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FOREWORD

Editor's Report

IT IS CUSTOMARY FOR THE EDITOR OF A JOURNAL TO REPORT periodically on the number of submissions, time to decision, time to publication, acceptance rate, and so on.

Econ Journal Watch is produced, however, as a journal that is both scholarly and editorially led—the editors respond freely to inquiries and work with prospective authors in developing papers. They often recruit authors and suggest ideas for papers.

The core editorial team consists of me and the four Co-Editors, Bruce Benson (Florida State University), Fred Foldvary (Santa Clara University), George Selgin (University of Georgia), and Lawrence H. White (University of Missouri, St. Louis). Our procedures allow any of them to serve as first-editor on a paper, and prospective authors are welcome to contact any of them (rather than me).

A realized *EJW* article usually goes through the following editorial stages: (1) communication between author and editor; (2) a first draft; (3) editorial iterations, including heavy line-edits, involving two of the five core editors; (4) a tentative decision by the two editors to publish; (5) review by one or two external readers; (6) final revisions and publication. Further elaboration is found in the Peer Review Statement.

Thus, external review usually follows tentative acceptance. Most papers sent out for external review are subsequently published in the journal.

When an author contacts an editor with an idea or draft, the editor usually returns an initial reaction within two weeks. It is rare that the initial reaction takes more than four weeks.

I am deeply indebted to the Co-Editors Bruce Benson, Fred Foldvary, George Selgin, and Lawrence White for their hard work and unfailing, meticulous collaboration. Each of them is crucial to the success of the project. I am deeply indebted also to the Managing Editor Matthew Brown (Florida State University), whose dedication and attention to countless corrections enables *EJW* to move quickly and meet high standards of scholarship. We are grateful to Jane Shaw (PERC, the Property and Environment Research Center) for sharing her extensive editing experience, and to Warren Gibson (Professional Engineer) for helping us with mathematical and statistical uncertainties. For thematic suggestions, help in cultivation, guidance and support, we thank the project Directors Donald J. Boudreaux, Deirdre N. McCloskey, and Richard L. Stroup and the members of the Advisory Counsel.

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Finally, I thank the authors. Each has contributed significantly to the project.

Daniel Klein
Editor
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Miscounting Money of Colonial America

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A COMMENT ON: FARLEY W. GRUBB. 2004. THE CIRCULATING MEDIUM OF EXCHANGE IN COLONIAL PENNSYLVANIA, 1729-1775: NEW ESTIMATES OF MONETARY COMPOSITION, PERFORMANCE, AND ECONOMIC GROWTH. *EXPLORATIONS IN ECONOMIC HISTORY* 41:329-360.

Abstract, Keywords, JEL Codes

[TO THE READER: We relegate much material to appendices. The main text concludes less than half way through this PDF document.]

OVER THE LAST FEW YEARS, ECONOMIC HISTORIAN FARLEY Grubb has published a number of articles in leading journals that aim to rewrite the economic and political history of the United States in the second half of the eighteenth century. Grubb's revisionist views are based on a technique he introduced for determining the composition of the medium of exchange in early America. Applying his technique to the confederation period, he concludes that state-issued paper money (a.k.a. "bills of credit") was the preferred medium of exchange and disappeared only when it was banned by the Constitution (Grubb 2003, 2005a). From this he draws several highly revisionist conclusions: Confederation-era state-issued paper money had been successful; the public had shunned banknotes; and the Constitution's ban on state paper money was inserted against widespread opposition at the behest of bankers who wanted to eliminate competition. In the paper under consideration here, Grubb applies his technique to

* Michener: Department of Economics, University of Virginia. Wright: Department of Economics, Stern School of Business, NYU.

colonial Pennsylvania and derives a time series for the total money supply of colonial Pennsylvania (specie plus paper money). Since no other time series exists for the total money supply of any American colony, cliometricians inevitably will use it, though it has startling and far-reaching implications.

One implication concerns the debate over the adequacy of the colonial money supply. Most historians now argue that, for the most part, it was adequate.[1] However, the quantity of bills of credit outstanding in most colonies (particularly the Middle Colonies) was very modest. Grubb's series shows negligible amounts of specie in the circulation, so if his series is accepted, the implication is that the colonial money supply was seriously inadequate.[2]

Another profound implication concerns the validity of the quantity theory of money. R.C. West (1978) finds that in many American colonies, despite turbulent financial conditions, there was no correlation between the price level and the quantity of paper money in circulation. Most explanations of West's findings fall into one of three camps. The first takes West's finding as a convincing repudiation of the quantity theory (Smith 1985a, b; Wicker 1985). The second explains away the anomaly by pointing to the role that unobserved expectations can play in the demand for money (Calomiris 1988; Sumner 1993). The third camp attributes an important role to specie and rejects the assumption that the quantity of paper money outstanding proxies the money supply, arguing paper money was but one component, and sometimes a small component, of the total money supply (Michener 1987, 1988; Marcotte 1989; McCallum 1992; Brock 1992). One of the reasons that interpretations are so divergent is that, as McCallum (1992, 144) notes, "data on both stocks and flows of specie are extremely sparse." Debate participants spar vigorously over how best to parse the thin data. Smith (1985a; 1985b; 1988) argues that the specie stock was small compared to the quantity of paper money in circulation; Michener (1987; 1988; 2003) reaches the opposite conclusion.

Farley Grubb (2004, 2005b) aims to settle the matter with his data series. Paradoxically, Grubb, despite finding scarcely any specie in circulation, purports to vindicate the quantity theory. That vindication,

[1] McCusker and Menard (1985, 338) conclude that "the colonists' stock of money was adequate," and Perkins (1994, 54) concurs, noting that "there is little reason to believe that the population of British North America suffered much, if at all, from an inadequate money supply."

[2] This is the conclusion of Rousseau (2004). Rousseau simply ignores circulating specie, but Grubb's time series supports Rousseau's conclusion.

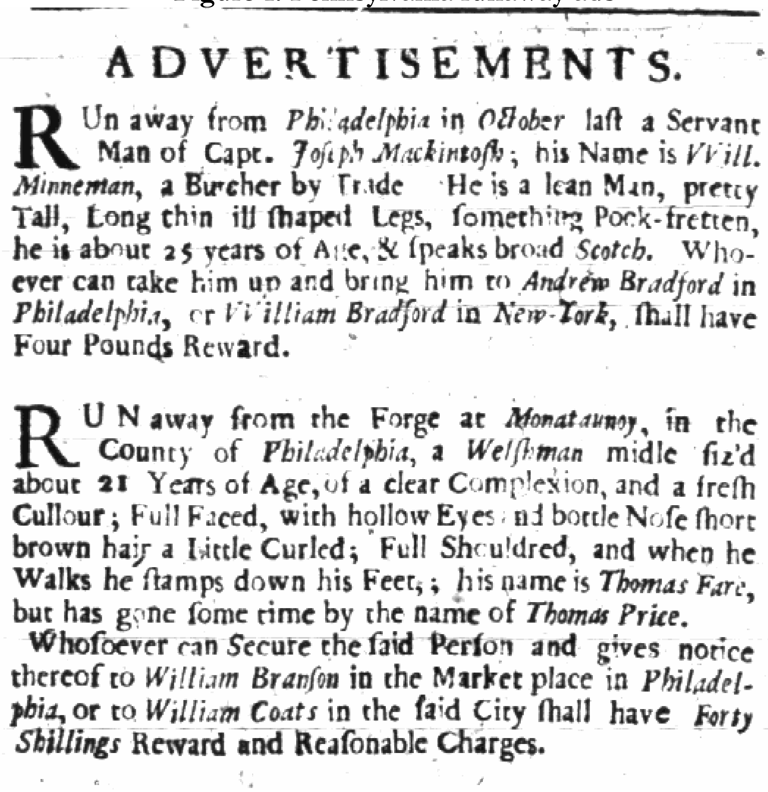
however, arises from Grubb's devotion to what Ziliak and McCloskey (2004) deride as "sign econometrics." Grubb finds statistically significant coefficients with the correct sign. Grubb's data, however, show the total per-capita money supply increasing by 386 percent between 1754 and 1759 with only an 11.4 percent increase in prices, and such data will inevitably reinvigorate critics of the quantity theory.

Grubb's Pennsylvania estimates also matter for the support they lend to his interpretation of the Confederation era and the Constitutional Convention. In working paper form, the Pennsylvania estimates were the "proof of concept" application justifying the use of his technique in the Confederation period. Moreover, Grubb relies on his Pennsylvania estimates to dismiss Rolnick, et. al. (1993), who, citing colonial precedents, attribute the Constitutional ban on state-issued paper money to a concern that the interstate circulation of state-issued paper money would tempt states to compete for seigniorage. Grubb argues that the colonial precedent is false, that colonial currencies did not circulate in adjoining colonies.

Were Grubb and his data correct, American history textbooks would have to be rewritten and the study of the early U.S. macroeconomy could leave its "statistical Dark Ages" and join the ranks of subjects susceptible to the gaze of "Science." We question Grubb's research for the simple reason that it is wrong. Grubb has discovered the economic history equivalent of phlogiston, black bile, or geocentrism. His work rests on the premise that he can discern the medium of exchange in certain types of contracts. We argue here that he cannot. We then contend with the many "epicycles," the many contortions of fact, logic, and theory that he must construct to support his fundamentally flawed view of the economic universe of eighteenth century America.

Grubb derives his estimates by tabulating rewards offered in the *Pennsylvania Gazette*'s advertisements for runaway servants and slaves. The ads in question resemble those in Figure 1.

Figure 1: Pennsylvania runaway ads



ADVERTISEMENTS.

RUn away from *Philadelphia* in *October* laſt a Servant Man of Capt. *Joſeph Mackintoſh*; his Name is *Will. Minneman*, a Butcher by Trade He is a lean Man, pretty Tall, Long thin ill ſhaped Legs, ſomething Pock-fretten, he is about 25 years of Age, & ſpeaks broad *Scotch*. Whoever can take him up and bring him to *Andrew Bradford* in *Philadelphia*, or *William Bradford* in *New-York*, ſhall have Four Pounds Reward.

RUN away from the Forge at *Monataunoy*, in the County of *Philadelphia*, a *Welſhman* midle ſiz'd about 21 Years of Age, of a clear Complexion, and a freſh Cullour; Full Faced, with hollow Eyes and bottle Noſe ſhort brown hair a Little Curled; Full Shouldred, and when he Walks he ſtamps down his Feet; his name is *Thomas Fare*, but has gone ſome time by the name of *Thomas Price*.

Whoſoever can Secure the ſaid Perſon and gives notice thereof to *William Branſon* in the Market place in *Philadelphia*, or to *William Coats* in the ſaid City ſhall have *Forty Shillings* Reward and Reaſonable Charges.

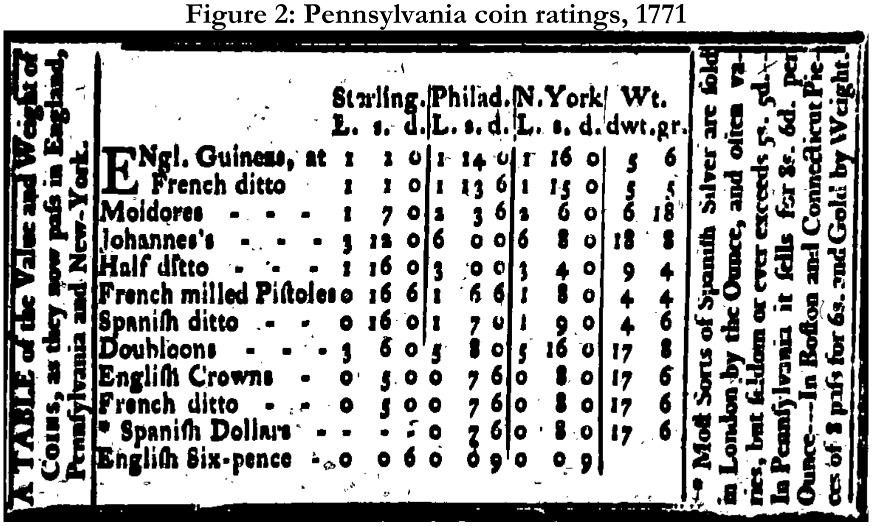
Source: *American Weekly Mercury*, 17 March 1720.

Grubb uses reward data to estimate the yearly ratio of specie to paper money transactions, combines that ratio with the known quantity of Pennsylvania paper money outstanding, and derives a time series for the colony's total money supply. The viability of this technique depends crucially on the meaning of "pounds," because 83.3 percent of Grubb's ads, like those reproduced in Figure 1, offered a reward in undesignated "pounds" or "shillings." Grubb infers that these rewards were always settled with paper money.

The resulting series is fundamentally flawed, however, because the medium of exchange mentioned in the advertisements cannot in most instances be ascertained. The inference that "pounds" rewards were settled in paper money is unwarranted because pound-denominated rewards mentioned in the ads refer to Pennsylvania's unit of account, not its medium of exchange. In a sense, each colony had its own 'money'. But, as Charles M. Andrews (1918, 74) notes, a colony's so-called money "was not money at all, but only a method of reckoning values, a statement of the amount in shillings at which a Spanish dollar would be accepted in a given colony." In the same vein, John J. McCusker (1978, 3-6, 121) argues that the distinction was "an important one to remember, because goods were bought and sold by coin but books were kept and exchange transactions negotiated in moneys of account."

Media of exchange were several and included country produce, book account transfers, foreign coins (such as Spanish dollars, pistareens, or guineas), and paper money (including that of other colonies) (Andrew 1904; Michener 2003). The multiplicity of media of exchange was made viable by virtue of a local unit of account, "Pennsylvania pounds" in this instance. Colonists assigned foreign coins values in terms of the local unit of account, published those ratings in almanacs, and made economic calculations and agreements in those terms (see Figure 2).

Figure 2: Pennsylvania coin ratings, 1771



A TABLE of the Value and Weight of Coins, as they now pass in England, Pennsylvania and New-York.

	Sterling. L.	s.	d.	Philad. L.	s.	d.	N. York L.	s.	d.	Wt. dwt.	gr.
ENgl. Guineas, at	1	1	0	1	14	0	1	16	0	5	6
French ditto	1	1	0	1	13	6	1	15	0	5	5
Moidores - - -	1	7	0	2	3	6	2	6	0	6	18
Johannes's - - -	3	12	0	6	0	0	6	8	0	18	8
Half ditto - - -	1	16	0	3	0	0	3	4	0	9	4
French milled Pistoles	0	16	6	1	6	6	1	8	0	4	4
Spanish ditto - -	0	16	0	1	7	0	1	9	0	4	6
Doubloons - - -	3	6	0	5	8	0	5	16	0	17	8
English Crowns -	0	5	0	0	7	6	0	8	0	17	6
French ditto - -	0	5	0	0	7	6	0	8	0	17	6
* Spanish Dollars - -	-	5		0	7	6	0	8	0	17	6
English Six-pence -	0	0	6	0	0	9	0	0	9		

* Most Sorts of Spanish Silver are sold in London by the Ounce, and often varies, but seldom or ever exceeds 5s. 5d. In Pennsylvania it sells for 8s. 6d. per Ounce.—In Boston and Connecticut Pieces of 8 pass for 6s. and Gold by Weight.

Source: *Father Abraham's pocket almanack.*

This article marks the second time that we have challenged Grubb's methods. The first exchange concerned the Constitutional period and appeared in the *American Economic Review.* It may be summarized as follows. Grubb (2003a, 2005a) counted the use of the terms "pounds," "dollars," "sterling," and "guineas" in indentured servant transactions recorded in Philadelphia between 1785 and 1804. Practically all were recorded in "pounds" until the last years of the eighteenth century, from which Grubb concludes that Pennsylvania's state-issued bills of credit remained the preeminent medium of exchange until driven from circulation by a Constitutional prohibition engineered by a cabal of profit-seeking bankers. Michener and Wright (2005) dispute these conclusions and object that "pounds" can not be equated with bills of credit. Indeed, many of Pennsylvania's bills of credit were denominated in dollars, not pounds, and one emission was denominated in both. By 1794, a year in which all 200 servants' contracts were recorded in pounds, there remained only $0.06 per capita in Pennsylvania paper money outstanding, compared to estimates of the specie stock ranging from $3.00 to $7.77 per capita. Grubb (2005b) is unpersuaded; in his rejoinder he argues that participants chose their language deliberately in arms-length transactions between strangers such as servants' contracts because these were binding contracts enforceable at law.

That is where our first exchange currently rests. The present article again criticizes Grubb for mistaking the unit of account for a specific medium of exchange, but directs the criticism to the particular historical material treated in Grubb's article in *Explorations in Economic History* (2004), namely, the colonial period. Thus, the nature of the basic criticism is like that of the previous exchange, but the evidentiary dispute is different. We feel that the criticisms made here about the colonial period reinforce the conclusion that Grubb's entire project is riddled with errors.

Grubb's argument that his procedure must work in arm's-length transactions between strangers is an excellent illustration, for Grubb (2004, 333) defends his use of runaway ads in the colonial era in precisely the same language: "Only the enforceable intention to pay the currencies advertised is relevant to the soundness of the measure of currency composition constructed here." What evidence do we have that, as Grubb asserts, there was an "enforceable intention to pay the currencies advertised"? As Grubb concedes elsewhere (2004, 331), colonists ubiquitously used Pennsylvania "pounds" to signify unit-of-account money. This fact makes it plausible that courts interpreted ads promising "pounds" as a promise of payments in a form equaling the amount of "pounds" specified. Grubb presents neither statutes nor case law to support his contrary interpretation, and never acknowledges that all paper money issued by Pennsylvania before the mid-1760s was a legal tender at its face value even in contracts where specie was explicitly promised.[3] An ad's promise to pay specie was not legally enforceable during most of the colonial era.

FURTHER PROBLEMS

Readers may feel that an econometrician desperately in need of colonial money supply data could sensibly use Grubb's and that our alternative explanation, while plausible, is unproven. The aura of ambiguity, this section shows, is false. Grubb's estimates are wrong, and all that is needed to prove it is a detailed knowledge of the colonial economy and monetary system, much of which we have relegated to appendices and notes.

First, Grubb's method, applied to Pennsylvania's early history, yields obviously fallacious results. Grubb reports that 83.3 percent of the ads he studied resemble those in Figure 1, offering rewards in "pounds" or "shillings" with no further designation, which he contends record transactions in Pennsylvania bills of credit. The ads in Figure 1 date from 1720, a year when 29 out of 34 unique ads (85.3 percent) offered pounds and shillings, a figure quite similar to Grubb's finding. Yet *these* ads indisputably refer to Pennsylvania's unit of account money, not bills of credit, because the colony did not issue its first bills of credit until 1723.[4] A reward of Pennsylvania "pounds" did not mean bills of credit in 1720. There is no reason to believe it meant bills of credit after 1723.

Second, Grubb asserts that two types of arms-length transactions, runaway ads and servants' contracts, can be used to infer the medium of

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[3] For Pennsylvania's legal tender provisions see Pennsylvania, Mitchell et al. 1896, vol. 3, c. 261, 329; vol. 4, c. 300, 103-4; ibid., c. 353, 348-49; vol. 5, c. 363, 9; ibid., c. 370, 48; ibid., c. 402, 193; ibid., c. 406, 210; ibid., c. 412, 247; ibid., c. 422, 299; ibid., c. 423, 306; ibid., c. 431, 349; ibid., c. 437, 393; ibid., c. 444, 431; vol. 6, c. 453, 18-19; ibid., c. 513, 363. The last of these, passed in 1764, is the first to exempt quit rents. The Currency Act of 1764 prohibited new issues of legal tender paper money but did not nullify the legal tender status of existing issues; Pennsylvania's paper money retained its legal tender status until the issues extant in 1764 were retired.

[4] The ads appearing in Figure 1 are from the *American Weekly Mercury*, 17 March 1720. In a survey of all the ads appearing in the *Weekly Mercury* during 1720, we discovered ads for a total of 50 servants, slaves, and prisoners. In the 34 unique ads offering rewards, one ad offered two guineas, one ad offered a pistole, and three ads made mention of the reward being in "current money." The other 29 ads offered sums in pounds, shillings, and pence.

exchange. When one reverses Grubb's procedure, however, and uses servants' contracts to estimate the colonial money supply or runaway ads to estimate the confederation money supply, one gets results that strikingly contradict his estimates. Servants' data like that used in Grubb (2003a, 2005a) are available only for 1771-1773 (Philadelphia Mayor 1773). The records for 1773 show 1,687 transactions recorded in undesignated pounds, shillings and pence, 8 in guineas, and 1 in sterling. (One garbled transaction is omitted.) Counting sterling as specie, Grubb's technique indicates 9 out of 1,696 transactions used specie. Yet among runaway ads in the same year, Grubb reports 64 out of 153 transactions in specie. Those point estimates (9/1696 = .0053 and 64/153 = .4183) are wholly inconsistent. If one were to test the null hypothesis that the two point estimates are random samples from the same population, the result is a z-statistic in excess of 25 and a decisive rejection of the null. Michener and Wright (2005, online appendix) examine runaway ads in the confederation era and discover an overwhelming majority offered rewards in "dollars" even when indenture contracts recorded the same year are almost without exception in "pounds."

Third, did the transactions velocity of specie differ from the transactions velocity of paper money in early America? Grubb's (2004) procedure for estimating the specie stock from the relative frequency of specie transactions implicitly assumes the two velocities were the same. Yet for the Confederation era, when there was vastly more specie than paper circulating, Grubb (2005b, 1343) asserts paper money dominated in transactions because it circulated far more quickly than specie. Indeed, he maintains that in 1794 virtually all transactions were executed with just $0.06 per capita of Pennsylvania's "well managed" paper money, despite the fact that there were several dollars per capita of banknotes and specie circulating (Michener and Wright 2005, 686). For 1 percent of the money supply to account for 99 percent of all transactions it would have to turn over approximately 10,000 times faster than the rest of the money supply. Although we doubt the dissimilarity was so great, we agree that specie probably circulated more slowly than paper money in early America; small denomination money tends to have a higher transactions velocity than large denomination money and the denominations of bills of credit were small compared to the coins in circulation (Hanson 1979, 1980).[5] Inequality in

[5] At its website, the Federal Reserve says that its large denomination notes last up to five times longer than small denomination notes, suggesting that the small denomination notes circulate up to five times faster.

transactions velocity would itself be sufficient to invalidate Grubb's (2004) estimates.

Fourth, Grubb's estimates do not agree with what scholars know about Pennsylvania's money supply in the late 1740s and early 1750s, the period for which our knowledge is richest. King George's war, which ended in 1748, presented the Middle colonies with extraordinary opportunities in trade and privateering that they eagerly exploited (Smith 1972 1:233). The result was that the Middle colonies enjoyed a relatively abundant specie stock in the immediate postwar years. Appendix 1 reproduces the evidence describing the abundance of specie during those years. The vast discrepancy between Grubb's estimate and the archival record means that one of them must be wrong.

Fifth, Grubb's estimates are inconsistent with what historians know about the paper money circulating in colonial America. Virtually all historians of colonial currency believe that bills of credit frequently circulated as a medium of exchange in neighboring colonies.[6] Contemporary statements affirming their judgment abound. "Under the circumstances of America before the war," a Maryland resident wrote in 1787, "there was a mutual tacit consent that the paper of each colony should be received by its neighbours" (Hanson 1787, 24). In Grubb's (2004, 336-337) view, "If [the paper money] of the various colonies circulated freely in Pennsylvania, then some Maryland, New York, New Jersey, and Virginia pounds should have been offered as rewards by Pennsylvania residents for their runaways. But none of these other-colony currencies were so offered." Our view is that the absence of mention of those currencies is further proof that "pounds" in runaway ads refer to Pennsylvania's unit-of-account, not its medium of exchange. Grubb's reasoning, however, forces him to dismiss cross-colony circulation of bills as a mistaken notion based on "a couple isolated statements made by a few merchants and politicians" (2004, 336, fn. 5).

Dismissing cross-colony circulation of bills is absolutely essential to Grubb's agenda. Even if neighboring colonies' bills did not circulate within Pennsylvania, Grubb's inferential technique would fail if a significant fraction of Pennsylvania's paper money ever circulated *outside* Pennsylvania. Moreover Grubb (2004, 330, 339) uses his "discovery" of the absence of

[6] Grubb (2004, 336, fn. 5) cites Michener (1987, 236, 244), Sachs (1957, 201), and Smith (1985a, 539) as examples of those who have believed in the cross-colony circulation of bills of credit. A fuller list would be longer and include the most prominent experts in the field: Ernst (1973, 248-249), Brock (1975, 86-89) and McCusker (1978, 169-170, 181, 193).

cross-colony circulation to dismiss Rolnick et. al.'s (1993) argument that seigniorage competition played a role in the Constitutional ban on state-issued paper money. Cognizant of the importance of persuading readers that no cross-colony circulation of bills occurred, Grubb defends the claim vigorously. "Philip Mazzei, in his 1782 *History of the Beginning, Progress, and End of Paper Money in the United States* . . ." writes Grubb (2004, 339), "concluded that state [colony] paper money circulated freely only within it." Grubb's editorial inclusion "[colony]" misleads because Mazzei referred to the Revolutionary, not the colonial, era. The quotation's full context is revealing.

> [Congress's] money circulated throughout the States, for each State was obliged to redeem its own quota and stand surety for all the other States. The state paper money circulated freely only within it, and *the ease or difficulty with which it circulated in other states depended on the distance and trade between the state where it was spent and the state issuing it.* (Mazzei, Marchione et al. 1983, 1:326, emphasis added)

Grubb misrepresented the situation as well as the period because Mazzei suggested that state paper money circulated between neighboring states with strong commercial ties.[7]

In Appendix 2, we detail some of the extensive evidence showing that bills of credit circulated across colonial boundaries, so that the reader can make an independent determination whether it ought to be dismissed as "a couple isolated statements made by a few merchants and politicians".[8]

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[7] Grubb (2004, 339) also cites Jones (1980, 131-132). The passage cited is the one in which Jones tentatively concludes that the probate records recorded cash that was "largely in the form of current local money, in paper, of the particular province." The passage gives us little insight into Jones's position on this issue. If, as we believe, Delaware and New Jersey paper money were freely accepted as a medium of exchange in Pennsylvania, they would have been a part of Pennsylvania's "current local money." Only by reading the phrase "*of* the particular province" as equivalent to "*issued by* the particular province" can Jones be said to be denying cross-colony circulation. There is no reason to presume that was her intention.

[8] In Appendix 2, we focus narrowly on evidence pertaining to Pennsylvania, but the cross-colony circulation of bills of credit is also well documented for other colonies. Philip Cuyler wrote Cornelius Cuyler (26 August 1756) from New York that "Jersey money passes here as current as N.Y." (McCusker 1978, 159, fn. 102). The value of New Jersey money as it passed in New York was tabulated in nearly every issue of New York's *Gaine's New Pocket Almanack*. The New York Chamber of Commerce became embroiled in a lively controversy over the premium accorded New Jersey money when tendered in New York (Stevens 1867, 143, 151-153, 160-161, 168, 185-186, 296). When it narrowly voted that the preference given New

Sixth, Grubb (2004, 340) "discovers" a highly implausible explosion in the use of Spanish silver dollars, observing that the proportion of ads offering rewards in "dollars" skyrocketed from the negligible levels prevailing before the end of the French and Indian War to nearly 50 percent of all ads by 1775. The explosion of Spanish silver dollars is implausible because Spanish silver was substantially undervalued in Pennsylvania relative to Portuguese gold, an undervaluation that contemporaries recognized and discussed. An increase in the usage of Spanish silver, would, in these circumstances, be a violation of Gresham's Law. Appendix 3 documents this and suggests several reasons why "dollars" appear more frequently in runaway ads on the eve of the Revolution. As detailed there, the circulation within Pennsylvania of newly emitted Maryland bills of credit denominated in dollars may have played a role.

GRUBB AND THE HISTORICAL RECORD

With the exception of Pennsylvania in the late 1740s and early 1750s, evidence on the quantity of specie circulating in colonial America is sparse. Grubb (2004) adds little new, attempting to persuade the reader of the plausibility of his data series by relying heavily on the same evidence as Smith (1985a, 1985b, 1988). Contradictory evidence is ignored or slanted so that it superficially appears to support Grubb's position.

For example, Grubb (2004, 342) quotes Benjamin Franklin to the effect that "Pennsylvania, before it made any paper money, was totally stript of its gold and silver." Chronic scarcity of specie in Pennsylvania before 1723 would increase the plausibility of Grubb's estimates, which show little

Jersey money be rescinded, dissenting merchants advertised they would continue to accept New Jersey money on the old terms (*New York Gazette or Weekly Mercury*, 14 September 1772). The question was finally settled by statute in 1774 (New York 1894, 5:1654). In 1775 a group of New York merchants subscribed to a plan designed to give Connecticut's bills of credit "a currency equal to those of the other neighbouring provinces" (*New York Journal, or General Advertiser*, 13 July 1775).

The "promiscuous circulation" of the bills of credit of the four New England provinces (Massachusetts, Connecticut, Rhode Island, and New Hampshire) in the several decades before 1750 is, if anything, even more notorious (Brock 1975, 35-36). Some hint of the extent of this circulation can be gleaned from Governor Shirley's address to the Assembly, 9 February 1744 (Massachusetts House of Representatives 1919, 20:329-333).

specie in the colony before the French and Indian War. But specie was not chronically scarce in early Pennsylvania. In the late seventeenth century, it was plentiful (Watson and Hazard 1898, 75). An account from 1698 even declared that per capita silver circulation in Pennsylvania was more plentiful than in England (Thomas 1698, 329). Franklin arrived in Philadelphia as an apprentice printer in 1723, shortly before paper money was issued. He found the colony gripped by a serious depression, a liquidity crisis brought on by the collapse of the South Sea bubble in England. Given that he made the claim in a tract designed to persuade Parliament to repeal the Currency Act of 1764, Franklin's account is consistent with other surviving descriptions of the depression Pennsylvania experienced in 1721-23 (Lester 1938) and there is no reason to suppose he was describing a chronic state of affairs. Indeed, Franklin's use of the archaic version of the word stripped implies that Pennsylvania had specie that it subsequently lost.

In another instance, Grubb (2004, 342, fn. 12) writes that Alexander Hamilton "opined that specie comprised 27 percent of total currency (paper plus specie) in circulation before the Revolution." Here, Grubb is referring to Hamilton's statement (Morris, Ferguson et al. 1973, 1:35) that the money supply before the Revolution consisted of 30 million dollars, of which 8 million was specie. The implication that 22 million dollars in bills of credit were outstanding on the eve of the Revolution is absolutely incredible. Brock's estimates of the quantities of paper money outstanding—the ones Grubb relies on—reveal there could hardly have been 3 million dollars in bills of credit left outstanding in 1774 (Michener 2003). Nor is the basic observation sensitive to the exact year for which one does the calculation. Grubb, therefore, would have to dismiss the denominator of his "27 percent" calculation as inaccurate. The numerator ought to be equally troublesome for him because 8 million dollars of specie would mean that per-capita specie holdings were in excess of $3. As probate evidence makes clear, per-capita money holdings in the middle colonies were considerably greater than in New England or the South; no scholar has ever doubted Pennsylvania, arguably the most commercially advanced colony on the continent, possessed more than its per-capita share of the colonial specie supply.[9] Grubb points out that his percentage estimate aligns very well with Hamilton's if Hamilton was referring to a year between 1770 and 1772. Yet, according to Grubb, Pennsylvanians possessed only $1.04 per capita in

[9] Even Smith (1988), who, like Grubb, regards specie as a small fraction of the colonial money supply, declares that "Pennsylvania was probably the most specie-rich of the colonies."

specie in 1771. While the percentage estimates align well, the absolute amount of specie Grubb says was circulating was far less than Hamilton opined. Nonetheless, Grubb (like Smith (1985a, 537) before him) is content to compute the ratio and report it as supporting evidence.

Grubb (2004, 343) also cites Alice Hanson Jones, who conjectured that the money supply in 1774 was mostly paper. Jones, however, was extremely tentative in her conclusion. She wrote (1980, 130-132) that "The meaning of 'cash' as a financial asset is not entirely clear," and that "whether the cash was in coin or paper was rarely stated." Of the 38 percent of probated decedents in New Jersey, Delaware, and Pennsylvania whose estates included cash, only 0.2 percent specifically mentioned having coin. The infrequent mention of coin led Jones to believe "that the inventoried cash was largely in the form of current local money, in paper, of the particular province." We believe Jones, like Grubb, was misled by "pounds"; in any event Jones's findings do not support Grubb's findings. Barely 0.5 percent of probated estates possessing cash mentioned gold or silver, yet Grubb concludes specie was 45 percent of the money supply in 1774.

Finally, in an attempt to discredit Michener (1987) and defend his own estimates, Grubb makes spurious adjustments to probate evidence. The evidence in question is due to Jones (1980), who infers the wealth of the living by examining samples of probate inventories from 1774 detailing the possessions of the recently deceased. The endeavor is, of course, fraught with difficulties. How ought one to correct for the fact that the deceased are disproportionately old or for the fact that not everyone's estate was probated? Jones makes plausible assumptions to bridge the gaps, and the result, though not without its critics, is still the best evidence extant on colonial wealthholding. She (1980, 41, 128) concludes that the Middle colonies possessed £1,160,000 sterling in cash, which, when divided among a population of 640,695, yields an estimated per-capita cash-holding of £1.81 sterling. As argued in Michener (1987, 275), the quantity of paper money per-capita extant in the Middle colonies in 1774 was equivalent to only £0.58 sterling, so the remaining £1.23, or 68 percent of the total money supply, must have been specie.[10]

Grubb (2004, 343) disputes this conclusion, pointing out that Jones derives her estimates only from probated wealthholders, not all wealthholders, and arguing that non-probated wealthholders were probably

[10] McCallum (1992, 152-153) uses a different technique to estimate the colonial money supply; his estimates are consistent with Jones's.

poorer and less likely to hold cash. On the implicit assumption that non-probated wealthholders held no cash at all, Grubb reduces Jones's estimate of the total money supply by the fraction of non-probated wealthholders. Since probated wealthholders constituted just 63.1 percent of all wealthholders, Grubb concludes the actual per-capita money supply was but (.631)*(1.81) = £1.14 sterling per capita. This adjustment by Grubb presumes that Jones imputed the same amount of wealth to non-probated wealthholders as she found for probated wealthholders. That presumption is simply erroneous. After detailing several reasons other than abject poverty that an estate might be settled without probate, Jones (1980, 349) concludes that those reasons "preclude the simple assumption that all non-probates had zero wealth." Her solution is to presume that the non-probated wealthholders possessed just one-quarter of the wealth of the probated wealthholders. Therefore, even if non-probated wealthholders held no cash at all, Grubb's adjustment seriously understates the colonial money supply.

This is not the only sleight of hand that Grubb performs in attempting to reconcile his estimate of the 1774 money supply to the probate evidence. Michener's estimate (1987, 275) of £0.58 sterling is for the Middle colonies taken as a whole. The per-capita quantity of Pennsylvania paper money extant in 1774 was considerably less than the average in the Middle colonies, amounting to only £0.467 sterling.[11] Even though Grubb insists that the quantity of paper money circulating in a colony was the quantity issued by that colony, and even though £0.58 sterling is inconsistent with the data contained in his own Table 1, Grubb uses £0.58 sterling as the quantity of paper money in circulation in Pennsylvania.

The fraction of specie in Pennsylvania's money supply implied by the probate evidence depends on whether the paper portion of the money supply is taken to be £0.58 or £0.467 sterling and the assumption one makes about the wealth of non-probates, as detailed in Table 1.

[11] Money supply and population data are those used by Grubb (2004, 335); the conversion to sterling uses an exchange rate from McCusker (1978, 186).

Table 1: Probate estimates of the Pennsylvania Money Supply in 1774

	Per-capita money supply in £ st.	% Specie, assuming £0.58 st. in paper.	% Specie, assuming £0.467 st. in paper.
Jones (Non-probate W = ¼ probate W)	1.81	68	74
Grubb (over adjustment)	1.14	49	59
Grubb (Nonprobate W=0)	1.58	63	70

Michener's estimate is the 68 percent in the middle column. Grubb's estimate is the 49 percent just below it, a number he finds reassuringly close to the 45 percent he derives from runaway ads. However, since Grubb steadfastly maintains that there was no cross-colony circulation of bills, he ought to adopt an estimate from the rightmost column. If Grubb's over adjustment for non-probated wealthholders is discarded, his assumptions lead inexorably to the conclusion that 70 percent or more of Pennsylvania's 1774 money supply consisted of specie, a figure quite different from the 45 percent he seeks to defend.

All the estimates in the table are probably too low because colonial probate inventories are notoriously incomplete. Two examples not involving money illustrate the general problem. In Jones's collection of inventories, over 20 percent of the estates did not include any clothes (Lindert 1981, 657). In an independent survey of Surry County, Virginia probate records, Anna Hawley (1989, 27-28) notes that only 34 percent of the estates listed hoes, despite the fact that the region's staple crops, corn and tobacco, had to be hoed several times a year.

In Jones's 1774 database an amazing 69 percent of all estates were devoid of money. [12] While the widespread use of credit made it possible to do without money in most transactions, it is likely some estates contained cash that does not appear in probate inventories. Peter Lindert (1981, 658) surmises that "cash was simply allocated informally among survivors even before probate took place." McCusker and Menard (1985, 338, fn. 14) concur, noting that "cash would have been one of the things most likely to have been distributed outside the usual probate proceedings." If Jones

[12] Sixty-nine percent of the inventories in the thirteen colonies, that is. For the middle colonies, the comparable percent is 55.6 percent, while for just New Jersey, Pennsylvania and Delaware, it is 62.0 percent (Jones 1980, Tables 5.3 and 5.6, 130, 140).

actually underestimates cash holdings in 1774, the implication would be that even more of the prewar money supply in the Middle colonies was specie.[13]

Jones's estimate of the total money supply in 1774 provides another perspective on the situation prevailing in Pennsylvania in the early 1750s. If Grubb is correct, Pennsylvania's total money supply, reduced to sterling terms, averaged only £0.39 sterling per capita for the years 1750-1754. If Pennsylvania possessed something in the neighborhood of £3 to £4 of specie for every pound of paper—Brock's opinion, discussed in Appendix 1—the implication would be that the total money supply for the years 1750-1754 averaged an amount in the range of £1.50 to £1.87 sterling per capita.[14] Grubb's estimate implies a dramatic increase in per-capita real

[13] Grubb (2004, 342, fn. 14) chides Michener for quoting Mazzei as having said of colonial America that "in 1773 . . . all transactions were made almost entirely in specie." Mazzei, Grubb asserts, was referring only to Massachusetts, a colony that returned to specie long before 1773. We disagree with Grubb's interpretation. Here is the disputed passage, so readers may judge for themselves. (Mazzei wrote these lines in 1782, and the "present war" refers to the Revolution.)

Mazzei began his essay, "History of the Beginning, Progress, and End of Paper Money in the United States," with two paragraphs arguing that the "American States" (no mention is made of Massachusetts here) never had an abundance of specie in colonial times because of their extremely unfavorable trade with England. Here is his next paragraph, verbatim.

> Since for the above reason specie was often lacking, it had to be made up by bills of credit, that is, paper money. However, this was nothing new for the Americans at the beginning of the present war. They already knew from experience that too much paper causes it to depreciate, for during the previous war the American states, because of their voluntary and excessive zeal in helping England, had gone into debt for almost ten million pounds. As a consequence the paper money of the state of Massachusetts lost so much value that it lost up to 10/11 of its face value, so that 11 paper pounds had to be paid for what could be had for one of specie. But as the Americans had paid off the said debt before the Revolution, very little paper money had remained in circulation and had regained its full value. In 1773, the year disorders began, that is, ten years after the end of the previous war, all transactions were made almost entirely in specie, which, however, did not abound. (Mazzei, Marchione et al. 1983, 1:325)

[14] The calculation yielding the range of £1.50 to £1.87 sterling neglects the possible cross-colony circulation of bills. If we compute the average per-capita paper money balances for New York, Pennsylvania, and New Jersey and reduce them to sterling, we find £0.50. If those colonies collectively had 3 or 4 times as much specie as paper, total money balances would have been in the range of £2.0 to £2.5 sterling. New York, however, had issued a disproportionate share of the total, and New York paper money did not circulate in

balances between the early 1750s and 1774, whereas Brock's implies the two eras had roughly comparable sterling money balances. In our opinion, Brock's scenario is the more plausible one.

CONCLUSION

"Dollars" in runaway ads cannot be taken to mean specie dollars; "pounds" cannot be taken to mean Pennsylvania bills of credit. Grubb's technique for inferring the quantity of specie that circulated in colonial Pennsylvania leads him to underestimate its importance, dramatically so in the years immediately before the French and Indian War. It is highly unlikely that Spanish dollars became increasingly prominent in Pennsylvania's money supply in the decade before the Revolution, although it is possible Maryland's paper dollars did. The time series that Grubb produces for the total money supply in colonial Pennsylvania is so thoroughly flawed that scholars ought to disregard it.

Pennsylvania. While New Jersey's paper money did circulate in Pennsylvania, New Jersey had relatively little paper money outstanding. For Pennsylvania's paper money balances in those years to have reached £0.50 would have required practically *all* of *both* Pennsylvania's and New Jersey's paper money to circulate in Pennsylvania, which is highly unlikely. The calculations are based on money supply data from Brock (1992), exchange rate data from McCusker (1978), and population data from United States Bureau of the Census (1975, 2:1168). Delaware is neglected in these calculations because of the absence of detailed data on her outstanding paper money balances. Its absence does not seriously bias the conclusions. Delaware's population and paper money outstanding were both approximately a quarter of Pennsylvania's, giving Delaware roughly the same amount of paper money per-capita as Pennsylvania (Brock 1975, 98, fn. 77).

APPENDIX 1:
Pennsylvania's specie stock, 1749-1754

Here we present archival evidence that Grubb has dramatically underestimated Pennsylvania's specie stock during these years, which, we believe, undermines the credibility of his estimates for other years as well. Grubb (2004, 334-5) estimates that between 1749 and 1754 specie comprised, on average, five percent of the money supply, £4,317 Pennsylvania pounds compared to a paper money supply averaging £83,500. But Brock (1975, 354) argues that "during King George's War the amount of specie in . . . [Pennsylvania] had increased until in 1749 there was more in circulation than at any previous time in the history of the province. There were at this time perhaps three or four pounds circulating in specie for every pound in paper." Brock's statement is based on ample evidence.

1. In 1749, a Massachusetts pamphleteer described New York and Pennsylvania's circumstances in this way:

> At New-York, and Philadelphia Silver is their Medium, and mill'd Dollars pass current at a known determinate Rate, and other foreign Coins in proportion: Paper Bills are sometimes the Instrument in Payments, but the Proportion is small compar'd with the Silver. (Davis 1964, 4:387)

2. Pennsylvania's proprietary secretary, Richard Peters, wrote Pennsylvania's owners on 29 April 1750 to criticize an effort by the assembly to emit bills of credit when he remarked that:

> The increase of the Currency by the vast Quantities of Gold and Silver brought into the Province by the Spanish & West India Trade is so very remarkable that one would have thought it cou'd not enter into the heads of any to make any addition to the Paper Currency, and yet the Members were no sooner got together but . . . they determin'd on a Bill to add £20,000 to the £80,000 now Current, so as to make the whole paper Currency amount to £100,000, and the reason assign'd for going into the Bill at this time is that all the Gold & Silver may very soon be

> sent out of the Province, & that a scarcity of Money ensuing on a Plenty the People will be reduc'd to great hardships. I believe it is true that great quantitys of Gold & Silver are daily going to Virginia & Maryland for Bills, & that as the Exchange is £72½ & in some few Instances I have heard £75 abundance will be Shipp'd off, but what then? There is at least £300,000 of Gold & Silver now Currt. The Havannah & St. Augustine Trade does now subsist & is likely to do so, & will be every three or four Months bringing in fresh Supplies. The West India Trade must always pour in Gold & the Bills of Jersey & NewCastle will always make some addition, so that it is thought by some of the most eminent merchants that notwithstanding the drains of Gold to Virginia & ~~Maryland~~ England there will still be enough to answer all the Purposes of a Currency. (Peters 1750)

3. The legislature's own pronouncements confirm Peters' assessment. An assembly committee in 1754 reported that in the previous twenty years Pennsylvania's foreign and domestic trade and population had tripled and the need for a medium of exchange had increased in proportion. Over that time, the quantity of paper money had not increased, and "our Foreign and Domestic Commerce could not have been carried on as it has been for some years past, had not the accidental Introduction of great Quantities of Silver and Gold, by the War, supplied the Deficiency of our Paper Currency for that Purpose." The committee went on to call for a new emission of paper money on the grounds that the quantity of gold and silver in circulation was diminishing and that the existing bills were scheduled for retirement (Franklin, Labaree et al. 1959, 5:194). In 1757, in reply to a message from the governor, the assembly remarked that in the early 1750s, "when we had but Eighty Thousand Pounds current in Bills of Credit, there was current in the Province at least Four Hundred Thousand Pounds of Gold and Silver" (Hoban and MacKinney 1931, 6:4522).

4. In 1753, the receiver of quit rents, Richard Hockley, wrote Thomas Penn expressing a skeptical attitude about the shortage of money then said to be prevailing:

> The People are very pressing for an Emission of more money, P[er]haps their application may be though a little

> unseasonable, however[.] If they must have it then twill not be granted until you come, and by Yourself[.] Our produce still continues very high and when brought to markett always finds purchasers frequently outbidding each other, which I think plainly proves there is not that scarcity as pretended, and full four fifths of the money rec[eive]d into your Office is Gold and Silver but chiefly the latter. (Hockley 1753)[15]

[15] An earlier version of Grubb's paper dismissed Hockley's letter on the grounds that the proprietor's tenants were required to pay their quit rents in sterling, and therefore were under a contractual obligation to pay in specie. Hockey's comment that "four fifths of the money received ... is Gold and Silver," Grubb (2001, 35-6, fn. 11) concluded, is "patently unrepresentative of the ratio of specie to paper money circulating as a medium of exchange within the colony."

There was a longstanding dispute between the proprietor and the Assembly over the payment of quit rents and Grubb fails to recognize the role of Pennsylvania's legal tender laws in the controversy. When Pennsylvania first issued its bills of credit it made them a legal tender in the strongest terms.

> The tender of the said bills for payment or discharging of any debt or debts, bargain, sale of lands, or other things, bonds, mortgages, specialties and contracts whatsoever, already made, or hereafter to be made, either for sterling money, silver money of America, dollars, or any other species of gold or silver, or any quantity of plate or gold, shall be as effectual in the law, to all intents and purposes, as if the current silver coin of this province had been offered and tendered for the discharge of the same or any part thereof. (Pennsylvania, Mitchell et al. 1896, 3:329)

The act further stated that bills of credit were to be taken as "proclamation money," which Queen Anne's Proclamation of 1704 set at four pounds proclamation money to three pounds sterling; by the 1730s the market exchange rate was substantially higher, roughly five Pennsylvania pounds for every three pounds sterling (McCusker 1978, 183-6). The legal tender act forced the proprietor to take Pennsylvania bills of credit valued *at the Proclamation rate* in fulfillment of sterling quit rent obligations. By paying in bills of credit, tenants saved and the proprietor lost approximately one Pennsylvania pound for every three pounds sterling in quit rents due. Beginning in 1732, the Proprietor tried to put an end to this practice by requiring new tenants to sign an agreement to pay their quit rents either in sterling money "or its value in Currency, regard being had to the rate of exchange between Philadelphia and London." Such an agreement, however, was an example of a "specialty," for which the bills had already been declared a tender. It is doubtful the courts enforced it. In 1739, the governor, in response to the proprietor's instructions, refused to consent to a re-emission of bills until the assembly acted to redress this grievance (*Pennsylvania Gazette*, 25 January 1739, 15 February 1739). The assembly responded by passing an act paying partial compensation to the proprietor for the losses he had sustained. In return, the bill stipulated that the proprietor would accept bills of credit "as then current" (i.e., at Proclamation rates)

The evidence strongly supports the conclusion that colonial Pennsylvania possessed at least £300,000 in specie in those years rather than the £4,317 that Grubb estimates.

APPENDIX 2: Cross-Colony circulation of bills of credit: The evidence for Pennsylvania

This appendix reviews the archival evidence showing that Pennsylvania bills of credit circulated outside Pennsylvania and that bills of other colonies circulated within Pennsylvania. The evidence also reveals that contemporaries understood and commented on seigniorage and that it was indeed a bone of contention in colonial times.

We begin with the circulation of Pennsylvania bills of credit in Maryland, particularly in the late 1750s and early 1760s. Understanding how Pennsylvania's bills became entrenched in Maryland requires some knowledge of the peculiarities of Maryland's own bills of credit. Uniquely, Maryland paper money was redeemable with fixed and certain quantities of specie in 1748 and 1764. Maryland carried out its 1748 redemption as promised and its redemption fund in London was more than adequate to meet the second payment, so its bills of credit, which had depreciated well below their par value, became an attractive investment security hoarded by those who wished to profit from their appreciation (Brock 1975, 422-423).

in payment of quit rents upon grants made before 1732, and that on grants made after 1732 payment would be made "according to the tenor of the grants" (Pennsylvania, Mitchell et al. 1896, vol. 4, 324). Since all grants made after 1732 included the provision that payment should be in sterling or its value in currency, the provision would have permitted payment in bills of credit at their actual value. However, the assembly did not amend its legal tender act. According to Hutson (1970, 431), the post-1732 tenants persisted in using the legal tender provisions to pay quit rents with overvalued bills of credit until the Privy Council intervened in 1760 (Franklin, Labaree et al. 1959, vol. 9, 150-1). Only under duress did Pennsylvania amend its legal tender provision in 1764 to exclude sterling rents due to the proprietor (Pennsylvania., Mitchell et al. 1896, vol. 6, 363). If Hockey's estimate is biased, it is likely to *understate* the ratio of specie to paper in circulation, since the legal tender acts clearly gave pre-1732 tenants an incentive to pay in bills of credit. If Hutson (1970) is correct, the incentive existed for all tenants.

Before 1764, Maryland bills of credit were at best an auxiliary medium of exchange, and the colony possessed parallel accounting systems, one based on specie or "hard currency" and one based on its bills of credit. In 1752 and 1753, Maryland's courts and legislature cooperated to raise the rating of foreign coins so that they mostly agreed with the values prevailing in Pennsylvania, making Maryland's "hard currency" system nearly indistinguishable from Pennsylvania's monetary system. The dollar, the bellwether coin in colonial America, was thereafter rated at 7s. 6d. in Maryland, just as in Pennsylvania and New Jersey (McCusker 1978, 191-194; Brock 1975, 415-417). That condition provided impetus in Maryland for the use of specie and Pennsylvania and New Jersey bills of credit, especially when Maryland's own bills of credit began disappearing from circulation in anticipation of their redemption (Gould 1915, 14-15).[16] In January 1762, Henry Callister, an Eastern shore tobacco merchant and planter, wrote to a correspondent: "When I said currency, which does not imply Maryland [paper] money, of which there is hardly any current—I think I was yet more particular, for I spoke of money and exchange as current in Pennsylvania, which is our current money at present" (McCusker 1978, 193). On 10 December 1765, William Lux of Baltimore echoed that sentiment in a letter to Reese Meredith of Philadelphia: "My brother only mentioned to me to pay you £75 Curr. I cant tell which he meant but I imagined it be to Pen[n]sy[lvania] as we hardly ever deal for Mary[lan]d as there is a difference from 15 to 25 p. Ct. therefore all our Transactions are for such Current Money as is in Circulation" (Lux 1763-1769).

Other evidence confirms that many Pennsylvania bills of credit circulated in Maryland in those years. To protest the Stamp Act, the Currency Act, and various other imperial regulations, Philadelphia merchants in 1765 laid out their grievances in a memorial to London merchants. Objecting to the Currency Act, the Philadelphia merchants pointed out that Pennsylvania bills of credit outstanding had already shrunk from £600,000 to about £293,000 between 1760 and 1765 and were scheduled to continue diminishing at the rate of £30,000 per annum. "A great Part of the said Bills, now current," they noted, "serve as a Medium of Trade for the neighboring Colonies of New-Jersey and Maryland, and particularly the last, which has received them, from a full Conviction of the Solidity of their Establishment, as well as from Necessity, having had no Currency of their own

[16] The Leslie Brock papers at Alderman Library, Series II, box 3, contain Brock's notes on Henry Callister's papers. Brock records a letter from Henry Callister to Mr. White, dated July 22, 1760, in which Callister writes: "Pennsylvania and Jersey money . . . are current here."

for some Years past, and without them, must have been greatly distressed in their commercial interest" (Merchants and Traders of Philadelphia 1765; for a similar petition, see Hoban and MacKinney 1931, 7:5826). John Dickinson concurred, writing in one of his pamphlets that "A great part of [Pennsylvania's] bills now circulating, are passing in the neighbouring provinces" (Dickinson 1895, 219).

A letter in the *Maryland Gazette* of 15 September 1763 complained of the "inundation" of bills of credit from neighboring colonies that Maryland experienced in 1759 and went on to point out how impolitic it was to permit Maryland's commercial rivals to profit from the circulation of their paper money within the colony. When Maryland drafted a report on its currency in 1764 in response to an inquiry from London, it reported its own paper money was almost entirely sunk "so that after the 29th of September next there will be no Paper Currency or Bills of Credit circulating in this Province except such as have been emitted in the Neighbouring Colonies and may for want of a sufficient Quantity of Specie in Circulation be brought in and paid away to the Inhabitants of this Province" (Browne, Hall et al. 1883, 32:95).

The account book of William Fitzhugh (1761-1764), a prominent merchant on Maryland's western shore, shows the dangers of assuming that references to pounds means bills of credit issued by that colony. Fitzhugh's extensive accounts contain many references to "pounds," while Pennsylvania and Virginia money are scarcely mentioned. Grubb (2004, 337) interprets this as proof that Fitzhugh relied on Maryland paper money and that Pennsylvania and Virginia bills of credit rarely circulated in Maryland.[17] But Grubb's analysis overlooks the existence of Maryland's parallel accounting systems—that for "hard currency" and that for Maryland bills of credit. Maryland's original bills of credit were never widely accepted on Maryland's western shore.[18] An English traveler who visited Maryland in 1742 remarked that "The Maryland [paper] Money is generally pretty good, but of a low Value, and this, again, is not taken on the Western

[17] Grubb makes this argument despite the fact that he observes in another context (2004, 331, fn. 2) that "merchant account books tend to record values in unit-of-account 'money.' They do not necessarily reflect the transfer of the physical currency itself."

[18] Grubb (2004, 337, fn. 6) cites a Philadelphia dry goods merchant's advertisements, placed in 1747 and 1750 in the *Pennsylvania Gazette* and offering to exchange Maryland for Pennsylvania money, to show that colonial paper money did not circulate as a medium of exchange in neighboring colonies. Grubb is certainly correct that colonial paper money did not *always* circulate in neighboring colonies, and since this issue of Maryland money was not even circulating as a medium of exchange *on Maryland's western shore*, it would be somewhat surprising to find it accepted *outside* Maryland.

Shore of Chesapeak, where only Gold and Silver is current" (Kimber 1998, 55). We do not have to conjecture that Fitzhugh used the "hard currency" system because his accounts clearly show it. By 1764 Maryland's paper money had appreciated and was on the verge of being redeemed at its par value of £133.33 Maryland money for £100 sterling. Accordingly, it had nearly reached its par value in the marketplace (McCusker 1978, table 3.9). Maryland's hard currency, being nearly indistinguishable from Pennsylvania's, purchased sterling bills at approximately the same rate as in Philadelphia: £166.77 for £100 sterling, compared to £172.86 in Philadelphia (McCusker 1978, tables 3.7, 3.8). To verify which accounting system Fitzhugh used, one must compare his exchange transactions to those benchmarks, a comparison that verifies he indeed used the "hard currency" system.[19] The near absence of annotations distinguishing between Maryland and Pennsylvania pounds is therefore easily explained: there was no need for Fitzhugh to distinguish between Maryland and Pennsylvania currency values unless he was proffered one of the few coins that bore a different rating in the two colonies.

Pennsylvania bills circulating in Maryland are only part of the story. Pennsylvania and New York bills of credit circulated in New Jersey, and those of New Jersey circulated in both Pennsylvania and New York. Although New Jersey emitted small quantities of bills beginning in 1709, by 1723 those initial emissions had largely been retired (Lester 1939). Meanwhile, both New York and Pennsylvania had begun issuing bills of credit. When New Jersey farmers took their produce to one of those colonies, as they often did, they were paid in New York and Pennsylvania paper currency, which then began to circulate in New Jersey, although it was not a legal tender there and was sometimes refused in payment. In 1724, James Alexander estimated that £20,000 of New York and Pennsylvania bills of credit circulated in New Jersey, 20 percent of all the paper money outstanding in those two colonies. Alexander computed that even at an annual interest rate of 5 percent, 3 percent below the common rate, New Jersey annually paid to her neighbors £1,000, which was more than the annual cost of New Jersey's government. It was slavery, he declared, for any people to have to support their own government and that of their neighbors too. New Jersey needed an emission of legal tender paper money, he concluded, so they would not be forced to rely so heavily on New York and Pennsylvania bills (Whitehead, Ricord et al. 1880, 1st series,

[19] McCusker (1978, table 3.8) derived many of his "hard currency" exchange rates from the Fitzhugh account books discussed here.

5:96. See also Governor Burnet to the Lords of Trade, 12 May 1724, Whitehead, Ricord et al. 1880, 1st series, 5:86-93).

New Jersey emancipated itself by getting its paper money to circulate in New York and Pennsylvania. The merchants of Perth Amboy, two years after the emission, signed a certificate attesting that New Jersey's paper money "Ever Since the Issueing thereof passed Current not only thro' all this province but also in the province of Pensilvania without any Scruple or discount thereon betwixt the Currency of Pennsylvania & of this province" (Whitehead, Ricord et al. 1880, 1st series, 5:154). Richard Partridge, New Jersey's agent, reported to the Board of Trade in September 1731 that New Jersey "situated between New York and Pensilvania and their paper money being currant in each, occasions the dispersing it through the whole and it's scarce a third part of it continues in their Province" (Partridge 1938).[20] In the mid-1730s the preamble of a law emitting more paper money stated that "the Bills of Credit of this Province have obtained a general Currency in our Neighboring Provinces, and by reason thereof great part of said Bills remain in the Hands of Persons residing in the said Provinces" (Whitehead, Ricord et al. 1880, 3rd Series, 2:474). In 1740, William Douglass (Davis 1964, 3:322) commented that "New-York bills not being current in Pensylvania, and Pensylvania Bills not being current in New-York; but Jersey Bills current in both, all Payments between New-York and Pensylvania are made in Jersey Bills." McCusker (1978, 170, fn. 124) provides several specific instances of New Jersey bills being so used. Douglass based his comments in part on a memorandum prepared for him by Alexander, who reported that "Philadelphia has always paid equal or more respect to New Jersey bills than their own and bills of Exchange being generally more plenty in Philadelphia than in New York great and many sums have been remitted in New Jersey money from New York to Philadelphia for purchasing bills of exchange there." Alexander also mentioned a specific reason New Jersey money was so widely used in Philadelphia.

> Philadelphia markets are chiefly supplyed with beef mutton and other eatables by the butchers going into New Jersey for the business of them where the people will hardly take

[20] Partridge's statement seems to echo that of Colonel Montgomerie, made in a letter dated 20 November 1730 (Whitehead, Ricord et al. 1880, 1st series, 5:289). Partridge made a very similar remark in a paper prepared for the Board of Trade 13 August 1735 (Whitehead, Ricord et al. 1880, 1st series, 5:418).

> any money but their own, which occasions Jersey money to be given in Philadelphia before their own money and the butchers sometimes give an advance to get it. A Monmouth County man lately told me (tho I believed he spoke something at Large) that the Philadelphia Butchers brought at Least ten thousand pounds a year into his county for fatt Cattle all in Jersey money. (Alexander 1740)[21]

After passage of the Currency Act of 1751, the Board of Trade became increasingly hostile to legal tender paper money. In response, the New Jersey assembly argued that "as it is chiefly the merchants of New York and Philadelphia that give life to our trade, our money must consequently sometimes pass through their hands" (Whitehead, Ricord et al. 1880, 1st series, 8:15). If New Jersey's money ceased to be a legal tender, even in New Jersey, while New York and Pennsylvania's bills of credit remained a legal tender, it would, the assembly feared, undermine the credit of their money in New York and Philadelphia. Governor Bernard wrote the Lords of Trade on 31 August 1758 confessing that he had no good answer to this argument, and in so doing made the following comment: "They [members of the assembly] say that this Province having a continued intercourse with the two neighboring Provinces of New York and Pennsylvania it is quite necessary that their bills should be current in the counting houses of New York and Philadelphia, which at present they are and it is the greatest Test of their Credit. In like manner the bills of N. York and Pennsylvania are current within New Jersey" (Whitehead, Ricord et al. 1880, 1st series, 9:134).

If the combined testimony of the assembly, merchants, and royal governors is thought inadequate, consider that of Anglican clergyman Andrew Burnaby, who passed through New Jersey in 1759-60. "The paper currency of this colony [New Jersey]," Burnaby wrote, "is at about 70 per cent. discount, but in very good repute; and preferred by Pennsylvanians and New Yorkers to that of their own provinces" (Burnaby, Wilson et al. 1904, 110).

[21] When Alexander's note was written, Pennsylvania had only £80,000 of its own paper money in circulation; if the anecdotal account of Philadelphia butchers accumulating £10,000 of New Jersey currency is to be credited, it would amount to fully 1/8th of the Pennsylvania currency extant.

Maryland and New Jersey were not the only colonies sharing paper money with Pennsylvania. On 4 October 1768, the New York Chamber of Commerce officially sanctioned the use of Pennsylvania's bills of credit in New York, each 7.5 s. of Pennsylvania bills to be treated as equivalent to 8 s. in New York money, an advance that reflected the rating of the Spanish dollar in New York (8 s.) relative its rating in Pennsylvania (7 s. 6 d.) (Stevens 1867, 10, 11, 18).[22] In addition, Delaware paper money "although without government sanction in Pennsylvania, was accepted by the merchants there" (Keith 1917, 2:673). Indeed, the merchants of Philadelphia advertised that they would work to abolish all distinctions between Delaware's bills of credit and Pennsylvania's and that they stood ready to accept one quarter of any money due to them in Delaware's bills. At the same time, the trustees of Pennsylvania's loan office also advertised their willingness to accept Delaware's bills for up to a quarter of the sum borrowers were obliged to repay (*Pennsylvania Gazette*, 4 April 1730). Twenty years later, Peters, in listing the reasons he felt a new emission of Pennsylvania bills of credit was unnecessary, wrote that "the Bills of Jersey & NewCastle [that is, Delaware] will always make some addition [to Pennsylvania's money supply]" (Peters 1750).

APPENDIX 3:
The rise of dollars in Pennsylvania runaway ads

Grubb (2004, figure 1, 340) uncovers an interesting pattern in the runaway ads: The proportion of ads offering rewards in "dollars" skyrocketed in the late colonial period. Before 1760, the fraction of ads offering rewards in "dollars" was negligible; at the end of the French and Indian War, the fraction was still only about 5 percent. However, in the late 1760s and early 1770s, the fraction increased steadily, reaching nearly 50 percent of all ads by 1775. Pistoles (a Spanish gold coin), which had been mentioned in 5-10 percent of ads before 1760, appear less frequently after 1760. Grubb interprets "dollars" as literally as he does "pounds" and takes

[22] Pennsylvania currency enjoyed a limited circulation in New York from a much earlier date—e.g. *New-York Gazette*, 8 May 1732. In July 1768, before the Chamber agreed to officially sanction the circulation of Pennsylvania bills of credit in New York, it first voted down a motion that would have "hereafter discouraged [it] from passing in this colony."

this as evidence that Spanish silver dollars were playing a dramatically more important role as a medium of exchange in the late 1760s and early 1770s. We believe that Grubb has misinterpreted the meaning of "dollars," although the pattern is nonetheless striking and calls for some kind of explanation.

Grubb attributes the phenomenon (in his view a greater use of silver) to an improved balance of trade in the late colonial period and to the fact (2004, 340) that in the New World, "the ratio of silver to gold produced rose by 75%" after 1760. This explanation is weak, at best. First, specie stocks were vastly larger than annual production, so it would take a long time for the change in flow to greatly influence the comparative size of the available stocks. Second, in runaway ads the ratio of dollars to pistoles (the gold coins Grubb finds mentioned in runaway ads) increased by well over 1,000 percent, not 75 percent.

As evidence of the improved balance of trade, Grubb (2004, 341) cites Altman's estimates of the current account trade deficit of the colonies with England for 1771-1775. Much of this improvement, however, reflects political—not economic—developments. After Britain closed the port of Boston, the Continental Congress adopted a policy of nonimportation, effective 1 December 1774, a policy that doubtless had much to do with the improved trade deficit in 1771-1775. However, between fleeing Tories liquidating their estates and apprehensive British investors disinvesting in America, the net effect on the colonial specie supply was to diminish it, not enhance it.[23]

Gresham's law provides a convincing reason to believe that the greatly increased mention of "dollars" in runaway ads did not arise from an inflow of Spanish silver dollars. In London in the late colonial period the gold/silver price ratio was less than 15 to 1, as shown in the rightmost column of Table 2.

[23] According to Philip Mazzei, who resided in Virginia between 1773 and 1779, "The first step taken by the Americans toward alienation from England was when they agreed not to buy anything from her anymore, but because private individuals were generally and heavily indebted, exportation was allowed for a set period of time, and not only of merchandize, but of specie as well...with the result that before communication between the two countries was cut off, America was left almost entirely without hard money" (Mazzei, Marchione et al. 1983, vol. 1, 325-6).

Table 2: London Silver and Gold Prices

Year	London price in d. of an ounce of foreign coined silver	London price in d. of an ounce of foreign coined gold	Ratio of gold to silver prices
1767	64.95	953.4	14.68
1768	64.96	950.9	14.64
1769	65.68	963.2	14.67
1770	66.29	961.2	14.50
1771	66.11	958.8	14.50
1772	65.48	958.9	14.64
1773	62.81	936.8	14.91
1774	62.23	932.0	14.98
1775	63.50	930.0	14.65
Source: *The Course of Exchange.* Average of monthly observations taken from the middle of each month.			

In Pennsylvania, the Spanish dollar passed current at a value of 7 s. 6 d. By the late 1760s, the most highly rated gold coins circulating in Pennsylvania (relative to their gold content) were the Portuguese Johannes ("joes") and half Johannes ("half joes"), rated at £6 and £3 respectively. Had the weights in the almanac table accurately reflected the weights of joes and Spanish dollars as they circulated in Philadelphia, the gold/silver price ratio would have been 15.06.[24] In fact, half joes of just 9 dwt. were routinely paid and accepted without a discount in Philadelphia (*Pennsylvania Gazette*, 3 December 1767; Stevens 1867, 69). Once allowance is made for the circulation by tale of underweight half joes, the gold/silver price ratio becomes 15.33.[25]

The point of those calculations is to demonstrate that Portuguese gold coins were overvalued relative to the Spanish dollar during the very period when the number of "dollars" appearing in runaway ads exploded.

[24] Seven shillings and six pence, Pennsylvania's rating of the dollar, is equal to 90 pence. Seventeen pennyweight and 6 grains is 0.8625 ounces, making silver's value 90/.8625 = 104.35 d. per ounce. A half joe, worth £3 = 720 d., and weighing 9 pennyweight and 4 grains = 0.4583 ounces, is gold at 720/.4583 = 1571 d. per ounce. Finally, 1571/104.35 = 15.06.

[25] Nine pennyweight is 0.45 ounces, so a half joe of this weight would be gold at 720/.45 = 1600 d. per ounce, making the gold/silver price ratio 1600/104.35 = 15.33. The dollars in circulation seem to actually have been slightly heavier than the value given in the almanac. Records of dollars remitted to London from Philadelphia in 1768 show the dollars sent had an average weight of 0.8675 ounces (Mildred and Roberts 1768).

Pennsylvanians should have retained joes and exported dollars, so it is impossible to reconcile Gresham's law with the sudden increase in "dollars" in runaway ads if the "dollars" were Spanish silver. A contemporary newspaper article penned by "Eugenio" (*Pennsylvania Gazette*, 3 December 1767) recognized this and criticized the high rating that merchants had bestowed on Joes. After calculations similar to those presented above, "Eugenio" concluded "if the present advanced Price continues, we must expect that every other Gold or Silver Coin will be as scarce as Ducats or Chequins [two particularly scarce and under-rated coins]." We know from the almanacs that Pennsylvania retained the high rating of joes that "Eugenio" protested.[26] Economic historians have long known that joes were flowing into the Middle colonies during this time, although they have generally failed to note the role that coin ratings played in the process (Lydon 1965).

Why, then, did more "dollars" appear in runaway ads? More precisely, why did Pennsylvanians find it increasingly convenient to use dollars as a unit of account?

Likely, there were several reasons for the change. First, reckoning in dollars became easier. Before 1760 the most commonly used gold coin in Pennsylvania was the pistole, rated at 27 shillings[27] or 3.6 dollars, a somewhat cumbersome number. But in 1767, when the ratings on joes and half joes were raised to £6 and £3 respectively, their values translated neatly to 16 and 8 dollars.

Second, colonial economic integration made it increasingly expedient to reckon in dollars, the one universally understood unit of account in British America. The Continental Congress and the new nation denominated its currency in dollars for that very reason. Local pounds, shillings, and pence were nettlesome for those who did business or traveled in colonies where different valuations ruled. Runaways often fled to other colonies, which might be why Pennsylvania runaway ads in the late colonial and early national period often offered rewards in dollars, while indentured servant contracts written at the same time were still being written in pounds.

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[26] In 1775, the overvaluation of Johannes and half Johannes became an issue once again, when several merchants attempted to raise the value of other gold coins that were rated "under their value, in proportion to Half Johannes" so as to put them "as nearly on a footing as possible" (Pennsylvania Gazette, 15 February 1775).

[27] That pistoles were rated at 27 s. in the late colonial period is evident from almanacs, one of which is reproduced as Figure 2, page 5 above. Pistoles were so rated at least as early as 1742 (*Pennsylvania Gazette*, 16 September 1742).

Finally, it is possible that Pennsylvanians were led to make greater use of dollars as a unit of account by the more extensive circulation of a dollar-denominated medium of exchange. If so, that medium of exchange was not silver but Maryland paper money. Maryland's cash on deposit in the Bank of England, against which it issued paper money, enabled it to be the one colony that quickly hit on a successful formula for emitting paper money under the restrictions imposed by the Currency Act of 1764. As neighboring colonies were forced by the Currency Act to redeem and burn the currency they had emitted during the French and Indian War, Maryland emitted a quantity of notes denominated in dollars, not pounds (Newman 1986).

The first emission of this new dollar-denominated money was authorized by an act passed on 6 December 1766 (Browne, Hall et al. 1883, 61:264-275). The next day, Governor Sharpe wrote Lord Baltimore to assure him that Maryland actually had a larger sum on deposit in England than the total face value of all the bills to be issued and that he was confident the bills would meet with wide acceptance although they were not a legal tender. "I am told," Sharpe reported, "that One of the principal Merchants in Philadelphia who knows on what Foundation the bills are to be emitted has declared that he will make no Difference between those Bills & the number of Dollars to be therein mentioned" (Browne, Hall et al. 1883, 14:351-2). So well was the new money received that six months later Sharpe was already toying with the possibility of a further emission. If the ministry approved, Sharpe noted, Maryland would be indifferent to the restrictions embodied in the Currency Act of 1764. "I think it is for our Interest [the Currency Act] should remain in force," Sharpe opined, "since our Mercantile People have now money for the purposes of Trade while those in the neighbouring Colonies are stinted as it were for Want of a Circulating Medium & if necessity makes them receive & circulate our Money they must in fact pay us Interest for it as in that Case we are to them in the nature of Bankers" (Browne, Hall et al. 1883, 14:390-391). Between 1767 and 1775, the *Pennsylvania Gazette* specifically mentioned Maryland bills of credit in ads warning of the circulation of counterfeit notes, in ads reporting thefts, and in ads seeking the return of lost pocketbooks, which strongly suggests that Maryland bills did circulate to some extent as a medium of exchange in Pennsylvania, as Sharp had predicted they would.[28]

[28] See *Pennsylvania Gazette*, various issues. For warnings about counterfeit notes, see 3 March 1768, 3 May 1770, 23 June 1773; for thefts, see 14 September 1769, 12 March 1772, 13 December 1775; for lost pocketbooks, see 12 March 1772, 9 July 1772, and 18 January 1775.

Moreover, Maryland money passed in Pennsylvania interchangeably with Spanish dollars in accord with the face value of the bills. The best evidence of this is *The Gentleman's and Citizen's Pocket Almanack for ... 1772*, a Philadelphia almanac that printed the following table.

Figure 3: Value of Maryland currency in Pennsylvania

The following Table ſhews the Value of the Bills of Credit of Maryland, as they are divided into different Parts of Dollars, in Pennſylvania Currency.

Maryland. Parts of a Dollar		Pennſylvania. *l.*	*s*	*d.*
1 9th	is	0	0	10
2 ditto		0	1	8
1 6th		0	1	3
1 3d		0	2	6
2 ditto		0	5	0
1 dollar		0	7	6
2 ditto		0	15	0
4 ditto		1	10	0
6 ditto		2	5	0
8 ditto		3	0	0

Source: *The Gentleman's and Citizen's Pocket-Almanack For…1772*.

Spanish dollars, it may be recalled, passed current in Pennsylvania at 7 s. 6 d., exactly like the Maryland dollars listed in the table above. The multiples and fractions of dollars in the table are all in this ratio. Moreover, the fact that this information was published in an *annual* publication

In addition, an ad placed 12 September 1771 mentions a Pennsylvania runaway who absconded with a ragged ninth of a dollar bill, Maryland money.

suggests the value of Maryland bills in terms of Pennsylvania money were not subject to day-to-day fluctuations.

A nice snapshot of Pennsylvania's money supply on the eve of the Revolution can be found in a letter from William Pollard to William Killen on 26 October 1773 (Pollard 1773). Killen, in Dover, Delaware, had remitted some money to Pollard, a Philadelphia merchant, but when Pollard counted it, he found the remittance a bit short, forcing Pollard to write and explain the difficulty.

> I have duly rec'd your Favour by Mr. Thos. Rodney, but instead of £.54..7..6 there was only £.51..7..6 the particulars of which you have at Foot... my young Man counted the Money in Mr. Rodney's presence & Mr. Rodney also counted it but could make no more of it

1 Jersey Bill	£6		
2 Eight Dollars	6		
1 Pennsylvania bill	3		
2 Four Dollar	3		
1 One Dollar		7	6
11 Half Joe	33		
	£51 . .7. .6		

There were no coins in "Eight Dollar" and "Four Dollar" denominations but Maryland bills of credit did come in them. Even "1 One Dollar" seems unnecessarily redundant to refer to a single Spanish dollar; it is likely all these referred to Maryland bills of credit, and "One Dollar," "Four Dollars," and "Eight Dollars" were their denominations. Note the predominance of Portuguese half joes, the presence of a "Jersey bill," and the comparative unimportance of Pennsylvania bills among the sum tendered. That transaction nicely illustrates how Pennsylvania pounds functioned as a unit of account. All the disparate media of exchange were reduced to a common unit of account, in this case Pennsylvania pounds, despite the fact that Pennsylvania bills of credit played a minor role in the transaction.

Maryland's dollar-denominated bills of credit, as certain as their security must have seemed, nonetheless met an unhappy end. During the Revolution, Maryland's deposits at the Bank of England were frozen pending settlement of other outstanding claims, and Maryland never recovered the funds. Years after the Revolution, Maryland's colonial bills of credit still had not been redeemed, so a meeting of all the holders was held in Philadelphia "endeavouring to procure justice...to the holders of said bills of credit" (*Pennsylvania Gazette*, 27 October 1790). That it was found necessary to hold such a meeting in Philadelphia suggests that many of Maryland's bills of credit were in the possession of Pennsylvania residents.

Readers should keep in mind that in our view the medium of exchange cannot be inferred from the unit of account. So the discussion above emphatically should *not* be taken to mean that every mention of a "dollar" in late colonial Pennsylvania referred to Maryland's dollar-denominated bills of credit, but only that, without further information, it *could* have referred to them. References to "dollars" could also signify use of the dollar as a unit of account and hence simply be a means of accounting for a variety of other exchange media, including even *gold* coins.

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GO TO REPLY BY FARLEY GRUBB

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Theory, Evidence, and Belief—The Colonial Money Puzzle Revisited: Reply to Michener and Wright

FARLEY GRUBB *

There's letters seal'd: and my two schoolfellows,
Whom I will trust as I will adders fang'd,
They bear the mandate; they must sweep my way,
And marshal me to knavery. Let it work;
For 'tis the sport to have the engineer
Hoist with his own petar: and 't shall go hard
But I will delve one yard below their mines,
And blow them at the moon: O, 'tis most sweet
—*Hamlet*, Act III, Scene IV

Preface

Econ Journal Watch contacted me for the first time about this exchange just one month before this appears. I had about two weeks to write this reply such that it would appear along with the Michener and Wright comment published here, rather than in the subsequent issue. Because I believe for readers' sake that authors should reply and should follow hard on the heels of their attackers, I have had to work in sudden haste. I apologize in advance for the reply's rough and unpolished hue. This

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The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research. Editorial assistance from Tracy M. Cass is gratefully acknowledged

Michener and Wright comment on my article in *Explorations in Economic History* was rejected by *Explorations in Economic History* for reasons unknown to me by referees unknown to me. Given that this Michener and Wright comment is being published here I feel I owe it to these unknown referees to defend their judgment and integrity.

INTRODUCTION

AMONG THE GENERAL POPULACE OF COLONIAL AMERICA DID cash transactions take place predominantly in specie or in the paper money of particular colonies? To what extent did one colony's paper money circulate freely as a media of exchange among the general populace of other colonies? Answering these questions is important for defining the proper geographic unit for monetary analysis, i.e. the individual colony, some subset of colonies, or just the entire globe; for analyzing how the colonial paper money system worked; and for assessing the political problems with that system. Lacking quantitative evidence, modern scholars have relied on literary sources and anecdotal quotes from the colonial period to answer these questions. These sources, however, are ambiguous, and modern scholars have come down on one side or the other (and sometimes on both sides) depending on which of these anecdotal quotes they select to champion.

Many of these literary sources and anecdotal quotes are also highly partisan, biased, polemical, and full of subterfuge. Taking them at face value can be misleading. Some colonial writers used the assertion of specie plentitude as a tool to argue against the emission of paper money that they opposed on other grounds. And other colonial writers used the assertion of specie scarcity and/or that their colony's paper money had disappeared into other colonies as a tool to argue for emissions of more paper money that they desired on other grounds. In addition, colonial merchants involved in international trade and in cross-colony trade handled significant amounts of specie passing through the colonies (imports then exports of specie) and dealt in the exchange of one colony's paper money for another colony's paper money. Their experiences were different from, and the anecdotal evidence from this group is unrepresentative of, the general populace. My recent examinations of the context, representativeness, polemical bias, motivations, relevance, and veracity of this body of evidence has led me to conclude that the claims for specie-plenitude and cross-colony circulation of paper money *among the general populace* cannot be credibly sustained. In addition, based on an extensive survey of pamphlets, broadsides, and other literary sources on money from 18th-century colonial America, Christine Desan (2005) recently concluded that the preponderance of evidence falls on the specie-scarcity side.

Ronald W. Michener and Robert E. Wright (2005, 2006) and Ronald W. Michener (1987, 1988) passionately disagree. They believe that the colonies were awash in specie, enough to dominate the media of exchange most of the time, and that paper money flowed freely and extensively across colonial borders as a circulating media of exchange. Michener and Wright, however, do not grasp or care about the issues mentioned above. They frequently conflate evidence on monetary usage among merchants dealing in international and cross-colony trade with what was going on among the general populace. And Michener and Wright typically select only the anecdotal evidence that supports their beliefs—seldom parsing it to determine the context, representativeness, polemical bias, motivations, and veracity of this evidence. The anecdotal evidence that contradicts their beliefs they typically ignore. For example, Brock (1975)—Michener and Wright's definitive authority on colonial money—presents as many or more anecdotal quotes on specie scarcity as on specie plenitude. Michener and Wright simply ignore the specie-scarcity quotes.

The ambiguity of the literary and anecdotal evidence on money from colonial America cannot even be completely suppressed by Michener and Wright (2006). In their zeal to attack Grubb (2003, 2004, 2006) and promote their two beliefs, they present evidence for one belief that contradicts the evidence they just presented for their other belief. For example, Michener and Wright's evidence for cross-colony circulation of paper money is frequently based on colonists' claims of specie scarcity—contradicting Michener and Wright's claim of specie plenitude.

The attack here by Michener and Wright (2006) is not an isolated event. Over the last 20 years the debate over colonial money has become strident, in no small part due to Michener and Wright. For example, see Michener's attack (1987) on Bruce Smith (1985a, 1985b) and Smith's reasons for not replying (2000); Michener's attack (1988) on Charlie Calomiris (1988a) and Calomiris' reply (1988b); Michener and Wright's attack (2005) on Farley Grubb (2003) and Grubb's reply (2005a); and now this exchange. In addition, other writers on colonial money have privately related stories to me of being accosted by Michener after they published a book or article touching on colonial money, e.g. Newell (1998). Why such strong passions, such vituperative verve? Why do Michener and Wright wage such unrelenting and uninvited war against all who do not blindly accept their beliefs—especially given such ambiguity in the colonial literary evidence on these issues?

I cannot hope to uncover the full mystery of this or debunk everything they say—not to say that it cannot be debunked. But space and time limit this discourse. All I can do is give examples. This restricted outcome may be a deliberate strategy of Michener and Wright. Their tactic is to bury their opponent in a pile of anecdotal quotes, more than their opponent can sift through and deconstruct. It is far easier for them to grab another quote out of context from some website or secondary source, than it is for their opponent to take the time to track it back to its original source and determine the context, veracity, relevance, and motivations of the writer. Michener and Wright's response to being challenged on this is to up the ante by accusing those who question the veracity of their evidence of the same behavior they are engaged in and then pile on more unparsed anecdotal quotes. Such strong passions, such strong wills-to-believe are typically driven by theoretical and not by empirical considerations. So the search for why must begin there.

THEORY TURNED INTO IDEOLOGY

The story begins when West (1978) found no correlation between changes in the amount of paper money issued by a colony and changes in prices in that colony. Was this a violation of the classical quantity theory of money? A spate of new research into colonial money tried to explain West's finding. One line of research focused on the possibility that money demand was not invariant over time and might be systemically related to changes in the supply of colonial paper money (Smith 1985a, 1985b; Wicker 1985). This line of research assumed that the colonial institutional setting led to a violation of the assumptions underlying the classical quantity theory of money.

Now the classical quantity theory of money was a more-or-less sacrosanct theory at the University of Chicago, at least in the late 1970s and early 1980s among many of my and Michener's fellow graduate students there. It was truth with a capital "T". Thus Michener's attack (1987) on Smith (1985a, 1986b) is not surprising. Michener (1987) offered an explanation of West's findings (1978) that did not violate the classical quantity theory of money—based on the simplistic currency substitution models that were often used as teaching tools in economic graduate programs in the late 1970s and early 1980s.

In particular, a colony's paper money was not the only money available. International monies, gold and silver specie coins, also flowed in and out of a colony and were used as money. Thus a colony's total money supply was the sum of that colony's paper money and the amount of specie coins in that colony. If exchange rates are perfectly fixed between a colony's paper money and international specie coins, and if the colony's money demand is perfectly invariant, and if there is a large reservoir of specie in the colony at all times (enough to fully offset any changes in paper money), and if transactions and information costs are zero, then changes in a colony's paper money supply will be fully offset by counter-flows of specie coins into or out of the colony leaving the colony's total money supply perfectly constant. The result of such a set of circumstances is that there will be no correlation between changes in the colony's paper money supply and changes in prices in that colony. Thus, West's findings are explained without violating the classical quantity theory of money.

While "saving" the classical quantity theory of money, this model also makes it a useless empirical tool for studying colony-specific shocks, because only the global money supply matters. It also implies that a colony's paper money supply should circulate freely in all other colonies—actually freely around the globe the same way that specie coins did. Any location's money supply is determined solely by its money demand, a demand that is assumed to be invariant. The assumptions used by Michener also constrain the quantity theory of money to hold perfectly even in the short-run.

Structured as a logical argument, Michener's model is as follows:

If (A): (i) There is a perfectly fixed exchange rate between a colony's paper money and foreign specie monies (they are perfect substitutes), and
(ii) money demand is invariant over time even in the short-run, and
(iii) there is a large reservoir of specie in the colony at all times, and
(iv) transaction and information costs are zero,

Then (B): Changes in the paper money supply will be unrelated to changes in prices.

West (1978) showed that (B) is true. Because (B) is true, Michener deduced that (A) must also be true, i.e. as theory it must be the truth with a capital "T". In Michener's world, evidence to show that (A) is true or research to investigate whether (A) is true is unnecessary and any evidence that shows that (A) is not true must be wrong—because theory tells us that (A) must be true. As such, evidence on (A) is not and should not be taken seriously and is only offered when some misguided empiricist wants some evidence—because theory has already told us that (A) must be true.

Anyone who dares to claim that (A) is not true or who offered evidence that (A) is not true must be wrong and must be attacked and dispatched. Calomiris (1988a) and Smith (1985a, 1985b) did not hold that (A) was true, and so were attacked by Michener (1987, 1988). Grubb (2003, 2004, 2005a, 2005b, 2006) offered evidence that is inconsistent with (A) being true, and so he too must be attacked and dispatched, see Michener and Wright (2005, 2006). Michener and Wright must find some way to discredit any evidence that shows that (A) is not true, even if that means discrediting all evidence—for evidence is not really necessary in their world. Belief in (A) overpowers all.

Michener's Logical Fallacy

Freshmen logic teaches us that given the true proposition—"If (A) is true, then (B) is true"—it does not follow logically that you can conclude "If (B) is true, then it must follow that (A) is also true." To so deduce that (A) is true is to commit a fallacy of logic. Intuitively, this is because the true proposition—"If (A) is true, then (B) is true"—does not rule out many other circumstances (C), (D), (E), etc. that could also make (B) true. Thus, the truth of (B) cannot be used to deduce the truth of (A). It could just as easily be that (D) is true rather than (A). Yet this is exactly the logically fallacy that Michener has fallen into. Covering up this error of logic may explain why Michener has to defend his position so stridently.

THE EVIDENCE—DO MICHENER AND WRIGHT CARE?

In the rest of this reply I will show that Michener and Wright are only interested in evidence in so far as it can be used as superficial propaganda for their theory of colonial money. They are not interested in the integrity of evidence, or history, for its own sake. Their belief in (A) overpowers all else.

Setting the Tone—Michener and Wright's Opening Contradiction

Leopards do not change their spots and so I suspect neither would Michener and Wright (2006). And right at their opening, in footnote 1, they do not disappoint. Michener and Wright assert that "Most historians now argue that, for the most part, it [the colonial money supply] was adequate." As proof for this assertion they cite McCusker and Menard (1985, 338) and Perkins (1994, 54) in their footnote 1. They also deduce right after this sentence that "adequate" means a lot of specie in circulation, more specie than paper money.

Now if you actually read McCusker and Menard (1985, 338) their conclusion that "the colonists' stock of money was adequate. . . ." is derived from Hamilton's estimate were he implies that 83 percent of the money supply was paper money and only 27 percent was specie money (Ferguson 1973, vol. 1, 35). Not only does this directly contradict how Michener and Wright characterize "adequate money supply" in their paragraph, but later in Michener and Wright (2006), recycled from Michener (1987, 278), they trash and reject Hamilton's estimate because it shows too little specie to paper money.[1] Michener and Wright (2006) appealing to McCusker and Menard (1985, 338) as support for their position and then trashing the evidence that generated that support is contradictory. They cannot have it both ways.

[1] Michener and Wright (2006) and Michener (1987, 278) argue that Hamilton's estimates of the magnitudes of specie (x) and paper money (y) in the pre-revolutionary economy are off by some factor, i.e. $a*x \neq x$ and $b*y \neq y$. Michener and Wright infer from this that it must also be true that $(x/y) \neq (a*x/b*y)$. But clearly this is only true if $a \neq b$, something Michener and Wright do not know. By contrast, if Hamilton's error is purely the result of sampling the population of transactions and he just got the scalar wrong when aggregating up to the national level, then it is likely that $a = b$, and thus his implied ratio of specie to paper money is still correct.

Michener and Wright (2006) also cite Perkins (1994, 54) as concurring with their position here and quote him as saying "there is little reason to believe that the population of British North America suffered much, if at all, from an inadequate money supply" (Michener and Wright 2006, fn. 1). Now Perkins does not quite say this. Michener and Wright have misquoted him. Perkins says "inadequate monetary system" not "inadequate money supply." The difference is subtle but important. Perkins is not talking about the money supply at all here, or implying that there is a lot of specie in circulation. He is merely noting that the colonies experienced real economic growth and a high standard of living, and whatever monetary system they had did not inhibit that. This is classic Michener and Wright behavior—repeated often throughout their work.

The Unit-of-Account Doomsday Weapon

Michener and Wright could always counter those who disagreed with them based on anecdotal evidence by simply piling on more anecdotal evidence that favored them—since there is tons of such on both sides of the debate. Faced with quantitative evidence that disagreed with them (Grubb 2003, 2004, 2005a, 2006), they needed a different strategy. That is when they came up with the unit-of-account doomsday weapon. They could dismiss any evidence that disagreed with them simply by declaring it to be unit-of-account evidence and not media-of-exchange evidence. They offer no criteria for determining when a record reflected unit-of-account money and when it reflected media-of-exchange money, except that any evidence that disagreed with them must be unit-of-account evidence, and any evidence that agreed with them must be media-of-exchange evidence. They do not actually know whether or when any given piece of evidence is being expressed in unit-of-account terms, they just assert that it is. Without a time machine, who can prove them wrong? This is a doomsday weapon in that it can be used to eliminate all evidence on the media of exchange, which would be fine with Michener and Wright because they already know the truth from their theory—who needs evidence anyway?

What is missing in Michener and Wright's analysis is any model for determining what money the unit of account will be in, when and why this unit will shift to being a different unit of account, when and why multiple units of account will exist in society at large or within the same individual transaction, and so on. Related to this is Michener and Wright's failure to craft any testable hypotheses or do any hypothesis testing regarding unit-of-account usage.

For example, one model might suppose that the whole point of using a unit of account is to translate all other values into it for comparison purposes across space and time, as illustrated in Michener and Wright (2006, Appendix 3, William Pollard Letter). As such, there should be only one unit of account across society and any change in the unit of account should be a discrete complete shift. Multiple units of account simultaneously used by society at large, and within the same transaction, should not be observed. Under such a model, were rewards in runaway-servant ads expressed just in units of account? The evidence in Grubb (2004, 340) rejects this. There were at least three monetary units in use in society over the relevant period often used in the same year and by varying degrees. In addition, multiple monetary units show up often in the same transaction.

Now let's suppose an alternative model. Following Michener and Wright-style logic, because unit-of-account monies were all fixed one to the other in coin-rating tables in almanacs (see Michener and Wright 2006, Figures 2 and 3) the choice of particular monies for use as a unit of account was indeterminate or a random act, i.e. since all unit-of-account monies were perfect substitutes it would not matter which was used. Under this model, were rewards in runaway-servant ads expressed just in units of account? Again, the evidence in Grubb (2004, 340) rejects this. The coin-rating tables list a dozen or more monetary units, but the runaway reward evidence at best only reveals three monetary units in substantial use. In addition, even if one confines the potential units of choice to these three, the evidence in Grubb (2004, 340) rejects any random pattern of selection over space and time.

Finally, let's go at it a different way. There are four *indisputable* markers or point estimates from non-runaway-reward sources between 1730 and 1775 that deal with the ratio of specie to paper money as media of exchange in Pennsylvania. (Indisputable here means that even Michener and Wright have not disputed or have accepted these estimates in their published work to date.) What are the chances that Grubb's evidence (2004, 340) could hit all four media-of-exchange markers on the nose as a random accident—using random as our model of unit-of-account choice? The first indisputable marker is that, as Benjamin Franklin put it, there was next to no specie in Pennsylvania in the late 1720s (Lester 1938; McCallum 1992; Michener and Wright 2006; Nussbaum 1957, 27). The second marker is from Pelatiah Webster who estimated that 50 to 60 percent of the money in

Pennsylvania in 1774 was paper money (Webster 1969, 142). Grubb's evidence (2004, 340) hits both these markers pretty much on the nose. Suppose we use 10 percentage point increments as our margin of error for randomness. Then even with just these two markers, the chances that Grubb's evidence is really just unit-of-account evidence that happens by chance to hit these media-of-exchange markers is (0.1 * 0.1) or 0.01—very improbable.

Now let's add in two more indisputable markers. Virtually all sources agree that specie flowed in during King George's War increasing the specie to paper money ratio only to flow back out afterwards decreasing the specie to paper money ratio. And this pattern repeated itself during the Seven Year's War. What are the chances that Grubb's evidence (2004, 340) captures this rise then fall in the specie to paper money ratio during both these wars as a random accident? Given that the other two possible patterns would be no change and an inverse movement to what did happen, let's use one-in-three as our margin of error for randomness. Grubb's evidence (2004, 340) hits these two markers on the nose. Combining hitting these two markers with hitting the two markers mentioned above means that the chances that Grubb's evidence is really just unit-of-account evidence that happens by random chance to hit all four media-of-exchange markers is (0.1 * 0.1 * 0.34 * 0.34) or 0.001—extremely improbable. In conclusion, declaring some evidence to be units of account rather than media of exchange in order to dismiss it simply because one does not like the outcome is not sound empirical methodology.

Were Exchange Rates Fixed?

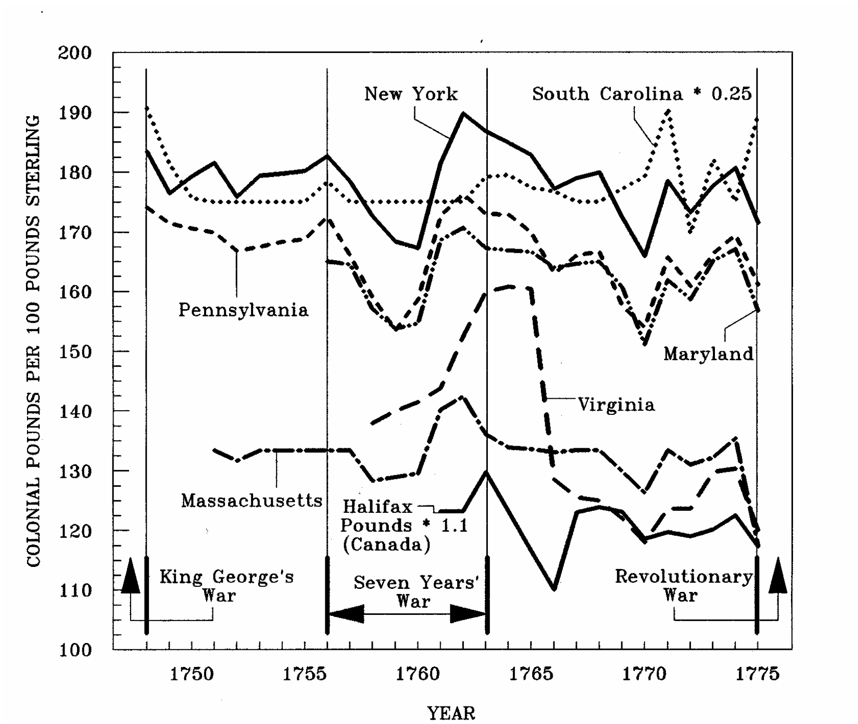
Michener and Wright assume that exchange rates were universally fixed in the colonial period. They focus less directly on this in Michener and Wright (2006) than they do in their prior work (Michener and Wright 2005, Michener 1987), but they assume it nonetheless for without this assumption their two core beliefs, that paper money circulated as media of exchange freely across colonial borders and that specie dominated the media of exchange, are jeopardized. If exchange rates were flexible then cost wedges arise between the paper monies of the various colonies, inhibiting their use as media of exchange outside the colony of issue, and specie flows become non-frictionless producing a time-dimension in the monetary equilibration process, i.e. short-run specie scarcity could occur.

The problem with Michener and Wright's fixed-exchange-rate-regime hypothesis is that there is no mechanism by which colonial governments could maintain fixed exchange rates in their paper monies. Colonial governments never redeemed on demand their paper money for specie or the paper money of other colonies, nor did they enter the market at their discretion to buy and sell their paper money for specie or the paper money of other colonies to defend a fixed exchange rate. Colonial governments did not have specie or foreign-exchange reserves as modern central banks do. Elsewhere Michener and Wright (2005) and Michener (1987) acknowledge this problem, but argue nevertheless that fixed exchange rate regimes were created, maintained, and enforced by custom or by agreements among merchants in the marketplace. However, they present no direct evidence of such a merchant cartel or discuss how it could rationally work. Instead, they deduce such from coin-rating tables published in almanacs, e.g. see Michener and Wright (2006, Figures 2 and 3). Rates of exchange are typeset in these almanac tables and so appear fixed and constant.

There are two problems with using this almanac coin-rating evidence to infer fixed exchange rate regimes. First, publishing exchange rates at a point in time does not make for fixed exchange rate regimes, no more than the currency-exchange table in today's *Wall Street Journal* proves that all the listed currencies are under fixed exchange rate regimes. Second, these almanac coin-rating tables are for unit-of-account exchange rates and not media-of-exchange exchange rates (Michener and Wright are hoisted on their own petard here). Thus, this evidence reveals nothing about what the exchange rates were among the media of exchange—which is all that matters for monetary behavior.

Finally, the universally used and accepted time-series on exchange rates in colonial America for the last quarter century (McCusker 1978) shows no such fixity or constancy in exchange rates, see Figure 1 below. Even the one exchange rate that we know has to be a pure unit-of-account exchange rate (because there was never any paper money issued in it)—the Halifax pound—is not perfectly constant or fixed. This evidence that exchange rates were flexible destroys Michener and Wright's core model of the colonial monetary system, and with it their core contention that all monies were perfect substitutes and so flowed freely and extensively all over.

Figure 1: Exchange Rates Between Colonial Pounds and Pounds Sterling, 1748-1775



Notes: Except for Lower Canada (Montreal and Quebec) where the unit of account money was called "Halifax pounds", each Colony refers to the exchange rate between in that Colony's pounds and pounds sterling, e.g. "New York" refers to New York pounds to pounds sterling.
Source: McCullough (1984, 266-68); McCusker (1978, 141-142, 164-165, 185-186, 198-199, 211-212, 223-224).

Oh Those Irrational Colonists—The Pennsylvania Evidence circa 1750

Michener and Wright (2006), citing Brock (1975, 354, 386), claim that 80 percent of the money supply in Pennsylvania circa 1750s was specie—contradicting and so discrediting the estimates in Grubb (2004) based on runaway rewards. Their point estimate and its source, recycled from Michener (1987, 282), is one they place great faith in. The original source for this 80 percent estimate is a single anecdotal quote taken from a letter Richard Hockley wrote to Thomas Penn in 1753. Hockley was the Pennsylvania receiver of quit rents for Penn who was the proprietor of the province. In reference to these quit rents, Hockley said that "full four fifths of the money recd into your Office is Gold and Silver. . . ." (Michener and Wright 2006). Michener and Wright lift material from an unpublished working paper of mine where I deconstruct this quote and in a preemptive strike attempt to discredit it (Michener and Wright 2006, fn. 15). Therefore, it is only fitting that the reader should see some of my original work.

Most quit rents, payments to the Lord Proprietor of Pennsylvania (the Penn family), were required to be paid in sterling (specie). Such payments, after all, were to be remitted to the Penn family in England (see *Pennsylvania Gazette*, January 25, 1739). Only with the Pennsylvania Legislature's Currency Bill of 1739, did the Proprietor agree, after much debate and assured compensation, to take Pennsylvania paper money instead of sterling in payment of quit rents, but only "upon grants made before the year one thousand seven hundred and thirty-two and upon all grants afterwards, according to the tenor of the said grants . . ." (*Statutes at Large*, vol. 4, 324; vol. 5, 14; *Pennsylvania Gazette*, September 13, 1739).

Based on the correspondence between Hockley and Penn, most quit rents on grants made after 1732 were required to be paid, by explicit contract, in sterling. For example, Hockley wrote to Penn on February 26, 1752 that "I have agreed with . . . the people at Reading for 100 lotts of 5 acres each at 15/ sterling [quit rent] per lot which will make 75 pounds sterling per annum. . . ." (*Penn Manuscripts*, vol. 5, 215). Property-sale advertisements tell the same story, as the following advertisement from the *Pennsylvania Gazette* (February 26, 1754) illustrates: "To be sold by John Snowden, sadler, living in Market street, Philadelphia, a good brick house...subject to a ground rent of Four Pounds, Ten Shillings, a year, and a quit rent to the proprietaries, of Two Shillings, and Six pence, Sterling, per annum..." For other examples, see the *Pennsylvania Gazette* issues 9/15/1737, 11/29/1744, 5/28/1747, 8/13/1747, 5/3/1750, 7/5/1750, 9/28/1752, 10/12/1752, 9/27/1753, 11/21/1754, 12/5/1754, 2/4/1755, 6/12/1755, 12/13/1759, 1/28/1762, 3/18/1762, 7/8/1762, 6/7/1764, 10/25/1764, 5/23/1765, 6/20/1765, 8/8/1765.

Between 1750 and 1765, 80 percent of the property sales advertised in the *Pennsylvania Gazette* that were listed as subject to quit-rent payments, and that enumerated that payment, explicitly indicated that said payments were required to be made in sterling. This is exactly the same percentage as the "four fifths" quoted above. Therefore, by contractual design the proportion of quit rents collected in specie is unrepresentative of the ratio of specie to paper money circulating as a media of exchange within the colony.

Lastly, while Hockley was an avowed opponent of paper money and openly lobbied Penn to resist approving of said (*Penn Manuscripts*, vol. 5, 183; vol. 6, 17, 67), he nevertheless had to admit to Penn on October 10, 1751 that "money [meaning specie for quit rents] is become very scarce. . . ." and again on May 11, 1753 that "in answer to your orders of making seizures for neglect of payment of your quit rents . . . [I] thought it not so prudent at a time when the country was clamouring for more money. . . ." (*Penn Manuscripts*, vol. 5, 183; vol. 6, 59, respectively).

Michener and Wright (2006, fn. 15) dismiss this analysis by saying that any agreement between Penn and the Pennsylvania assembly to pay quit rents in specie if so contracted after 1732 was overridden by Pennsylvania's legal tender law which allowed payment in overvalued bills of credit. Their proof of this consists of saying, "According to Hutson (1970, 431), the post-1732 tenants persisted in using the legal tender provisions to pay quit rents. . . ." (Michener and Wright 2006, fn. 15). However, Hutson (1970, 431) does not say this. Hutson's actual statement is ambiguous over whether it is pre- or post-1732 quit-rent contracts to which he refers legal tender laws thwarting. If it was the post-1732 quit-rent contracts of which tenants were thwarting payment in specie by using the legal tender law to pay in paper money, then Hockley's statement that "four-fifths" were paid in specie (quoted above) cannot be right. Michener and Wright's interpretation of Hockley's statement and Hutson's statement are mutually contradictory. Michener and Wright cannot have it both ways.

In addition, Michener and Wright's interpretation implies that the colonists were irrational fools. First, it implies that Hockley in his quit-rent contracts as well as property sellers in their newspaper listings, both cited above, were knowingly writing nonsense. Second, it implies that anyone who paid their quit rent in specie rather than in overvalued bills of credit, the 80 percent Hockley claimed paid him in specie, were fools.

To further support their point Michener and Wright (2006, Appendix 1) cite a Massachusetts pamphleteer, material recycled from Michener (1987, 295), who in 1749 wrote, "At *New York* and *Philadelphia* Silver is their Medium, and mill'd Dollars pass current at a known determinate Rate, and other foreign Coins in proportion: Paper Bills are sometimes the Instrument in Payment, but the Proportion is small compar'd with the Silver. . . ." The quotation is taken from the pamphlet *A Brief Account of the Rise, Progress, and Present State of the Paper Currency of New-England* written by an unnamed author and printed in Boston in 1749 (Davis 1964, vol. 4, 377-405).

The pamphlet is a polemic against paper money and its purpose is to convince Massachusetts residents that they should swear off paper money and return to a pure specie monetary standard. The three sentences in the pamphlet that immediately precede the passage quoted by Michener and Wright are instructive. They are: "I think therefore we may rest satisfy'd, no stable Currency can be projected, other than that of Silver and Gold. And here I expect to be ask'd: Why may not *New-England* have a Currency of Bills of Credit, as well as *New-York* and *Pennsilvania*? I answer" (Davis 1964, vol. 4, 387). There is no evidence in the pamphlet that the author knew the true state of affairs in New York and Pennsylvania. The author says nothing about these colonies in the rest of the pamphlet. As such, and given the polemical purpose, the author's claim that specie was the primary media of exchange in Pennsylvania in the late 1740s lacks credibility. Another polemical pamphlet that Michener and Wright (2006) use uncritically is Hanson (1787). Partisan polemical tracts should not be mistaken for truth or taken at face value.

Finally, Michener and Wright (2006, Appendix 1) present evidence that the governor of Pennsylvania in the early 1750s resisted approval of new paper money emissions by the Pennsylvania assembly because he thought there was lots of specie in the colony and so paper money was not needed. Michener and Wright take this evidence at face value. And yet, Brock (1975, 354-362)—Michener and Wright's definitive source on colonial money—shows that this talk of specie abundance was disingenuous, even calling it political "subterfuge." Penn knew that the temporary inflow of specie during the war would soon be gone and specie scarcity would return (which is also consistent with Grubb's (2004, 340) new evidence series). In London on October 9, 1749 Penn wrote to his governor in Pennsylvania, "[E]very one is sensible that in two or three years almost the whole of the Gold and Silver that during the war was brought into the Colonys will be shipped hither, and wee shall have little but paper left. . . ." (Brock 1975, 356).

Yet, Penn and his governors argued against new paper money emissions. The reason was that they wanted more shared control with the assembly over the spending of the paper money authorized. But they could not come right out and say that because as the governor explained "such an amendment 'would raise a great ferment among the people, and be considered . . . as a violent attack upon their liberties and priviledges'" (Brock 1975, 357-358). So instead, the governor resisted the paper money bills (disingenuously) on the grounds that they were not necessary given the supposed abundance of specie in the province. Brock (1975, 362) concluded that even by 1752 "The assembly was yet in the dark as to the real reason for the governor's refusal of their paper money bill." Taking anecdotal evidence at face value is often misleading, something Michener and Wright do not seem to grasp or care about.

Retaliatory Strikes Turn into Self-Refutations—The Jones and Mazzei Mess

Michener and Wright (2006) spill a lot of ink over two issues (1) how to interpret Jones (1980) and (2) how to interpret Mazzei (Marchione 1983, v. 1, 325-326). Michener (1987, 275) was the first to introduce Jones into this debate by manipulating her evidence to support his claim that far more specie was in the colonies than paper money circa 1774—far more than even Jones (1980, 132) claimed. Figuring out how Michener manipulated Jones' evidence to make it fit his view is not easily done. In Grubb (2004, 342-343) I figured that out and showed how it was done. Then I showed that plausible alternative assumptions could yield very different results—even one that hit my estimate of the ratio of paper money to specie based on runaway ads on the nose. The point being that the Jones evidence can be plausibly manipulated to say almost anything, from a money supply of almost no specie to one that is like 80 percent specie. The whole reason for searching for alternative money supply estimation methods was because evidence such as Jones (1980) does not yield the definitive answer that people like Michener wanted it to.

Michener and Wright misunderstood what I was doing and thought that I was asserting that my manipulation of Jones was the only plausible manipulation. And so they embarked on a lengthy diatribe of re-manipulation to show that I am wrong. But in the process they have shown exactly what I was showing, and better than I did, namely that the Jones evidence can be made to say almost anything and so is pretty useless for determining the composition of the money supply. Since Michener (1987) was the first to argue that Jones (1980) could be so used to determine the composition of the money supply, I take their analysis here as self-refutation, and as such Michener (1987) should not have introduced the Jones evidence in his cause in the first place and that he will not again in the future.

The debate over quoting Mazzei is most interesting. Michener and Wright accuse me of being intentionally misleading by inserting [colony] into the quote "state [colony] paper money circulated freely only within it . . ." (Grubb 2004, 339). Michener and Wright point out that Mazzei was referring to paper money during the revolution. However, before the Treaty of Paris recognized U.S. sovereign independence (1783), U.S. states were still colonies—albeit colonies in rebellion. Apparently, Michener and Wright are unaware of this. Second, if anything, state/colony paper money issued during the revolution should have circulated more freely in other states/colonies than it did before the revolution—a condition that makes Mazzei's quote here so interesting—an issue that Michener and Wright sidestep. Third, in their commentary on this quote, Michener and Wright do not make a distinction between paper money circulating "freely" among the general populace versus just within merchant communities dealing with cross-state/colony trade.

But the debate over quoting Mazzei goes deeper. Michener and Wright's attack on me over the use of Mazzei is actually a classic Michener and Wright tactic, namely preemptively accusing your opponent of what you yourself have done. Michener (1988, 687) was the first to introduce Mazzei into the debate over colonial money in his attack on Calomiris (1988a)—using Mazzei to support Michener's claim that specie dominated the money supply of the colonies. Michener (1988, 687) wrote, "Much of the prewar money supply consisted of specie. 'In 1773,' wrote one observer, 'all transactions were made almost entirely in specie.'" The observer was Mazzei. This statement didn't sound right when I first read it. How did this Italian know so much about colony money supplies? And Brock (1975, 1992) showed that all the major colonies except Massachusetts had issued significant amounts of paper money after 1760 and/or had significant amounts outstanding in this period. So I tracked down and read the treatise that was the original source of Michener's quote.

This is when I decided to never cite or quote evidence from Michener until I had tracked down the original source and confirmed it. For the actual sentence in question in Mazzei read, "In 1773, the year disorders began, that is, ten years after the end of the previous war, all transactions were made almost entirely in specie, which, however, did not abound." Michener had truncated and mis-punctuated the sentence in a way that substantially altered its meaning in reference to the issue addressed. It is hard to think that anyone is that incompetent. If a student of ours had done this, we all know what we would call it. I did not raise this issue in Grubb (2004), but I did quote Mazzei to let Michener, who had been stalking my working papers and publications, know that I knew what he had done—possibly the only person who knew. In his zeal to attack me, however, Michener fell for the bait provided in Grubb (2004, 342) and reported in Michener and Wright (2006, fn. 13) the larger Mazzei quote and so provided his own self-incrimination.

What I did raise in Grubb (2004, 342) was what I also discovered, namely that Mazzei was writing about Massachusetts and not about all the colonies. Besides only mentioning Massachusetts in his treatise and doing so in the antecedent sentence to the one in question, Mazzei's phrase above "In 1773, the year disorders began . . . " can only sensibly be referring to Massachusetts, e.g. the Boston Tea Party and consequent closing of Boston harbor by the British. Massachusetts had not issued new paper money since before the Seven Years War. Its lack of paper money *and* lack of specie in 1773 is both not surprising and a refutation of the Michener and Wright model of colonial money. Its lack of paper money in 1773 is also not representative of the rest of colonial America in this period.

On top of that, beside the ambiguity over specie scarcity or plenitude in the sentence by Mazzei quoted above, Mazzei also says in his treatise that "The extremely unfavorable trade the American States had with England . . . was responsible for their never having an abundance of specie. . . ." and "since for the above reason specie was often lacking, it had to be made up by bills of credit, that is, paper money. . . ." and "[B]efore communication between the two countries [England and America] was cut off, America was left almost entirely without hard money" (Marchione 1983, v. 1, 325-326). While one might be able to make Mazzei say anything one wants on the composition of colonial money via selective quotation, on balance he seems to come down on the side of specie scarcity and specie not being the dominant money supply within the colonies.

Anecdotal Quotes versus Archival Context—The Fitzhugh and Callister Examples

The Michener and Wright strategy is to pile on anecdotal quotes—any that give the appearance of supporting their beliefs—without deconstructing the context, veracity, or bias of their sources. Their tactic is to overwhelm their opponent who could never fully assess all the anecdotal quotes they pile on. I will give two examples that I have had time to parse.

In Appendix 2 on cross-colony circulation of paper money, Michener and Wright (2006) present evidence from Maryland merchants Henry Callister and William Fitzhugh as proof of their contention that Pennsylvania paper money circulated freely and extensively in Maryland. These are not new discoveries but evidence cited and recycled from Michener (1987, 236, 244). Michener also berated me with this evidence a while back, touting it as definitive proof of his contention. Having learned from the Mazzei quote above not to trust Michener and Wright evidence until I had verified it in the original source, I spent months hunting down and reading the Callister and Fitzhugh papers and ledger books in the archives.

Regarding Callister, Michener and Wright quote from McCusker (1978, 193) "In January 1762, Henry Callister, an Eastern shore tobacco merchant and planter, wrote a correspondent: 'When I said currency, which does not imply Maryland [paper] money, of which there is hardly any current—I think I was yet more particular, for I spoke of money and exchange as current in Pennsylvania, which is our current money at present.'" However, when this quote is put in context it implies little about the circulation of Pennsylvania currency in Maryland. In 1762 Callister was in Townside [Crumpton] Maryland, 9 miles from Delaware and 15 miles by cart trail from the Delaware River port of Duck Creek. He was shipping wheat to, and importing goods from, Philadelphia. He engaged in frequent correspondence with Philadelphia merchants, such as Robert Greenway (*Callister Papers*, Tyler 1978). As such, Callister was likely to have had extensive dealings in Pennsylvania currency and would have found it useful if he could lay his hands on any.

By 1762, Callister was also desperate for payment of any kind from his customers. He went bankrupt at year's end. The quoted passage above was directed to Nathan Wright, Callister's storekeeper. Wright had asked what Callister would accept in payment for Callister's goods, i.e., was only Maryland money acceptable or would any money do; was corn, wheat, or tobacco acceptable as payment; were bills of exchange acceptable as payment; and so on. Callister's response was that he would take almost anything, i.e., any money, bills, or goods (*Callister Papers*, material just prior to the letter of January 18, 1762 quoted from above). In the rest of Callister's rather extensive correspondence with Wright, there is no indication that Pennsylvania currency was ever in frequent use in Maryland. On the rare occasion when Pennsylvania currency was offered as a means of payment by Maryland customers, it was regarded as unusual (*Callister Papers*).

The claim that the evidence in the account books of William Fitzhugh (*William Fitzhugh Ledgers*), a prominent merchant on Maryland's western shore, shows that Pennsylvania paper money circulated freely as currency in Maryland is just plain wrong. Fitzhugh kept his accounts in Maryland pound units-of-account. For "cash" transactions that were not in Maryland pounds, he meticulously recorded what currencies were used, e.g., dollars, pistoles, sterling, Pennsylvania paper pounds, Virginia paper pounds, and so forth. He also recorded the rate of exchange he used to translate these other media of exchange into Maryland pounds for accounting purposes. Between 1761 and 1764, out of well over 1,000 cash transactions, only 8 and 13 (well under 2 percent for either) were in Pennsylvania paper pounds and in Virginia paper pounds, respectively (no other colony's paper monies were recorded as being used). In addition, most of Fitzhugh's transactions in Pennsylvania and Virginia paper pounds can be traced directly to his travels to, or trade with merchant from, these two colonies. Based on this evidence, Pennsylvania and Virginia paper pounds did not freely circulate as media of exchange in Maryland.

What I learned from this exercise is that Michener and Wright make no distinction between merchants dealing with cross-colony trade and the general populace—conflating one with the other. Second, even with their merchants dealing in cross-colony trade, the evidence for paper money "circulating freely" in other colonies was not strong. And lastly, I learned that Michener and Wright do not go to the archives and do not labor over the extent of the manuscript sources they cite. It was at this point that in an e-mail reply to Michener I asked him to stop accosting me with ersatz anecdotal quotes pulled from websites and secondary book sources until he had hunted down the original sources and evaluated the context, veracity, relevance, and motivation of the writers—that what took Michener only a day or two to find and toss at me took me months to properly evaluate in the archives. He stopped e-mailing such evidence to me and took the route we have here now.

The Maryland Dollar Gambit

Michener and Wright (2006) try to explain away the finding in Grubb (2004, 340) that shows a rise in the use of specie, in particular Spanish silver dollars, in the decade before the Revolution—rising from 10 percent in 1765 to almost 50 percent in 1775—by arguing that the reference to "dollars" in Pennsylvania runaway ads is not to specie but to Maryland paper money. Michener and Wright, however, are caught in a conundrum here. They note that the rise in the use of "dollar" rewards begins when Maryland started issuing paper money that listed "dollar" values on it, and they assert that this money circulated into Pennsylvania. They give the impression that Pennsylvanians switched to "dollar" rewards because they had this flood of actual Maryland paper dollars in hand as a media of exchange to offer.

However, such a claim would overturn their assertion that all rewards are just unit-of-account money. In other words, if the switch to "dollars" by Pennsylvanians was caused by a flood of Maryland paper dollars into Pennsylvania so that they could then be offered as rewards, as Michener and Wright imply, then Grubb's (2004) argument that rewards reflect media-of-exchange and not unit-of-account money is upheld by Michener and Wright. As such, the whole edifice of Michener and Wright's objection—that Grubb confuses media-of-exchange with unit-of-account money—collapses. In addition, if they claim that dollars are actually Maryland paper dollars and not specie, then the media of exchange in Pennsylvania would be all paper and little specie, which would overturn another of their paramount claims—that specie dominated the money supply at all times in the colonies. To avoid this possibility they also have to claim at the same time that the reference to "dollars" in this period is not to the actual Maryland paper money but only to "dollar" unit-of-account money. They cannot have it both ways—a true dilemma for them.

For the moment let's grant Michener and Wright their assertion that a "dollar" reward was just a unit-of-account expression. Why would Pennsylvanians switch increasingly to that unit of account from 1765 through 1775? Michener and Wright (2006) mention the convenience of dollar-conversion to other currencies—the mathematics of easy division—but this was always true. So why did they not switch earlier and why at best only half way by 1775? The best answer Michener and Wright can give is that while Pennsylvanians placing ads for runaways did not actually have Maryland paper dollars, the supposed increasing presence of this media of exchange in their economy made it beneficial to switch to "dollar" units of account.

This suggests some testable (falsifiable) hypotheses. (Michener and Wright (2006) never suggest or craft any testable or falsifiable hypotheses, which appears to be their goal, i.e. to prevent all testing of their theory by denying any evidence that could be used to so test it.) One test would be to look at the usage of currency in Maryland. Following Michener and Wright logic, Marylanders after 1767 in all their accounting documents should switch over more completely than Pennsylvanians did to using "dollar" units of account. Second, Pennsylvanians living in counties bordering Maryland compared with Pennsylvanians living in say Bucks and Northampton Counties (about 77 and 114 miles from the Maryland border, respectively) should switch faster and more completely into using "dollar" units of account. Third, Marylanders who advertised rewards for runaways in both the *Maryland Gazette* (Annapolis) and the *Pennsylvania Gazette* should be more likely to use "dollar" rewards than Pennsylvanians just advertising in the *Pennsylvania Gazette*.

Michener and Wright's hypothesis is falsified by all three tests. Pennsylvanians who lived the farthest away from Maryland [Berks, Bucks, and Northampton Counties] nevertheless used "dollar" rewards in their ads as much if not more than Pennsylvanians who lived on the border with Maryland [Chester, Lancaster, Bedford, Cumberland, and York Counties] (Grubb 2004, 341). In addition, 99 Marylanders placed the same ad for their runaway in both the *Maryland Gazette* and the *Pennsylvania Gazette* between 1767 through 1775. Only 12 percent of the rewards they offered were in dollars (the rest were in pounds or in "the currency where taken" with one offering a pistole). This is a lower (not a higher) percentage in dollars than what Pennsylvanians offered when only advertising in the *Pennsylvania Gazette* (Grubb 2004, 340-341).

Finally, looking at a sample of Maryland merchant and government records from 1767 to 1775 reveals that "dollar" units of account were hardly ever used, just Maryland pounds. For example, one can see this in the account books and convict auction records of James Cheston, of Cheston, Stevenson, and Randolph, a convict merchant in Annapolis and Baltimore (*Cheston-Galloway Papers*), and in the Baltimore County Court Convict Records from 1770-1774 (*Baltimore County Court*).

While Michener and Wright's hypothesis (2006) is falsified by these tests, there is one hypothesis that this evidence does not falsify, and that is Grubb's hypothesis (2004) that "dollars" in Pennsylvania runaway ads refers to specie not Maryland paper dollars, and that specie availability differed across colonies due to differing short-run fluctuations in international trade flows. And why didn't Marylanders use dollars as a unit of account after 1767? In part, it may have been because those "dollar" Maryland bills of credit issued after 1767 were not redeemable in silver dollars in Maryland, but only in London. In addition, these bills reported on their face a value both in dollars and in pounds (Newman 1997, 167-169). In conclusion, Michener and Wright could have easily crafted testable (falsifiable) hypotheses and readily tested them against the evidence. However, they did not, nor have they in the past.

Straw Men, Misdirection, and Things I Never Said

Michener and Wright (2006) accuse me of many things I never said or did. For example, I never estimated or asserted "chronic scarcity of specie in Pennsylvania before 1723" going back into the 17th century. In my estimate of Pennsylvania's total money supply (Grubb 2004, 334-335) I did not "implicitly" assume that the velocity of circulation of specie was the same as for paper money. I explicitly stated it—nothing is hidden there. I did not claim that the velocity of circulation of Pennsylvania paper money in the late 1780s was higher than that of specie (Grubb 2005a, 1343). I only said that people at the time seemed to think that the velocity of circulation of Pennsylvania paper money was quite high. As a student of McCloskey, I am not devoted to "sign econometrics" as anyone who has read my research knows. Michener and Wright simply use this assertion—that someone is devoted to "sign econometrics"—to dismiss any econometric results that do not support their beliefs. Michener and Wright (2006) mischaracterize my statement on "ubiquitous use" of unit-of-account money (Grubb 2004, 331) and my estimate of real money balances over time—erroneously splicing one of my estimates with a different one they made up. I actually estimate that the long-run trend in per capita real money balance in Pennsylvania was approximately zero (Grubb 2004, 350; 2005b, Fig. 1).

As in Michener and Wright (2005), Michener and Wright (2006) spend a lot of time attacking Grubb (2006) which is a chapter in an edited volume that is still unpublished. This volume has languished in limbo for a few years due to editorial problems unrelated to my chapter. As such, it is the ideal target, the ideal straw man, for Michener and Wright to attack because the reader cannot consult the work to see if Michener and Wright are justified in their attack and I, as the author, cannot reproduce the material here without violating faith with the volume's editors and publisher. All I can do is hope the reader will find the work when it is finally published, read it, and then for themselves judge the soundness of the research and the validity of Michener and Wright's attack.

Michener and Wright (2006) complain that various servant records produce different results when used to make monetary inferences, claiming that this invalidates all servant records—as though any record that has a servant in it must be the exact same kind of record. But not all servant records are identical. They differ by who is recording them, by what purpose they serve, by what group gets included in the record, and so forth. For example, the *Record of Indentures* (1771-1773) was recorded by the mayor's office as a contract registration exercise whereas the *Book A of Redemptioners* (1785-1804) was recorded under the auspices of the German Society of Pennsylvania as an honesty-in-contracting monitoring device for German immigrant servants. Because of these differences I would not expect them to reflect monetary usage in the same way (see also Grubb 1989, 1994). As I have said before, records must be scrutinized closely and evaluated on a record by record basis before using them, something Michener and Wright do not grasp or do not care about.

CONCLUSION

If there is a lesson here it is to side with Hume and Popper rather than with Plato and Hegel, for economics is first and foremost an empirical science and not an ideology (Grubb 2001), and to seek out the original sources of anecdotal quotes, deconstruct their meaning, and determine the context, veracity, representativeness, relevance, bias, and motivations of the writers—especially when dealing with colonial money.

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GO TO COMMENT BY MICHENER AND WRIGHT

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INTELLECTUAL TYRANNY OF THE STATUS QUO FOLLOW-UP

In Defense of the Real Bills Doctrine

PER HORTLUND*

Comment on Richard Timberlake's article in the August 2005 issue.

For over seventy years, the question of what caused the Great Depression in the United States (1929–1933) has been one of the most debated economic issues. Since Friedman and Schwartz (1963), the cause has prominently been attributed to monetary mismanagement by the Fed, which let the money stock contract and thus failed to act as a lender of last resort. Recently, some authors have seen this contraction as a necessary consequence of the gold standard, which "fettered" the Fed's hands making it unable to respond to increased currency demands (Bernanke 1993, Eichengreen 1992 and 2002, Temin 1989 and 1994, Wheelock 1992). In the previous issue of *Econ Journal Watch*, Richard Timberlake takes issue with this view. In my judgment, Timberlake successfully argues against "golden fetters" and exonerates the gold standard. But there is a secondary aspect of Timberlakes's article. Timberlake blames the Great Contraction on the Fed's adherence to the so-called Real Bills Doctrine.

The Real Bills Doctrine (RBD) roughly holds that a central bank should limit its operations to the discounting of short term commercial paper. To Timberlake, it was the Fed's faithful adherence to this doctrine that led to the contraction of the money stock in 1929–1932. The RBD has for a long time been criticized by economists in the quantity-theory tradition, who think that the central bank should focus on stabilizing the quantity of money rather than on acquiring only a certain type of assets. They, therefore, favor active open-market policies such as the monetization

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of government debt to ensure a stable quantity of money. It has also been criticized by economists favoring free banking, who think that competition in money issue will put better constraints on money creation. The scholarly activity of these economists has given rise to a 'tyranny of the status quo' regarding the unsoundness of the real bills doctrine. Some have even been so bold as to brand it a fallacy (Mints 1945, 30; Friedman 1960, 43; Humphrey 1982, 12; Selgin 1982; Bordo and Schwartz 1995, 469; White 1995, 122; Laidler 1995, 258; Timberlake 1993, 259; 2005, 219). The most recent example of this is to be found in Timberlake's piece. In this comment I want to defend the RBD against what I believe to be unjust charges brought against it in the literature. Although the RBD is not a monetary panacea, it may in practice be a useful rule of thumb that can improve the workings of a central banking system on the gold standard. Its particular merit is that it prohibits the monetization of government debt. In defending the doctrine, I will not only comment on the piece by Timberlake, but also on others critical of the RBD, including Allan Meltzer (2003) and Thomas Humphrey (1982). The defense will show

1. That criticisms of the RBD have involved errors and ambiguities.
2. That the empirical case against the RBD is weak, if not non-existent.
3. That the substance of the Fed's error in the debated period was *not* that it followed the precepts of the RBD. On the contrary, Timberlake himself shows the error to have been the Fed's *straying* from the doctrine.

WHAT IS A REAL BILL?

Roughly, the RBD states that if the central bank issues money only against "real bills", then the money stock will automatically conform to the demand for payment media (Mints 1945, 9). Much confusion in the discussion is due to vagueness of concepts. What constitutes a "real" bill of exchange? According to Timberlake (2005, 205), the term "real bills doctrine" was coined by Mints, who derived the term from a passage of Adam Smith.

> When a bank discounts to a merchant a real bill of exchange drawn by a real creditor upon a real debtor, and which, as soon as it becomes due, is really paid by that debtor, it only advances to him a part of the value which he would otherwise be obliged to keep by him unemployed and in ready money for answering occasional demands. The payment of the bill, when it becomes due, replaces to the bank the value of what it had advanced, together with the interest. The coffers of the bank, so far as its dealings are confined to such customers, resemble a water pond, from which, though a stream is continually running out, yet another is continually running in, fully equal to that which runs out; so that, without any further care or attention, the pond keeps always equally, or very near equally full. Little or no expence can ever be necessary for replenishing the coffers of such a bank. (Smith 1776, 304)

This lengthy and somewhat obscure passage opens the way for two senses in which the term "real bills" may be understood. In the first sense, a real bill is a bill that has arisen from an authentic transaction of goods. The primary function of an "authentic" bill is to transfer ownership of goods. The credit function is secondary and attached to the transaction. When banks began to discount bills, so called "loan" or "accommodation" bills emerged, which had the legal form of a bill, but without there being any goods transactions involved. For example, a merchant could write a bill on himself, promising to pay $100 three months hence, and then discount the bill with a bank, receiving, say, $98 from the bank in exchange for the bill. Real bills are opposed to this type of loan bills—the "real" is synonymous with authentic and genuine, and opposed to "pseudo."

In the second sense, "real bills" are taken to be bills that represent "real value." Real is here opposed to nominal. The discussions by Timberlake (2005), and Humphrey (1982) is very much concerned with this sense of the term.

I would argue that it is the first sense that was of concern for practical banking policy in former times. In Sweden, the distinction was between "commodity bills" (*varuväxlar*) and loan or accommodation bills. Commodity bills were bills that carried the words: "value received in commodities." The Bank of Sweden would only discount commodity bills (Thunholm 1960, 33). As late as in the 1960s, commercial handbooks taught that commodity bills were self-liquidating, wherefore commodity bills carried the lowest discount rate with the banks (Thunholm 1960, 88).

Note that the real bills criterion in this sense is a purely formal, legal one—similar for example to the criterion of monetization of gold on fixed terms. Nothing says that a loan bill of a certain nominal amount could not represent "real value" or "real wealth." Timberlake's (2005, 206) discussion about the distinction between monetization on gold and on real bills I therefore believe to be somewhat off the mark.

The rationale of the RBD

The RBD gets its rationale from historic patterns of trade. In the 19th century trade volumes varied seasonally. In March and September–October the demand for bank notes regularly increased. In the case of Sweden, the note stock expanded about 15 percent in September each year. About 30 percent of this increase was due to the withdrawal of deposits, and 70 percent due to an expansion of (non-reserve) assets (Hortlund 2005a, 164). The variation in the demand for notes was thus not only (or even mostly) "form-seasonal," as Timberlake (1993, 254; 2005, 209) writes, but also "quantity-seasonal." Since banks regularly needed to expand their volume of assets to accommodate the note demand, the question naturally arises whether certain assets were more correlated than others with the needs for payment media. The RBD states that bills generated within the commercial exchange process should be more correlated with the needs of trade, and in this way it quite ingeniously links the volume of exchange media to the exchange process itself. In my own research, I find some limited empirical support for this. The Bank of Sweden subscribed to the RBD in the early twentieth century, and I find that real bills were more correlated with the seasonal patterns for exchange media than were other forms of credit (Hortlund 2005b).

The Real Bills Doctrine and the Productive Credit Doctrine

The RBD should be distinguished from a number of related but distinct doctrines. One of them is what one might call the Productive Credit Doctrine (PCD). The RBD, at least in its original nineteenth century European form, is concerned with whether the bill arose out of authentic commercial transactions. It does not much concern itself with the *purpose* to which the debtor will put the money. Such purpose is more the concern of the PCD, which says that the central bank should extend credit only for "productive" commercial purposes (i.e., for buying capital goods or intermediate goods, but not for buying shares of company stock).

Although related, the RBD and the PCD are conceptually distinct. Consider the following two events. First, a merchant that discounts a commodity bill, and uses the proceeds to buy stocks. Second, a merchant that discounts a "loan bill," and uses the proceeds to buy intermediate goods. The first event is in accordance with the RBD, but not with the PCD. The second is in accordance with the PCD but not with the RBD. This distinction is important and potentially substantive. Yet Meltzer (2003) consistently equates the two doctrines (for example, the index entry for the RBD on page 791 reads: "real bills (productive credit) doctrine"). In Meltzer's account, and also in Timberlake's, the Fed's main concern in 1929–1932 seems to have been to avoid the granting of credit for the purpose of speculation, particularly in the stock market. In this regard its operators seem to have been guided by the PCD rather than by the RBD. That the PCD and not only the RBD played a role in the operations of the Fed is also seen from the Federal Reserve Act which defined "eligible paper" as "notes and drafts, and bills of exchange arising out of actual commercial transactions . . . issued or drawn for agricultural, industrial, or commercial purposes" (Timberlake (2005, 207).

This definition of "eligible paper" encompasses not only the real bills criterion, but also the productive credit criterion. Because of this definition it is easy to conflate the two doctrines. However, in my view, they are conceptually distinct.

Now, although we have defined the RBD and the PCD as two separate practices, one may legitimately question whether the adherence of the one versus the other makes much of a practical difference. An example. Suppose John writes a bill on himself, "I promise to pay bearer 6 months hence $100." This is not a real bill and a bank's discounting it would be against the RBD. Nonetheless, if a bank discounts it, John gets cash in exchange for the note. He takes the cash and buys tools for it. This would then be considered "productive credit." Now, suppose instead that the banks do adhere to the RBD, so John and everyone else knows that they need a real bill to get credit from the bank. John then pays the tools by writing a bill, and the tool seller goes to the bank and discounts it. This would be a real bill, and the outcome would be the same as in the case where credit was granted according to the productive credit doctrine.

Thus, in practice the substantive difference between real bills and productive credit may sometimes be only subtle. One may question whether it is not better to conflate the two, in the name of parsimony. However, I believe that under special circumstances the practical difference may be substantial. An analogy. In a frictionless world with no genuine uncertainty, one may create "synthetic" options out of dynamic portfolio strategies, thus substituting for "real" options. In the same way, productive credit may be considered a "synthetic" real bill that under normal circumstances work well. However, the productive credit rule is more forward looking than is the real bills rule. In times of genuine uncertainty, it may be more difficult to assess the purpose to which credit is to be used, and credit may be undersupplied. In uncertain times, therefore, bank policy according to the RB and to the PC criteria may yield different results.

The Banking Principle

Also, the RBD should not be equated with the "Banking Principle." In writings on historical monetary doctrines, the RBD is thought to be but one of the "banking (school) principles" (Schwartz 1995, 149; White 1995, 121). The banking principle in the singular, however, is mostly associated with the view that banks, given only the constraint of gold redeemability, may safely be left to issue money according to their own criteria, and that external (quantitative) restrictions on banks—even central banks—are unnecessary to stop them from overissuing (White 1995, 121). The underlying view is that redeemable bank-issued money is always and everywhere endogenous and demand-determined. There is a "law of reflux" at work, whereby unwanted money is always returned to the issuer—banks therefore do not have the power to over-issue money and can therefore never be the cause of inflation. The banking principle holds that money will always be demand-determined—even if it comes about through the central bank's monetization of government debt. The RBD, in contrast, holds that money will be demand-determined only if issued against real bills.

Similarly, the RBD should not be equated with the "needs-of-trade doctrine." This doctrine holds it desirable for the central bank (or other banks) to have discretion to accommodate fluctuations in the demand for payment media (Schwartz 1995, 149). This is in contrast with what might be called the "quantity-doctrine," which holds that any quantity of money is always adequate to perform its task. The critique of the RBD by quantityminded economists is often in reality a critique against the needs-of-trade doctrine. An important example is given next.

Dynamic instability

Thomas Humphrey (1982) reviews a number of alleged fallacies of the RBD brought forth in the literature. Some of the criticisms make sense, for example that the volume of real bills presented for discount depends not only on the level of economic activity, but also on the discount rate charged by the central bank. Here I criticize the most important alleged fallacy, namely what Humphrey calls dynamic instability. Humphrey explains at great length that the RBD is dynamically unstable, because it leaves the price level indeterminate. It does not appreciate that not only does the price level depend on the quantity of bills, but the demand for bills depends on the price level. If the price level rises, more bills will be demanded. But if these are supplied, the price level will rise, since the price level is a function of the quantity of money. More bills will then be demanded, and so on, in a never-ending spiral.

This argument, it seems to me, draws on the classical mechanics-inspired "quantity equation of exchange." Money is supposed to move mechanically according to a postulated "velocity;" given this velocity, "M" mechanically increases "P." However, people trade for a purpose. When there is a greater demand for exchange media, more of it can be supplied without causing prices to rise. Historically, March and September–October were times at which contracts were made and prices set. When the time came to execute the trades, more money was needed. More money supplied to meet seasonal needs would then keep the short-term interest from rising, but not affect prices.

Furthermore, the charge of dynamic instability proves too much since it is not only a critique against the RBD, but against any doctrine that claims that the quantity of money should be allowed to vary with the nominally fluctuating demand for it. The dynamic instability argument is valid against all theories supporting the needs-of-trade doctrine, for example against free banking theories (to the extent that those theories do not posit a nominal anchor like the gold standard). If applied consistently, the dynamic-instability argument would oppose any kind of an elastic currency, possibly even deny the value of fractional reserve banking. The logic points to a policy of a rigid, government-controlled money stock. This road was taken by Mints (1950), who argued for 100 percent reserves on all bank-issued money. The dynamic-instability argument is therefore vulnerable, because history clearly shows that the nominal demand for money fluctuated, and that rigid, inelastic currencies can be destabilizing.[1] History also shows that gold-anchored banking systems exhibiting elasticity of money supply did not explode into hyperinflation.

THE EMPIRICAL CASE

The previous section showed that the theoretical case against the RBD is not as strong as some might think. But the empirical case against the doctrine is even weaker.

England in the early nineteenth century

The original case of inflation induced by RBD is England during the early half of the 19th century, particularly during the suspension era of the Napoleonic wars, 1797–1817. What has made this view credible is that in the bullionist controversy the Bank of England and the anti-bullionists defended their actions with real bills arguments (Laidler 1995, 258; Bordo and Schwartz 1995, 470). The relevant empirical question, however, is whether the inflation in this period was due to the Bank of England's excessive discounting of commercial bills, or whether it was due to its monetization of government debt. Wars are normally plagued with inflation when governments finance themselves through the printing presses. White (1995, 123) claims that the English inflations in the early nineteenth century did *not* occur because the Bank of England discounted real bills excessively. Rather, the cause was the Bank of England's open market purchases of government bonds.

> Under the Banks normal policy of holding its commercial bill discount rate steadily above the market rate in non-crisis times, that appearance [that the volume of bills offered for discount was beyond the control of the bank]

[1] Selgin and White (1994) show how elastic the Canadian currency was compared to the rigid currency of the National Banking system.

> was not so misleading. Any serious over-issues of the Bank in this period doubtless came from open-market purchases of government debt that the bank made under political pressure (these purchases may be viewed as the quid pro quo for its monopoly privilege). In theory, however, there was no obstacle to the Bank's over-issuing through aggressive discounting or open-market purchases of real bills. (White 1995, 123)

Germany 1922–1923

Allegedly, the most severe example of RBD-induced inflation is the German hyperinflation of the early 1920s. The German experience is cited by Bordo and Schwartz (1995, 471), Timberlake (2005, 206), and Humphrey (1982, 12), who consider this case as the prime example of the "dynamic instability" inherent in the RBD. The evidence put forth by Timberlake is that the Governor of the *Reichsbank* is said to have subscribed to an extreme version of the 'banking principle' (Timberlake 2005, 206). But, again, the banking principle is not the RBD. A central banker that clings to the banking principle holds that money will always be endogenous, and that the central bank could never be the cause of inflation, not even if it monetizes non-real bills such as government debt. The relevant empirical question is: did the German hyperinflation come about because the *Reichsbank* was overly and solely discounting commercial bills?

After WWI, the German government ran huge budget deficits, in large part as a result of the obligations of the Versailles Treaty. The deficits were financed by the discounting of German Treasury bills with the *Reichsbank*. As soon as this policy was discontinued, inflation stopped "all at once" (Welcker 1995, 229). Thus, instead of showing the dynamic instability of the RBD, the German experience must be considered a warning example of the inflationary potential inherent in monetizing government debt.

The United States 1929–1932

The real bone of contention is whether the Great Contraction was caused by the Fed's faithful adherence to the precepts of the RBD. Allegedly, the dynamic instability of the RBD now worked the other way. Insufficient quantities of real bills caused a decrease in the price level, which caused the volume of real bills to shrink even further, and so on in a downward spiral. To convict the RBD, it has to be shown: a) that the Fed discounted all real bills (eligible paper) presented to it for discount; and b) that these quantities were insufficient to sustain a stable quantity of money. In my opinion, Timberlake has not attempted to substantiate this. In fact, Timberlake presents evidence indicating that in practice the Fed did *not* follow the RBD. The true real-bills central bank discounts *if and only if* real bills (eligible paper) is presented to it for discount. That is, the real-bills central bank acts according to these two rules:

> Rule 1: Do not discount non-real bills (ineligible paper) presented for discount.
>
> Rule 2: *Do* discount all real bills (eligible paper) presented for discount.

A central bank that does not follow *both* of these rules cannot be said to follow the RBD. For the RBD adherent, the real bills criterion provides a rule that renders monetary policy more or less automatic. The volume of real bills corresponds by itself in a desired manner with the "needs of trade," wherefore no discretionary evaluation or prudence on the part of the central bank is needed. The central banker's only task is to check whether the instruments presented for discount is on the list of eligible paper or not. Did the Fed follow the rules?

1914–1920: Inflationary war-finance. The Fed was buying Treasury bonds. The Fed violated Rule 1.

1920–1929: Price stabilization the goal of policy. The Fed engaged in open market purchases of Treasury bonds. Again it violated Rule 1.

1929–1932: This is where the debate is. Was the Fed now taken over by RBD advocates such as Adolph Miller, who faithfully adhered to the RBD? To convict the RBD, it has to be shown that the Fed discounted all eligible paper presented to it, yet a deflationary spiral set in. Here Timberlake presents evidence indicating that the Fed actually acted contrariwise to the RBD. Quoting Clark Warburton, Timberlake provides a smoking gun that shows that the Fed violated Rule 2.

> [In the early 1930s the Fed Banks] . . . virtually stopped discounting or otherwise acquiring "eligible" paper. . . . Nor was this virtual stoppage…due to any forces outside the Federal Reserve System. It was due to direct pressure

> [from the Federal Reserve Board] so strong as to amount to a virtual prohibition of rediscounting for banks which were making loans for security speculation, and a hard-boiled attitude towards banks in special need of rediscounts because of deposit withdrawals. . . . Federal Reserve authorities had discouraged discounting almost to the point of prohibition. (Warburton 1966, 339–40, quoted in Timberlake 2005, 216)

Assuming the accuracy of Warburton's account, we see clearly that the Fed did *not* operate according to the RBD. Nothing in the RBD says that the central bank should refuse to discount real bills in time of deposit withdrawals. On the contrary, this is the very time to do so, and for which the Federal Reserve System was set up. Anxious to make amends for past breaches of Rule 1, the Fed apparently erred in the other direction and grossly violated Rule 2. In so doing, its operators seem to have been inspired by the PCD rather than by the RBD.

ACQUITTED: THE REAL BILLS DOCTRINE

In sum, Timberlake successfully defends the gold standard against the view that it caused the Great Contraction. Timberlake is also successful when he puts the blame on errors in the monetary policies of the Fed. However, he is unsuccessful when he attributes the substance of those errors to the Fed's faithfully following the real bills doctrine. Timberlake's own account suggests the very opposite. From its inception, the Fed seems to have strayed from the real bills rules—at times by monetizing government debt, and at times by refusing to discount real bills. One may come away with the impression that if only the Fed's operators had followed the wise counsels provided by the system's founding fathers, everything would have been fine.

In my view, the current presentation therefore calls for a revision of scholarly understanding of the real bills doctrine. Admittedly, the RBD is water-proof neither in theory nor in practice, and is therefore probably not a sufficient guide for monetary policy. The RBD perhaps should come into use in systems with limitations on the quantity of base money—natural limitations in gold regimes and quantity rules in fiat money regimes. In such systems, however, one must seriously ask whether the RBD may not actually improve policy performance. Although not perfect, the RBD might be considered efficient, when compared with the performance of its rival doctrines. I propose the following questions to ponder:

1. The classical gold standard period of 1870 to 1914 is acknowledged as one of the more stable periods of monetary history. Was the RBD not an integral policy of most central banks in this period? If so, can it be said that the RBD contributed to the stability of the classical gold standard era?

2. Is there in history an unambiguous case in which the RBD caused a major inflation? Has there ever been a major inflation that did not involve the monetization of government debt?

3. On the role of the RBD in the Great Depression: Can it be shown quantitatively that the volume of eligible paper generated in the economy was insufficient to accommodate increased demands for currency in the early 1930s?

4. For free bankers: Given that a free banking system is not politically feasible, what is wrong with a rule that prohibits monetization of government debt? Could a central banking system with such a rule be considered second best (given a nominal anchor such as gold)?

5. The monetarists' critique of the RBD significantly contributed to the adoption of government debt as the main instrument of monetary policy in the 1930s. This opened the way for unprecedented monetary expansion and inflation in period 1940 to 1990. Somewhat ironically, therefore, the monetarists, the champions of the fight against inflation, could rightly be considered some of the architects of the postwar inflations. In view of the historical record, would monetarists be willing to revise their critique of the RBD? In practice, would RBD-style "quality-rules" that emphasize restriction to certain types of assets be more efficient than quantity rules, to limit inflation?

These questions, I think, show that the real bills doctrine is alive and kicking, and capable of generating important questions for future research.

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GO TO TIMBERLAKE (2005) ARTICLE ON THE REAL BILLS DOCTRINE

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INTELLECTUAL TYRANNY OF THE STATUS QUO FOLLOW-UP

Editorial note: The following exchange was solicited after David Altig, Brad Setser, and Kurt Schuler had generated a back-and-forth at Altig's Macroblog. Each part of the following was formulated afresh, to produce an exchange that stands in lieu of that which took place at Macroblog.

Damned If You Do: Comment on Schuler's Argentina Analysis

DAVID ALTIG*

IN THE PREVIOUS ISSUE OF *ECON JOURNAL WATCH*, KURT Schuler (2005) surveys the truly painful travails of the Argentine people around the turn of the millennium, and the expert commentary from U.S. economists. Schuler sees messy footprints.

> Economists whose work in other areas I admire failed to do the research necessary for understanding Argentina's situation accurately. As a result, their analysis was faulty. When Argentina followed the main recommendations of the consensus, the economy's rate of decline accelerated. (235)

> A review of what U.S. economists said about Argentina shows that many failed to define key terms in their

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> arguments; most ignored readily available data that contradicted the consensus view about Argentina's economy; and nearly all neglected to examine the legal and statistical material, available for free online, necessary for understanding how Argentina's monetary system worked. The episode is important because it raises the question of whether the public can trust economists who claim expertise on controversial issues of economic policy. (236-37)

If legitimate, this is a stinging indictment indeed. I am identified as a perpetrator, for a 1999 article I wrote with my colleague Owen Humpage. Schuler lists several transgressions, but this one in particular put Owen and I in the crosshairs.

> Among the 100 most active commentators on Argentina, 91 of 94 who mentioned the topic called the convertibility system a currency board. Yet examination reveals important differences between the convertibility system and an orthodox currency board. The system was a central bank that mimicked some currency board features; it is perhaps best termed a currency board-like system, or even a pseudo currency board. (243)

We are one of the indicted 91. I plead not guilty.

My defense is this: I don't think Owen and I said anything substantially different than what Schuler claims the truth to be. Here is what we actually wrote.

> **Although the new monetary institution created by the Convertibility Law is not a pure currency board,** such an unadulterated arrangement is a useful benchmark from which to begin thinking about Argentina's monetary structure. (2, bold added)

We followed up our description of this "useful benchmark" with a section describing the actual institutional monetary arrangement in Argentina, which we titled "Argentina's Almost Pure Currency Board." Although Kurt complains about the failure of economists (like me) "to do the research necessary for understanding Argentina's situation accurately"

(235), this section of Owen's and my paper (which contains about 15 percent of the article's total word count) was based on the work of a pretty reliable source: Kurt Schuler.[1]

In fact, the central point of our article was that the Convertibility Law contained special non-currency-board-like provisions. Owen and I enumerated the specific ways in which Argentina's monetary institutions departed from a pure currency board arrangement—namely, allowing central bank reserves to be held in non-dollar reserves, standing lines of credit via repurchase agreements with large multinational banks, and provisions to lend dollar reserves to domestic banks—and we offered those departures as a sensible response to critics who claimed that a currency board arrangement was too rigid for Argentina's own good. We did not merely acknowledge the differences between the creation of the Convertibility Law and a pure currency board. Those differences were the heart of our analysis.

This would all be of small moment if my comments here amounted to nothing more than an attempt to salvage my co-author's and my honor. But I think our particular case is symptomatic of a deeper problem with Schuler's analysis. The table that puts Owen and me in the category of terminology-abusers, includes this qualification: "[The authors] occasionally mentioned that the convertibility system was not an orthodox currency board, but on balance seemed to consider the system a currency board." By my count, Schuler grants the same qualification to 39 of the 91 authors who ran afoul of the Schuler get-the-definition-right mandate.[2] I have not gone back and checked each of these 39 cases, but here is a sampling from one of the listed offenders.

> Some critics argue that the convertibility plan was not a meaningful currency board because it did not force strict equality of the money base with external reserves…[I]n the second quarter of 2001 in the face of recession, Minister of Economy Domingo Cavallo boosted the money base in a catch-up to the level of reserves, which had by then eased back to about $20 billion. However, he did not shrink the

[1] Footnote 4 of our article reads as follows: "Steve H. Hanke and Kurt Schuler's 'A Dollarization Blueprint for Argentina,' Cato Institute *Foreign Policy Briefing,* no. 52 (March 11, 1999) provides an excellent review of the Convertibility Law…" We further reference the Hanke-Schuler work in the "Almost Pure" section. From footnote 6: "For a good description, see Hanke and Sculer, op cit."

[2] See Schuler's Table 1, which appears on pages 238-240.

> money base again despite the subsequent plunge of reserves to $10 billion, breaking the tie between the two on the downside. **While the period 1995-2001, and more convincingly 2001, may disqualify the plan from the designation as a true currency board, for at least the period 1991-2000 the system achieved the economic objective of a currency board by providing strong confidence that the national currency would be backed by reserves.** Moreover, it would likely have been a mistake to pursue more rapid money expansion in 1996-2000, aggravating the economic cycle. (Cline 2003, 21 footnote 8, bold added)

Here are passage from another set of authors given bad marks by Schuler:

> In March 1991, Congress passed **the "Convertibility Law", which pegged the peso to the dollar one-to-one, and transformed the monetary and exchange rate functions of the Central Bank into (almost) a currency board**… The Central Bank had to maintain liquid international reserves to cover (almost) 100% of the monetary base (but not broader monetary aggregates), and thus could not increase the monetary base except when international reserves expanded (through trade surplus or net capital inflows) (Díaz-Bonilla et al 2004, 4, bold added)
>
> The supply of liquidity beyond the monetary base was still affected by Central Bank monetary instruments such as the reserve requirements for the banking system and the use of short term swaps. This allowed some room for maneuver in monetary policy. Also **a percentage of the backing of the monetary base could be covered by dollar denominated debt of the Argentine government, which permitted some monetization of fiscal deficits. Hence, the use of "almost" in the previous paragraphs**. (Díaz-Bonilla et al 2004, 4-5, footnote 4, bold added)

And yet another example:

> [Argentina's] economic policies, which were largely the brainchild of President Menem's formidable economy minister, Domingo Cavallo, featured a hard peg of the Argentine peso at parity to the US dollar, backed by the Convertibility Plan, which strictly limited domestic money creation under an arrangement **similar to a currency board**. (Mussa 2002, 1, emphasis added)

You get the idea. In none of these cases is there a failure "to do the research necessary for understanding Argentina's situation accurately." In each instance the special characteristics of the Convertibility Plan are made clear, and the distinctions relative to the strictest interpretation of a pure currency board highlighted. It is true that the generic phrase "currency board" tends to be liberally applied as a substitute for the more unwieldy "currency-board-like arrangement" or something of the sort. But this is a stylistic shortcut that is not the least bit misleading in the context of the articles in question.

In addition, the authors on Schuler's list are apparently in good company when using the "currency board" shorthand in their discussions:

> Although it is true that some analysts referred to the Argentine arrangement as "mimicking" a currency board, it is also true that before the crisis many super-fixers praised the Argentine regime. [Footnote: Kurt Schuler, the author of some of the most serious work on currency boards, included Argentina in his "team of currency boards," in a public wager that he posted on the Internet.] (Edwards 2002, 238)

I'm not sure how accurate that is, but either way it is sort of ironic.

The Schuler critique is serious because he claims that this is not a semantic dispute, but a substantive one: In giving uniformed and misleading descriptions of the Convertibility Plan and the Argentine monetary regime, various authors set back the cause of workable reform. On this particular point, I think the case is not made. Schuler refers to several other "mistakes"—opinions on whether the currency was overvalued, whether exports from Argentina were uncompetitive, whether dollarization was technically feasible. He treats the answers to these questions, the last two especially, as definitive, but it seems to me that there is more room for honest disagreement than the author's views allow.

The advice given by economists may have been good or bad, and the record on that score is well worth exploring. Furthermore, it is hard to argue with Schuler's plea that those who proffer such advice make the effort to truly understand the institutional arrangements (and socio-political realities) with which the targets of their attentions must deal. But my belief is that a close and objective reading of the record will reveal a better score in this dimension than he claims.

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GO TO SCHULER (2005) ARTICLE ON ARGENTINA

GO TO SCHULER (2006) REPLY

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INTELLECTUAL TYRANNY OF THE STATUS QUO
FOLLOW-UP

Argentina's problems went far beyond the absence of a strict currency board: Comment on Schuler

BRAD SETSER*

KURT SCHULER ARGUES MOST ECONOMISTS (MYSELF INCLUDED) failed to get the facts right. Schuler writes, "economists whose work in other areas I admire failed to do the research necessary for understanding Argentina's situation accurately. As a result their analysis was faulty" (Schuler 2005, 235). This mischaracterization of Argentina's economic situation led them to prescribe inappropriate policies, not the least recommending that Argentina end its tight link to the dollar.

According to Schuler:

- Talk of Argentina's currency board was misleading since Argentina did not have a "true" currency board. Schuler's preferred terms: a currency board-like system, or pseudo-currency board.
- Argentina did not have a trade deficit. Consequently, those who postulated such a deficit as a sign that the peso was overvalued were wrong.
- Argentina's exports were growing, so it was inaccurate to talk of the burden an uncompetitive exchange rate placed on Argentina's exports.

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- Argentina had the ability to dollarize in 2001 if it had so wanted.

Schuler thinks the right policy in late 2001 was "default and dollarize" and by failing to dollarize, Argentina increased the cost of its crisis.

I will address each of Schuler's points in turn, and then lay out why I think his preferred solution, dollarization, would not have worked. Replacing pesos with dollars would not have changed the fact that Argentina's government could not pay its debts or the fact that Argentina's banks, by the end of 2001, lacked both liquidity and performing dollar-denominated assets. Dollarization would have made it even harder to achieve the adjustments in Argentina's real exchange rates needed to reflect Argentina's reduced ability to attract international capital, ensuring a continued recession. I also believe that the cost of Argentina's crisis was greater than it needed to be, but largely because Argentina refused to devalue the peso and seek a restructuring of its debt more rapidly. There were many potential responses to Argentina's crisis—Argentina (and the IMF) no more followed the policy course Nouriel Roubini and I have advocated than the policy course Kurt Schuler advocated (see Roubini and Setser 2004).

I doubt that many who, for the sake of simplicity, spoke of Argentina's currency board (generally known as "convertibility" in Argentina) rather than the awkward "currency board-like arrangement" had major illusions about the nature of Argentina's regime. Remember, Hong Kong's currency board-like arrangement also falls short of Schuler's strict standard—Hong Kong has more reserves than it needs, and it used those reserves to intervene to prop up the stock market in a big way back in 1998. It was widely known in policy circles that Argentina's currency board-like arrangement allowed the central bank to hold dollar-denominated government of Argentina bonds as backing for a certain amount of the monetary base. Plus, as Schuler emphasizes, Argentina also held more reserves than required to back the monetary base—in part because a fraction of the banking system's mandated dollar reserves were held in the central bank. That too was widely known; Argentina's high levels of reserves and access to emergency liquidity through its contingent "repo" line with international banks were generally considered to be a key point in Argentina's favor.

Any confusion about the nature of Argentina's exchange rate regime, though, certainly disappeared over the course of 2001. The original architect of Argentina's currency board arrangement, Domingo Cavallo, argued that he knew how to make the currency board more consistent with growth, which in practice meant making it less of a currency board. Yet Argentina also insisted that its exchange rate arrangement was more than a "mere" peg. It consequently insisted on holding on to the one peso to one dollar parity even as the costs of that peg become more and more apparent.

Schuler's charge that economists critical of Argentina's peg did not pay sufficient attention to the ways "convertibility" differed from a pure currency board puts far too much emphasis on the ways in which Argentina's peg differed from a currency board, and ignores the ways in which Argentina's currency board-like arrangement required the same basic adjustments that a pure currency board would have required. So long as Argentina remained pegged to a (then rising) dollar, the only way Argentina's real exchange rate could adjust was through falls in domestic prices.[1]

Schuler's argument also ignores the fact that in 2001, Argentine policy makers, backed by leading Argentine economists, consistently sought to find ways to avoid implementing the monetary tightening implied by even a pseudo-currency board, let alone the more draconian tightening implied by a true currency board. After two years of recession and slow deflation, Argentina did not want more recession or more deflation—so it is not a surprise that Argentina's policy makers spent the first half of 2001 trying to find ways to defer further adjustment.[2] One example: Argentina's end-of-2000-IMF program was designed to provide the financing Argentina needed to implement a pause in Argentina's fiscal consolidation in order to provide more room for growth (Independent Evaluation Office 2004). Inflows from the IMF also meant that Argentina would not have to dip into its reserves to finance private capital outflows, and thus helped Argentina maintain a less strict monetary policy than otherwise would have been the case. Another example: After missing its first quarter fiscal targets, Argentina opted for Domingo Cavallo, who promised that he knew how to make the currency board arrangement more consistent with growth, rather

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[1] As external inflows first fell and then turned into massive outflows, Argentina's pseudo-currency board still required monetary tightening. Base money fell substantially in 2000 and 2001. A pure currency board would have implied an even more draconian monetary tightening, particularly in 2001, as base money would have had to fall inline with Argentina's reserves. The likely results would have been even faster falls in domestic prices, more rapid real exchange rate adjustment through deflation, and even larger falls in output that Argentina.

[2] Mussa (2002, 5) noted: "They [the policies that ultimately led to the crisis] were the policies desired and implemented by the Argentine government. In general, the Fund supported these policies . . . but the Fund did not press the Argentine government to adopt policies that it did not willingly choose to implement."

than Ricardo Lopez-Murphy, who promised more fiscal austerity. Cavallo's program for growth effectively amounted to a program for loosening convertibility's constraints while formally preserving the peg. Bank regulation would be changed to generate a de facto monetary loosening, and a combination of import tariffs and export subsidies would generate a "fiscal" devaluation.[3] He also announced his intention to shift from a pure dollar peg to a joint euro/dollar peg. A final example: faced with a bank run in the summer of 2001, Argentina sought, and obtained, an immediate $5 billion cash infusion from the IMF to allow Argentina's central bank to act as a lender of last resort. John Taylor, the Treasury Under Secretary, strongly backed this operation (Blustein 2005)—even though a domestic "lender of last-resort" is incompatible with Schuler's pure currency board.

Even after all these options had been tried and failed, Argentina still refused to accept the draconian monetary tightening that would have been implied by a pure currency board. As revenues shrunk in line with Argentina's shrinking economy, Argentina's provinces started to pay people with script—funny money—rather than cut salaries to match falling revenue (among others, IMF 2003a). That basically amounted to printing money—again, the opposite of what a strict currency board required.

Schuler's points on trade are also misleading. He looks at average export growth rates from 1991 through 2001. There is no doubt that Argentina's trade boomed in the first years of currency board, driven by economic recovery created by the end of hyperinflation, successful debt restructurings in both Argentina and Brazil, and a relatively weak dollar. In 1995, though, the dollar started to appreciate and in early 1999, the game changed completely. Brazil was forced to abandon its peg to the dollar and the dollar started to appreciate substantially against the euro. The result was a substantial appreciation of the peso's real value, a real appreciation that shows up whether one looks at the CPI-based real exchange rate or the PPI real exchange rate (see IMF 2003b).

In the face of this appreciation, Argentina's exports—measured by export volumes—really were stagnant. Schuler's own volume index shows an average increase in volumes of only 2 percent between 1999 and 2001;

[3] Specifically, Cavallo reduced the reserve requirements imposed on Argentina's banks to free up more funds for new lending. By allowing (or forcing) the banks to hold a high-yielding dollar denominated bond issued by the government as part of their reserves, Cavallo obtained additional financing for the government, and a capital inflow that boosted Argentina's reserves. (The banks ran down their international dollar deposits in New York to buy this bond). The combination of tariffs and export subsidies generated a "fiscal" devaluation of 8 percent. See Hausmann and Velasco 2002 and Mussa 2002.

IMF data shows an average volume increase of over 7 percent for all emerging and developing economies over the same period. In nominal terms, Argentina's exports did even worse—a rally in commodity prices in 2000 did prompt large increase in the dollar value of Argentina's exports that year, but that rally came after a large fall in prices in 1999. Argentina's 2001 exports—$26.5 billion—were no larger than they were in 1998—$26.4 billion. In the mean time, Argentina's external debt has increased from $147.6 billion to $166.3 billion and interest payments on that debt increased by $5.3 billion to $8.2 billion. Nothing suggested the trend would change: interest payments were set to keep on rising, and Argentina's export growth would be constrained by its link to the dollar.

It is impossible to argue away the weakness in Argentina's export performance after 1998. Global trade was booming in 1999 and 2000, propelled by strong demand growth in the dot-com US economy. Most emerging economies benefited from surging exports—and surging export volumes. Not Argentina. As Ted Truman has emphasized (see the commentary on Hausmann and Velasco 2002), Argentina's exports grew more slowly than any other emerging market in the second half of the 1990s—a period that coincides with the dollar's broad appreciation from its 1995 lows.

Schuler is right that Argentina's trade deficit peaked in 1998, and Argentina's deficit shrank in 1999 and turned to a small surplus in 2000. An economic contraction led imports to fall substantially. Schuler is wrong though to argue that the absence of a trade deficit is sufficient to prove that Argentina's exchange rate was not really that overvalued—at the end of his paper, he even suggests "calculations based . . . on wholesale or producer prices . . . would show that in 2000 and 2001 the real exchange rate was perhaps undervalued" (Schuler 2005, 261). For a country with as large an external debt as Argentina, the absence of a trade deficit is not sufficient to provide external sustainability. Large (and growing) interest payments implied relatively large ongoing current account deficits even if Argentina's trade was in rough balance. In 2000, interest payments were 30 percent of Argentina's exports revenues—far more than any other emerging economy.[4] Since the real interest rate on Argentina's external debt far exceeded the real growth rate of Argentina's economy, Argentina needed to run a significant trade-and-transfers surplus in order to stabilize its external debt to GDP ratio. Perry and Serven's (2003) calculation, which shows a

[4] Hausmann and Velasco (2002, 33). External debt service was a bit over 20 percent of Brazil's export revenues, and well under 20 percent for all other emerging economies.

large real overvaluation of the peso, takes into account the fact that a country with rising external debt needs a real depreciation over time to free up export revenues to service its external debt.

Argentina consequently faced two obstacles to external sustainability even after a deep recession had eliminated its trade deficit. First, Argentina either needed ongoing inflows of capital from abroad to finance its current account deficit (along with the ability to refinance maturing debt) or its economy needed to shrink to reduce imports to the point where Argentina could finance interest payments on its external debt out of a substantial trade surplus. The fact that Argentina's exports were such a small share of Argentina's economy made such an adjustment particularly difficult. Generating a 3 percent of GDP trade surplus off a 9 percent of GDP export base implied imports of only 6 percent of Argentina's GDP. Second, even with ongoing market access, keeping Argentina's debt to GDP ratio from exploding required both a trade surplus (though not as large a trade surplus as would be the case if all interest payments had to be financed out of export revenues) and the resumption of growth. Yet the resumption of growth, at Argentina's 2000 real exchange rate, would have pushed Argentina's trade back into a trade deficit.

Argentina was caught in a trap. Improving its trade balance in the short-run (barring a huge increase commodity prices) required squeezing imports. Argentina's peg, even in a pseudo-currency board, implied that (real) depreciation of the peso could only come through domestic deflation. Deflation implied an economic contraction. The political tensions associated with the need to cut government spending to match falling revenues made creditors reluctant to extend Argentina the ongoing financing it needed on any but the most onerous terms. Those onerous terms hindered growth, and led Argentina's interest payments to soar (interest payments on government debt doubled between 1997 and 2001). Hausmann and Velasco (2002, 4) put it well: "In this sense, Argentina's financial crisis [was] a growth crisis: if income keeps dropping, at some point debts become impossible to pay."

Schuler's solution: dollarize and default on the debt. Schuler is right to note that Argentina had enough gross reserves to replace all pesos in circulation right until the end—though some of those reserves were borrowed from the IMF.[5] Indeed, the fact that Argentina still had

[5] Net reserves fell from $21.9 billion at the end of 2000 to $0.4 billion. Argentina's banks also ran down their external assets, helping to finance the massive outflow of capital from Argentina. Remember, Argentina still ran a current account deficit in 2001, so all these

substantial gross reserves was one reason why Lavanga's economic team was able to stabilize the peso after Argentina's default even without additional support from the IMF.

The fact that dollarization was technically possible, however, though does not mean dollarization late in 2001 (or for that matter in 1999) was a good idea, or that it would have solved Argentina's problems. Schuler and other proponents assert dollarization would have generated a surge in confidence, particularly a surge in confidence in the banking system, which would have saved Argentina. Capital flight would have stopped—even in the face of a default on the government's debt.

That possibility cannot be totally ruled out. Robert Rubin (Rubin and Weisberg 2003) likes to emphasize that there are no certainties in life, or in finance. But it hardly seems the most likely possibility. Argentina's core problems—an overvalued currency that stifled export growth after 1998, a banking system that lacked dollar liquidity, and insufficient access to external financing to cover interest and principal payments on Argentina's external debt—would have remained. Further economic contraction was needed to bring about the deflation needed to generate the real exchange rate adjustment. Defaulting on the government's debt alone would not have eliminated all external payments—private companies also had significant external debts.

Moreover, default was hardly likely to restore confidence in Argentina's banks, or to stop capital flight. That in many ways is the numb of the problem. Dollarization would have taken all (or almost all) of the central bank's gross reserves. The banks themselves lacked dollar liquidity at the end of 2001, so any further run on the banks would have forced a bank holiday. Proponents of dollarization claim that depositors with dollar deposits would have no reason to run. But depositors worry about more than currency risk. Dollars deposited in Argentine banks ultimately were claims on the banks assets, and those assets did not look particularly good at the end of 2001. Dollar denominated loans to Argentina's government made up a substantial share of the bank's assets. And in the face of a continued economic contraction, it is safe to assume that a rising share of

outflows had to be financed out of existing external assets. The argument that peso deposits fell faster than dollar deposits in 2001 is a red herring. Had Argentine depositors been concerned solely by a depreciation, they could have protected their wealth by shifting from domestic peso deposits to domestic dollar deposits. So long as Argentina maintained a peg, what mattered was the overall fall in domestic deposits – since money leaving the banking system moved abroad and was a drain on either Argentina's reserves or the banks' foreign assets.

the banks' dollar denominated loans to private companies also would have failed to perform (Gelpern 2004; IMF 2004; Lagos 2002).

Actually, there would have been one potential way to avoid a comprehensive bank holiday: The head offices of the major international banks could have provided big credit lines to their local subsidiaries. The locally owned banks would have closed, but such a credit line could have allowed the subsidiaries of international banks to remain open. However, convincing the head offices to put up that kind of credit line would have been difficult—after all, they would have been increasing their exposure to a country that in Schuler's scenario would have just defaulted on most of its external debt. Moreover, they would be lending to a country with a still overvalued exchange that was in the midst of a deep contraction. Political risk was rising—a grand gesture like dollarization would not have led the world's big banks to ignore Argentina's remaining problems.

Schuler puts too much emphasis on the ways in which Argentina's monetary arrangement differed from a pure currency board, and too little emphasis on the fact that the basic mechanism for real exchange rate adjustment in Argentina's currency board-like arrangement was no different than in a pure currency board. Barring a miraculous increase in confidence, a pure currency board implied a more significant tightening of monetary conditions, faster deflation and a stronger economic contraction. Schuler's analysis of Argentina's trade fails to look carefully at the major real appreciation that occurred in 1999, and ignores the fact that Argentina's rising external debt and soaring interest payments implied that a significant trade surplus was needed to stabilize Argentina's external debt to exports ratio. Last-second dollarization—particularly in a context where the banks lacked both liquidity and performing assets (IMF, 2004)—was unlikely to generate a magic restoration of confidence. That is all the more the case if dollarization was combined with default. Nor would dollarization have eliminated the need for deflationary real adjustment. Ecuador, it should be remembered, dollarized after defaulting on its government debt, after freezing much of its banking system and after a substantial devaluation.

Argentina's mistake was not its refusal to dollarize, but rather its unwillingness to devalue and initiate a restructuring before it had depleted both its own reserves and its the capacity to borrow additional reserves from the IMF. Had it moved earlier it would have been in a better position to limit the impact of the devaluation and default on the domestic banking system. Debt restructurings are inherently disruptive. But an earlier restructuring combined with IMF lending to help "soften the blow" might have reduced the disruption. Argentina would have had a greater capacity both to intervene in the currency market to try to limit the overshooting of the peso, and to backstop the banking system during the restructuring process (see Roubini and Setser 2004 and Blustein 2005 for details). An agreed program of fiscal adjustment might have helped Argentina reach agreement with its creditors more rapidly. Such a policy course carried with it significant risks—there truly were no good options available to Argentina in 2001. But it also just might have produced a smaller cumulative loss in output.

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GO TO SCHULER (2005) ARTICLE ON ARGENTINA

GO TO SCHULER (2006) REPLY

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INTELLECTUAL TYRANNY OF THE STATUS QUO FOLLOW-UP

Reply to David Altig and Brad Setser

KURT SCHULER[*]

DAVID ALTIG AND BRAD SETSER ASK HOW IMPORTANT IT IS WHAT we call Argentina's monetary system of April 1991-January 2002. I consider it crucial because clear, consistent terminology helps us understand how the system worked and to what extent it resembled other systems to which we may wish to compare it. Unclear, inconsistent terminology hinders our understanding. It can result in bad advice that hurts many people. Economists contributed to Argentina's severe economic troubles of 2001-2002 by misunderstanding the monetary system and foreclosing consideration of policies that a more accurate diagnosis would have left open.

I may have unwittingly contributed to the lack of precision about Argentina's monetary system. For at least a decade I have usually called it "currency board-like," specifying that I meant the system had some elements of a currency board, but was missing other important elements. In retrospect, I regret not using a term that would have more strongly urged economists to pay attention to the missing elements. Most economists reasoned as though the system was quite close to a currency board. Calling Argentina a "pseudo currency board" instead might have been a better strategy.

Like many other economists, Altig and Owen Humpage (1999, 2) noted legal provisions allowing the central bank discretionary powers that a currency board would not have had, but did not seem to consider them

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important in practice, because they called the result "Argentina's Almost Pure Currency Board." To show how Argentina's system worked in practice, my article offered five statistical measures based on balance sheet figures from Argentina's central bank, as standardized by the International Monetary Fund (Schuler 2005, 244, Table 3). For a currency board, the measures should be near or equal to 100 percent. Appendix 3 of the article compared Argentina's measures with those for Brazil, universally acknowledged to have had a central banking system, and Bosnia, which among the countries the IMF covers is perhaps closest to a pure currency board. For Bosnia, all five measures were in the range 90-100 percent. For Argentina, the measures were 34, 76, 47, 31, and 241 percent. Calling Argentina a currency board, or almost a currency board, implies that it was like Bosnia, when in fact four of the five statistical measures indicate it was closer to Brazil.

In private correspondence, Charles Calmoris remarked that he was quite aware of the differences between Argentina's system and a currency board even if he did not dwell on them in print. Although I do not doubt it in his case, I classified economists' views according to their statements available in print because I lacked knowledge of their private thoughts. I disagree with Brad Setser that awareness was widespread of the differences between Argentina's system and a currency board. For example, in July 2002 the National Bureau of Economic Research held a conference on Argentina that included prominent U.S. and Argentine economists. Among the 15 speakers recorded as mentioning currency boards, only Edwin Truman offered any caveat to the view that Argentina's system had been one. Argentina's system continues to be cited as literally a textbook example of a currency board. Doing so groups it with several current cases and dozens of historical cases with which it has little in common, and hinders understanding of the strengths and weaknesses of currency boards.

David Altig refers to a footnote by Sebastian Edwards indicating that I myself had identified Argentina's system as a currency board. Edwards's footnote was incomplete. I mentioned the differences between currency boards and systems such as Argentina's, but indicated willingness to compare systems such as Argentina's to more typical central banks.

Regarding Brad Setser's remarks on dollarization, I limited my article to matters of diagnosis rather than prescription. I intend in future writings to revisit prescriptions I offered from 1999 to 2003, to explain how dollarization and related policies could have worked in Argentina, and to consider points such as those Setser usefully makes. This is not the place for a full reply. Briefly, though, Setser thinks monetary policy would have been "tight," discouraging economic growth, under any arrangement that preserved the exchange rate of one Argentine peso per U.S. dollar. I think dollarization at one peso per dollar would have "loosened" monetary policy in a vital sense by eliminating the currency risk premium in interest rates charged by banks. That was what happened in Ecuador when it dollarized in January 2000 and in El Salvador when it dollarized a year later.

I wish to make some small emendations to my article. In discussing remarks by Sebastian Edwards I said that Mercosur, the common market to which Argentina belongs, did not begin until 1995. Many commentators have used "Mercosur" as a shorthand to include steps in the early 1990s that led the formal establishment of the common market in 1995, and I should have accepted the shorthand. In private correspondence, Charles Calomiris has informed me that he did not consider the Argentine peso overvalued, contrary to what I inferred from one of his writings. Also, I incorrectly coded his views on dollarization, stating that he expressed no view about it even though a passage by him reproduced in Appendix 2 said it was feasible.

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DO ECONOMISTS REACH A CONCLUSION?

Do Economists Reach a Conclusion on Taxi Deregulation?

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ABSTRACT, KEYWORDS, *JEL* CODES

TAXIS ARE AN IMPORTANT ELEMENT OF MOBILITY IN THE transportation network of any city. Nationwide, taxis carry at least 40 percent more passengers than all other mass transit combined (Wohl 1982, 329; Rosenbloom 1985). Some transportation analysts consider taxis in metro areas to be a largely untapped transit resource (Trudel 1999 and Arnott 1996). Because taxis are more expensive than other transit services, they must offer something that other transit modes do not. In particular, taxi services are important to certain segments of the population. Seniors, housewives, the disabled, and the poor each account for a much higher share of taxi trips than their share of the population (Rosenbloom 1985; Weiner 1982).

Taxi markets are typically heavily regulated. Most cities control entry into the market and set prices, as well as set requirements for drivers, vehicles, finances, and operations (Shaw et al 1983, 1-7).

The research on taxi market regulation has been authored by economists, planners, engineers, geographers, and other transportation experts. Our task here is to focus on the judgments of economists who have written substantively on taxi regulation (publishing at least a few pages devoted to the subject). We include judgments expressed in published work if at least one of the authors is an "economist." The broad standard for

* Reason Foundation.

being an economist is that the individual has a post-graduate degree in economics or has a position with the title "economist," including a post in an academic economics department (we include our determinations in an Excel sheet linked from Appendix 1). We try to assess whether economists who exercise vital judgment on taxi regulation reach a conclusion.

Our investigation cleaves away all the taxi regulation research that is not authored by economists. This may seem unfair, but it is a necessary part of the method. Readers should be aware that the substantial literature by non-economists does tend to be more interventionist than the economic literature (e.g. Yang et. al. 2002 and 2005, Teal and Berglund 1987, Dempsey 1996, and Kang 1998), although some of it finds deregulation to be beneficial (e.g. Morrison 1997, Garling et.al. 1995) or neutral (e.g. Rosenbloom 1985).[1]

KEY FACTORS IN THE ANALYSIS OF TAXI MARKETS

The conventional wisdom is that the taxi market is unique and requires restrictions that few other markets do. But many markets have unique problems, and in few of them are draconian entry and price regulations considered necessary. Indeed, only utilities face similarly entry and price regulation, and taxis are not a natural monopoly. Frankena and Pautler's (1986, 139-40) review of the literature on economies of scale in taxi markets found no economies of scale in cruising or taxi stand markets, and some scale economies in radio-dispatch service. But Pagano and McKnight (1983, 299) argue that the literature on scale economies in radio-dispatch service posits scale economies but fails to show them empirically and that since most existing taxi companies offer all three kinds of service, economies of scale as a whole must be examined, and they find economies of scale only in small markets.

Evidence for systematic market failure in taxi markets is thin. Economists such as Cairns and Liston-Heyes (1996) create simplified models of taxi markets and find that search costs lead to market failures. Yet not all cities regulate taxi markets— Shaw et al (1983, 30, 48) reported that 12 percent have open entry and 23 percent do not regulate fares.

[1] For a very small number of works (e.g., Gelb 1982), we were unable to determine whether it met any of the "economist" criteria, and omit them.

Indeed, many cities have successfully deregulated taxi markets and not experienced substantial market failures (Frankena and Pautler 1984).

But rent seeking plays a large role in taxi market regulation. There is largely one source of pressure for most regulatory strictures—the incumbent taxi firms (Frankena and Pautler 1986, 147). Entry restrictions, combined with the independent-contractor system for drivers, means that taxi license owners make good profits off each license while leaving the drivers to bear most of the financial and customer-service risk and liabilities. In the early 1980s total monopoly rents for taxi license holders in New York City were $590 million and in Boston $48.8 million in 1980 dollars (McCarthy and McCarthy 2000, 369).

Eckert (1970) maps the evolution of taxi-monopoly franchises in Los Angles arising from the self-interest of regulators and franchise holders. New York is the quintessential example of rent seeking in taxi market regulation. There are almost 1400 *fewer* taxicab permits (medallions) in New York City than there were in 1937 (Markowitz 2004). Strong-arm power plays and elaborate schemes to allocate benefits from reduced competition have shaped the history of the city's taxi permits and continue to shape the debate today (Markowitz 2004). Rent seeking interests are so powerful that Dixit and Nalebuff (1993, 363-364) advocate renting taxi licenses so the city can capture the monopoly rents.

Others (such as Eckert 1970, Beesley and Glaister 1983, and Frankena and Pautler 1986 advocate removing entry restrictions and eliminating monopoly rents. Beesley and Glaister (1983, 611) estimated that entry and price restrictions lead to nearly $10 million per year in welfare losses in the city of London alone. Embedded in those welfare measures are the poor without cars, the elderly, the disabled, and others who now and then need affordable door-to-door transportation services and would benefit from a more competitive market. They are on the wrong side of the political calculus, with their dispersed costs overlooked in a regulatory process dominated by the concentrated benefiaries (Taylor 1989).

Regulation of taxi markets became widespread during the Depression era, but in the 1960s economists and transportation researchers began questioning the assumptions that underlie regulating entry and prices in taxi markets. Soon after, many cities began to experiment with loosening and even eliminating many of the regulations (Frankena and Pautler 1984). This spurred further interest in economic research into taxi market policy.

Implementing regulatory changes focused on the questions: Can the special problems of the taxi market only be solved by restricting entry and controlling prices? If not, what are the alternative mechanisms to prevent problems in the market? Often discussions revolved around how to modify regulations so as to ensure customer safety and the opportunity for satisfaction while letting the competitive market decide other factors. Meanwhile the new economic literature on taxi markets examined the theory and practice of deregulation and its outcomes.

Frankena and Pautler (1986) summarize the theoretical and empirical studies of less regulated taxi markets that led advocates of deregulations to assert benefits from greater competition. As summarized by Frankena and Pautler, the asserted benefits include:

- Lower fares, as more service providers compete in the market.
- Lower operating costs, due to competitive incentives.
- Improved service quality, as competition encourages taxi drivers to provide friendly reliable service and clean vehicles, and to avoid taking advantage of passenger ignorance. With competition reputation becomes more important.
- Innovations such as shared-ride markets and special services for the disabled, creating market niches where none had existed.
- Increasing demand for taxi services, as prices fall and quality improves.

It seems apparent that removing barriers to entry would increase the number of taxis operating and increase service levels. This means that more taxis are available in any given hour of the day, which makes the service more attractive to riders. Frankena and Pautler (1986, 150-154) found up to 30 percent increases in service levels in cities that opened up entry. But others, (Paratransit Services 1983 and Rosenblum 1985 for example) found that in some cities service levels changed little after deregulation. Teal and Berglund's seminal 1987 paper concluded that "taxicab deregulation cannot be demonstrated to have produced, in most cases, the benefits its proponents expected" (p. 54).

Increasing service levels is an important outcome. Greater service levels overall usually means greater service to the poorer sections of the city. The more competitive the taxi market, the better these areas tend to be served. Traditionally, poor areas of town receive the lowest levels of taxi service. At the same time, in poor areas general levels of mobility are often lower, and demand for taxi services higher.

Also, increased service levels have more impact at peak hours, at dense trip-generating sites, and during bad weather. A lot of people will only consider using a taxi if they are in a hurry or are traveling to or from the airport, special events, and shopping destinations. If service levels for these locations are too low, taxis will no longer be an attractive option, and passengers look elsewhere, or drive themselves.

Outcomes from deregulation are equally mixed in other dimensions. Frankena and Pautler (1986), Paratransit Services (1983), and Rosenbloom (1985) examine results from dozens of cities and find improvements in some cities and no improvement in others in terms of fares, operating costs, service variety, and total trips. Some detailed case studies of cities that deregulated, such as Beesley (1979) and Moore and Rose (1998) found substantial positive outcomes from deregulation, while others, such as Avants et. al. (1996) and Fingleton et. al. (1998) find few positive outcomes.

One might conclude from these conflicting results that sometimes deregulation works, and sometimes it doesn't. The devil is in the details of implementing deregulation and in what is measured to define success.

CHALLENGES IN EVALUATING THE OUTCOMES OF DEREGULATION

There are three main sources of disagreement in the literature.

Trips originating at airports

Taxi markets suffer from information problems. Customers at the curb are uncertain about the terms offered by any particular cab, and about alternative offers. With taxicabs in a queue at the airport and the stand coordinator instructing passengers to take the lead cab, there is no role for price or quality competition. Unrestricted fares in this case could mean severe price gouging and "rip-offs." When taxis are free to roam at the airport, and cabbies enter the terminal to solicit passengers, the visitors get a general sense of chaos. Some argue that without fare restrictions there are high transaction costs (Gallick and Sisk 1987, 127).

At the same time, regulators suffer information problems. As Beesley and Glaister (1983, 612) put it, "the chain of required observations is long," and "in light of the complex reasoning involved, a natural question arises about the feasibility of improving welfare by regulatory action." Yang et al

(2000) suggests that a very complex simultaneous equation system using a very large and rich data set, both of which likely exceed the grasp of most city regulatory agencies, only starts to predict some parameters of taxi market performance.

Even researchers who are very sympathetic to taxi deregulation maintain that at the airport fare deregulation might create severe problems (Rosenbloom 1985, 15, 18; Styring 1994, 35; La Croix et al 1985; Kirby's comments following LaGasse 1986). Some cities have responded with price controls for trips originating at the airport and manage congestion by limiting the number of taxis allowed to queue at airport and other congested taxi stands, and by establishing proprietary curb zones where only one taxi company may stop (Cervero 1996, 21). At various airports there have emerged system of exclusive contracts, special permits, and open entry, each of which, as La Croix et al (1992, 152) put it, "has its own advantages and disadvantages."

Airport authorities have some incentive to find rules that reduce conflicts at the curb over waiting passengers, and that assist passengers in finding the cab company they desire. Such site-based rules to deal with local information problems do not have the widespread repercussions that citywide regulations have. There are two general site-based solutions for the airport taxi market that take advantage of contracting by the airport authority rather than city regulatory authority: either the airport should manage service and fare differentiation with multiple taxi stands and a designated coordinator to aid passengers, or the airport should arrange uniform rates for all trips originating at the airport.

Information about fares in all taxi markets might be improved by requiring a uniform measure, such as the following: if the taxi establishes rates by distance, it must set its flag drop charge for the first 1/5 mile and additional travel per 1/3 mile (Doxsey 1986, 8). Imposing such units for rates would facilitate fare comparisons by consumers. Taxis could set their own flag drop charge high enough to make short trips worthwhile. Taxis ought to be permitted to utilize other rate structures—by zones, by journey duration, by time of day, etc.—but be required to use a uniform measure of distance *if* they elect to charge by distance.

The airport-origination issue is a tricky one for our review. First, there is the issue of whether, in the abstract, restrictions on such services should be deemed "intervention" or simply contract within the nexus of property. Just as a hotel owns its property and may lay down contractual rules that restrict the taxi operations at its drive-ups, an airport authority may be seen as the owner of the airport and may impose similar rules. In this view, unauthorized taxi operations are akin to trespass. To suggest that any problems arising from lawlessness at airports are failings of the free-market is not really fair, as the market and laissez-faire presuppose a law of property and contract relations. This is a matter of interpretation, and regardless of how we might come down on the matter, the second issue is whether the economists surveyed agree with this property-interpretation. Only La Croix et al (1992) among the economic literature focuses on airport issues and while they consider both property rights and regulatory mechanisms as a means of resolving the problems. The rest of the literature is not clear on this issue, so we have no easy way to resolve it.

Deregulation has been only partial

Little entrepreneurial flair has been observed in cities that have deregulated (Rosenbloom 1985, 191). However, deregulation has been only partial. Although many cities, including such widely studied cities such as Atlanta, San Diego, Seattle, and Indianapolis permitted free entry, they did not fully deregulate fares and services, nor allow market mechanisms to overcome information problems (Luciani 1997, 32-33). This limited the scope of competition between incumbents and new entrants and prevented taxis from offering new services or fares to win customers. Shared-ride services generally remained forbidden. Yet shared-ride might be a valuable service at high-volume origins like airports where an agent or stand operator helps passengers arrange shared rides. Shared-ride service is unlikely to develop in the absence of curbside coordinators or in dispersed origins and especially edge cities where virtually everyone drives (Teal 1986).

Service provision in less dense markets such as suburbs and rural areas did not always improve with partial deregulation. In some deregulated cities taxis still could not price the short haul specially, and continue to refuse such trips (Frankena and Pautler 1986, 155; Teal 1986). And while the fixed costs of entering the taxi market are low, the variable costs of full-service, especially dispatch, can limit competition and have led some to suggest subsidizing taxi travel (Arnott 1996) or separating dispatch from taxi production and running dispatch as an independent regulated monopoly and leaving competition on the streets among cabs (Hackner and Nyberg 1995).

Some effects of deregulation have gone unnoticed

There are some effects of deregulation that have gone unnoticed, effects associated with the changing status of cabs that were formerly illegal. Some argue that municipalities saved money by reducing the extent of regulation that they had to enforce (Frankena and Pautler 1986, 155), others say that almost all cities had to spend more than before, to track down independent and formerly illegal cabs and enforce safety codes (Rosenbloom 1985).

The deregulation literature has almost completely ignored the impact of erstwhile illegals (Suzuki 1985 and 1995). The existence of black market cabs in most regulated markets meant that total trips by taxi were underestimated, and real average prices in the market overstated (Schkolnik 1992 and Chavez 1992). Prior to deregulation, problems with illegals, which, like any black market service, probably had given ample cause for complaint, were not heard. With deregulation, large numbers of cabs suddenly enter the legitimate market, so we should expect the absolute number of complaints to increase. One would expect it to take some time for these taxis to bring themselves into compliance with safety and insurance codes.

Classifying Economists' Research by Style and Judgment

There is a substantial economic literature on taxi policy, much of which is model-building. A great deal focuses on refining taxi-market models and developing variations on regulatory schemes (for example, Yang et al 2000; Rometsch and Wolfstetter 1993; Arnott 1996; La Croix et al 1992; Beesley 1979; Schaller 1999).

We were able to identify 28 articles on taxi deregulation by economists.[2] Theoretical approaches dominate, with 8 articles taking model-building theoretical approaches, 10 mostly plain-talk theoretical

[2] It is perhaps worth clarifying that for the purposes of Table 1 we are looking for judgments on substantial forms of decontrol, worth speaking of as "deregulation." On this basis, we have omitted papers dealing with only one minor dimension of liberalization, for example, Flores-Guri (2005), which considers liberalizing merely the permissible pick-up domain of cruising taxis (and comes across as favoring such a liberalization). There are probably a few other papers of this type that we have passed over. Flores-Guri is included in the next section's list of quotations on taxi liberalization.

papers, and then 2 taking an empirical approach and 8 a case-study approach. Empirical studies evaluate data and use statistical significance to assess outcomes. Case-study articles examine deregulation descriptively in a city or set of cities.

As shown in Table 1, most economists who examine taxi deregulation conclude that it is on net beneficial. Of the 28 articles, nineteen concluded that deregulation is beneficial (on net), two conclude that the results are mixed, seven conclude deregulation is net harmful. Some of the articles do not state their conclusions so matter-of-factly or avoid stating what their results mean.[3] We have taken the liberty of inferring policy conclusions from the thrust of their analysis.

Note that the literature concluding that taxi deregulation is net beneficial is the richer literature, with articles from each approach to the issue. The literature concluding that taxi deregulation is net harmful is mostly model-building. And, without delving too much into criticism, it is clear that those articles derive their results from strong assumptions about information and transaction costs. The literature finding net benefits often uses a richer set of assumptions that include mechanisms for overcoming information and transaction cost challenges (Beesley and Glaister 1983; Gaunt 1996; Frankena and Pautler 1986; Williams 1980; Moore and Rose 1998).

[3] And for that reason, some of the authors listed in the table (namely, De Vany, Rouwendal et al, Styring, Fingleton et al, Doxsey, Shreiber, Gentzoglanis, LaGasse, and Toner) are not quoted in the next section of quoted judgments. Also, we have refrained from quoting Moore and Rose 1998.

Table 1: Classifying the Economic Literature on Taxi Market Deregulation

28 works	Mostly Model Building Theoretical Studies	Mostly Plain Talk Theoretical Studies	Mostly Empirical Studies	Case Studies
19 works Deregulation is good	*4* Beesley and Glaister De Vany Hackner and Nyberg Rouwendal et al	7 Boroski and Mildner Eckert Frankena and Pautler Kitch et al Kenny and McNutt Lephardt and Bast Williams	1 Pagano and McKnight	*7* Barrett Beesley Fingleton et al Gaunt Moore and Rose Staley Styring
2 works Mixed results	0	1 La Croix et al	0	1 Doxsey
7 works Deregulation is bad	*4* Cairns and Liston-Heyes Flath Gentzoglanis Schreiber	2 Gallick and Sisk LaGasse	1 Toner	0

ECONOMISTS' JUDGMENTS ON TAXI LIBERALIZATION

In the literature you do not often find economists expressing a firm judgment on taxi liberalization. But sometimes you do find judgments, either firm or reserved, and we have compiled as many such expressions as we could find.[4] Positive judgments dominate, perhaps because economists with positive judgments are more willing to express them.

A. Positive judgments of taxi liberalization (ordered chronologically)

The following judgments lean toward liberalization of restrictions on taxi services. We do not mean to suggest that they all support complete liberalization.

> A regulatory policy more hostile to the interests of taxi consumers [than territorial monopoly franchises] could scarcely be imagined. Taxi monopolies have doubtless raised prices and reduced output relative to those which would have existed in a competitively organized and unregulated market. (Eckert 1970, 449-50)

> Students of economics and urban transportation frequently cite the limitations of the number of taxicabs in most American cities as a clear case of unwise government policy. They argue that a limitation on the number of cabs can only operate to raise the price and decrease the supply of taxicab service as compared to that which would otherwise be provided. The authors of this article share the academic view. . . . Checker, Yellow and the independents share a common interest in preserving their legal protection against new competition. To further this interest they have been able to generate the myth that the industry, under competition, has been proven irresponsible

[4] Several of the quotations are from works that are not included in Table 1 (namely, works by Gomez-Ibanez and Meyer, Gordon and Richardson, Winston and Shirley, Flores-Guri, and Fingleton et al). Such quotations are usually passing remarks drawn by transportation economists but from works that do not significantly analyze taxi market liberalization.

> and unstable. Their version of the history of the taxicab industry ignores more than fifty years of apparently free entry and free rate regulation prior to 1929. It hints darkly of violence, but fails to note that the two major violent events apparently resulted first from the efforts of an existing company to obtain a de facto monopoly, and second from the grievances of drivers unhappy with their position under the regime of limited competition. This fabricated history has given the city's regulatory policies an air of propriety they would not otherwise have. (Kitch, Isaacson, and Kasper 1971, 285, 343)

> We have explored several hypotheses about reasons for the trend in total supply in the London taxi trade [there was substantial growth], reaching the broad conclusions that much of the apparent ability to keep real costs down in the face of rising real input prices has to do with adjustment in labour supply, itself a function of free entry, [and] competition with the hire car trade. (Beesley 1979, 130)

> We have provided a more intuitively satisfying description of the operation of modern taxicab markets, and demonstrated that there is no reason to believe than an unregulated taxicab industry will not be efficient. We conclude that there is little reason to regulate either price or entry. (Williams 1980, 111)

> [G]ranting of licenses on a municipality basis, which constrains the size of the firms, may not lead to a service being provided by a firm of the most efficient size. A licensing scheme involving several municipalities could result in more cost-efficient taxicab service. Secondly, in areas where the number of trips exceeds 100,000 per year, more than one firm can provide service efficiently. Thus, deregulation of larger markets probably would not result in monopoly providers. (Pagano and McKnight 1983, 309-310)

In light of the complex reasoning involved, a natural question arises about the feasibility of improving welfare by regulatory action. Regulators are dependent on restricted information. (Beesley and Glaister 1983, 612)

Although we believe the city officials' goals should ultimately be to refrain from future market intervention and allow the supply and price of taxi services to be determined by decisions made by owners, drivers and the riding public, a gradual policy of disengagement would minimize the disruptive effects of such a return to the free market. (Lephardt and Bast 1985, 14)

Experience with open entry in the radio-dispatch market has generally been favorable. This is important because typically about 75 percent of taxi trips are produced by radio-dispatched cabs. In marked contrast . . . there have been many problems at airport cab stands following regulatory reform. . . . These problems do not provide an argument in favor of entry restrictions, however. Rather, they suggest that there would be significant efficiency gains from either increasing fare competition at airports by altering the queue system or imposing or lowering fare ceilings on airport taxi service. (Frankena and Pautler 1986, 157-58)

The experiences of Colombo and Santiago suggest that if competition can be maintained, fare deregulation probably will not lead to large increases in fares or monopoly profits. In Colombo the CTB's low fares were clearly an important constraint on the pricing behavior of private operators, but no published reports of collusive or anticompetitive behavior have emerged. Santiago's experiences of stable fares in the competitive shared-ride taxis and large fare increases among the collusive taxibus and microbus route associations offer direct evidence of the importance of competition. (Gomez-Ibanez and Meyer 1993, 30)

> Increased vehicle occupancy in door-to-door vehicles is therefore the key to resolving the regions's traffic congestion problem. HOV lanes and transitways for buses, vanpools and carpools would reduce these vehicles' trip times—a competitive advantage. Deregulation of shuttle vans and taxis would permit these kinds of vehicles to offer shared-ride door-to-door services more competitive with individual automobiles. (Gordon and Richardson 1994)

> The sunk costs of an entrant cab is likely to be small . . . [and] the fixed costs are likely to be moderate. . . . All this together makes for a strong case for deregulation. (Hackner and Nyberg 1995, 204)

> [After deregulation of the taxi industry in New Zealand] large cities experienced significant new entry and real fare reductions, [but] only a modest increase in entry and minor reductions in real fares in medium sized cities, and minor reductions in industry size and minor increases in real fares in small towns. . . . Deregulation results, then, in significant adjustments to output and pricing in the large cities but only minor changes in the small centres. (Gaunt 1996)

> The authors would prefer a swift move to complete entry liberalisation, but in the absence of such a move, the revocation of the right to transfer the taxi plate is a necessary and inevitable step for any sustainable long term market configuration with entry liberalisation. A more efficient entry regime would, in our opinion, abate the principal-agent problem and allow urban sharecroppers to reap the benefits of ownership. It would also create an avenue for people of limited means to enjoy the benefits of an enterprise culture and the free market, while increasing consumer surplus for taxi users. (Kenny and McNutt 1998)

> This study does not call for more or "better" regulations. Instead, this paper argues that an improved taxicab market can arise by removing regulation and promoting

competition. Elements of this proposal have been tested in places such as Indianapolis, Washington D.C., Denver, Phoenix, and other cities, where deregulation has revived local taxicab markets. (Boroski and Mildner 1998)

On average our data suggest that, controlling for operating environments, fares are slightly higher and taxi availability (number of taxis) is slightly lower in those cities that have deregulated fares and market entry. . . . Taxi deregulation is likely to be most beneficial if it is part of a broader policy to stimulate competition in urban transportation. . . . The increased intermodal competition and coordination in a privatized and deregulated urban transportation system should lower taxi fares, improve services quality, and enable taxi operations to provide some competitive discipline for transit. (Winston and Shirley 1998, 104-5).

The fact that almost all cities that deregulate their local taxicab market experience an increase in the number of taxis in operation suggests that substantial unmet demand exists for these services. More importantly, this unmet demand can expand economic opportunities for central city residents. . . . Even though wages for existing drivers might fall, the benefits of putting more people to work as taxicab entrepreneurs, increasing the availability of taxicabs, and increasing the variety of taxicab services may more than outweigh the income losses experienced by existing companies. . . . More importantly, the mere fact new taxicab operators enter the market, providing new levels of service, suggests that economic opportunities are better driving taxis than other jobs. (Staley 1999, 10)

The Irish taxi deregulation resulted [in] a dramatic increase in new market entry unprecedented by international standards. Large reductions in passenger waiting times have made deregulation popular among the public. There has not been a reduction in either driver or vehicle standards. The Irish experience is that there should be full and immediate deregulation rather than mere liberalisation of taxi markets. (Barrett 2003, 39)

> Radical changes in the institutional organization of taxicab markets, such as outright elimination of the medallion system, do not seem to be politically feasible in most instances. Local regulators are often reluctant to confront the special interests and monopoly rents that entry regulations have created. Smaller regulatory changes, such as the elimination of exclusive cruising areas in adjacent cities with similar socio-economic characteristics, can improve market efficiency and increase benefits to consumers without being opposed by the taxicab industry. (Flores-Guri 2005, 165)

B. Mixed judgments of taxi liberalization

> Experience at airports using the open taxi system indicates that competition has brought neither lower prices nor better taxi service. Indeed, airport administrators at both the Los Angeles and the Atlanta Airports, who opened their airports due to political pressure, quickly found that the quality of taxi service deteriorated at both airports because it was difficult to "fix blame for poor quality service." Since 1989, both Seattle and Detroit have switched back to exclusive airport taxi service indicating that airport administrators and lawmakers are now placing greater value on service quality than the provision of equal airport access to all taxicab operators. . . . [However] [o]ur comparative analysis finds that criticism of exclusive contracts and open systems is often misplaced, as it fails to acknowledge the necessity to achieve political equilibrium and the differential importance assigned to particular goals by airport administrators. (La Croix et al. 1992)

> There are not enough taxis in Dublin and this has arisen because the regulatory system does not work. We propose that entry to the market be de-regulated and have suggested that this be done by issuing a new license to existing holders as a first stage in the full de-regulation of entry. This new entry should be accompanied by measures

> to improve both the quality standards and the enforcement of those standards. . . . On the other hand, we are not convinced by either the arguments or the evidence in favour of de-regulation of price and hence we recommend that fare controls continue. Because the existing system of regulation has not worked, a new system of regulation is needed. This should both regulate and manage the taxi market to the benefit of the customers of taxis and hence to society. (Fingleton, Evans, and Hogan 1998)

C. Negative judgments of taxi liberalization

> We have argued that average price regulation can in some circumstances significantly reduce exchange costs. (Gallick and Sisk 1987, 127)

> Price regulation is necessary to produce equilibrium in a simple model of the taxi industry. . . . This paper should be interpreted as implying that there are good reasons for regulation of this industry. (Cairns and Liston-Heyes 1996, 12)

> The rationale for taxi regulation now becomes apparent. First, even if regulatory capture entails collusive fare setting, its net distortions are made less than they otherwise would be by the fact that under laissez faire too pricing entails a degree of local monopoly. Second, even under regulatory capture, the number of vacant cabs would be set closer to the efficient level, given the prices, than would be true under laissez faire pricing and free entry. (Flath 2002, 19)

CONCLUSION

Two out of three articles on taxi-market policy by economists find taxi deregulation beneficial, and their judgments expressed in their writing show that a strong majority support deregulation. That some articles judge deregulation negatively arises in part from deregulation not having gone far enough. Also, there are unresolved issues about whether rules limiting airport services should be deemed "intervention," and about the effect of deregulation on the largely-unobserved illegal market.

Our own judgment is that taxi deregulation can work well when done right. We hope this body of research will begin to weigh against the rent seeking and bureaucratic self-interest that currently dominates the making of taxi-market policy.

Appendix 1

Link to Excel file showing our determination of who we counted as an "economist." Again, we treated only works coauthored by at least one economist, and we counted someone as an economist if he had a post-graduate degree in economics or a position with title "economist," including a post in an academic economics department.

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ECONOMICS IN PRACTICE: FOLLOW-UP

[*Editors' note*: In a previous issue of this journal, Dan Johansson tells of the results of a vocabulary analysis he conducted of the leading PhD economics textbooks. He shows that ideas of entrepreneurship, institutions, property rights, and freedom have almost no place in the textbooks of the core classes and industial-organization classes. (Johansson's article is available here.) We invited Professor Baumoul to comment on the article. Professor Baumol's outstanding professional achievements have involved a deep immersion in issues of entrepreneurship and innovation, and their relation to economic growth. That immersion is represented by such works as *The Free-Market Innovation Machine: Analyzing the Growth Miracle of Capitalism* (Princeton University Press, 2002). More information about Professor Baumol is found at the end of his contribution. We are grateful for his participation.]

Textbook Entrepreneurship: Comment on Johansson

WILLIAM J. BAUMOL*

I CAN ONLY APPLAUD DAN JOHANSSON'S EXCELLENT AND highly illuminating article (Johansson 2004). I have already and repeatedly joined other voices in noting the virtual expulsion of the entrepreneur from the contemporary mainstream literature of economics. I have also joined the call for the restoration of the entrepreneurs' place in theory, given the fact that no one seems to deny their importance for the workings of the free-market economy in general and for its growth and innovation in particular. Johansson's systematic review of the postgraduate textbook literature underscores these concerns. Here I should only like to repeat my

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own explanations for the entrepreneur's exclusion and to offer a general observation on the issue.

WHY THE ENTREPRENEUR HAS BEEN EXILED FROM STANDARD MICRO THEORY

There are, actually, at least two very good reasons why the entrepreneur is virtually never mentioned in modern theory of the firm and distribution. The first is that innovation is an entirely heterogeneous output. Production of whatever was an invention yesterday is mere repetition today. So that entrepreneurial activities do not incorporate the homogeneous elements that lend themselves to formal mathematical description, let alone the formal optimization analysis that is the foundation of the bulk of micro theory.

The more critical explanation of the absence of the entrepreneur is that in mainstream economics the theory is generally composed of equilibrium models in which structurally nothing is changing. Equilibrium models exclude the entrepreneur by their very nature. She is absent from such a model because she does not belong there. This has been definitively argued by Schumpeter and Kirzner who have demonstrated that sustained equilibrium is something that the entrepreneur does not tolerate, any more than she tolerates sustained disequilibrium. Here, Schumpeter's key insight is that the entrepreneur's occupation is the search for profitable opportunities to upset any equilibrium. That is exactly what any innovation, in the broadest sense, entails. But the rest of the story is told by Kirzner who recounts that the entrepreneur, with her critical ability, alertness, recognizes in any disequilibrium a profit opportunity, and by taking advantage of that opportunity she provides the pressures that move the economy toward an equilibrium condition. So the job of Schumpeter's entrepreneur is to upset all equilibria, while Kirzner's works to achieve them. The entrepreneurial mechanisms underlie continuous industrial evolution and revolution, and surely are not the stuff of which stationary models are built. Thus, it should hardly be surprising that a stationary Walrasian model, even in a more sophisticated variant, has no room for the entrepreneur.

This is particularly evident of the standard theory of the firm, which analyses the repetitious decisions of the enterprise that is already present and fully grown. In such a scenario the entrepreneur has already completed his job and left for places where her firm-creation faculties can be exercised. Even if the creator of the firm has not departed, she has transformed her role from entrepreneur to manager, so that though she, herself, remains in place, the entrepreneur has gone.

THE SIGNIFICANCE OF THE OMISSION

My conclusion is not that the neoclassical theory is wrong in excluding the entrepreneur, for it is dealing with subjects for which she is irrelevant. But that does not mean that no theory of entrepreneurship is needed. Here, let me be clear. I have always believed that fruitful research requires a thousand flowers to be left to bloom. It would, in my view, be as indefensible to require *all* micro writing to give pride of place to the entrepreneur as to exclude him universally. In particular, static analysis has offered many valuable insights and its body of theory is an admirable accomplishment. So the entrepreneur is legitimately not offered a place in static theory, without undermining the value of that theory.

But universal exclusion condemns us to leave out of our discussions what I consider to be the most critical issues that should be examined (though not exclusively) in microeconomic terms: the determinants of innovation and growth and the means by which they can be preserved and stimulated. Dan Johansson has helped to demonstrate that we economists are leaving to the hands of others, such as the economic and business historians, what I regard as the greatest and most important mysteries that economics faces: Why have the relatively free-market economies in the past two centuries been able to outstrip, probably by more than an order of magnitude, the performance in terms of growth and innovation, of all other forms of economic organization? The answer is not merely a matter of pandering to what Veblen called the economic researcher's idle curiosity. Rather it is the missing underpinning for growth policy in both the developed and the developing world.

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GO TO JOHANSSON (2004) ARTICLE ON GRADUATE TEXTBOOKS

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CHARACTER ISSUES

364 Economists on Economic Policy

GEOFFREY WOOD*

Abstract, Keywords, JEL Codes

IN 1981, 25 YEARS AGO, THERE WAS A REVOLUTION IN BRITISH economic policy. Completely rejecting the conventional wisdom which had dominated the post-war years, the government then in office tightened fiscal policy in the depths of a recession and committed itself to using monetary policy to reduce and then control inflation. Three hundred and sixty four economists, mostly academic, but with five retired senior government advisers among them, then signed a letter to the London *Times*. It was very hostile to these economic policies, which were proposed by the first administration led by Mrs. Thatcher, who had taken office as Prime Minister in 1979.

Engagement in policy debate of a large number of "public intellectuals," is not common in Britain. (364 signatories is perhaps the equivalent of 1500 signatories in an American context.) Policy benefits from open debate. More interventions would certainly be desirable. But whether intervention in the form of a letter with so many signatories is a good way of intervening is considered briefly in the concluding section of this paper.

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I am indebted to Forrest Capie, Max Corden, and Anna Schwartz for their most helpful comments on a draft of this paper. The comments of two anonymous referees have also been useful.

The letter is reproduced here.

"We, who are all present or retired members of the economics staffs of British universities, are convinced that:

a) there is no basis in economic theory or supporting evidence for the Government's belief that by deflating demand they will bring inflation permanently under control and thereby induce an automatic recovery in output and employment;

b) present politics will deepen the depression, erode the industrial base of our economy and threaten its social and political stability;

c) there are alternative policies; and

d) the time has come to reject monetarist policies and consider urgently which alternative offers the best hope of sustained recovery."

1981: THE TIME OF THE LETTER

What prompted the letter? In 1981 the British economy was undoubtedly beset with problems. Sir Geoffrey Howe, then Britain's Chancellor of the Exchequer (that is, minister of finance), gave a public lecture just two months after the publication of the letter. When the lecture was republished in 2001, he added a postscript (Howe 2001). A quotation from that postscript sets the scene well. "The 'fight against inflation,' which had peaked at 22 percent [during 1980], was indeed of the highest importance" (53). That sentence describes perfectly the belief which pervaded his 1981 budget. His original 1981 lecture well describes how policy participants in that year saw the situation:

> The average rate of inflation under successive governments in the years to 1979 has marched remorselessly upwards: 3.5 percent, 4.5 percent, 9 percent, 15 per cent. Meanwhile, unemployment also rose: 300,000, half a million, three-quarters of a million, one and a quarter million. (Howe 2001, 43)

Then he went on to explain why his budget policy had been determined as it had:

> All kinds of shocks can affect prices in the short run . . . but to control inflation on a permanent basis it is necessary to control the rate of monetary growth . . . . In short, if the underlying causes of inflation are not tackled a policy of price control can only check price rises for a short time. (Howe 2001, 44-45)

Next he cited evidence from other countries in support of his views, and remarked how previous British governments, Labour governments, had carried out policies that they could defend only if they shared his view on inflation. He also cited previous experience to illustrate that one could not control domestic monetary conditions without letting the exchange rate move as necessary for these monetary conditions to be achieved.

So far, he was close to what is nowadays conventional wisdom in Britain—at least among those who work on this area of policy—and were in 1981 close to the mainstream of U.S. economic opinion. We come next, however, to something which needs to be explained in its institutional context—what he called "Supporting Policies." He wrote that "fiscal policy must be compatible with our monetary policy" (48). By "compatible" he covered a range of connections.

> Experience shows that it is virtually impossible to finance an excessive public sector deficit without adding to the money supply. Even were it possible, it could jeopardise success against inflation by adding to nominal incomes or precipitating a fall in the exchange rate. Excessive public borrowing could also, in some circumstances, increase the

> transitional costs of reducing inflation. The high interest rates which might be necessary to finance an excessive PSBR [Public Sector Borrowing Requirement—the name by which the consolidated borrowing of the British government was at that time known] would bear most heavily on companies, leading to reductions in investment and stockbuilding. If this more than offset the direct effects on aggregate demand of the PSBR itself, there would be higher unemployment in the short run as well as a weakening of growth prospects in the long run. (Howe 2001, 48)

Intertwined here are several factors. The one which requires explanation at this point is his concern over financing deficits without money creation. It is well known that behind almost every very rapid inflation were large deficits leading to money creation (Capie 1986), but such problems have tended to emerge in much more extreme economic and political circumstances than Sir Geoffrey Howe was describing. The common problem up to 1981 had been that governments (which still instructed the Bank of England on interest rates) were often unwilling to vary interest rates sufficiently to sell debt to finance their expenditure. This reluctance led to monetary accommodation of government spending. Thus, while what concerned Sir Geoffrey Howe was not *necessarily* a problem in his circumstances, institutional practices made it likely that it would be.

That, then, describes the intellectual background to the policies he carried out. What were the actual policies, and what were their outcomes? In the 2001 postscript Howe wrote:

> Targets for progressive reduction in the rate of monetary growth had been set, as required, for the second and third years of my Medium Term Financial Strategy.[1] Notwithstanding the depth of the recession we were experiencing, I had proposed substantial tax increases to reduce public sector borrowing to levels consistent with the lower monetary targets. All hell had broken loose. (Howe 2001, 53-54)

[1] That was the practice of announcing targets for money growth and public sector borrowing for several years ahead.

This "hell" included the above-mentioned letter with 364 signatories Sir Geoffrey continues, with understandable satisfaction:

> Their timing (i.e. of the letter) could not have been more apt. The fall in national output came to an end in that very quarter. Over the next eight years, real GDP grew by an average of 3.2 per cent per annum…By the end of my last year in the Treasury (June 1983) all the measured monetary aggregates were for the first time ever within their target range and inflation was down to 5 per cent – lower than at any time since 1970. (Howe 2001, 54)

It is now almost time to turn to the criticisms expressed by the 364 signatories. But two matters remain.

First, I have mentioned that nowadays the views expressed in 1981 by Sir Geoffrey Howe are in the policy mainstream in Britain, and were in the United States back in 1981. But in Britain in 1981 the cost-push theory of inflation was common. Academic economic opinion by and large maintained that only incomes and prices policies could deal with inflation. According to some the Phillips curve provided a long-run trade-off. These views were certainly not held universally, however. For example, when I started my graduate studies (at the University of Essex, in 1967) the textbook we used in the introductory macroeconomics course was Martin J. Bailey's *National Income and the Price Level*. The views of Sir Geoffrey Howe, with monetary policy changes being in the long-run neutral but having short-run real effects, can be found in that book. So, too, can much of the analysis of the short-run effects of fiscal policy that is implicit in Howe's words quoted above. I disagreed with the views of the 364 at the time[2] and do today; but why their opinions on inflation remained so common in Britain is a puzzle to me still.[3]

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[2] I was moved to write an article, "Can 364 Economists be wrong?", which appeared in *Economic Affairs*. This article, along with the original letter, a list of its signatories, and additional commentary, is to be published by the Institute of Economic Affairs in March 2006 (Booth 2006).

[3] What the views on inflation of academic economists are in Britain nowadays it is not really possible to say. Interest in and discussion of the subject have both fallen with the inflation rate. What discussion there is accepts a monetary view; but the discussion is primarily among central bankers charged with controlling inflation by use of monetary policy, and economic historians, who have in general always been sympathetic to monetary explanations.

Second, while what Sir Geoffrey Howe wrote is in no way misleading, it is a series of snapshots. It may be useful to have a table showing the performance of the British economy from 1973 to1984.

Table 1: UK Economic Aggregates, 1973-1984

	GNP Growth % pa	Inflation % pa	Unemployment %
1973	7.3	9.18	1.9
1974	-1.7	15.98	1.9
1975	-1.1	24.11	2.9
1976	2.6	16.77	3.9
1977	2.6	15.89	4.1
1978	3.0	8.28	4.1
1979	2.6	13.35	3.8
1980	-2.3	18.07	4.8
1981	-1.6	11.59	7.6
1982	2.0	8.66	9.0
1983	3.2	4.61	9.9
1984	2.4	4.96	10.1

Sources and Definitions: Column 1. UK Accounts 'Blue Book,' 1985: Rates of change of GDP at constant factor cost. Column 2. Datastream: UK Retail Price Index, % pa. Column 3. Datastream: Claimant Count Measure of those registered for unemployment benefit.

Table 1 shows for 1973 to 1979 the GDP slowdown followed by acceleration, and the reverse pattern taken by inflation. It can also be seen that through 1984 unemployment remained high, (although it did subsequently fall). The phenomenon of income recovering from recession while employment fails to do so even with a substantial lag is not unique to Britain. It is, for example, a problem that Finland still faces after its 1992 recession. Whether the policies of the 364 might have achieved a faster fall in unemployment is considered briefly below.

THE VIEWS OF THE 364

The letter's four paragraphs of comments are best reviewed in the order they appear.

> (a) "There is no basis in economic theory or supporting evidence that by deflating demand they [i.e. the government] will bring inflation permanently under control and thereby induce an automatic recovery in output and employment."

This paragraph requires perhaps decoding rather than just reading. What is meant by "deflating demand"? Certainly reading the Budget at the time, and looking at the Geoffrey Howe quotations provided here does not suggest that the Thatcher government intended to reduce demand permanently so that the price level would fall without limit. Nor did they aim for a temporary squeeze, followed by return to excess monetary expansion. What the Thatcher government plainly intended was monetary control so as first to reduce inflation and then keep it at a tolerable level. As it turned out, the weight they placed on money supply measures to guide to monetary policy turned out to be perhaps greater than the measures could bear. Long-established relationships between money growth and future inflation suddenly seemed not as reliable as they had been—indeed, this is implied by Sir Geoffrey Howe's quoted remark that money growth was within its target ranges "for the first time ever," at the same time that inflation had fallen more or less as planned. But the government did pursue monetary stringency and inflation did fall. In recent years the Bank of England has had sole responsibility for controlling inflation, and has done so. Monetary policy is the only policy the Bank can implement. This observation alone suggests that monetary policy is what matters for inflation. But if anyone wished to look further, they could look at the numerous studies of the long run relationship between money and prices.

What about the claim that the 364 attribute to the policy makers, that deflating demand will control inflation "thereby inducing an automatic recovery in output and prices"? The criticism is puzzling. Unless continual monetary shocks are administered to an economy, eventually money will become a "veil," with real economic performance determined by numerous other factors. Once the rate of change of prices has stabilised, output and unemployment would tend to revert to their long-run level, whatever that was.[4] There is neither theory nor evidence to suggest that stopping inflation depresses demand forever.

So, to summarise, if point (a) is decoded as meaning that stopping inflation requires permanent recession, there is, in the words of the letter, "no basis in economic theory or supporting evidence" for the proposition.

> (b) "Present policies will deepen the depression, erode the industrial base of our economy and threaten its social and political stability."

The assertion certainly does not look too good in retrospect. Indeed, one might argue that the policies restored social and political stability. The years of high inflation had been years of accelerating wage claims, and attempts to resist these in the public sector had led to increasing economic and social disruption. These culminated in the winter before Mrs. Thatcher's 1979 election victory. In that winter, which became known as the "Winter of Discontent," bodies were left unburied.

What about the "industrial base"? Here, too, there is lack of clarity. Did the signatories mean manufacturing industry? That has on average been falling as a share of Britain's output since the 19th century. If the production of services qualifies for membership of the "industrial base," then it should be noted that service industries are thriving. But of course, most important of all, output per head has been rising.

> (c) "There are alternative policies."

To that elliptical statement, one is tempted to respond, "no doubt," and leave it there. But one can go a little further. High wage claims and high wage settlements characterised Britain's years of high inflation. Wage claims remaining high despite falling inflation might have been a factor in the employment-recovery lag. But what policy could deal with this? Controls over incomes, perhaps supplemented if only for political reasons by controls over prices, might appear to be a possibility; but a recent study in the *Scottish Journal of Political Economy* (Capie and Wood 2002) found that such controls in the UK had a systematic effect on prices only in wartime, when they were supplemented by a complex rationing system.

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[4] Multiple real equilibria are possible in theory, but this possibility can not affect the conduct of monetary policy, for one could not know in advance of being there where these equlibria were, or how monetary policy could move an economy from one to another.

It is also worth reflecting whether, without the fiscal squeeze, there would have been a faster recovery in output and employment. This is a possibility; but there are reasons to doubt it. Britain had at that time a floating exchange rate, which at the least diminishes the effect of fiscal policy.[5] Second, the recovery in output was already rapid, and, as remarked above, Britain is not the only economy to have experienced what is sometimes called a jobless recovery. Last, and perhaps most important, in view of the reluctance to vary interest rates so as fully to cover government deficits by debt sales, a laxer fiscal policy might indeed, as Sir Geoffrey Howe feared, have undermined monetary policy.

> (d) "The time has come to reject monetarist policies and consider urgently which alternative offers the best hope of sustained economic recovery."

The trouble with that statement is, of course, that monetarist policies were, and are, based on sound theory and evidence, if at least one regards monetarist policies as the use of monetary policy to control inflation. It is hard to see how the signatories of the letter could think otherwise; monetarism, whatever its theoretical novelty to the letter's signatories, was not new to them in practice. The UK had ample experience in the use of monetary policy. It just happened to be monetary policy of excessive ease. The most notable had been the monetary promiscuity of the last Conservative government to precede Mrs. Thatcher's 1979 administration. That government, led by Edward Heath and with Anthony Barber as Chancellor of the Exchequer, had pursued a very easy monetary policy and thus given Britain its greatest peacetime inflation since the reign of Henry the Eighth.

[5] This diminution is consequence of the interaction of interest rate and exchange rate movements that is explicated in the well known Mundell-Fleming framework. That effect is of course aside from the other qualifications to the effectiveness of fiscal policy, which hold regardless of the exchange rate regime.

CONCLUDING REFLECTIONS

Was there behind the letter a belief that inflation did not matter? It is undeniable that the effects of inflation on growth rates of output, when inflation is below about 10 percent per year, are in the short run not easy to measure. They do, however, seem to accumulate over time (Barro 1996). It is also undeniable that higher inflation does not buy us a higher level of output forever.

Further, people seem to dislike inflation. Economic theorists may say that they are foolish so to do; but it is not clear that economic theorists are entitled to tell people what their tastes ought to be.

Why, then was the letter written, and written so vaguely? The second part is easier to answer; the vaguer a statement is, the harder it is to object to. The desire for a large number of signatures may have led to a vague document. If that is the case, then perhaps such letters are not a good way of getting academics involved in policy debate. A large number of signatories reduces the individual cost of being wrong, so people may think less carefully about what they are doing, or be more susceptible to peer pressure. In addition, the vagueness necessary to get so many signatories does little for the credibility of academic economists as contributors to policy debate.

Why was the letter written at all? It is clear that there could be, within the standard analytical framework that plainly was behind the budget, dispute over whether the time was appropriate for a fiscal contraction, and if so, how big the contraction should be. But the letter went much wider in its criticisms. To repeat, I find it puzzling why the signatories held the views they did. At the time I thought the letter's assertions wrong, and I still think them wrong. In that I rest not on what has happened since the letter appeared—although by and large that supports my view—but on the preceding centuries of economic theorising and economic history.

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CHARACTER ISSUES

AEA Ideology: Campaign Contributions of American Economic Association Members, Committee Members, Officers, Editors, Referees, Authors, and Acknowledgees

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Abstract, Keywords, JEL Codes

The purpose of the American Economic Association, according to its charter, is the encouragement of economic research, the issue of publications on economic subjects, and the encouragement of perfect freedom of economic discussion. The Association as such takes no partisan attitude, nor does it commit its members to any position on practical economic questions. It is the organ of no party, sect, or institution. People of all shades of economic opinion are found among its members, and widely different issues are given a hearing in its annual meetings and through its publications.

—American Economic Review Papers & Proceedings *May 2004,* vi.

FOR YEARS THOSE WORDS HAVE APPEARED ON THE OPENING page of the May *American Economic Review*. What evidence speaks to whether the Association encourages "perfect freedom of economic discussion" and is "the organ of no party"? Does the AEA represent "people of all shades of economic opinion"? One way to get at these questions is to examine the party affiliation of those involved with the Association's leadership and publications. A dominating ratio of one party, particularly among officers,

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editors, and authors, would raise questions about the ideological character of the Association.

The problem is uncovering party affiliation. Because party registration is public information, one could in principle identify the party registration of each AEA officer. Voter registration records, however, remain local, and AEA leadership is not concentrated in a particular geographic area, so one would have to embark on a large research circuit to gather the information. Eventually all voter registration records may become available online, but until then uncovering the party registration of AEA leaders seems out of reach.

Another approach is to survey the target groups. Several recent studies ask academics about their political leanings (e.g., Brookings 2001; Rothman, Lichter, and Nevitte 2005). Klein and Stern (2006, 2005b) report on their survey of AEA members, including a question about voting behavior. But a survey directed specifically at the AEA leadership would face response gaps and the possibility of strategic responding. With the growing public attention given to academic ideology, there are increasing concerns about response biases.

Another possibility is campaign contributions. Under Federal election law, political organizations must report the source of any contribution of \$200 or more.[1] Contributions capture the intensity of political preferences in a way that other measures of party affiliation do not. Research suggests that those who contribute to campaigns are more likely to be politically engaged in other ways, as with meeting attendance, letter writing, talking with others, and voting (Anslolabehere, de Figueiredo, and Snyder 2003, 118, and references therein). The party-attachment signal of a campaign contribution may be stronger than that of party registration, self-identification, or voting pattern.

Using campaign contributions, however, creates some problems. First, some contributors fail to provide all the information the law requires. For example, in the 2004 election cycle, about 17 percent of \$200+ Kerry contributors lacked full disclosure, as did 6 percent of Bush contributors.[2]

[1] Such contributions make up most of the money coming from individual contributors. For example, 70 percent of all individual contributions to presidential candidates in the 2004 election cycle came from \$200+ contributions (based on figures reported by Opensecrets.org).

[2] These percentages were reported by Opensecrets.org. Campaigns are allowed to accept contributions with less than full disclosure as long as a "best effort" is made to secure the information.

But even in such cases, there is usually enough information to identify the donor.

A more significant problem is that only a tiny minority contributes a reportable amount. In the 2004 election cycle, for example, roughly 600,000 contributors gave $200 or more to presidential candidates or to national party committees.[3] Since the U.S. voting-age population was about 220 million, those giving a reportable amount constituted just 0.3 percent of that population.

Thus, campaign contributions reflect party preference, but few give a reportable amount. Even so, campaign contributions may still shed light on the ideological character of those who control the Association and its publications. For example, examining campaign contributions can tell us whether there are at least *some* supporters of each party among AEA leadership and publications. It may also tell us whether there is a noticeable preponderance of supporters of one party.

AMERICAN ECONOMIC ASSOCIATION

To get an idea of the political makeup of the Association, I begin with contributions from a large sample of members. Reports to the Federal Election Commission of individual contributions of $200 or more to presidential campaigns or to national party committees have been compiled online at Fundrace 2004 (www.fundrace.org/neighbors.php). Using that search engine, I identify each individual contributor based on a first and last name cross-referenced with an occupation, employer, and address. Whenever I talk about campaign contributors, I mean contributors of $200 or more to the presidential candidates or to national party committees in the 2004 election cycle.[4] This covers contributions made throughout 2003 and during 2004 up to the cutoff date of October 13, 2004.[5]

[3] This is based on actual figures for presidential contributors from Opensecrets.Org reports and estimates for those giving to national party committees. Though estimates of contributions by party are less precise, the ratio of Democrat to Republican contributors was about 1.4 to 1.

[4] Candidates include all Democrats and Republicans who sought the presidential nomination. National parties include the Democratic National Committee and the Republican National Committee. Not captured are contributions to third-party presidential candidates and parties, but such contributions were miniscule in the 2004 election cycle.

AEA Members: 5.1 to 1

I investigate the contribution records for a sample of 2,000 AEA members with U.S. addresses appearing in the Association's online directory.[6] The sample is limited to those with U.S. addresses because campaign officials cannot legally accept contributions from foreign nationals (except those with "green cards").[7] Among this sample, 77, or 3.8 percent, gave to Democrats. Fifteen members, or 0.7 percent, gave to Republicans. The Democrat-to-Republican contributor ratio was 77 to 15, or 5.1 to 1. The 77 Democrat contributors gave an average of $1,391, or about one fifth more than the $1,152 averaged by the 15 Republican contributors. The overall contributor rate among the AEA member sample

Contributors of $200 or more to Ralph Nader, for example, amounted to only 0.001 percent of the voting-age population. And he drew twice the support of any other third-party presidential candidate.

[5] Since President Bush and Senator Kerry accepted public funds for the general election, neither could accept individual contributions after their nominating conventions. Thus, nearly all individual contributions to presidential campaigns would have been reported by the October 13, 2004 cutoff date. But contributions could still have been made to the national party committees after that date, so they would not be captured in my survey.

[6] The directory reflects the roster as updated by AEA members through December 31, 2004. The sample originally consisted of the first 500 names with U.S. addresses to appear in the online directory, which limited it to last names beginning with "A." The editor asked that the sample be expanded to 2,000 by adding the first 60 names with U.S. addresses for the letters B through Z. The letters Q and X had insufficient entries to reach 60 so names were added to the next letters. A 2,000 member sample amounts to well over 10 percent of all AEA members with U.S. addresses. Excel files in Appendix 1 show amounts given by each contributor in this sample and by all the other AEA groups examined in this study. Even though contribution data are public, contributor names are redacted to keep the focus on the larger issues of the study. For a few key individuals, such as AEA presidents, top editors, and committee chairs, even though names are redacted, a curious reader could easily identify them. More generally, a curious reader could use the search engine at Fundrace 2004 to look up anyone's campaign contributions of $200 or more.

[7] In addition, a member who in the directory identifies his or her "Principle Current Position" as "student" or "graduate student" was excluded from the sample, first because many students, as foreign nationals without green cards, could not legally contribute to U.S. campaigns and second because few of the rest would have sufficient income to contribute $200 or more. Because the contribution profile of members will serve as a benchmark for comparison with those involved in the Association's leadership and publications, including in the member sample those clearly identified as students would merely introduce noise into such a comparison. Many AEA members offer no information beyond a mailing address, so the sample inevitably includes some students. But no campaign contributor in the AEA sample identified himself or herself as a "student" or a "graduate student," so it's reasonable to conclude that few, if any, AEA student members in the sample made campaign contributions during the 2004 election cycle.

was about 15 times that for the U.S. voting-age population. Some of this difference was likely due to the higher average incomes of AEA members compared to the voting-age population.[8] Among the member sample's academic contributors, 32 gave to Democrats and 8 gave to Republicans, for a contributor ratio of 4 to 1. Among nonacademic contributors, 45 gave to Democrats and 7 to Republicans, for a ratio of 6.4 to 1.

How does the Democrat advantage found among AEA members compare with other findings using other measures of party affiliation? In a 2001 phone survey, 160 mostly academic economists were asked about their political affiliations. Forty-one percent described themselves as Democrats and 6 percent said Republicans, for a ratio of about 7 to 1 (Brookings 2001, 54). Another 22 percent identified themselves as independent but leaning Democrat, while 11 percent said they were independent but leaning Republican. When those groups are added in to get a broader definition of political leanings, the comparison is 63 percent Democrat or independent but leaning Democrat versus 17 percent Republican or independent leaning Republican, for a contributor ratio of 3.7 to 1. This is similar to the 4-to-1 ratio found among academics in my AEA sample.

Klein and Stern conducted a mail survey of 1,000 AEA members, asking among other things "To which political party have the candidates you've voted for in the past ten years mostly belonged?" (2005c). The response rate was 26.6 percent, but nearly all who responded to the survey answered that question. Weeding out those not in academia and those 71 years of age or older reduced the sample further. The bottom line is that 72 said they voted mostly for Democrats in the last ten years and 24 said mostly Republicans, for a ratio of 3 to 1. This is not far below the 4-to-1 ratio found among academics in the AEA sample.

The 3.8 percent Democrat contributor rate and the 5.1-to-1 ratio of Democrat-to-Republican contributors found for the AEA member sample will serve as a frame of reference, or a benchmark, for comparison with AEA leaders.

[8] According to Ansolabehere, de Figueirero., and Snyder (2003, 118), survey research in political science and sociology finds income the best predictor of campaign contributions.

AMERICAN ECONOMIC REVIEW

The *American Economic Review* (*AER*) is the flagship publication of the Association and arguably the premier economic journal in the world.[9] Submissions are blind-refereed, and the reviewing process is fairly decentralized, with an acceptance rate in recent years under 10 percent. Editors have little control over what gets submitted and, consequently, are more reactive than proactive.

AER **Editors: 9 to 0**

My focus will be on articles published in 2003 and 2004, but, on average, articles were initially submitted two years before publication,[10] so I examined the campaign contributions of editors serving anytime between 2000 and 2004. Of the 88 such editors (including the editor, co-editors, managing editor, and editorial board members)[11] 84 were from U.S. institutions and four had foreign affiliations and are excluded.[12] Nine of the 84 contributed to Democrats. None gave to Republicans. Thus, 10.7 percent of *AER* editors from the United States gave to Democrats, a rate nearly triple the 3.8 percent among the AEA member sample. The nine editors gave an average of $1,044 to Democrats.

AER **Referees: Data unknown**

It would have been nice to track down *AER* referees' contributions. Identifying contributors requires at least a first and last name, however, and an affiliation is also helpful. The *AER* editor thanks referees in the annual

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[9] In a world-wide ranking of journals, the *AER* finished at the top by a comfortable margin (Kalaitzidakis, Mamuneas, and Stengos 2003, 1349).
[10] In January 2004, the editor reported that the average lag between submission and publication had declined from three years to two years (Minutes 2004, 489).
[11] Here and elsewhere, the term "editors" excludes professional staff serving as assistant editors.
[12] Because the *AER* does not list editor affiliations, I used the AEA online directory as a first pass to determine affiliation. If an editor was not among listed members, I searched the web to find the affiliation. By ruling out those with foreign affiliations, I could possibly miss contributions from those Americans working abroad, who can legally contribute to U.S. campaign. But a check found that no editor or author with a foreign affiliation contributed during the 2004 election cycle. This holds for all publications examined in this study.

report but recognizes them only by a first initial and a last name without an affiliation.

AER Authors: 19 to 2

A total 379 authors published articles or shorter papers in regular issues of the *AER* in 2003 and 2004 (*Papers* & *Proceedings* issues are examined separately). The 112 authors with foreign affiliations are excluded, leaving 267 with U.S. affiliations.[13] Nineteen authors, or 7.1 percent, contributed an average of $871 to Democrats. The Democrat contributor rate of authors was below that of editors but nearly double that of the AEA member sample. A lone Republican contributor, who gave $500, authored two papers, or 0.7 percent of the 267 total. Recall that no editor gave to Republicans. The Republican contributor rate among *AER* authors of 0.7 percent is the same as for the AEA member sample. With 19 giving to Democrats and two to Republicans, the ratio of Democrat-to-Republican contributors among *AER* authors is 9.5 to 1.[14]

AER PAPERS & PROCEEDINGS

In May of each year, the *AER* publishes papers from selected sessions of the Association's annual meeting. According to AEA Bylaws, the president-elect is responsible for the program. After appointing a committee to help, the president-elect identifies sessions for inclusion in the May *AER Papers* & *Proceedings* (*P*&*P*). In 2003, the Association solely or jointly sponsored 145 sessions, but papers from only 25 sessions were published in the May issue. In 2004, only 25 of 138 made it. Assembling the

[13] An individual who authored two articles was counted twice in the author totals. Thus, the total author count of 267 does not mean 267 unique authors. There were 267 author slots, and an author of two papers filled two author slots. No distinction was made between sole and joint authorship.

[14] All *AER* authors who made campaign contributions were from academia. One might be tempted to conclude that *AER* authors lean more Democratic than do AEA members because authors are more academic. But recall that the Democrat-to-Republican contributor ratio of 4 to 1 for academics in the 2,000 member sample was below the 6.4-to-1 ratio for nonacademics.

P&*P* issues are two special editors not on the program committee or among regular *AER* editors. They have little say in what's published. Once the president-elect identifies a session for the May issue, a paper presented at that session is rejected by the special editors only if found to be "utterly without merit," a disclaimer that has appeared verbatim in every *P*&*P* issue dating back at least to 1999. The president-elect and program committee thus have much freedom in setting the agenda, selecting session chairs and authors, and deciding which sessions will appear in the *P*&*P* issue.

AER P&P Leadership: 7 to 1

Counting the presidents-elect, there were 18 members of the program committee in 2003 and 19 members in 2004, all with U.S. affiliations and none appearing on both committees. Of the 37 program committee members, seven, or 18.9 percent, contributed an average of $936 to Democrats. One member, or 2.7 percent, gave $3,000 to Republicans. The lone Republican donor was the 2003 president-elect (and 2004 president) responsible for the 2004 program.

AER P&P Authors: 32 to 1

Since program committee members had wider discretion in selecting authors than did regular *AER* editors, and since committee members had a higher Democrat contributor rate, some might be more willing and able than regular *AER* editors to select fellow Democrats and exclude Republicans. I am not suggesting that program committee members would be aware of political contributions of potential authors (for one thing, most authors were selected before the 2004 campaign was underway) or would even be thinking about the political affiliation of paper authors *per se*. By using campaign contributions to signal party affiliation, I am trying to uncover an affinity between the political sensibilities of some program committee members and the political sensibilities of some authors. Do birds of a feather flock together? A total of 305 authors appeared in *P*&*P* issues for 2003 and 2004. Sixteen had foreign affiliations, leaving 289 in the two-year sample. Thirty-two (16 each year), or 11.1 percent, contributed to Democrats. This exceeds the 7.1 percent Democrat contributor rate among authors in regular *AER* issues and is about three times the 3.8 percent Democrat contributor rate among AEA members. Democrat contributions averaged $2,056, well above the Democrat averages for regular *AER* authors and for AEA members. Only one author, or 0.3 percent of the 289, gave to Republicans—the 2003 president-elect who headed the 2004 program. The Republican contributor rate of 0.3 percent is less than half the 0.7 percent for regular *AER* authors and for AEA members. The 32-to-1 contributor ratio is more than three times that for regular *AER* authors and more than six times that for AEA members. The Republican influence of the 2004 president seems to have been limited to getting himself on that program and into the 2004 *P&P* issue.[15] On the other hand, each year 16 authors contributed to Democrats. Sessions involving Democratic contributors covered international trade, globalization, the IMF and World Bank, monetary policy, fiscal policy, health care, federal health insurance, Social Security reform, welfare reform, environmental regulation, antitrust policy, labor markets, minority faculty representation, and gender issues in labor markets.

JOURNAL OF ECONOMIC LITERATURE

The *Journal of Economic Literature* (*JEL*), another publication of the Association, offers articles that describe and summarize research in various economic fields:

> The *Journal*'s purpose is to help economists keep up with the ever-increasing volume of economics research. This goal is effected by publishing survey articles and essays, book reviews, and an extensive bibliographic guide to the contents of current economics periodicals. (Editor's Note 2004)

According to *JEL* policy, articles are commissioned by the editors, as are book reviews. *JEL* editors, therefore, have far more discretion than do regular *AER* editors in choosing topics and authors. They are also free to

[15] Apparently, this is not customary. In the five years prior to 2004, only one president-elect had a paper in the May issue. But all presidents-elect get their moments in the sun at the next annual meeting, when, as president, they address the membership. This presidential address is published in the *AER* as the lead article the following March.

choose which books to review and who should review them. For example, the *Journal* reviews only about 10 percent of the books sent by publishers. *JEL* editors do, however, invite proposals and use referees for some manuscripts.

JEL Editors: 5 to 0

Of the 46 *JEL* editors (including the editor, associate editors, and editorial board members)[16] who served sometime between 2001 and 2004, 38 had U.S. affiliations.[17] Five of the 38 editors gave an average of $640 to Democrats, for a contributor rate of 13.2 percent. None gave to Republicans.

JEL Referees: 16 to 0

Again, the *JEL* sometimes uses outside referees. To some extent, referees could limit an editor's freedom to select papers. Thus, we might expect some editors to rely on referees who share their political sensibilities. This affinity should show up in campaign contributions by referees. The editor's annual report thanks referees by listing their first and last names but without affiliations (McMillan 2002, 507; 2003, 501; 2004, 517; Gordon 2005, 500). Because no affiliations were provided, I first checked each referee's name against the AEA online directory to rule out those with foreign affiliations. If the individual was not listed in the directory, I searched the web to find the affiliation. Excluding those with foreign affiliations cut the number by about one third, leaving 155 referees with U.S. affiliations. Sixteen, or 10.3 percent, gave an average of $1,338 to Democrats. None gave to Republicans.

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[16] An associate editor serves as what the other journals refer to as managing editor.

[17] I assume a lag here that is one year shorter than for the regular *AER* issues. Although *JEL* editors pick authors, they also invite proposals, and some submissions are refereed. All this takes time, so an article appearing in 2003 may have been conceived by an editor or first presented to an editor in 2001.

JEL Authors: 24 to 0

Since *JEL* editors have wider discretion in selecting authors than do regular *AER* editors, and since *JEL* editors have a higher Democrat contributor rate, *JEL* editors might be more inclined than *AER* editors to select authors who also gave to Democrats and less inclined to select authors who gave to Republicans. In 2003 and 2004, a total of 292 authors with U.S. affiliations published articles or book reviews. Twenty-four gave to Democrats, for a contributor rate of 8.2 percent.[18] None gave to Republicans. The 24 *JEL* authors gave an average of $1,279 to Democrats. Topics addressed by Democrat contributors include the effects of globalization, economic development, growth divergence across countries, world poverty, Russia in transition, the soft budget constraint, law in transitional economies, international labor markets, business strategy, barter economies, political economy, the commons, technological innovation, electronic markets, the Civil War, slavery, higher education, faculty diversity, and school choice.

JOURNAL OF ECONOMIC PERSPECTIVES

The third publication by the AEA is the *Journal of Economic Perspectives* (*JEP*). According to its editor,

> The *Journal* seeks to contribute to the economic profession in a number of ways: introducing readers to state-of-the-art thinking on theoretical and empirical research topics; encouraging cross-fertilization of ideas among the fields of economics; providing analyses of public policy issues; providing readings for students; offering illustrations that are useful in lecture; sparking discussions among colleagues; suggesting directions for future research; and analyzing features of the economics profession itself. (Shleifer 2004, 518)

[18] Four of the 45 authors of articles gave to Democrats, as did 20 of the 247 authors of book reviews. Thus 8.9 percent of article authors and 8.1 percent of review authors gave to Democrats.

To those ends, the *Journal* commissions and publishes individual articles in addition to symposia on special topics, such as Political Economy, Cultural Economics, The Middle East, Activist Antitrust, and Global Poverty Reduction—some topics covered in 2003 and 2004 symposia. Such topics by definition would seem to call for a range of perspectives.

JEP Editors: 2 to 0

According to the *JEP*'s "Statement of Purpose," "Articles appearing in the journal are normally solicited by the editors and associate editors" (2004, ii). So *JEP* editors get to pick their topics and authors.[19] The *JEP* apparently does not use outside referees, as the editor made no mention of them in annual reports going back five years. A total of 25 editors (including the editor, co-editors, associate editors, and managing editor) served sometime between 2001 and 2004,[20] all with U.S. affiliations. Two contributed an average of $600 to Democrats, for a contributor rate of 8.0 percent. None gave to Republicans.

JEP Advisory Board Members: 12 to 0

Although there were no referees, the *JEP* does have a 12-member advisory board listed prominently at the beginning of each issue. Editors presumably consult board members when casting about for issues and authors. Of the 23 members to serve on that board sometime between 2001 and 2004, all but one had U.S. affiliations. Twelve of the 22 U.S. board members gave to Democrats, for a contributor rate of 54.5 percent. None gave to Republicans. Advisors gave an average of $2,146 to Democrats. How much influence the advisory board actually exerts remains unclear, but with only a dozen members, the group is at least small enough to operate

[19] According to the editor, the *JEP* also receives 150 to 200 unsolicited proposals a year, but only "a handful" ultimately get published in the *Journal* (Shleifer 2004, 518).
[20] As with the *JEL*, I assume a publication lag for the *JEP* that is one year shorter on average than for the regular *AER*. The manuscript stage at the *JEP* is no doubt shorter than at the *AER* , but more up-front time is required to identify topics, line up authors, and arrange symposia.

effectively if advisors choose to do so.[21] Democrat contributors on the board would also seem to have little difficulty mustering majority support for suggestions or recommendations to the editors.

JEP **Authors: 20 to 1**

JEP editors had more discretion in choosing authors than did *AER* editors. And, to the extent the advisory board had political say, that say would likely lean Democratic. *JEP* editors, therefore, might be more able and more inclined than *AER* editors to select authors who share the political outlook of some editors and most advisors as reflected by campaign contributions. Of 148 U.S. authors of symposia, articles, or features appearing in 2003 and 2004, 20 gave to Democrats, for a contributor rate of 13.5 percent. Democrat contributions averaged $995. One author gave $2,000 to Republicans, for a contributor rate of 0.7 percent. With author contributions favoring Democrats by 20 to 1, we might question whether the *Journal of Economic Perspectives* had enough "economic perspectives" in 2003 and 2004, especially for a journal with a public-policy focus. Topics addressed by Democrat contributors include globalization, Middle East policy, monetary policy, consumer behavior, consumer confidence, cost-of-living index, alternative minimum tax, welfare policy, antitrust enforcement, political economy of voting, incentive pay contracts, gender issues, and academic labor markets.

AUTHOR ACKNOWLEDGEMENTS

Acknowledgees: 11 to 0

Most authors typically thank or acknowledge colleagues who help shape the manuscript. I compiled a list of those acknowledged in the three discretionary journals—*P&P*, *JEL*, and *JEP*—in 2003 and 2004. There were 828 distinct individuals acknowledged, which breaks down into 731 who

[21] C. Northcote Parkinson argued that once a committee exceeds 20 people, it becomes dysfunctional. For a discussion of group size and policy effectiveness see McEachern (1987, 56).

were acknowledged once, 61 acknowledged twice, and 36 acknowledged three or more times. To keep the task of tracking campaign contributions manageable, I limited the investigation to the 97 acknowledged two or more times. Because an individual is usually acknowledged by name but not affiliation, I used the AEA online directory as a first pass to rule out those with foreign affiliations. If the individual was not in the directory, I searched the web. Of the 61 acknowledged twice, 5 with foreign affiliation were excluded. Among the 56 two-timers with U.S. affiliations, seven, or 12.5 percent, contributed an average of $1,357 to Democrats. None contributed to Republicans. Thirty-four of the 36 acknowledged 3+ times had U.S. affiliations. Four, or 11.7 percent, contributed an average of $1,350 to Democrats. None contributed to Republicans.

The 34 thanked three or more times are ranked in Table 1 by the number of times each was acknowledged (in parentheses). Six of the seven with double-digit acknowledgements were *JEP* editors. Because *JEP* editors have much discretion in commissioning pieces and because the journal does not use outside referees, the editors figure prominently in conceiving and shaping manuscripts and are acknowledged accordingly. Timothy Taylor, the *JEP* managing editor, received the most—57, or 60 percent of *JEP* publications during 2003 and 2004. Co-editor Michael Waldman was not far behind with 44 acknowledgements.[22] John McMillan, who ranked fifth, was the only non-*JEP* editor among those in double digits. He edited the *JEL*.

At each stage of the publication process, Democrat contributors dominate Republican contributors, especially among the three discretionary journals—from the author, to those whose comments helped shape the manuscript, to the referees who evaluate the manuscript, to the editor who accepts it for publication. Since the editor is the critical link in the publication chain, editorial appointments deserve a closer look.

[22] These totals exclude blanket thanks to "the editors." Incidentally, *JEP* top editor Alan Krueger and later Andre Shleifer each had three or more acknowledgements not counting those from *JEP* authors. The names of all those acknowledged even once, including those with foreign affiliations, along with the journal in which they were acknowledged can be found in Appendix 1.

Table 1
Rank of Those Acknowledged Three or More Times in the *AER P&P*, *JEL*, and *JEP* 2003 & 2004(Total Acknowledgements in Parentheses)

1. Timothy Taylor (57)	13. Jesse Shapiro (4)
2. Michael Waldman (44)	19. Daron Acemoglu (3)
3. Andrei Shleifer (28)	19. John Caskey (3)
4. Brad DeLong (18)	19. Frank Diebold (3)
5. John McMillan (17)	19. Ron Ehrenberg (3)
6. James Hines Jr. (16)	19. Robert Gibbons (3)
7. Alan Krueger (11)	19. Edward Glaeser (3)
8. Olivier Blanchard (7)	19. Bengt Holstrom (3)
9. Lawrence Katz (6)	19. Michael Kremer (3)
10. Gary Becker (5)	19. David Laibson (3)
10. Joshua Hausman (5)	19. Ellen Magenheim (3)
10. Richard Posner (5)	19. Pablo Montagnes (3)
13. Angus Deaton (4)	19. John Siegfried (3)
13. Mihir Desai (4)	19. Kent Smetters (3)
13. Daniel Hamermesh (4)	19. Lawrence Summers (3)
13. Ben McCallum (4)	19. David Wilcox (3)
13. Sam Peltzman (4)	19. Janet Yellen (3)

COMMITTEES, PRESIDENTS, AND TOP EDITORS

How does someone become an editor? When a vacancy is expected at the top of one of the Association's journals, the president appoints an ad hoc search committee to recommend a replacement to the executive committee, which consists of the dozen elected AEA officers (a group to be examined shortly). All other editorial positions are filled by the top editor, subject to committee approval.[23] Thus, the ad hoc search committee, in finding a top editor, still plays the pivotal role in the publication.

[23] The executive committee in 2003 approved a measure to exercise more ongoing oversight over the journals by having the president select from the executive committee a four-member advisory committee on editorial appointments. This advisory committee oversees any *reappointment* of a top editor and all editorial appointments below that of top editor

Ad Hoc Search Committee Members: 7 to 1

I examine campaign contributions by members of the four search committees appointed since 2000.[24] An ad hoc search committee was appointed in 2000 to replace the *AER* editor, who had served since 1985. Two of its seven members gave an average of $1,253 to Democrats in the 2004 election cycle, and the committee chair gave $2,000 to Republicans. The new editor was appointed in 2001 ("Minutes" 2001, 469). In September 2003, that editor announced he would not seek a second three-year term because he joined the Federal Reserve Board (he later briefly headed the President's Council of Economic Advisors before being named to replace Alan Greenspan as chairman of the Federal Reserve). A search committee was appointed in late 2003 to find a replacement. Two of its 10 members gave an average of $1,625 to Democrats. None gave to Republicans. The editor found through that search gave $600 to Democrats in the 2004 election cycle. Thus, the first search committee, chaired by a Republican contributor, helped find an *AER* editor who would later head the Council of Economic Advisors for a Republican president and then be appointed to head the Fed. The second search committee, with two of 10 members giving to Democrats and none to Republicans, helped find an *AER* editor who also contributed to Democrats.

A search committee for a new *JEP* editor was appointed in 2001 and reported in 2002 ("Minutes" 2002, 488). One of five committee members, the chair, contributed $250 to Democrats. None gave to Republicans. A *JEL* search committee was appointed in 2003 and reported in 2004 ("Minutes" 2004, 488). Of its nine members, two, including the chair, gave an average of $700 to Democrats. None gave to Republicans. Thus, of the 31 named to the four ad hoc search committees appointed since 2000, seven, or 22.6 percent, contributed an average of $1,058 to Democrats. One appointee, or 3.2 percent, contributed $2,000 to Republicans.

(Minutes 2003, 476-77). Of the four appointed to this advisory committee, one, the 2003 chair, gave $1,250 to Democrats. None gave to Republicans. The president still appoints an ad hoc search committee to help fill an opening for top editor, but two search committee members are appointed from the ranks of the advisory committee on editorial appointments.

[24] Prior to 2000 the most recent search committee was appointed in 1997, and prior to that in 1995.

Nominating Committee Members: 4 to 0

I'll skip to the nominating committees, which help select presidents-elect, a process to be treated shortly. These committees are appointed by the president. The five presidents-elect serving between 2000 and 2004 were identified by five nominating committees with six to eight members each. Four of the five committees each had one member who gave to Democrats. No member gave to Republicans. Of the 37 members appointed to the five nominating committees, four, or 10.8 percent, gave an average of $963 to Democrats. The one nominating committee with no contributors to either party proposed the 2003 president-elect/2004 president, who was a Republican donor.

Presidents: 5 to 2

In light of the central role that presidents play in organizing annual meetings, in appointing ad hoc search committees to find top editors, in appointing a nominating committee to help pick the next president and identify candidates for other offices, and in appointing other committees, we should have a special interest in their political contributions. Of the 23 living AEA presidents as of January 2005 (including the 2005 president and the 2005 president-elect, who will become the 2006 president), five, or 21.7 percent, gave to Democrats in the 2004 election cycle, and two, or 8.7 percent, gave to Republicans. Democrat contributions averaged $1,140 and Republican contributions $2,500. Contributing to Democrats in the 2004 election cycle were the presidents serving in 1961, 1973, 1981, 1986, and 2006. Contributing to Republicans were presidents serving in 1967 and 2004.

Former or Current Top Editors: 5 to 0

I have already profiled campaign contributions from all editors and editorial board members serving in recent years, but what about all former or current top editors? Among the 15 top editors of the *AER*, *JEL*, or *JEP* still living as of January 2005, five, or 33.3 percent, gave an average of $3,820 to Democrats in the 2004 election cycle. None gave to Republicans.

Two of the six top *AER* editors averaged $1,300, and three of the five top *JEP* editors averaged $5,500.

AEA "Democracy"

A search committee recommends a new editor, but the president appoints the committee. Since the president is elected, the membership would seem to have ultimate control over editorial appointments. Members delegate that control to the president, who delegates it to a search committee, which makes a recommendation to the executive committee. This principal-agent story, however, is misleading. Technically, AEA members elect the president, but that's a formality. One of the first responsibilities of a president-elect is to appoint a nominating committee to come up with a slate of candidates for the next election. The nominating committee proposes "at least two names" for president-elect and may add to one of them a recommendation to the executive committee. Acting jointly as an "electoral college," the nominating committee and the executive committee[25] together nominate one candidate to go on the ballot sent to the membership. At this point the result becomes a fait accompli.[26] Thus the nominating committee helps find the president-elect, who appoints a nominating committee to repeat the process. There is no bottom-up input from the general membership. In a proximate sense, it is only the small group of organization elites who determine the president.

Each year, five executive committee slots become open: president-elect, the two vice-presidents, and two other elected members. As for the latter four slots, the nominating committee proposes two candidates for each slot. Once approved by the executive committee, names of the two candidates also go on the ballot sent to the membership. At this level, the general membership has democratic input into the determination of leadership: a choice between two executive-committee candidates who have been handpicked by the existing leaders.

[25] The executive committee has twelve voting members: the president, the president-elect, the two immediate past presidents, two vice-presidents, and six other elected members.
[26] The process may be something of a fait accompli even at the nominating committee's recommendation stage. In 2005 only the nominating committee chair attended the executive committee meeting; the other members sent their proxies, noting that the nominating committee had "reached unanimous agreement about the candidates they proposed for president elect" ("Minutes" April 22, 2005). In other words, there was nothing to discuss.

According to AEA Bylaws, the nominating committee must be chaired by a former AEA officer and must have no fewer than five other Association members. The practice has been to appoint a past AEA president to chair. Thus, the nominating committee chaired by a former president helps pick the president-elect, who then appoints the next nominating committee chaired by a former president to help pick the next president-elect. The vote for president-elect is insular and self-perpetuating, with no real member involvement short of petitioning the membership.[27]

LOOKING INTO 2006

All that brings us up to 2004, but I want to push this line of inquiry one year further. No member of the nominating committee appointed by the 2004 president/Republican donor gave to either party. Of the five new AEA officers resulting from that nominating committee's efforts, however, three gave to Democrats. None gave to Republicans. The 2005 president-elect, who will become the 2006 president, gave $2,500 to Democrats. Two other new officers averaged $750 to Democrats. Thus three of these new officers contributed to Democrats in the 2004 election cycle (and none gave to Republicans).

Executive Committee Members: 4 to 2

The 2005 additions to the executive committee represent a jump in Democrat contributors. To put this in perspective, consider that of the 30 members to fill the 12 executive committee slots sometime between 2000 and 2004, four gave an average of $1,225 to Democrats, and two, including the 2004 president, gave an average of $2,500 to Republicans. But three of the five new officers elected for 2005 gave to Democrats and none to Republicans. Recall that the 2004 president had no apparent effect on the contributor profile of the *Papers* & *Proceedings* beyond his own presence on

[27] There are provisions for an AEA member to get on the nominating committee by securing signatures from two percent of the membership, or about 370 signatures. Later in the process, nominees can be added to the ballot by securing signatures from six percent of the membership for president-elect and four percent for other elected positions. No petition could propose a slate of officers.

the planning committee and among published authors. Ironically, as a result of the nominating committee appointed by this Republican contributor, Democrat contributors on the 12-person executive committee doubled from two in 2004 to four in 2005. The 2004 president was the only Republican contributor on the board in 2004 and 2005.

Finally, the Association's publication footprint is growing. The executive committee has asked the *AER* editor to expand the publication by about 100 pages each year through 2007 or 2008. As noted earlier, the current top *AER* editor contributed to Democrats in the 2004 election. His term began with the September 2004 issue. Over the next two issues, the number of co-editors increased from five to eight. Two of the three additional co-editors gave an average of $750 to Democrats; none gave to Republicans. The AEA president in January 2005, who contributed to neither party, appointed an ad hoc committee on journals to consider introducing some specialized field journals sponsored by the AEA (Minutes January 6, 2005). Two of the president's six choices gave an average of $750 to Democrats; none gave to Republicans.

SUMMARY OF FINDINGS

One way of summarizing the findings is by showing those populations with no Republican contributors, those populations with one Republican contributor, and those populations with two Republican contributors, as is done in Tables 1, 2, and 3. These tables include all samples (aside from my small out-of-time-period digressions). Among the entire eligible set listed in the three tables, the overall tally is 182 Democrat contributors to 10 Republican contributors. Democrat contributors filled 182 of a possible 1,583 slots, or 11.5 percent. Republican contributors filled 10, or 0.6 percent. Incidentally, four of the 10 Republican slots were filled by the 2004 president, first as president-elect serving on the *P&P* program committee, second as a *P&P* author, third as president, and fourth as an executive committee member. Four of the remaining six Republican slots were filled by two individuals, one who authored two regular *AER* papers and another who served both on the executive committee and on an ad hoc search committee.

Table 2: Subsample with No Republican Giver

Of this population,	*None gave to Republicans and . . .*
84 *AER* editorial officers	9 gave to Democrats
38 *JEL* editorial officers	5 gave to Democrats
155 *JEL* referees	16 gave to Democrats
292 *JEL* authors	24 gave to Democrats
25 *JEP* editorial officers	2 gave to Democrats
22 *JEP* advisory board members	12 gave to Democrats
90 acknowledged 2+ in *P&P*, *JEL*, or *JEP*	11 gave to Democrats
37 nominating committee members	4 gave to Democrats
15 former or current top editors	5 gave to Democrats
758 Total	**88 gave to Democrats, or 11.6 percent**

Table 3: Subsample with One Republican Giver

Of this population,	*Just one gave to Republicans and . . .*
37 AEA program committee members	7 gave to Democrats
289 *P&P* authors	32 gave to Democrats
148 *JEP* authors	20 gave to Democrats
31 ad hoc search committee members	7 gave to Democrats
505 Total	**66 gave to Democrats, or 13.1 percent**

Table 4: Subsample with Two Republican Givers

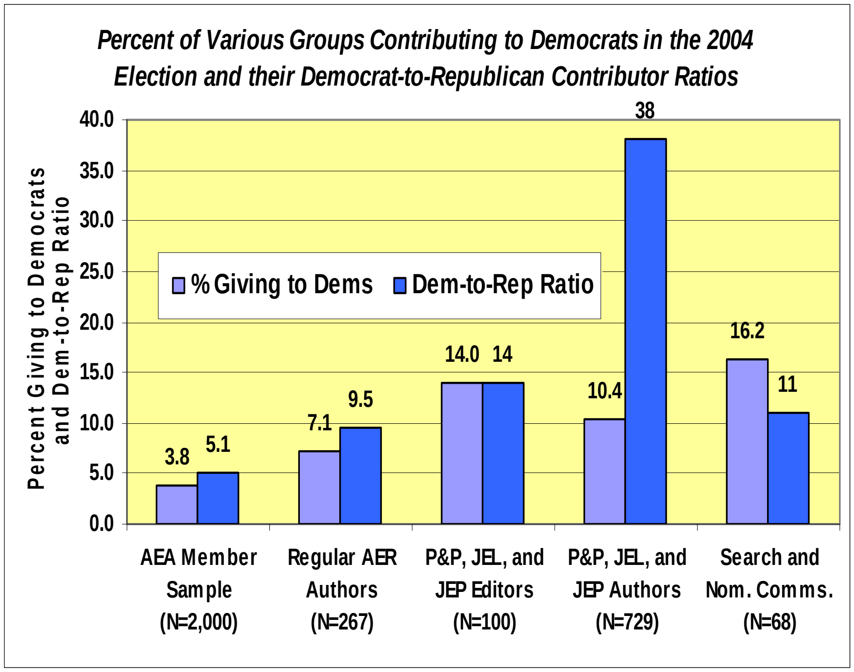
Of this population,	*Two gave to Republicans and . . .*
267 regular *AER* authors	19 gave to Democrats
23 AEA presidents	5 gave to Democrats
30 executive committee members	4 gave to Democrats
320 Total	**28 gave to Democrats, or 8.8 percent**

For the 2,000 AEA member sample, the ratio of Democrat-to-Republican donors was 5.1 to 1. For *AER* authors, it was 9.5 to 1, or nearly twice as large. The *P&P* program committee members and the *JEL* and *JEP* editors all had more discretion in selecting authors than did regular *AER* editors and also had a higher average Democrat contributor rate. Their wider discretion and greater willingness to contribute to Democrats suggest they might be more inclined than *AER* editors to pick authors who share the same political sensibilities as reflected by campaign contributions. In the *P&P*, *JEL*, and *JEP*, there were a total of 729 authors with U.S. affiliations, 76 of whom gave to Democrats and only 2 to Republicans, for a contributor ratio of 38 to 1, or four times that of regular *AER* authors and more than seven times that of AEA members. The Democrat contributor rate among these authors was 10.4 percent versus 7.1 percent among regular *AER* authors and 3.8 percent among the sample of AEA members. The Republican contributor rate of 0.3 percent for authors in the discretionary journals was less than half the 0.7 percent among regular *AER* authors and AEA members.

Figure 1 summarizes the Democrat contributor rates and the Democrat-to-Republican contributor ratios for those groups with at least one Republican contributor. This figure provides visual representation of the important finding that the Democrat-to-Republican imbalance increases, first, as we go from general membership to authors needed to satisfy regular *AER* editors and referees, and again sharply increases as authors need to satisfy AEA editors with greater editorial discretion. It seems that birds of a feather do flock together. A contributor ratio of 38 to 1 among the discretionary journal authors poses circumstantial evidence challenging the claim that the Association is "the organ of no party," represents "people of all shades of economic opinion," and that "widely different issues are given a hearing in its annual meetings and through its publication."

Committees that search for top editors and committees that nominate executive officers combine for a contributor rate of 16.2 percent, the highest among the groups shown. Ratios for some populations could not be shown because these groups had no Republican contributors. Still, I'll repeat that Democrat contributors accounted for 10.7 percent of regular *AER* editors, 10.3 percent of *JEL* referees, 12.2 percent of those acknowledged more than once in the three discretionary journals, 33.3 percent of former or current top editors of the *AER*, *JEL*, and *JEP*, and 54.5 percent of *JEP* advisory board members.

Figure 1



ARE THE RATIOS TELLING?

For several reasons, the Democrat-to-Republican contributor ratios may exaggerate the influence of Democrats among AEA leadership and publications. Steven Levitt (1994) found that additional campaign spending has little impact on who wins. Most economists are likely aware that minor campaign contributions have little chance of affecting the outcome, and presumably contribute because they derive utility from political expression and political solidarity. If the utility of political expression is less for Republican than for Democrat economists, we would expect a lopsided ratio even if AEA personnel were not lopsided. The resulting ratio would exaggerate Democrat dominance of the groups observed.

Also, perhaps those with Republican leanings are reluctant to publicize their views with campaign contributions. Being identified as a Republican may not be a good career move in academia. Using a random survey of 1,643 faculty from 183 four-year institutions, Rothman, Lichter, and Nevitte find that even after accounting for the effects of individual characteristics and scholarly achievements, Republicans teach at lower quality institutions than do Democrats (2005, 12). Surveying six scholarly associations, Klein and Stern (2005b) show at the 0.01 significance level that Republican scholars are more likely to have landed outside of academia. If Republicans are less inclined than Democrats to reveal their political preferences by contributing $200 or more, an act that becomes part of the public record, then the Democrat-to-Republican contributor ratio overstates the underlying sentiments of the two groups.

As a way of untangling the possibility of greater Democrat solidarity from Republican reluctance to self-identify, I looked at campaign contributions among groups that otherwise identify their political preferences. The first sample draws on voter registration information among economists at 11 California universities collected by Daniel Klein and several colleagues.[28] Since registering as a Democrat or a Republican becomes public information at least in the local community, such economists could be viewed as self-identified partisans. Among the 84 economists registered as Democrats, 17, or 20.2 percent, gave an average of $3,253 to Democrats (and none gave to Republicans). Among the 30 economists registered as Republicans, two, or 6.7 percent, gave an average of $2,000 to Republicans (and none gave to Democrats). Thus, among this sample of academic economists, registered Democrats gave at 3.0 times the contributor rate as registered Republicans.

Another group of economists to publicly identify their political preferences are appointees to the President's Council of Economic Advisors. By accepting appointments, these economists were implicitly expressing at least some support for the economic policies of the president who appointed them. Seven of the 10 appointed by President Clinton gave an average of $1,743 to Democrats in the 2004 election cycle, for a contributor rate of 70 percent (none gave to Republicans). Only three of the 11 appointed in total by Presidents George H.W. Bush during his only term and by George W. Bush during his first term gave an average of $2,000 to Republicans, for a contributor rate of 27.3 percent (one gave to

[28] The data are the same used by Klein (2006, 202, Appendix 1).

Democrats[29]). Among this group of self-identified partisans, Clinton-appointed CEA members gave at 2.8 times the contributor rate as Bush-appointed CEA members.

A third group of self-identified partisans emerged during the 2004 campaign. A month before the 2004 election, on the eve of the second presidential debate, 169 business school academics signed and published a letter to President Bush saying "As professors of economics and business, we are concerned that U.S. policy has taken a dangerous turn under your stewardship. Nearly every major economic indicator has deteriorated since you took office in January 2001" (Open Letter 2004). Although the letter makes no mention of Senator Kerry, the timing and content leave no doubt whom the professors support. Forty-three of the 169 gave an average of $2,417 to Democrats in the 2004 election cycle, for a contributor rate of 25.4 percent (none gave to Republicans). One week after the anti-Bush letter, 368 mostly academic economists signed a letter critical of Senator Kerry's economic plan: "We, the undersigned, strongly oppose key aspects of the economic agenda that John Kerry has offered in his bid for the U.S. presidency…. All in all, John Kerry favors economic policies that, if implemented, would lead to bigger and more intrusive government and a lower standard of living for the American people" (Letter 2004). The letter was released by the "Bush-Cheney '04" campaign. These economists were obviously willing to publicly declare their views. Thirty-three of the 368 gave an average of $1,405 to Republicans in the 2004 election cycle, for a contributor rate of 9.0 percent (none gave to Democrats). Thus, the contributor rate among the open Democrats was 2.8 times that of the open Republicans.

For these three groups of economists—registered partisan voters, CEA appointees, and letter signers—self-identified Democrats contributed at a rate that averaged 2.8 times that of self-identified Republicans. If these results carry over to partisan economists more generally, then relying on campaign contributions could underestimate Republican partisanship measured by party registration, CEA appointment, or letter signing. If campaign contributions underestimate other measures of Republican partisanship among economist by a factor of 2.8, then the Democrat-to-Republican partisanship ratio among discretionary journal authors, instead of being 38 to 1, would be more like 14 to 1. But, of course, the benchmark contributor ratio among AEA members would also have to be reduced as

[29] One member appointed by the first President Bush gave $500 to Democrats (and did not give to Republicans) in the 2004 election.

well, from 5.1 to 1 to 1.8 to 1, so the relationship between the discretionary journal authors and the member sample would not change. Authors in discretionary journals would still have a Democrat-to-Republican contributor ratio that is 7.5 times greater than that of the membership sample.

Another reason why the Democrat-to-Republican contributor ratios in the 2004 election cycle may exaggerate the influence of Democrats on the Association is that 2004 may not have been a typical election. Some Republican economists may have sat out the election because they disagreed with President Bush on any number of issues, including the Iraq war, the Patriot Act, federal deficits, Medicare prescription drug coverage, the nationalization of airport security, stem-cell research, immigration policy, farm subsidies, and steel tariffs. At the same time, some Democratic economists may have been especially mobilized against President Bush and his policies. Thus, the 2004 campaign may have exaggerated what the Democrat-to-Republican contributor ratios would look like in a more typical election. But the polarity of the 2004 election should also be reflected in the contributor ratio among AEA members, so a comparison of contributions in the discretionary journals with member contributions should still be telling.

Hence there are some reasons to believe that the contributor ratios may overstate the eclipse of Republicans measured in other ways. But these reasons do not challenge the finding that authors in the discretionary journals had a Democrat-to-Republican contributor ratio that was 4 times that of regular *AER* authors and 7.5 times that of the AEA member sample. One could also argue that contributors of $200 or more are making at least as strong a political statement as someone who registers with one party, signs a letter, or even accepts a CEA appointment.

After making the rough adjustments, when all the qualifications are in, including the results of surveys by others, we have the following plain facts: the AEA is a predominately Democratic organization. Those responsible for the journals are especially Democratic, and they run the journals in a manner that tends to reflect that particular ideology.

Are AEA members representative of academic economics more generally? According to a 1996 survey of academic economists, 55 percent belonged to the AEA.[30] Klein (2006, 198) provides data on rates of AEA membership by party registration for the faculty from the 11 California

[30] According to a table on the AEA website, 55 percent of the 7,704 academics surveyed in 1996 were AEA members.

schools. The sample of Republicans is so small as to render the evidence anecdotal, but the numbers do so show Democrats as having higher rates of membership than Republicans.

Finally, this study has not asked why Democrats dominate political contributions among those involved with the AEA journals. Nearly all of the editors, advisors, authors, reviewers and those acknowledged in the Association's three journals are from academia and academics generally lean Democrat. Based on contributions of $200 or more from 1999 through 2004, college faculty across disciplines had a Democrat-to-Republican contributor ratio of about 8 to 1.[31] But recall that in the AEA member sample, nonacademics had a higher Democrat-to-Republican contributor ratio than did academics, so we can't necessarily trace Democrat domination of AEA publications to the academic ties of those involved.

ECONOMICS THROUGH DEMOCRAT LENSES

What's the harm of having extremely high Democrat-to-Republican contribution ratios among those involved with AEA publications, especially among the discretionary journals? The Association recognized the possible harm more than 80 years ago when the Certificate of Incorporation called for "perfect freedom of economic discussion." Recall that campaign contributors are also more likely to be politically engaged in other ways. We should not expect editors, referees, authors, reviewers, and acknowledgees who have contributed to campaigns to just turn off that mindset in their dealings with the Association's publications.

As an example of possible harm of a lopsided political representation, consider the absence of a Republican contributor among the 247 book reviewers with U.S. affiliations appearing in the *Journal of Economic Literature* in 2003 and 2004. A *JEL* review will likely be the most visible, if not the only, review some books will ever receive. Couldn't the same political sensibilities that motivated a reviewer to contribute to Democrats also shape his or her assessment of a book? As economists, we like to think we

[31] By way of comparison, journalists gave to Democrats by a ratio of about 4.5 to 1, trial lawyers gave to Democrats by a ratio of about 7 to 1 (Campaign Finance in American Politics 2005), and faculty at the nation's top twenty law schools gave at a ratio 5.4 to 1 (McGinnis, Schwartz, and Tisdell forthcoming).

are above political bias even though we are usually the first to examine the personal motives of others. Any book author realizes that an editor's decision about whether to review a book and who should review it is something of a crapshoot. But loading the dice, however unintentionally, with 20 Democrat contributors and no Republican contributors seems unfair to some authors and unhealthy for the profession. As mentioned earlier, some topics addressed in books reviewed by the 20 Democrat contributors include the effects of globalization, economic development, world poverty, transitional economies, international labor markets, higher education, faculty diversity, and school choice.

Mark Bauerlein, a professor of English at Emory University and research director at the National Endowment for the Arts, has argued that:

> Any political position that dominates an institution without dissent deteriorates into smugness, complacency and blandness. . . . Groupthink is an anti-intellectual condition, ironically seductive in that the more one feels at ease with compatriots, the more one's mind narrows (2004).

During the appointment of the current *AER* editor, who took over with the September 2004 issue, the AEA executive committee ventured an opinion about the "diversity and openness" of its editors. According to the minutes,

> There followed a brief discussion during which it was recognized that diversity and openness is best promoted through editors who individually are open to different viewpoints rather than building balance through a portfolio of editors and co-editors who hold less compromising views (Minutes 2005, 464).

Thus, the committee favors choosing editors who "individually are open to different viewpoints" rather than trying to achieve a balance through diversity of views across individual editors. Based at least on the metric of campaign contributions, the appointment of the current *AER* editor and, indeed, editorial appointments in general, reflect neither approach. Recall that the current *AER* editor contributed $600 to Democrats in the 2004 election cycle, and two of the three co-editors he added over the next two issues contributed an average $750 to Democrats.

More generally, none of the 147 AEA editors or editorial board members serving in the last few years gave to Republicans in the 2004 election cycle, though 16 gave to Democrats.

The AEA claims to be the "organ of no party." That is, of course, true *de jure*, but contributor ratios that favor Democrats 9.5 to 1 among regular *AER* authors and 38 to 1 among authors in remaining publications at least raise a question whether the Association is *de facto* an "organ of no party." The AEA is an influential and powerful organization. It is expanding the *AER* and is considering some specialized field journals. One third of the six appointees to the ad hoc committee now exploring the field journals gave an average of $750 to Democrats; none gave to Republicans.

Recent Nobel prize winner Thomas Schelling noted in a committee report that the Association's three journals "officially represent the scholarly profession; their policies and procedures determine what gets published in them; and what gets published strongly influences the image of economics in America" (Shelling 2000, 528). What gets published also directly influences who gets hired, promoted, and tenured. The AEA is of course central to the legitimation of economic ideas and opinions. To the extent that editors, referees, reviewers, and program committee members, at the margin, favor the political sensibilities expressed by authors who contribute to Democrats, this extends any political bias in these journals to systems that hire, promote, tenure, and pay economists based on the decisions rendered by these eminent journals. The entire process becomes self-referential and self-reinforcing.

One parting remark: as time goes by, research on campaign contributions could suffer from the Heisenberg principle. Focusing on campaign contributions could alter contributor behavior. If Democrat-to-Republican contributor ratios become relevant for policy consideration, then some economists will simply stop contributing. As a result, campaign contributions will become a less reliable indicator of ideological orientation.

Appendix 1:
Link to Excel file listing contributions from each AEA group (with names redacted) plus the names of all acknowledged in *P&P*, *JEL*, or *JEP*.

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CHARACTER ISSUES

Sense and Sensibilities: Myrdal's Plea for Self-Disclosure and Some Disclosures on AEA Members

DANIEL B. KLEIN[*]

I confess that I am one of those who think that . . .

—Frédéric Bastiat (1964, 12)

Abstract, Keywords, JEL Codes

IN THE PRESENT ISSUE OF THIS JOURNAL, WILLIAM MCEACHERN (2006) writes about the campaign contributions of individuals involved in various ways in the American Economic Association (AEA). Those results supplement survey findings (Klein and Stern 2006a, 2006b) to give a picture of the ideological character of the AEA as an organization. Comparative rates of AEA membership by party registration help to clarify the issue.

In private communications, anonymous referee reports, and other murmurs, people have conveyed to me that they find these investigations to be inappropriate. It's disrespectful to pry into the personal information of fellow economists. It's irrelevant whether economists vote one way or the other or hold certain personal opinions. If you want to challenge economic research, analyze the research, not the researcher.

But the Nobel-prize winning economist and leading social democrat Gunnar Myrdal probably would have deemed such investigation to be appropriate. Myrdal pleaded for candor and openness about who we are. In

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I thank William McEachern for suggesting the investigation of AEA membership by voter-registration category, Christopher Cardiff for his care in collecting the bulk of the voter-registration data, and Niclas Berggren for valuable comments.

this article I discuss the three AEA investigations, but first address the appropriateness of such investigation.

MYRDAL'S PLEA

In his small book *Objectivity in Social Research* (1969), Myrdal explained that, like anybody else, an economist is a creature with values, perspectives, and purposes. Like anybody else, he has ways of interpreting the information he uses and deploys. An economist who takes on real issues necessarily makes many deep judgments—about what is important, what evidence and arguments deserve attention, what formulations illuminate the issue, and so on. These judgments reflect his moral and ideological sensibilities. The idea of doing important economics without deep-seated judgments and commitments is intellectually untenable. An economist who projects a voice about how the enterprise speaks to human kind—that is, an economist who fulfills the purpose of science—must exercise judgments that to some extent differentiate himself, not so much from non-economists, but from anyone, including other economists, with conflicting ideological sensibilities. Myrdal said that the notion of separating economics from ideology is folly or fraud—or both. He asked us to resist the pretense.

When ideological sensibilities are kept in the dark, it is more likely that ideological commitments warp discourse. Myrdal made a specific request: Whenever your ideological sensibilities might influence your behind-the-scenes judgments, you should tell the reader just who you are. You should tell the reader where you are coming from. Disclosing your sensibilities will improve the reader's ability to make sense of what you say. The reader is on the look out for where warping may have occurred. Also, such practice communicates openness and invites others to take up larger challenges against you. Maybe the listener would like to criticize the characterization you give of your sensibilities, and would be ready to relate that characterization to how you have conducted your research. You, the author, are giving your critic more to go on. You are more answerable and hence more alert to warping in your own work. That, said Myrdal, is our best way of respecting those with differing sensibilities. That is our best hope of bridging the sensibilities, and of refining and better justifying our own.

Myrdal's Own Words

Here they are (all italics are Myrdal's):

> In the course of actual day-to-day living, acting, thinking, and talking, a person will be found to focus attention on the valuations on one plane of his moral personality, while leaving in the shadows for the time being, the often conflicting valuations on other planes. The basis for this selective focusing is plainly opportunistic. (1969, 17)

> We are imperfect beings, and it is most of the higher valuations that are pushed into the shadows in everyday living. They are preserved for expression on occasions that are more ceremonial in nature or that in one way or another are isolated from daily life where the '*lower*' valuations more often predominate. (1969, 17)

> [V]aluations are 'objectified' by being presented as beliefs or simple inferences from beliefs—which implies hiding them and thereby also keeping their lack of consistency out of sight. Through this process beliefs become distorted. (1969, 18)

> A scientific scrutiny of popular beliefs shows not only that they are often wrong but also that they are twisted in a systematic way. It also shows blind spots of unnecessary ignorance and, on the other hand, an astonishing eagerness to acquire knowledge when it is opportune for the urge to rationalize. ¶ *All ignorance, like all knowledge, tends thus to be opportunistic.* Every educational effort aimed at correcting distorted beliefs in a society meets strong resistance. (1969, 18-19)

> Like people in general, social scientists are apt to conceal valuations and conflicts between valuations by stating their positions as if they were simply logical inferences from the facts. Since, like ordinary people, they suppress valuations *as valuations* and give only 'reasons,' their perception of reality easily becomes distorted, that is, biased. (1969, 50)

Biases are thus not confined to the practical and political conclusions drawn from research. They are much more deeply seated than that. They are the unfortunate results of concealed valuations that insinuate themselves into research at all stages, from its planning to its final presentation. As a result of their concealment, they are not properly sorted out and can thus be kept undefined and vague. (1969, 52)

[B]iases in social science cannot be erased simply by 'keeping to the facts' and refining the methods of dealing with statistical data. Indeed, data and the handling of data are often more susceptible to tendencies towards bias than is 'pure thought.' The chaos of possible data for research does not organize itself into systematic knowledge by mere observation. . . . If, in their attempts to be factual, scientists do not make their viewpoint explicit, they leave room for biases. (1969, 51)

Every student, as a private person and as a responsible citizen, is more or less entangled in the web of conflicting valuations that I discussed [previously]. Like the layman, the scientist is influenced by the psychological need for rationalizations. ¶ The same is true of every executive responsible for other people's research and of the popular and scientific public before which the scientist performs—and whose reactions he has opportunistic reasons to respect. The fact that his fellow scientists usually are conditioned in the same way strengthens the effect of the irrational influences. Generally speaking, we can observe that the scientists in any particular institutional and political setting move as a flock, reserving their controversies and particular originalities for matters that do not call in question the fundamental system of biases they share. (1969, 52-53)

The only way in which we can strive for 'objectivity' in theoretical analysis is to expose the valuations to full light,

> make them conscious, specific, and explicit, and permit them to determine the theoretical research. (1969, 55-56)
>
> I am arguing here that value premises should be made explicit so that research can aspire to the 'objective'—in the only sense this term can have in the social sciences. But we also need to specify them for the broader purposes of honesty, clarity, and conclusiveness in scientific inquiry. (1969, 56)
>
> [B]y insisting on the necessity of value premises in all research, the social sciences should be opened more effectively to moral criticism. . . . When these valuations have been brought out into the open, anyone who finds a particular piece of research to have been founded on what he considers wrong valuations can challenge it on that ground. He is also invited to remake the study and remodel its findings by substituting another, different set of value premises for the one utilized. (1969, 73-74)

Today, the Trend Is Myrdalian

Although Myrdal's lectures would have seemed mundane to Adam Smith and Isaiah Berlin,[1] in economics in 1969 they were ahead of their

[1] Smith (1790: 337): "Frankness and openness conciliate confidence. We trust the man who seems willing to trust us. . . . But this most delightful harmony cannot be obtained unless there is a free communication of sentiments and opinions. We all desire, upon this account, to feel how each other is affected, to penetrate into each other's bosoms, and to observe the sentiments and affections which really subsist there. The man who indulges us in this natural passion, who invites us into his heart, who, as it were, sets open the gates of his breast to us, seems to exercise a species of hospitality more delightful than any other. No man, who is in ordinary good temper, can fail of pleasing, if he has the courage to utter his real sentiments as he feels them, and because he feels them. It is this unreserved sincerity which renders even the prattle of a child agreeable."

Berlin (1969: 115-16): "[H]istorians [re: economists] [cannot] avoid the use of normal language with all its associations and 'built in' moral categories. To seek to avoid this is to adopt another moral outlook, not none at all. The time will come when men will wonder how this strange view, which combines a misunderstanding of relation of value to fact with cynicism disguised as stern impartiality, can ever have achieved such remarkable fame and influence and respectability. For it is not scientific; nor can its reputation be due entirely to a commendable fear of undue arrogance or philistinism or of too bland and uncritical an time and did not make a splash. However, during the 1980s the Myrdalian ethos busted onto the economics stage in part by virtue of the heroic works of Deirdre McCloskey (e.g., 1985). McCloskey reminded all of us that at the end of the day we were just regular humans, and that our humanness is essential to our scholarly discourse. McCloskey reminded us to ask ourselves what our scholarly effort was really all about. Her frankness and openness about herself, including her ideological sensibilities, and her openness's undeniable relevance to what she was saying, helped to make her basic points convincing. The economics profession took up an immediate fascination with McCloskey's ethos. Other prominent figures from other ideological quarters, such as Amartya Sen (1987), also project the Myrdalian ethos. It is my impression that, since 1990, younger economists have been more thoughtful about the moral, rhetorical, sociological, and ideological aspects of academic economics.

Today the trend of economic discourse is unmistakably Myrdalian. The Myrdalian trend is not manifest in the "top" journals. Rather, the trend is in the changing composition of "economic discourse." Increasingly it is led (in the United States) by Paul Krugman, Brad DeLong, Gary Becker, Tyler Cowen, and dozens of leading economist bloggers, columnists, and book-writers. Just as the successful merchant respects his customers, the successful blogger respects his readers. He makes plain where he is coming from.

The internet not only makes it easy to self-disclose, it makes it harder to self-conceal. In discussing the work of another researcher, bloggers are quick to link to that author's homepage. "What's his story?," we all want to know. Wikipedia (wikipedia.org) includes many entries on economists, and such entries usually directly tell of the subject's ideological sensibilities.

Yet another Myrdalian trend is the proliferation of think tanks, especially free-market think tanks, whose literature is often widely read and ideologically frank.

The new communication is more natural, more genuine. It may be presumed that young economists increasingly enter into the Myrdalian

imposition of our own dogmas and standards upon others. In part it is due to a genuine misunderstanding of the philosophical implications of the natural sciences, the great prestige of which has been misappropriated by many a fool and imposter since their earliest triumphs. But principally it seems to me to spring from a desire to resign our responsibility, to cease from judging provided we be not judged ourselves and, above all, are not compelled to judge ourselves."

ethos, and recognize that much of what appears in "top" journals is pseudo discourse.[2]

I CONFESS

I confess that I am one of those who think that the distinction between voluntary and coercive action is, as such things go, highly cogent, that coercion is still coercion when done by government (the imposition of a minimum wage at threat of physical aggression is coercive whether by neighbor or government), and that, for a large variety of reasons, including morals, political culture, and social structure, in nearly all things we should oppose coercion. Accordingly, I think that the vast majority of government restrictions and agencies should be abolished, though not necessarily forthwith. Indeed, one reason to oppose coercion is that coercion makes it harder for people to be sincere and open: governmentalization (of wage rates, drug use, schooling, safety assurance, social insurance) complicates issues and injects fearsome power variables.[3] I have never voted Democratic or Republican. In national politics, where foreign policy matters, my preferences have no tendency either way, but in state and local elections I would usually prefer the Republican to the Democrat.

Self-Disclosure or Exposing Oneself in Public?

Such "confession" might make you uneasy. It seems gratuitous and egotistical. In scientific discourse we seek harmony in interpretation and belief, yet the confession seems to posit deep-seated disharmony.

Well, there are deep-seated differences. The sensibility to the contrary comes from norms emergent from the institutions and practices of people with an establishment ideological orientation. They tacitly agree to keep

[2] Most model-building is pseudo discourse. As for empirical papers, Ziliak and McCloskey (2004) show that most empirical *AER* articles during the 1990s do not try to argue that their statistical evidence packs economic significance (or "oomph").

[3] At the start of this paper I quoted Bastiat saying "I confess that I am one of those who think that." The reader may be interested in reading the full passage: "I confess that I am one of those who think that the choice, the impulse, should come from below, not from above, from the citizens, not from the legislator; and the contrary doctrine seems to me to lead to the annihilation of liberty and human dignity" (1964, 12).

policy discourse between the 40-yard lines and jacketed by the convention political formulation "liberal versus conservative." Deviants are denied status; they either submit or are sorted out.

Researchers are inherently egotistical. In the light or in the dark, consciously or subconsciously, they rationalize their habits, sentiments, and commitments. They rationalize their selfhood. The confession informs readers of the ego-emergent agenda. It helps to resolve asymmetric information problems. Any uneasiness is the minor cost of the concomitant benefits.

As a device, the confession is a rhetorical extreme. Instead, one may disclose by making passing remarks, such as: "the findings may be welcomed by those who, like me, support . . ."

The plea, then, is not only to disclose your sensibilities, but to tolerate what might seem to you like unscientific exhibitionism.

STUDIES IN AEA IDEOLOGY

OK, so ideological sensibilities matter to scholarship. It's reasonable for stakeholders to want to know about them. This recognition has helped to authorize another Myrdalian trend: the inquiry into who academics are. In the past few years, numerous scholarly investigations have placed the professors under the microscope. The findings have tended to confirm Myrdal's central conjecture: Professors and other intellectuals are human beings. Social science, it seems, is the handiwork of creatures with their own values, perspectives, and purposes.

The finding also holds for economists, as illuminated by some investigations.

McEachern on 2004-Cycle Campaign Contributions

William McEachern (2006) investigates the 2004-cycle campaign contributions of people who play a part in the AEA. McEachern's results appear in this issue of this journal, so the reader can easily find the details. The Democratic-to-Republican giving ratios are overwhelming. The major problem in drawing conclusions is that a large majority gave to neither party. One might nonetheless find the results significant. First, among regular AEA members the rate of Democratic giving is 3.8 percent (itself remarkably high relative to the general U.S. population), yet the rate is 10.4 percent among authors in the discretionary AEA journals, 14 percent among editors of those journals, and 16.2 percent among search and nominating committee members. Thus, the leadership is vastly disproportionately populated by highly motivated Democrats (that is, individuals who contributed $200 or more).

Second, the scantiness of Republicans is truly remarkably. Setting aside the general AEA membership and the two retrospective categories (past Presidents and former top editors), the remaining categories covered in McEachern's exhaustive investigation yield an overall "part" count as follows: 172 Democrats to 8 Republicans. Now, in many cases, one person plays multiple parts. The 8 Republican parts are actually just four individuals. Of the four individuals, two accounting for 5 of the 8 parts had held (and likely had hoped to again hold) top appointed posts in Republican administrations. Especially if we chalk up their contributing to personal networking, we may conclude that basically no one who was playing a meaningful part in the AEA, in a population of 1,545 parts, wanted to give to Republicans in the 2004 cycle.[4] Now, as McEachern notes (173), giving to Republicans here pretty much means giving to the re-election bid of George W. Bush. And as McEachern indicates, the Bush administration had given people plenty of reason to conclude that it was inimical to what Smith called "natural liberty." Those with classical-liberal/libertarian sensibilities, even ones who tend to vote Republican, would hardly care for Bush, much less give money to his campaign. Some genuinely rooted for John Kerry.[5] As for conservative Republicans who value the Bush administration and want to contribute to his re-election, we may conclude that McEachern provides significant evidence of their absence from the AEA power structure (including acceptance into any of its publications).

McEachern shows that among AEA leadership there are vastly disproportionate groups of presumably strongly motivated Democrats and virtually no counterparts who supported Bush's re-election. These results

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[4] The numbers in this paragraph may be checked against Tables 1, 2, and 3 in McEachern. I have simply summed all categories in those three tables, exclusive of "former or current top editors" and "AEA presidents."

[5] On this point, here is anecdotal evidence: In 2004 I organized a major campus lecture about the upcoming presidential election, by the libertarian economist David R. Henderson. He came out unequivocally in favor of John Kerry over George Bush, on the grounds of divided government, a symbolic rejection of the Iraq invasion, and Bush's being not much better on domestic policy than Kerry would be.

tell us something about the ideology of the AEA. But they are more meaningful in combination with other results. To get a better read of the AEA's ideological distribution, we need an instrument that directly reads members' policy views.

Survey of AEA Members, 2003

In 2003, I surveyed 1000 AEA members using a list randomly generated by the AEA (the survey was sent out and handled by an independent controller, as explained at the survey homepage[6]). The survey contained questions about 18 policy issues, voting behavior, and background variables. The response was 264 (nonblank) surveys, about 27 percent (adjusting for PO returns). Here I report findings that bear on the ideological profile of the AEA and leave the details to the other papers where the results are properly reported.[7]

The voting question was as follows:

To which political party have the candidates you've voted for in the past ten years mostly belonged?

☐	☐	☐	☐	____________
Democratic	Green	Libertarian	Republican	other

Among the 264 respondents, 153 (58 percent) reported voting Democratic and 61 (23 percent) reported voting Republican. The other 50 respondents either checked Green (2), Libertarian (7), gave miscellaneous responses (17), or declined to answer the question (24). It is significant that 90.9 percent of the respondents answered the question. The data yields a Democrat to Republican ratio of about 2.5 to 1.

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[6] At the survey homepage one can view a sample survey and documents explaining the methods, independent control, and certification of the results. Link to survey homepage.

[7] Klein and Stern (2006a) gives a naïve account of the results, showing the distribution of responses for each of the policy questions. Klein and Stern (2006b) breaks down the policy-response data by voting behavior (Democratic or Republican), analyzes the distribution of policy-scores, and addresses the question of why free-market views are attributed to economists when the data indicates that few AEA members support free-market principles. Klein and Stern (2006c) focuses on the academic subset of the AEA respondents and compares them to the academic respondents from five other associations (of other disciplines) also surveyed.

Another question asked whether primary employment was in academia or elsewhere (with three alternatives specified). When we confine the sample to the academics up through the age of 70, there are 72 Democrats and 24 Republicans, for a ratio of 3 to 1. These D to R ratios are consistent with other surveys of AEA members. Of course, one may conjecture that there is a response bias, such as Democrats being more likely to return the survey than Republicans, but there is no evidence of such bias.

The format of the 18 policy questions was in the form of a statement to which the respondents were asked to indicate their view. The question on tariffs can be used as an example:

Tariffs on imported goods to protect American industries and jobs:

☐	☐	☐	☐	☐	☐
support strongly	support mildly	have mixed feelings	oppose mildly	oppose strongly	have no opinion
1	**2**	**3**	**4**	**5**	

The numbers 1-5 did not appear in the survey. They show how we weighted each response when creating a mean response. Here, as in all cases. The “5” value corresponds to strong support of free-market principles.

On the tariff question, all AEA respondents had a mean score of 4.46 and the Democrats had a mean of 4.35. Another question asked about “Government ownership of industrial enterprises,” and all 264 had a mean of 4.28 and the 153 Democrats 4.08. A free-market economist would hope to have seen a more robust opposition, but at least the answers are above 4.0.

The responses to the other questions are another matter. The other issues were minimum wage, occupational safety regulation (OSHA), the FDA, air and water regulation (EPA), discrimination restrictions, controls on hard drugs, prostitution controls, gambling restrictions, gun control, redistribution, government schooling (k through 12), tuning the economy with monetary policy, tuning the economy with fiscal policy, immigration, military action, and foreign aid. On these issues, only three mean scores are above 3.0. On minimum wage laws, for example, the overall average score was 2.83, with Democrats averaging 2.25 and Republicans 4.07. My survey results are highly congruent with those of Robert Whaples, who has recently conducted a policy-views survey of AEA members with 84 respondents (Whaples 2006).

The monetary-policy question and the military question do not fit the statist-libertarian spectrum (for elaboration, see Klein and Stern 2006b), and here I remove them. We can average a respondent's scores on the remaining 16 issues to arrive at that individual's 16-issue policy index. We then average those within the political-party groups, arriving at Table 1.

Table 1: 16-issue policy index of economists by voting behavior

	All n=264	Democratic n=153	Republican n=61	Libertarian n=7	Green n=2
Mean	2.66	2.34	3.30	4.30	2.38
(St.D)	(0.78)	(0.47)	(0.79)		

Democratic (and Green) voters are much more supportive of government intervention.

The data make clear that Democratic voters are not supporters of free-market policy. The highest 16-issue policy index among the Democratic voters is 3.5. Among the Republican voters, 39 percent are above 3.5.

I would suggest 4.0 to be a reasonable cut-point for being a supporter of free-market policy (on the 16-issue policy index). For the 264 AEA respondents, only 22 individuals, or 8.3 percent, met that cut-point.

As McEachern (148) notes, the AEA has long repeated the claim that "People of all shades of economic opinion are found among its members." Yet free-market supporters are very few in the AEA, and evidently none of them votes Democratic. The survey and campaign-giving results develop a picture of the AEA as being dominated by Democrats and antipathetic to libertarian sensibilities.

Given these data, it is no wonder that AEA leaders and officers might feel that the libertarians can be counted out of the game. Consider the following recent words from AEA Vice President Robert J. Shiller:

> Mandatory social insurance was one of those difficult pills to swallow that delayed the adoption of important social insurance innovations. But when the arguments for it were made persuasively enough, the innovations eventually did happen and are now accepted by all shades of political

> leanings, from the most conservative to the most liberal. (Shiller 2005, 280)

But some economists, including a few Nobel laureates, oppose such coercive government programs (though the discourse situation might lead them to focus on diminishing rather than abolishing them). They may even be vocal. But they do not have a place in the AEA. Their libertarian sensibilities are not recognized within "all shades of political leanings."

My Impressions of the *Journal of Economic Perspectives*

The Journal of Economic Perspectives in 1995 published a wonderful article, "The Economic Case Against Drug Prohibition" (Miron and Zwiebel 1995). I assigned the article to students and cited its evidence and judgments. With a "we economists" pride, I told students that it came from a journal of the American Economic Association, the nationwide association of professional economists.

That article, however, was exceptional. And especially since that time, I have found the discretionary AEA journals (*JEP*, *JEL*, *AER-P&P*) to be highly unsatisfactory. Most notable is a sort of error of omission: They fail to illuminate the most terrible things that governments are doing to us. Unlike the article by Miron and Zwiebel, they almost never criticize status-quo domestic intervention and make the economic case for liberalization. Indeed, only a small percentage of articles really involve a general evaluation of any economic policy. To test my impressions, I settled into a desk among the library stacks and spent three days examining *JEP* and *JEL* published 1995 thru 2005.

Myrdal reminds us that behind "economic science," "economic analysis," and the like are individuals with deep-seated sensibilities. The leadership of *JEP* since inception in 1987 is shown in the insert below. I did not review issues prior to 1995, but during the period 1995 thru 2003, and particularly during the editorship of Alan Krueger 1996 thru 2002, the *JEP* projected an establishment ideology leaning in the social-democratic direction. Not only has the overall balance been social-democratic, but there was hardly a single article that ran significantly counter to that perspective. The journal's title is misleading. Since the article by Miron and Zwiebel, there has not been a trace of abolitionist judgment on any issue. Search the journal during those years for an article that speaks unequivocally in favor of freeing up markets in the United States, or that levels any significant criticism against the welfare state, and you will find none. It should not be controversial to suggest that the status quo offers plenty of examples of bad policy calling for sober economic analysis and abolitionist judgments. The diligent reader of *JEP* might get the impression, however, that the democratic process in the U.S. rarely gets policy terribly and obviously wrong. That notion is characteristic of the social-democratic mentality; it is necessary to their fancying themselves stewards of the public culture.

Editorial leadership of *The Journal of Economic Perspectives*
1987 thru 2005

Editor	Joseph E. Stiglitz, 1987-1993
	Carl Shapiro, 1993-1995
	Alan J. Auerbach, 1995-1996
	Alan B. Krueger, 1996 thru 2002
	Andrei Shleifer, 2003-present
Co-Editors	Carl Shapiro, 1987-1993
	Alan B. Krueger, 1993-1996
	J. Bradford DeLong, 1996 thru 2003
	Michael Waldman, 2000-present
	James R. Hines, Jr., 2004-present
Managing Editor	Timothy Taylor, 1987-present

In the entire 11-year period reviewed, the *JEP* ran blank on many egregious policies (e.g., the FDA, reproductive, adoptive, and organ policies, agricultural policy, the freeway system, rail transit, union privileges). The journal published two dreadful articles on school vouchers, and one in which the author asked economists to reconsider their opposition to rent control. There have been a few articles critical of intervention, but only tepidly. A close look at those articles (e.g., on the results of deregulation, occupational licensing, housing/land-use restrictions, the European Union, the postal service) reveals that authors pull their punches, fail to make powerful economic arguments, and refrain from drawing abolitionist implications. Meanwhile, many articles have put a favorable spin on statist policies, policies that other economists have severely criticized.

The social-democratic character of Joseph Stiglitz, Alan Krueger, and Brad DeLong is evident from their activities and well known. Less well known is the ideological character of Timothy Taylor, the Managing Editor since inception. I raise this because the influence that he has had on the journal seems to have been substantial (and his dedication admirable). Of the 462 regular and symposium articles[8] 1995 thru 2005, 63 percent specifically thank him, quite often in a special way. If we omit articles that thank "the editors of this journal" or that have no acknowledgements at all, then it would be 74 percent that thank Taylor by name. Using the internet I investigated Taylor's writings and activities. "Investigated" here will be appreciated, I hope, as a term of research, not snooping. Again, Myrdal should persuade us that individuals and their deep-seated sensibilities matter, and are part of the scientific debate. "What's Timothy Taylor's story?" is part of the economic conversation, for the important role he plays in determining prominent economic discourse. I found that Taylor is an excellent, informative writer with an enviable record of writing for the popular press. He has published dozens of newspaper opinion articles (particularly in the *San Jose Mercury News*), and these generally project a centrist, economically-informed view, on the whole leaning in the social-democratic direction. His writings have favored free-trade, but also raising cigarette taxes, subsidizing recycling, redistributive goals, and universal health insurance coverage. In *Updating America's Social Contract: Economic Growth and Opportunity in the New Century* (Penner, Sawhill, and Taylor 2000), Taylor and his coauthors write:

> [The issues explored in this book] are all part of America's "social contract," a term that describes the explicit and implicit agreements among the members of a political community that define the rights and responsibilities of people vis-à-vis their government. Americans place a high value on allowing individuals to pursue their own happiness in their own way. However, collections of individuals with no common vision and no social mechanisms for dealing with problems affecting the whole can be highly vulnerable. The challenge is to find the right

[8] From this denominator I have omitted symposium introductions of seven pages or less and articles coauthored by Taylor, just 9 articles in all.

> balance in the social contract between individual freedoms and what Americans must do as members of a community acting through and with the assistance of a democratically elected government. (16)

The passage displays the social-democratic tendencies of seeing society as an organization administered by "contract" by government, of downplaying non-governmental mechanism for dealing with problems that affect "the whole," and of affirming government as a spiritual project in community enterprise and "common vision." By contrast, classical liberal/libertarian sensibilities oppose the organizational view of society. They tend to see the idea of individual liberty and the basic forms of property (beginning with ownership of one's own person) as salient and emergent within liberal civilization, and even largely self-enforcing in the absence of institutionalized depredations, and the government as a coercive institution operating within a realm otherwise consisting of diverse voluntary institutions and practices. These two worldviews generate a polarity of thinking and social networks within the economics profession, a polarity that is far more significant than the academic culture is ever inclined to admit.[9] One of the reasons that the economics profession is not more enlightenment about the 100 most terrible things government is doing to us is that the editors of *JEP* (and the other AEA journals) have neglected illumination of those issues.

Beginning in 2004, however, there seems to have been a change in the character of *JEP*, under the editorship of Andrei Shleifer, whose ideological sensibilities seem to be somewhat classical liberal. Shleifer has not fixed the core problem—the neglect of terrible status-quo policies—but he has reduced the social-democratic tendencies, and even published a few articles with a classical-liberal flavor.

What would a journal that wanted to explore diverse economic perspectives do? The answer is obvious. For policy issues and broader economic themes, it would invite economists with clashing perspectives to clash. It would arrange debates that included a second round where each tries to destroy the other's arguments. Readers like debates. They would read and learn more. Free competition is, as Hayek put it, a discovery procedure. For cultural competition especially, Schumpeter's *creative destruction* is apt.

[9] Evidence of such polarity is found in the patterns of AEA member's responses, in my survey and in Whaples' (2006).

However, *The Journal of Economic Perspectives*, even under Shleifer, is practically devoid of criticism and debate. *JEP* symposia sometimes have articles pointing in opposite policy directions (e.g., on anti-trust activism or on personal Social Security accounts), and the journal gives a small amount of space at the back to comments and replies,[10] but its primary modus operandi is to select a field authority to survey the literature and integrate the recognized works into a single overarching interpretation, as though to bring us all into a condition of "common knowledge." The impetus may be not so much ideological as existential. Cultural elites like to think they lead an enlightened consensus. They promulgate a face of establishment consensus, especially in fields anxious to claim the status of science. That may be the larger explanation for the avoidance of real criticism and debate.

One can peruse the AEA journals and make one's own judgments. If economists have impressions like mine, then we would expect to find that those who do not favor the social-democratic ideology are less likely to join the AEA. We would expect to find that classical liberals and conservatives are less likely to join the AEA, while Democrats are more likely.

Rates of AEA Membership by Party Registration

In collaboration with me, Christopher Cardiff, Andrew Western, and Patrick Peterson have collected voter-registration data on tenure-track faculty at eleven California colleges and universities (UC-Berkeley, UC-Los Angeles, UC-San Diego, Stanford University, California Institute of Technology, University of San Diego, San Diego State University, Claremont McKenna College, Pepperdine University, Santa Clara University, and Point Loma Nazarene University). Our purpose in collecting the data was to study faculty generally, not just economists. In correspondence, however, William McEachern suggested that I look into the economist data to see if there were differences in AEA membership rates by voter registration category.

In the previous discussion of the survey results, we saw that Republican and Libertarian voters' policy views are less social-democratic than Democratic and Green voters. The basic issue is whether the AEA is perceived to have a social-democratic orientation, so I sorted the economists into three groups:

[10] The following average-per-issue quantities are for the 44 issues 1995 thru 2005: 4.7 pages devoted to correspondence/comments/replies, 2.3 letters/comments, 1.1 replies.

- Registered Democrats or Greens; denoted here as "Dem/Gr".[11]
- Registered Republicans, Libertarians, or American Independents (the AI party is a Pat Buchanan-type conservative party and is the California affiliate of the Constitution Party); denoted here as "Repub/L/AI".[12]
- Others, which includes those who were not found, registered nonpartisan or "decline to state", indeterminate because of multiple and conflicting information for the same name, and one member of the Reform Party, whose centrist platform seemed to me to fit neither the social-democratic category nor the non-social-democratic category.

Using the online AEA membership directory, I determined whether the individual was an AEA member. I put into Appendix 1 details about the sample and the data collection, a link to the line-by-line Excel sheet with names redacted, and the overall statistics for each school.

In this data set there are just 34 Repub/L/AIs. Moreover, because the Repub/L/AIs are disproportionately found among the less prestigious schools,[13] where AEA membership rates are lower for all categories, it is appropriate to divide the sample into two sets, high-tier (UCB, UCLA, UCSD, Stanford, and Cal Tech) and lower-tier (USD, SDSU, Claremont, Pepperdine, Santa Clara, and Point Loma Nazarene). This attempt to control for the tier effect *weakens* the flavor of the results.

The AEA membership rates for high-tier and lower-tier are shown in Figures 1 and 2.

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[11] The entire economist sample contains just one registered Green, who was an AEA member.

[12] The entire economist sample contains two registered Libertarians and two registered American Independents, none an AEA member. Line-by-line data (with identifying information redacted) is found in the linked Excel file. For information on the American Independent Party of California, click on this link to its homepage.

[13] In selecting schools, Chris Cardiff and I sought variety in ideological reputation (e.g., Pepperdine as a conservative school) and religious orientation, so the reader should not infer anything into this particular finding. There is a fair amount of evidence, however, that, in general, more elite schools are more solidly Democratic.

Figure 1: High-Tier AEA Membership rates by Party Registration

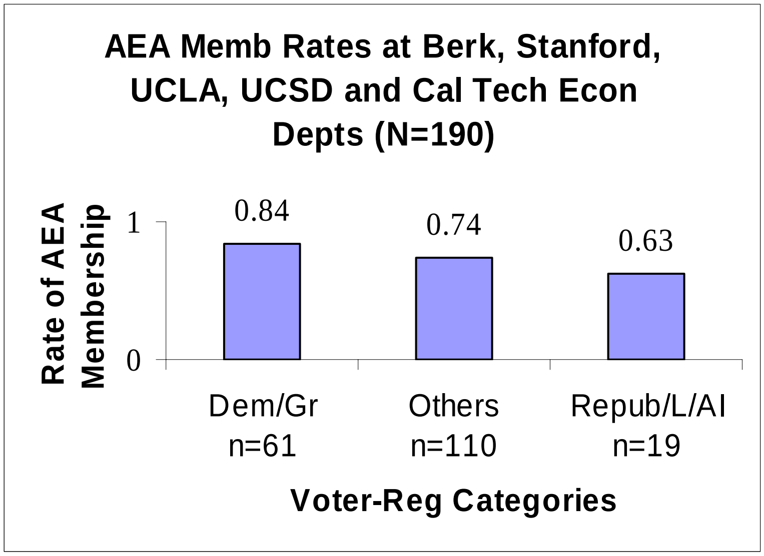
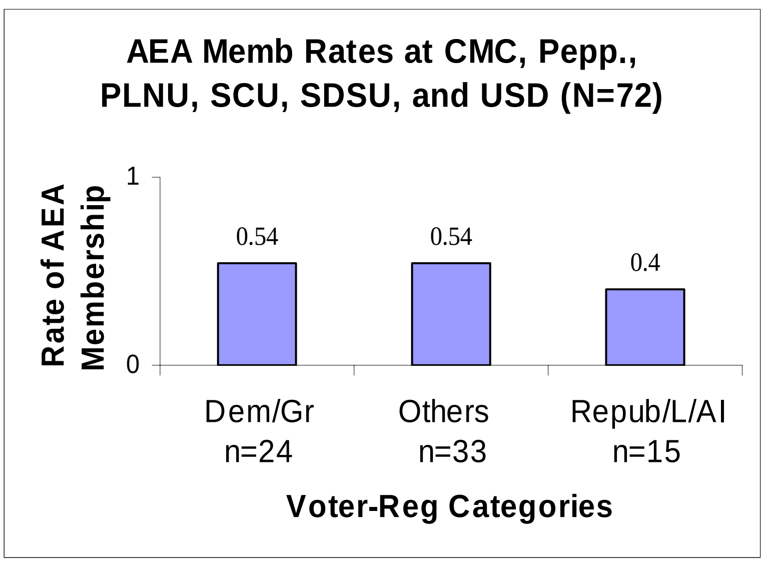


Figure 2: Lower-Tier AEA Membership rates by Party Registration



For the high-tier schools, the rate of AEA membership among Repub/L/AIs is 76 percent of the Dem/Gr rate. For the lower-tier schools, it is 74 percent. Averaging the two, we get Repub/L/AIs being about 75 percent as likely to join the AEA as Dem/Grs. The sub-samples are small, yet for the high-tier group a Pearson Chi-square test shows there would be only a 5.7 percent chance that samples of such size would show (at least) that much difference if the two voter categories were in fact equally likely to join the AEA. At any rate, the results help to clarify the point and indicate the need for further research. Expanding the sample is not easy, because voter registration data generally resides only at dispersed voter-registrar offices.

If meaningful empirical research were to find that Dem/Grs were more likely than Repub/L/AIs to join the AEA, one possible explanation would be that economists perceive a somewhat social-democratic bent to the AEA and are attracted or repelled according to their own ideological proclivities. We would not, however, be able to rule out an alternative explanation: That conservatives and libertarians are less inclined to join a professional association (that is, that they would be less inclined to join the AEA even if it were, to their mind, ideologically unbiased). I am unaware of any evidence that speaks to that proposition.

WHO ARE YOU?

The Journal of Economic Literature takes as its logo a medal bearing the profile of Adam Smith, a man who labored hard and meticulously to establish a strong presumption of natural liberty. We are well aware of the numerous natural-liberty exceptions and inconsistencies in Smith's comprehensive survey of public policy. His libertarianism was not adamant. I believe that to some extent the non-libertarian moments should be understood as the compromises and fudgings necessary of a libertarian individual holding and utilizing a position of cultural royalty. Although he lived prior to the age of social democracy, he gave a visible thumb's down to social-democratic sensibilities and attitudes. There seems to be little place for a Smithian minority report in the array of AEA publications and activities. With an AEA dominated by Democrats, young economists are not going to become exposed to abolitionist ideas or fundamental criticism of government and politics. Although the lead articles in the *JEL* since 1995 (and particularly under John McMillan 1998 thru 2004) have *not* leaned particularly in the social-democratic direction, the neglect of terrible status-quo policies has been so characteristic that the legitimacy of journal's invocation of Smith is highly questionable.

Questions about the political culture within which the AEA structure has emerged address the broadest frame and reach back to the origins of liberalism. They open up the larger questions of who we are and what we are up to. A broad frame demands that we heed Myrdal's call to keep the fundamental judgments and sensibilities out in the open. The surest way to achieve genuine discourse is to be upfront about where we are coming from.

Consider the short article entitled "Toward National Well-Being Accounts," appearing in the May 2004 *American Economic Review Papers and Proceedings*. The authors write: "Here we propose measuring national well-being by weighting the time allocated to various activities by the subjective experiences associated with those activities" (p. 433). The idea is to rate daily activities like washing the dishes based on how happy people say they are as they do them. Compared to GDP, "a better measure of well-being could help to inform policy" (p. 433). The authors do not specify how these accounts will be used, nor how they will be paid for. Nor do they say much about how they are constructed and how they relate to regard for such things as dignity, individuality, and personal narrative. Now, wouldn't it be refreshing if the authors, Daniel Kahneman, Alan B. Krueger, David Schkade, Norbert Schwarz, and Arthur Stone, came out and said:

> "We are proposing an ambitious new empirical formulation of national well-being, and we are lobbying the government to have it paid for by taxpayers. This project involves deep-seated ideological judgments and purposes, and our results could have far-reaching political and policy consequences. Accordingly, we think it only fair that everyone know where we are coming from. We confess that we are among those who think that the government should take a leading, guiding role in social affairs, to promote equality of opportunity and the general welfare, not too constrained by regard for what some call 'individual liberty.' We all vote Democratic and consider ourselves to be liberal in the modern sense of the term."

Or whatever description they would give of themselves.

How nice it would be! How productive when economists tell us who they are—how they vote, how they identify themselves ideologically, and the like. That practice shows respect for our differences, and helps us fit whatever they have to say into larger cultural engagements.

Some economists do not know who they are. Others lack a developed, integrated faculty of judgment. But if you do have a developed faculty of judgment and you know the patterns of your judgment, you do a service by letting on. Next time you write or speak publicly of things of political significance, consider the simple words: "I confess that I am one of those who think that . . ." With those words or passing remarks that serve the same purpose, you earn the esteem of those, like Adam Smith and Gunnar Myrdal, who favor frankness and openness. You advance a culture of frankness and openness. And, inversely, the public culture is degraded by those who conceal who they really are.

Appendix 1: Data on Voter Registration and AEA Membership

Table 2 provides a breakdown of the data by individual school. For a key to the school-name abbreviations, please see the text above p.196.

Table 2: Voter Registration and AEA Membership at 11 California Schools

		Dem/Grs		**Repub/L/AIs**		**Others**			**All**	
N	**School**	*AEA*	*not*	*AEA*	*not*	*AEA*	*not*		*AEA*	*not*
55	Berkeley	19	3	1	1	21	10		41	14
16	Cal Tech	2	0	2	1	9	2		13	3
43	Stanford	12	2	4	2	17	6		33	10
44	UCLA	8	3	3	2	19	9		30	14
32	UCSD	10	2	2	1	15	2		24	8
18	CMC	2	1	2	1	9	3		13	5
14	Pepp.	0	0	3	4	2	5		5	9
3	PLNU	1	0	0	1	0	1		1	2
15	SCU	4	3	0	2	3	3		7	8
9	SDSU	2	3	0	0	3	1		5	4
13	USD	4	4	1	1	1	2		6	7
188	High-tier	51	10	12	7	81	29		141	49
70	Lower-tier	13	11	6	9	18	15		37	35
262	TOTAL	64	21	18	16	99	44		178	84

Line-by-line data, with identifying information redacted, and the figures are contained in the Excel file linked here.

Data collection: The data is collected on tenure-track economics department professors (excluding emeriti faculty). For Pepperdine, the economists come from three different schools within Pepperdine (Seaver College, Graziado School of Business and Management, the School of Public Policy) and we included all faculty listed as "economics" faculty. UCLA's Anderson School of Business has a number of economists associated with it, but they are all doing double-duty at the undergraduate level and hence were included in the normal Economics Department listing. For Cal Tech, we included all the faculty listed as "economics" or "business economics" faculty. Table 3 provides information about the data collection.

Table 3: Voter-Registration Data Collection

Schools	**Gatherer of The data**	**Date voter-reg data gathered**	**Co. voter-records used**	**Citation to relevant works**
Los Angeles area: UCLA, Cal Tech, CMC, Pepp.	Christopher Cardiff (tifchris@aol.com)	September – December 2005	Los Angeles, Orange, Ventura, San Bernardino, Riverside	Cardiff and Klein (in progress)
San Diego area: UCSD, SDSU, USD, PLNU	Christopher Cardiff (tifchris@aol.com)	August – September 2005	San Diego	Cardiff and Klein (in progress)
UCB and Stanford	Andrew Western (awestern@scu.edu)	January – May 2004	Alameda, Contra Costa, San Fransico, Santa Clara, Solano, San Mateo, Marin	Klein and Western (2004)
Santa Clara	Patrick Peterson (patrickpeterson@yahoo.com)	October – December 2004	Santa Clara, San Mateo, Alemeda, Santa Cruz	Cardiff and Klein (in progress)

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ABOUT THE AUTHOR

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CORRESPONDENCE

Editors,

This journal has published research that describes some of the practices of Thomson ISI in producing the Social Science Citation Index (click here for an article from the April 2004 issue). As noted there, one of the important issues in producing the index is using citation activity as a criterion for journal inclusion. The primary issue I wish to highlight is as follows: If the journal belongs to a cluster of related journals that cite each other, the citation criterion becomes indeterminate, in that if the leading journals of the cluster are all included, then they all meet the criterion, but if only one journal (whether at the intensive or extensive margin) is held to that criterion, it may fail the criterion. A more meaningful citation criterion would be one that applied to entire clusters of journals that stood or fell together. To apply such a criterion, the editors of journals in question, presumably, would submit their own within-cluster citation activity—a task that would be manageable and very much in their interest to do. It may well be that at present the index includes some clusters and excludes others, without there being any cluster-level citation criterion that would discriminate between those that are in and those that are out.

Readers of this journal might wish to know about such issues as they relate to the history of economics. Here I provide the text of a letter that I drafted, but which was later modified, and shortened, and then sent over the signatures of the members of the Executive Committee of the History of Economics Society this past August 2005 to Thompson ISI, publishers of the Social Sciences Citation Index.

> Dear Mr. [redacted],
>
> In April of this year, the editors of *History of Political Economy* (*HOPE*) were informed by a concerned colleague

that the journal had been dropped from the Social Sciences Citation Index (SSCI). A May 4 e-mail message from you to the editors confirmed that that was indeed the case. Naturally, the editors are eager to be restored to the SSCI and are confident they have a strong case.

We are the officers of the History of Economics Society, outside of Japan the oldest organization in this scholarly discipline. We know that *HOPE* is the longest-running journal in the subfield of economics known as the history of economic thought. In the judgment of nearly all historians of economics, it is the leading journal in the field. The first issue was published in 1969, and the journal has been continuously published since then. Each annual volume of *HOPE* contains four regular issues of 200 pages plus one supplemental book- length issue that is published in hardback; the supplement contains the proceedings from an international conference *HOPE* organizes each year. *HOPE* is published by Duke University Press, which is widely regarded as one of the very finest scholarly presses in the world. *HOPE* averages one thousand subscribers a year—a not insignificant number for a scholarly journal devoted to a subfield.

In your e-mail of May 4, you offer to the editors that *HOPE* might have been dropped from the SSCI because of a "low citation activity." If that is indeed the case, it is likely true for the simple reason that the SSCI does not index other journals in the history of economic thought—the journals that are most likely to cite *HOPE* articles. Two of those journals are the *Journal of the History of Economic Thought* (*JHET*) and the *European Journal of the History of Economic Thought* (*EJHET*). To cite some numbers to support this claim, in the 2004 volume of *JHET*, there were 31 citations to articles in *HOPE*; in the 2004 volume of *EJHET*, there were 23 citations to *HOPE* articles. Those two journals, along with *HOPE*, constitute the core journals in the field, and, as such, all three should be indexed by the SSCI.

> There is a danger to all historians of economics in the SSCI's practice of using citations to *HOPE* alone as an index of "activity", as you are of course aware. The field of the history of economics is defined in the American Economic Association's *Journal of Economic Literature* Classification system as "Bxxx" and is thus one of the main subfields of the profession. Very few general interest journals in economics these days publish history of economics papers, regarding that field as one in which the "field" journals are the normal outlet for scholarly work. The field journals are in fact many, with the core international ones like *HOPE*, *JHET*, and *EJHET*, and then the national ones like those published in Italy, Japan, Australia, England, and elsewhere. This field is well organized, and scholars in it publish frequently and cite one another's work. If though only one journal had been in SSCI (*HOPE*), it was unlikely that its citation count would be high. If the top three were in SSCI, the results would be quite different.
>
> For our subdiscipline, the matter is a serious one. Various government agencies, like those responsible for determining research funding in a number of countries, look at citation studies as one measure of productivity. We scholars in the history of economics are now defined on that measure as absolutely non-productive! As officers in the History of Economics Society, with 300 members in total (180 in the US and Canada, and 120 in other countries), linked by a web-list of over 800 members in 40 countries, we want you to be aware of our concerns.

Subsequently, the editor of the *Journal of the History of Economic Thought*, the editors of the *European Journal of the History of Economic Thought*, and the officers of the European Society for History of Economic Thought sent letters to Thompson ISI. Discussion of these matters can be found in the Archive of HES-LIST at http://eh.net/pipermail/hes/ for July, August and September 2005. There was also a short article on this matter by David

Glenn in the "Hot Type" column of the *Chronicle of Higher Education* for September 2, 2005.

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