May-October periods.

Strategy-II: Long one S&P 500 futures contract over the November-April periods and long T-Bills over the May-October periods.

CONCLUSION

Bouman and Jacobsen examine the period January 1970–August 1998 and document unusually high monthly returns during the November–April periods for both U.S. and foreign equity markets, and label this phenomenon the Halloween effect. The Halloween effect is considered an

⁹ For example, a \$100,000 investment in the value-weighted CRSP index with dividends on December 31, 1999 declines to \$62,250 by December 31, 2002. However, 70% of the decline in value, or \$26,376, is attributable to the May–October periods.

exploitable anomaly, which is taken as another example of market inefficiency. The rule is to sell stocks at the end of April and buy stocks at the end of October with all proceeds invested in a risk-free investment in the interim.

This paper re-examines Bouman and Jacobsen's results for the U.S. stock market and extends the analysis to S&P 500 futures. The futures' data set covers the period April 1982–April 2003. On re-examination, the documentation of a Halloween effect in the U.S. disappears after an adjustment is made for the impact of outliers, in particular the large monthly declines for October 1987 and August 1998 associated with the stock market crash and collapse of the hedge fund Long-Term Capital Management, respectively. For the U.S., the empirical evidence indicates that the Halloween effect is not an exploitable anomaly, and this is true for both spot and futures prices. However, in bear market years there exists anecdotal evidence that most of the negative decline in equity prices occurs during the May–October periods.

Bouman and Jacobsen report significant Sell in May effects in 36 out of 37 countries examined. The current paper argues against the existence of exploitable Sell in May effects in U.S. financial markets. However, the existence of an exploitable Halloween effect in the other 35 foreign markets is not addressed in this paper and is a subject for future research. However, preliminary results for Japanese return data do not support the existence of an exploitable Halloween effect in Tokyo.¹⁰

In a recent *Financial Analysts Journal* editorial comment, Robert D. Arnott remarked, “liquidation of all stocks in an institutional portfolio . . . is a ‘zero-tolerance decision,’ in which a decision must succeed or else the manager is fired” (2003, 8). Followers of the Halloween strategy liquidated all stocks on April 30, 2003, which qualifies the Halloween strategy as a “zero-tolerance decision.” The decision must succeed or else the manager is fired. Ex post, this decision was disastrous as S&P 500 index returns over

¹⁰ The Japanese stock market is currently the world's second largest in terms of market capitalization. The period since the internationalisation of the Japanese stock market in the mid-1980s is examined for a significant Halloween effect. Equation (2) coefficients are estimated based on monthly returns to the Nikkei 225 index over the period January 1985–October 2003. Four outliers are identified: August 1987, August 1990, September 1990, and August 1998. The results seriously question the existence of an exploitable Sell in May anomaly in Tokyo. In particular, the Halloween effect is represented by $\mu_1 = 0.8451$ percent, but given a p-value of 0.293, this coefficient is insignificant. The August 1990 invasion of Kuwait by Saddam Hussein had a major impact on Japanese stock prices in both August and September of 1990 (Japan is heavily dependent on imported oil).

the period May 2003–October 2003 were unusually large at 14.59 percent and even larger for Japanese stocks (Nikkei 225 index) at 34.15 percent.

Richard Wyckoff, broker, trader and publisher during the early twentieth century, was prescience when he remarked some 70 years ago,

At the time many thought that the market could be beaten by mechanical methods; that is, by some means other than human judgment. [Charles] Dow suggested a few of these. [Roger] Babson had one or more. All kinds of individuals came forward with ways of beating the stock market; each was certain his method would make a fortune. Not long afterward, however, after further study, I decided that methods of this kind, which substitute mechanical plays for judgment, must fail. For the calculations on which they are based omit one fundamental fact, i.e., that the only unchangeable thing about the stock market is its tendency to change. The rigid method sooner or later will break the operator who blindly follows it. (Wyckoff 1930, 163-164).

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An Ivory-Tower Take on the Ivory Trade

MICHAEL DE ALESSI*

A COMMENT ON: KREMER, MICHAEL, AND CHARLES MORCOM. 2000.
ELEPHANTS. *AMERICAN ECONOMIC REVIEW* 90(1): 212-234.

[ABSTRACT, KEYWORDS, JEL CODES](#)

THE MARCH 2000 ISSUE OF THE *AMERICAN ECONOMIC REVIEW* featured an article by Michael Kremer and Charles Morcom, titled simply, “Elephants.” As one would expect in such a prestigious journal, the article is clever and the mathematics are sound. The paper, essentially, takes a species-extinction model of the kind pioneered by Colin Clark (1973) and introduces the wrinkle that the valuable good derived from certain species is storable, and hence subject to speculation. Kremer and Morcom focus on how the price of ivory affects the incentive to poach elephants and how government policies can be developed to address this problem. Despite an ostensible emphasis on policy, however, the ‘state of the world’ that is assumed throughout the paper is so far removed from the real world of elephant conservation that the authors’ policy recommendations ring hollow. Indeed, they are effectively irrelevant. This comment reviews the shortcomings of Kremer and Morcom’s article and briefly explains a more important institutional approach to elephant conservation.

Speaking to the *New York Times*, Kremer described the problem of elephant poaching.

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If some people go out and poach a lot of elephants, and everyone thinks that the elephants are going to go extinct and there's not going to be any more ivory, that can induce speculation on ivory prices. . . . And that speculation on ivory prices can induce more people to go poach. So you get the possibility of self-reinforcing expectations (quoted in Postrel 2000, C2).

Under this scenario, Kremer and Morcom present two options for preventing extinction. First, if possible, governments should create credible threats to poachers that kick in once a species dips below a certain critical level, because “expectations of future government antipoaching enforcement will affect current poaching” (Kremer and Morcom 2000, 227). Credibility, however, is a problem in many parts of the world. The second option Kremer and Morcom would have governments pursue is to stockpile supplies of ivory, releasing it into the market at a rate that keeps prices low and, therefore, limits the incentives for poaching.

Kremer and Morcom make several egregious assumptions about the “world” their article describes. First, they assume that trade in ivory is legal; second, that elephant habitat is constant; third, that all elephants exist in a state of open-access; and forth, that state intervention is the only viable approach to conservation for its own sake. All four assumptions depart from the real world.

The UN sponsored Convention on International Trade in Endangered Species of Flora and Fauna (CITES) has banned any international trade in elephant ivory since 1989. Of course, the 160 nations that are a party to CITES have varying degrees of enforcement, but, in general, legal trade routes are well monitored. CITES has no domestic jurisdiction, so ivory may be sold internally, but the really lucrative markets are international. Thus, underground markets have developed, especially in Asia.

The parties to CITES have made some exceptions for one-off sales of stockpiled ivory, not to affect the price of ivory, but to help pay for conservation programs and community development. In the case of the first one-off sales allowed in 1998, Zimbabwe, Botswana, and Namibia spent years pushing for the opportunity. In South Africa's case, it took over ten years of trying before it was finally permitted a one-off sale in 2002. Even aside from what public choice theory has taught economists about how, in the real world, governments work, long delays (measured in years) in obtaining international sanction for any sale makes it unlikely that

Kremer and Morcom's second policy proposal—to stockpile and sell off ivory to fine tune the price—could work.

The Kremer-Morcom model holds habitat *constant*. Yet the greatest threat to elephants is not poaching, but habitat loss. The United Nations Environment Program predicts that because of rapid growth of human populations in Africa, throughout the elephant's range, habitat loss and degradation will be the major threats to elephant survival¹ (Barnes 1994, Infield 1990 as cited by UNEP 2003).

Kremer and Morcom also ignore the fact that, along with the positive value of their ivory, elephants have a negative value. Elephants trample crops, humans, and even dwellings. To the people living around them, elephants are a nuisance. So even if CITES, or the Kremer-Morcom credible threat/stockpile approach, were successful, there would still be a tremendous incentive for rural Africans to convert habitat into cropland and to support reductions in elephant populations.

The only way that rural communities will support large elephant populations is if they see *value* in those elephants. This is the reason why many southern African countries have initiated programs to devolve some control over wildlife, and wildlife revenues, to local communities. The most notable is the CAMPFIRE program in Zimbabwe. Despite government corruption, these programs have been more successful than the prohibition schemes attempted in countries like Kenya.

Zimbabwe and other Southern Africa countries, like Botswana, Namibia, and South Africa, have also ceded some management authority to private landowners as well, and the growth of private wildlife conservancies has been one the real conservation success stories in Africa—so much so that even Kenya has experimented with them (De Alessi 2000). Clearly, Kremer and Morcom's assumption that all elephants exist in a state of open access is way off base.

Kremer and Morcom do discuss “the case in which it becomes profitable to protect the resource as private property at a sufficiently high price” (214), but again, only from a state of open-access. And by focusing only on the consumption of ivory, they ignore other, private approaches to conservation. Elephants are charismatic species, as evidenced by the

¹ In fact, as unlikely as it may sound to the casual observer, African elephants are *not* in danger of extinction. Their numbers did decline rapidly in the 1980s, but in many Southern African countries, there are actually *too many* elephants. In Zimbabwe, for example, the International Union for the Conservation of Nature (IUCN), a respected authority, last published a census for elephants in 1998. They estimated the population between 60,000 and 80,000 elephants, while the carrying capacity in Zimbabwe is closer to 30,000 (IUCN 1998).

millions of dollars spent every year on photo-safaris in Africa, i.e., non-consumptive use, and donated to groups like the World Wide Fund for Nature (WWF). The people who spend their money this way are deeply concerned about elephants, not because their tusks make nice carvings, but because they are magnificent beasts. But Kremer and Morcom consider only the market value of ivory (indeterminable since there is no real market), and assume that only the state values non-consumptive use.

Kremer and Morcom attempt to play up the importance of their storability wrinkle angle by referring to the decline of the American bison. The near-extinction of the bison “followed an improvement in the tanning process for buffalo hides, which presumably increased their storability” (214). Elephants, Kremer and Morcom argue, face similar problems. Hides from bison and ivory from elephants can be stored, and so these species face an increased likelihood of extinction due to speculation that prices will increase as they near extinction. Storability surely played some role in the depletion of the bison (although the most famous anecdotes are of bison killed only for their tongue meat and otherwise left to rot), but the heart of the problem was that bison, like many elephants, were valuable but unowned. Such was the case until six individuals, who either saw commercial opportunities or simply wanted to save the species, privatized some of the bison in the 1870s. Today, virtually all plains bison in the United States are descended from these animals (Sugg 1999). With time, however, the only real profits from these animals came from zoos and other displays, and from their meat—leaving little to be explained by the storable-goods model.

The authors also try to fit rhinos to their model. When Zimbabwe undertook a program of de-horning its black rhinos², poachers still killed the de-horned animals. Kremer and Morcom attribute this behavior to poachers trying to raise the price of their stockpiles. But since it often takes days to track an individual rhino, it is far more likely that poachers simply wanted to make sure they did not waste their time tracking the same hornless animal a second time (De Alessi 2000).

Until recent political upheavals in Zimbabwe, rhino numbers had been rising since the de-horning program was abandoned in favor of moving the remaining animals to private game reserves. The rhinos on these private conservancies are tracked every day by a protective scout, and

² The makeup of rhino horn is similar to a mass of hair, and a de-horned rhino takes about five years to return to form.

local communities are rewarded for turning in potential poachers—effectively eliminating poaching (De Alessi 2000).

An even greater rhino success story exists in South Africa, where the Natal Parks Board reintroduced white rhinos to its parklands, then set out to commercialize the species by selling them to private land owners “as a ‘draw-card’ species for both hunting and non-consumptive tourism” (t Sas-Rolfes 1998, 3). As a result, the Southern white rhino is the strongest rhino population in the world. Once again, the ‘storability’ of rhino horn had little to do with this conservation success.

The private approach, ignored by Kremer and Morcom, also offers the greatest hope for elephant conservation—an approach that depends not in devaluing the animals under an open-access regime, but in making them *more* valuable under differing types of private ownership regimes (see Simmons and Kreuter 1989). Kremer and Morcom simply ignore institutional change, and the evolution of property rights, as discussed by Anderson and Hill (1975 and 2001).

Kremer and Morcom state that “It is expensive to protect elephants as private property” (p. 214), but expensive for whom? For Kremer and Morcom, it is state “expenditures on game wardens, helicopters, and other antipoaching efforts” that matter (227). But why consider only state expenditures, especially in poor, developing countries?

In Zimbabwe tremendous resources were allocated to creating and improving wildlife habitat through a simple change in the law that allowed landowners and communal villages to manage wildlife as if it were their own. The law maintained the state as the ultimate management authority, but in practice, game animals were devolved to property owners (Child 1995). Very quickly, much agricultural land was converted back to wildlife habitat. Under a strict regulatory, no-take regime, protecting elephants is expensive for the state. Allowing wildlife to be privately protected incurs little cost to the state.

The work of Terry Anderson and P. J. Hill (1975 and 2000) is especially encouraging in this regard. They looked at the evolution of property rights in the American West, and how a number of innovative solutions to the problem of how to protect cattle grew out of private ownership. In the frontier American West, no one could initially imagine how privately owned cattle could be monitored and protected, but left to their own devices, cattlemen developed a complex system of brands and cattlemen’s organizations to sort out ownership on the range. Then outside entrepreneurs developed barbed wire as an inexpensive way to fence in cattle. One can only imagine what an expensive and ineffectual mess would have resulted if

the West were still an open range with a government bureaucracy devoted to keeping the price of cattle low enough to discourage widespread depletion. Why promote such a program for elephants?

Kremer and Morcom also indirectly take on endangered species protection in the United States. Their anti-poaching model “provides a potential justification for laws which mandate protection of endangered species with little or no regard to cost” (Kremer and Morcom 2000, 215), which is exactly the approach taken by the U.S. Endangered Species Act (ESA). Paying little regard to cost is a suspect approach to begin with, but it becomes wholly counterproductive when those costs are borne by the private sector. The U.S. ESA prevents the usage of private land when federally listed endangered species are present. This creates a perverse incentive, which has led landowners to engage in preemptive habitat destruction to avoid the potentially devastating financial impact of the ESA. For example, owners of forests that could evolve into endangered red-cockaded woodpecker habitat (the woodpeckers prefer old-growth trees) tend to cut their trees ahead of schedule to avoid attracting the birds (Lueck and Michael 2003). Taking a similar approach to ‘protecting’ species in Africa is likely to be just as counterproductive as it has been in the United States.

Programs like CITES and the U.S. ESA rely on devaluing species, while the devolution model (creation of private property rights) relies on just the opposite. Prohibitions have never really worked, and even if they could be made to work, there would still be no positive conservation incentives. As noted by Virginia Postrel, in the *New York Times*, the real policy innovations for protecting elephants will come when economists and others start to recognize the importance of institutions and “how the structure of property rights might be changed to encourage people to protect elephants” (Postrel 2000, C2).

The exclusive focus on state intervention, as opposed to private action, is the ultimate failure of the Kremer-Morcom approach. To propose ‘credible’ government threats, or to task a government bureaucracy with keeping ivory prices low, is to indulge the vice of letting neat model-bound ideas pass as policy discourse. A far more valuable article would have explored the kinds of institutional arrangements that have resulted in species conservation, and how those institutions might be applied and adapted to the elephant case.

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Response to De Alessi

MICHAEL KREMER*

IN “ELEPHANTS” CHARLES MORCOM AND I ARGUE THAT THE economics of open-access, storable natural resources, such as ivory, differ fundamentally from those of nonstorable, open-access resources, such as fish (Kremer and Morcom 2000). In general, overharvesting of open-access resources reduces long-run yield or, in extreme cases, leads to extinction. Overharvesting, therefore, makes the future price high. We argue that if the good can be stored, the expectation of high prices in the future can lead to high prices in the short run, as speculators buy up the resource. This stimulates increased harvesting, or poaching, in the short-run. Thus, for storable, open-access resources, expectations of high harvesting rates can be self-fulfilling and there may be multiple rational expectations equilibria: for example, one in which the species is driven to extinction and one in which it survives.

A government which seeks to preserve the resource can potentially eliminate the extinction equilibrium if it can *ex ante* credibly commit to endangered species laws that mandate that if a species nears extinction the government will spend enough to protect the species, even if this would not be justified on cost-benefit grounds *ex post*. However, many governments may not be able to credibly commit to do this. We argue that such governments could eliminate the extinction equilibrium by building up stockpiles of the storable good and threatening to release them on the market, if the species became extinct. This would eliminate the extinction equilibrium by depressing the price in the event of extinction, making poaching less attractive now.

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I won't go through all of De Alessi's points, but will focus on his two main arguments. First, he argues our analysis rests upon four assumptions and that these assumptions are incorrect. Second, he argues that private conservation initiatives are worthwhile (De Alessi 2004).

De Alessi's statements about our assumptions are incorrect. First, he states that we assume that trade in ivory is legal and that in reality the market value of ivory is indeterminable, since trade is illegal. Saying that ivory does not have a market value because it is illegal is equivalent to saying that marijuana does not have a market value because it is illegal. Illegal goods still have markets and prices. It is odd that De Alessi at one point says "prohibitions have never really worked" and then criticizes us for assuming trade in ivory exists. One point of our article is that if stockpiles could be sold by governments, if elephants became extinct, this might help keep prices down now. Given that we are arguing for less regulation of international commerce, it seems odd to object on grounds that this would not be consistent with CITES—selling stockpiles if elephants became extinct would not be illegal, and even if it were, existing regulations could be changed.

Second, he argues we assume habitat is constant. Our qualitative results would go through as long as some open-access land remains. Models abstract from reality—but this assumption is not critical to our results. Our results are relevant to species for which poaching contributes to population decline and, as discussed in our paper, that is true for many species.

Third, De Alessi writes that we assume that all elephants must exist in an open-access state. In fact, we explicitly note that under appropriate circumstances elephants will be protected as private property.

Fourth, De Alessi says we assume that state intervention is the only viable approach to conservation. In fact, as noted above, we explicitly say otherwise, albeit not in De Alessi's ideological language. Typical of De Alessi's rhetorical style here is his criticism of us for discussing "credible" government policies, although a central point of our original article was that government claims are often not credible.

The remainder of De Alessi's article is devoted to extolling private conservation initiatives and criticizing governments. There is a large literature on private initiatives, which De Alessi cites. I am sure many are worthy. Had I been writing a report to a foundation on how to protect elephants, I would have discussed these programs. But the purpose of a journal article is to contribute new knowledge, not to review existing knowledge. Existing models assume that ivory prices are not influenced by

expectations about the future. That is surely wrong. The purpose of our article was to examine the implications of correcting that mistake.

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Reply to Kremer

MICHAEL DE ALESSI*

MICHAEL KREMER RAISES SOME REASONABLE POINTS IN HIS response, but fails to address my main critique, which is that despite offering a development of the Clark extinction model, Kremer and Morcom's analysis tells us little or nothing about the real world. Fair enough for an intellectual exercise, but the specific policy prescriptions of the article would only exacerbate the problem for endangered species worldwide. This kind of disconnect underscores the gap between the positive influence that economics *could* have on important issues such as the conservation of natural resources, and the abstractions that seem to have gripped the profession and torn it away from pragmatic analysis.

Storability certainly affects extinction pressure when poaching is the greatest threat to species survival. But poaching pressure pales in comparison to habitat loss for species ranging from African elephants and rhinos, to tigers in India, to sea turtles in the Caribbean, all of which are prized for various 'storable' parts. The World Conservation Union (IUCN), an internationally respected consortium of governments and conservation organizations, has identified habitat loss as the primary threat to 85 percent of all species on the IUCN Red List of threatened and endangered species worldwide (IUCN 2003).

Of course, illegal wildlife trade also poses a threat, but academic economists should know better than to propose saving species by *devaluing* them—which is exactly what Kremer and Morcom have done. In the case of elephants, even reducing their commercial value to zero will not save them, as the poor in the developing world will simply not tolerate such a

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nuisance unless elephants have some redeeming value. Making open-access elephants less valuable is counterproductive, and some form of secure tenure, whether private, communal, or something in-between, is the only way to change the incentives for depletion that underlie the tragedy of the commons.

Hence my objection to the assumption of open access, a defining criteria for the eventual decline of species, and certainly a far better indicator of extinction pressure than storability. Kremer and Morcom mention the possibility of private property arising in some instances, but only in passing, and say nothing about the effect of a devaluation program on the elephants that exist outside of state-controlled areas, where less-valued elephants would surely be less well protected.

Kremer is right, of course, that despite an international trade ban, there is still a market for ivory. But CITES is predicated on restricting trade, and it is hard to imagine any official government strategy of dumping stockpiled ivory into the marketplace as long as CITES remains in place. Kremer maintains that the article is “arguing for less regulation of international commerce,” but that was not clear to me (even after the fourth reading). An institutional examination of whether CITES has been, or will be, good or bad for endangered species would be a far more promising way to “contribute new knowledge” to the substantive issue.

Economics has the potential to offer tremendous value to conservation efforts worldwide, but not by further refining ways to devalue whatever it is we want to see protected. Enough mischief has been caused by this approach already, and economists, more than anyone, should be leading the movement to recognize the importance of the definition of property rights, and to substantiate how different management institutions affect the conservation of natural resources.

It is tempting to conjecture that the distance between reality and Kremer and Morcom’s deductive reasoning is driven by an endemic and institutionalized practice of passing models off as empirically relevant without holding up one’s assumptions to anything like the real state of the world—thus, providing no basis for any policy implications. I am reminded of Barbara Wootton’s remark: “the economists feed on their own tails by busying themselves with the analysis of imaginary worlds which they have themselves invented” (1938, 35).

Finally, Professor Kremer disparages me for “ideological language,” without saying just what he means by that. Webster’s defines ‘ideological’ as “of or concerned with ideas,” which seems to be exactly the point of this exercise. I suspect he intended a different, more pejorative meaning, such as

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‘arrogant but unproven’ or ‘knee-jerk’, or perhaps simply a viewpoint different from his own. Regardless, my use of theory and evidence to show that Kremer and Morcom’s models are not applicable to the realities of elephant conservation in Africa hardly fits the pejorative definition. Professor Kremer’s remark about “ideological language” is not a legitimate criticism of my Comment.

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DO ECONOMISTS REACH A CONCLUSION?



Postal Reform

RICK GEDDES*

DO VITAL ECONOMISTS REACH A POLICY CONCLUSION ON POSTAL REFORM?

[ABSTRACT, KEYWORDS, JEL CODES](#)

THE UNITED STATES POSTAL SERVICE IS THE OLDEST AND LARGEST public enterprise in America. Officially called the Post Office for almost 200 years, it facilitated communications between the colonies and carried messages to the nation's farthest reaches. Since the Postal Reorganization Act of 1970 it has distributed mail as the U.S. Postal Service (USPS).

USPS is massive by almost any standard. In 2002 it earned revenues of over \$67 billion and processed over 203 billion pieces of mail (U.S. Postal Service 2002). It is also important internationally and handles about 40 percent of the world's mail (Universal Postal Union 1993). The Postal Service also plays an important civic role, since it is the only federal agency with which a majority of Americans interact on a daily basis.

The Postal Service's total employment in 2002 was 854,000, which is larger than the population of Delaware. It is also over 50 percent more people than General Motors, the largest company in the world in terms of sales, employed in that year. By comparison, in 1999 there were approximately

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923,000 lawyers, and 837,000 automobile mechanics, in the United States (U.S. Department of Commerce 2000).

Although it has made progress recently in cutting costs, the Postal Service appears to be undergoing a slow-motion train wreck. Its revenue base is eroding. In 2002, for the first time in recent history, the number of pieces of first-class mail delivered actually declined, by 1.28 billion pieces or 1.23 percent—the largest recorded percentage decline. First-class mail constitutes over 57 percent of the Postal Service's total revenue from mail. The decline in demand caused the Postal Service's revenue from first-class mail to fall by \$607 million—1.7 percent.

The Postal Service's other big mail class is standard mail—mostly advertising items—accounting for almost 25 percent of revenues from mail delivery. The number of standard mail pieces delivered in 2002 declined even more precipitously than first class, by 3 percent, while revenues from standard mail increased slightly due to rate hikes.

Other mail classes also showed declines in 2002. The number of priority mail pieces declined by 10.7 percent, express mail pieces declined by 8.6 percent, and the number of periodicals mailed declined by 1.8 percent. The number of packages mailed declined by 1.64 percent, and international airmail pieces declined by 15.4 percent. Unsurprisingly, the Postal Service's financial condition has suffered as a result. Its net loss in 2002 was \$700 million, following a \$1.7 billion loss in 2001.

There is a clear reason for this decline in demand for mail delivery. In the 34 years since the 1970 Postal Reorganization Act was implemented a momentous transformation has taken place in the communications marketplace. Through electronic mail, people can send written messages instantaneously anywhere in the world at low cost. New technologies, such as direct broadcast satellite (DBS) allow users to receive those messages without accessing their computers. The wide availability of facsimile machines and cellular phones, as well as lower long-distance telephone rates, have also played their part. Innovations in communications technology are likely to continue apace, causing further declines in the demand for physical mail delivery.

Additional rate increases will only encourage further substitution into communications alternatives. Nor is enhanced commercial freedom for the Postal Service, in its current form, a viable alternative. That would only encourage it to compensate for revenue shortfalls by unfairly competing in its non-monopolized services with private firms that do not enjoy the Postal Service's wide variety of privileges and immunities.

Barring a return to large annual deficits, and the accompanying direct taxpayer subsidies, the USPS is unlikely to remain viable for long in its present form. Meaningful structural change is necessary. The General Accounting Office has recognized this and issued a report to the Senate Government Affairs Committee stating that the “basic business framework of the Postal Service doesn’t look like it will work in the future” (quoted in Chen 2002).

WHAT HAVE VITAL ECONOMISTS SAID ABOUT POSTAL REFORM?

I define a vital economist as one who has produced scholarly research on postal services *and* who has expressed an opinion on the direction that reform should take. It is surprising how few economists have carefully studied this important institution, i.e., how small the pool of vital economists is. For example, no chapters of either the *Handbook of Public Economics* or the *Handbook of Public Finance* deal directly with the Postal Service, and very few articles in academic journals examine postal issues.

It is difficult to know why this important institution has received so little careful analysis from the economics profession, but the substantial amount of institutional detail required to understand postal issues may represent a substantial fixed cost. Moreover, that detail is highly idiosyncratic; it cannot easily be cross-applied to other firms or industries.

I now review the work of ten vital postal economists. Given the limited number of economists who have studied postal services, this represents a relatively broad survey of views on postal reform. These vital economists represent a variety of institutions, including government, business schools, economics departments, and public policy institutes. Academic institutions represented include Northwestern Business School, Ohio University, Rutgers Business School, the Wharton School, Yale Law School, and Yale School of Management. I first provide a biographical sketch for each vital economist and then summarize their views on postal reform.

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DOUGLAS K. ADIE

Douglas K. Adie is a professor of economics at Ohio University in Athens, Ohio, where he has taught since 1968. He holds a Ph.D. in economics from the University of Chicago. He has published numerous books and articles in professional journals on postal economics, minimum wages, privatization, and monetary history.

After a lengthy discussion of the social costs of the delivery monopoly, as well as the benefits of de-monopolization in other industries, Professor Adie recommends both repeal of the delivery monopoly and privatization of the Postal Service.

It will take positive constructive policy to turn this situation around and give the Postal Service a chance. Instead of waiting for the situation to worsen, events to overtake us, and much folly to be perpetrated, Congress should take a series of actions that will lead to the repeal of the private express statutes, divestiture, and privatization of the postal service (Adie 1989, 157).

He then discusses in detail how privatization and competition should be implemented. This vital economist is clearly in favor of substantial reform of postal markets in the United States, including both de-monopolization and privatization.

ROBERT H. COHEN

Robert H. Cohen has been the Director of the Office of Rates, Analysis, and Planning of the Postal Rate Commission since 1978. He has worked and written on economic issues related to postal rates for decades. He has written extensively on a variety of postal policy issues (as evidenced by the number of works cited in the references), but has focused on several issues of crucial importance to postal reform. He holds B.A. degrees in philosophy and mathematics from the University of Michigan and an M.A. in philosophy from the City University of New York.

The main justification given for the continuation of the postal monopoly is the preservation of a cross-subsidy from urban to rural customers created by a uniform rate. The size and cost of that cross-subsidy are crucial because they must be set against the costs of a legally enforced monopoly.

Mr. Cohen and his research team were the first to examine those issues empirically.

Strikingly, Mr. Cohen's research has revealed that *there is no significant cross-subsidy from urban to rural postal customers*—largely because of differences in the quality of mail service in rural versus urban areas. The rural customer must provide what is called “the last mile” of delivery service by picking up his or her mail from a cluster box, or from the end of a driveway, while urban customers often receive mail directly at their doors. The implications of this finding are far-reaching. The most concise statement of its policy implications were presented in Mr. Cohen's February 20, 2003 testimony before the President's Commission on the Postal Service. Mr. Cohen begins his testimony by stating that:

My colleagues from the technical staff of the Postal Rate Commission (PRC) and I have been studying these topics and related matters for more than a decade. Our conclusions cast grave doubt on much of what passes for conventional wisdom in discussions of universal service and the monopoly (Cohen 2003, 1).

The testimony also summarized the universal service argument for monopoly and should be quoted in detail (emphasis as in original):

The conventional view is that a monopoly is necessary to preserve universal service. Proponents of this position reason that a monopoly is required to sustain a cross-subsidy from profitable operations in urban areas to money-losing services in rural areas. If profits earned in urban areas are not protected by the monopoly, “cream skimmers” will undercut uniform prices and capture so much urban volume that the Postal Service will be left unable to afford delivery to rural areas and universal service would be lost. Moreover, the thinking continues, without the monopoly, the nation would lose the benefits of scale economies in delivery operations where fixed costs are high.

Although this economic rationale for the postal monopoly is widely accepted, our findings indicate that it is fundamentally mistaken. In brief, our major findings are as follows:

The cost of universal service is a surprisingly small portion of the Postal Service's \$70 billion budget. In 1999, losses on unprofitable routes were \$2.6 billion; about half of the losses were sustained on just ten percent of the routes. The cost of the 10,000 smallest post offices (out of a total of 28,000) was \$567 million. Six-day-a-week delivery is also frequently cited as a universal service requirement. An upper bound on the savings from eliminating a delivery day is \$1.9 billion (the daily fixed cost of residential delivery).

There is no urban to rural cross-subsidy. Analyses of revenues and costs by route show that routes serving rural areas are, in total, quite profitable. Overall, because the Postal Service is required to break even (i.e., earn no net profit), a large number of routes are necessarily unprofitable. However, the proportion of unprofitable routes in the U.S. is approximately the same for urban and rural areas. Volume, not population density or urban character, is the major determinant of profits on delivery routes in the U.S. . . .

The monopoly is not necessary to preserve universal service. An analysis of the competitive upstream market shows that only 16 percent of the mail would be susceptible to diversion for delivery by competitors of the Postal Service. Thus, for the foreseeable future, it would be difficult for competitors to accumulate sufficient volume to achieve unit costs below those of the U.S. Postal Service. The experience of countries that have abolished their monopolies confirms this finding. Moreover, posts in those countries have had very large cost reductions as a result of liberalization.

The costs of the monopoly exceed its benefits. In 1993, the Postal Service estimated its wage premium to be \$9 billion (i.e., the total amount by which postal wages exceeded wages for comparable jobs in the private sector). We calculated the scale benefits of having a single provider, as opposed to a duopoly, for delivery to be \$6 billion (Cohen 2003, 1-2).

Perhaps the most important elements of those findings for postal policy are: (1) that rural routes are profitable, (2) that the delivery monopoly is not necessary to preserve universal service, and (3) that the costs of the delivery monopoly exceed its benefits. This vital economist's conclusions support liberalization of postal market in the United States.¹

MICHAEL A. CREW AND PAUL R. KLEINDORFER

Michael A. Crew is Professor of Economics and Director of the Center for Research in Regulated Industries, Rutgers University School of Business. Paul R. Kleindorfer is the Anheuser-Busch Professor of Management Science and Professor of Decision Sciences, Economics, and Business at the University of Pennsylvania's Wharton School. As indicated by their contributions in the reference list, Professors Crew and Kleindorfer have published a large number of books and articles on postal issues.² They have also been instrumental in raising the level of discourse on postal reform by organizing a series of international conferences on specialized postal topics.

Because Professors Crew and Kleindorfer have conducted much of their postal work jointly, it is useful to consider their views together. Professors Crew and Kleindorfer have concisely expressed their views on postal reform in a 2000 book chapter entitled, "Privatizing the U.S. Postal Service." They begin by noting the lack of reasons for government ownership of postal services.

There are no strong technological, strategic, or economic reasons why postal service should be publicly operated. While it may be difficult to make a case for privatizing the armed services, there are no such strategic considerations with postal service. Postal service is a network industry. Other network industries – for example, electricity, gas, and telecommunications – are privately owned and operated. Postal service is arguably less important than any of the

¹ For additional background on Cohen's perspective see: Cohen and Chu (1997), Cohen, Ferguson, and Xenakis (1993), Cohen et al. (1999), Cohen et al. (2000), Cohen, Pace, et al. (2002), Cohen, Ferguson, et al. (2002), and Cohen et al. (2003).

² See, for example: Crew and Kleindorfer (1992), Crew and Kleindorfer (1993), and Crew and Kleindorfer (1994).

other network industries. It would be much more painful if the lights were out for half a day than if postal service ceased for an extended period (Crew and Kleindorfer 2000, 161).

They then move on to enumerate the benefits of privatization.

Absent privatization, there is no strong residual claimant to ensure a proper allocation of scarce resources. . . . The implications for the USPS are clear. Absent privatization or a schedule to privatize, the benefits to be expected from incentive regulation are likely to be reduced significantly.

Similarly, in the absence of privatization and residual claimants there is little incentive on the part of postal management to address the issue of the current labor relations framework within which the USPS operates. Unless there is a change in labor practices, the improvements in efficiency are likely to be small or nonexistent . . . (Crew and Kleindorfer 2000, 155-6).

We could go on at length as to the potential benefits from privatization of USPS and the creation of USPSI (U.S. Postal Service, Inc.). However, we recognize that privatization of the USPS, as in the USPSI, is no more than a dream at this stage. That does not reduce our belief that it should happen. Whether it will happen is a political matter . . .

We conclude that the case for privatization is strong. Although we recognize that a powerful case does not imply political feasibility we also know that political decisions arise from ideas. Privatization of the Postal Service is an idea whose time is overdue (Crew and Kleindorfer 2000, 152-3).

These two highly regarded postal researchers are clearly in favor of postal reform through privatization.

GEORGE L. PRIEST

George L. Priest is the John M. Olin Professor of Law and Economics at Yale Law School. His areas of legal research include antitrust, regulated industries, products liability, insurance, and civil procedure. Although he teaches in a law school, much of his research addresses economic questions. He authored an often-cited article on the history of the postal monopoly in the *Journal of Law and Economics* and has written a number of essays on the Postal Service (Priest 1975). Writing in 1994, in reference to his famous 1975 article, Professor Priest stated:

After a lengthy description of the colorful history of the Post Office, that article ended with what I regarded as a devastating critique of the postal monopoly as a mechanism of economic organization, revealing the monopoly's great failings and proving that no rational person could support its continued existence . . . (Priest 1994, 46).

The Postal Service remains today as the most significant example of socialism in the United States. We know from theory and the American public knows from the success of Federal Express, Purolator, and other competitors that there is no inherent reason for a government monopoly of the delivery of written communication. My article showed that the historical reasons for the survival of the monopoly were not strong (Priest 1994, 50).

Noting that standard economic arguments against the postal monopoly have been unsuccessful in bringing about reform, Professor Priest goes on to provide a spirited argument for liberalization of postal services.

In my view, to reform the Postal Service, it will be necessary to move beyond the calculation of subsidies, beyond the mechanics of efficiency, and even beyond the analysis of organizational form. However important these concepts and the values underlying them may be to economists, public policy regarding postal operations, just as public policy regarding the great revolutions against socialism in Eastern Europe and around the world, is motivated by a set of values

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related to the political commitment to freedom and democracy . . . (Priest 1994, 56).

The strongest argument in favor of elimination of the monopoly and of privatization of the Postal Service is that the citizenry and thus democracy in America can be made better off by freeing the forces of innovation and experimentation to empower the discovery of new methods of delivery that advance communication... (Priest 1994, 58).

It is clear in my view, thus, that the most fervent defenders of the role of postal delivery in promoting democracy must be the critics of the monopoly, not the reverse. Supporters of the monopoly and of the socialized features of Postal Service operations must be portrayed – as I believe they are in fact – as the enemies of true democracy who seek, through advocacy of the principle of universality, to tax the communication of all of us, stifling innovation and experimentation and burdening the communication of the citizenry to subsidize particular mail classes or high-cost routes (Priest 1994, 58).

It is clear that Professor Priest strongly favors liberalization of postal services.

PETER J. FERRARA

Peter J. Ferrara is General Counsel and Chief Economist at Americans for Tax Reform. He holds a bachelor's degree in economics and a law degree from Harvard University. He was associate deputy attorney general from 1992 to 1993. He has written extensively on economic issues, including Medicare and Social Security reform.

He has also written on postal reform, and has expressed his views on the direction he thinks reform should take. In his 1990 book on the Postal Service, Mr. Ferrara stated that:

The U.S. Postal Service is a monopoly by government decree. That is the problem. The law prohibits any other

firm or individual from delivering letters or other first class mail . . .

In a society that values freedom, the prohibition on private mail delivery should seem not only anomalous but authoritarian. The decision of one individual to hire another to deliver his or her mail hardly seems to pose the kind of threat to others that should be punishable as a crime. The prohibition of private mail service deprives consumers of the freedom to choose who will deliver their mail, and it deprives entrepreneurs and their employees of the freedom to pursue economic opportunities. The policy consequently adds up to a substantial restriction on economic freedom, which in itself should be good enough reason for repeal (Ferrara 1990, 1).

Mr. Ferrara then goes on to advocate repeal of the postal monopoly and privatization of the Postal Service, and is, thus, clearly in favor of postal liberalization.

SHARON M. OSTER

Sharon M. Oster is the Frederic D. Wolfe Professor of Management and Entrepreneurship at the Yale School of Management. Professor Oster has written extensively on non-profit enterprise and has studied the U.S. Postal Service.

Professor Oster concisely presented her views on postal reform in a 1995 book chapter. In a section entitled, "The Eroding Basis for Postal Regulation," Professor Oster states:

A critical look at the legislative history of the postal monopoly suggests that economics played very little role in stimulating government intervention in this market; the principal force behind the granting of the monopoly was the political desire for universal service as a way to support the growth of the democratic state (Priest 1975). . . . But, however legitimate or not this motivation for market interference was in the 18th century, it is difficult to take this argument seriously in the modern period. In the current

period, Americans are surely more linked together by electronics than by mail and few would argue that democracy rests on as thin a reed as a universally priced stamp or universal home delivery.

A more modern basis for the monopoly involves scale economies. Here, too, the story is familiar to students of the other regulated sectors. To the extent that scale economies in mail are substantial, granting a postal monopoly and then subjecting it to regulatory oversight could well be in the public interest. Here, too, however, developments both in technology and in our broader understanding of the possibilities of economic engineering have reduced the salience of this story . . . In sum, the sands on which the postal monopoly rests are rapidly eroding (Oster 1995, 110).

Professor Oster goes on to examine the current organizational structure of the Postal Service, as created by the 1970 Postal Reorganization Act, and states that, "In sum, the 1970 Act replaced the overly-meddlesome, highly politicized oversight of the postal organization by Congress with oversight by a board which is under almost no control at all, coupled with sporadic Congressional inquiry when particular interests are threatened!"

She concludes her essay by suggesting some policy changes. She first advocates privatization of the Postal Service.

The advantages of moving in the direction of privatizing the postal service seem clear. Indeed, substantial improvement seems to be impossible within the current institutional structure. However well meaning reform efforts might be, the current structure of the Postal Service provides too few managerial incentives for any real re-engineering of the organization to occur (Oster 1995, 118).

Professor Oster goes further, however, to advocate horizontal dismantling of the Postal Service in addition to privatization.

The localized nature of postal scale economies suggests an additional avenue for reform: breaking up the postal monopoly horizontally, creating locally distinct – and locally

managed operations – to replace the cumbersome, nationwide system. Rather than having a national, U.S. Postal Service, in which central bureaucrats try to manage a vast network of local operations, the postal operations would be divided into local operations, perhaps organized around the regional bulk collection operations (Oster 1995, 118-9).

Sharon Oster, a vital economist on this issue, is an advocate of substantial reform of postal services.

JOHN C. PANZAR

John C. Panzar is the Louis W. Menk Professor of Economics at Northwestern University. He was head of the Economic Analysis Research Department at Bell Telephone Laboratories and is Associate Editor of the *Journal of Regulatory Economics*. He has written extensively on postal issues, and has expressed his views on how reform should proceed. Professor Panzar summarizes his policy views in a section of a book chapter entitled, “Should the Delivery Monopoly Continue?”

There remains the treatment of the local delivery monopoly itself. Even if natural-monopoly cost characteristics are present, there is no guarantee that the Postal Service is cost efficient. Removal of statutory entry barriers would subject it to a market test. Such a policy, however, has its risks. Even if the Postal Service were producing efficiently, there might still be profit opportunities for inefficient, cream-skimming entrants. In addition, the social objectives of universal service at uniform rates may require restriction on entry. . . .

Of course, a policy of free and open entry may be the only way to push the system to efficiency. Given the risks involved, however, it seems more prudent to reserve this policy as a last resort, to be implemented only after efficient pricing policies and franchise contracting have been fully exploited. These policy tools can provide powerful incentives for the creation of a cost-efficient network (Panzar 1994, 6).

Professor Panzar is somewhat more conservative in his policy prescriptions than other commentators, in that he advocates attempts at novel pricing policies, such as efficient discounts, before the monopoly is repealed. He also advocates franchise contracting as an alternative, which is a way to introduce competition while maintaining more bureaucratic control. Notably, Professor Panzar accepts the notion that the current system of postal regulation can be improved upon.

J. GREGORY SIDAK AND DANIEL F. SPULBER

J. Gregory Sidak is the F. K. Weyerhaeuser Fellow in Law and Economics emeritus at the American Enterprise Institute and is a senior lecturer at the Yale School of Management. He holds a bachelors and a masters degree in economics, as well as a law degree, from Stanford University. Daniel F. Spulber, an economics Ph.D., is the Thomas G. Ayers Professor of Energy Resource Management and professor of management strategy at the J. L. Kellogg Graduate School of Management at Northwestern University. They have both written extensively on regulatory issues and have expressed their views about the proper direction for postal reform.

They conclude their book, *Protecting Competition from the Postal Monopoly*, with a cogent policy prescription for postal reform. They are quite clear on the need to introduce more competition into mail delivery, but also specify that competition must be introduced before privatization.

The path to more competitive and innovative mail service in the United States is not to facilitate predatory cross-subsidization by a government-owned monopolist. In other words, the proper policy is not one of congressional acquiescence to the unconstrained diversification and corporate aggrandizement of the Postal Service.

Rather, the policy most conducive to greater economic welfare is one of commercialization of the Postal Service. Such a reform package would repeal the Private Express Statutes and other statutory privileges enjoyed by the Postal Service, explicitly subject the Postal Service to the antitrust laws and all other laws of general applicability to private businesses, and relieve the Postal Service of its incumbent burdens, including the duty to deliver at a uniform national

rate to high-cost areas. The Postal Rate Commission would oversee the transition to competition and then cease to exist. This set of reforms might eventually lead to the privatization of the Postal Service, though it need not. Indeed, privatization would be unconscionable on economic grounds if it failed to provide for repeal of the Private Express Statutes (Sidak and Spulber 1996, 162-3).

These two vital economists clearly favor postal liberalization through the introduction of competition and the removal of special privileges for the Postal Service.

There is, thus, a surprising consensus among these ten vital economists on the need for postal reform and the direction it should take. The majority of economists surveyed here advocate some combination of demonopolization and/or privatization. There are differences of opinion about the order and details of reform, but none about its expediency. Indeed, only one vital economist advocates a go-slow approach.

NON-VITAL ECONOMISTS ON THE POSTAL SERVICE

The contrast on this policy issue between the views of non-vital economists, i.e., those who have not studied postal services, and vital economists is striking. In particular, non-vital economists often assume that government ownership, and legally enforced monopoly, are necessary in this basic commercial activity. For example, in the 9th edition of *Economics* Paul Samuelson writes in a subsection entitled “Government Production” that:

The post office was long a function of government . . . The reasons for drawing the line at one place rather than another are partly historical and are to some degree changing; but, economically, the distinction is not completely arbitrary. Thus, the courts have held that, in the special case of ‘public utilities affected with public interest,’ there is limited possibility of effective competition among many independent producers, so they must be *publicly regulated or owned*; but one would not expect the production of soap or perfume to be a

natural candidate for governmental operation (Samuelson 1973, 153).

Samuelson, in contrast to vital economists, although vague, leaves the textbook reader with the impression that a government-run post office is necessary. Also, where he italicizes his core conclusion, it is ambiguous whether the conclusion is that of the courts' or his own. He is content to rely on the courts for economic judgment in either case.

Writing twenty years later, and with much more research to call upon, Joseph Stiglitz includes a "Close-Up" sidebar in his textbook, *Economics*. The complete sidebar, "Productivity in the U.S. Postal Service," follows.

Gripping about the shortcomings of the U.S. Postal Service is a great American tradition. But the anguished complaints every time the price of a first-class stamp goes up are not really about money. After all, even if you sent a letter every day, which is probably more than most of us do, an extra 5 cents per stamp adds up to only about \$18 in a year.

More likely, knocking the post office is part of a deep-rooted suspicion about the inefficiencies of government. If the Postal Service is controlled by the government, it must be inefficient, right? Well, the prejudice used to have some truth in it, but no longer.

About four-fifths of total Postal Service expenditures go to paying postal employees, so measuring the productivity of these employees is the real test of the Postal Service. In 1950, the Postal Service delivered 90,000 pieces of mail per employee. By 1960, it was 113,000 per employee; in 1970, 115,000; and in 1975, 127,000.

These figures help explain why people do not have much faith in the Postal Service. During the 1960s, the productivity of the average postal employee barely budged. Over the quarter century from 1950 to 1975, the productivity of the typical employee increased by 41 percent. By way of comparison, business sector productivity for the economy as a whole increased by 85 percent from 1950 to 1975. The

Postal Service spent twenty-five years building its reputation for backwardness and inefficiency.

But unfortunately for all the complainers who enjoy bashing the Postal Service, the productivity figures have now reversed themselves. By 1980, the post office was delivering 159,000 pieces of mail per employee. By the end of the 1980s, the average had risen to 198,000 per employee. From 1975 to the end of the 1980s, the letters delivered per postal employee increased by 56 percent, which is a substantially larger increase in fifteen years than had been managed in the preceding quarter century. Over that same time, business sector productivity increased by only 18.4 percent.

The post office has become an American leader in productivity gains. Clearly, if led and managed appropriately, a government agency is no barrier to dramatic gains in productivity. (Stiglitz 1993, 188)

There are several profound differences between the views of this non-specialist and those of the vital postal economists surveyed above. First, postal economists are aware that rate hikes are, at least partially, passed on to consumers receiving items shipped through the mail even if they don't pay the higher postage directly. Second, Stiglitz suggests that critics of current postal organization are grippers and complainers who enjoy bashing the USPS for its own sake. The qualifications and introspection reflected in the above comments of vital economists suggest that is not the case. Third, Stiglitz presents disturbingly superficial evidence on increasing postal productivity. For example, he measures only labor productivity (pieces per worker), rather than total factor productivity. Using broader measures, most analysts find increasing productivity right after the 1970 act and slowing postal productivity subsequently. Fourth, he provides no analysis of why such gains might have occurred at that particular time. Perhaps those gains were the result of increasing competition from electronic mail, telephones, fax machines, and alternative providers, which suggests that there are further gains from repealing the monopoly. Does Stiglitz want us to conclude that government-owned monopoly is likely to result in more rapid productivity gains than private business as a general principle? The broad economic history of the twentieth century does not

support that conclusion, and it stands in stark contrast to the dismal assessment of postal performance offered by vital economists.

CONCLUSION

There are surprisingly few vital economists who have expressed views on the important topic of postal reform. The views of the ten surveyed here are, thus, likely to be representative.

The policy prescriptions of economists surveyed here are similar. The vital economist suggesting the most limited reforms merely recommends trying novel pricing policies before repealing the postal monopoly. All others advocate some combination of rapid de-monopolization and privatization and differ only in the order that those reforms should take. None of the vital economists surveyed here suggest that postal reform is unnecessary. In contrast, economists who have not studied the Postal Service in detail seem content with its current institutional structure.

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DO ECONOMISTS REACH A CONCLUSION?



Drug Policy

MARK THORNTON*

PROHIBITION VS LEGALIZATION: DO ECONOMISTS REACH A CONCLUSION ON DRUG POLICY?

[ABSTRACT, KEYWORDS, JEL CODES](#)

THE POLICY OF PROHIBITING THE SALE AND CONSUMPTION OF cocaine, heroin, and marijuana is of great public interest, with much public debate about the effectiveness of the “war on drugs” and alternative policies such as legalization, decriminalization, drug treatment, and medical marijuana. Economists have been at the forefront of the debate, criticizing the effectiveness of the war on drugs, drawing attention to its “unintended consequences,” such as violent crime and corruption, and proposing alternative policies, such as drug legalization and decriminalization.

Milton Friedman (1972, 1980, 1984, and 1989) has long advocated the legalization of drugs. Gary Becker (1987, 2001), George Schultz (1989), Thomas Sowell (1989), and William Niskanen (1992) have also endorsed the liberalization approach. Milton Friedman and Gary Becker have both been awarded the Nobel Prize in economics, while George Schultz served in the Cabinet and William Niskanen served as the Chairman of the President’s Council of Economic Advisors, both in the Reagan administration.

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Given that these noteworthy economists are associated with conservative politics, it might seem that there is a bipartisan consensus on the direction of reform in drug policy.

Do these views represent the views of the profession? Are they indicative of those economists who are actively engaged in research on the topic of drug policy? Or, might they be a minority view? After all, the economists noted above are strongly associated with the Chicago School of economics and a policy agenda of economic liberalism. Furthermore, only one of the endorsements, Gary Becker's, comes from an economist whose primary research is even related to drug policy (addiction). Therefore, it is less than obvious that they are a reflection of the profession at large or those economists who research this issue.

In order to answer these questions I conducted two surveys of economists' policy views, one of the profession as a whole and the other a survey of economists who are actively engaged in drug policy research. The results of both surveys were then examined against demographics of the profession and public opinion polls on drug policy.

ECONOMISTS ARE PEOPLE TOO

In 1995, I surveyed 117 randomly selected professional economists based on the membership of the American Economic Association.¹ Of those who offered an opinion, 58% were in favor of changing public policy in the general direction of decriminalization. When asked to choose from among five policy options, only 16% of economists favored complete legalization. Among the economists who gave a response *other than keeping the status quo*, 71% favored either legalization or decriminalization. Less than 2% endorsed measures stronger than longer prison sentences and increased enforcement budgets. It is clear from the survey that in 1995 a majority of economists, though not a strong consensus, favored changes in public policy in the direction of decriminalization.²

¹ These results were initially reported in Thornton (1995, 73). Subjects were randomly selected from the 1993 Biographical Listing of Members of the American Economics Association. One subject was selected from alternating pages of the directory, contacted by phone and surveyed.

² Above average support for decriminalization is prevalent among economists specializing in monetary theory, public finance, and labor economics. Business economists were the

Table 1: SURVEY OF ECONOMISTS ON DRUG POLICY

Do you favor the decriminalization of illegal drugs?	YES = 61 (52%)	NO = 45 (38%)	NO OPINION = 11 (9%)
What is your preferred policy choice to deal with illegal drugs?	Legalize = 19 (16%) Decriminalize = 45 (38%) Total = 64 (55%)	Status Quo = 18 (15%) Increased efforts = 24 (21%) Total = 42 (36%)	NO OPINION = 9 (8%)

A 1991 opinion poll of Americans showed that 36% favored legalization or controlled distribution of most drugs. The poll, sponsored by the pro-liberalization Drug Policy Foundation, also found that 40% felt decriminalization of cocaine would reduce violent crime and that addiction treatment and counseling for drug users was preferred by more than 3 to 1 over fines and/or imprisonment.

These results contrast with a survey of college freshmen in 1988, which found that 19.3% favored the legalization of marijuana. A survey of the high school class of 1987 found that 15.4% favored legalization, 24.6% favored decriminalization, and 45.3% felt it should remain illegal. Approximately 80% favored the prohibition of L.S.D. and heroin. These surveys show a precipitous decline in support for legalization of marijuana from 1977-1989 to 16%. But then support began to increase among high

strongest supporters of prohibition. Among non-academic economists, those working for private institutions were more likely to support decriminalization, while those working in the public sector were more likely to support the status quo, or increased enforcement. Age and rank appear to be largely unrelated to policy preferences. The evidence also suggest that economists trained in the Chicago, Public Choice, and Austrian traditions are more likely to support legalization, so ideology and/or training may have a strong influence on policy views.

school students after 1989 and had doubled to 32% in 2000 (36.5% of college freshmen in 2001) with nearly 50% supporting marijuana decriminalization and 73% supporting medical marijuana. Also, majorities favor treating drug use as a disease and believe that too many people are put in jail for drug use (Maguire and Pastore 2001).

An examination of responses relative to demographic characteristics of the general population is revealing. Prohibitionists are more likely to be female, older, from the south, blue collar, low income, Protestant, high school drop outs, and Democrat. Supporters of legalization in the general population are more likely to be male, younger, from the north and west, professional, highest income category, Jewish or nonreligious, college graduate or more, and independent in politics. In recent years these demographic distinctions have become less dramatic than they were ten or fifteen years ago.

Table 2: DEMOGRAPHIC CHARACTERISTICS AND DRUG LIBERALIZATION

Reformers	Characteristic	Prohibitionist
Male	Sex	Female
White	Race	Non-White
College or more	Education	Grade school
Professional/farmer	Occupation	Clerical/manual
High and Middle	Income level	Low
Young and Middle	Age	50 years or more
Northeast & West	Region	South
Jewish or none	Religion	Protestant
Independent	Politics	Democrat and Republican

Given that the members of the economics profession tend, relative to the general population, to be more closely match the characteristics of the reformers in the general population, it is unclear that being an economist *per*

se has much impact on the choice between prohibition and legalization. This is especially true given the wide confidence interval of the survey (+/- 9%). Brian Caplan has found that being male, well educated, and having rising income is associated with the tendency to think like an economist and to generally favor liberal economic policies (Caplan 2001).

Another reason to doubt that economists have a solid and strong commitment to the liberalization of drug policy and aversion to prohibition is that this has not always been the case. Economists Gordon Tullock and Richard B. McKenzie suggested that economists have always opposed prohibition.

In the early part of this century, many well-intentioned Americans objected to the consumption of alcoholic beverages. They succeeded in getting the Constitution amended to prohibit the sale of alcohol. By the 1930s most of them had given up because they discovered how difficult it was to enforce the law. If they had consulted economists, I'm sure they would have been told that the law would be very difficult and expensive to enforce. With this advice they might have decided not to undertake the program of moral elevation. The same considerations should, of course, be taken into account now with respect to other drugs (Tullock and McKenzie 1985, 7).

However, one of the leading economists of the day, Irving Fisher, was an outspoken proponent of alcohol prohibition, writing three books in support of the policy. As late as 1927, Fisher could claim that he was unable to find a single economist to speak against prohibition at a meeting of the American Economic Association (Thornton 1991a). At the end of the 1930s, Fisher remained solidly behind the policy of severe alcohol prohibition.

Summing up, it may be said that Prohibition has already accomplished incalculable good, hygienically, economically and socially. Real personal liberty, the liberty to give and enjoy the full use of our faculties, is increased by Prohibition. All that the wets can possibly accomplish is laxity of enforcement or nullification; in other words, enormously to increase the very disrespect for law which they profess to deplore. Hence the only satisfactory

solution lies in fuller enforcement of the existing law
(Fisher 1930, 454-5).

Clearly, economists were not always supporters of liberal drug policies. But they have come a long way since Prohibition in the 1920s.

VITAL ECONOMISTS ON DRUGS

“Vital economists” can be defined as those economists who publish directly on a particular policy, come to embrace policy-reform positions that are more than vague generalities, and plainly express their judgments about desirable reform (which would include support for retaining the status quo policies). Admittedly, these criteria for “vitalness” are somewhat fuzzy. For example, consider the first requirement, that the economist must be published on the policy topic. Does an economist have to have published topically *in academic or scholarly literature*? Or does a newspaper op-ed devoted to the topic qualify him as having published on the topic? The reader can decide this matter for himself. To cover the bases, I have included both here among the vital economists on the issue.

Another demarcation line concerns who is, and who is not, an economist. I count as an economist anyone who has a economics graduate degree (such as a Master’s or PhD) or anyone employed in a college or university economics department.

Below is a list of postwar American economists’ quotations about drug policy. In some cases they express clear recommendations about how drug policy should be reformed, in some cases their remarks are less clear, but seem to suggest one view rather than another. This is not a systematic survey, but is simply the writings that I dug up or have come across, and it certainly is not exhaustive. *I have been scrupulous in looking out for anti-liberalization judgments, and in including any judgments that tend toward that position.* (Dear reader, please alert me to other items that I should include! Send to mthornton@prodigy.net.)

Based on a search of EconLit, I sent email requests to economists whose work on drug policy I was not familiar with, mostly from foreign countries, but received only two replies (less than 10%), and both indicated that they did not have a position on drug policy. As a result, this survey is based on the conclusions of American economists.

The set of judgments do not show a clear consensus on what exactly is to be done. But there does seem to be broad—not perfect—consensus on three general matters. First, most economists found the current policy to be somewhat ineffective, very ineffective, or harmful. Second, most economists agree that the current policy should be changed. Third, most economists agree that the policy should be changed in the general direction of liberalization. Disagreement is generally based on the direction and degree of liberalization. Thus, we find suggestions for political decentralization (which would allow for experimentation and differentiation), downsizing of the drug war, decriminalization, reallocation from criminal prosecution to treatment (or more broadly from supply-side to demand-side policies), to qualified or limited legalization, sin taxes, and outright legalization, to my own policy recommendation that I have dubbed “perfect legalization” (Thornton 1998).

ROBERT J. BARRO: LEGALIZATION AND SIN TAXES

The experience with drug enforcement shows that prohibitions of recreational drugs drive up prices, stimulate illegal activity, have only a moderate negative effect on consumption, and impose unacceptable costs in terms of high crime, expansion of prison populations, and deterioration of relations with the foreign countries that supply the outlawed products. A better idea would be to leave intact the existing regulatory structure for cigarettes—which includes substantial but not outrageous tax rates and restrictions on sales to minors—and apply this apparatus to the currently illegal drugs (Barro 1997, 143).

GARY S. BECKER: LEGALIZE AND TAX

Legalizing drugs is far from a panacea for all the distress caused by drugs, but it will eliminate most of the profit and corruption from the drug trade. Ending Prohibition almost immediately cleaned up the liquor industry. To be sure, legalization will increase drug use by, among other things, lowering street prices, but that can be partially offset

through sizable excise taxes on producers. In many nations, retail prices of cigarettes, alcohol, and gasoline are several hundred percent higher than their wholesale prices because of large “sin” taxes on them. The revenue collected from large taxes on drugs could be used to treat addicts and educate youngsters about the harmful effects of many drugs (Becker 2001).

DANIEL K. BENJAMIN AND ROGER LEROY MILLER: CONSTITUTIONAL APPROACH

Our proposal—the Constitutional Alternative—is that the power to control the manufacture, distribution, and consumption of all psychoactives revert to the states, under provisions identical to those of the Twenty-first Amendment. As with repeal of Prohibition, the Constitutional Alternative would repeal only the federal prohibition of psychoactives. As was true with the repeal of Prohibition, the Constitutional Alternative would return to the states the powers that they held from the inception of the nation; thus, the states would regain full powers to control the manufacture, distribution, and consumption of psychoactives within their borders (Benjamin and Miller 1991, 194).

WALTER BLOCK: LEGALIZATION

This paper argues the case for legalizing drugs such as marijuana, cocaine and heroin. It claims there are no market failures that justify prohibiting of these opiates, and there is nothing in positive economics that precludes legalizing drugs. On the contrary, a free market in marijuana and other drugs enhances economic welfare (Block 1996, 433).

MARY M. CLEVELAND: “DOWNSIZING” DRUG PROHIBITION

Cleveland (1998, 573) states that she is a critic of drug prohibition. She concludes, “Policies that stigmatize and imprison drug users may hurt rather than help troubled young people and problem users.”

The abstractions of “prohibition” or “legalization” have little to do with the behavior or needs of troubled people. There must always be some policing of illegal drug markets, just as with bootleg liquor markets. But the drug war makes the black markets very dangerous, and therefore attractive to troubled young people with limited opportunities and a high risk of becoming problem users of hard drugs. It doesn’t cause the family and social problems that put young people at risk, but it does divert resources and attention from education and treatment programs that could help them. “Legalization” in any of its many possible variations cannot solve family and social problems either—any more than repeal of alcohol Prohibition solved the problems leading some individuals to become alcoholics. However, combined with a downsizing of the drug war, “legalization” can help restrict casual access to drugs while making it easier for problem users to find treatment (Cleveland 1998, 573).

WILLIAM DAVIS: COSTS OUTWEIGH BENEFITS

The government's current strategy, by measures of economic efficiency and equity, has been costly and its burden distributed unfairly. Taxpayers fund the explicit cost of drug control and the spillover costs have been borne by parties usually not associated with illegal drug activity. Current attempts to eradicate illegal drugs appear to create the very phenomena they are supposed to correct—spillover costs (Davis 1998, 176).

MILTON FRIEDMAN: FAVORS LEGALIZATION OR DECRIMINALIZATION

Legalizing drugs would simultaneously reduce the amount of crime and raise the quality of law enforcement. Can you conceive of any other measure that would accomplish so much to promote law and order? In drugs, as in other areas, persuasion and example are likely to be far more effective than the use of force to shape others in our image (Friedman 1972, 104).

Decriminalizing drugs is even more urgent now than in 1972, but we must recognize that the harm done in the interim cannot be wiped out, certainly not immediately. Postponing decriminalization will only make matters worse, and make the problem appear even more intractable (Friedman 1989, A14).

MICHAEL GROSSMAN, GARY S. BECKER, AND KEVIN M. MURPHY: ?

Clearly, we have not provided enough evidence to evaluate whether or not the use of heroin, cocaine, and other drugs should be legalized. A cost-benefit analysis of many effects is needed to decide between a regime in which drugs are legal and one in which they are not. What this paper shows is that the permanent reduction in price caused by legalization is likely to have a substantial positive effect on use, particularly among the poor and the young (Grossman, Becker, and Murphy 1991, 83).

JOEL W. HAY: STRONGER PROHIBITION

I do not have the answer to the drug-policy dilemma other than to keep moving ahead pretty much as we have been. I would focus substantially more effort, using both carrots and sticks, on discouraging demand. I agree with the critics that supply interdiction, by itself, is extremely expensive and ultimately futile. If we are going to make policy for this difficult and tragic problem with simplistic solutions

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that can be fit into 30-second TV sound bits, then I would definitely prefer a real drug war, with swift and certain punishment of casual drug users, to a drug-legalization surrender (Hay 1991, 219).

DAVID R. HENDERSON: LEGALIZATION

I oppose the drug war. I also advocate legalizing drugs whose sale and use is currently illegal. Although not problem-free, the case for legalization is much stronger than the case for criminalization (Hederdson 1991, 655).

Most of the problems that people think of as being caused by drugs are not caused by drugs per se. Rather, they are caused by drug laws (Henderson 1991, 675).

Further, the morally proper way to prevent drug use is to persuade people, not to imprison them (Henderson 1991, 675).

ROBERT HIGGS: LEGALIZATION

The Drug War is an ugly sight, too, and opposition is growing, especially among judges, who see its futility up close. It still awaits its equivalent of Richard Nixon and Henry Kissinger, who will declare "peace with honor" and bring the troops home. By abandoning this costly, quixotic crusade, the authorities could spend more time protecting life and property and relieve us of an obnoxious invasion of our natural rights, which include the right to decide how we use—or abuse—our own bodies (Higgs 1995, 36).

RANDALL G. HOLCOMBE: LEGALIZATION

An argument for legalization is that most of the harm caused by recreational drug use comes from the fact that drugs are illegal, not that they are drugs. This implies that

to minimize this harm completely free and open markets for drugs should be established (Holcombe 1995, 158).

MIREIA JOFRE-BONET AND JODY L. SINDELAR: INCREASE TREATMENT FOR ADDICTS

For our sample, we find that treatment reduces drug use. . . Reduced drug use due to treatment is associated with 54% fewer days of crime for profit, *ceteris paribus*. Our evidence suggests that, reduced drug use is causally related to reduced crime. This finding is robust to different specifications and sub-samples. Our findings broadly suggest that drug treatment may be an effective crime-fighting tool. Given the huge and growing expense of the criminal justice system, drug treatment might be cost-effective relative to incarceration (Jofre-Bonet and Sindelar 2002, abstract).

DANIEL KLEIN: PRO-LIBERALIZATION

A barrage of research and opinion has pounded it [the Drug War] for being the cause of increased street crime, gang activity, drug adulteration, police corruption, congested courts and overcrowded jails. Drug prohibition creates a black-market combat zone that society cannot control (Klein 1993, 11).

LI WAY LEE: LIBERALIZATION?

The paper has advanced a theory of illicit drug markets in which buyers and sellers face large transaction and consumption penalties, and it has used the theory to analyze whether harassing users would lower both consumption and price. The analysis implies that, under the present criminal justice system, escalating the hostility towards users is unlikely to be the win-win policy that standard theory suggests (Lee 1993, 957).

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JEFFREY A. MIRON AND JEFFREY ZWIEBEL: LEGALIZATION

The existing evidence relevant to drug policy is far from complete. Given the evidence, however, our conclusion is that a free market in drugs is likely to be a far superior policy to current policies of drug prohibition. A free market might lead to a substantial increase in the total amount of drugs consumed. But that policy would also produce substantial reductions in the harmful effects of drug use on third parties through reduced violence, reduced property crime and a number of other channels. On net, the existing evidence suggests the social costs of drug prohibition are vastly greater than its benefits (Miron and Zwiebel 1995, 192).

MARK H. MOORE: SUPPORTS STATUS QUO

Mark Moore is a political scientist, but has published on the subject of drug policy in the American Economic Review.

The real lesson of Prohibition is that the society can, indeed, make a dent in the consumption of drugs through laws. There is a price to be paid for such restrictions, of course. But for drugs such as heroin and cocaine, which are dangerous but currently largely unpopular, that price is small relative to the benefits (quoted in New York Times 1989, A21).

In sum, I am sympathetic to the notion that society should have a rational regulatory scheme for controlling the availability of psychoactive drugs according to reasoned estimates of their potential for abuse and their value in legitimate medical use. I believe that the current statutes create a workable framework for such a regime. In answer to the question of whether society would be better off if it widened legitimate access to drugs such as heroin and cocaine, I would say no (Moore 1990, 724).

WILLIAM A. NISKANEN: SUBSTANTIAL LIBERALIZATION

In summary, the popular perception that drug legalization would lead to a large increase in health problems and demands on the medical system by drug users appears to be without merit. The potential net effects (of legalization) appear to be small and may be negative (Niskanen 1992, 244).

CHRIS PAUL AND AL WILHITE: LEGALIZATION/LIBERALIZATION

[With drug prohibition] competition for market control creates negative externalities which take several forms. First, violence increases as sellers attempt to monopolize markets, enforce contracts and protect property risking harm or harming non-participants, Second, as a consequence of the higher “monopoly” price, the number and severity of crimes increase as buyers attempt to support their use. Third, some of the revenue is used to corrupt police, politicians and otherwise legitimate businesses. Fourth, as illustrated by the current “war on drugs,” non-participants civil liberties are eroded as law enforcement agencies attempt to identify voluntary market participants. Finally, steps taken by the public to insulate themselves from these crimes and civil liberty disruptions constitute additional social costs (Paul and Wilhite 1994, 114).

DAVID RASMUSSEN AND BRUCE BENSON: LOCALIZATION AND DECRIMINALIZATION

Thus, a more pragmatic policy may be both economically and politically superior: A regime of local control and more or less “permanent experimentation,” not seeking to solve the problem all at once with a federally mandated universal policy, but simply letting local officials make changes in policy that are politically feasible and likely to

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yield more benefits than costs (Rasmussen and Benson 1994, 177).

The crucial point is simple: most serious policy analysts actually agree on more than the mass media soundbites from the public debate on drug policy imply. In a localized experimentation approach to drug policy, particularly in the early years of such a regime, it appears that many prohibitionists and advocates of legalization would find considerable common ground (Rasmussen and Benson 1994, 179).

First among federal reforms in drug policy should be a reduced role for or, perhaps better yet, elimination of the Office of the National Drug Control Strategy (Rasmussen and Benson 1994, 182).

A third federal reform appropriate in the drug policy experiment is both important and very modest: federal decriminalization of marijuana possession. It is important because it provides an environment for effective local experimentation with de-emphasis of marijuana enforcement (Rasmussen and Benson 1994, 83).

PETER REUTER: REALLOCATE RESOURCES FROM ENFORCEMENT TO TREATMENT-LIBERALIZATION

This suggests that we should examine the possibility of enforcement moving to the fringes of drug policy, aiming at getting dependent users into treatment and making drug dealing less conspicuous, and thus drugs less available to novice users. The case is far from proven but the truth is that we are far from knowing either whether toughness has been tried or whether its potential gains are worth the potential costs, given the other means available to us for achieving comparable reductions in drug use (Reuter 1991, 152).

MURRAY N. ROTHBARD: LEGALIZATION

There is, of course, a very strong connection between addiction and crime, but the connection is the reverse of any argument for prohibition. Crimes are committed by addicts driven to theft by the high price of drugs caused by the outlawry itself! If narcotics were legal, the supply would greatly increase, the high costs of black markets and police payoffs would disappear, and the price would be low enough to eliminate most addict-caused crime (Rothbard 1978, 111).

HARRY SAFFER AND FRANK CHALOUKKA: LIBERALIZATION

The main findings from the regression results are that drug control spending reduces drug use. However, the results suggest for marijuana users, the marginal cost of drug control exceeds the social benefits of drug control. This may not be the case for users of other illicit drugs. Spending for drug enforcement by police and drug treatment are found most effective in deterring drug use. However, spending for correctional facilities is never significant which suggests that a more efficient method of reducing drug use might be to reduce correctional facilities spending and increase spending on treatment (Saffer and Chaloupka 1999, abstract).

DAVID SOLLARS: LIBERALIZATION

Thomas Sowell (1987, 74) has written, "Policies are judged by their consequences, but crusades are judged by how good they make the crusaders feel." There is little doubt that the current drug war has elements of a crusade. This, however, does not necessarily imply that the crusade is misguided or that the costs of the policy are larger than the benefits. In the Florida case, however, evidence suggests that the drug war policy has failed to achieve its goals and has probably created many unintended consequences.

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While Florida may be unique in many geographic and demographic categories, the Florida "War on Drugs" model encompasses most elements of a "get-tough," supply-side approach. Evidence suggests that the past drug policy in Florida may be misguided as the assumptions which undergird the policy are suspect. Revising the assumptions may result in the formulation of a new policy which is better able to reach desired ends without the explicit and implicit costs associated with the current policy. If the twin goals of reducing drug use and reducing property crime are to be realized, then other policy options must be formulated (Sollars 1992, 36).

THOMAS SOWELL: DECRIMINALIZATION

What would make still more sense would be to admit that we are not God, that we cannot live other people's lives or save people who don't want to be saved, and to take the profits out of drugs by decriminalizing them. That is what destroyed the bootleggers's gangs after Prohibition was repealed (Sowell 1989).

SAM STALEY: DECRIMINALIZATION

American drug policy should be realigned according to the potential harms of drug abuse and the economic development needs of American cities. As long as drug policy ignores the demand side of the drug-use equation, little headway will ever be made in the battle to reduce drug addiction and abuse. Drug policy, through most of U.S. history, has been supply-side oriented, implicitly assuming that eradication of the source would miraculously reduce the demand for illicit drugs. The reality has been the persistence of a drug industry feeding on the demand for illicit psychoactive substances. As law enforcement efforts become more concentrated, the drug industry becomes more violent, profitable, and debilitating.

Decriminalization is a strategic shift to a demand-side strategy that concentrates on education and treatment. Decriminalizing drug use and trafficking will greatly increase our ability to cope with the human dimensions of drug abuse. Moreover, by shifting to a demand-side strategy, that uses comprehensive decriminalization as a cornerstone, urban policy can concentrate more fruitfully on the problem of urban economic growth and development (Staley 1992, 249).

PAUL TAUBMAN: LEGALIZATION IS PROBABLY BAD

Although the quality and quantity of available research reported above could be improved. It seems likely that the price elasticity of demand is not zero. Since decriminalization would sharply lower prices, there would probably be a noticeable increase in use of drugs and new users and addicts. People other than users would be affected, with children being one of the largely impacted groups. The costs to make these children “whole” would be large: There would probably be an increase in homelessness, imposing health and other costs on society and its members. An increase in child, spouse, and parental abuse is likely, especially if cocaine and crack are used more heavily. Putting a value on these changes is difficult.

The estimate of all the effects of drug use need to be improved substantially before a firm judgment can be reached on whether the value of the benefits outweigh the costs of decriminalization (Taubman 1991, 106-7).

CONCLUSION (OR CONFUSION?)

Do economists reach a specific conclusion on drug policy? Certainly not. Do economists reach a weak conclusion? Yes. Based on my, admittedly incomplete and imperfect, investigation, I am comfortable saying that economists who think enough about drug policy to publish (and hence be accountable for) judgments on the topic largely point in the liberalization direction.

The set of policy judgments do not, however, present a clear and unified perspective that the general public can understand, trust, and willingly accept. Therefore, the impact of their efforts and pronouncements is dispersed and easily countered by those who tout prohibition and generally defend it with fear tactics (crime, addiction, and children). The general consensus that does exist among drug policy researchers and economists as a whole could be characterized as anti-prohibition, but only timidly pro-decriminalization and even less so about the prospects of legalization.

This consensus among economists may have contributed significantly to a movement towards liberalization of drug policy. There has been a significant increase in the public support for liberalization. Starting in the late 1990s, there has also been a significant and successful movement to legalize “medical marijuana” where doctors can legally prescribe the use of marijuana for a variety of illnesses. Canada has enacted a medical marijuana law and is seriously considering decriminalizing marijuana altogether. Several European countries have taken significant steps to liberalize their drug laws. It would be difficult to argue that the endorsement by economists for drug law liberalization has had no effect on public opinion or on public policy.³

I believe that, with more research and a great deal of critical introspection, economists can move from this general consensus to a firmer pro-liberalization conclusion, and that establishing a firm conclusion would greatly enhance the transmission of research findings to opinion makers, the general public, and ultimately into public policy. This is important because current liberalization policies such as medical marijuana, decriminalization and state-run drug stores and addiction treatment facilities are “halfway” measure between prohibition and legalization and such policies are politically highly

³ See Thornton (1991b) where it is shown how the endorsements of experts are translated into public policy and where the current liberalization reforms are predicted.

unstable. As Randall Holcombe, an expert in public policy who has examined reforms, such as decriminalization, observed:

The problem with all of these proposals is they leave the problems of illegal drug use intact. The problems are caused by the existence of underground markets. The only way to truly eliminate those problems is to legalize the sale of drugs. If half-way measures do not succeed, there will be a renewed push for stronger drug laws using the argument that decriminalization was tried and did not work. Half-way measures are not likely to work, because they retain the incentives to trade in illegal markets (1995, p. 158-9).

This suggests that economists should continue their research on drug policy, continue to refine their understanding of prohibition in general, drug laws in particular and all suggested reforms, develop alternative reforms, and continue to translate their findings into meaningful recommendations for public policy.

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ABOUT THE AUTHOR



[Mark Thornton](#) received a doctorate in economics from Auburn University in 1989. A revised version of his dissertation was published by the University of Utah Press in 1991 as *The Economics of Prohibition*. He was editor of the *Austrian Economics Newsletter* from 1983-1992. From 1989-1997 he was the O. P. Alford III Chair and adjunct professor of economics at Auburn University. He was appointed the Assistant Superintendent of Banking in Alabama and served from 1997 to 1999. He served on the faculty of Columbus State University from 1999 until 2002. Currently he is a Senior Fellow at the [Ludwig von Mises Institute](#) and the book review editor of the [Quarterly Journal of Austrian Economics](#).



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INTELLECTUAL TYRANNY OF THE STATUS QUO



Agricultural Economists and the State

E. C. PASOUR, JR.*

[ABSTRACT, KEYWORDS, JEL CODES](#)

THE EDITOR OF *ECON JOURNAL WATCH* ASKED ME TO REFLECT on the development and character of the research field known as “agricultural economics,” and especially the narrower field, “agricultural *policy* economics.” It was suggested that I first say something about my professional experience and background.

I received master’s and Ph.D. degrees in agricultural economics at North Carolina State and Michigan State Universities, respectively. My graduate work and early professional work—in farm management and agricultural marketing—were quite conventional for a faculty member working in agricultural economics.¹ However, my view of economics and of economic research began to change following a sabbatical in the Department of Economics at

* Professor Emeritus, Department of Agricultural and Resource Economics, North Carolina State University, Raleigh, North Carolina. Numerous individuals provided information and opinions in the development of this paper. J. Bruce Bullock, B. Delworth Gardner, Dale M. Hoover, W. L. Nieuwoudt, and Michael K. Wohlgenant also made helpful comments and suggestions on an earlier version of the manuscript. The usual disclaimer applies.

¹ At the Ph. D. level, agricultural economics courses, including agricultural policy, agricultural production economics, and agricultural marketing courses were taken in the Department of Agricultural Economics. Micro, macro, international trade, and other economic theory courses were taken in the Department of Economics.

the University of Chicago. There, I became aware of the work by James Buchanan, Gordon Tullock, George Stigler, and other public choice economists that emphasizes the implications of the separation of power and responsibility in the political process for government regulation.

About the same time, I read Hayek's "The Use of Knowledge in Society" and began to read other works of Austrian economists (including Mises, Kirzner, Rothbard)—and, notably, James Buchanan's book *Cost and Choice*, which emphasized the implications of the subjective nature of cost as it influences individual choice. I then began to use these ideas—the subjective nature of cost, the separation of power and knowledge in the political process, and the separation of power and responsibility in the political process—to analyze government regulation in agriculture and related areas. Both the approach used, and the topics addressed, have been quite different from those of most other agricultural policy economists.²

My reflections on the field will emphasize the following points:

- The field of agricultural policy research grew enormously with the New Deal.
- Funding sources for agricultural policy research appear to affect its focus and findings.
- Agricultural policy economists have been moving toward liberalization of agricultural policy.
- The interventionist sentiment of current agricultural policy economists remains dominant.
- Agricultural economists are reluctant to move beyond positive economics to make judgments.

² Topics studied include: implications of the economic calculation debate (ECD) for agricultural land use planning, for economic development in agriculture, and for environmental policy; measurement of economic efficiency; Pareto optimal income redistribution; limitations of estimated rates of return on subsidized agricultural research; rent seeking; cost of production as a basis for farm price supports; market failure versus government failure; and Chicago political economy vs. conventional views in public policy.

AGRICULTURAL ECONOMISTS AND THE NEW DEAL

During the Great Depression, many prominent economists, including agricultural economists, favored central planning.³ They contended that classical economic doctrines were no longer relevant. Many, if not most, agricultural economists at the time thought that “scientific management” must replace the “clumsy mechanisms of unregulated price determination” (Kirkendall 1966, 44). M. L. Wilson, a prominent agricultural economist of that era, even proposed the creation of schools of “agricultural social engineering” (Wilson 1938, 3).

Leading agricultural economists played a major role in increasing government action in agriculture during the New Deal.⁴ Victor Christgau of Minnesota, the first agricultural economist to serve in Congress, introduced an agricultural planning bill in 1930. The Christgau bill (never enacted into law) called for the use of scientific procedures to increase profit in farming, plan land use, and serve the interest of the consumers and producers of farm products (Kirkendall 1966, 44).

Roosevelt’s New Deal administration quickly took steps to increase centralized control of American agriculture. Even before passage of the Agricultural Adjustment Act in 1933, Roosevelt authorized the reorganization of the Department of Agriculture to make it into an instrument of national planning. Later enactment of New Deal farm programs reflected “virtually full acceptance, for agriculture, of most of the techniques of monopolies, trusts, and cartels” (Benedict 1953, 514).

The New Deal farm programs markedly increased the demand for agricultural economists. From 1929 to 1939, the total number of agricultural economists increased about four times in the United States Department of Agriculture (USDA) and by about two-thirds in land-grant universities (Schultz 1941, 183). A prominent agricultural economist at the time concluded that after 1933, “almost every agricultural economist” was engaged, directly or indirectly, in the development, administration, or appraisal of government farm programs (Wells 1938, 753). Agricultural economics emerged as a major discipline during the New Deal Era.

³ This section draws heavily on (Pasour 1988).

⁴ Most of these nationally prominent agricultural economists were later named AFEA fellows, the highest honor bestowed by the American Farm Economic Association, which eventually became the current American Agricultural Economics Association.

IMPLICATIONS OF FUNDING SOURCES

Over the years, the USDA land-grant university system has played the dominant role in agricultural policy research (Benedict 1953). This has had important implications for the course of research.

The USDA was established in 1862 with the primary objective of discovering cost-reducing methods of producing and marketing farm products. It operated mainly as a scientific and statistical information agency for farmers until 1932, when it became a New Deal “action agency,” aiming to regulate the production and marketing of farm products.⁵

Also in 1862, Congress enacted the Morrill Act to encourage the establishment of an agricultural and mechanical college in each state. The Act provided for a grant to the states of thirty thousand acres of land for each representative and senator in Congress. The proceeds were to be used for the endowment and support of, at least, one land-grant college in each state.

Today, most agricultural economics research and educational activity is jointly funded (from federal and state funds) through co-operative agreements between the USDA and land-grant universities. Moreover, with few exceptions, agricultural economists in educational institutions in the United States now are employed in land-grant universities. According to the American Agricultural Economics Association, 70 percent of its members in 2002 were employed in educational institutions and 16 percent in government.⁶

Moreover, the economic livelihood of many agricultural economists is closely tied to the government’s role in agriculture. As economists, we must at least entertain the idea that economic interests will affect policy analysts’ views toward government intervention in agricultural production,

⁵ The Federal Farm Board, created in 1929, was the immediate forerunner of New Deal action programs in agriculture. The basic idea was to raise prices of wheat, cotton, and other products by government purchase and storage to cope with temporary overproduction and low prices. President Roosevelt abolished the Board in 1933.

⁶ The breakdown of U.S. membership in the American Agricultural Economics Association in 2002 (based on members specifying place of work) was as follows: 70%, educational institutions; 16%, government; and 14%, private business, cooperatives, farm/trade associations, etc. Although detailed data are not available, the lion’s shares of agricultural economists in government and educational institutions are in the USDA and land-grant universities, respectively. Source: Data provided by AAEA office.

marketing, and international trade. And, indeed, there is evidence that such interests do affect policy views.

Programs administered by the Department of Agriculture span a wide range of activities and agencies, including Farm and Foreign Agricultural Services; Rural Development; Food, Nutrition, and Consumer Services; Food Safety; Natural Resources and Environment; Research, Education, and Economics; and Marketing and Regulatory Programs. The programs are highly complex, and USDA offices are maintained in counties throughout the United States to administer the programs. These government programs currently employ more than 110,000 people (USDA 2003, 103). Included in this number are professional agricultural economists in land-grant universities, the Economic Research Service, Risk Management Agency, Cooperative Extension Service, and other agencies of the USDA.

Over time, federal financial support relative to state funding has decreased and no longer is the main source of support for agricultural economics. Yet, government farm programs continue to influence heavily the work agenda of agricultural economists, especially those engaged in policy research. The complexity of government farm programs provides increased opportunities for analyzing the effects of the programs.⁷

To some extent, policies in U.S. farm programs have shifted toward a market orientation. Even so, recent farm bills have done little to decrease work opportunities for agricultural economists. The 1996 “freedom to farm” bill did appear to mark a change toward free-market agriculture. It reduced reliance on price supports and the complexities associated with restrictions on land use in producing and marketing farm products. It instituted a system of seven annual fixed, but declining, payments to farmers from 1996 to 2002 that were independent of farm prices and largely independent of current production.

With the passage of the 2002 farm bill, however, the link between government payments and market prices was firmly re-established, and spending on U. S farm programs under the new six-year bill quite likely will set new records. In short, the operation and analysis of government farm programs continues to provide fertile ground for employment of agricultural economists.

I have always lamented the lack of competent criticism of the numerous restrictions on economic freedom in American agriculture, especially compared with economists’ criticism of restrictions in many other

⁷ A prominent agricultural economist labeled the 1986 farm bill a “full employment act for agricultural economists” (Ruttan 1986, 81).

sectors of the economy. Funding may be one big reason, as Nobel Laureate and agricultural economist T.W. Schultz contends. He suggests, for example, that it is outside the power of the Economic Research Service of the USDA to finance competent criticism of its own activities (Schultz 1979, 468). The procedures used in reviewing and publishing research in a government agency, such as the Economic Research Service, are unlikely to lead to the dissemination of results that fail to support current administration policy. As several scholars have pointed out, bureaucrats are unlikely to advocate positions that would result in reduced budgets (Stroup and Baden 1983, 49).

Agricultural economists in land-grant colleges and universities, not as dependent on federal funding, are under less pressure from federal policy makers. The influence of narrowly-focused, intrastate groups, however, can be strong. Many of these groups assume that agricultural economists should be “working for farmers.” State funding, and the placement of specialized federal agricultural research laboratories, respond to state farm lobbies. The local crop and livestock interests and farm lobbies work against any conceivable impulse of state-employed agricultural economists to remark on the emperor’s wardrobe—misbegotten government programs.

Academic freedom, also, is likely to be compromised where research criticizes or fails to support the programs of narrowly-focused groups. The Iowa margarine incident is the most notorious example of the danger to academic freedom when it adversely affects a powerful clientele. As I have previously noted:

In 1943, an agricultural economist at Iowa State College wrote a pamphlet on dairy policy. The study concluded that margarine “compared favorably” with butter in nutrition and palatability and argued for changes in federal and state legislation that impeded consumption of margarine. Following attacks on the pamphlet by groups of dairy farmers and the subsequent recommendation by a review committee that the pamphlet be retracted and revised, Professor (later Nobel Laureate) Theodore Schultz and several other agricultural economists resigned (Pasour 1988, 40).

More recently, cases have occurred in which agricultural economists who have questioned—or even failed to defend—restrictions on competition in milk, tobacco, and other products have faced political pressure from

within agriculture (Pasour 1988, 40). In 1984, agricultural economists at North Carolina State University published a report analyzing the effects of eliminating the “tobacco program”—a governmentally enforced producer cartel (Sumner and Alston 1984). The study merely discussed the effects of deregulation and did not explicitly attack the tobacco cartel. Yet, it created outrage among tobacco interests. The university and the authors were forced to hold a news conference to “clear the air.” In contrast to the Iowa State incident, however, university officials defended the research that was under attack. It is interesting to speculate, however, whether the university would have supported the authors if the study had explicitly gone after the tobacco cartel!

B. Delworth Gardner, former Director of the Giannini Foundation of Agricultural Economics, at the University of California, states that agricultural producer interests have exerted political pressure in cases involving subsidized irrigation water in agriculture in the western United States.⁸ In one such case, a report by agricultural economists criticized the Bureau of Reclamation for not enforcing the 160-acre limitation for receiving subsidized federal water on agriculture in the Central Valley of California. In response, agricultural interests demanded a meeting with the economists and the director of the Giannini Foundation and demanded that the report be suppressed. Although the report was not suppressed, the threat of such pressures against academic freedom is enough to make agricultural economists wary of opposing producer interests. And the pressures on the researcher are intensified when those affected can influence the policy economists’ source of funding.

Thus, social and economic pressures from college officials and funding agencies may make agricultural policy economists “pull their punches” in criticizing restrictions on competition in agricultural production and marketing.⁹ A free-market tobacco economist may merely engage in “positive analysis” of the tobacco program, without explicitly arguing for reform or elimination of the producer cartel. Similarly, free-market policy economists may lend support to an anti-liberal farm policy by failing to analyze it—by taking it as a given. In my experience, social and economic pressures are more likely to lead agricultural policy economists to refrain

⁸ Personal correspondence, October 31, 2002.

⁹ The Dean of the College of Agriculture and Life Sciences at North Carolina State University communicated to me, via the department head, that I should “do economics instead of philosophy,” following my criticism of government farm programs.

from expressing what they believe rather than to falsely represent what they do believe.

The views of agricultural economists on farm commodity programs appear to be influenced by the economic importance of the commodities in the region of employment. A survey of American agricultural economists by Pope and Hallam supports the hypothesis that their expressed views on policy issues are not independent of whom they work for (Pope and Hallam 1986). The survey, for example, showed significantly greater support for the tobacco program by agricultural economists employed in the Atlantic region (where tobacco is grown) compared with economists in other regions. Similarly, there was more support in the Midwest for government grain policy.

Commodity specialization was also found to be an important factor. Indeed, the major finding was the existence of “sympathy for programs and problems in one’s chosen area of study” (Pope and Hallam 1986, 582). Economists working as dairy specialists were unusually pro-interventionist, strongly favoring dairy marketing orders,¹⁰ and both dairy and livestock specialists tended to view agricultural markets as unusual, with characteristics suggesting intervention (Pope and Hallam 1986, 581).

LIBERALIZATION—EVOLVING VIEWS

Since World War II farm policies have become somewhat more liberal, and there appears to have been a corresponding shift toward liberalization in the views of agricultural economists working on policy issues. In a recent paper, Bruce Gardner (1996) observes that until about 1950, agricultural economists generally recommended intervention by government to improve the functioning of commodity markets. By 1990, overt support for such intervention by agricultural policy economists was much less frequent.¹¹ Gardner cites D. Gale Johnson, Luther Tweeten, and Willard Cochrane as examples of leading agricultural policy economists

¹⁰ Marketing orders empower government-appointed panels of producers and middlemen to make industry-wide marketing decisions about sales volume and standards.

¹¹ Gardner focuses on the change in views toward commodity programs that affect the production and marketing of farm products. His analysis of the changing views of agricultural policy economists does not include their stance toward subsidized conservation, subsidized credit, subsidized research, and other farm programs.

whose views on government intervention shifted substantially during the post-World War II era.¹²

Shortly after World War II, D. Gale Johnson, who served as president of both the American Farm Economic Association and the American Economic Association, recommended “forward prices for agriculture” policy (Johnson 1947). In this policy, the government would determine, and announce, the price for each commodity for the upcoming crop year, setting the price at the level that would clear anticipated supply and demand. If market prices fell sufficiently far below the forward price level, farmers would receive government payments. By 1991, however, Johnson was calling for a “consistent and gradual reduction of price supports and subsidies that affect output to levels that approach the prices that would prevail under a liberal world trading regime”—a recommendation clearly in the direction of liberalization (quoted in Gardner 1996, 228).

In spite of his forward pricing policy, however, Johnson’s views, even right after World War II, always favored reform in the direction of liberalization compared with the status quo. Johnson’s views were more market-oriented than either then-existing agricultural policies, or the views held by most other economists working on agricultural policy issues at the time.

The evolution of Luther Tweeten’s views is more striking. As late as 1971, Tweeten justified government commodity programs in agriculture on the grounds that they would create an orderly economic environment by stabilizing the production and marketing of farm commodities. He wrote, in his treatise on agricultural policy, that “the stability function is so important that a free market is now mostly an academic exercise” (quoted in Gardner 1996, 228).

In Tweeten’s updating of the treatise almost two decades later, in 1989, however, the emphasis on the stabilizing role of government farm programs was gone: “[A] greater market orientation in farming threatens neither the family farm nor food supplies” (quoted in Gardner 1996, 228). Tweeten’s more recent work reveals an even more favorable stance toward a free market in the production of food and fiber. He propounds a “new paradigm” for agricultural policy, emphasizing that agricultural commodity markets work—indeed, almost always better than the alternative (Tweeten 2002, 2).

¹² The following discussion of “forward pricing” relies heavily on Gardner (1996).

[C]ompelling historical experience demonstrates that . . . the market rarely can be improved upon for economically efficient provision of food and fiber and for economic growth, international competitiveness, and food security (Tweeten 2002, 2).

In short, Tweeten's views of the appropriate role of markets, in the production and marketing of farm products, have evolved considerably in the direction of liberalization.

Willard Cochrane's change is less pronounced. Cochrane, a government adviser on farm policy during the Kennedy era, was a staunch supporter of agricultural price supports and production controls. By the mid-1980s, however, he had changed his mind, writing that "we should eliminate the price and income support features of the commodity programs as quickly as possible" (quoted in Gardner 1996, 228). More recently, Cochrane characterized as "a little obsolete" his view that farmers face the "inescapable choice" between production controls and the ravages of the free market (quoted in Levins 1996, 18).

Gardner's survey concludes that the positions of Johnson, Tweeten, and Cochrane reflect a trend among agricultural economists from 1947 until 1990. In his words, there was "a movement from a position that appropriate commodity price regulation including price supports is called for, to a position that such supports should be eschewed" (Gardner 1996, 229).

There is some evidence that views of agricultural *policy* economists toward economic liberalization of farm policy are similar to those of other agricultural economists. In Pope and Hallam's survey of agricultural economists published in 1986, a majority favored intervention over laissez-faire in agriculture. The views of those specializing in public policy were not much different in this respect from other agricultural economists (Pope and Hallam 1986). The survey found less support for free markets by agricultural economists than a similar AEA study found for economists generally (Pope and Hallam 1986, 578). But it also reported that younger agricultural economists were less interventionist, and more critical of commodity programs, than members of the profession who were older (Pope and Hallam 1986, 591).

Why did the views of agricultural policy economists evolve, at least to some extent, toward liberalization during the decades following World War II? Gardner suggests that developments in economic theory have caused agricultural economists to be less inclined to see uncertainty as a source of

market failure requiring government intervention. He cites the theory of behavior under uncertainty generally, and of optimal insurance, futures, and cash contracting related to uncertainty—especially the theory of optimal storage policies—as important in the evolution (Gardner 1996, 239).

INTERVENTIONIST SENTIMENT

Even though there has been an evolution of professional opinion toward liberalization, there remains substantial interventionist sentiment among agricultural economists conducting agricultural policy research. “Market failures” still appear in many of the writings of agricultural economists, but as Gardner points out, “they are today more subtle and less amenable to translation into a commodity policy remedy” (Gardner 1996, 239).

Today, Johnson, Tweeten, and Cochrane would each reserve a significant role for government in the production and marketing of farm commodities. Johnson favors continued government support of agricultural research, the present farm credit system, and a more limited role for government in agricultural conservation programs.¹³

Tweeten would retain a significant role for government in coping with “market failure” in agriculture. “To be sure,” he wrote in 2002, “the government needs to play a role in provision of public goods (e.g., grades, standards, basic research, information systems, infrastructure, competition) so the market can function well” (Tweeten 2002, 2).

Cochrane, while calling for the elimination of price and income features of commodity programs (in contrast to Johnson and Tweeten) would continue to intervene to stabilize farm incomes—and would “distribute government income stabilization payments far more fairly” (Cochrane and Runge 1992, 261). He favors a more liberal international trade policy for agriculture, but not free trade. He writes:

The reason we support more liberal trade in agriculture is not that we think that “free trade” is possible or even desirable. It is because . . . a *more* open trading system in agriculture *will work to the overall advantage of U.S. farmers*

¹³ Personal correspondence, July 31, 2002.

because they are among the most individually efficient producers in the world (emphasis in original text) (Cochrane and Runge 1992, 265).

In short, Cochrane supports a more liberal trade policy in agriculture, but explicitly rejects the free market in American agriculture, contending that “there will always be a need for some kind of safety net for farmers” (quoted in Levins 1996, 18).

Other prominent agricultural policy economists strongly reject the free market as a model for American agricultural policy. For some, the rejection stems from, what they perceive to be, a failure of economic models to explain the operation of agricultural markets and an inability of agricultural markets to satisfactorily coordinate the production and marketing of farm products. In other words, they see a continuing “market failure.”

Darrell Ray, an agricultural economist who holds the Blasingame Chair of Excellence in Agricultural Policy and is the director of the Agricultural Policy Center at the University of Tennessee, challenges the view that agricultural commodity markets work.

There is no recognition that, when crop prices capsize, market demand does not provide a rigging to raise them back up again. . . . There is also no recognition that market response on the supply side is no help in the search for a cure for low prices. . . . While belief in market self-correction via supply and demand response to depressed prices may have been a reason to embrace the 1996 legislation, why would we want to take that dog out to hunt again this time around (Ray 2002).

Neil Harl, Charles F. Curtis Professor in Agriculture, Professor of Economics at Iowa State University, and former president of the American Agricultural Economics Association, supports Ray’s views and proposes a global food and agriculture policy.

Farm policy debate in the United States in the 1920s was largely about whether it was appropriate to have a national food and agriculture policy. To a considerable extent, the decision was in the negative until 1933. In many respects, farm policy today poses a similar question: should efforts be directed toward a global food

and agriculture policy? In the opinion of this commentator, the answer is yes (Harl 2003, 11).

Ronald D. Knutson, agricultural policy economist at Texas A&M University and co-author of a widely used textbook in American agricultural policy, is a staunch defender of agricultural commodity programs. Knutson indicated recently that “the 1996 Freedom to Farm Act and World Trade Organization negotiations have left American farmers with little more than a seed bag full of unkept promises” (Smith 2001, 17).¹⁴ In Knutson’s view (according to a press report), the “Promised Land” for farmers is the 1940s-era agricultural policy, which set parity prices and production controls (Smith 2001, 17).

C. Robert Taylor, Alfa Eminent Scholar and Professor of Agricultural Economics at Auburn University, contends that market power is a serious problem in American agriculture, especially in the meat industry. He argues that meatpackers and food retailers have too much market power and blames professional economists, including those involved in antitrust enforcement. Taylor holds:

The permissive attitude behind approval of recent mergers, acquisitions and joint ventures appears to be based on the single-minded pursuit of economic efficiency. Legislation including GATT, NAFTA, and Freedom to Farm also reflect the pursuit of economic efficiency, as does the teaching of many present day professional economists (Taylor 2002, 1).

Richard Rogers, Professor of Resource Economics at the University of Massachusetts, and Richard Sexton, Professor of Agricultural and Resource Economics at the University of California-Davis, agree. They contend that “markets for raw agricultural products are likely to be structural oligopsonies” and that “monopsony/oligopsony issues deserve strong consideration in food policy debates” (Rogers and Sexton 1994, 1149).

Richard Levins, Professor and Extension Agricultural Economist in the Department of Applied Economics at the University of Minnesota, holds that the market power problem warrants collective bargaining by farmers.

¹⁴ Ron Smith, the author of the article, indicated in an e-mail to me: “To the best of my knowledge, those are his exact words.” However, the words were not in quotes in the article.

Collective bargaining, unlike competition, has the potential to increase economic power in the farm sector (by “collective bargaining,” I mean face-to-face negotiation between a powerful farmer collective bargaining unit and some other food industry value chain powerhouse)(Levins 2001-2002, 17).

Neil Harl, a proponent of a global agricultural policy, and an ardent supporter of the proposed national ban on packer ownership and feeding in the American meat industry, agrees that the time for collective bargaining may be near:

The key question is whether producers will be willing to sacrifice independence of action in order to bargain collectively for access to inputs and for greater market power in marketing their products. The most likely avenue for such collective action is through organizations specifically created for that purpose. The time may be near when that will be the only practical alternative to vulnerability and serfdom (Harl 2003, 25).

The concern of a vocal minority of populist economists recently focused on market power in the American meat industry. The prevailing paradigm among these analysts is that concentration ratios are a measure of “market power” of firms in the industry (Bullock 2002, 1). In the analysis of the proposed national ban on packer ownership and feeding, Conner et al conclude:

The packer ownership amendment addresses real problems in the competitive environment of the livestock industry. The claimed harms arising from the amendment are largely not credible, and certainly less significant than the potential benefit to the marketplace. If any negative market effects occur, such effects will be the result of packers using their tremendous power over the market place (Conner et al. 2002, 10).

These views have not gone unchallenged. Barry K. Goodwin, Professor of Agricultural Economics at North Carolina State University,

shows that scale economies, or other competitive advantages, may lead to concentration in many industries without implying the existence of welfare-diminishing market power pricing.

In the context of this debate, it is also important to again note that increasing concentration does not confirm the existence of discriminatory market power pricing practices. A balanced approach will recognize the potential for economies of scale as a factor leading to increasing industry concentration (Goodwin 1994, 1165).

Specifically, in the debate over the importance of monopsony/oligopsony in the American meat industry, Bullock shows that based on packer profits the performance of this market is quite consistent with a well-functioning competitive market. Many other agricultural policy economists also have expressed judgment that a government ban on packer ownership and feeding—a move away from economic freedom—would be harmful to the American cattle industry, resulting “in reduced coordination, efficiency and global competitiveness of the beef and pork sectors” (Bullock 2002, 3).

DO AGRICULTURAL POLICY ECONOMISTS GENERALLY FAVOR LIBERALIZATION?

So, today the views of agricultural economists vary. As Daniel Sumner suggests, most agricultural policy analysts are “fairly market oriented in their assessments of policy.”¹⁵ However, it is easy to overstate the implications of this finding, for several reasons. First, many agricultural policy analysts strongly disposed toward freedom do not take a public position favoring reform when analyzing restrictions on competition in agriculture. Second, many agricultural policy economists—while opposing the regressive character of current programs—continue to support government subsidies to farmers. As shown, some prominent agricultural economists remain unconvinced that the free market can coordinate satisfactorily the production and marketing of farm products and continue to favor farm

¹⁵ Personal correspondence, May 28, 2002.

programs. Third, although agricultural policy economists' support for liberalization of commodity programs is mixed, their support for liberalization of non-commodity farm programs is even less enthusiastic.

In general, whether or not an individual is considered a free-market agricultural economist mainly depends on whether the individual favors price supports and/or income subsidy programs for those producing and marketing farm products. When it comes to other farm programs, they are much less skeptical of government intervention. These programs include subsidized crop insurance, subsidized credit, subsidized conservation, subsidized food, subsidized agricultural research and extension, marketing orders, and tax preferences in agriculture. The following, admittedly brief and sketchy, review of policy analyst's views provides some information on the direction of reform proposed by agricultural policy economists for non-commodity farm programs.

In government conservation programs, relatively little attention has been devoted to the costs and benefits of programs to "preserve agricultural land" through central direction—or of other "smart growth" policies. In large part, the voices of agricultural economists have been muted in the defense of market forces in the allocation of land to agriculture and other competing uses. Instead, most of the defense of liberal economic policies in land use has fallen to economists operating outside the USDA land-grant university complex.¹⁶

Agricultural policy economists express strong support for *increased* government subsidies for research and development in agriculture. In a host of studies estimating the return on government-funded agricultural research and educational activities, the authors argue for an expanded role for government funding of agricultural research and education. Vernon Ruttan, for example, citing a number of studies with estimated rates-of-return ranging from 30 to 60 percent, concludes: "There is little doubt that a level of expenditure that would push rates of return to below 20 percent would be in the public interest" (Ruttan 1980, 531). A more recent analysis of the results of similar studies of subsidized agricultural research during the period since 1958 concludes that a larger role for government is warranted for research and development in U.S. agriculture (Alston and Pardey 1996, 324).¹⁷

¹⁶ See, for example, (Baden 1984).

¹⁷ Both the rate-of-return estimates and policy recommendation are challenged in Pasour and Rucker (forthcoming).

In the case of government-subsidized crop insurance programs, the conventional wisdom among agricultural policy economists has been that systemic risk in agriculture is too large for private markets to handle. Consequently, most agricultural policy economists conclude that there is a welfare-improving role for government, as a re-insurer of last resort (Miranda and Glauber 1997, 206).

This claim does not hold up under scrutiny, as some agricultural economists have been pointing out. Shiva S. Makki, in a recent paper, argues in favor of liberalization of the current crop insurance program. “With some changes to the existing program design and limiting the government to provide only broad safety-net-type insurance products and area reinsurance, crop insurance programs could be made more efficient and less market distorting” (Makki 2002, 123). Goodwin and Smith, in a recent comprehensive analysis of government subsidized crop insurance and disaster relief programs, go much further. They conclude that current programs are rooted in political expediency and rent seeking instead of “market failure”—and that government subsidization of crop insurance is not justified on social welfare grounds (Goodwin and Smith 1995). B. Delworth Gardner, too, has expressed grave doubt about crop insurance and disaster programs in U.S. agriculture (Gardner 1995, 35-38). However, these authors are virtually alone in their bold criticism of subsidized crop insurance and disaster relief programs in U.S. agriculture.

Marketing orders for milk, fruits, and vegetables empower government-appointed panels of producers and middlemen to make industry-wide marketing decisions about sales volume and standards. Marketing orders utilize various measures, such as holding lower-grade products off the market, to raise consumer prices. A current, and widely used, agricultural policy textbook by prominent agricultural policy economists provides implicit support for these government-sanctioned and government-enforced producer cartels. It leaves open the question of whether “consumer losses in terms of higher prices” are more or less than “the consumer benefit in terms of uniform product quality” (Knutson, Penn, and Flinchbaugh 1998, 330).

Agricultural marketing orders have not been widely criticized by agricultural policy economists. The analysis by B. Delworth Gardner is a notable exception:

Eliminating the orders would impose some wealth losses on producers . . . but society as a whole would be better off. Like most other policies to protect agricultural

producers, marketing orders are anachronisms. . . . And the protection of growers from the rigors of full competition has produced industries that are bloated and inefficient to the detriment of consumers and efficient producers (Gardner 1995, 32).

Finally, there is water policy. Large, heavily federally subsidized water projects are very important in irrigation of agricultural production in the American West. These irrigation subsidies stimulate the production of crops and contribute to the problem of excess resources in agriculture. B. Delworth Gardner, long-time student of governmental water policy in American agriculture, recommends a liberalization of current water policies to produce a more efficient allocation of water.

[P]roperty rights in federal water should be created at the level of individual farmers so that markets can be formed to allocate water among potential uses and users. Legal and institutional impediments to market transfers of water must be identified and removed. Rights must be well-defined, enforced and transferable. . . . Prices should be freely negotiated between buyers and sellers without governmental intervention and no-profit constraints (Gardner 1995, 319).

POSITIVE ECONOMICS: DODGING RESPONSIBILITY

Despite numerous examples of interventionist sentiment, Daniel Sumner, prominent agricultural policy economist and director of the Agricultural Issues Center at the University of California, Davis, contends that “most economists” now accept the long-term goal of open agricultural markets and much-reduced government control of commodity supplies and prices (Sumner 1999, 1). Even so, agricultural policy economists frequently do not express judgment in favor of liberalization.

Although the source of funding may be one factor in this failure to express judgment, many, if not most, policy analysts prefer to perform “positive” analyses of farm programs—that is, to avoid the exercising and expressing of judgments on significant policy reform questions. Even agricultural

policy economists strongly supportive of the market in agriculture frequently do not state that they favor elimination of interventionist programs in agriculture, even in the case of such blatant infringements on economic freedom as the government-privileged tobacco and peanut cartels.

There is a difference of opinion concerning the extent to which an economist should maintain the diffidence of “positive economics” when analyzing policy issues. Some economists, touting “positive science,” may limit their study to the effects of the current programs—without mentioning that the programs waste resources, increase prices, restrict economic freedom, and therefore are undesirable.

Coase, however, suggests that once the effect of an egregious government policy is established, there is no good reason for economists not to say that the policy is undesirable:

Thus, we (in positive analysis) can say that certain agricultural policies (say collectivization) will lead to widespread starvation, but we cannot say whether collectivization is or is not desirable. Such restraint is I think unnecessary. . . . [I]t hardly matters, once it is established that a certain policy will lead to widespread starvation, whether we add that the policy would be undesirable, although to refrain from doing so on principle seems like an affectation (Coase [1975], 33-34).

The harmful effects of government programs, of course, generally are not as obvious or dramatic as they are in Coase’s example, so there is seldom a consensus on the policy at issue. However, the ninnyism of “positive economics” deliberately neglects an opportunity to strike a vital blow against government sponsored, and government-monitored, cartels. Edwin Cannan suggested that the pretension of economists to know nothing of good and bad ends has dire consequences for the profession. To cite Cannan’s example: If people ask an economist whether a change will be good or bad and the professor can only talk about the costs and benefits of different ways of obtaining a given end, the people merely “will find the economist tiresome.” In this case, said Cannan, the individual “wanted bread and the professor has given him a stone” (quoted in Hutt [1936], 64).

There is yet another potential problem in maintaining a strict judgment-free approach. In limiting the analysis to the effects of actual changes in current government programs, the researcher omits consideration of an

alternative institutional arrangement that may improve human welfare more than the change or changes in policy actually studied (Pasour 1993).

The book *Sustainable Development in Agriculture* provides a good illustration (Parikh 1988). It reports on a project designed to alleviate world food problems and to prevent future ones. Written in 1988, before the fall of the Soviet Union, the book presents the results of seven case studies of both market and centrally planned economies—including the Soviet Union. The authors agreed that attention be paid to resources, technology, and environmental consequences. But there was no recognition, in any of the studies, of the importance of the type of political and economic system—or of the link between the method used to coordinate economic activity and agricultural output. The optimal control model developed for the Soviet Union, for example, considered only physical factors of production, completely ignoring the knowledge and incentive problems associated with central planning. Not surprising, increases in agricultural output were found to hinge on changing technologies (such as new drought-resistant varieties of wheat). The importance of the institutional arrangement was clearly demonstrated by the food production on “private plots” in the Soviet Union and by the huge increase in agricultural output associated with the limited privatization policies in China. But the authors ignored these facts.

The foregoing is consistent with Philbrook’s example of analysts who implicitly defend the status quo by supporting farm products at, say, ninety instead of ninety-five percent of parity because the lower level of intervention is considered to be politically achievable (Philbrook [1953]). That is, the policy economist cooperates with things as they are, rather than explicitly criticize bad public policy. The alternative, of course, is to recommend major policy reform—in this case, abolition of farm price supports. In short, “Philbrook’s article is aimed at applied economists who pull punches with status-quo policies, in the name of ‘positive analysis,’ ‘realism,’ ‘science,’ etc” (Klein 1999, 22).

In conducting “positive economics,” researchers often proceed on policy judgments tacit and inchoate. One example is the failure to acknowledge government imperfection. In analyzing an externality, researchers may consider the effects of Pigouvian taxes or other government programs to cope with “market failure.” However, few studies take into consideration analogous problems that arise when governments intervene in markets. The problem of government imperfection is implicitly deemed to be less severe than the market imperfection supposedly remedied.

Consider, for example, a recently published analysis of the optimal Pigouvian tax when development of peat land for agricultural production

results in groundwater and surface water pollution. Ostensibly, the author engages in positive analysis. Yet, the bias in favor of intervention is inherent in the proposed approach for the development of peat land: “Now, we are able to state the optimal control problem for the social planner” (Goetz 1997, 229). This approach—central planning—assumes away the knowledge and incentive problems inherent in government planning. There is no recognition or estimation of the costs in productivity or human freedom associated with central direction. Instead, there is an implicit assumption that the costs associated with government planning are less than the benefits from the policy proposed to cope with the pollution problem. A similar criticism can be leveled at most studies purporting to determine the “optimal tax” to internalize externalities. They fail to take into account government imperfection—the problems and costs of policies designed to cope with spillover problems.

The failure to compare the costs of government and market imperfections is not limited to studies of externalities. It is inherent in all studies proposing government programs to cope with “market failure” problems—including market power, public goods, imperfect information, and distribution of income. In studies of a market imperfection, most analysts implicitly assume that the benefits of the proposed governmental policy to correct a market flaw outweigh the deadweight costs and infringements on individual freedom inherent in the intervention.

CONCLUSIONS

Looking back over my 40 years as an agricultural economist, I am pleased that there has been some movement toward liberalization, but that satisfaction is outweighed by the disappointment in the role played by agricultural economists in providing a rationale for government intervention in agriculture. Agricultural economists have a long legacy of supporting restrictions on competition in the production and marketing of farm products.¹⁸

¹⁸ Over the years, however, there have been a number of general presentations of agricultural policy by agricultural economists in monographs published by free market groups. These include: (Johnson 1974), (Borcherding 1981), (Gardner 1981), (Baden 1984), (Kahl 1985), (Luttrell 1989), (Pasour 1990), (Sumner 1995), (Gardner 1995), (Goklany 2000), and (Anderson and Yandle 2001).

Indeed, they played key roles in the development and administration of agricultural cartels in the New Deal. Although agricultural economists have increasingly criticized government farm programs, particularly price support and income subsidy programs, during the past half-century, there remains significant, explicit support for government intervention in agriculture.

The extent to which agricultural policy economists express judgment in favor of liberalization is influenced by a number of factors, including the mode of funding agricultural policy research. Funding by the United States Department of Agriculture gives government farm programs a larger effect on the work agenda of agricultural economists than government programs do in most other economic disciplines. There is evidence that the funding arrangement influences both the views of policy economists and the extent to which they express judgment about government farm programs.

There are two reasons for this impact. First, government farm programs over the years have provided a fertile field of job opportunities for agricultural economists. Second, and closely related, the funding arrangement for agricultural economists in the USDA land-grant university complex gives policy analysts an incentive not to question the appropriateness of the government programs they are analyzing. The implication is most obvious in the case of policy research within the Department of Agriculture. The review and publication process discourages research that is inconsistent with the policies of the current administration.

Although federal government funding is a less significant source of financial support for policy research in land-grant universities than it is within the USDA, political pressure from state and local farm commodity groups militates heavily against criticism of government farm programs there, too. These groups expect research and extension personnel to support government programs for their products. They often exert pressure on college officials and agricultural policy analysts who propose policy liberalization.

Many, if not most, agricultural policy economists today are fairly market oriented—at least in their view of farm commodity programs. Despite their incentives not to “rock the boat,” more market-oriented analysts often recommend liberalization of price supports and other “government stabilization programs.” Still, even free-market-oriented agricultural policy economists frequently fail to express judgment when analyzing government-enforced commodity cartels in agriculture. The mode of funding reinforces the incentive that many market-oriented agricultural policy economists have to withhold judgment by relying on “positive” analysis. Moreover, explicit support for farm commodity programs is strong among a small group of agricultural economists with a populist bent, who

contend that the market will not satisfactorily coordinate the production and marketing of farm commodities.

Even those policy economists overtly critical of farm commodity programs often support a substantial role for government in other areas, including conservation, research and extension, subsidized credit, subsidized food, and tax preferences. They may even explicitly express judgment *against* liberalization of current policy for these programs, most notably for subsidized agricultural research. Judgment frequently is expressed also for government programs to cope with public goods, externalities, and other “market failures.” In addition, agricultural economists also often tacitly support non-commodity programs by ignoring them in policy analysis.

In short, the extent to which agricultural policy economists explicitly express judgment in favor of liberalization is limited. While many policy economists criticize current farm commodity programs, a vocal minority defends the programs, and many agricultural economists provide substantial support for non-commodity farm programs. Much of this support can be traced to the approach used in the analysis of “market failures” in agriculture. Problems inherent in interventionism—government imperfection—almost never receive the same degree of analytical scrutiny as the market imperfection.

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INVESTIGATING THE APPARATUS

The Social Science Citation Index: A Black Box—with an Ideological Bias?

DANIEL B. KLEIN*
WITH ERIC CHIANG**

[ABSTRACT, KEYWORDS, JEL CODES](#)

THE SOCIAL SCIENCE CITATION INDEX (SSCI) IS A DATABASE of scholarly literature. SSCI is used in many important ways. The most conspicuous use of SSCI is showing whose work gets cited in other research. Which other research? The articles published in journals on the SSCI journal list. As of June 2003, SSCI included 1,768 journals. Your citation count is the number of times your work has been cited by articles in SSCI journals (actually, in those journals during the years that the journal was included in the SSCI).¹

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** *Note on authorship:* Although two authors are shown above, this paper is written in the first-person singular. Daniel Klein led the investigation and wrote up the paper. He is the “I” here. Eric Chiang is a student at Santa Clara University. He did most of the data collection and some of the institutional investigation.

We thank the following ISI employees for their help with understanding SSCI and checking facts: Chris Pasquini (Technical Support Representative), James Testa (Director of Editorial Development), and Rodney Yancey (Manager of Corporate Communications). For comments on an earlier draft, we thank Niclas Berggren, Dan Johansson, Per Hortlund, and Richard Johnsson.

¹ James Testa, Director of Editorial Development at Thomson ISI, writes in an e-mail correspondence to Eric Chiang: “When a journal is dropped it is from that moment forward. No future issues are indexed. What has been indexed remains in [ISI’s Web of Science].”

The SSCI is a product of Thomson ISI, which is a business of the Thomson Corporation, an information services provider that had revenues of \$7.8 billion in 2002. It is customary to refer to Thomson ISI as simply ISI.

The name ISI continues the legacy of the company that Eugene Garfield started in 1958—the Institute for Scientific Information. In 1961 Garfield, the “undisputed patriarch of citation indexing” (Cronin and Atkins 2000, 1), launched what remains the central citation index of the “hard” sciences, the Science Citation Index (SCI). SCI is ISI’s flagship product. Building on the SCI model, Garfield launched several other index products, including the Social Science Citation Index (SSCI) in 1973 and the Arts and Humanities Citation Index (AHCI) in 1978. In 1992 Garfield sold ISI to Thomson, but continues as Chairman Emeritus. In 2003, ISI offered a wide array of citation indexing services and employed about 850 people worldwide, with offices in the United States, United Kingdom, Ireland, Tokyo, and Singapore (Garfield Undated-a, and Undated-b; Thomson ISI Undated, 2003a, and 2003b).

IN ACADEMICS, CITATION COUNTS COUNT FOR QUITE A LOT

Academic success depends chiefly on getting published in “the good journals.” But another standard indicator of professional standing is getting cited. ISI is the only serious producer of citation data, so the term “citations” is synonymous with citations as recorded by ISI in its various products (such as SSCI). Peers, administrators, and grant-makers regard citation counts as a key measure of recognition and importance. Professor Doe might be much better published than Professor Johnson, but if Johnson is much better cited he might enjoy far greater eminence.

Citation counts influence more than professional esteem and respect. Institutional decisions about appointments, promotions, salaries, resources, awards, and prizes often hinge on citation counts. When a tenure committee or department wants to avoid internal conflict—potentially deep and bitter—over the purpose, character, and central teachings of their science, when

When a journal is added the coverage is from that moment forward and not retrospective” (Testa 6 May 2003).

they need to reach a decision in an “objective” way, to tacitly agree to disagree, they turn not only to the professional conventions of publication, but often also to citation counts.² In conjunction with this paper, Eric Chiang and I conducted a survey of 30 Economics Department chairs about the importance of citation counts and the SSCI in their departments. Half of the respondents reported that citations counts are usually or always considered in promotion cases. Fourteen respondents reported that the trend over the past 10 years has been toward putting greater weight on citation counts, while only one said the trend was toward less weight. Also, fourteen indicated that he or she expects the trend over the next 10 years to be toward greater weight on citation counts, while only one indicated that he or she expected the trend to be toward less weight (see Klein and Chiang 2004).

SSCI citations include citations to books, manuscripts, and journals not included in SSCI. However, some of the packaged citation tabulations, including those produced by ISI and utilized by the National Research Council (see Holcombe 2004), count only citations to articles that appear in SSCI journals.

The ISI indices, therefore, are important to the institutional gears of science and academia. Although SSCI pertains to all of the social sciences, I will focus on economics. However, I suspect that most of the concerns generalize to the other social science disciplines.

Researchers create formulas to generate rankings of economics departments, of individual economists, and of journals.³ Usually, citation count is an explicit and major variable in the formula, but even when a ranking is based solely on journal publication, the journals themselves are ranked by citation count. More weight is given to the more-cited journals. Thus, all rankings, either directly or indirectly, build on the SSCI. We see that the two major systems of gauging academic achievement—publication and citations—are highly circular.

Citation counts are a way of tracking one’s own professional recognition and keeping score. Even for academicians who would otherwise have little regard for citation counts, the institutional functionality comes to be internalized

² Hamermesh (1989) estimated that a citation correlates to an extra 0.2% in salary, and Moore et al. (2001) find that salary premiums are more strongly associated with a small number of highly-cited articles than a large number of scarcely-cited articles.

³ Tom Coupé (2003a) provides an extensive review of the various ranking formula and his own blends to generate rankings of departments and economists worldwide.

as a value. Whose is bigger, mine or his? Citation anxiety is sometimes palpable and enlargement strategies are sometimes pursued.⁴

Thus, the SSCI is an important component in the academic apparatus of rank and prestige. Those who decide which journals are to be included in the SSCI exercise an enormous influence over the social sciences. But surprisingly little scrutiny has been given to SSCI and its journal selection process—I have searched on “SSCI” in ISI’s own Web of Science and found little pertinent to this investigation.⁵ In fact, in reading the scientometricians, including the economists who work with citation data, one perceives an attitude that Citation is of a divine and immaculate nature. The researchers who work with citation data almost never so much as raise a question about who is making the all-important decisions about journal inclusion, how they make those decisions, and whether they are fair or reasonable. This paper seems to be the first critical examination of SSCI.

OTHER CONCERNS WITH USING CITATION COUNTS

Inconsistencies and biases in SSCI journal selection would represent problems in using citation counts as an indicator of a scholar’s social value. The matter taken up here, however, is merely one set of concerns. Other concerns range from practical matters to very broad problems involving the circularities and fads of academic culture and, indeed, the intellectual culture at its broadest and deepest levels. Here I note some of the practical problems and merely indicate the existence of broader problems.

Practical Problems with SSCI Citation Counts

These can be overcome, but it requires meticulous attention to the individual’s curriculum vita and intensive utilization of the SSCI records.

1. SSCI identifies items by initials, so items by David B. Klein will be mixed together with items by Daniel B. Klein. If the citing author does not

⁴ Enlargement strategies include self-citing, coauthoring, citation swapping, being academically-correct, and self-promotion in general.

⁵ The closest thing I could find to a critical examination of SSCI was an article by E.T. Funkhouser (1996) on the omission of many Communications journals from both SSCI and AHCI.

include the middle initial B, which is not uncommon, the item (and citation to it) is listed for D KLEIN. For common names like Klein, Lee, and so on, making a tally is tedious.

2. The citation records include self-citations.⁶

3. The SSCI gives a citation to each author of a coauthored work. Incidentally, since the late 1960s, the percentage of coauthored articles has soared from about 20 % of economics articles to 46 % in 1998 (Coupé 2003b).

4. ISI records citations from the citing article's Reference list. But for articles that do not have a Reference list, notably articles in the numerous law reviews included in SSCI, ISI records citations from the footnotes or endnotes. In such cases, if the article has a citation to Coase 1960 p. 17 and another citation to Coase 1960 p. 18, then Coase picks up two citations. In contrast, when an article has a Reference list (the norm in economics journals) and refers to Coase (1960, 17) in one spot and Coase (1960, 18) in another spot, the citing article nonetheless generates only one citation to Coase. Thus, economists who get cited in law review articles can rack up dozens of citations from a single citing article (and all to a single cited article).

A Few Broader Issues

Whether the following points represent “problems” is highly debatable, but surely some would feel that they do.

5. Relative to books, journal articles disproportionately cite other journal articles rather than books. Excluding books and other media from SSCI slights book authors.

6. An economist who publishes economic history, history of thought, or methodology might get cited in academic philosophy and history journals, which are generally covered by ISI's Arts and Humanities Citation Index rather than the SSCI, though SSCI does include some history and philosophy journals.

7. In economics, some subfields cite more than others. Touchstone theory or model-building papers might receive more than 50 or a 100 cites,

⁶ Incidentally, the all-time record for self-citations in a single article probably goes to Joseph E. Stiglitz. In his Nobel speech published in the *American Economic Review*, June 2002, Stiglitz cited 191 of his own works (including coauthored works). A fitting foil is Adam Smith: Never did *WN* (last edition 1789) nor *TMS* (last edition 1790) cite the other, except that in the preface to the last edition of *TMS* Smith mentions *WN* in remarking on how his advanced age will keep him from completing investigations originally proposed.

while the most cited empirical papers are cited less frequently (Diamond 1989).

8. People cite papers to mark an idea. The citation is a monogram of the idea. It is my impression that, for this reason, articles that make a single, sealed point, rather than make and join multiple points in an open-ended discussion or essay, tend to pick up more citations.

9. An article might be highly cited because of its badness. The citing author might feel that the cited paper was a misstep that needs to be answered or corrected.

Broad Concerns

This is not the place for broad criticism, but many thoughtful critics feel that economics and academic social sciences in general are inherently prone to self-legitimation, politics, and irrelevance. Critics often suggest that problems in particular disciplines cannot be separated from very broad cultural considerations.⁷ The reader is urged not to take from this paper a view that citations counts would become a reliable indicator of a scholar's social value if the practical problems with the SSCI were overcome. I think there are much more fundamental problems; but this investigation and its findings do not depend on the broader issues.

SSCI: A BLACK BOX GENERATING PATENTLY INCONSISTENT DECISIONS

In a 1990 essay, "How ISI Selects Journals for Coverage," Garfield wrote:

We receive a steady stream of calls and letters asking how
ISI decides what journals are covered in various [ISI index

⁷ For example, see books by Martin Anderson, W.W. Bartley, Jacques Barzun, David Clarke, David Colander, David Damrosch, George C. Douglas, Julius Getman, Paul R. Gross, Victor Hanson, John Heath, Russell Jacoby, Alan C. Kors, Norman Levitt, Lionel S. Lewis, David Lodge, Thomas Mayer, Alfred L. Malabre, Deirdre McCloskey, Camille Paglia, Edward Shils, Page Smith, John W. Sommer, Charles J. Sykes, Richard Whitley, and Bruce Wilshire.

products.] Editors in particular are the most inquisitive . . .
(Garfield 1990, 185).

Over the years ISI has issued various statements about how journals are selected for inclusion, usually mentioning many factors. But these statements are scanty and noncommittal. No single factor is sufficient, but many are presented as important or even necessary. However, examination of the journal lists and other forms of probing reveal that many of the criteria that seemed to be necessary are not, in fact, necessary. ISI has not even seen fit to issue statements specific to the diverse indices, such as Science Citation Index, Social Science Citation Index, and the Arts and Humanities Citation Index, where numerous basic differences would seem to call for criteria tailored to the rubric covered. Thus it is no wonder that ISI receives so many inquiries asking for clarification of the process. It is also noteworthy that the people chiefly interested in discerning the criteria, namely journal editors and publishers, are people disinclined to question or criticize ISI. Like pharmaceutical companies seeking approval from the Food and Drug Administration, the parties most likely to have first-hand knowledge of the process, including its disappointments, are those least likely to make noise about it. So far as I know, there has been no scholarly inquiry, examination, or criticism of ISI's journal selection practices. Here I examine the five main ISI statements that range over 30 years about the journal selection process (Garfield 1973, Garfield 1979, Garfield 1985, Garfield 1990, and Testa 2002). My examination will be supplemented by investigations done by Eric Chiang and myself.

The five statements consistently mention a number of factors ISI looks for when considering a journal for inclusion. Let's first consider several that are of a non-circular nature:

- Meeting its own publication schedule.
- Maintaining "international editorial conventions." "These conventions include informative journal titles, fully descriptive article titles and abstracts, complete bibliographic information for all cited references, and full address information for every author." "English language articles titles, abstracts, and keywords are essential." (Testa 2002, 2).
- Being peer reviewed (Garfield 1990, 12; Testa 2002, 2).
- Having broad geographic representation among the authors of the articles in the journal and of the articles cited (Garfield 1990, 10-11; Testa 2002, 2).

These criteria are diverse and vaguely worded. Does “essential” mean necessary? A cursory investigation of SSCI finds some clear inconsistencies. If (1) peer review, (2) international participation, (3) scholarly referencing, and the presentation of (4) abstracts and (5) keywords are all required, how is it that *The Nation*, *The New Republic*, and *Commentary* are included in SSCI? Most of the 17 periodicals listed later on in this paper in the top rows of Tables 1 and 2 fail to meet at least one of these criteria. It is very likely that many, many other SSCI journals also fail to meet at least one of the five criteria. If “essential” does not mean necessary, what does it mean? If it does mean necessary, ISI is not following its own rules.

Regarding peer review, we put the matter to James Testa, Director of Editorial Development at Thomson ISI. We asked him how *Commentary* and the *New York Review of Books* (which is in AHCI, not SSCI) get included in ISI indices, and he responded by email (6 May 03):

Peer review is required for all journals in the natural sciences. As one moves away from the natural sciences toward the social sciences and humanities, ‘peer review’ takes on a different meaning and importance in the process. I think this is universally understood.

Hogwash. No one would regard *Commentary* and the *New York Review of Books*—and many of the other periodicals in the “included” rows of Tables 1 and 2—as peer reviewed. And if some of the criteria simply do not apply to the social sciences and humanities, why doesn’t ISI say so?

It is important to know that certain factors are *not said* to be important in the journal selection process. First, nowhere in the five key statements examined is it said that journal age is important. Indeed, several remarks suggest that a journal that is just one or two years old is ready for full consideration (Garfield 1973, 6; Testa 2002, 2). Second, although external nomination is welcomed (Garfield 1990, 12; Testa 2002, 4), it is never said to be necessary or even important. To confirm, we asked Testa, and he replied by email (30 April 03): “Nomination serves only to alert us to a new journal if we have not already discovered it on our own.” Third, and confirmed by the same email message, size of circulation and size of the periodical are said not to be factors.

Two other factors that ISI consistently says are crucial raise serious concerns about circularity and self-legitimation. One is *the emphasis on citations*.

If a journal has existed long enough to accrue citations, its citation record is of prime importance to us in deciding whether or not it should be covered (Garfield 1979, 7).

Several types of citation data are used. For established journals, these include overall citation rate, impact factor, and immediacy index. For brand new journals, the editors examine the publishing record of the journal's authors and editorial board members, noting where their articles have been published and if their work has been cited (Testa 2002, 3).

Thus, we see citations playing a pivotal role at yet another key point in the process of academic legitimization. So far as justifying its inclusion decision goes, ISI can effectively pick itself up by its own bootstraps. If it includes a set of journals that cite each other, those journals and those scholars by definition become "significant." If it excludes a community of journals and scholars, they thereby remain insignificant.

The other factor is the *reliance on journal evaluations* by both ISI staff and ISI's "networks of advisors."

Each journal goes through an extensive evaluation process before being selected or rejected. The ISI editors performing journal evaluations have educational backgrounds relevant to their areas of responsibilities as well as experience and education in information science. Their knowledge of the literature of their field is extended by consultation with established networks of advisors who participate in the evaluation process when needed (Testa 2002, 1; see also Garfield 1979, 6; and 1990, 12).

We wrote to ISI's Manager of Corporate Communications Rodney Yancey. In our message we reproduced the Testa passage just quoted and asked a series of specific questions about whether journal selection procedures are public information. Yancey's reply, provided in its entirety in the accompanying textbox, is a concise and complete statement that ISI journal selection is a black box. It confirms that (1) the identities of ISI journal selection editors are concealed; (2) the identities of advisors are concealed; (3) the records and reviews are concealed, except that (4) the publishers and editors of the journal reviewed may receive some kind of

decision letter or report; and (5) there are no descriptions of the process other than the vague and cursory documents by Garfield and Testa that we have surveyed here (we find it remarkable that Yancey would describe Testa's piece as offering "complete details").

[Email message]

From: "Yancey, Rodney" [email address redacted]
To: 'Eric Chiang' <echiang@scu.edu>
Date: 9/23/2003 8:54:55 AM
Subject: RE: Several concerns regarding the ISI

Dear Mr. Chiang:

Please find my responses to your questions below.

- 1.) The individual editors are in the fields of the seven editions of Current Contents. Their names are [not*] given out to the general public.
- 2.) We build advisor networks as needed. They come from academia and like the names of the Thomson ISI editors, we do not disclose their names to the general public.
- 3.) Yes there are records of the reviews. However, this information is proprietary.
- 4.) The reviews and decisions are only shared with the journal publishers and editors.
- 5.) Mr. Testa's essay offers complete details on our selection process. If you would like more information on citation analysis, I invite you to visit Dr. Garfield's Web site at <http://www.garfield.library.upenn.edu/>.

Kindest regards,
 Rodney Yancey
 [Manager of Corporate Communications, Thomson ISI]

* The "not" was inadvertently omitted in the original message. Follow-up correspondence from Mr. Yancey of September 24, 2003 confirmed that he had intended to write "not" here.

Yancey's message makes clear that the only possible window on ISI's decision about a particular journal would have to come from that journal's publisher and editor, once they received a decision letter from ISI. (That may occur, as I understand it, only as a result of the journal specifically requesting a review and decision of ISI.) But this window is not in fact available to the public at large. First, there is no coordinated effort to collect

from the journals such decision letters. Moreover, in each case the journal's publisher would most likely not be willing to make public the decision letter he received. A negative letter might be thought to reflect badly on the journal. Also—and it is from more than mere speculation that I say this—the publisher would not want to antagonize ISI. Instead, he must try to stay on ISI's good side in hopes of eventually getting the journal in question or other journals he publishes included in ISI lists, or keeping them included. Again, the situation is very much like that of a pharmaceutical company receiving a New Drug Application decision from the FDA—by its monopoly in permitting all future products, the FDA has a stranglehold on the company, so the company would be reluctant to protest publicly.

We may conclude that the ISI journal selection process is a black box. Examples like *The Nation* and *The New Republic* prove that ISI's stated criteria are not criteria at all; several SSCI journals fail to meet just about every stated criterion. One might have assumed that there is a consistent method and exalted fairness—a “rule of law”—in the ISI journal selection process. Not only is there absolutely no evidence for that, there is very clear evidence against it.

WHAT IF SSCI WERE IDEOLOGICALLY BIASED?

It may be easy to show, as we have here, that SSCI is a black box, and even that it acts inconsistently. But if SSCI were inconsistent in a patterned way according to political orientation, if it were *ideologically biased*, that would be much harder to show. Here, I clarify what I mean when I say a journal has an ideological orientation, describe what the ramifications of an ideological bias in SSCI would be, and then investigate whether SSCI is ideologically biased.

“Being ideological” has two definitions, one that makes it a bad thing: pigheadedness and foolish prejudice in matters relating to political or policy judgment; and one that makes it a not-inherently-bad thing: being relatively consistent or outspoken in political or policy judgment, and perhaps passionate and motivated to explore or advance what one regards as the more enlightened political ideas. Here I am using the second definition. Being ideological is *not* inherently at odds with being scholarly, scientific, reasonable, scrupulous, fair, and so on. Indeed, since relevance and judgment are key to science, eschewing outspokenness can itself be at odds

with being scientific. Failing to consider pertinent issues and factors and failing to exercise vital judgment can be scientific *errors of omission*.

Now, suppose SSCI were biased against ideology-*i*. The ideology-*i* literature predominately—or at least disproportionately—cites the works of ideology-*i* scholars. By causing the authors to have lower citation counts, a bias against ideology-*i* journals would injure ideology-*i* scholars. And SSCI inclusion has a second significant impact: The author's publication in a journal is often deemed "peer reviewed" partly on the basis of whether the journal is included in SSCI. From our survey of Economics Department chairs we know that at about half of the schools a journal's inclusion in the SSCI is a factor (though usually not a decisive one) in deciding whether to deem a publication "peer reviewed" (Klein and Chiang 2004).

For these combined reasons, a bias against ideology-*i* journals would cause ideologically kindred scholars to have less prestige and eminence than they deserve. Such scholars might avoid the non-SSCI journals and focus their efforts on publishing in the more academic, non-ideological journals that are included in SSCI. Or they might refashion what they believe to make it palatable to ideological-but-not-ideology-*i* journals that are included in SSCI. The community of ideology-*i* scholars loses vitality because its journals are not included in SSCI, and the discourse in general suffers a decline in ideological diversity.

Another way in which a bias would impact ideology-*i* scholars is that the number of citations *to* a journal goes into what is called that journal's "impact factor." These measurements of citation to the journal feed directly into the rankings of the journals—a journal is defined as important because it is cited. Thus, any one ideology-*i* journal would have a higher impact factor if it and other ideology-*i* journals were in the SSCI.

Garfield has repeatedly reported that many editors have implored him that ISI recognition is a matter of life and death for their journals (see Garfield 1979, 5; and 1985, 3). If a number of ideology-*i* journals get included, they thereby achieve citation impact and are academically respectable, and authors get "good publication" credit for articles that appear in them. Also, more prospective authors will send their manuscripts to the journal, and more individuals and institutions will subscribe to the journal. If the ideology-*i* journals do not get included, they then lack citation impact and are not respectable, and their authors do not get "good publication" credit.

By the same token, if some ideological orientation, ideology-*k*, is *favored* by SSCI, if journals of some ideological orientation are disproportionately included in SSCI, then that literature gains academic

legitimacy across the board—impact factors, journal ranking, citation to authors, a livelier flow of manuscripts, and more subscriptions. Indeed, university definition of “peer-reviewed” sometimes hold ISI inclusion (such as in SSCI) to be a *sufficient condition* for calling a journal peer-reviewed.⁸ About a third of Economics Department chairs reported that if a journal is included in SSCI, the department would count a publication in that journal as peer reviewed unless there were some salient reason not to (Klein and Chiang 2004). A faculty member, then, gets credit for a peer-reviewed publication when he publishes an article in an SSCI journal, but not necessarily for articles in journals not included in SSCI.

A Bone to Pick

From time to time I check my SSCI citation count. This has never been a source of satisfaction. Meanwhile, I subscribe to numerous scholarly classical liberal (or libertarian) journals, including *Cato Journal*, *Critical Review*, *Economic Affairs*, *Independent Review*, *Journal des Economists et Etudes Humaines*, *Knowledge, Technology & Policy*, *Regulation*, and *Review of Austrian Economics*, and to less scholarly journals and magazines like *Ideas on Liberty*, *Liberty*, and *Reason*. This literature cites my work with some regularity. But only one of them is included in SSCI – *Critical Review*, which is much less decidedly or consistently classical liberal than the others.⁹ Maybe SSCI steers clear of ideological periodicals? Some of those mentioned aren’t scholarly journals at all. But SSCI does include a few political magazines of news commentary and opinion. And, as for more scholarly journals, a scrutiny of the SSCI list shows that plain political leanings do not necessarily disqualify a journal.

EVIDENCE OF A SOCIAL DEMOCRATIC BIAS IN SSCI

Eric Chiang and I have investigated whether SSCI is ideologically biased. We approached the universe of social science periodicals in a way

⁸ Using Google, I found several Australian universities with guidelines reading: “For journal articles, any of the following are acceptable as evidence [of peer review]: [the first bulleted item is] the journal is listed in one of the Institute for Scientific Information indexes” (Newcastle 2002). The context strongly indicates that “acceptable” here means sufficient.

⁹ The editor Jeffrey Friedman has specialized in hosting fruitful critiques of libertarianism and debates between classical and modern liberals; in fact, *Critical Review* is a core journal in the Left Index.

that first asked whether the periodical has an ideological character. We divided those with an ideological character into social democratic, conservative, and classical liberal/libertarian. The “social democratic” category is really a catchall for the “Left”—I recognize that there are important divisions within the big tent of the Left, but, for a number of reasons, we have chosen not to concern ourselves with the possibility of bias against certain camps within the Left.

It would have been nice if there were a definitive classification of periodicals by ideology, but there isn't.¹⁰ Instead we have had to assemble titles from different sources and make our own distinctions. We have attempted to be fair, transparent, and reasonably systematic. Here we present three tables of periodicals by ideological orientation and SSCI inclusion. Like the title of this paper, our investigation of ideological bias ends with a question mark. We find some evidence of ideological bias, but it is not conclusive.

At the time of our investigation during 2003, there were 1,768 journals in SSCI, and we certainly did not investigate all or even a quarter of them. When we investigated a journal, we did so via the web and only rarely consulted hard copies. The spirit of this quantitative evidence obviously is not comprehensive accounting, but rather sufficiency in raising the issue of ideological bias. I know the classical liberal and conservative periodicals well enough to know that their inclusion in SSCI has not been understated here. We have documented our claims about the social democratic periodicals well enough to know that their inclusion has not been overstated.

SSCI contains some ordinary political magazines. We distinguish between magazine and journal on the basis of submission and review policy, format, style, tone, length of articles, scholarliness, and periodicity. We have deemed those listed in Table 1 as magazines. *The Nation* and *The New Republic* are ordinary political commentary magazines that appear almost weekly. No one would consider them to be scholarly journals. *The Nation* announces that it will not even consider unsolicited manuscripts. It describes itself as a weekly “of left/liberal opinion” (thenation.com, 12 Sept 03). It is included in the Alternative Press Index and the Left Index and

¹⁰ There is no full-spectrum ideological guide to the scholarly journals. There are two major leftist indices: (1) The Alternative Press Index is a product of the Alternative Press Center; in a 1992 issue of *Library Journal*, Marie F. Jones described API as the “leading index of liberal and radical serial publications” (quoted in Alternative Press Index 2004). (2) The Left Index is a product of the National Information Services Corporation. There are also websites such as PoliticalUSA.com and Leftist Links Archive, but these websites include mainly opinion/current affairs type journals, not scholarly journals.

listed among magazines of the Left on such websites as PoliticalUSA.com and Leftist Links Archive. *The New Republic* is oriented toward the establishment and has since 1960 generally favored the Democrats, been pro-free trade, and pro-Israel. *Commentary*, published by the American Jewish Committee, is a centrist neo-conservative, pro-Israel monthly and has an explicit focus on Jewish affairs. SSCI includes no classical liberal/libertarian magazines.

Table 1: Magazines by Ideology and SSCI Inclusion

	Magazines		
	Social Democrat	Conservative	Classical Liberal (Libertarian)
Included in SSCI	<i>Nation</i> <i>New Republic</i>	<i>Commentary</i>	[None]
Not Included in SSCI	Many (see Alternative Press Index, Left Index) just 2 examples: <i>The Progressive</i> <i>Mother Jones</i>	<i>American Enterprise</i> <i>American Spectator</i> <i>Chronicles</i> <i>Human Events</i> <i>Insight</i> <i>National Review</i> <i>Weekly Standard</i> [And perhaps a few others]	<i>Economist</i> <i>Ideas on Liberty</i> <i>Liberty</i> <i>Reason</i> [And perhaps a few others]

The bottom row of Table 1 lists several conservative and classical liberal magazines not included in SSCI. If *The Nation* merits SSCI inclusion, why not them?

Next, consider all periodicals that are more properly described as journals, rather than magazines, even highly scholarly journals, *but that are plainly of an ideological character*. Our bases for saying that a journal exhibits its ideological character “plainly” are the consistency and outspokenness of the views expressed, its institutional affiliations (especially, the publisher), and the way it describes itself—with any one basis being sufficient. Being more relevant, timely, and outspoken, many of these journals are less academic than ordinary academic journals—but not necessarily less scholarly.

Table 2: Plainly Ideological Journals by Ideology and SSCI Inclusion

	Scholarly or Semi-scholarly Journals		
	Plainly Social Democrat	Plainly Conservative	Plainly Classical Liberal
Included in SSCI	<i>Alternatives Journal</i> <i>Critical Asian Stud.</i> <i>Dissent</i> <i>Econ. and Indus. Democracy</i> <i>Economy & Society</i> <i>International Labour Review</i> <i>Monthly Review</i> <i>New Left Review</i> <i>Politics & Society</i> <i>Race & Class</i> <i>Radical Philosophy</i> <i>Science & Society</i> [And probably a few others]	<i>Policy Review</i> <i>Public Interest</i>	[None]
Not Included in SSCI	Many (see Alternative Press Index) just 2 examples: <i>American Prospect</i> <i>Rethinking Marxism</i>	<i>Federalist</i> <i>Hoover Digest</i> <i>Intercollegiate Review</i> <i>Modern Age</i> [And probably a few others]	<i>Cato Journal</i> <i>Economic Affairs</i> <i>Independent Review</i> <i>J. Ayn Rand Studies</i> <i>J. Libertarian Studies</i> <i>Q.J. Austrian Econ.</i> <i>Regulation</i> [And probably a few others]

The ideological character of the social democratic journals listed in the upper left cell is evident from the way they describe themselves. *Dissent* is “a magazine of the left;” *Economy & Society* is a “radical interdisciplinary journal of theory and politics;” *New Left Review* is “a key journal of the international Left;” *Politics & Society* pursues the “development of Marxist, post-Marxist and other radical perspectives;” *Race & Class* is subtitled “A Journal for Black and Third World Liberation;” *Radical Philosophy* is “a journal of socialist and feminist philosophy;” *Science & Society* “is the longest continuously published journal of Marxist scholarship.” Appendix 1 (Panel A) provides quotations and URLs from the websites of the 12 journals listed in the upper-left cell. Of the 12, all but two are included in the Alternative Press Index (as of 27 Sept 03) and all but three had at least 40 articles indexed in the Left Index (as of 18 Nov 03).

SSCI includes two plainly conservative journals, both American. *Policy Review*, published by the Hoover Institution, focuses on international affairs

and foreign policy. *The Public Interest* offers a neo-conservative perspective on domestic social and policy issues. Both of these journals are quite establishment oriented. SSCI includes no plainly classical liberal journals.

Now consider journals that are reservedly ideological and consistently of a more academic nature. In Table 3 we have joined conservative and classical liberal into one column. Most of these journals fashion themselves as serious academic journals. Almost none of the journals listed in Table 3 would ever be found in any bookstore. Nonetheless, each of these journals seems to exhibit an ideological orientation, though in a way more reserved than those of the previous two tables.

Table 3: Reservedly Ideological Journals by Ideology and SSCI Inclusion

	Journals that are:	
	Reservedly Social Democrat	Reservedly Conservative or Classical Liberal
Included in SSCI	[Depending on the benchmark for biasedness, the number of journals belonging to this cell could be anywhere from 75 to many hundreds . See the list of 83 journals in Appendix 1B.]	<i>Am. J. Econ. & Soc.</i> <i>Critical Review</i> <i>J. Inst. and Theoretical Econ.</i> <i>J. Law & Econ.</i> <i>J. Legal Stud.</i> <i>Kyklos</i> <i>Public Choice</i> <i>Social Phil. & Policy</i> [And perhaps a few others]
Not Included in SSCI	Many	<i>Academic Questions</i> <i>Constitutional Pol. Econ.</i> <i>J. Economistes et Etud. Hum.</i> <i>Knowledge, Tech. & Pol.</i> <i>Planning & Markets</i> <i>Rev. Austrian Econ.</i> <i>Humanitas</i> [And probably a few others]

With 1,768 journals in SSCI, it is impractical to try to give a comprehensive account of the journals with some ideological character. Again, the spirit here is evidentiary sufficiency.

The main issue for Table 3 is how to define having an ideological orientation. We usually say that an organization is oriented toward a particular ideology if, *relative to the norm*, it dwells on, expresses, or espouses

the sensibilities of that ideology. But in assessing the matter for, say, a top ranked sociology journal, what is the relevant norm? The general population of the U.S.? Academics in general? Or sociologists? This question of the benchmark is central because it is well established that social democratic sensibilities dominate the social sciences and humanities.¹¹

As far as the ordinary Republican citizen is concerned, if he could penetrate the academic journals at all he would probably regard a significant portion of the anthropology, history, political science, and sociology journals as having a social democratic orientation, and surely the same can be said of psychology, law, communications, environmental studies, education, philosophy, planning, social work, public health, gender studies, ethnic studies, and cultural studies—that is, every social-science or humanities discipline except economics.¹² In the top ranked sociology journal, a large portion of its articles exhibit a focus on race, class, and gender in a manner typical of social democratic academics. Leon Bramson (1961, 16-18, 51-52, 85-86) and Edward Shils ([1978], 141-42) suggest that many of the social science disciplines developed in the United States as projects to marshal science in the service of melioristic reforms and were oriented in their fundamental outlook toward collectivist ideas and government intervention. In Europe sociology was more directly rooted in socialism and anti-liberalism (Bramson 1961, 11-18, 48-50). Even within the discipline of economics, the ordinary Republican citizen would regard many journals to have a leftist or “liberal” orientation.

The three salient benchmarks are the general population, academic social science in general, and the particular discipline or field. In Appendix 1 (Panel B), we list 83 journals that we have deemed to be “reservedly social

¹¹ One indication of the ideological lopsidedness in academia is political-party affiliation ratios among the faculty. Voter registration studies done by the Center for the Study of Popular Culture and others report extreme lopsidedness in Democrat v. Republican ratios. I am involved as a principal author in two separate scholarly investigations of this matter, one being a meticulous treatment of voter registration among faculty at two top universities, and the other being a large scale survey of academics asking them what party they vote for. Both studies are in progress, but the data collection is complete in one case and nearly complete in the other. The upshot is that the lopsidedness is extreme. I expect that both the voter registration findings and the survey findings will be released by the end of 2004.

¹² Irving Kristol (2000: A 26) writes: “The feminine, maternal version of the welfare state [that is, social democracy] now has the support . . . of institutions and professions that have been nourished by this state. . . . These are now designated collectively as the ‘helping professions,’ and include social work, nursing, psychology, public health, librarianship, teaching, and branches of TV journalism.” As of 13 August 2003, there were 15 SSCI journals with “social work” in the title, 128 with “psychology,” and 83 with “health.”

democratic” in orientation. In these cases we have deemed the orientation “reserved” rather than “plain” on the grounds that in its self-description on the web the journal does not seem to make the ideological point of view as central to its character as do the plainly ideological journals. The “social democratic” designation is based on any of three criteria: (1) their self-descriptions on the web; in many cases we have provided in Appendix 1B relevant quotations and URLs; (2) inclusion in the Alternative Press Index; or (3) inclusion in the Left Index with at least 40 records (that is, articles) indexed.¹³ The list of 83 was developed by pointed search and investigation, such as by inclusion in the leftist periodical lists (such as Alternative Press Index and the Left Index), by social-democracy oriented publishers, and by journal title-words that correlate to a social-democrat orientation. On the sheer basis of posted self-description, continued investigation would surely expand the list. We did not examine *the contents* of journals. But, in principle, one could examine the contents of every one of the 1,768 SSCI journals and assess its contents for overall ideological orientation, and on that basis quite possibly add hundreds of journals—again, depending on the benchmark used—to the list of reservedly social democratic journals.

It is on that broader basis that specific journals are listed as conservative or classical liberal in the right column of Table 3. There is nothing in the self-descriptions of *Journal of Law & Economics*, *Journal of Legal Studies*, *Public Choice*, and *Kyklos*, for example, that indicates their ideological orientation, except that, relative to academic social science in general, “law and economics” and “public choice” themselves indicate a classical liberal orientation. Thus, *there is an important asymmetry between the bases for listing the social democratic journals in Appendix 1B and the journals in the right column of Table 3*. If one were to apply symmetrically the basis upon which *Journal of Legal Studies* is listed in the right column, one might well add *Stanford Law Review*, *California Law Review*, and many, many other law journals to Appendix 1B (as of July 03 SSCI included 90 journals with “Law” in the title). To carry out the symmetric investigation would be extremely time consuming and impossible to verify on a broad plain.

As for the completeness of the listings in the top right cell of Table 3, I’m pretty sure it is reasonably complete. Bear in mind that many journals are too dry to be regarded as ideological at all. For example, *Southern*

¹³ We excluded from Appendix 1 one journal that had more than 40 records in Left Index: *Critical Review* had 396 records. As noted earlier, that journal is devoted basically to exchange between social democratic and classical liberal ideas. It is, in fact, more appropriate to view it as a classical liberal journal.

Economic Journal, the journal of the Southern Economic Association, has traditionally been more congenial to classical liberal ideas than the average social science journal or even the average economics journal. But, by and large, the articles in the journal are narrow and highly technical. One could choose to add *SEJ* to the top-right list, but correspondingly one then would have to add scores of journals to the top-left list.

In the bottom right cell we list six journals that are reservedly conservative or classical liberal and not included in SSCI (*Planning & Markets* is an electronic journal, and ISI seems to consider electronic journals equally (Testa 2002, 3)). There are probably a few others that could be listed in that cell.

The ideological lopsidedness of academics in general might get ISI off the hook. ISI might respond that SSCI includes a lot of social democratic journals because there are a lot of social democratic journals, and it includes few classical liberal journals because there are few classical liberal journals. The general lopsidedness of academics surely goes a long way in explaining the lopsidedness of the first row of Table 3.

Eugene Garfield's Ideological Orientation

Again, ISI was built and directed for more than 30 years by Eugene Garfield. In the introduction to a volume honoring Garfield, Cronin and Atkins write, "For many people, the name Garfield is synonymous with citation indexing, an idea that he championed indefatigably for more than four decades" (2000, 1).

The scrutiny of personal character and values is often said to be an illegitimate form of evidence or argument, and disparaged as an "*ad hominem* attack." Indeed, it is often unpleasant and invidious to scrutinize an individual, and hence inappropriate. But everyone knows that character and purpose are vitally relevant and meaningful. Considerations of motive are crucial in reaching court decisions, and rhetoric scholars recognize that the wholesale rejection of "*ad hominem*" arguments is a fallacy (e.g., Walton 1985). When getting a better reading of a matter is important, and the cold hard facts do not speak clearly, "circumstantial evidence" and the analysis of motives are especially crucial. Whether people realize it or not, Eugene Garfield and his associates have played a major role in shaping the definition and pursuit of the social sciences. They have played a major role in the machinations that determine which scholars, which ideas, and which values attain legitimacy and eminence. It is appropriate to ask if a social

democratic bias in SSCI would conform to Garfield's own ideological orientation. Hence we have investigated Garfield's copious writings for ideological indicators. For decades Garfield wrote a wide-ranging, often political column for ISI's periodical *Current Contents*. There is significance in the fact that Garfield editorialized freely in his journal, officially devoted to scientometrics, for it shows his tendency to personalize the larger mission and to use it for wider causes. Moreover, it meant that Garfield put his own views and personal history into the public domain; more recently, he has made all of these writings available on his website.

Garfield's columns clearly exhibit a social democratic orientation, in advocating expanded welfare-state programs, nationalization of certain services, and new ambitious regulatory interventions. We have gathered 14 sample quotations, with citation, in Appendix 2.

The relevance of this "ad hominem" material is for the reader to decide. Whether Garfield personally signed off on each journal for inclusion in SSCI, I don't know. More likely, he delegated much of that to ISI staff. But during his 30 plus years up through 1992 as ISI chief, and continuing today as Chairman Emeritus, if he did not make the journal decisions himself, he surely did influence the selection of the top people making those decisions. We offer these facts about the man who has been the guiding force of ISI as something to consider within a broad set of information about SSCI and ISI's journal selection process.

Are We Talking about a Significant Number of Citations?

Again, if SSCI is ideologically biased, that will impact the ideology favored and the ideology slighted. Here we provide some numbers.

ISI produces a product called Journal Citation Reports that tallies citations by a journal's articles. That is, it tallies how many citations a journal generates (meaning how many articles it cites), including citations to books, manuscripts, and journals not in ISI's databases. Of course, the report is made only for ISI-indexed journals, such as those in SSCI. At Stanford University we accessed Journal Citation Reports to see how many citations the social democratic periodicals listed in Tables 1–3 generate.

Table 4: Citation Generation by a Sample of Social Democratic Journals

Social Democratic Periodical	Number of Citations Generated 2002 ¹⁴
Journals from Table 1	
<i>Nation</i>	170
<i>New Republic</i>	150
Selected Journals from Table 2	
<i>Alternatives Journal</i>	854
<i>Dissent</i>	56
<i>Economic and Industrial Democracy</i>	964
<i>International Labour Review</i>	464
<i>New Left Review</i>	678
<i>Race & Class</i>	366
<i>Radical Philosophy</i>	400
<i>Science & Society</i>	696
Selected Journals from Table 3	
<i>Cambridge Journal of Economics</i>	1,953
<i>Feminist Economics</i>	718
<i>Journal of Economic Issues</i>	2,126
<i>Journal of Post-Keynesian Economics</i>	916
<i>Labor History</i>	927
<i>Review of International Political Economy</i>	1,630
<i>World Development</i>	6,078

The table reports the citation-generation numbers for the two social democratic magazines, several of the plainly social democratic journals, and a sample of the reservedly social democratic journals from Appendix 1. This is just a sample that has been selected haphazardly and is probably representative, though we wanted to be sure to show here the remarkable number for *World Development*, which alone each year creates 6,078 citations

¹⁴ A detail: As was confirmed by an email message (May 27, 2003) from ISI Technical Support Representative Chris Pasquini, the number reported here is the number of citations generated by articles *indexed*—not published—in 2002. For example, if ISI does not receive and index the December 2001 issues of *The Nation* until early 2002, then the citations generated by those issues will be counted in the 2002 number, not the 2001 number. This difference between indexed and published would even out if the process remained the same year after year.

for the scholars cited in that journal, and boosts the impact factors of the cited journals. A complete listing of counts for the 98 SSCI social democratic journals shown of Tables 1, 2, and 3 is provided in Appendix 1 (in a few cases the JCR did not supply the number).

The numbers for the non-SSCI classical liberal journals are not, of course, available from Journal Citation Reports because they are not included in SSCI; so we have manually counted the number of citations generated in 2002 (for a subset of the classical liberal journals appearing in Tables 1, 2 and 3).

Table 5: Citation generation by a Sample of Classical Liberal Journals

Classical liberal periodical	Number of citations generated 2002
Selected Journals from Table 1	
<i>Ideas on Liberty/Freeman</i> ¹⁵	535
<i>Reason</i>	55
Selected Journals from Table 2	
<i>Cato Journal</i>	780
<i>Economic Affairs</i>	615
<i>Independent Review</i>	934
<i>J. of Libertarian Stud.</i>	864
<i>Q. J. of Austrian Econ.</i>	646
<i>Regulation</i>	198
Selected Journals from Table 3	
<i>Constitutional Pol. Econ.</i>	667
<i>J. Economistes et Etud. Hum.</i>	882
<i>Knowledge, Technol. & Pol.</i>	722
<i>Planning & Markets</i>	119
<i>Rev. of Austrian Economics</i>	808

If these journals were included in SSCI, then the cited scholars would enjoy higher citation counts and the cited journals would enjoy higher impact factors.

¹⁵ *Ideas on Liberty/Freeman* uses endnotes. We counted citation as if the journal used Reference List style—that is, even if an article had one endnote citing Hayek 1960, p. 53 and another endnote citing Hayek 1960, p. 133, we counted just one citation to Hayek 1960 (SSCI would count that as two citations to Hayek 1960).

Being on Schedule: A Factor that could Explain the Seeming Pattern

Even if we agree that there seems to be an ideological pattern in SSCI inclusion, we must keep in mind the possibility that it is just a result of chance. In particular, one criterion said to be important for SSCI inclusion is being on schedule. I have not done a systematic investigation of whether the classical liberal journals have been reasonably punctual. However, I can say that as a subscriber of many of them, I have been aware of publication lags for only two or three of them. Also, I suspect that it is common among SSCI journals to experience occasional publication lags. The only way to find out would be to do a journal-by-journal investigation, and it is unlikely that the journals themselves would readily cooperate with such an investigation and reliably report on their own punctuality.

Ways of Correcting the (Alleged) Bias

We have provided a variety of evidence of social democratic bias in SSCI. Perhaps the matter will become clearer with further investigation or a response by ISI.

If we are to conclude that such a bias exists, how could ISI correct it? One way would be to weed out overt ideological orientation and judgment by trimming the current SSCI journal list. This would mean deleting a lot of social democratic periodicals.

In my opinion, it would be better to embrace the social sciences and humanities as, not consensus-oriented, but dialogue-oriented, in which policy relevance, judgment, outspokenness, and debate are essential, and therefore expand rather than truncate the list. A good place to start would be by adding to SSCI many of the conservative and classical liberal periodicals listed in the “not included” rows of Tables 1, 2, and 3.¹⁶

A third way deserves serious consideration: ISI could make inclusion an option that any periodical meeting minimal requirements may exercise by paying a fee to ISI and regularly providing its formatted data to ISI. This would avoid any problems or allegations of biasedness, since its treatment of journals would be truly equal. This would seem to be good business—ISI would sell its products as usual, but now would also collect fees from

¹⁶ Of those, the following are included in EconLit as of 11 February 2004: *American Enterprise*, *Cato Journal*, *Constitutional Political Economy*, *The Independent Review*, *Regulation*, *Quarterly Journal of Austrian Economics*, and *Review of Austrian Economics*.

the journals. Would the product altered in this way be less in demand? I don't think so. I think it would be *more* in demand. Citation analysis could continue to serve the vetting and ranking functions for which people now look to SSCI. The scholars evaluate journals by citing or not citing them, participating in or shunning them, and by speaking well or ill of them. We do not also need ISI to vet and certify journals preemptively. Furnish the full data and diverse rankings will emerge and compete in a more spontaneous fashion.

We should bear in mind that the idea of reducing the apparent ideological lopsidedness in the SSCI by trimming the over-represented sides or adding to the under-represented sides is limited under ISI's current practice with respect to retroactive citations. Under current ISI practice, if ISI were to drop *The Nation* from SSCI today, all the citation from *The Nation* up to today would remain in the system. And if ISI were to add *Constitutional Political Economy* today, it would only start counting citations from that journal beginning today. The legacy of lopsidedness would remain in the system.

Finally, if it seems that ISI is biased and will persist in this fashion, maybe someone else should get into the business and do it right. The ambitious plan would be to replicate SSCI entirely but correct the bias. A less ambitious and seemingly quite feasible project would be to create a corrective supplement to SSCI. The social democrats have the Alternative Press Index and the Left Index, although these products, like EconLit, are not *citation* indices—not *yet*, that is. Classical liberals (along with conservatives) could embark on compiling their own citation index, the Social Affairs Citation Index (SACI). A classical liberal citation index covering the classical liberal periodicals could be promoted as a supplement to the ISI citation indices (particularly, SSCI and the Arts and Humanities Citation Index), designed to advance intellectual diversity and prevent ideological discrimination. For tenure cases and the like, the individual could submit his SACI citation count along with his SSCI (or AHCI) count. If SACI provided tallies of citations *not counted by SSCI (or AHCI)*, the individual could conveniently assure reviewers that there is no double-counting in summing the SSCI and SACI numbers. In time, the proposed classical liberal citation index might come to be recognized and utilized in institutional machinations.

CONCLUSION

When I hear someone telling a private company how to run its business, I usually figure he's just a loud mouth. The loud mouth doesn't have the local knowledge that the company's officers have. The loud mouth has little appreciation of the systemic effects of the changes he urges.

Yet here I am telling the Thomson Corporation to make SSCI more consistent, transparent, and ideologically neutral. In some cases it is appropriate to criticize private companies. Thomson ISI is a major player in the world of science. It seems to me that our esteem for ISI ought to hinge on its abiding by the scientific norms of consistency and transparency.

And even if ISI's opaque, inconsistent practices have some explanation—beyond the outsider's comprehension—this investigation suggests that the rest of us ought to question more seriously the meaning of ISI citation counts.

APPENDIX 1:

[Linked Excel Spreadsheet:
Panels A and B](#)

APPENDIX 2:

14 Passages from Eugene Garfield that Project a Social-Democrat Point of View (followed by source)	
"My uncles were Marxists. One of them gave me Bernal's book 'The Social Function of Science' when I was 14 years old. It may have had some influence on me."	
	Deeds and Dreams of Eugene Garfield; <i>Chemical Intelligencer</i> , Oct 1999, p.26-31; Quote on p.28. [URL: http://www.garfield.library.upenn.edu/papers/chemicalintelligencerp26y1999.pdf] [DA: 6/6/03]
"Unfortunately, the US ranks well below other developed nations in its support for working parents. In the past decade, the US government has failed to enact any legislation to provide comprehensive child care. And many of the day-care programs that had been serving the poor have been eliminated."	
	Child Care: An Investment in the Future. Part 1; Current Comments column in <i>Current Contents</i> , Vol. 6 Feb 7, 1983, p.5-11; Quote on p.7. [URL: http://www.garfield.library.upenn.edu/essays/v6p031y1983.pdf] [DA: 6/6/03]
"The Yellow Cab Company chain is a small fraction of what could constitute a national system, and we need one. There ought to be federal standards of safety, courtesy, and comfort, if not uniformity of prices and service."	
	Confessions of a Cab Driver; Current Comments column in <i>Current Contents</i> , #20, May 16, 1977, p.5-7; Quote on p.6. [URL: http://www.garfield.library.upenn.edu/essays/v3p116y1977-78.pdf] [DA: 6/6/03]
"Massive cuts made in the US mental health and social services budgets preclude any additional aid to these people. And cuts in funding for social science research will, of course, mean that their plight won't be investigated."	
	Of Beggars, Bagladies, and Bums; Current Comments column in <i>Current Contents</i> , #6, Feb 8, 1982, p.5-15; Quote on p.414. [URL: http://www.garfield.library.upenn.edu/essays/v5p395y1981-82.pdf] [DA: 6/6/03]
"For example, Demark is considered a leader in waste management. It's Kommunekemi, or "community chemical" plant, is run by a government-owned waste-management firm that also profits from consulting with other countries."	
	Hazardous Waste. Part 2; Current Comments column in <i>Current Contents</i> , #36, Sept. 8, 1986, p.3-10; Quote on p.5. [URL: http://www.garfield.library.upenn.edu/essays/v9p264y1986.pdf] [DA: 6/6/03]

"US legislators could learn from the Seveso Directive, which shows that a uniform approach is more effective than the current US situation of having different hazardous-waste laws in each state."	
	Hazardous Waste. Part 2; Current Comments column in <i>Current Contents</i> , #36, Sept. 8, 1986, p.3-10; Quote on p.6 [URL: http://www.garfield.library.upenn.edu/essays/v9p264y1986.pdf] [DA: 6/6/03]
"It is not inconceivable that in this century basic telephone service, like health services, might come to be regarded as a fundamental right provided by government."	
	What This Country Needs is a Free Phone Call; Current Comments column in <i>Current Contents</i> , #38, Sept. 19 1977, p.5-10; Quote on p.7. [URL: http://www.garfield.library.upenn.edu/essays/v3p226y1977-78.pdf] [DA: 6/6/03]
"The educational infrastructure of the country urgently requires both strong leadership and many more federal dollars to stem its decline."	
	The Military Threat to R&D; <i>The Scientist</i> , 1(15):9, June 15, 1987, pp. [not given][URL: http://www.garfield.library.upenn.edu/essays/v14p243y1991.pdf] [DA: 6/6/03]
"The lack of federal support for instrumentation and facilities, which can be traced as far back as the late 1960s, has ben left unaddressed and has prompted many universities to pursue the pork-barrel route to federal funds."	
	The Military Threat to R&D; <i>The Scientist</i> , 1(15):9, June 15, 1987, pp. [not given]. [URL: http://www.garfield.library.upenn.edu/essays/v14p243y1991.pdf] [DA: 6/6/03]
"The scientific communitiy has a long-standing commitment to political activism concerning matters of conscience, but this new attention to the politics of science funding, is in my view, welcome and overdue."	
	Scientists Must Learn to Lobby; <i>The Scientist</i> , 1(12):9, May 4 1987, pp. [not given]. [URL: http://www.garfield.library.upenn.edu/essays/v14p234y1991.pdf] [DA: 6/6/03]
"Of course, labeling regulations as they now exist are not all bad. They guarantee that with all the processing our good goes through these days, we'll have, at least, an indication of some of the things used."	
	Toward Ending the Confusion Surrounding Food Additives; Current Comments column in <i>Current Contents</i> , #24, June 11, 1979, p.5-17; Quote on p.12. [URL: http://www.garfield.library.upenn.edu/essays/v4p180y1979-80.pdf] [DA: 6/6/03]

	<p>"Two bills, now in the Pennsylvania General Assembly, would allocate funds from the sales of cigarettes for such worthwhile programs. One would place a 1 cent per pack tax on cigarettes to be paid to the Pennsylvania Cancer Control and Research Fund. The other would place a 1 cent per pack tax on cigarettes to be paid to the state's Department of Health for grants and low interest loans for the payment of cancer treatment."</p>
	<p>Nicotine Addiction is a Major Medical Problem; Current Comments column in <i>Current Contents</i>, #31, July 30, 1979, p.5-13; Quote on p.12. [URL: http://www.garfield.library.upenn.edu/essays/v4p229y1979-80.pdf] [DA: 6/6/03]</p>
	<p>"The philanthropic foundation is very much a part of the American way of life. If, however, as I have suggested above, it can't emotionally espouse the cause of basic scientific research, then I propose that we enlist another phenomenon at which Americans show great skill: the political lobby. . . . Since the Heart, Cancer, Fibrosis, etc. organizations must know of their dependence on basic research, I suggest that it would be sensible for them to do exactly what they would do if they were 'business' organizations rather than 'scientific' organizations: they would support a lobbyists in Washington to promote support of their common need, basic research. They might even be shrewd enough to instruct such a lobbyist to vigorously promote support of <i>any</i> specific research that currently strikes the public's and the Congress's fancy, so as to strengthen the scientific research front at every point possible. For example, such a lobby might have worked full time to prevent the recent disastrous cuts in training programs." (p. 6)</p>
	<p>We Need a Lobby for Basic Research; Here's How It Might Be Done; Current Comments column in <i>Current Contents</i>, #11, March 14, 1973, p.5-7; Quote on p.6. [URL: http://www.garfield.library.upenn.edu/essays/V1p418y1962-73.pdf] [DA: 6/6/03]</p>
	<p>"In my capacity as President of the Information Industry Association, I have prepared a proposal to create legislation for establishment of a National Information Funding Authority. Through this Authority, funds would be channelled [<i>sic</i>] directly to information consumers so that each one could choose and test from the variety of commercially available information services those most relevant to his needs. Direct stipends would be allocated to scientists based on simple criteria, such as size of research grant, number of scientists to be served, etc. . . . Eventually I would hope that this approach would be adopted by international organizations in developing information consciousness and utilization in developing countries." [This quote comes at end of the article.]</p>
	<p>Since Information Isn't Free, Why Not Direct Stipends to Prime the Information Pumps?; Current Comments column in <i>Current Contents</i>, #43, October 25, 1972, pp. [not given]. [URL: http://www.garfield.library.upenn.edu/essays/V1p372y1962-73.pdf] [DA: 6/6/03]</p>

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INVESTIGATING THE APPARATUS

Citation Counts and SSCI in Personnel Decisions: A Survey of Economics Departments

Daniel B. Klein and Eric Chiang*

[ABSTRACT, KEYWORDS, JEL CODES](#)

THIS INVESTIGATION IS AN AUXILIARY TO OUR PAPER ABOUT the Social Science Citation Index (SSCI), which appears in the same issue of this journal (Klein and Chiang 2004). In researching that paper, we sought evidence on the extent to which economics departments utilize SSCI citation counts in hiring and promotion decisions, and on the weight given to a journal's inclusion in SSCI in deciding whether a publication is to be deemed "peer reviewed." Because we could not find any existing empirical evidence on those matters we undertook this survey. Our method was simply to send an email message containing the survey to departmental chairs and request a response.

We stratified our sample according to a ranking of economics department. The purpose of stratifying was to see if the importance of citations and SSCI status varied with the eminence and prestige of the department. We created three groups of departments—A, B, and C—by marking off three points (1st, 61st, and 111th) in a worldwide top-200 ranking

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We are grateful to the 30 economics department chairs who responded to our survey.

of economics departments (Coupé 2003: Table 2); the ranking is constructed by averaging 11 methodologically-distinct rankings, each based on publication output. We proceeded to solicit responses from American departments in each group until 10 responses were received. Thus, we have three sets of 10 responses, or a total of 30 responses. The surveying occurred during November and December 2003. The responding universities are listed in Table 1.

Table 1
Departments that Participated in the Survey

Groups	Responding Universities
A10 Formed by working down from the 1st position in the ranking.	Harvard University University of Pennsylvania Stanford University Massachusetts Institute of Technology University of California-Berkeley Northwestern University Yale University Princeton University University of California-Los Angeles New York University
B10 Formed by working down from the 61st position in the ranking.	University of California-Irvine Boston College Iowa State University North Carolina State University Dartmouth College University of California- Santa Barbara Florida State University University of Houston Louisiana State University University of Connecticut
C10 Formed by working down from the 111st position in the ranking.	Tulane University American University State University of New York-Buffalo University of California-Santa Cruz Rice University Emory University Wayne State University University of Missouri University of Alabama University of Miami (Florida)

RESULTS

The results are presented in Table 2. Highlights follow.

- For the 11 multiple-choice questions, the average number of responses is 29.
- 15 respondents report that citation counts are usually or always considered in promotion decisions.
- Only 2 respondents report that citation counts are part of annual evaluations.
- 14 respondents report that the trend over the past 10 years has been toward putting greater weight on citation counts, while one said the trend was toward less weight.
- 14 respondents indicate that he or she expects the trend over the next 10 years to be toward greater weight on citation counts, while one expects the trend to be toward less weight.
- 20 respondents indicate that he or she thinks citation counts are currently given about the right amount of attention and weight.
- The responses indicate that at about half of the schools a journal's inclusion in the SSCI is a factor (though usually not a decisive one) in deciding whether to deem a publication "peer reviewed."
- The sample sets are small, but the results might suggest that the top schools (the A10 group) are somewhat less concerned with citation counts (in particular, see Question 3).
- There are signs that the middle group (B10) puts more weight on citation counts (compare response rates at 1D, 3B, 4A, and 8C). This might be interpreted as an anxiousness or inducement to be like the higher ups.

Table 2: SSCI Use Survey Questions and Responses

Questions	Results by Group			
1. Which sentence best describes your department's common practice <i>IN HIRING</i> ? Put an x after (that is, immediately below) your selection.				
	A10	B10	C10	ALL
As a matter of procedure, the department obtains the candidate's citation count, and this information is made common knowledge to voting members of the department	0	2	2	4
Obtaining and distributing a citation count is not a strict matter of procedure, but usually someone does come up with a citation count and the information is brought up and considered as a factor.	3	3	2	8
Obtaining and distributing a citation count is admissible information and does occur sometimes, but is not common.	3	5	1	9
Any discussion of the candidate's citation count is rare. Usually, there is no information or discussion of the candidate's cites.	4	0	5	9
Total Responses: 30				
<i>Authors' comment about the previous question: The question should have distinguished entry-level from senior-level hiring. Two of the "a" respondents specified that the response was with respect to senior hires, and two of the "d" respondents specified that it was with respect to entry level hiring. Clearly, in answering the question with respect to senior hiring, the responses would be more toward "a," and for entry-level toward "d".</i>				
2. Apart from formal procedures and discussions at meetings, consider what matters in the mind of the individual voting member of your department when you are hiring. Please x after the statement that best captures your own impression.				
	A10	B10	C10	ALL
One way or another the individual faculty member usually gets at least a ballpark estimate of the candidate's citation count, and that factor BY ITSELF is significant in his or her decision.	1	2	2	5
One way or another he or she gets at least a ballpark estimate, but that factor is not significant in his or her decision.	1	2	3	6
It is not common that individual voting				

members have even a ballpark estimate of the candidate's citation count.	8	5	5	18
Total Responses: 29				
<i>Again, like Q. 1, the previous question is seriously flawed because it does not separate entry-level from senior-level hiring.</i>				
3. Which sentence best describes common practice IN PROMOTION?				
	A10	B10	C10	ALL
As a matter of procedure, the department obtains the candidate's citation count, and this information is made common knowledge to voting members of the department.	1	1	2	4
Obtaining and distributing a citation count is not a strict matter of procedure, but usually someone does come up with a citation count and the information is brought up and considered as a factor.	2	6	3	11
Obtaining and distributing a citation count is admissible information and does occur sometimes, but is not common.	3	2	4	9
Any discussion of the candidate's citation count is rare. Usually there is no information or discussion of the candidate's cites.	4	1	1	6
Total responses: 30				
A SIMILAR QUESTION, BUT FOR ROUTINE ANNUAL EVALUATION:				
4. Which sentence best describes common practice FOR ANNUAL EVALUATION?				
	A10	B10	C10	ALL
As a matter of procedure, the department obtains the candidate's citation count, and this information is part of the routine annual activity reporting and evaluation.	0	2	0	2
A citation count is not a formal part of annual activity reporting and evaluation.	8	8	10	26
Total Responses: 28				
A QUESTION ABOUT THE UTILIZATION OF CITATIONS BEYOND YOUR DEPARTMENT BUT WITHIN YOUR UNIVERSITY:				

5. Looking beyond the department, for hiring and promotion, do the Dean, the university rank and tenure committee, the administration, and so on, utilize citation counts (check one):				
	A10	B10	C10	ALL
Yes, very typically, if not absolutely strictly.	0	1	3	4
Generally yes, but it is hit or miss.	3	1	3	7
Not typically.	1	5	1	7
Rarely.	5	2	3	10
Total responses: 28				
6. Which statement best expresses your impression about the trend at academic institutions such as yours over the past ten years?				
The utilization and weight given to citation counts has been increasing.	2	6	6	14
The utilization and weight given to citation counts has stayed about the same.	6	4	4	14
The utilization and weight given to citation counts has declined.	1	0	0	1
Total responses: 29				
A QUESTION ABOUT THE FUTURE:				
7. Which statement best expresses your guess about academic institutions such as yours over the next ten years?				
The utilization and weight given to citation counts will increase.	1	7	6	14
The utilization and weight given to citation counts will stay about the same.	8	3	4	15
The utilization and weight given to citation counts will decline.	1	0	0	1
Total responses: 30				
AN OPINION QUESTION (Your response will be kept unidentified per your preference given in Q12 below):				
8. Which statement best expresses your opinion about the utilization and weight that academic institutions such as yours give to citation counts?				
Cites are currently given too much attention and weight.	1	1	0	2
Cites are currently given about the right amount of attention and weight.	7	4	9	20

Cites are currently given too little attention and weight.	2	4	0	6
I don't have a well formed opinion on the matter.	0	0	1	1
Total responses: 29				
TWO QUESTIONS ABOUT WHETHER A JOURNAL'S INCLUSION IN SSCI IS AN IMPORTANT FACTOR IN ITS BEING A "GOOD" JOURNAL.				
THE SSCI INDEXES ABOUT 1750 JOURNALS. AN SSCI JOURNAL IS ONE FOR WHICH THE SSCI COUNTS AND INDEXES THE CITATIONS THERE GENERATED.				
[WE RECOGNIZE THAT OPTIONS FOR Q9 AND Q10 OVERLAP AND YOUR ANSWERS MIGHT BE REPETITIVE.]				
9. IS SSCI A NECESSARY CONDITION FOR "PEER REVIEWED"?				
Please evaluate the following statement: At your institution (within or beyond the department), for a journal to be regarded as peer-reviewed and publications there as peer reviewed publications, the journal must be included (indexed) in SSCI. In other words, at your institutions SSCI inclusion is NECESSARY to make a faculty members' work in that journal count as a peer reviewed publication.				
	A10	B10	C10	ALL
That is correct.	1	1	0	2
That is not strictly the case, but by and large a journal would not be counted as peer reviewed if it is not included in SSCI.	4	2	1	7
My institution readily recognizes journals as peer reviewed even though SSCI inclusion is lacking, but SSCI inclusion does help in leading us to count it as peer reviewed.	0	3	3	6
SSCI inclusion is not a factor in recognizing a journal as peer reviewed.	4	4	5	13
Total Responses: 28				
10. IS SSCI A SUFFICIENT CONDITION FOR "PEER REVIEWED"?				
Please evaluate the following statement: At your institution if a journal is include (indexed) in SSCI, then that journal is regarded as a peer-reviewed journal. In				

other words, at your institutions SSCI inclusion is SUFFICIENT to make a faculty members' work in that journal count as a peer reviewed publication.				
	A10	B10	C10	ALL
That is correct. If a journal is included in SSCI, we automatically count it as peer reviewed.	0	0	1	1
That is broadly correct: If a journal is included in SSCI, we would count it as peer reviewed unless there were some salient reason not to.	2	5	1	8
SSCI inclusion is not sufficient, but it helps.	0	3	3	6
SSCI inclusion is not a factor in recognizing a journal as peer reviewed.	4	4	5	13
Total responses:27				
INVITATION TO MAKE OPEN-ENDED COMMENTS				
11. Would you like to make any clarifications or related comments about the utilization of cites or about this survey? If you would like to receive notice of the posted results of this survey, please indicate that here.	[Nine respondents wrote something.]			
YOUR PREFERENCE REGARDING BEING IDENTIFIED				
12. Would like to keep the source of your responses unidentified? Please check one of the following options:				
	A10	B10	C10	ALL
There is no need to keep any of my responses unidentified.	1	3	4	8
Keep my responses unidentified.	8	7	6	21
Other [write in your instructions]	0	0	0	0
Total responses: 29				

CONCLUSION

Though not the case for all schools, for the economics profession on the whole citation counts play a significant role in personnel decisions, and a journal's SSCI inclusion plays a significant role in deciding whether a publication is "peer reviewed." The trend has been toward putting greater weight on citation counts.

Investigating the behavior of economists is always ripe with irony. Many economists maintain an official belief that policy judgments must be backed up by "serious research" or are otherwise mere "advocacy." Yet there seems to be very little research to back up the policies that economists actually pursue as academics and administrators. In particular, we are aware of no economic work that suggests that the profession's common practices are led by an invisible hand to promote social welfare. This modest paper documents what some of the profession's practices are. Assessing those practices calls out for scholarly attention and discussion.

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ECONOMICS IN PRACTICE

In Sweden, Anti-Globalizationists Dominate Public Discourse, Econ Profs Do Little

PER SKEDINGER* AND DAN JOHANSSON**

[Abstract, Keywords, JEL Codes](#)

ALONG WITH RESEARCH AND TEACHING, THE “THIRD DUTY” OF researchers at Swedish universities is the dissemination of knowledge to the public. This obligation is part of the Professors’ Code of Honor—approved by the Association of Swedish Professors in 1993—which also emphasizes the importance of objectivity in the public debate. Frey (2000) argues that crude views on public policy are advantaged to the extent that sophisticated scholars do nothing, and that the participation of researchers should improve the public debate. Sweden has had a long-standing tradition of leading economists being active in public discourse, including Knut Wicksell, Gustav Cassel, Eli Heckscher, Gunnar Myrdal, and Bertil Ohlin (Carlson and Jonung 1996).¹

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We are grateful for valuable comments and suggestions from Niclas Berggren, Magnus Henrekson, and Tobias Lindqvist.

¹ Knut Wicksell, Gustav Cassel, and Eli Heckscher, three of Sweden’s most prominent economists at the beginning of the twentieth century, participated vigorously in the public

The academic activities of economists in Sweden are reported by most university departments and research institutes. Participation in public discourse, however, is not as well documented. At an institutional level, the “third duty” seems not to be taken seriously. Björklund (1996) claims that just a small fraction of all Swedish economists who publish in leading international journals also take an active part in the debate in the daily papers.

We investigate the recent public debate on globalization and the extent of economist involvement. The study is limited to Swedish economists and the Swedish debate. Do free trade and free capital movements contribute to economic growth and wealth, or do regulations and government controls perform better? Some countries have chosen free trade, others protectionism. Evaluating and spreading research results about these policy experiments should be a prime concern for economists.

Attac, a non-governmental organization, is an inspiration for many European participants in the public debate on globalization. The organization was founded in France in 1998 and its Swedish branch in January 2001. Through the work of local groups and national networks, Attac aims at influencing public opinion in various ways: direct contact with politicians and journalists, writing articles, demonstrations, and other street actions. Both the international Attac and its Swedish branch participate actively in the World Social Forum, a movement of anti-globalizationists all around the world. Attac can indeed be regarded as emblematic of this movement, so it seems warranted to focus our attention on their ideas and contrast their involvement in the globalization debate with that of academic economists.

THE PLANKS OF ATTAC

Attac’s planks can be succinctly summarized by quoting from their Swedish homepage (in our translation).

Under slogans of free trade, deregulation, and globalization,
the power over economic and social development is passed

debate. Cassel alone wrote more than 1,600 articles in the daily press. Nobel Laureate Gunnar Myrdal served as a government minister, and Nobel Laureate Bertil Ohlin served as leader of the largest opposition party.

from democratic institutions to the market. The consequence is increased inequalities, wrecked welfare systems and an unstable economy (Attac 2002).

A number of concrete policy suggestions are offered.

Attac wants to introduce a tax on international financial transactions, known as the Tobin tax...[T]o make the Tobin tax work well, it is also required that the tax havens—the free zones where economic activities are conducted beyond all control—be dismantled.

Attac demands that debts of the poor countries be written off. As quick as possible and without any obligations on part of the debtor nations.

Attac Sweden objects to using Swedish citizens' government-administered retirement funds for speculation purposes, forcing Swedish companies to fire employees in order to squeeze out the highest possible stock market value (Attac 2002).

Do free trade, deregulation, and globalization really contribute to increased inequalities, wrecked welfare systems, and an unstable economy? And if so, to what extent do Tobin taxes, unconditional debt relief and changed investment rules for retirement funds represent efficient remedies? In our humble opinion, economic research—theoretical as well as empirical—can shed light on these matters. Attac's opinion is otherwise: "[E]conomics contributes to lending those who administer the current policies an appearance of 'scientific' seriousness."

METHOD

Those best suited for presenting scholarly findings and judgments on globalization are full professors conducting research on international trade or capital movements. These scholars have been deemed important by their peers. Further, full professors feel less "publish or perish" and other pressures so they especially ought to assume the responsibility of the "third

duty.” Therefore, we have chosen for investigation Sweden-based full professors specializing in international economics, with recent scholarly publications in international trade or capital movements.² We include those who have published internationally between 1996 and 2002. An article, book, or book chapter is considered scientific if it is included in the *Econlit* database. Publications within the research areas F1 (trade) and F2 (international factor movements and business) qualify for inclusion in the sample.³

Out of a population of more than 70 professors of economics at universities in Gothenburg, Jönköping, Lund, Örebro, Stockholm, Umeå, Uppsala, and Växjö, eleven met the selection criteria.⁴ Those eleven form the basis of our investigation.

We define activity in the public debate as authoring articles in the daily papers, business magazines, and *Ekonomisk Debatt* (a non-technical journal for academic economists and practitioners), or being interviewed in the papers or magazines. We used four press archives, available on the Internet by subscription.⁵ The archives cover, *inter alia*, more than fifty daily papers as well as two business publications. Not all daily papers are included, but all the major ones, such as *Dagens Nyheter*, *Svenska Dagbladet*, and *Göteborgs-Posten*. In the databases, we searched for each professor’s name, both as an author and as a subject mentioned in the text. In the case of a “hit”, if the headline and introduction seemed to be relevant for this study, we went on to read the whole article. The articles in *Ekonomisk Debatt* were examined manually. The period of study is the three-and-a-half years from 1 January 1999 to 30 June 2002. The start of the period is chosen to approximately coincide with the first mentions of Attac in public discourse.⁶

² Neither adjunct professors nor *emeriti* are included in the sample.

³ We have not taken the research area F3 (international finance) into account since the professors’ publications in this field mainly concerns the EMU, which we consider to be of less relevance for this study. We have made a similar assessment regarding F4 (macroeconomic effects of international trade and finance).

⁴ The eleven professors in the sample are Arne Bigsten (Gothenburg University), Magnus Blomström (Stockholm School of Economics), Harry Flam (Stockholm University), Göte Hansson (Lund University), Henrik Horn (Stockholm University), Mats Lundahl (Stockholm School of Economics), Lars Lundberg (Örebro University), Torsten Persson (Stockholm University), Paul Segerstrom (Stockholm School of Economics), Peter Svedberg (Stockholm University), and Clas Wihlborg (Gothenburg University).

⁵ *Mediearkivet*, *Press-Text*, *Dagens Industris arkiv*, and *Affärsvärldens arkiv*.

⁶ The earliest reference to Attac that we have found is from 1 February 1999 (*Aftonbladet*, 1999).

RESULTS

During the period, attention given to Attac was copious. The organization is mentioned 986 times in articles!

Yet we have found only one article in which an academic economist in our sample referred directly to the planks of Attac. In the article, Torsten Persson, of the Institute for International Economic Studies,⁷ is interviewed about the Tobin tax.

“It is not the size of the capital movements that causes sharp swings in the foreign currency transactions,” says Torsten Persson, professor at the Institute for International Economic Studies. Instead, it is the expectations that make prices fluctuate on financial markets. If one reduces the flows of capital, this may, on the contrary, make trade in foreign currency more volatile. In that case, a Tobin tax is counterproductive (Gatu 2001, 10).

We have found an additional seven articles written for the public, which, in some respect, can be seen as more general contributions to the debate on globalization. Only two of these are self-written articles (Bigsten 1999 and Bigsten and Levin 2001). The first deals mainly with adjustment problems on the Swedish labor market as a consequence of globalization, while the second one, among other things, shortly discusses the effects of different trade strategies in developing countries. The other five articles contain interviews with academic economists. Arne Bigsten comments on Africa’s lack of integration in the world economy (*Borås Tidning* 2002, and *TT* 2002) and the globalization that occurred around 1900 (Haldesten 2002). Magnus Blomström is interviewed about the effects of direct investments of multinational companies in developing countries (Carlsson 2001) and Harry Flam on the distributional effects of free trade in developing countries (Koblanck 2002). None of these interview articles mean to convey what economists think about Attac and the other anti-liberal groups.

Professor activity, then, is very low. We have not found a single self-written article that discusses the planks of Attac and only one interview article, in which the issue is addressed in only a few sentences.

Consider a thought experiment: A militant organization appears in the public arena, which in a thousand articles suggests a new controversial

⁷ The Institute for International Economic Studies is affiliated with Stockholm University.

cure for cancer. What activity level would we expect from the country's professors of medicine?

It may be of interest to compare the Swedish professors with Jagdish Bhagwati, one of the world's leading researchers in international trade. For Bhagwati we found four articles directly referring to Attac and, in total, 24 articles on globalization (all of which are interview articles).⁸ Hence, in our data, he occurs in about three times as many Swedish articles as all the Swedish professors taken together.

There are several sources of uncertainty in our study. We have, for example, no information on appearances on television and radio (besides *Dagens Eko*, a daily news program on national radio, and we had no "hits" there). There are also uncertainties pertaining to the classification of the journal articles. There is a possibility that a professor would be excluded from the sample or that another professor would be added using a different classification. However, we have no reason to believe that our results would be modified in any significant way if the method were varied.

CONCLUSION

We conclude that Swedish professors, who have published internationally within the research fields international trade and capital movements, have let Attac run amuck in public discourse. The arguments of Attac have not been commented upon in any self-written article and only in one interview article. We find this hard to understand for several reasons. First, the debate is about economic issues with fundamental importance for economic growth and prosperity. Second, at least one distinguished *foreign* economist has engaged himself in the Swedish debate. Third, according to our examination of the press archives, the professors in our sample have been involved in *other* public debates during the period, for instance, in discussions about the EMU and foreign aid.

The professors' absence in the debate on globalization has created a vacuum that other economists have tried to fill.⁹ The university professors, most of whom draw a salary from Swedish taxpayers, need to attend to their

⁸ See Bhagwati (2002) for a summary of his arguments.

⁹ See, for instance, Berggren, Bergström, Bornefalk, and Sandström (2001a and 2001b), Berggren, Bornefalk, and Sandström (2002), and Suvanto (2001), all of which are critical of Attac, and Pålsson Syll (2001a, 2001b, and 2002) for an example of a more sympathetic attitude.

“third duty”—deploying economic understanding to improve economic policy. That means participating in public discourse.¹⁰

The potential consequences of the professors’ absence from public discourse are far from trivial. According to a recent survey of the Swedish International Development Cooperation Agency (SIDA, 2003), only a few percent of the Swedish respondents believe that during the last thirty years living conditions in poor countries have improved. Over 50 percent think that the literacy rate in developing countries is below 30 percent, whereas the actual rate, according to UN statistics, is over 70 percent. So Swedes seem to be badly informed about important developments in poor countries. The survey respondents were also asked about the most effective means for reducing poverty. Over 60 percent stated that “trade policy” was important, but it is not known whether less or more regulation was desired. “Debt relief”—one of the planks of Attac—was chosen by about 50 percent as was “foreign aid”, whereas “reduced agricultural subsidies” received less support, about 35 percent. Although the survey results are not without any ambiguity, they seem to support the view that Attac has made a great mark on public opinion.

Could the professional incentives facing academic economists help explain our findings? Yes, we think so. When the professors in our sample do participate in public discourse, their activity is often linked to their being appointed as an expert in a government commission, a task associated with some professional prestige as well as monetary reward.¹¹ In such cases, the issue addressed tends to be closely tied to current government deliberations over policy. But when it comes to larger, permanent issues, less directly linked to current policy deliberations, the incentives for participation in the debate are weak. The problem with this incentive structure is, of course, that economics may be seen as largely irrelevant by the general public. This increases the risk that ill-advised popular opinions will influence government policy in the longer run.

¹⁰ After the publication of a Swedish version of this article (Skedinger and Johansson 2002), Bigsten (2003) addressed the effects of globalization and discussed the arguments of its opponents. He explicitly referred to Skedinger and Johansson (2002) as motivation for the article.

¹¹ In Sweden these incentives are probably stronger than in the US, since relatively more academic economists are involved in work for government commissions in a small country.

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ABOUT THE AUTHORS



Per Skedinger received his Ph.D. from Uppsala University in 1991, and since 1993 he has been a research fellow at the Research Institute of Industrial Economics (IUI) in Stockholm. His empirically-oriented research deals with effects of economic policies, mainly in labor markets, and is published in international and Swedish journals and books. Research topics include effects of various active labor market policies, union-bargained minimum wages, employment protection legislation, and transaction taxes in the housing market. He has served as secretary of the Economic Council of Sweden, a group of academic economists commissioned by the Swedish government to initiate and provide information about important economic-policy research, and has taught at Stockholm University and Stockholm School of Economics.



Dan Johansson received his Ph.D. from the Royal Institute of Technology in 2001. Since 2002 he has been a research fellow at the Ratio Institute in Stockholm. His research mainly deals with the role of entrepreneurs, the importance of new and small firms and the effects of institutions on entrepreneurship, firm formation and firm growth. His research, which is both applied and theoretical, currently focuses on the Swedish Growth School, a unique Swedish tradition influenced by among other things Austrian and early Schumpeterian theory. He has published in international and Swedish journals, co-authored and edited several books, and has taught at Stockholm School of Economics and at the Royal Institute of Technology.

WATCHPAD

Ken Kam and Market Efficiency

DANIEL B. KLEIN*

[Abstract, Keywords, JEL Codes](#)

THE ECONOMIST BURTON G. MALKIEL, A PROPONENT OF THE efficient market hypothesis, writes in the *Journal of Economic Perspectives*: “I will use as a definition of efficient financial markets that such markets do not allow investors to earn above-average returns without accepting above-average risks” (Malkiel 2003, 60). Here I propose to broadcast to anyone who can read this little article an investment opportunity that offers above-average returns and lower-than-average risks.

How Masters 100 Fund Works

An entrepreneur named Ken Kam has created a website that simulates bona fide stock trading. He invites all comers. A contestant can sign up and virtually manage more than one portfolio, each portfolio starting with one million virtual dollars. Kam tracks their virtual performance, and then picks 100 of the contestants to be the actual stock pickers for the actual mutual fund, the Masters 100 Fund. The ticker symbol is MOFQX. MOFQX is a fund owned by Kam’s company Marketocracy.¹

So, Rupert goes to the website, as you and I can, and starts virtual trading (let’s assume he has just one virtual portfolio). After a year or two

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¹ <http://www.marketocracy.com>

his performance is so good that Kam instates him as one of the 100 masters for the new month, say October. During the period October 1 to October 31, Rupert's trades are actually implemented by MOFQX in real time. Rupert's trades for October also augment and count on his own personal track record. The following month Kam and his team revise the set of 100 masters. The monthly decision of whom to put in the 100 group is based on the traders' performance history. It is not a fixed or formal algorithm. Kam opened the virtual field in July 2000, so for some contestants the track record is more than 3.5 years. At present there are about 55,000 active contestants competing to be selected each month as a master.

Ken Kam is the fund manager of MOFQX. But he does not pick stocks. He and his team pick and revise formulas for instating masters into the one-month tenure as a stock picker. Members are rewarded for being instated as masters. At the website you can learn the details on being a contestant, managing a portfolio, and being remunerated as a master.

It Seems to Work

Kam began the virtual competition in 2000, but the actual trading, the mutual fund, began in November 2001. Table 1 shows cumulative and annualized returns compared to S&P 500, NASDAQ, and DJIA. Figure 1 shows performance (returns including dividends, clear and away after all trading costs and after the annual MOFQX fund fee of 1.95 percent) of MOFQX versus the S&P 500 for the roughly 2.3 years from the inception of MOFQX through 29 February 2004. The data used here is the legally reported information and is available at the MOFQX website. The inset shows that the conventional measure of volatility "Beta" is 0.48, versus 1.0 for the S&P 500.

Table 1: Cumulative and Annualized Returns

Returns as of 29 February 2004				
	Cumulative		Annualized	
	YTD	Since Incept.	Avg. 1-Year	Ave. Annual Since Inception
MOFQX	5.91%	47.70%	59.25%	18.28%
S&P 500	3.25%	8.07%	38.62%	3.40%
NASDAQ	1.39%	14.32%	52.64%	5.94%
DJIA	1.60%	18.04%	37.29%	7.41%

Source: Legally reported data from MOFQX.

**Figure 1: Inception to Date Performance
(as of 29 February 2004)**



Source: Legally reported data from MOFQX.

Possible Explanations of the Standing Success

When someone comes home from Las Vegas a winner, we say he was lucky. But when he comes home a winner 90 of 100 visits, we figure it is more than luck.

The track record of MOFQX would seem to satisfy the law of large numbers. Most importantly, largeness seems to be satisfied by the following numbers: (1) the number of positions held by MOFQX (the number has never been below 700 and is now about 1200), (2) the diversification of positions (it is rare that the largest position exceeds 2 percent of the fund),² (3) the number of percentage points by which MOFQX has beaten the S&P 500, and (4) the number of days of success (more than 2.3 years). And other numbers in the mix are large: (5) the number of positions each contestant must hold in his virtual portfolio,³ (6)

² The statement requires a minor qualification: The Fund invests in S&P Depositary Receipts (SPDRs) as a substitute for all positions that are each too small to buy individually, and the Fund's investment in SPDRs is typically between 2 and 6 percent of the Fund. In a technical sense, then, SPDRs is typically the largest "position" in the fund.

³ Each contestant's largest position must not comprise more than 25 percent of his portfolio, and 50 percent of his portfolio must consist of positions each less than 5 percent of the portfolio. Thus, the absolute minimum number of positions in a portfolio is 13.

the number of contestants (some 55,000), (7) the number of “masters” (100 at any given time and changing every month), and (8) the diversification by the standard industry sector and “style” categories. Every number in the scheme is large. If a mutual fund determined comparable daily trading by throwing darts, the likelihood of it performing as far above the S&P 500 as MOFQX has would probably be one in a billion, or something like that.

Still, maybe in a subtler way luck is the explanation. Maybe MOFQX has enjoyed some kind of fortuitous bias. Suppose the whole MOFQX strategy tends to get MOFQX disproportionately invested in some types of positions. The types might be defined by industry sector, by firm capitalization size, or by “style.” As noted, these standard categories don’t seem to provide a ready explanation. But maybe the fortuitous bias selects for certain types of positions in terms of some other, unknown and possibly unknowable typology of positions. By “tends” I mean that, if all types were to perform equally, MOFQX would, for whatever reason, invest disproportionately in, or gravitate to, some particular types. One could potentially explain the success of MOFQX by the fact that those types happened to do well during the past couple of years. It is one among a field of about 7,600 publicly traded mutual funds, and some of these are bound to get lucky. And each lucky fund is bound to have a true believer touting the success as something more than luck. Maybe MOFQX has enjoyed fortuitous bias, and I’m that jackass.

But then there is the other explanation: The scheme works. Contestants might hit upon interpretations and veins of information that are valuable, even if only for a period. These interpretations both make them winners in the competition (and hence masters) and make their ensuing picks as masters good ones.

The master might believe in the winning interpretation for the wrong reasons. In other words, the MOFQX strategy might manage to capitalize on the survivor principle as applied to interpretations, judgments, and veins of information, even if those things are generated randomly (Alchian 1950). Alternatively, masters might simply arrive at and act on superior interpretations—whether it is rooted in superior information, superior interpretive powers, or superior judgment. That is, they “put stock in” winning interpretations for right reasons.

If MOFQX selects for winning interpretations, fantastic. It doesn’t matter whether winning trades are based on random experimentation or on superior insight and judgment. It doesn’t matter if masters “deserve” to be masters.

I am very much inclined to believe that MOFQX works.

“But if you’re so smart, why ain’t you rich?” To that I can respond: By virtue of believing in and acting on MOFQX, I am richer than I otherwise would have been.

An Entrepreneurial Discovery

Friedrich Hayek (1978) and Israel Kirzner (1985) have emphasized that the greatest virtue of free competition is the propensity to discover previously unknown opportunities that correspond to social betterment. Assuming that MOFQX works, Ken Kam will have illustrated the Hayek-Kirzner point on two levels. First, the competition in virtual trading is a form of Hayekian discovery process; it discovers the traders with superior knowledge. Second and more fundamentally, MOFQX is itself a discovery that Kam, one entrepreneur in a fairly free market, has discovered and brought to the investment world. The whole idea of MOFQX is a new interpretation of how one may use available technology to go about picking stock pickers. It is an epiphany, an inspired idea made real. Kam transcended the conventional interpretation of how to go about picking stocks (or, rather, picking stock pickers). He has brought a new interpretation to the field, an interpretation that was there for thousands of others to grasp but was seen and seized only by him.

Regulations tend to regiment industries and to lock in conventional interpretations. Fortunately, regulations of the mutual fund industry were not so restrictive as to lock in the conventional interpretation and make the MOFQX idea moot.

When it dawned on Kam, the primordial insight behind MOFQX was new knowledge. It would not be good English diction to call Kam’s gain in knowledge an acquisition of new information. A defense of the free enterprise system that flattened knowledge down to information would do a poor job of relating its virtues in generating and advancing discoveries like MOFQX. An understanding of economics that allowed such flattening would likely under-appreciate those virtues. As noted by most game theory textbooks, the whole model-building genre depends on the “common knowledge” assumption, which means that all knowledge between agents in the model is uni-interpretational, or that knowledge is flattened down to mere information. The “top” journals publish thousands of papers on asymmetric information, but that kind of economics has not figured out how to deal with *asymmetric interpretation*, and hence simply ignores it, even

though it is the ubiquitous, ineradicable condition of real-life economic processes.

Kam is making profits, but the concomitants include better capital markets, better investment projects, and better living conditions for people in general. Joy tips her hat to Ken Kam.

Assuming the strategy works, eventually it will become self-limiting. If Kam just let the fund grow and grow, and implements the same formula of trades but at greater and greater volumes, it will be moving prices to such an extent that it will do less well than at lower volumes. Presumably he won't let things balloon in that fashion. But he cannot control the competition. The strategy will no doubt be replicated (contestants will probably be able to play a portfolio in both Kam's contest and in a competitor's contest), so competition will eventually push things back to "normal returns"—returns, that is, for the fund and for schmos like us, not for the superior stock pickers (their rewards will rise). But Kam seems to have at least a two year jump on the competition, so I am keeping my money parked.

The Efficient Market Hypothesis: How Rapid is Rapid?

Does Ken Kam falsify the efficient market hypothesis? If not, why not? And if not, what would?

Burton Malkiel writes: "Many of us economists who believe in efficiency do so because we view markets as amazingly successful devices for reflecting new information rapidly" (2003, 60). May we presume that here the expression "new information" means "new knowledge"? Perhaps not: perhaps that distinction tells all and allows efficient market proponents to squirm out of frontal empirical challenges. On the other hand, if we may substitute "new knowledge" for "new information" in Malkiel's statement, the question becomes: How rapid is rapid? MOFQX debuted in November 2001 with about \$3 million and has grown to \$91 million (as of 7 March 2004). How long it will take to reach \$100 billion is to be seen. Is the path of fund growth sufficiently rapid to satisfy the efficient market hypothesis?

I leave these questions to others. I wish to suggest that MOFQX illustrates market efficiency in an altogether different sense: Blessed discoveries—as MOFQX appears to be—are, by and large, more likely the freer the market.

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