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Rejoinder to Wittman: True Myths

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CONTINUATION OF THE EXCHANGE BETWEEN BRYAN CAPLAN AND
DONALD WITTMAN FROM THE APRIL 2005 ISSUE OF *EJW*.

[Caplan Comment on Wittman \(April 2005\)](#)

[Wittman Reply \(April 2005\)](#)

WITTMAN HAS WRITTEN A CHARACTERISTICALLY ENGAGING response to my critique. While he emphasizes his continued disagreement with me, I am struck by the important concessions he makes. In particular, he has virtually abandoned the rational expectations assumption that drives his trademark results. This retreat has a high price, because his new watered-down standards of rationality are consistent with vast democratic inefficiency. Apparently, democratic failure is not a myth after all.

Wittman's main theoretical effort to salvage his position is to shift a great deal of weight to arguments about discrete choice. While there is a kernel of truth to his approach, it is fairly easy to show that false beliefs about discrete choices are about as dangerous as false beliefs about continuous choices. Furthermore, existing empirical evidence contradicts Wittman's conjecture.

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Wittman correctly observes that I do not present any formal statistical evidence that people are more rational as consumers than they are as voters. I applaud his proposals for new empirical tests to resolve the matter. However, Wittman overlooks the direct approach: Design and administer tests of consumer knowledge comparable to existing tests of voter knowledge.

WITTMAN'S SURPRISING CONCESSIONS

Rational Expectations

The assumption that voters have rational expectations is central to Wittman's defense of democracy. Critics of democracy have argued, for example, that voter ignorance leads to wasteful pork barrel spending. But Wittman has repeatedly pointed out that if ignorant voters have rational expectations, this does not follow.

[A]ll their arguments rely heavily on some asymmetry in voter behavior. For example, Fiorina and Noll (1978) assume that the voters are aware of the benefits, but not the costs, of incumbent facilitation of constituents' needs. Shepsle and Weingast (1981) assume that the voters recognize the job gains from pork barrel in their district but underestimate the job loss from the sum total of pork barrel in all other districts. Clearly, such asymmetry is the driving force for too much pork barrel. But I have argued in the previous sections that the assumption of voter asymmetry is unwarranted; that is, imperfect information does not imply biased estimates. (Wittman 1989: 1410)

As far as I can tell, Wittman (2005) quietly repudiates his assumption that voters have rational expectations. He used to take it for granted; now Wittman (2005) states, "when we deal with levels instead of comparative statics, determining what behavior is irrational is extremely difficult, and there is likely to be little consensus on what is irrational" (29). Wittman (1995 and 1989) ably used rational expectations as a battering ram against

traditional public choice; Wittman (2005) is almost agnostic about whether it was true in the first place.

Wittman now distances himself from the standard predictions of rational expectations. If voters have rational expectations about the share of the budget devoted to foreign aid, for example, the public's average belief is supposed to approximately equal the true value.

If the average voter is informed, or has rational expectations, then the average voter will predict that foreign aid is 1% of the federal budget. If the average voter is uninformed and/or does not have rational expectations, then the prediction will be not 1%. (Wittman 2005, 28)

Wittman no longer expects this. In fact, he asserts that the prediction “makes no sense.”

One is comparing a point estimate to the whole space minus the one point. Obviously, it makes no sense to compare the two (as they are not the same thing) and stated this way, all that irrational expectations predicts is that the average estimate will not be 1%, which is not a prediction at all. (Wittman 2005: 28)

This is the most puzzling statement in Wittman's reply. To me, it obviously *does* make sense to compare the two. Economists test point estimates all the time—for example, that the effect of expected inflation on output equals zero. Admittedly, Wittman is correct if he means that you should not reject rational expectations just because the public's average belief is not *exactly* one percent; the difference has to be statistically significant. But what statistically literate person has ever argued that you should reject hypotheses for failing to work *perfectly*?

A valid concern that Wittman raises about tests of rational expectations is that some issues have a lower or upper bound. For example, foreign aid cannot be less than zero percent of the budget. Evidence of bias might, therefore, simply be a “statistical artifact” (Wittman 2005, 23). But Wittman misses the fact that bounded responses can mask biases as well as exaggerate them. For example, when the Survey of Americans and Economists on the Economy asks respondents to classify foreign aid as a major, minor, or non-reason why the economy is not doing as well as it otherwise would, economists cluster at “not a reason,” while the public clusters at “major

reason” (Caplan 2002a). If members of the public had more extreme options, some would probably endorse them, revealing the apparent lay-expert gap to be *understated*.

After dismissing standard tests of rational expectations, Wittman proposes an idiosyncratic alternative.

One solution is to have uniform priors on the set of possible irrationalities . . . so that irrational expectations predicts 50%, but then the actual average voter perception of 10% is much closer to 1% than 50% so we should reject the irrational expectations model in favor of the rational expectations model. And even if the set of irrational beliefs were confined to being less than 50%, uniform priors would suggest an expected irrationality of 25%, which is still further away from 10% than 1%.¹ (Wittman 2005, 28)

If I understand Wittman’s proposed test, then, we should “reject the irrational expectations model in favor of the rational expectations model” even if voters overestimate the budget share of foreign aid *by a factor of ten*. This dilutes the rational expectations assumption to the point of meaninglessness. Suppose I believe that I, personally, earn *twenty-four percent of world income*—about \$13 trillion dollars per year. This overestimates my true income by more than a factor of a hundred million. Wittman’s test would count my belief as evidence in favor of the rational expectations model, because twenty-four percent is closer to zero percent than it is to fifty percent.

In any case, if Wittman insists on this approach, he faces a serious problem. His new version of “rational expectations” implies nothing about democratic efficiency. The authors that Wittman (1995 and 1989) criticized now have a simple response. Fiorina and Noll (1978), to take only one case, could now object: “Of course we assume rational expectations. In our

¹ Wittman seems to mistake my hypothetical ten percent figure for an actual data point. The original text (Caplan 2005, 10) reads: “If the average response of a representative sample is 10 percent, there is strong evidence that the public systematically overestimates government spending on foreign aid. Empirical work along these lines finds large systematic errors on important questions. For example, the National Survey of Public Knowledge of Welfare Reform and the Federal Budget (1995) reports that the public heavily overestimates the share of the federal budget devoted to welfare spending and especially foreign aid, and underestimates the share going to Social Security.” Since Wittman showed me a draft of his manuscript, I share responsibility for his misinterpretation.

model, voters think that pork barrel spending is free, and that is much closer to the truth than the assumption that pork barrel spending is 50 percent of GDP.” The same goes for all the other writers Wittman faults for equating ignorance and bias.

Elsewhere in his reply, Wittman seems to drop rational expectations altogether in favor of a much weaker benchmark of rationality.

The main way that economists have tested consumer rationality is to see whether demand goes down when price goes up. If demand curves were upward sloping, that would be evidence for consumer irrationality. Because they don't, we are pretty confident that consumers are rational. The same type of test should be employed to see whether voters are rational. Do they have (weakly) downward sloping demand curves? (Wittman 2005, 26)

I agree that this is *one way* that economists have tested for consumer rationality, though since the rational expectations revolution of the 1970s it has not been the “main way.” In any case, the problem with the “demand-slopes-down” standard of rationality is that it is consistent with massive democratic inefficiency.² Take Fiorina and Noll's (1978) assumption that voters treat pork barrel spending as free. If the cost of pork doubles, voters' desired quantity stays the same, because they still think it is free. These voters, therefore, have a (weakly) downward-sloping demand curve. But the political equilibrium is inefficient, because voters eagerly support candidates who deliver goods that cost them far more than they are worth.

The bottom line is that Wittman can no longer derive his conclusion that democracy is efficient. If he is agnostic about rational expectations, he has to be agnostic about democratic efficiency. If he adopts another benchmark of rationality, rationality no longer implies democratic efficiency. Either way, Wittman cannot stand by his original democratic optimism.

Admittedly, just because democracy falls short of the happy picture that Wittman (1995 and 1989) painted, it does not follow that markets are better. If we can test the rationality of voters and find them wanting, it is also possible to discredit the rationality of consumers. Although Wittman

² It is worth mentioning that demand is supposed to have a negative slope with respect to *private* cost—the price that the consumer actually pays. But many of Wittman's arguments seem to assume that voter demand has a negative slope with respect to *social* cost—the price that society as a whole pays.

does not provide or cite any statistical evidence to this effect, I find his example of medical quackery fairly convincing.³

Suppose I undertook a parallel test of consumer rationality. One might ask consumers whether homeopathic medicine works, Ginko Biloba improves memory, Echinacia prevents colds, and colloidal silver helps the immune system. I suspect that the answers provided would differ greatly from experts at the major medical schools and the National Institute of Health and that consumers' understanding of many medical matters was greatly off the mark. (Wittman 2005, 26)

Wittman could appeal to a great deal of research in behavioral economics for additional support (Rabin 1998; Thaler 1992). However, he is too hasty to put me in a "quandary."

Either he [Caplan] agrees that this data implies that consumers are irrational, thereby agreeing with Lenin that neither economic markets nor democracy works because the actors are irrational, or he believes that this data does not prove that consumers are irrational, thereby undermining his parallel evidence that voters are irrational (unless he can find a very clever way of distinguishing between the two irrationalities). (Wittman 2005, 26)

Wittman strangely ignores the fact that irrationality is a question of degree.⁴ You do not have to become a Leninist just because you find that both consumers and voters have some beliefs that are irrational to some

³ It is possible, however, that people who *underestimate* the benefits of alternative medicine—most obviously, by mistaking useless treatments for harmful ones—are as common as people who overestimate the benefits.

⁴ Wittman raises this issue later in his reply: "[T]here is often a counting problem. We can point to instances of irrationality, but we can also point to instances of rationality. If we are forced to assume either that people are always rational, or always irrational, because we have no good way to predict when one is operative, we will have to choose the hypothesis that works best over all cases. This means considering all cases, not just providing examples that fit with our notions (rational voters for Wittman; irrational voters for Caplan). This is a hard thing to do and people make little effort in this direction. This problem has plagued the debate." But economists (along with other scientists) overcome such "counting problems" all the time. There is no perfect solution, but this does not stop authors in, e.g., the *Journal of Economic Literature* from summarizing the "overall results" in a field.

degree. For example, my empirical research on beliefs about economics did not find that the public has a few mild biases. It found that the public has large biases on a long list of policy-relevant topics. As a rule, voters heavily underestimate the benefits of markets, especially international and labor markets.⁵ Wittman has not shown—nor, to the best of my knowledge, has anyone else—that consumers suffer from comparable defects. The hundreds of millions of dollars Wittman says consumers waste on “worthless cures” are a small fraction of GDP.

Incidentally, which side of the preceding “quandary” would Wittman choose for himself? Does he dispute the medical consensus against homeopathy? Or does he, like me, side with the experts, and conclude that consumers are wasting their money? If he is willing to trust experts in other fields, why not his own? When economists and the public disagree about the benefits of free trade, for example, why is Wittman so reluctant to say “We’re right, they’re wrong”?

His answer, as best as I can tell, is that economists correctly maintain that free trade is good for the economy *as a whole*, but non-economists who oppose free trade correctly maintain that free trade is bad for them *personally*.

[S]ome differences are to be expected. A very large percentage of economists are in favor of free trade. But it would be irrational for all voters to be in favor of free trade as a great number of voters are hurt by it. (Wittman 2005, 25)

But this story does not fit the facts. Even if you specifically ask about the effects of free trade on the economy as a whole, non-economists are much more negative than economists (Blendon et al. 1997). One could argue that their belief gap stems from self-serving bias: Those who benefit from free trade convince themselves that it is good for society, and those who lose convince themselves of the opposite. But this story does not fit the facts either, because large lay-expert belief differences persist controlling for income, income growth, job security, and other measures of self-

⁵ Wittman (2005, 23) says that “It would be interesting to discover whether the survey response of those who regularly vote differs systematically from those who vote rarely, if at all.” Caplan (2002b, 429) finds that controlling for other characteristics, the beliefs of registered voters differ from those of non-registered voters less often than would be expected by chance.

interest⁶ (Caplan 2002a). Wittman may not want to take a side when economists and non-economists disagree, but he does not have much choice.

Rational Irrationality

Wittman is surprisingly willing to accept my premise that consumption of irrationality is sensitive to its price.

There is little cost to being misinformed when *your choice* would be the same if you were informed... It would make little sense for strong vegetarians to stay abreast of the latest research on meat. . . . As a result, strong vegetarians might be misinformed, possibly holding irrational views, about meat. (Wittman 2005, 23)

But he refuses to accept my deduction: “this is to be distinguished from one of Caplan’s arguments, with which I disagree, that voters make irrational choices because their choice will not affect the outcome” (2005, 23).

I want to know *why* Wittman disagrees. He accepts the premise that rationality depends on incentives. Does he believe that individual voters *do* have a substantial effect on political outcomes? If not, Wittman has at least as much reason to accept my claim than his own. If a voter does not bother being rational when it would not change his *conclusion*, one would also expect the voter to not bother being rational when it would not change *policy*.

Wittman’s disagreement is all the more puzzling because my rational irrationality model is consistent with one of his main challenges to critics of the rationality assumption.

⁶ Caplan (2002a) also suggests a simple explanation for Wittman's evidence about the political orientation of Harvard, Yale, and Princeton faculty: ideological bias. The belief gap between economists and the general public persists controlling for party identification and ideology. The gap between Harvard/Yale/Princeton faculty and the general public would be far less robust to these controls, because in a random sample, there is little correlation between education and party identification (Caplan 2001).

If most people's cognitive abilities are so poor that they cannot achieve what they want, why have they been able to survive in the presence of superior beings who occupy a similar ecological niche? Hunters (human or otherwise) who incorrectly estimate the likelihood of finding food in various locations will have a lower chance of survival and ultimately lower reproductive success than those who make correct estimates. (Wittman 1995, 60)

My response is that false beliefs about policy, unlike false beliefs about how to find food, have essentially no effect on an individual's chance of survival or reproductive success. Why does Wittman find this answer unsatisfactory?

WITTMAN'S DISCRETE RETREAT

Wittman (1995 and 1989) relied heavily on the assumption that voters have rational expectations. Wittman (2005) argues that he may never have needed this assumption, because severely biased voters hold the same positions as they would have if they did have rational expectations.

[P]eople who greatly overestimate are against foreign aid and would still be against foreign aid even if they were informed of the true value. If this is the case, there is little cost to their being uninformed since they would take the same position (reduce foreign aid) even if correctly informed. (Wittman 2005, 24)

Wittman gives a striking example from the 2004 election.

According to an October 21, 2004 Harris Poll, 52 percent of those who preferred Bush thought that Saddam had helped plan and support the hijackers who attacked the U.S. on September 11 (it was 23 percent for those who preferred Kerry) and 58% of those who preferred Bush thought that Iraq had weapons of mass destruction when

the U.S. invaded (it was 16% for those who preferred Kerry). Neither of these assertions is true. . . . Now some might say this would demonstrate how irrational Bush supporters are, but I think it is entirely rational. So let us try a little thought experiment. If you were strongly in favor of one of the candidates, and then you found out that you were wrong about several facts regarding the candidate, would you be in favor of the other candidate? If the answer is no, then why bother checking your facts in the first place, as it is unlikely to alter your vote. (Wittman 2005, 24)

Part of Wittman's story is correct. When people face discrete choices, their behavior *often* remains the same when conditions change. But Wittman ignores an equally important feature of discrete choices: Some people's behavior changes *drastically* when conditions change *slightly*. If a car manufacturer raises the price by \$100, most people who were going to buy the car still do. So why not raise the price? Because that \$100 price hike leads some people to buy zero cars instead of one. When you net the two effects, there is no reason to think that demand for discrete products is less price elastic than demand for continuous products.

Similarly, Wittman is arguably correct that most Bush supporters would have continued to support him even if they abandoned their foreign policy misconceptions. But you do not need *most* supporters to change their favorite candidate in order to change the result of the election. In fact, if merely 2.5 percent of Bush supporters would have changed their vote upon learning the truth about Iraq, Kerry would have won the popular vote. Of course, neither Wittman nor I would expect Bush to take this lying down. If voters lost their biases about Iraq, costing Bush 2.5 percent of his votes *holding his policies constant*, his natural response would be to change his policies—for example, by not invading Iraq.

Incidentally, this suggests an answer to an objection that Wittman did not explicitly raise: Since the median economist is politically independent and ideologically moderate, policy would remain roughly the same even if all voters thought like economists.⁷ The problem with this argument is that economists do want large *policy* changes; they just happen to be policy

⁷ Similarly, he could have argued that since education makes people think more like economists, but correlates weakly with party identification, policy would remain roughly the same even if all voters were well-educated.

changes that both parties currently oppose. If the median voter turned into an economist, politicians of all parties would revamp their platforms to curry his favor.

Wittman's reply is supposed to be exploratory, so it would be churlish to ask for empirical evidence in support of his claim that biased beliefs have little or no effect on policy preferences. However, it is fair to point out that my original article surveyed evidence that contradicts Wittman's speculation. My own empirical research admittedly takes the link between biased beliefs and policy preferences for granted. But the "enlightened preference" literature does not. It demonstrates precisely what Wittman denies—individuals' policy preferences systematically change when their knowledge increases (Althaus 2003; Caplan 2005, 10-11).

Admittedly, people's support for some policies *seems* extremely stubborn. The minimum wage is a prime example. In my experience, it is hard to raise doubts even in a captive audience of freshmen. Is it possible that Wittman's claim is at least correct for policies like the minimum wage that enjoy deep-rooted support? A well-designed 1996 Gallup poll suggests that the answer is no.⁸ This survey split a sample of about a thousand people into two groups. The first was asked: "Do you favor or oppose raising the minimum wage from four dollars and 25 cents an hour to five dollars and 15 cents an hour?" Responses were typical: 81 percent in favor, 17 percent opposed. The second group was asked: "Would you favor or oppose raising the minimum wage if it resulted in fewer jobs available to low paid workers in this country?" Mentioning this moderate drawback drastically reduced support for a higher minimum wage; only 40 percent favored it, with 57 percent opposed. Even if one doubts the disemployment effect of the minimum wage, the point is that its popularity depends heavily on beliefs about its effects. Policy preferences are stubborn in large part because beliefs about what works are stubborn, and not—as Wittman argues—the other way around.

⁸ May Wave 1 Questionnaire, 1996, Questions 24 and 24a. Available with registration at <http://brain.gallup.com>.

WITTMAN'S EMPIRICAL CHALLENGES

The Best of Wittman

Wittman's best point is that "voter rationality and consumer rationality should be tested in the same way and compared" (2005, 25). I agree. I have focused on voters' beliefs because that is where my experience led me to suspect that irrationality would be easiest to find. Running parallel tests on consumers' beliefs is the next logical step for research in this area to take.

The challenge is to credibly create questions of comparable difficulty. A determined critic of consumer rationality could probably devise questions hard enough to generate systematic errors similar in size to those I report (Caplan 2002a). He might even make the circular argument that if the errors are not similar in size, the consumers' questions must be too easy. A defender of consumer rationality would face the opposite temptation.

For example, I would like to compare consumers' beliefs about their own budgets to voters' beliefs about the governments' budget. That seems like a reasonable comparison to me, but I doubt that Wittman will be impressed if consumers, unlike voters, know where their money goes. Voters frequently think they spend more on foreign aid than national defense. Will Wittman give consumers credit if they realize they spend more on housing than charity?

I can think of two solutions. One is to use a survey designed for orthogonal purposes. For example, according to the authors of the Survey of Americans and Economists on the Economy (Blendon et al. 1997), their *aim* was not to find the areas where economists and the public disagree the most. They selected topics primarily because they had long been in the news and popular discussion. A survey of consumers' beliefs with a similar motivation might let us sidestep controversy about question selection.

Another approach is for scholars who disagree about consumer rationality to join forces to write a mutually acceptable survey. Perhaps it is naive to hope for a meeting of the minds, but it is easier to reach a consensus *ex ante* than *ex post*. A test of consumer rationality jointly written by Donald Wittman and Bryan Caplan would have more credibility than two studies of this question that we ran independently.

Readers may be wondering: Hasn't behavioral economics (Rabin 1998; Thaler 1992) already marshaled a great deal of evidence against consumer rationality? Unless he has changed his position, however,

Wittman is *less* impressed by this literature than I am (Wittman 1995, 38-61). My main reservation about behavioral economics is that experimental conditions are usually too different from real-world conditions to confidently move from one to the other. In contrast, Wittman calls it a “hodgepodge of contradictory results rather than an intellectual foundation” (1995, 38). Since his response to my research on voter irrationality is markedly less negative, perhaps he should tentatively adopt my view that consumers are more rational than voters, pending the arrival of better data.

Wittman’s Eight Hypotheses

Wittman is a constructive critic. He does not merely argue that I am mistaken; he advances eight testable hypotheses to help resolve our dispute. Since he has gone to this trouble, I now consider their merits one by one.

Hypothesis #1: Those people who overestimate the cost of a program (say foreign aid) are more likely to be against the program than those people who underestimate it, both before and after they are given the true facts of the situation.

The “before” part is trivial. Obviously, the worse a person thinks a program is, the less he supports it. The “after” part is not trivial, but I have some concerns. First, one of my main claims is that people irrationally fail to change their beliefs when you give them the “true facts.” Indeed, unless you have a solid gold reputation, perfectly rational agents could easily doubt that you really are the bearer of true facts. So Wittman’s hypothesis should be revised to say “after they *accept* the true facts of the situation.”

My other concern is that hypothesis #1 is weaker than the surrounding text, where Wittman suggests that debiasing will have *no effect* on policy preferences. Existing evidence contradicts this claim; all else equal, voters who know more want systematically different policies. (Althaus 2003) But as written, hypothesis #1 requires only that debiasing fails to make the policy preferences of the initially biased *match* those of the initially unbiased. I do not strongly disagree with this weaker claim, nor does my theory commit me to it. Maybe support for protection is 50 percent due to irrational beliefs, and 50 percent due to expressive voting (Brennan and Lomasky 1993).

In sum, hypothesis #1 is *not* inconsistent with existing evidence, but it is not inconsistent with my position either. If Wittman revises hypothesis

#1 to state, “people will not change their policy preferences after debiasing,” I disagree. But existing evidence already shows that I am right.

Hypothesis #2: Republicans are more likely to overestimate the cost of welfare than Democrats.

This hypothesis is trivial. Almost every model predicts it. For example, if voters have rational expectations about the cost of welfare, some will overestimate it and others will underestimate it. The over-estimators will be more likely to support the party that wants less welfare; the under-estimators will be more likely to support the party that wants more welfare.

A non-trivial substitute for Hypothesis #2 is that, “Independent voters will have unbiased beliefs about the cost of welfare.” I predict the opposite. But to be fair, existing data already confirm that I am correct.

Hypothesis #3: Voters who are strongly in favor of one candidate are likely to have biased beliefs favoring that candidate, but when such voters are informed of the truth, they are unlikely to prefer the other candidate.

This hypothesis suffers from several ambiguities. Again, instead of “informed of the truth,” Wittman should say “accept the truth.” Furthermore, does “the truth” mean the truth on *one specific issue*, or *overall*? If supporters of a candidate abandon their false beliefs about one minor issue, for example, I would not expect many to switch teams either. On the other hand, I definitely predict that correcting many small biases, or a few important biases, would have an effect on voter choice. Bartels (1996) already uses the enlightened preference method to confirm that voters’ false beliefs have a substantial effect on vote shares. And as stated earlier, this underestimates the effect of voters’ biases because politicians would adjust their platforms to appeal to a more rational electorate.

One related test that Wittman does not consider would be to study people who actually change their minds about factual issues, and see whether they subsequently change their vote. Take, for example, Republicans who used to think that Iraq had WMDs, but changed their mind before the election. Were they more likely to vote against Bush than other Republicans, controlling for other variables (including strength of partisanship)? This issue is sufficiently important that I predict they would. Does Wittman disagree?

Hypothesis #4: When the cost of a policy increases, voters on average will be less likely to vote for the policy.

This hypothesis suffers from an ambiguity between objective and subjective cost. I agree that when the *perceived* cost of a policy increases, voters will be less likely to support it. For Wittman to distinguish our positions, he would have to predict that when the *actual* cost of a policy increases, voters will be less likely to support it. Even then, however, this is not a good test of my approach.

First, I am not aware of any data suggesting that voters have biased beliefs about *changes* in costs. It would not surprise me if they did, but this should be confirmed before we test hypothesis #4.

Second, even if voters have highly biased beliefs—for example if everyone believes that cost changes are twice as large as they really are—their behavior passes Wittman’s test. To fail his test, changes in perceived and actual costs would have to be negatively correlated! But milder biases still imply inefficient choices. If the price of gas rises by 25 cents, but you believe it has risen by fifty cents, you reduce your consumption by an inefficiently large amount—without violating the law of demand. Similarly, if the price of prescription drugs rises by one percent, but voters believe it has risen by two percent, they reduce their support by an inefficiently large amount. Wittman’s fourth hypothesis sweeps these inefficiencies under the rug.

Hypothesis #5: Voters do not have significantly more money illusion than consumers and workers.

Fair enough. I predict the opposite, and agree to become marginally less confident in my overall position if proven wrong. Of course, since this is only one topic, it would not be reasonable for either Wittman or me to surrender to the other on the basis of this test.

Hypothesis #6: Scan the brain and see whether voters use more primitive centers of the brain when voting than when making purchases.

Despite the rising profile of neuroeconomics (Camerer et al. 2005), this test raises more questions than it answers. Wittman of all people should presume that the division of cognitive labor in the brain is functional. If we use our “primitive centers” to form political beliefs, why not conclude that these are the optimal centers to use for this purpose? It is at best premature

to equate the output of the “primitive centers of the brain” with irrationality. In fact, since lower animals do not hold political ideologies, my guess is that ideological thinking uses centers of the brain unique to man.

In proposing this test, Wittman makes a fascinating admission. “One would have to control, however, for the possibility that people get more excited about politics than about what clothes to wear (at least this is true for the people that I know).” It is true for the people that I know, too, but we should not control for it. Maybe the *reason* why people hold irrational political beliefs is that they let their excitement cloud their judgment.

Wittman’s last two hypotheses refer to the following proposed experiments.

In experiment 1, the subject gets the payoff from A, B, C or D if he chooses A, B, C or D. Further, he gets the highest monetary payoff if he chooses D, but somehow the experiment is designed so that it takes complicated logic for the person to understand that the choice should be D. In experiment 2, the subject gets the payoff from A, B, C, or D if a majority chooses A, B, C or D. (Wittman 2005: 30)

Hypothesis #7: The majority decision will, on average, be more accurate than the individual decision.

Hypothesis #8: The larger the number of potential voters, the more accurate the decision is likely to be. If Caplan’s argument is correct, then individuals will be more irrational because they are less likely to have an effect on the outcome.

This is a valuable approach, but I have a major reservation. People are far less likely to have strong emotions about choices labeled A, B, C, or D than they are about real political issues such as tariffs and the minimum wage. In fact, even if you used familiar emotionally-charged labels, they would not pack their usual punch in an experimental setting. I abhor murder in real life, but have slaughtered millions in computer games.

If experimental subjects lack strong emotions about their choices, I find Wittman’s prediction plausible. The average guess of the weight of an ox is notoriously accurate, and large groups are more accurate than small groups (Surowiecki 2004, xii-xiii). Wittman’s proposed experiment makes beliefs about policy no more emotionally engaging than beliefs about the

weight of an ox. It would not be surprising if democracy performed well in this case.

Still, there is a variant of Wittman's experiment that I would find probative. Pick topics where we *already know* that beliefs are biased, even *under experimental conditions*. Monty Haul's Three Doors problem is a good example (Friedman 1998). Give subjects the opportunity to either have fun or research the question—for example, by giving them a half hour of Internet access. Do not pressure subjects to solve the problem; for example, tell them “When you are satisfied that you have the correct answer, feel free to browse the web until your time is up.” Then collect their answers. I predict that the experiment with individual rewards will reveal substantially less biased beliefs than the experiment with collective rewards. I also predict that democracy will do worse as the number of voters rises.

One last worry about Wittman's experimental design is that it seems to *simultaneously* test (a) the relative merits of markets versus democracy *and* (b) the relative merits of plurality rule versus proportional representation.⁹ Suppose that consumers are substantially more accurate than voters. When people choose for themselves, 40 percent choose D, and A, B, and C get 20 percent each; when they vote, 28 percent choose D, and A, B, and C get 24 percent each. Under proportional representation (voters get 28 percent of the D payoff, plus 24 percent of the A, B, and C payoffs), markets have higher average payoffs than democracy. Under *plurality rule*, however, democracy has a perfectly efficient outcome, because everyone gets 100 percent of the D payoff.

Wittman might reply: “That's my whole point.” But the putative aim of his experiment is to test whether consumers are more rational than voters, not whether democracy out-performs markets holding rationality constant.¹⁰ More importantly, plurality rule yields bimodal outcomes; it is easier to get 100 percent efficiency, but also easier to get 0 percent efficiency. If voters were systematically biased against D, then plurality rule

⁹ Since there are four options in Wittman's proposed experiment, he should have said *plurality* rule, not majority rule.

¹⁰ Wittman defends the latter position elsewhere: “False political advertising may fool a minority, yet it will have no harmful effect since votes for the minority will not be translated into political power. In contrast, a business does not have to persuade a majority of consumers, only a few, to have any sales. So a majority may want to protect a minority in the commercial arena” (1995, 16-17). Wittman's conclusion would reverse, however, if the majority were wrong. Then markets at least let the rational minority take the optimal action, but democracy forces a bad decision on everyone.

would *never* select it, leading democracy to perform worse than markets even if consumers are equally biased. The simplest solution is to use proportional representation, not plurality rule, to rate democracy's performance.

Some of Wittman's hypotheses are trivial or irrelevant, some are already known to be false, some would be good after reformulation, and one is fine as written. By no means is that a bad track record; creating good testable hypotheses is not easy. But before pursuing any of Wittman's hypotheses, we should heed his simpler exhortation—test voter rationality and consumer rationality in the same way. Existing research confirms the reality of large systematic biases about policy-relevant questions. For purposes of comparison, the next step is to write a good test of consumer knowledge, and see if large systematic biases frequently emerge there too.

CONCLUSION

Wittman (2005) gives a great deal of ground, but it is not obvious whether he has really changed his mind, or is playing devil's advocate. The answer is probably a mix of both, but he makes important revisions that appear genuine. In particular, Wittman retreats from the rational expectations assumption that drives so many of his results. When Wittman (1995 and 1989) challenged the political failure literature for assuming "voter irrationality," his evidence was that voters need biased beliefs to generate the standard conclusion. But now he says,

The major method of testing rationality of consumers is via comparative statics and in particular the test of (weakly) downward sloping demand. This should be the prime method of testing rationality of voters, as well. (Wittman 2005, 27)

Wittman used to set the bar of rationality extremely high; now he sets it extremely low. If downward-sloping demand is all that "voter rationality" means, then Wittman's original challenge to the political failure literature was off the mark. Though many models of political failure require what Wittman calls "extreme voter stupidity," I am aware of none that assume that voters want more when the perceived cost of a policy rises.

Wittman's main defense of his original position is that biased beliefs do not change people's policy preferences. While there is a kernel of truth here—a discrete choice is more likely to stay the same when conditions change—it is basically wrong. Discrete choices are also more likely to change sharply when conditions change. The net effect is ambiguous. Furthermore, existing empirical evidence shows that biased beliefs have systematic effects on policy preferences.

Wittman is on firmer ground when he criticizes my views instead of defending his own. Econometric evidence reveals that voters have deeply biased beliefs about the economy. But are voters more biased than consumers? I *believe* that they are. When I listen to non-economists discuss their consumption behavior, I find them remarkably insightful; when I listen to non-economists discuss policy, I find them disturbingly obtuse. And in my experience, learning economics often drastically changes people's political and economic outlook, but has only marginal effects on their personal behavior. That was how it worked for me.

Still, I can hardly expect my casual empiricism to convince Wittman, and to the best of my knowledge, there is no econometric evidence on consumers' beliefs comparable to my research on voters' beliefs. One response to this lacuna would be to pursue Wittman's eight hypotheses, but designing and administering tests of consumer rationality to parallel existing tests of voter rationality has a higher rate of return. If Wittman and I can reach a consensus on what these tests should contain, the answer to one of the biggest of the Big Questions—the relative merits of democracy versus the market—is within our reach.

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[GO TO SECOND REPLY BY DONALD WITTMAN](#)



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Second Reply to Caplan: The Power and the Glory of the Median Voter

DONALD WITTMAN*

CONTINUATION OF THE EXCHANGE BETWEEN BRYAN CAPLAN AND DONALD WITTMAN FROM THE APRIL 2005 ISSUE OF *EJW*.

[Caplan Comment on Wittman \(April 2005\)](#)

[Wittman Reply \(April 2005\)](#)

[Caplan Rejoinder \(August 2005\)](#)

IT IS ALWAYS A PLEASURE TO HAVE THE LAST WORD. I WILL NOT make a point-by-point counter-argument to Brian Caplan's Rejoinder (2005b) because doing so would exhaust my patience, as well as the readers' (but probably not Caplan's). Instead, I will present some general arguments that can be employed in answering a variety of questions. In my response I will: explain why there is a demand for democratic failure theories; predict which voters will appear to act irrationally; explain why evidence of voter irrationality does not imply that government policy is irrational; show why Caplan's argument that voters are rationally irrational when they vote does not conform with the facts; and suggest empirical tests that might be employed to gain greater insight into voter behavior.

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THE DEMAND FOR THEORIES OF DEMOCRATIC FAILURE

Suppose that government policies reflect the preferences of the median voter. Because very few people are actually at the median, most people are, to varying degrees, bothered by government policies. If you are one of the disgruntled, then you face the following conundrum: I know what policy is best, but the government does not choose that policy.

Your solution to this puzzle might be that preferred policies are just revealed preferences and there is no accounting for tastes. Alternatively, you might say that the median voter knows more than you do. But I suspect that the following responses are more commonly employed: (a) the median voter is irrational and/or uninformed or (b) the system does not reflect the *true* median voter's preference, which of course coincides with your own preference. As a result, we find people on both the right and left criticizing the same government policy for being too far to the left and for being too far to the right. So the only thing that both sides agree on is that there is a political failure. Of course, the reasons for the failure are inconsistent (news sources are said to be biased to the right or to the left, depending on one's own political biases). Hence, we have an explanation for the demand for theories of democratic failure.

DIFFERENTIAL RESPONSE TO ELECTION CHOICES

Because voters differ in their preferences, we can generate more interesting hypotheses than the hypothesis that extremist voters are the most disgruntled with government policies. For example, consider the following: A voter whose position is between the candidates' positions needs to gain more accurate information than a voter whose position is extreme. In order to make the correct decision, those in the middle, who are close to being indifferent, will want to get their information from less biased sources or from a basket of sources that are on average unbiased. In contrast, those voters with extreme preferences do not need more information to decide which candidate is best as long as they know which candidate is to the "right" of the other (this information is not sufficient for those in the middle). Such extreme voters are more likely to want reassurance in their beliefs and thus choose biased information as their

source. As a result, voters with extreme preferences may have very exaggerated views on the differences between the candidates, or on how far away both candidates are to the left (or to the right, as the case may be). But given their position, extremist voters would be very unlikely to change even if they were correct in their beliefs.

Despite such “irrational” beliefs held by extremist voters, I will now argue that the election outcome reflects the preferences of the median voter who, as suggested above, is more likely to have unbiased beliefs.

THE MEDIAN VOTER: WHY VOTER IRRATIONALITY MAY BE UNIMPORTANT

In the United States, legislators and presidents are chosen by plurality rule. If one believes in the Downsian model, where candidates are only interested in winning, then elections will tend to elect candidates who are at the median voter. If one believes that candidates have policy preferences, then candidates may move away from the median voter; but the more informed the voters and candidates are, the less the movement away from the median voter will be (Wittman 1983). Let us stick with the Wittman version, but note that in either model the median voter determines the outcome.

The important role of the median (or near median) makes many irrational voter results close to being irrelevant.

Suppose that many citizens are completely uninformed. If these uninformed citizens are rational, then they will not vote (there is strong empirical evidence to support this contention). So the result will be close to the median of the informed voters. Abstentions by the uninformed involve some movement away from the median overall, but perhaps not significantly so.

Suppose instead that these uninformed voters are also irrational—that is, they vote against their own best interests. Unless the voters on one side of the median are significantly more irrational than the voters on the other side, then such irrationality will again have a modest impact on the outcome. Being an uninformed or irrational voter is a lot less dangerous than being an uninformed or irrational consumer because a large number of voters have to be uninformed or irrational in the same way to have an effect on the outcome. As a result, we may expect that there will be more

uninformed and irrational voters, but because of the law of large numbers and the median voter result, the impact of such irrationality is likely to be modest.

Furthermore, because of the discrete nature of the choice, voters on the extreme left or extreme right need know very little, except which candidate is on the left and which candidate is on the right (see Wittman 2005b). Hence, these committed partisans need little information. Indeed, as argued above, they may be wrong about a number of details, but being knowledgeable would not change their choice. So being uninformed—but not completely so—is perfectly sensible when voters have convex preferences. The additional information is extremely unlikely to change the cost-benefit calculations. Of course, the same does not hold for people in the middle (who will want to be informed). So once again, lack of information by many of the voters does not imply democratic failure.

Caplan mentions a number of surveys where the average response is far from the truth (for example, the average voter overestimates the cost of welfare). Caplan argues that this shows that voters do not have rational expectations. Sticking with the median voter model, we can see that even if the average estimate of the cost of welfare is far from the truth, this does not make any difference if the median voter is close to the mark. Suppose that the percent of the federal budget for Medicare is 3 percent. Some people will underestimate it, but such under-estimates are limited to 3 percent; others may overestimate the percentage and some of these overestimates could be 10 percent or higher. As a result, the average could be considerably above the true value, but this is not relevant if the median voter is correct. While the median and the mean often coincide, this seems less likely to be true in the survey questions that Caplan considers.

In a nutshell, the median voter may have rational expectations even if many of the voters are irrational and uninformed and the average voter has incorrect expectations.¹ Thus, survey evidence that that the average voter is mistaken regarding the cost of welfare is useful, but not compelling.

¹ In doing empirical work one should not conflate the median voter with the independent voter. Some voters are between the preferences of the candidates and therefore might choose to not be affiliated with either political party. Other voters don't care much about electoral politics and therefore might not bother to know much of anything about politics including which party best represents their interests. These voters too are independents.

WHAT DO THE DATA MEAN?

I have no doubt that there are cases where even the median voter is mistaken. The question is what to adduce from these examples. A guide (whether good or bad) is to see how we deal with the analogous situation in economic markets. As I noted in my first reply, many consumers seem irrational or misinformed when they buy Ginkgo Biloba and Echinacea. Yet I doubt that many economists would conclude from this evidence that economists should give up on the standard model of rational consumer behavior.

To gain additional insight, let us consider stock market behavior. According to economic theory the expected price of tomorrow's stock is equal to today's price.² If we were to look at a time series of stock prices and regress stock price for day t on the price of the stock in day $t-1$, the coefficient of yesterday's stock price would not be 1 as predicted by the theory, but *significantly* different from 1 (even when accounting for the implied interest rate).³ Should we conclude that investors are irrational and that psychologists and sociologists should be teaching finance instead of economists? I think that the answer should be no. The contrary theory, irrationality, does not give us any precision.

Next suppose that there was some empirical regularity such as the February effect where the rate of return was higher for stocks on the American Stock exchange that were purchased in February. Furthermore, suppose that some psychologist came up with a theory to explain the February effect (perhaps people were more optimistic after Valentine's day). Even if the results were statistically significant, we would not consider this as a general theory of stock behavior, because the data was specifically selected to test the theory.

Now let us turn to politics and apply the same standards. Suppose that rational voter theory predicts that the median voter's perception should be an unbiased estimate of the true expenditure on a government program. Suppose that the median voter's beliefs were significantly different from the

² Because of the limited resources and risk aversion, the price of a stock depends on marginal buyers and sellers not the average buyer and seller of stock (even after weighting for the amount of stock ownership). So here too, the outcome depends on the pivot rather than the average.

³ The intercept would also be significantly different from 0 contrary to the theory. I consider the coefficient to be close to 1, but the standard for being close may vary across individuals and across situations.

true value. Does this imply that we should throw out the rationality theory for the uninformed irrational voter hypothesis? No, for the same reason we did not throw out the efficient market hypothesis when the coefficient of yesterday's price is significantly different from 1. The alternative theory of voter irrationality is unlikely to give us any precision. So voter rationality may be the best explanation going even if it does not always give us accurate predictions.

And of course it is statistically incorrect to prove one's case by choosing examples to fit one's arguments, even if the examples are "statistically significant." This is not to say that promoters of rational voter behavior have not data mined, as well.

ARE VOTERS IRRATIONAL BECAUSE THEIR VOTE DOES NOT COUNT?

Because individual voters have virtually no effect on the outcome of the election, there is little cost to their being irrational when they vote. Caplan argues that this implies that voters do in fact act irrationally. In both essays, Caplan (2005a, 2005b) refers to the work on expressive voting by Brennan and Lomasky (1993). Their argument is that voters treat voting as being expressive rather than instrumental. While the connection between expressive voting and being irrational is tenuous (one could be irrational without being expressive and vice versa), I too will consider expressive voting.

There are several steps in the logic of Caplan's argument that can be subjected to empirical investigation. The first and perhaps most important step is whether voters act as if their vote does not count.

The evidence is to the contrary—most voters treat voting as instrumental—that is, they act as if their vote counts. In the United States, only a very small percentage of people vote for a candidate other than the two leading candidates. Very few people waste their vote by expressing their preference for a third candidate even if they prefer this candidate (of course, often there is not a third candidate in the first place, precisely because voters are so instrumental). It is true, that in the 2000 election enough people voted for Nader to cause Gore to lose the election. But in the 2004 election, the argument that the democratic candidate was no different from Bush no longer held sway, and as a result very few voters

voted for Nader even though Kerry was no farther to the left of Bush than Gore had been. Even if we were to say that Nader voters were irrational in the 2000 election, they certainly learned their lesson fast. So the evidence suggests that an overwhelming majority of voters act as if their vote counts. The evidence is in favor of voters being instrumentally rational and against their being rationally irrational.

As I suggested in my previous reply, whether individuals act more irrationally when their vote has little probability of affecting the outcome can easily be tested in an experimental setting. One can have the subjects vote (or abstain) on a logical or empirical question. The experimenter can then observe whether the median voter's accuracy deteriorates as Caplan argues or improves as I argue as the size of the electorate increases (say from 3 to 5 to 15 to 100 voters). I think that we all know the answer to this even in the absence of an experiment.

As I understand it, Caplan does not like this test because the voters are not emotional and irrational about logic, but they are emotional and irrational about politics. Presumably, the most emotional and irrational voters are those in the extreme, particularly those who vote for candidates, like the libertarian candidate for president, who has no chance of winning.⁴ From the opposite perspective, the median voter is likely to be the most rational. And, if we believe the median-voter models, then the irrationality of the extremes has at best a modest effect.

EMPIRICAL TESTS

Caplan raised two general issues in his original critique: (1) the existing empirical evidence shows that voters do not have rational expectations and (2) more empirical work on voter behavior is needed. With regard to the first point, I have argued both here and in my previous response that Caplan's evidence is not persuasive.

I agree with Caplan's second point and therefore I made an effort to devise some interesting empirical hypotheses about voter behavior. These empirical hypotheses were inspired both by Caplan's rational irrationality hypothesis and by my argument that candidates try to appeal to the median voter. Indeed, the empirical hypotheses concerning extreme players are

⁴ In case you miss the point of this sentence, check out Caplan's webpage.

generated from the fact that median voters are the pivotal players and close to being indifferent between the two candidates while extreme voters are neither pivotal nor close to being indifferent and therefore bias is not costly.

Caplan finds something wrong with almost all of my proposed tests. I suppose that this is inevitable whenever two people have opposing views. They cannot even agree on a test. Nevertheless, in response to his criticisms, I have tried to adjust some of my hypotheses to take into account his concerns.

Hypothesis #1: Those people who overestimate the cost of a program (say foreign aid) are more likely to be against the program than those people who underestimate it, both before and after they are given the true facts of the situation.

Caplan argues that the respondent might not believe that the true facts are indeed true. In order to get around this issue, I propose the following alternative version to #1.

Hypothesis #1':

- A. Ask subject the cost of a federal program (say foreign aid).
- B. Ask subject how strongly the subject supports the program: (2 strongly positive; -2 strongly negative).
- C. Ask subject whether he/she would support the program if the cost were \$X. (X is the true cost of the program, but the subject is not informed that this is the case).

My hypotheses are:

- a) That the stronger the support, the lower the estimate of the cost of the program. That is, A and B are negatively correlated; and
- b) Those who strongly overestimate (underestimate) will be less likely to change their mind given the conditional statement in C.

Hypothesis #6: Scan the brain and see whether voters use more primitive (and presumably more emotional and less rational) centers of the brain when voting than when making purchases.

Caplan argues that irrational ideology arises from the more advanced centers of the brain because animals do not have political ideology. Why don't we do a brain scan and see what differences there are, if any, between voting and buying (or solving crossword puzzles)? Caplan and I can fight about the interpretation later.

I have suggested that, other things being equal, voters who are close to being indifferent are more likely to seek (unbiased) information than voters who are strongly partisan. This too could be tested.

CONCLUDING REMARKS⁵

In this paper, I have argued that the median voter is rational even if other voters are not. For a variety of reasons, including the difficulty of deciding what is rational (see Wittman, 2005a) and the lack of convergence on this issue between Caplan and myself, I believe that this debate will not be resolved in the near future.

In this and the previous response, I have tried to move the debate about voter rationality to a different level by suggesting a number of new hypotheses about voter behavior. Some of these hypotheses are only indirectly related to the question whether the median voter has rational expectations. I hope that these hypotheses will inspire others to conduct empirical research along these lines, as I have more confidence that such research will be conclusive (one way or the other).

⁵ Caplan's concluding statement says that he and I are engaging in a debate about the relative merits of democracy versus economic markets. I do not agree. I have never argued that democratic markets are in general superior to economic markets. Instead, I have argued that democratic markets will tend to let decisions be made by economic markets when economic markets provide a superior outcome.

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INTELLECTUAL TYRANNY OF THE STATUS QUO

Gold Standards and the Real Bills Doctrine in U.S. Monetary Policy

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[Abstract, Keywords, JEL Codes](#)

[The English gold standard after Waterloo] was a perfectly 'free' or 'automatic' gold standard that allowed for no kind of management other than is implied in the regulatory power of any central bank that is a 'lender of last resort.' . . . [Despite much opposition] the gold-standard policy was never in real danger politically, and if it was not, until much later, adopted by all other countries, [their delay] was not a matter of their choice: in spite of all counterarguments, the 'automatic' gold standard remained almost everywhere the ideal to strive for and pray for, in season and out of season.

—J.A. Schumpeter (1954, 405)

IN RECENT DECADES SEVERAL JOURNAL ARTICLES AND SOME mainline books have appeared blaming what the authors label “the gold standard” for the failure of the Federal Reserve System to pursue a counter cyclical monetary policy that would have prevented the Great Contraction of 1929-1933, and the subsequent Great Depression of 1933-1941. While the authors of these publications note differences between the classical pre-World War I gold standard and the post-World War I gold-exchange standard, they nonetheless claim that the latter “gold standard” was operational during the 1920s and early 1930s. They insist that significant

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changes in the quantity of money, or the lack of such changes, were dictated by fixed values of gold for the units of account, and that this restriction was responsible for the misconduct of monetary policy during the period. They seem unaware that if central bankers are managing a “gold standard” in order to control monetary policy, the words “gold standard” do not apply.

These authors also seem to understate the extent to which the Fed and other central banks’ deliberate “management” of the gold-exchange standard prevented monetary adjustment in the period from 1929 to 1933 from resembling the pattern of equilibrium typical of the classical gold standard. Indeed, none of the “gold standard” critics specifies the attributes of a true—classical—gold standard. Nor do any of them make any reference to the legal provisions in the Federal Reserve Act that the Federal Reserve Board could have used to abrogate the gold reserve requirements for Federal Reserve Banks, or to the fact that *all* Fed-held gold was on the table in a crisis. Most importantly, none of these publications includes any reference to the real culprit in the monetary machinery of that era—the Real Bills Doctrine, which was then the working blueprint of the primary policymakers in the Federal Reserve System.

This paper seeks to rectify these errors of commission and omission for the sake of historical accuracy. I do not lobby for any particular monetary policy or system. However, until the policy history of such an important episode as this one is properly analyzed and understood, the general public, including its representatives in Congress, is being misled, and is understandably confused. Policymakers, likewise, are forever in danger of repeating past mistakes or inventing new ones.

THE CONSTITUTIONAL GOLD STANDARD

Joseph Schumpeter’s observation about the gold standard that the English crown restored between 1819 and 1822 reflects the high esteem in which the ‘world’ held the operational, automatic gold standard. Somewhat relaxed lending policies by the Bank of England, after Parliament ordered the Restriction of gold payments in 1797, had allowed the market price of gold to rise above its mint price. But after the travail brought about by the Napoleonic Wars, Parliament prescribed policies that eventually restored the pre-war gold parity of the pound sterling.

A few years earlier, the Constitution of the United States declared that Congress should have the power, “To coin money, regulate¹ the Value thereof . . . and fix the Standards of Weights and Measures.” It further stipulated, in Section 10 that, “No state shall . . . coin Money; emit Bills of Credit; [or] make any Thing but gold and silver Coin a Tender in Payment of Debts.” These few sections provided for a bimetallic monetary standard in the United States.²

To make a metallic standard operational, a legislature must follow certain principles and procedures. First, it must specify the value of the unit of account in terms of a weight of gold (and/or silver). It does so by prescribing a gold coin of a convenient denomination. For the United States, Congress defined the gold dollar as 24.74 grains of pure gold. The basic gold coin it authorized was a ten-dollar gold Eagle that contained 247.4 grains of gold, with an additional ten percent base metal to make the coin suitable for practical use (Bordo 1997, 264; Officer 2001). Other gold coins were proportioned by weight in the appropriate denominations. These coins were legal tender for all debts private and public, and, by the proscription of Section 10, nothing else, except silver, would be so privileged.

A legal tender specification for a weight of gold initiates a gold standard. Having done that, the government need do nothing more than subject itself to the dictates of the standard. It may produce legal tender gold coins, or it may leave the coinage of money entirely to private coin smiths.³

Once a metallic standard is in place, the institution becomes self-regulating. Individuals, banks and other financial institutions, business firms, foreign exchange dealers, and the world’s gold industries unwittingly

¹ Choice of the verb “regulate” was unwise. Given the principles of a metallic standard, and the proximity of the clause, to “fix the standards of weights and measures,” “regulate” could only mean to *specify* marginal adjustments of their metallic contents in order to keep both gold and silver moneys in circulation. Denominational problems were critical in many regions during the nineteenth century. For more detailed arguments on this issue, see Timberlake (1993, 2, 414, and passim).

² For simplicity’s sake, I use the term “gold standard” as a proxy for a “metallic standard,” with the understanding that a gold and silver bimetallic system included two metals with legal tender properties. In what follows, reference to a “bimetallic standard” would needlessly burden the exposition.

³ During the first half of the nineteenth century in the United States, more than a dozen coin smiths produced legal gold coins. Some were above the legal standard for purity and weight. Private minters also produced lower denominational currency at opportune times (Cribb, Cook, and Carradice 1990; Timberlake 1993, 118-128, and passim).

cooperate to make the system work. Other conditions are also necessary or desirable: the supply of common money that banks and individuals generate on the gold base must be responsive to the quantity of monetary gold; market prices must be sensitive to changes in the quantity of money; and gold must be allowed to flow freely in and out of the economy in response to private initiatives (Hepburn 1924, 482-486; Timberlake 1993, 2-4; Officer 2001).

A true gold standard is a complete commodity-money system and, therefore, has an appeal not found in some other monetary arrangements. Under an authentic gold standard, the demand for, and supply of, money react simultaneously, through market prices for all goods and services and the monetary metal, to determine a given quantity of money. If prices of all goods and services and capital tend to fall, say, because of an increased demand for common money, the value of monetary gold being fixed in dollar terms rises in real terms, stimulating increases in the production and importation of gold, and the supply of gold to the mints. Since gold is the necessary base for currency and bank deposits, the quantity of common money also increases arresting the fall in market prices. Alternatively, when additional gold enters the monetary system from whatever source, it tends to raise money prices. Offsetting the potential price level increase are the nominal increases in goods, services, and capital that normally occur. In either case, successive approximations of goods production and money production through the market system generate an ongoing monetary equilibrium.⁴

Frederick Soddy, a chemical engineer interested in applying scientific principles to monetary phenomena, observed that under an ideal gold standard system the “proportionate increment of the [economy’s real] revenue . . . [is] always as great as the proportionate increment of its aggregate quantity of gold” (Soddy 1933, 179). While the world’s gold mines could not be counted on to satisfy this norm precisely, gold prices of

⁴ A. Piatt Andrew explained the operation of a gold standard within a market system of prices as follows: “In the case of gold, the amount that will be produced, the amount that will be imported, and the amount that will be coined evidently depend upon its value. A change in the general price level in such a case obviously is apt to be the cause as well as the effect of changes in the quantity of money. It is equally true in the case of wheat or iron or cotton or any other commodity. The value at a given moment depends upon the quantity that has been produced, imported and manufactured in the past, yet at the same time the present value acts as cause with regard to the quantity to be produced, imported and manufactured in the future. Value is thus almost always the cause as well as the effect of changes in quantity” (1905, 115).

commodities over the centuries have been extraordinarily stable (Jastram 1981, chart #1, 9f).

A true gold standard provides an economy with a set of rules prescribing the conditions for the supply of common money. Once the rules are in place, the system works on the principles of a spontaneous order. Human design is limited to the framework for the standard, and must refrain from meddling with the ultimate product—the quantities of both base and common money.

Fractional reserve commercial banks, operating within a gold standard system, create non-gold notes and deposits as a by-product of their lending operations. They knowingly accept the fact that they must be able to redeem the common money they create with the gold reserves they retain. As cost-recovering competitive enterprises seeking to stay in business, they must judge accurately the proper quantity of gold reserves necessary to support their demand obligations if they hope to maintain convertibility of their notes and deposits into gold.

Departures from true gold standards tended to occur when governments that had initiated such standards began to issue paper moneys. A government's money, unlike that of a competitive commercial banking system, attempts to mimic or rival gold. If its paper currency becomes irredeemable and its metallic currency is underweight, a government using its power of *fiat* ("Let there be") declares its money to be *legal tender*. People then must accept it, willy-nilly.

THE U.S. TREASURY GOLD STANDARD

Congress revoked the operational gold standard for an indefinite period on 30 December 1861. It then passed *Legal Tender Acts* in 1862-1863 authorizing the U.S. Treasury Department to issue \$400 million of United States notes—"greenbacks," plus some other fiat currency. By 1870, outstanding Treasury currencies were five times the amount of bank-held specie in 1860. Currency and bank deposits over the same period increased to about \$1,300 million, or roughly two-and-a-half times their total in 1860 (Friedman and Schwartz 1963, 704; Timberlake 1993, 90, 105).

Prices, including the market price of gold, also increased substantially during the war. However, by 1870 the federal government's post-war monetary policies had brought the price level back down to 145 percent of

its 1860 level, while the price of gold was down to 120 percent of its pre-war parity (Hepburn 1924, 225-227; Timberlake 1993, 111). The gold standard, however, was still in remission. Government policies centering on the Treasury's issues of greenbacks ruled the monetary system and determined the course and magnitude of price level variation.

Treasury currency did not end with the greenbacks. In 1863 Congress passed the *National Currency Act* (amended in 1865), which created a national banking system under the administration of the Comptroller of the Currency in the Treasury Department. Banks that joined the system could issue national bank notes that were legal tender for all dues to, and payments from, the federal government, and they also became the fiscal depositories for Treasury balances. The U.S. Treasury now controlled, within legislative limits, all U.S. paper currency, and had important regulatory powers over a large component of the banking industry.⁵

As if two new government-controlled currencies were not enough, silver currency, which, like gold, had gone out of circulation owing to its wartime price rise, came back into the monetary picture. Major silver discoveries in the West in the mid-1870s, and the abandonment of silver as a legal tender monetary metal in several European countries and the United States,⁶ started an ongoing decline in silver's world price. Despite U. S. silver purchase legislation in 1878 and 1890 in support of silver, the price of silver declined through the 1880s and 1890s, and continued downward throughout the twentieth century (Friedman and Schwartz 1963, 111; Timberlake 1993, 222; Jastram 1981, chart, 9f).

Silver's falling world price meant that it was also becoming cheaper relative to gold, with which it had an artificial "legal" mint price. Circulation of silver currency could occur at face value only because the U.S. government limited its monetization and distribution, and because the U.S. Treasury held a gold reserve against outstanding silver currencies.

Consequently, by the 1880s, the U.S. Treasury Department was an overseer, custodian, and regulator of three fiduciary currencies, against which it held fractional gold reserves. The Gold-Silver Bimetallic Standard had become a U.S. Treasury Gold Standard.⁷ By their fiscal policies, Congress and the Treasury made more or fewer government bonds

⁵ State banks still existed, but could no longer issue state bank notes because of a prohibitive excise tax (10 percent) on their issue.

⁶ Due to the Coinage Law ("Crime") of 1873, the silver dollar was no longer a freely minted legal coin in the United States.

⁷ As this label suggests, "the" gold standard has many variants. More appear below. Milton Friedman treated their fundamental differences for policy purposes (Friedman 1961, 61-79).

available as collateral for national bank notes, and determined the quantity of silver money that went into circulation or was stored in government vaults. Finally, Treasury fiscal operations could alter, in some degree, the quantity of greenbacks in its vaults and, therefore, the complementary amount of greenbacks in the private economy.

The Treasury’s reserve of gold and silver coin and bullion was now the ultimate monetary base on which the Treasury gold standard functioned. Treasury currencies included greenbacks, national bank notes, silver coin, and gold and silver certificates (Table 1).⁸ Some of this total was in the commercial banking system, where it served as a basis for conventional bank credit and deposits.

Table 1. Total gold and silver coin and bullion in Treasury, total Treasury currency outstanding, and ratios of the former to the latter, 1880-1900. (Figures in parentheses are Treasury gold and its percentage of total Treasury currency in circulation.)

Year	Treasury Gold and Silver (Gold) <i>%millions</i>	Total Treasury Currency Issues <i>\$ millions</i>	Ratio (%)
1880	204 (139)	690	30 (20)
1885	410 (247)	854	48 (29)
1890	569 (311)	959	59 (32)
1895	412 (126)	999	41 (13)
1900	703 (435)	1317	53 (33)

Source: Reports of the Treasurer, 1895-1901. “Treasury gold and silver” includes the Treasury’s silver bullion at market prices. Data are for June 30.

In the early 1890s, the Treasury experienced a common problem of the era—managing redeemable paper currencies with fractional gold reserves. During the five years from March 1887 to March 1892, total Treasury currency in circulation increased by 29 percent, or slightly more than five percent per year. This modest increase, however, was enough to cause U.S. prices to rise relative to ‘world’ prices. Consequently, gold exports began during 1892 and continued for the next four years, accompanied by much hand wringing and complaining on the part of government policymakers and commercial bank managers. The Treasury sold U.S. securities to acquire gold so that it could go on redeeming its

⁸ Congress implied by Resolution that the Treasury should maintain a reserve of \$100 million in gold against its outstanding currency obligations (Hepburn 1924, 239).

outstanding currencies (Hepburn 1924, 348-360; Friedman and Schwartz 1963, 104-112).⁹ The Treasury's *gold* reserve-to-currency ratio, which had reached 36 percent in 1888 and was 32 percent in 1890, fell to 13 percent in 1895. Similarly, the dollar value of gold coin and bullion in the Treasury, which was \$311 million in 1890, declined to \$126 million in June 1895—a decrease of 60 percent. It fell to its lowest value of \$42 million in February 1895 (Hepburn 1924, 358).

Treasury policies at this time emphasize that its role as a quasi-central bank managing a paper currency had not overridden its responsibility as the overseer of the gold standard. The Secretary of the Treasury did not prohibit Treasury gold from going out into the world to make the adjustments that had to occur in the world's monetary systems. The cost of the Treasury's passive stance was a modest 10 per cent fall in U.S. prices, and a minor recession (Friedman and Schwartz 1963, 134). Over time, the decline in world prices, stimulated world gold production (Hepburn 1924, 360). The Treasury's gold balance, which had fallen so alarmingly through early 1895, then increased to \$435 million, or by 346 percent, by 1900.

The replenishment of the Treasury's gold stock, and bountiful world gold production, prompted the United States Congress to abandon bimetalism in favor of a monometallic gold standard. This change occurred with the passage of The Currency Act of 1900, 14 March 1900, often referred to as "The Gold Standard Act." Silver was officially reduced to a subsidiary currency, and was, for a time, no longer an important source of monetary controversy.

INFLEXIBILITIES IN THE COMMERCIAL BANKING SYSTEM

Government officials, economists, and bankers, however, constantly decried the apparent inflexibility of commercial bank operations—the

⁹ Grover Cleveland, who was a classically liberal, gold standard Democrat, stubbornly insisted on the maintenance of gold payments. He and his Administration realized that to promote this end the ongoing monetization of silver had to cease. Acting in his Executive capacity, Cleveland and his Democratic Party affiliates in Congress were able to repeal the Sherman Silver Purchase Act in its entirety by November 1893. The political struggle was extremely bitter, and cost the Democrats control of Congress and the Presidency in the 1896 election (Timberlake 1993, 166-179). By the terms of the Repeal Act, the Treasury retired silver currency as it came into Treasury offices as payment for taxes or tariffs.

banks' inability to furnish an "elastic" currency that would gear issues of money to the production of goods and services, and also provide liquidity to business firms in a panic. This lack of adequate monetary elasticity was largely traceable to policies that fixed the stock of greenbacks and tied the amount of bank currency (national bank notes) to the outstanding quantity of government securities. Reserve requirements, prohibition of branch banking, and other legal restrictions also contributed importantly to monetary inflexibility (McCulloch 1986, 79-85).

To adjust to these institutional rigidities, bankers extended the operations of their clearinghouse associations at critical times. The clearinghouse loan committee served as the surrogate of a central bank lending authority by discounting the conventional interest-bearing paper of participating banks. On the occasion of a panic, the clearinghouses issued Clearinghouse Loan Certificates that served as quasi-legal tender bank reserves until the panic abated. By 1907, however, clearinghouse associations were issuing all kinds of currency, many in the lowest denominations (Andrew 1908, 496-502). No losses of any significance ever occurred as a result of clearinghouse issues (Timberlake 1993, 198-212).

The success of the clearinghouse system emphasized the primary limitation that brought it into being—the largely unusable, legally required reserves in the central reserve city banks of New York and Chicago (Sprague 1910, 278-280; Andrew 1905, 111-115). The very act of setting a specific ratio for bank reserves implies that a bank must never breach this minimum. On this account, the bank's reserves below the minimum become virtually unusable. Reserves that should be a cushion, and allowed to vary with circumstances, become a line in the sand that the bank dare not cross. Consequently, banks feel obliged to keep higher-than-required reserve ratios because of the critically adverse effects they would suffer in the event their reserves fell, even temporarily, below the specified minimum.¹⁰

By this time, three institutions had entered the picture as possible or actual lenders of last resort: First, the major national banks in New York City that had lots of reserves, but were precluded from using them because of reserve requirements; second, the clearinghouse associations, which were also a part of the national banking system and were located throughout the

¹⁰ Deane Carson observed in an article written in 1964: "Bankers . . . consider *legally required reserve* balances as the most illiquid segment of their asset portfolios, useful over long periods only at a penalty rate of interest. . . . Without legal ratios it would appear that the 'liquidity cushion' aspect of cash reserves would be enhanced" (Carson 1964, as republished in Ritter 1967, 250, his emphasis).

country; and, third, the Treasury Department, which occasionally had surplus reserves of gold and other legal tender that it could make available in the “money market.” Following the Panic of 1907, both professional economists and government officials found much fault with the improvised policies of both these latter institutions.

The national banking system, despite its inflexibilities, was still the center of attention for policy adaptation. In the view of many observers, the banking system’s inability to adjust to crises resulted from risk-prone banks that loaned speculatively, or on long-term securities and mortgages. Seemingly oblivious to the excessive legal restrictions on banks, current opinion had it that banks suffered suspensions because they did not pay adequate heed to the commercial credit theory of banking—what came to be labeled in later years, the “real bills doctrine.” This guide to bank operations was paramount in the minds of a large segment of economists, financial analysts, bankers, and legislators. It was the necessary ingredient that had to be built into any institution supplying “credit” or reserves to troubled banking institutions, and it played a particularly important part in the creation of the Federal Reserve System.

THE REAL BILLS DOCTRINE IN THE FEDERAL RESERVE ACT

Professor Lloyd Mints of the University of Chicago concentrated his research career on the theoretical channels by which the real bills doctrine emerged in banking theory. Bankers and economists who subscribed to it, Mints noted, hold that, “if only ‘real’ bills are discounted [by banks], the expansion of bank money will be in proportion to . . . the ‘needs of trade,’ and that, when trade contracts, bank loans will be paid off. . . . I shall designate these ideas as ‘the real bills doctrine’” (Mints 1945, 9).¹¹

Following Mints, Thomas Humphrey has written several articles examining the historical pedigree of the real bills doctrine, and has meticulously dissected the theory behind it. Most importantly, Humphrey

¹¹ Mints derived the more succinct term, “real bills,” from a passage in Adam Smith’s *Wealth of Nations*, in which Smith discusses a bank that “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 (Mints 1945, 27 note)”

has explained, step-by-step, just how it became the model for Fed policy in the 1920s (Humphrey 1982, 2001).

Either gold or bank loans can serve as a basis for money creation. However, the two bases for creating money are fundamentally different. A gold standard monetizes gold on *fixed* legal terms, i.e., so many dollars for so many ounces of fine gold, no matter what the season, the state of business, the needs of the government, the direction of international trade, or any other real life variables. Significantly, no one has ever had to define ‘real gold,’ or decide which ‘real gold’ was ‘eligible’ to be monetized.

Bank monetization of real bills, however, cannot be done on fixed terms. As Mints argued: “whereas convertibility into a given physical amount of specie [gold or silver] . . . will limit the quantity of notes . . . the basing of notes on a given money’s worth of any form of wealth . . . presents the possibility of unlimited expansion of loans” (Mints 1945, 30).

A bank loan to a borrower must always include the banker’s estimate of the dollar value of the real goods or services that the borrower offers as collateral to secure the loan, as well as the likelihood of repayment. The interest rate charged reflects this judgment. If bankers are too optimistic, they overextend credit, thereby oversupplying deposits. New loans and derivative deposits exceed the value of the goods and services that the borrowers can generate, and monetary inflation results. If bankers are overly pessimistic, creation of bank money is insufficient to maintain prices at their current level, and deflation follows. These rising or falling prices raise and lower the nominal value of the real collateral that constitutes the basis for the creation or destruction of bank money. The system, when put into motion, does not move toward equilibrium. Humphrey emphasizes this *dynamic instability*. “Because it ties the nominal money supply to a nominal magnitude that moves in step with prices,” he observes, “the real bills doctrine provides no effective constraint on money or prices” (Humphrey 1982, 5. See also, Girton 1974, for an analysis of the theoretical conditions of instability).¹²

¹² Humphrey reviews the German Reichsbank’s “astronomical” issues of money in 1922-1923 as a real world example of inflationary instability due to real bills lending (Humphrey 1982, 3). Yeager (1966) also cites this example. The author of the report, which appeared in a League of Nations study, was Ragnar Nurske, who commented that, “Havenstein, President of the Reichsbank, in so far as he had any theoretical notions at all, adhered to a form of the ‘banking principle’ which told him that the rise in prices created a need for money on the part of business men as well as the government, a need which it was the Reichsbank’s duty to meet, and which it could meet without any harmful effects” (Yeager 1966, 271).

Fortunately, a genuine gold standard will not allow banks to generate too much or too little money for very long, no matter how much credence bankers attach to the real bills doctrine. The stock and rate of increase of monetary gold dominate monetary affairs by determining the stock of common money, the price level, and the trends in both. If real bills tend to generate too little money relative to what the gold standard demands, bankers' reserves continue to be excessive, and banker pessimism moderates. If bankers allow too much bank credit, gold flows out of the monetary system, depleting bank reserves and bringing bank lending up short. The important principle here is that no matter how invalid the real bills doctrine is in its role as a basis for creating the 'right' quantity of money, the system's higher ranking commitment to an operational gold standard completely overrides any weaknesses in that doctrine (Schumpeter 1954, 721-722; A.Piatt Andrew 1905, 114-115).¹³

But Congressmen who sponsored and passed the Federal Reserve Act in 1913 did not seem to understand this difference. They believed that commercial banks' and, especially, Reserve Banks' faithful adherence to the real bills doctrine would make the monetary system self-regulating, with or without the gold standard. To function properly, a Reserve Bank was supposed to discount only 'eligible paper,' which the Federal Reserve Act defined as "notes, drafts, and bills of exchange arising out of actual commercial transactions . . . issued or drawn for agricultural, industrial, or commercial purposes" (1961, 43). 'Eligible' also meant short-term and self-liquidating. "The only limit to a commercial bank's ability to discount," Charles Korbly, a congressman from Indiana stated during the congressional debates in 1913, "is the limit to good commercial paper. Such paper springs from self-clearing transactions" (quoted in Timberlake 1993, 224). Although supporters of the Federal Reserve Act who subscribed to the real bills doctrine did not acknowledge it, their stated beliefs made the gold standard appear superfluous.

The difference between gold and real bills, however, is crucial. Gold was naturally scarce—its supply did not depend directly or indirectly on the whims of bankers and other lenders. The quantity of "real bills," on the other hand, relied mainly on banker's judgments and not, as subscribers to the real bills doctrine would have it, on the economy's real output of goods.

¹³ Humphrey notes that Adam Smith allowed the gold standard precedence in determining the quantity of money and the price level *before* he spelled out the real bills doctrine. He thereby saved his analysis from the embarrassment of dynamic instability (Humphrey 1982, 8).

Intuitively, many congressmen may have sensed this difference. For the last item to be discussed in the debates was the propriety of the clause that stated: “Nothing in this act . . . shall be considered to repeal the parity provisions contained in an act approved March 14, 1900 [*The Gold Currency Act*].” The clause was left in to emphasize that the Federal Reserve System was to be a supplement, not a substitute, for the venerable self-regulating gold standard (Timberlake 1993, 227).

THE FEDERAL RESERVE SYSTEM’S PRICE LEVEL STABILIZATION IN THE 1920s

The Federal Reserve System, like the Bank of England and the Banks of the United States, was *not* designed to be a central bank. To the newly elected Democratic Congress and President in 1912, a central bank was politically unacceptable. Bad enough that it was a bank, a *central bank* was also monolithic and monopolistic, and would operate only to further the interests of bankers. Instead, the ruling Democratic majority devised a system that complemented the regional structure of national banking with a federal system of reserve-holding, super-commercial banks. Whereas there were only three central reserve cities—New York, Chicago, and St. Louis—twelve cities would eventually host Federal Reserve Banks (Timberlake 1993, 220-221).

The Federal Reserve System took over the functions of both the Treasury Central Bank and the clearinghouse associations. The new institution was to serve as a self-regulating adjunct to the self-regulating gold standard. It was to be a Gold Standard Central Bank, and to do in the short-run what the gold standard did secularly—provide seasonal money commensurate with seasonal productions of commodities (Friedman and Schwartz 1963, 191). It would also become a system-wide clearing institution for the member banks, since it held their reserve-account balances, and it was expected to issue currency in a liquidity crisis.

Virtually all of its Democratic supporters in Congress swore that it would be “non-political” (Timberlake 1993, 223). Fed policy during World War I contradicted this supposition. The temper of Congress, and the government’s wartime fiscal needs, led the Fed to adjust its policies to the

dictates of the Treasury.¹⁴ The *Annual Report* of the Board for 1918 began by stating: “The discount policy of the Board has necessarily been coordinated . . . with Treasury requirements and policies, which in turn have been governed by demands made on the Treasury for war purposes” (Timberlake 1993, 258). Throughout the war and early postwar period, the Reserve banks adhered to Treasury pressure by charging somewhat lower discount rates to member banks that used government securities as collateral for their loans and other things (Friedman and Schwartz 1963, 192-196; Meltzer 2003, 84-90). The predictable result was inflation.

The Board’s *Annual Report* for 1920, however, blamed the post-war inflation, not on the dominance of the Treasury, but on “an *unprecedented* orgy of extravagance . . . overextended business, and general demoralization of the agencies of production and distribution” (Timberlake 1993, 258). To end this “orgy,” Fed Banks raised discount rates, provoking the sharp post-war contraction of 1921-22 (Friedman and Schwartz 1963, 231-239). In the following year, Fed Banks’ earning assets, which had grown by \$2.5 billion from 1917 to 1920, almost disappeared, and the Fed’s gold holdings increased substantially. (See Table 2 below.)

The 1920s were the formative years that determined the institutional character of the Fed. The original Act had stated that Fed Banks were “to furnish an elastic currency,” which meant that they would rediscount commercial paper of member banks who wanted to convert deposits into currency—“form elasticity”—so as to prevent undue change in the total quantity of money. This task was also complementary to the function in everyone’s mind of serving as a lender of last resort for solvent but illiquid banks in a financial crisis—“cyclical elasticity,” to maintain the existing level of bank credit and deposits (Timberlake 1993, 111). In accordance with these principles, Fed Banks were to keep their rediscount rates higher than general market rates, so that they would become financially active only in a liquidity pinch (Hepburn 1924, 531-534).

The policies and reports of the Fed Banks and the Board of Governors during the 1920s reflect anything but such a defensive role. Starting in 1922, the New York Fed and some other Reserve Banks began open market operations (purchases) in government securities. Their purpose was to furnish themselves with enough income-earning assets to pay dividends to their member banks at times when their holdings of commercial bills for members were minimal, and also to cushion higher

¹⁴ Since the Secretary of the Treasury and the Comptroller were Chairman and Vice-Chairman of the Fed Board, the Treasury’s fiscal needs always received top priority.

discount rates charged member banks. After 1922, however, open market operations became a formalized and accepted means for manipulating the money stock (Friedman and Schwartz 1963, 251; Wicker 1965, 325-327; Humphrey 2001, 306-307).

Open market operations reflected the fact that the main thrust of Federal Reserve policy ignored both real bills principles and the gold standard. Fed Banks, particularly the Fed Bank of New York, were inundated with gold reserves. To prevent current gold monetization and inflation, and a subsequent deflation on the gold's anticipated return to European banking systems, Fed policy sterilized the gold and instituted a stable price level policy.

Table 2. Money Stock, M1 and Selected Items in All Federal Reserve Banks, 1920-1933, with Gold Reserve Ratios. (\$ Billions, except ratios)

Year	M1	Total Mon. Liab.	Gold and Other Reserves		Net Mon. Liab.	Change in Net Mon. Liab.	Bills Bought	Gold Res. Ratio
			Total	Exc.				
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1922	21.6	4.03	3.14	1.62	0.89	-2.10	0.98	77.8
1924	23.2	3.93	3.25	1.78	0.68	-0.21	0.86	82.5
1926	26.1	3.94	2.98	1.51	0.96	0.28	1.00	75.4
1929	26.2	4.04	3.10	1.51	0.94	-0.02	0.82	74.5
1931	23.9	4.14	3.50	1.96	0.64	-0.30	0.62	84.3
1932	20.5	4.80	2.80	0.99	2.00	1.36	0.25	58.4
1933 (March)	19.1	6.14	3.15	0.80	2.99	0.99	0.12	51.3

Sources: Board of Governors of the Federal Reserve System. *Banking and Monetary Statistics, 1943*, table 93, 347-349, and Friedman and Schwartz (1963) tables B-3 and A-1, 801-804 and 709-714.

Friedman and Schwartz, in their detailed analysis of this period, note that after 1923, "gold movements were largely offset by movements in Federal Reserve credit so that there was essentially no relation between the movements in gold and in the total of high-powered money." The Fed's gold sterilization policy made the operation of the international gold standard more difficult, they observe, because it threw an even heavier

burden of post-war adjustment on countries, especially Britain, that were trying to deflate their monetary systems to re-establish prewar gold parities (Friedman and Schwartz 1963, 282-284).

The principal driving force behind Fed policy at this time was Benjamin Strong, Governor of the New York Fed. Several studies have documented Strong's policymaking role (Chandler 1958; Friedman and Schwartz 1963; Wicker 1965; Hetzel 1985; Steindl 1994). Strong was instrumental in forming the Open Market Investment Committee (OMIC), a voluntary committee of Reserve Bank Governors whose purpose was to make open-market operations a system-wide policy for all the Governors who wanted to participate. These policies ranged much beyond the subordinate role to the gold standard intended and implied by the Federal Reserve Act.

In an appearance before the House Committee on Banking and Currency, Strong confirmed Friedman and Schwartz's later observation:

In recent years the relationship between gold and bank deposits is no longer as close or direct as it was because the Federal Reserve system has given elasticity to the country's bank reserves. Reserve bank credit has become *the equivalent of gold* in its power to serve as the basis of [commercial] bank credit. A bank can meet its legal requirement for reserves by borrowing from the Reserve bank, just as fully as though it deposited gold in the Reserve bank. (Quoted in, Hetzel 1985, 6. Emphasis added)

Clearly, the open-market operations and other activist policies that the Fed Banks and Board undertook between 1923 and 1928 had little to do with maintaining an elastic currency or serving as a lender of last resort. They confirm that the Fed had become a constant force in financial markets—manipulating gold flows, and negotiating with foreign central banks to control gold movements, while conducting open market operations to keep prices stable.

Strong was particularly inclined toward price level stabilization and, as a policymaker, was willing to promote it. Besides his practical experience as a banker who had witnessed clearinghouse operations in the Panic of 1907, he had the counsel of Professor Irving Fisher and some other economists who proposed such a policy through control over the quantity

of money (Chandler 1958, 194-206; Hetzel 1985, 7-8; Steindl 1994, 101-103).

At the same time, Strong felt that a *law* requiring stabilization was inappropriate. “Governor Strong believed that the government should not have the power to control the price level, and [that] the gold standard was the accepted means of keeping this power from the government” (Hetzel 1985, 8; Chandler 1958, 199). Strong’s policies and, therefore, those of the Fed Bank of New York were largely quantity-theoretic, meaning that, as Strong expressed it, “no influence upon prices is so great in the long run as is the influence of considerable changes in the quantity of money (Burgess 1930, 175).”

However much these policies were in lieu of a gold standard, they anticipated the restoration of an operational gold standard when the current period of instability had ended.¹⁵ Because he had this end in view, Strong opposed the stable price level legislation that came before Congress in 1926-1928. Moreover, Strong and his associates at the Fed Bank of New York pointedly and emphatically rejected all aspects of the real bills doctrine as either a guide to or a norm for effective policy (Burgess 1930, 182-184). Strong’s disavowal of that doctrine, however, did not speak for the opinions of the Fed Board and many other Fed Bank Governors.

By 1928, three operating methods and supporting arguments had appeared in Federal Reserve policy: the gold standard, in remission but still the ultimate norm in official discourse; price level stabilization by quantitative control of bank reserves through open-market operations; and the real bills doctrine that argued for ‘credit control’ under the discretion of the Board of Governors and the Reserve Banks, using the Fed Banks’ discount rate as the controlling mechanism. When Strong died in October 1928, real bills policymakers within the System moved to take charge of the policy machinery.

Both the administrations of the 12 Reserve Banks and the Federal Reserve Board in Washington had policymaking powers. The Board, which

¹⁵ Strong’s policy philosophy is thoroughly summarized in the paper he delivered to graduate students at the Harvard Graduate School of Business on November 28, 1922: “Control of Credit Through the Reserve System” (Burgess 1930, 173-197). In this paper Strong discussed his specific principles and methods for policy. He noted his experience as a banker during the panics of 1893 and 1907, and how the clearinghouse banks, in one of which he was an officer, had provided positive monetary relief. This experience obviously influenced significantly his role as Governor of the Fed Bank of New York, and his acknowledged leadership of the Fed System. His speeches in the years 1919-1928 confirm that he would never have abided nor overseen the Great Contraction that began in 1929.

operated as a supervisory-and-review body, had a veto power over discount rates set by individual Reserve Banks. It also made the final determination of the “character of paper eligible for discount,” and could set other regulations and limitations on discounting (Board of Governors 1961, 44-48).

Besides its proscriptive powers over Fed Bank discount rates and the eligibility of commercial paper, the Board also had extensive emergency powers that it could use actively in a panic or crisis. First, on the affirmative vote of five members, it could “require Federal reserve banks to rediscount the discounted paper of other Federal reserve banks at rates of interest to be fixed by the Board of Governors.” With this power, the Board could move gold from one Fed Bank to another whenever the gold-needy Bank required and requested such help.¹⁶ Additionally, the Board could order the suspension of “*any* [gold] reserve requirements specified in this Act” for a period of thirty days, and it could renew such suspensions every fifteen days thereafter for an indefinite period (Board of Governors 1961, 34-35. Emphasis added). This provision gave the Board the power to let the Reserve Banks use up *all* their gold if necessary, just as a banking system without a central bank might use its gold reserves for redemptions of bank-issued money when the situation demanded, and as the U.S. Treasury had come close to doing in the monetary shrinkage of 1893-96.

The Fed Board, however, had no tradition of active policy, and most of the other Reserve Banks were mainly concerned with local affairs (Friedman and Schwartz 1963, 411, and passim). Most important was the theory under which both Board and Banks operated. With the exception of the New York Fed, most of them embraced the real bills doctrine—as the Federal Reserve Act suggested was their duty.

An especially prominent member of the Board, who had served on it from the date of its establishment, was Adolph C. Miller, an economist who also had been instrumental in writing real bills norms into the Fed Act. Miller had been a student under J. Laurence Laughlin, the most influential real bills proponent in the economics profession.¹⁷ During the *Stabilization*

¹⁶ This provision was the one that persuaded A. Barton Hepburn to support passage of the Fed Act in 1913. He was particularly concerned that Fed Banks operate as a unified central bank in the event of a serious threat to gold reserves (Hepburn 1924, preface, x-xi).

¹⁷ Both Miller and H.Parker Willis were associated throughout their professional lives with Laughlin. They, in turn, were close associates and advisers of Carter Glass who was Chairman of the House Banking and Currency Committee that constructed and passed the Federal Reserve Act in 1913 (Bornemann 1940, 2,3,5,27,31,45,51,53,59). Laughlin was a

Hearings of 1926-1928 that Hetzel explores, Miller was the quintessential real bills advocate. He was also instrumental in writing the Board's *Tenth Annual Report* in 1923, which is virtually a handbook for real bills policy. It stresses the notion that goods create money, and that central bankers must be informed, skillful, and discretionary in applying the central bank's powers to "each specific credit situation at the particular moment of time when it has arisen or is developing." As his final observation in the *Stabilization Hearings*, Miller stated flatly, "The total volume of money in circulation is determined by the community. The Federal reserve system has no appreciable control over that and no disposition to interfere with it." Miller was particularly opposed to the price-level stabilization policies of Governor Strong, and was almost indiscreet in implying that Strong was one of those "amateur economists" who "constitute one of [the System's] dangerous elements" (Hetzel 1985, 10-11). Charles S. Hamlin, who had also served on the Board since 1914, was another uncompromising proponent of the real bills doctrine.

That the Fed should, under the circumstances, have slipped into a do-nothing policy after 1928 should cause no surprise. Few if any of the Fed's official family agreed with Strong's active policy of price level stabilization, and none had any interest in prolonging it. Fed officials now in charge of monetary affairs accepted the real bills doctrine as the guide to policy. They also believed that active control of the quantity of money was improper—that a return to "legitimate" lending alone would establish the correct amount of "credit" and money (Friedman and Schwartz 1963, 417, n.178; Humphrey 2001, 302-309).

This shift in control was decisive. In accordance with the precedent Strong had set in promoting a stable price level policy without heed to any golden fetters, real bills proponents could proceed equally unconstrained in implementing their policy ideal. System policy in 1928-29 consequently shifted from price level stabilization to passive real bills. "The" gold standard remained where it had been—nothing but formal window dressing waiting for an opportune time to reappear (Hetzel 1985, 15).¹⁸ Unless the

long-time opponent of the Quantity Theory of Money, and Miller and Willis actively assisted and supported his views. In Congress, Glass promoted their ideas into law.

¹⁸ Friedman and Schwartz, in discussing the shift in control from the Fed Bank of New York to the Board of Governors, observe "that something more than the characteristics of the specific persons or official agencies that happened to be in power is required to explain such a major event as the financial catastrophe in the United States from 1929 to 1933" (Friedman and Schwartz 1963, 419). The "something more" that they look for, I suggest, was not only the laxity of policy but the shift in power to those who acted on the principles

observer understands the extent of the belief in the real bills doctrine and the metamorphosis of Fed policy from active price level stabilization to passive real bills, he cannot properly understand The Great Contraction that followed.

THE REAL BILLS CENTRAL BANK IN OPERATION, 1929-1933

When the first signs of serious trouble appeared in financial markets in 1929, the concerns of Reserve Bank authorities centered on the quality of bank loans. In their view, the supply of credit included far too many speculative loans based on stock shares, real estate loans, and government securities. None of these forms of credit was consistent with the real bills doctrine. The Fed was, therefore, content to allow the supply of credit and, along with it, the money stock, to shrink. As Allan Meltzer has noted correctly, “The Federal Reserve had abandoned strict adherence to the gold standard in World War I and in the 1920s. It [now] followed the real bills guide. Policy was deflationary in 1930 when adherence to gold standard rules called for expansion” (Meltzer 2003, 401-2).

Fed authorities could have continued the quantity-theoretic approach that Strong had followed. But as Humphrey has pointed out, they “refused to have anything to do with this framework . . . [because price level stabilization] was incompatible with the type of institution created by the Federal Reserve Act” (Humphrey 2001, 286). That institution was supposed to “accommodate commerce and business,” not control the price level.

True. But the Federal Reserve System as originally envisioned was also supposed to be subsidiary to an operational gold standard. Since that gold standard was missing, however, Fed policymakers had provided in its place, first, a quantity-theoretic policy, and then shifted to a real bills model. They were adamant that an independent resurgence of production in the real sector of the economy was the only proper basis for growth in money and credit. They expected such growth to manifest itself in applications for new business loans, but they were first determined to see the monetary system purged of “speculative” and long-term “credit.”

of the real bills doctrine. They refer to this belief several times, but I could not find an explicit link in their treatment between it and the policy of do-nothing that they document so thoroughly.

Consequently, during the Great Contraction of 1929-1933, Fed Banks virtually stopped rediscounting while piling up gold reserves. Clark Warburton, writing some years later, emphasized the intensity with which the Fed Board insisted that Fed Banks deny discounts to member banks by ‘direct pressure’ tactics.¹⁹ In the early 1930s, he wrote, the Fed Banks

virtually stopped rediscounting or otherwise acquiring “eligible” paper. This [policy] was not due to any lack of 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 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)²⁰

At the same time that they refused to provide member banks’ requests for loans and discounts, Fed policymakers were also piling up gold. Fed gold (and other) reserves peaked at \$3.50 billion in 1931 (from \$3.10 billion in 1929), an amount that was 81 percent of outstanding Fed demand liabilities, and more than double the gold reserves required by the Federal Reserve Act. (See Table 2 and Timberlake 1993, 270.) By 1931, Fed-held gold was almost 40 percent of the world’s monetary gold stocks (Friedman and Schwartz 1963, 396; Officer, 2001).

With the bank credit contraction in full swing, from late 1931 to the summer of 1932 the System undertook a policy of open market purchases in a half-hearted attempt to provide some sort of monetary relief. However, the expansion ground to a halt when the Fed’s excess, or “free,” gold

¹⁹ “Direct pressure,” meant to “jawbone” negatively banks that applied for loans. Besides the discount rate a Fed Bank charged a borrowing bank, the bank also had to endure a severe cross-examination meant to discourage its application for assistance, especially if Fed authorities thought the new “credit” might be used for speculative purposes.

²⁰ Strong had been adamantly opposed to ‘direct action’ policies (Chandler 1958, 430-434, 466-469; Burgess 1930, 190-193).

reserves²¹ were still \$1.02 billion, and its overall gold reserve ratio was more than 58 percent of its demand obligations (Friedman and Schwartz, 1963, 346; Timberlake, 1993, 271). Even in March 1933, Fed Banks had almost \$1 billion of excess gold reserves, which could have been accounted even higher by simple bookkeeping adjustments.²² As Friedman and Schwartz state,

the conclusion seems inescapable that a shortage of free gold did not in fact seriously limit the alternatives open to the System. The amount was ample at all times to support large open market purchases. . . . The problem of free gold was largely an ex post justification for policies followed, not an ex ante reason for them. (Friedman and Schwartz, 1963, 406)

Neither were the Fed's legally required reserves—never mind the excess—a “line in the sand.” As explained above, the Fed Board had the absolute power to suspend gold reserve requirements entirely, so that the Fed Banks could use their gold—all of it, if necessary—by lending to member banks, thereby providing the gold liquidity that the situation demanded. Instead, the Fed sat on the gold, including the “excess,” while the economy disintegrated. In contrast to the Treasury Gold Standard operation of 1893-96 that witnessed Treasury gold reserves declining by sixty percent while maintaining gold redemption of Treasury currencies, the Real Bills Central Bank of 1929-1933 accumulated gold throughout the period. It had more gold in early 1933 than it had in the fall of 1929! Had Fed authorities allowed “their” gold to run down, not only might the U.S. contraction have been halted, but the rest of the world's monetary systems would also have benefited from the outflow of Fed gold (Friedman and Schwartz 1963, 412; Timberlake 1993, 272).

The reason Fed policy was so disastrous was neither technical nor legal. It had nothing to do with “the” gold standard, if for no other reason than the fact that “the” gold standard throughout this period was nothing more than a façade. Fed managers were operating on a real bills basis without reference to gold. They had sterilized gold inflows during the 1920s

²¹ Fed Banks were required to keep gold reserves of at least 35 percent of their member bank deposit liabilities, and 40 percent of outstanding Federal Reserve notes. Any gold reserves they held in excess of this minimum were labeled “free gold reserves.”

²² Accounted excess, or “free,” gold reserves could easily have been expanded by \$80 to \$200 million (Friedman and Schwartz 1963, 396).

and were now sterilizing gold outflows. To their way of thinking gold flows were superfluous in governing money growth, except to the extent that they happened to do so in a manner consistent with a real-bills rule (Meltzer 2003, 411-413). However, the Fed Board continued to explain “economic decline and then banking failures as occurring despite its own actions, and as the product of forces over which it had no control” (Friedman and Schwartz 1963, 419).²³

CONTEMPORARY VILIFICATION OF ‘THE’ GOLD STANDARD

Virtually all present-day economists agree, first, that the Great Contraction was largely a failure of monetary policy and of monetary arrangements that allowed monetary policy to provoke such a disaster, and, second, that the Great Contraction initiated the Great Depression. In a negative sense, economists also deny that a capitalist free-market economy in any way caused these two major catastrophes. Given these agreements, however, economists still record some major differences on just how monetary policy went awry, and just what was the crux of the problem.

An opinion that has become popular among many economists in recent decades is that “the” gold standard was at least a villain and possibly a demon. Two recent studies have concentrated on this theme and have elaborated it in publications that have received wide acceptance. I highlight these examples of gold standard vilification to show what I think are fundamental gaps in their facts or errors in their analysis. These faults include:

- (1) The authors’ conception of what they refer to as “the” gold standard;
- (2) Any recognition of variations in “the” gold standard, and what they implied;

²³ Failure to recognize the pro-cyclical effects of the real bills doctrine on Fed policy during the Great Contraction and after may have resulted from the common practice of using only that doctrine’s inflationary potential, e.g., the German hyperinflation of 1923, to emphasize its instability. The doctrine’s unstable deflationary dynamic became empirical reality in the United States during 1929-1933.

- (3) Failure to specify with easily obtainable data the magnitude of Federal Reserve gold stocks, both total and “free,” and what Fed policymakers could have done with that gold to abate the Contraction;
- (4) Omission in their arguments of the important statutory powers that the Federal Reserve Act provided the Fed Board, particularly the power to suspend gold reserve requirements for as long as necessary;
- (5) Above all, complete neglect of a long-time banking fallacy, the Real Bills Doctrine, used as the basis of Federal Reserve policies from 1929 to 1933.

Somehow, these data omissions and untreated concepts have gone unnoticed, or at least unpublicized. The community of economists seems to accept the conclusion that “the” gold standard caused the Great Contraction. The profession is, therefore, working with fundamentally flawed historical analysis, and the general public is still misinformed and bewildered.

The first work I criticize is Peter Temin’s book, *Lessons from the Great Depression* (1989), and also his subsequent NBER working paper, “The Great Depression” (*Historical Paper No. 62*, NBER, 1994).

Early in his argument, Temin states that, “The tight monetary—and fiscal—policies of the late 1920s were due to adherence of policymakers to the ideology of the gold standard” (Temin 1989, 7). He then asks: “What was the gold standard? There does not appear to be a single answer in the literature, despite the volume of work on the operation and effect of this system.”²⁴ Temin offers five features that he thinks a gold standard should include:

- (1) The free flow of gold between individuals and countries.
- (2) The maintenance of fixed values of national currencies in terms of gold and therefore to each other.
- (3) The absence of an international coordinating organization.
- (4) Temin claims that these three conditions imply his fourth condition—that “there was an asymmetry between countries experiencing balance-

²⁴ One can find a description of the authentic gold standard in several sources. For example, see White 1935, 80-82; Schumpeter 1954, 444; Hepburn 1924, 482-484. For a contemporary description, see Officer 2001, and Timberlake 1993, 1-3. Given the essential framework of a gold standard system, an economist conversant with market principles can infer how it must work.

of-payments deficits and surpluses.” Gold losers would be unable to maintain the fixed value of their currencies, but the gold gainers realized neither rewards nor penalties for accumulating gold.

- (5) Finally, “the adjustment mechanism for a deficit country was deflation rather than devaluation, that is a change in domestic prices instead of a change in the exchange rate” (Temin 1989, 8-9).

According to Temin,

the gold exchange system of the interwar period shared with the gold system [the *real* gold standard system?] the five characteristics listed above. *I therefore consider the interwar gold standard to be the gold standard*, as opposed to another institutional arrangement. In particular, it smiled on the accumulation of gold balances and offered only the bitter pill of deflation to countries experiencing a drain. This prescription unhappily had side effects that made it unsafe for use in the late 1920s. . . . *The gold standard* was alive, although hardly well, in the minds of economic policymakers into the early 1930s. (Temin 1989, 10-12. emphasis added)

Even though Temin notes that “the gold standard” he discusses was a modified “gold-exchange standard,” his principles for a genuine gold standard are grossly incomplete (see above, 2-3). Furthermore, his text implies, contrary to what he argues, that *no* version of a true gold standard was functioning. The “gold-exchange” or “gold-bullion” standards of the time on which he concentrates were nothing more than multiple central bank confederacies using a “gold standard” rubric.

The British did not resume any kind of gold payments until 1925. So, as Temin and others acknowledge, the gold standard was in remission at least until the British resumption. Once the gold standard was again “declared” in 1925, the disequilibria of exchange rates became manifest. Britain, whose pound was overvalued, endured a general strike, while France, where the franc was undervalued, “attracted gold like a magnet.” Both France and the United States accumulated gold, Temin notes, but did not expand their monetary stocks by anything like their accumulation of gold. “Consequently, both countries’ central banks held excess gold reserves—the familiar gold sterilization of the period” (Temin 1989, 17-20).

This argument includes a mistaken premise and a contradictory argument. Any authentic gold standard was not supposed to be responsible for setting the monetary price of gold and, therefore, exchange rates. Legislative specification of gold values for currencies was supposed to take place before a legitimate gold standard started working. Moreover, if central banks can initiate and implement the gold sterilization policies that characterized the 1920s, the advertised “gold standard” is a charade.

By the end of the 1920s, Temin continues, “the gold standard had been revived, but the conditions that had sustained it before the war no longer existed.” Exchange rates were all out of kilter, and

government policies everywhere were set to discourage economic activity. . . . It is no secret that the Federal Reserve pursued a deflationary policy in the 1930s. . . . Fed policy was part of a general governmental policy of deflation. It was not an artifact of the structure or personalities of the Federal Reserve System itself; it represented one aspect of a unitary national policy. . . . The Fed had contracted in the prosperous conditions of 1928 to stop the gold outflow; it did the same in the depressed climate of 1931. Adherence to the gold standard compelled the Federal Reserve to depress the economy further in the midst of the Great Depression [1936-37]. (Temin 1989, 25-29)

Temin here contradicts his presumption that “the” gold standard was doing the damage. He observes explicitly that the Fed was controlling both the quantity of gold it held and the monetary system, while “the” gold standard was still waiting in the wings for its cue to go on stage. Everyone agrees that Fed policy was deflationary, but Temin cites no evidence that deflation was a *national* policy. Indeed, it was not. Most non-central bank government officials, including many congressmen and most of the general public, just simply did not understand what was happening. In any case, virtually everyone, except Fed policy makers, eagerly looked for expansion of all the common variables. Moreover, nothing in Fed Banks’ balance sheets “compelled” them to depress the economy either in 1931-33, or in 1936-37 (Timberlake 1993, 400-444). Fed Bank gold holdings were enormous and constantly increasing (see Table 2 above).

In a subsequent working paper, Temin restates the arguments in *Lessons from the Great Depression*. He contends that when the Fed deflated the

monetary system to “preserve the gold standard . . . some Federal Reserve banks were running out of ‘free gold’ [and] were unwilling to pool their reserves by interbank borrowing. [Thus] the effective reserve of the system was set by the weakest [in terms of gold reserves] banks” (Temin 1994, 18-19).

This argument completely ignores, first, the data on the Fed’s gold stocks, and what could have been done with them, and, second, the Fed Board’s emergency powers over gold reserves and discounting, spelled out above. The Board had all the authority it needed to use all of the System’s gold reserves in any way it saw fit.

Following Temin, other economists in recent years have agreed that the Fed’s main reason for allowing the 25-30 percent decline in all the major monetary variables was to “save” “the” gold standard, “which it saw as its fundamental mission” (Wheelock 1992, 18). Undoubtedly, the most critical account of “the” gold standard, and the Fed’s effort to “save” it, is Barry Eichengreen’s, *Golden Fetters* (1992).

Eichengreen’s work has arguments virtually identical to Temin’s on “the” gold standard as the source of the Great Contraction.²⁵ However, he also examines the relationships and interactions of the world’s major central banks during the 1920s and early 1930s, and the futility and folly of their efforts in trying to “save” the gold standard.

As early as his Preface, Eichengreen presents a bill-of-particulars excoriating “the” gold standard. This system, he accuses, “set the stage for the Depression of the 1930s by heightening the fragility of the international financial system.” It transmitted

the destabilizing impulse from the United States to the rest of the world. . . . [It] was the principal obstacle to offsetting action [by central banks]. . . . It was the binding constraint preventing policymakers from averting the failures of banks and containing the spread of financial panic. For all these reasons, the international gold standard was a central factor in the worldwide Depression. (Eichengreen 1992, Preface, xi)

²⁵ I could not find in Eichengreen’s work a distinction between “The Great Contraction” and “The Great Depression.” He seems to lump both together as “The Great Depression.” Yet they were entirely separate events, occurred under different political regimes, and require separate interpretations.

The problem first appeared, Eichengreen argues, in 1928 when Fed officials tightened monetary policy. As the economic downturn, which the gold standard linked to international events, continued, policymakers either “had to sacrifice the gold standard, in order to reflate, which most opposed, or vice versa.” Not even the United States could reflate unilaterally, he claims, “as the open market operations in the spring and summer of 1932 reveal.”²⁶ In a supporting footnote, he states that in spite of the huge amount of monetary gold in the United States, the Fed’s gold reserve requirements and Fed policymakers’ “unwillingness to let their gold ‘work’” precluded any relief. “The gold standard posed an insurmountable obstacle to unilateral action [by the Fed]. Defending the gold parity might require the authorities to sit idly by as the banking system crumbled, as the FRS did at the end of 1931 and again at the beginning of 1933” (Eichengreen 1992, 18, and note 24). “The gold standard,” he concludes, “was responsible for the failure of monetary and fiscal authorities to take offsetting action once the Depression was underway” He acknowledges that the Fed and the Bank of France possessed “extensive gold reserves,” but claims that “they had very limited room to maneuver. . . . Abandoning the gold standard became a necessary precondition for recovery . . . [which] required discarding not just the gold standard but also the gold standard *ethos*” (Eichengreen 1992, 393).²⁷

Ben Bernanke, in a laudatory review of *Golden Fetters*, agrees with its main thesis. “Eichengreen,” Bernanke states, “has made the case that the international gold standard, as reconstituted following World War I, played a central role in the initiation and propagation of the worldwide slump” (Bernanke 1993, 252). “In this masterful new book,” he notes approvingly, “Barry Eichengreen has gone well beyond his previous work to marshal a powerful indictment of the interwar gold standard, and of the political leaders and economic policy-makers who allowed themselves to be bound by golden fetters while the world economy collapsed.” The United States, especially, absorbed and sterilized gold, “largely reflecting conscious Federal

²⁶ As Friedman and Schwartz’s work demonstrates, this assertion is just plain wrong. The open-market operations of 1932 had an expansionary, but lagged, effect. They were discontinued because Fed (real bills) policy makers had no real enthusiasm for them (Friedman and Schwartz 1963, 322-324).

²⁷ Eichengreen, like Temin, at some points distinguishes between the pre-war “classical” gold standard and the post-war “gold-exchange” standard. However, he never uses the term, “gold-ex standard,” or some other means to convey this distinction in his text, particularly in his most critical passages. The reader gets the definite impression that *any* gold standard shares the defects of the post-war model. Not that it matters: *No* true gold standard was in place anyway.

Reserve policy. . . Monetary policy became tight in the U.S. in 1928. . . High returns on both bonds and stocks attracted gold into the U.S., but the Fed, intent on its domestic policy goals, sterilized the inflows” (Bernanke 1993, 253-258).

Bernanke’s words, much like Temin’s and Eichengreen’s, contradict his argument. If central banks could absorb and sterilize gold, “reflecting conscious Federal Reserve policy,” the central bank, not the gold standard, was running the show. He also neglects any reference, as do both Eichengreen and Temin, to the emergency powers over gold reserves and System discounting that the Fed Board had—and was supposed to use, and to the huge amount of excess gold that the System had throughout the period. Nor does Bernanke, or Eichengreen, or Temin discuss, or even mention, any aspect of the real bills doctrine and its influence on policy. No reference to ‘real bills doctrine’ appears in the index of either book.²⁸ Indeed, the Fed was “intent on its domestic policy goals,” as Bernanke notes above—that is, making sure that their real bills norms for policy were secure.

Bernanke finally poses a very apt question that he leaves unanswered. “Why was there such a sharp contrast between the stability of the gold standard regime of the classical, pre-World War I period and the extreme instability associated with the interwar gold standard?” (Bernanke 1993, 261).

Here are two commentaries that may help answer his question. The first is from Lionel D. Edie, a prominent economist of the time. At a conference of economists in early 1932, he stated,

The Federal Reserve Act cut the tie which binds the gold reserve directly to the credit [money] volume, and by so doing automatically cut off the basic function of the gold standard . . . in an essential respect we abandoned [the automatic money supply function] some time ago. . . . We

²⁸ In a recent working paper, “Still Fettered after All These Years,” Eichengreen reaffirms the arguments he made in *Golden Fetters*. In this agreeable review of his former work, however, Eichengreen does mention the real bills doctrine, but only to say that Fed managers had “misinterpreted” it, not that it was their modus operandi as I have here described it. Eichengreen also confirms that Adolph Miller became the guiding force for policy after Strong died (Eichengreen 2002, 2-3 and note #5). Stephen Cecchetti, in another NBER paper, lauds Eichengreen’s work and makes a similar observation about Miller’s control of policy after Strong’s death. In passing, Cecchetti states axiomatically: “A gold standard [*not*, a gold-exchange standard] is very dangerous” (Cecchetti 1997, 4-6).

are not now on the gold standard . . . and we have not been for some time . . . it is time to recognize that the Federal Reserve mechanism does not constitute an automatic self-corrective device for perpetuating a gold standard. (Edie1932, 119-128)

And Leland Yeager in 1966 described the “gold standard” of the 1920s in these words:

The gold standard of the late 1920s was hardly more than a façade. It involved extreme measures to economize on gold. . . . It involved the neutralization or offsetting of international influences on domestic money supplies, incomes, and prices. Gold standard methods of balance-of-payments equilibrium were largely destroyed and were not replaced by any alternative. . . . With both the price-and-income and the exchange-rate mechanisms of balance-of-payments adjustment out of operation, disequilibriums were accumulated or merely palliated, not continuously corrected. (Yeager 1966, 290)

These commentaries provide the answer to Bernanke: “The” interwar gold standard was not a gold standard. It was an entirely different system than the pre-1914 gold standard that had existed for 100 years.²⁹

Bernanke might well have asked some related questions. If “the” gold standard was such a disaster in the 1920s and 1930s, why was it tolerated so long through some very turbulent financial episodes of the nineteenth century? Why was it so venerated through thick and thin for 100 years, as Schumpeter has noted? How could such a simple rule-based system be so pernicious? And, finally, if it was such a disaster for the world in 1929 and after, why did its faults not manifest themselves sooner?

The answer to these questions is that the “gold standard” of the 1920s was a pseudo-gold standard. The real gold (or bimetallic) standard had worked very well for the better part of a century as a rule-based system supplying the world with money. As monetary histories confirm, and as

²⁹ Friedman and Schwartz (1963, 240) make a similar observation. “The Federal Reserve System [following World War I] for the first time felt itself a free agent, relieved alike from the pressures of Treasury needs and of internal liquidity. . . . It had to face explicitly the need to develop criteria and standards of monetary policy to replace the automatic operation of the gold standard.”

noted above in my account of the Treasury Gold Standard in operation, most of the monetary turbulence—bank panics and suspensions in the nineteenth century—resulted from over issues of legal tender paper money, and were abated by the working gold standards of the times. Finally, the 1929-1933 disaster demonstrated how a non-gold standard central bank, ruled by an incurably flawed doctrine, could mismanage the monetary system into a world-wide debacle.

The conclusive datum that should have urged the anti-gold standard proponents to look for other answers is that both France and the United States all through the early 1930s and after had enormous amounts of gold reserves that were never set in motion. In 1933, the United States had 5,900 *tons* of gold in Treasury vaults, and the Bank of France had about half this much.³⁰

The question of the Fed's gold sufficiency has repeatedly arisen. Even seat-of-the-pants policy reactions, such as Fed policymakers might have had in 1931-1933 should have convinced them to carry out some degree of monetary expansion. Data from Friedman and Schwartz's *Monetary History* indicate that as of August 1932 the M2 money stock was \$34.0 billion and the monetary base \$7.85 billion, giving a money supply multiplier of 4.33 (Friedman and Schwartz 1963, table A-1, 713). At the same time, the Fed Banks-and-Treasury held \$2.91 billion gold (Board of Governors 1943, table 93, 347-349). If Fed Banks and Board had spent all of this gold discounting paper for member banks, so that the monetary base had increased by this amount (\$2.91 billion), it would have expanded M2 to \$46.6 billion, which was the level value for M2 in July 1929, with the attendant spending such a quantity of money would have generated. Of course, Fed expansion never would have had to go that far, for an expansion dynamic would have set in and restored all the major monetary vitals long before the Fed's gold had dissipated. Moreover, if expansion had occurred earlier before the banking crises and the great increase in the real demand for currency,³¹ the money supply multiplier would have been very

³⁰ How much gold is 5,900 tons? If this gold were loaded into a convoy of 590 ten-ton trucks for transport, allowing 100 feet for each truck, the convoy would stretch over 11 miles.

³¹ See, Timberlake 1993, Table 2, 267, for the disaggregation of money stocks and a comparison of real growth in their components between 1929 and 1933. This table shows how the increase in the demand for real currency and the corresponding increase in the currency-deposit ratio provoked the banking crises and significantly reduced money supply multipliers.

much greater, and the Fed's expansion procedure would have been much more effectual.

A study by Bordo, Choudri, and Schwartz (2002) has examined the question of gold sufficiency during 1929-1933 more rigorously. Using only the Fed's "free gold" reserve position, they show by means of a mathematical model and simulation of the banking crises that the Fed had plenty of gold to stop and reverse the ongoing deflation. They confirm their model with citations from official sources of the time that deny any lack of gold as a cause of Federal Reserve inaction. They conclude,

The simulations we constructed, based on a model of a large open economy, indicate that expansionary open market operations at two critical junctures of the Great Depression would have been successful in every scenario in averting the banking panics without endangering convertibility. (Bordo, Choudri, and Schwartz 2002, 9-11, 24)

If the observer also understands that the Fed's gold reserve requirement could have been completely abrogated by order of the Fed Board so that *all* Fed gold was on the table to be used *a la* the prescription of Walter Bagehot in his *Lombard Street*, the alleged "gold standard" constraint becomes even more imaginary.³² As Bagehot remarked, in the early stages of a panic the central bank "is not fettered" because it has enough gold; in the latter stages "the fetter has been removed" by remedial government action (Bagehot 1906, 206).

³² Bagehot in *Lombard Street*, prescribed five rules for any central bank to follow in the defense of the gold value of its currency: (1) Lend freely, (2) at "high" interest rates, (3) on paper that would be good in normal times. (4) Advertise this policy so that everyone would know it and be comforted by it. And (5) carry out the policy of lending until there is no more gold. The first two of these principles were explicit, and the latter three implicit (Bagehot 1906, 198-206).

INNOCENCE OF *THE* GOLD STANDARD AND THE GUILT OF THE REAL BILLS DOCTRINE

Looking closely at the history of the Federal Reserve from the Fed's beginnings in 1914, it is clear that an operational gold standard, either in its pure form or in the mode intended by the Federal Reserve Act, virtually never constrained Fed policies. During WW I, Treasury compulsions ruled the Fed's actions. In the 1920s, Strong's price level stabilization policies were dominant. After Strong's death, with Real Bills Central Bankers in charge, the Great Contraction devastated both the monetary and economic systems. As the Great Contraction ended, Roosevelt became President, and the wild swings of the New Deal took center stage. Gold became a political football; Congress hyper-devalued the gold dollar; the Supreme Court allowed contracts in gold to be abrogated; and the Banking Act of 1935 left gold as a useless adornment on Treasury and Federal Reserve balance sheets.

If the reader begins with the valid premise, as Yeager put it, that "the gold standard of the late 1920s was hardly more than a façade," Eichengreen's work suggests something very different from what he claims. The negotiations and machinations of the world's central bankers trying to provide a human design to the world's monetary system did not work. Their blueprint retained only the outward and visible sign from the working gold standard of a previous era; it had abandoned the inward and spiritual grace of that system. It neglected the fact that an authentic gold standard functioned on the principles of spontaneous order—set up simple rules and let human operatives make their own arrangements within that framework. The authentic gold standard provided long-term stability not matched by any other monetary system before or since. But in the interwar period, managing gold, as the central bankers tried to do, proved to be a disaster. The gold standard did not succeed; neither did it fail. The issue is not even moot, because the gold standard was not functional. What failed was the theory—the Real Bills Doctrine—that U.S. central bankers were using in its place to guide monetary policy into the monetary disequilibrium that never ended.³³

³³ The dominating effect of the Real Bills Doctrine on Federal Reserve policy is well documented. However, it must have had a significant influence on central bankers in France, England and Germany, too. So an interesting empirical question for further research is: How much impact did real bills have on foreign central bank policies in the late 1920s and early 1930s?

In an important sense, the authentic gold standard *did* include golden fetters. Joseph Schumpeter stated the case most elegantly, although he used the term ‘restrictions’ rather than ‘fetters.’

An ‘automatic’ gold currency is part and parcel of a laissez-faire and free-trade economy. It links every nation’s money rates and price levels with the money-rates and price levels of all the other nations that are ‘on gold.’ It is extremely sensitive to government expenditure and even to attitudes or policies that do not involve expenditure directly, for example, to foreign policy, to certain policies of taxation, and, in general, to precisely all those policies that violate the principles of [classical] liberalism. *This* is the reason why gold is so unpopular now [1950] and also why it was so popular in a bourgeois era. It imposes restrictions upon governments or bureaucracies that are much more powerful than is parliamentary criticism. It is both the badge and the guarantee of bourgeois freedom—of freedom not simply of the bourgeois *interest*, but of freedom in the bourgeois *sense*. From this standpoint a man may quite rationally fight for it, even if fully convinced of the validity of all that has ever been urged against it on economic grounds. From the standpoint of *etatisme* and planning, a man may not less rationally condemn it, even if fully convinced of the validity of all that has ever been urged for it on economic grounds. (Schumpeter 1954, 405-406, italics in original)

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INTELLECTUAL TYRANNY OF THE STATUS QUO

Ignorance and Influence: U.S. Economists on Argentina's Depression of 1998-2002

KURT SCHULER*

[Abstract, Keywords, JEL Codes](#)

ECONOMISTS CAN BE HAZARDOUS TO YOUR WEALTH

IN LATE 1998, ARGENTINA ENTERED AN ECONOMIC DECLINE that was to last until 2002. The decline deepened after Brazil, Argentina's largest trading partner, devalued its currency substantially in January 1999. Argentina could not devalue under the monetary system it then had, known locally as "convertibility;" the system maintained an exchange rate of one Argentine peso per U.S. dollar. As time passed, what started as a recession turned into a depression. At the end of 2001, a political upheaval resulted. In the space of two weeks Argentina had five presidents, one of whom defaulted on the government's foreign debt. Eduardo Duhalde, the last of the five presidents, came to power at the start of 2002. He made extensive changes in economic policy, including devaluing the peso, abandoning the "convertibility" system, and "pesifying" dollar assets and liabilities (forcibly

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I did most of the research and writing of this article before assuming my current job. It expresses my personal views, which are not likely those of the U.S. Treasury Department. I thank Matt Sekerke for research assistance at an early stage, and Steve H. Hanke and Martín Krause for comments.

converting them into pesos at rates involving substantial losses for creditors). Argentina's economy declined further, suffering its two worst quarters on record. It finally hit bottom around August 2002. From 1998 to 2002, Argentina's real gross domestic product fell 18 percent.¹

There was no shortage of advice about how to reverse Argentina's decline. The advice of economists in the United States particularly merits attention. The United States has the most prominent economists, the most renowned graduate programs in economics, and the most powerful national bureaucracy in international financial matters. It is also home to the three international financial institutions that have the most influence in Argentina: the International Monetary Fund (IMF), World Bank, and Inter-American Development Bank. Economists in the United States hence had more chance to influence policy in Argentina than their counterparts anywhere, except perhaps in Argentina itself. There arose a consensus, bridging the usual ideological divides, about what had caused Argentina's problems and how to overcome them; indeed, the consensus among economists reached beyond the United States and was nearly worldwide. 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.

Commentators on Argentina's crisis have suggested a number of causes for it. A nearly exhaustive list is: (1) the effect of the convertibility system on the real exchange rate, competitiveness, and willingness to issue dollar-denominated debt (bearing in mind that most observers considered the convertibility system to be a currency board); (2) external shocks, such as the reduced inflow of foreign capital; (3) budget deficits and their effect on the sustainability of the government debt; (4) inflexible labor markets; (5) three big tax increases the Argentine government imposed from January 2000 to August 2001; (6) mistakes by the IMF; and (7) political blunders by the Argentine government that reduced confidence in the economy, such as by upsetting established property rights. Almost all commentators agreed that the convertibility system lacked credibility by the last several months of its existence. Recommendations for replacing the system fell into two major groups. A majority favored a floating exchange rate, heavily managed if necessary and possibly supported by extensive exchange controls and forced conversion of dollar assets into pesos. A significant minority favored

¹ For longer accounts of these events, see Daseking and others (2004) and Schuler (2003).

official dollarization at one peso per dollar, at least before the government devalued in early 2002. A few remaining commentators held intermediate positions, such as favoring dollarization combined with a one-shot devaluation.

I will examine economists' views and the supporting evidence on four questions that were central to analysis of what caused Argentina's crisis and what might have cured it:

1. Was the convertibility system a currency board?
2. Was the peso overvalued, at least from 1999 onward?
3. Were Argentina's exports uncompetitive?
4. Was dollarization technically possible in late 2001 to early 2002?

The consensus on all questions was "yes." I will argue that only on the fourth question and possibly on the second was the consensus correct. Mistaken impressions that the convertibility system was a currency board and that Argentina's exports were uncompetitive made the case for abandoning the exchange-rate link to the dollar seem much stronger than it really was.

This study is not an explanation of why Argentina's crisis occurred and how it might have been avoided; I have already written on those topics elsewhere (including Hanke and Schuler 1999b, 2002; Schuler 2001b, 2003; Schuler and Hanke 2001/2002). Instead, the focus is on what U.S. economists said about the crisis and whether they knew what they were talking about. Business publications monitor how accurately forecasters predict the numbers in preliminary releases of macroeconomic statistics. Thorough reviews of the qualitative analysis of economists on important policy issues, though, are rare.² A review of what U.S. economists said about Argentina shows that many failed to define key terms in their 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

² Terence Hutchison (1977, 98-143) and Alan Walters (1993) surveyed the opinions of their fellow British economists on two crises of the pound sterling. The Argentine historian Ricardo Salvatore (2002) made a short survey of some views on Argentina among U.S. economists.

economists who claim expertise on controversial issues of economic policy. I hope that the failures I catalog will goad more economists to do serious research and thinking before commenting on matters that can affect the well-being of millions of people.

Table 1: Views of 100 U.S. Economists on Argentina

1a. Was Argentina’s monetary system a currency board?														
1b. Did the economist offer a definition of a currency board?														
2a. Was the peso overvalued, at least from 1999 onward?														
2b. Did the economist offer a definition of overvaluation?														
3a. Were Argentina’s exports uncompetitive?														
3b. Did the economist offer a definition of uncompetitiveness?														
4. Was dollarization technically feasible in late 2001 to early 2002?														
	<i>1a</i>		<i>1b</i>		<i>2a</i>		<i>2b</i>		<i>3a</i>		<i>3b</i>		<i>4</i>	
Proper response	No		Yes		Yes or No		Yes		No		Yes		Yes	
<i>Actual response</i>														
Yes	91		56		72		50		52		44		27	
Yes, but bad definition	NA		11		NA		10		NA		2		NA	
No	3		27		3		15		6		12		4	
Did not discuss	6		6		25		25		42		42		69	

Notes: NA = not applicable. Yes and No responses include both those that were explicit and those I classified as strongly implied; for the detailed views of each economist, see Table 2. *Source:* Table 2.

METHODS OF THIS STUDY

Table 1 summarizes the views of 100 U.S. economists or groups of coauthors who were active in offering public commentary on Argentina in the last several years. Table 2 shows the views of each economist on each question. Appendix 1 lists each economist’s relevant writings and statements on Argentina.³ It also contains citations to over 100 other, less active or less prominent commentators. That group includes, for example, economists whose only discussions of Argentina were in textbooks and

³ All appendixes can be accessed as attachments at the end of the article.

were therefore unlikely to influence the policy debates of the moment. Appendix 2 contains quotations by all of the 100 most active economists and many of the less active ones.

**Table 2:
Detailed Views of 100 U.S. Economists on Argentina**

<i>Key to headings of columns 1-5</i>								
Proper responses are indicated in bold within parentheses.								
1a. Was Argentina's monetary system a currency board? (No)								
1b. Did the economist offer a definition of a currency board? (Yes)								
2a. Was the peso overvalued, at least 1999 onward? (Yes or No)								
2b. Did the economist offer a definition of overvaluation? (Yes)								
3a. Were Argentina's exports uncompetitive? (No)								
3b. Did the economist offer a definition of uncompetitiveness? (Yes)								
4. Was dollarization technically feasible late 2001-early 2002? (Yes)								
5. Other.								
<i>Key to codes in columns 1-4 (see end of table for column 5)</i>								
✓ = Proper response (✓- if only implied).								
B = Proper response, but bad definition (columns 1b, 2b, 3b only).								
O = Improper response (O- if only implied).								
— = Did not discuss.								
<i>Name</i>	<i>1a</i>	<i>1b</i>	<i>2a</i>	<i>2b</i>	<i>3a</i>	<i>3b</i>	<i>4</i>	<i>5</i>
Mark Allen	O	O	✓-	✓	O-	O	—	—
Altig, Humpage	O	✓	—	—	—	—	—	a
Werner Baer	O	O	✓-	✓	—	—	—	a
Robert Barro	O	✓	✓	B	—	—	✓	a
Gary Becker	O	✓	✓	B	O-	O	—	b
Andrew Berg	O	✓	—	—	—	—	—	—
C. Fred Bergsten	O	✓	✓	B	O	✓	—	c, d
Nancy Birdsall	—	—	✓-	✓	—	—	—	—
Olivier Blanchard	O	✓	✓	B	O	✓	—	d
Robert Blecker	O	✓	✓	O	O	✓	—	—
Michael Bordo	O	O	✓	O	O-	O	—	a
Eduardo Borensztein	O	O	—	—	—	—	—	—
Ricardo Caballero	O	O	—	—	—	—	—	—
Charles Calomiris	O	✓	✓-	✓	—	—	—	e
Guillermo Calvo	O	✓	✓-	✓	—	—	✓	—
Roberto Chang	O	✓	✓	O	O-	O	—	a
William Cline	O	✓	✓	✓	O-	✓-	—	a
Benjamin J. Cohen	O	✓	✓-	O	O	O	—	a
W. Max Corden	O	✓	✓-	✓	O-	✓	—	a

Daseking and others	O	✓	✓	✓	O-	✓	✓	a
Augusto de la Torre	O	✓	✓	✓	O-	✓	✓	a
J. Bradford DeLong	O	✓	✓	B	O	B	—	—
David DeRosa	O	✓	✓	✓	O-	✓	✓	b
Padma Desai	O	✓	✓	✓	O	✓	—	a
Eugenio Díaz B.	O	✓	✓	O	✓	✓	—	a
Dominguez, Tesar	O	✓	✓	✓	—	—	—	—
Rudiger Dornbusch	O	✓	✓	✓	O-	O	—	—
Sebastian Edwards	O	B	✓	O	—	—	✓	a
Barry Eichengreen	O	✓	✓	✓	O-	O	✓	f
David Feldman	O	✓	✓	✓	✓	✓	✓	e, g
Martin Feldstein	O	B	✓	B	O	✓	—	a, d
David Felix	—	—	✓	✓	O	✓	O-	—
Stanley Fischer	O	✓	—	—	—	—	—	a
Kristin Forbes	O	O	✓	✓	—	—	O	—
Jeffrey Frankel	O	✓	✓	✓	O	✓	✓	a
Milton Friedman	O	✓	—	—	—	—	—	—
Andrés Gallo	O	✓	✓	✓	—	—	—	a
Ghosh, Gulde, Wolf	O	✓	—	—	—	—	—	a
Morris Goldstein	O	B	✓	O	O	O	—	—
Carol Graham	—	—	✓	B	O	B	—	e
David Hale	O	B	✓	✓	O-	✓	—	—
Steve H. Hanke	✓	✓	✓	✓	✓	✓	✓	—
Arnold Harberger	O	✓	✓	✓	—	—	—	a
Ricardo Hausmann	O	✓	✓	✓	✓	✓	✓	—
R. Glenn Hubbard	O	B	—	—	—	—	—	—
Jiri Jonas	O	✓	✓	✓	O	✓	✓	—
Graciela Kaminsky	O	O	—	—	—	—	—	—
Timothy Kehoe	✓	O	✓	✓	✓	✓	—	—
Peter Kenen	O	✓	✓	O	—	—	—	—
Jan Kregel	O	✓	✓	✓	O-	✓	—	—
Anne Krueger	O	O	✓	✓	O	✓	O	a
Paul Krugman	O	✓	✓	B	O	✓	✓	a
Finn Kydland	O	✓	—	—	—	—	—	a
Adam Lerrick	O-	O-	—	—	—	—	—	—
Paul Masson	O	✓	✓	O	—	—	—	e
Ronald McKinnon	O	O	—	—	O	✓	—	—
Allan Meltzer	O	O	✓	O	—	—	—	—
Frederic Mishkin	O	✓	✓	✓	O-	✓	—	a
Ramon Moreno	O	O	✓	✓	O-	O	—	a, b
Robert Mundell	O	✓	✓	O	—	—	—	a
Michael Mussa	O	✓	✓	✓	O-	✓	O	a
Maurice Obstfeld	O	✓	✓	B	—	—	—	a

Scott Pardee	O	B	✓	✓	O-	✓	—	—
M. Pastor, C. Wise	O	O	✓	✓	O	✓	✓	—
Guillermo Perry	O	O	✓	✓	O-	✓	✓	—
Michael Pettis	O	O	—	—	—	—	—	—
Edmund Phelps	O	O	—	—	—	—	—	—
Arturo Porzecanski	—	—	✓	O	O-	O	✓	—
M. Quispe-Agnoli	O	O	✓	O	O-	O	—	a
Carmen Reinhart	O	✓	—	—	—	—	—	—
Dani Rodrik	O	O	✓	O	O	✓	—	—
Liliana Rojas-Suárez	O	O	✓	✓	O	✓	—	—
Andrew Rose	O	O	—	—	—	—	—	—
Nouriel Roubini	O	B	✓	✓	O-	✓	✓	—
Jeffrey Sachs	O	✓	✓	✓	O	✓	✓	—
Dominick Salvatore	O	✓	✓	✓	O	✓	✓	—
Paul Samuelson	O	O	—	—	—	—	—	—
Miguel Savastano	O	✓	—	—	—	—	—	a
Sergio Schmukler	O	✓	✓	✓	O-	✓	✓	a
Kurt Schuler	✓	✓	✓	✓	✓	✓	✓	—
Luis Servén	O	O	✓	✓	O-	✓	—	a
Brad Setser	O	O	✓	✓	—	—	—	—
Mark Spiegel	O	✓	✓	✓	O	✓	✓	a
Joseph Stiglitz	O	B	✓	✓	O	✓	✓	—
Lawrence Summers	O	✓	—	—	—	—	—	—
John Taylor	—	—	—	—	O-	✓	✓	—
Lance Taylor	—	—	✓	✓	O-	✓	—	—
James Tobin	O	B	—	—	—	—	—	—
Edwin Truman	O	✓	✓	✓	O-	✓	—	a
Martín Uribe	O	✓	—	—	—	—	—	—
Carlos Végh G.	O	O	—	—	—	—	—	a
Andrés Velasco	O	O	✓	✓	O	✓	—	—
Velde, Veracierto	O	✓	—	—	—	—	—	a
G. von Furstenberg	O	✓	✓	✓	—	—	—	a
Sidney Weintraub	O	B	✓	✓	O	✓	✓	—
Mark Weisbrot	O	B	✓	B	—	—	—	—
Onno Wijnholds	O	O	✓	✓	—	—	—	—
Thomas Willett	O	✓	✓	O	O	O	✓	a
John Williamson	O	✓	✓	✓	O-	✓	—	a
Carlos Zarazaga	O	✓	—	—	—	—	—	a

Key to letter codes in column 5:

a = Occasionally mentioned that the convertibility system was not an orthodox currency board, but on balance seemed to consider the system a currency board.

b = Considered the peso overvalued, but did not think overvaluation was an important cause of Argentina's crisis.

- c = Defined overvaluation with respect to the bilateral exchange rate with the United States.
- d = Defined uncompetitiveness as a trade deficit or a growing current-account deficit that in fact did not exist at the time he commented.
- e = Stated or implied that the peso was overvalued, but favored addressing overvaluation by means other than devaluation.
- f = Favored combining dollarization with a one-shot devaluation.
- g = Seemingly contradictory passages on whether exports were competitive.
- Sources:* Appendixes 1 and 2.

The economists listed in Table 2 are the cream of the crop. More than half are or have been full professors at leading universities, while many of the rest hold research positions of comparable rank at international financial institutions, think tanks, or the Federal Reserve System. Seven have won the Nobel Memorial Prize in economics. At least 15 have held top policy-making positions at the IMF, World Bank, Inter-American Development Bank, Council of Economic Advisers, or U.S. Treasury Department, and at least 15 more have held other upper-level positions.⁴ Appendixes 1 and 2 list the affiliations of all commentators.

To gather material, I searched the Library of Congress, EconLit, Nexis, and the Internet. The commentators are mainly academic economists, because it was their ideas that set the terms of debate on Argentina's crisis. The study omits a number of business economists,

⁴ The winners of the Nobel Memorial Prize are Gary Becker, Milton Friedman, Finn Kydland, Robert Mundell, Paul Samuelson, Joseph Stiglitz, and James Tobin. Economists who have held top policy-making positions include, at the IMF, Stanley Fischer (first deputy director and later acting managing director, 1994-August 2001), Anne Krueger (first deputy managing director since September 2001), Michael Mussa (director of research, 1991-2001), and Onno Wijnholds (executive director, 1994-2002); at the World Bank, Stanley Fischer (chief economist, 1988-1990) and Joseph Stiglitz (chief economist, 1997-February 2000); at the Council of Economic Advisers, Martin Feldstein (chairman, 1982-1984), R. Glenn Hubbard (chairman, May 2001-February 2003), Joseph Stiglitz (member and later chairman, 1993-1997), Kristin Forbes (member, November 2003-June 2005), and Jeffrey Frankel (member, 1997-March 1999); at the Inter-American Development Bank, Guillermo Calvo (chief economist since June 2001) and Ricardo Hausmann (chief economist, 1994-2000); at the U.S. Treasury, Lawrence Summers (under secretary for international affairs, 1993-July 1999, and Secretary of the Treasury, July 1999-January 2001), John Taylor (under secretary for international affairs, June 2001-April 2005), C. Fred Bergsten (assistant secretary for international affairs, 1977-1981), and Edwin Truman (assistant secretary for international affairs, December 1998-January 2001). Kristin Forbes held another upper-level position at the U.S. Treasury that included responsibility for Argentina (deputy assistant secretary for quantitative policy analysis, Latin America, and Caribbean nations, 2001-2002).

economic journalists, political scientists, historians, and others who offered interesting commentary but were more repeaters than originators of economic analysis. (It is worth noting that the business economists, in this case mainly analysts for investment banks, were generally better informed about the facts than academic economists. Investment bank newsletters consist mainly of analysts' remarks on news and statistics, so as part of their job, analysts were required to follow the data more closely than academic economists did.) The selection is sufficiently broad to include the major currents and many minor currents of opinion about Argentina among U.S. economists. The material selected is mainly from 1999, when Brazil's devaluation focused attention on Argentina, to 2002, when Argentina abandoned the convertibility system near the start of the year. However, the material extends as far back as 1991 for a few economists who wrote about Argentina's convertibility system before 1999, and as recently as early 2005 for some major postmortem analyses.

I tried to find everything important that all commentators listed said or wrote about Argentina. Where an economist expressed opinions about Argentina in different formats, I usually preferred books to journal articles and other essays, essays to newspaper op-eds, and newspaper op-eds to newspaper interviews. Newspaper articles are often edited by persons who have no expertise about the topics being discussed, and it is almost always editors rather than authors who choose the titles. The chance of getting the undiluted flavor of a writer's ideas is greatest in longer writings. I also bore in mind that economists who work in high-level positions in governments or international organizations sometimes consider it inappropriate to express their personal views candidly (as Stanley Fischer 2004, 224, remarks about some statements he made on Argentina when he worked at the IMF). I was able to contact almost all of the economists in Table 2 by electronic mail to ask if they had publications relevant to Argentina, and I thank those who supplied me with publications I had missed. In many cases, the table pieces together economists' views from multiple writings, on the assumption that except where a change of opinion is obvious, the views remained the same.

The selection of U.S. economists includes foreigners who live in the United States. Widening the pool in this way extends membership to expatriate Latin Americans, who may understand Argentina's economy and history better than most economists born and raised in the United States. International organizations employ a number of such foreign nationals.

I am not an impartial spectator. I have been writing about Argentina's monetary system since 1990, and have expressed views few

economists share. However, I admire informed analysis from other perspectives, and will mention examples of good scholarship from those perspectives.

WAS THE CONVERTIBILITY SYSTEM A CURRENCY BOARD? NO

Argentina's "convertibility" monetary system began on April 1, 1991. It attempted to end Argentina's problems with chronically high inflation by linking Argentina's currency, the austral, to the U.S. dollar at a selling rate of 10,000 australes per dollar. The law set no buying rate, allowing in principle for an appreciation against the dollar. On January 1, 1992, the peso, Argentina's present currency, replaced the austral at 1 peso = 10,000 australes = US\$1.

Table 1 shows that 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.

An orthodox currency board is a monetary authority that issues notes and coins (and deposits, if any) fully backed by foreign reserves and fully convertible at a fixed exchange rate into an anchor currency. In combination, these characteristics imply that a currency board has no room for discretionary monetary policy. In particular, a currency board has not only a minimum ratio of 100 percent net reserves, held exclusively in foreign assets, but a maximum ratio. Historically, the maximum ratio has often been 100 percent; if higher, it has typically been 110 percent. When a currency board accumulates reserves in excess of the maximum, it periodically pays them out as profits (seigniorage). The purpose of the reserves in excess of 100 percent, if they exist, is to provide a cushion against losses in the capital value of assets, so that the reserve ratio always remains at least 100 percent.

An orthodox currency board does not hold significant domestic assets; does not engage in sterilized intervention (buying or selling domestic assets to offset the effects on the money supply of gaining or losing foreign

reserves); does not lend to the government; and does not act as a lender of last resort to banks. Acceptance of this definition extends to a number of economists, mentioned below, who are critical of currency boards. The definition has a background in economic theory stretching back about 180 years, and dozens of countries have in fact had currency boards that have fit the definition (Schuler 1992).

Argentina's convertibility system maintained a rigid exchange rate with the U.S. dollar and for most of its life imposed no restrictions on converting local currency into dollars, but in other respects it was unlike a currency board. Argentina never established a separate body to act as a currency board, nor did it establish a separate division within its central bank or even a separate balance sheet. Instead, the central bank retained its previous organizational structure, but was subjected to some new rules. The main legal basis of the convertibility system, the Convertibility Law of 1991 (Law 23.928), explicitly allowed the central bank to count Argentine government bonds payable in foreign currency as reserves, setting no limit on the extent of the practice. Later laws reduced the sized of this loophole, ultimately imposing a minimum foreign reserve requirement of 66-2/3 percent against the monetary base (Law 24.144, Article 1, [sub]articles 20, 33, and 60). No maximum existed. Foreign reserves could be held in assets payable in precious metals or currencies including the dollar, but no requirement existed that reserves be held in any particular currency.

**Table 3: Argentina's Central Bank During "Convertibility"
(percentages; expected ratios for a currency board: 100 percent)**

Median monthly ratio of foreign assets to total assets	34
Median monthly ratio of net foreign assets to monetary base	76
Correlation of monthly change in net foreign assets and change in monetary base	47
Median monthly reserve pass-through (ratio of change in monetary base to change in net foreign reserves)	31
Total reserve pass-through (sum for whole period)	241

Source: Appendix 3. Underlying data are from IMF, *International Financial Statistics* database, November 2004, lines 11-17, for the period April 1991-December 2001.

If one is looking at balance sheet figures of an orthodox currency board, there are several ratios that should be near or equal to 100 percent. Table 3 shows that for Argentina's central bank during the convertibility system, the ratios were far from 100 percent. More detailed data, in

Appendix 3, indicate no tendency for the system to become more like a currency board over time. For instance, the central bank's claims on the Argentine government, which are a type of domestic asset, ballooned from roughly 20 percent of total assets at the end of 2000 to 50 percent at the end of 2001. A currency board would have held no such assets. The central bank at some times engaged in sterilized intervention and at other times the opposite practice, namely, amplifying the effect on the monetary base of changes in foreign reserves. That is why the correlation figure and the reserve pass-through figures are far from 100 percent. The central bank sterilized through a number of channels, including lending to commercial banks through repurchase (repo) and swap arrangements.⁵

This review of the workings of the convertibility system, though brief, is already as long as many newspaper articles. It is not surprising, then, that many short popular writings by economists contained no definition of a currency board. Newspaper editors are prone to cutting technical discussions as a way of fitting articles into the limited space of a newspaper page. In longer writings, though, economists should define important terms that are open to differing interpretations. (The same considerations apply to the other questions of definition discussed below.) Economists who wrote longer pieces but failed to define what they meant by calling Argentina a currency board included many current or recent staff of international financial institutions.⁶

A number of economists offered definitions of a currency board that were incomplete or otherwise bad. Morris Goldstein (2002a, 21) wrote, "A currency board regime is one where the domestic currency (M0 money) is backed (usually 50 percent or more) with foreign currency, and where the currency board is obligated to convert domestic currency into foreign currency on demand at a fixed price." Goldstein included Argentina in his

⁵ For more details of the qualitative features of the convertibility system, see Schuler (2004a). Appendix 3 compares IMF data with central bank data, which use different accounting conventions but yield broadly similar results. It also includes IMF data on Bosnia, to show what the numbers look like for a system that is closer to an orthodox currency board. Finally, it includes IMF data on Brazil's *Real Plan*, which operated alongside Argentina's convertibility system. Everyone acknowledges that Brazil was then and is now a central banking system. Whether assessed qualitatively or quantitatively, the convertibility system looks more like Brazil than like Bosnia. Hanke (2002x, 205-11) reached similar conclusions.

⁶ Namely, Mark Allen (2003, 121); Guillermo Calvo, Alejandro Izquierdo, and Ernesto Talvi (2002, 35); Christina Daseking, Atish Ghosh, Timothy Lane, and Alun Thomas (2004, 1); Jiri Jonas (2002a, 1); Anne Krueger (2002c); Michael Mussa (2002d, 1—he termed the convertibility system "currency board like," with no elaboration); and Guillermo Perry and Luis Servén (2003, working paper version, 2).

list of currency boards. His definition fails to specify the minimum foreign reserve ratio of 100 percent, and the maximum of 100 to 115 percent, that prevent an orthodox currency board from undertaking sterilized intervention. Joseph Stiglitz and Carl Walsh (2002, 420) remarked, “Under a currency board the exchange rate between the local currency and, say, the dollar is fixed by law. The central bank holds enough foreign currency to back all the domestic currency and [commercial bank] reserves it has issued.” Their definition is incomplete because it fails to add that an orthodox currency board holds no domestic assets, thus precluding sterilized intervention.

Scores of economists offered correct definitions but applied them wrongly to Argentina. Thomas Willett (2002b, 52-53) noted that “Under a currency board regime, a country fixes the value of its currency to another and allows its own money supply to expand or contract only as its central bank’s holdings of the foreign currency rise or fall,” but nevertheless termed Argentina a currency board.⁷ In a comparison of stabilization programs in Argentina and Turkey, Barry Eichengreen (2002b, 111-112, n 22) commented, “the parallels [of Turkey’s monetary reform] with Argentina were extensive. The rules of the Turkish system required that the intervention of the currency be unsterilized and that the central bank had to hold foreign exchange reserves amounting to a significant fraction of the domestic currency issue. This was not a pure currency board, à la Argentina, but quite similar to one.” More than two dozen textbooks, including those by David Beim and Charles Calomiris (2001, 241), J. Bradford DeLong (2002, 436, 489-90), and Rudiger Dornbusch, Stanley Fischer, and Richard Startz (2004, 512 n. 12, 533), defined currency boards as lacking the ability to engage in sterilized intervention or other forms of discretionary monetary policy, yet still called Argentina a currency board. Milton Friedman (1998b) did likewise.

Lee Alston and Andrés Gallo (2002, 3) and Steve Hanke and I (for example, Hanke, Jonung, and Schuler 1993, 73-4) were the only economists in Table 2 who gave clear evidence of having looked at the legal framework of the convertibility system ourselves, rather than relying on summaries provided by others. Failure to read the Convertibility Law led to many muddled statements about its content, such as the claim by Sergio

⁷ Willett also railed against unnamed “fixed-rate fundamentalists,” while neglecting his own assessment of a few years earlier that despite some problems, “To date Argentina’s current program has provided the best example [among the recent Latin American stabilization attempts the authors surveyed] of success with a rigidly fixed exchange rate” (Martin, Westbrook and Willett 1999, 151).

Schmukler and Luis Servén (2002, 15 n13) that “The [Convertibility] law required the central bank to hold an amount of dollars equal to the entire monetary base at all times, although a limited proportion of this backing could be held in domestic government bonds.” As we have seen, the reserves were not required to be in dollars, though in practice most were, and the proportion of reserves against the monetary base that could be held in domestic government bonds was initially unlimited.

Many economists at some point acknowledged the differences between the convertibility system and a currency board, then proceeded as if the differences had no importance. An extreme case was Sebastian Edwards: in 1999 he described Argentina as a “(quasi)-currency board arrangement,” distinguishing it from a pure currency board (Edwards and Savastano 1999, 7), but three years later he heaped scorn on others for making the same distinction (Edwards 2002e, published version, 24). Four IMF economists (Daseking and others 2004, 1, 18-22, 27 n4) referred repeatedly to the convertibility system as a currency board, despite observing in a well-buried footnote that the correlation between the net foreign assets of the central bank and reserve money as they measured it was only 0.08, rather than 1.00 as would have been the case for a currency board. In other words, by that measure the convertibility system was 92 percent *not* a currency board. Economists should have paid far more attention to the particulars of the convertibility system, analyzing its potential to operate differently from an orthodox currency board, the extent to which it actually did so, and the effects on Argentina’s economy.

How did the idea that the convertibility system was a currency board originate and spread? The first article in Nexis that describes Argentina as a currency board without qualification is a survey of Argentina on May 14, 1992 in the *Financial Times* (Fidler 1992). It states, “The convertibility law turned the central bank into a currency board. It fixed the Argentine currency to the US dollar and only allows the central bank to issue local currency when backed by inflows of dollars to the central bank.” Anna Schwartz (1992, 17) seems to have been the first U.S. academic to associate the convertibility system with a currency board: she wrote that Argentina “possibly represents a currency board approach.” By the mid 1990s, it had become commonplace to call the convertibility system a currency board, in the business press, articles written by economists mainly for other economists, and in textbooks (for instance, *Economist* 1994; Bennett 1994; McCallum 1996, 222). Schwartz’s cautious “possibly” was neglected by later commentators, almost none of whom consulted the balance-sheet statistics of Argentina’s central bank. When the IMF revised its classifications of

exchange rate regimes in 1999, it classified Argentina as a “currency board arrangement.” It defined a currency board arrangement as “A monetary regime based on an explicit legislative commitment to exchange domestic currency for a specified foreign currency at a fixed exchange rate, combined with restrictions on the issuing authority to ensure the fulfillment of its legal obligation” (IMF 1999, 3). These vague criteria do not suffice to distinguish currency boards from cases that everyone would agree are central banks, such as the U.S. Federal Reserve System and the Bank of England for much of the time they adhered to the gold standard. Later, the IMF (2003a, 2) added to its definition, specifying that in a currency board arrangement, “domestic currency will be issued only against foreign exchange and that it remains fully backed by foreign assets.” But by then the convertibility system was dead, and the belief that it had been a currency board was a firmly entrenched part of conventional wisdom.

Calling the convertibility system a currency board was like calling a mule a horse, then wondering why it did not win the Kentucky Derby. The only economists in Table 2 who consistently stressed that the convertibility system was not a currency board, and frequently explained in some detail why, were Steve Hanke and I, in numerous writings.⁸

WAS THE PESO OVERVALUED? THE CASE NEEDED TO BE ARGUED, NOT ASSUMED

Especially after Brazil’s currency devaluation of January 1999, the view was widespread that the Argentine peso was overvalued. In an article perhaps written before Brazil devalued but not published until afterwards,

⁸ Hanke (1991) was apparently the first article by a U.S. economist on the convertibility system. The title of the article was “Argentina Should Abolish Its Central Bank,” and its thrust was that the central bank should be converted into the currency board it was not at the time. Hanke and I subsequently issued writings in almost every year of the convertibility system emphasizing that it was not a currency board. They included Hanke, Jonung, and Schuler (1993, 72-4, 77); Hanke and Schuler (1994a, 47-8); Hanke (1995m); Hanke (1996c, 19); Schuler (1996, 15); Hanke (1997a); Schuler (1997, 103); Schuler (1998); Hanke and Schuler (1999b, 8-9); Hanke (2000a, 51); Hanke (2001r); Schuler (2001b, section “Spillover from debt problems to the currency”); and Hanke and Schuler (2002, 43-44). At times (for example, Hanke 1998a), we neglected or editors suppressed the distinction between currency boards and hybrid, currency board-like systems, but in general we were consistent about the distinction.

Martin Feldstein (1999, 98-99) said Argentina had “maintained its ‘overvalued’ fixed dollar-peso exchange rate during the past decade [he should have said ‘the past eight years’] and stayed competitive because its domestic producers lowered the cost of Argentine goods by increasing productivity.” A few years later, though, he thought “An overvalued fixed exchange rate (locked at one peso per dollar since 1991) and an excessive amount of foreign debt were the two proximate causes of the Argentine crisis. Because the exchange rate was fixed at too high a level, Argentina exported too little and imported too much” (Feldstein 2002a, 8).

Impressions that the Argentine peso was overvalued came from various sources. One source was the bilateral real exchange rate of the Argentine peso against the U.S. dollar, based on consumer price indexes. Setting the index number of the real exchange rate at 100 in March 1991, just before the convertibility system began, the index peaked at 137 in January 1995; in December 2001 it was still 114. (The index is constructed so that higher numbers indicate appreciation relative to the base period.) Using *producer* or *wholesale* price indexes, though, the bilateral real exchange rate peaked in April and October 1996 at just 105; from May 1999, it fell and stayed below 100, and in December 2001 it was 94.⁹ Real exchange rates calculated multilaterally rather than against the United States alone show that under the convertibility system, measures based on producer prices remained much closer to 100 than did measures based on consumer prices (Figure 1). For gauging the effect of the real exchange rate on the competitiveness of exporters, the measure based on wholesale prices is presumably more relevant than the measure based on consumer prices, since exporters more often deal in wholesale markets than in retail markets (as Steve Hanke [1999a, 359-361] implied in an argument he made). Another possible measure is the real exchange rate based on unit labor costs. Some IMF economists (Daskeing and others 2004, 15; Jonas 2002a, 26) and I (Schuler 2003, 26) were apparently the only ones to refer to calculations of unit labor costs. Because labor productivity rose faster than wages, this measure of the real exchange rate depreciated substantially, except for a sharp but temporary reversal centered approximately on Brazil’s January 1999 currency devaluation. The growth of labor

⁹ Data are from the CEI, an Argentine government office dealing with international economic issues (<http://www.cei.gov.ar/estadistica/mercosur/cuadro32.xls> and <http://www.cei.gov.ar/estadistica/mercosur/cuadro33.xls>). For clarity, I have inverted and rebased the original numbers.

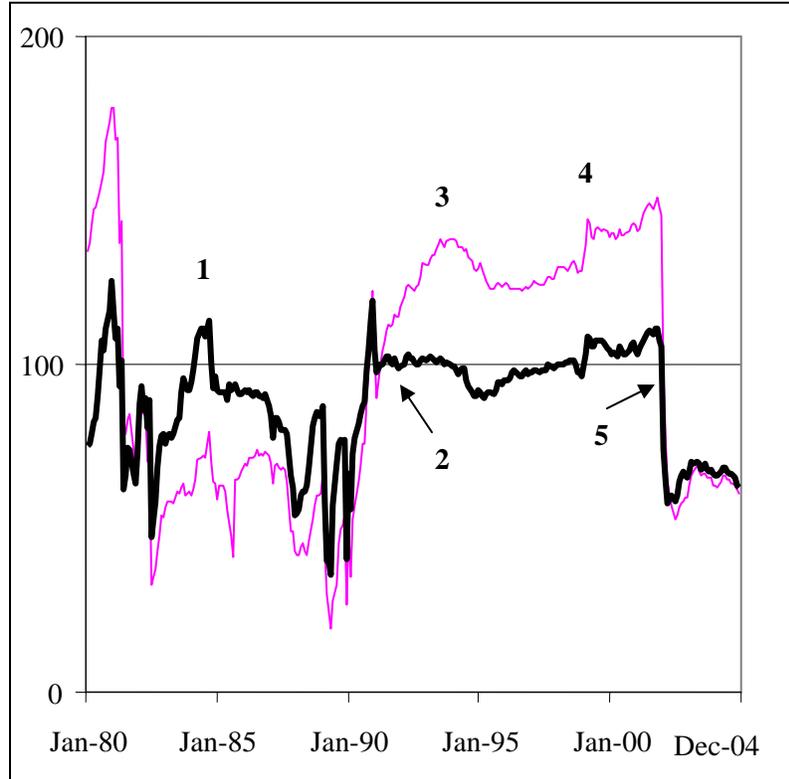
productivity under the convertibility system contrasts with the near stagnation in productivity from 1980 to 1990.¹⁰

Yet another way of trying to measure overvaluation is through economic models that estimate whether a country's foreign-exchange earnings seem sufficient to pay its foreign debt, or whether the inflows of foreign capital a country can plausibly attract seem sufficient to finance continuing deficits in its current account. When the models indicate insufficient foreign-exchange earnings or inflows of capital, economists often interpret the results as indicating that the currency should be devalued. Another possibility, which economists often neglect, is that the government should restructure its foreign debt but not necessarily devalue.

Several commentators based their case for overvaluation mainly on models (Calvo, Izquierdo, and Talvi 2002, 10-16; Perry and Servén 2003, working paper version, 17-25). William Cline (2003, 22) made simple calculations himself and cited the more complex calculations of Perry and Servén just mentioned. Jan Kregel (2003, working paper version, 5n-6n) also cited Perry and Servén's calculations, and briefly discussed their importance. Too many economists, however, took overvaluation as self-evident. They did not discuss different possible measures of overvaluation or even specify any measure according to which the peso was overvalued. It was not necessary that they should have made the measurements themselves; referring to the work of others would have sufficed. In early 2002, after seeing writings by half a dozen well-known economists asserting that the peso had been overvalued, I wrote to them asking what measure of overvaluation they were using. Sebastian Edwards immediately replied in exemplary fashion, citing two models by investment banks published in the banks' market letters. The other economists failed to respond or replied that in fact they had no specific measure in mind. Because the communications were private, I will not list names.

¹⁰ There seems to be no long series of consistent statistics of unit labor costs in Argentina, hence long-term estimates of labor productivity are subject to unusual uncertainty. Unpublished calculations by the Argentine economist Marcos Buscaglia (2002) indicate that unit labor costs increased 21 percent from March 1991 to December 1992, and peaked in February 1993 at 39 percent above their level of March 1991. A country report by the IMF (2001, 10) contains a graph of unit labor costs from 1993 to 2001. Daseking and others (2004, 19) have graphs showing stagnant or falling productivity from 1995.

Figure 1: Argentina's Real Multilateral Exchange Rates, 1980-2004
 (light line = consumer price-based; dark line = wholesale price-based; March 1991 = 100; higher numbers indicate appreciation)



- Notes: 1 = High and variable inflation of the 1980s.
 2 = Convertibility system begins, April 1991.
 3 = Tequila crisis begins, December 1994.
 4 = Brazil devalues, January 1999.
 5 = Convertibility system ends in devaluation, January 2002.

Source: CEI (<http://www.cei.gov.ar/estadistica/mercosur/cuadro37.xls> and <http://www.cei.gov.ar/estadistica/mercosur/cuadro38.xls>). For clarity, I have inverted and rebased the original numbers. Calculations from the IMF's *International Financial Statistics* database yield similar results, but as of the April 2005 release, the database no longer shows the wholesale price index for Argentina, and its coverage of the producer price index begins only with January 1994.

WERE ARGENTINE EXPORTS UNCOMPETITIVE? NO

Whether one thinks Argentine exports were uncompetitive follows closely from whether one thinks the peso was overvalued. Under the convertibility system, Argentina often had deficits in both its trade account and current account. Some observers took the deficits as indications that the Argentine exporters were uncompetitive because the peso was overvalued.

Table 4 shows some pertinent facts about Argentina's trade under the convertibility system. The dollar value of exports grew every year except 1991 and 1999. In 1991, exports fell enough in the first quarter, before the convertibility system began, to cause shrinkage for the year as a whole. In 1998, exports rose, but the rise was minuscule. In 1999, falling prices for commodities and the effect of Brazil's currency devaluation in January caused exports to shrink.

Because the convertibility system began in April 1991, Table 4 uses 1990 as the base year. From 1990 to 2001, exports, measured in current U.S. dollars, grew an average of 7.2 percent a year. In comparison, exports grew an average of only 4.0 percent a year from 1980 to 1990. *Industrial* exports grew an average of 8.6 percent a year from 1990 to 2001, versus 7.3 percent a year from 1980 to 1990. Argentina was not just relying on farm products, its export mainstay since the 1800s. Adjusted for inflation, export performance under the convertibility system looks even better, because inflation in the dollar was lower in the 1990s than in the 1980s.¹¹ Despite a setback in 1999, the trend under the convertibility system was for Argentine exports to become more rather than less competitive. In 2000 and 2001, exports were among the few growing sectors of the economy.

Many economists failed to define what they meant when they called Argentine exports, or the Argentine economy, uncompetitive. Because a notion of uncompetitiveness follows from the idea of overvaluation, it is understandable that economists who had already defined in what sense they considered the Argentine peso overvalued should not have offered a further definition of uncompetitiveness. Still, in light of the statistics, it is worth

¹¹ For example, the U.S. producer price index for finished goods rose an average of 1.5 percent a year from 1990 to 2001, versus 2.8 percent a year from 1980 to 1990. Figures here and in the main text are from the Web sites of INDEC (Argentina's statistical agency) and the U.S. Bureau of Labor Statistics. Averages, here and elsewhere in the text, are compound annual figures calculated from the endpoints of each period.

asking why exports were growing at a respectable pace if the peso was overvalued.

Some economists assumed that the fall in exports in 1999, resulting from Brazil's devaluation, had continued into 2000 and 2001. C. Fred Bergsten (2001) claimed in an interview that "with the rigid exchange rate relationship, Argentine products became increasingly overpriced in world markets. They lost export sales." Bergsten's remark implies that Argentina's exports were falling at the time he spoke, which was not the case. It is hard to give an interview and hunt through statistics simultaneously, though, so Bergsten's comments may have been nothing more than a slip of the tongue. Martin Feldstein (2003, 6—written in October 2001), stated, "Brazil, Argentina's largest competitor, has a floating exchange rate [since January 1999] that has made the Brazilian *real* increasingly competitive and the Argentine peso increasingly uncompetitive. The result has been to create a growing trade deficit in Argentina." Several lines later, he claimed that "the sharp decline of the Brazilian real has caused a major increase in Argentina's current account deficit." Paul Krugman (2004a), in an article focusing on U.S. budget deficits, remarked in passing that "Argentina retained the confidence of international investors almost to the end of the 1990's. Analysts shrugged off its large budget and trade deficits." Krugman and Maurice Obstfeld (2003, 694) contended that after Brazil's devaluation, "the country's current account deficit remained high," although they did not define what they meant by "high." In their textbooks, Olivier Blanchard (2003, 454) and David Colander (2004, 788) implied that following Brazil's devaluation of January 1999, Argentina's trade deficits increased. Robert Blecker (2003) also claimed that "Argentina developed a huge trade deficit" by the late 1990s.

Had these economists looked at the statistics, they would have seen that at its peak of the late 1990s, in 1998, the trade deficit was only 1.6 percent of GDP. The current-account deficit was much larger, at 4.8 percent of GDP. The trade deficit shrank in 1999 and turned to surplus in 2000 and 2001. On a year-over-year basis, the current-account deficit shrank every quarter from the first quarter of 1999 until turning to surplus in the last quarter of 2001. (Incidentally, if current-account deficits indicate an overvalued currency, it is hard to explain why Argentina had them every year from 1979 to 2001 except 1990, despite currency depreciations that at times made the real exchange rate undervalued according to calculations such as those of Figure 1.)

Table 4: Argentina's Foreign Trade
(in billions of U.S. dollars unless indicated)

	1990	1991	1992	1993	1994	1995
Exports	12.4	12.0	12.4	13.3	16.0	21.2
Trade balance	8.3	3.9	-2.6	-3.7	-5.8	0.8
—with Brazil	0.9	0.4	-1.4	-0.5	-0.3	2.1
Current account	4.6	-0.6	-5.7	-8.2	-11.1	-5.2
Capital account	-5.9	0.2	7.6	20.3	11.4	5.0
Nominal GDP	141	190	229	237	258	258
Share of world exports (%)	0.36	0.34	0.32	0.35	0.37	0.41
Export volume (1990 = 100)	100	98	97	104	122	152
Export prices (1990 = 100)	100	98	102	102	105	111
	1996	1997	1998	1999	2000	2001
Exports	24.0	26.4	26.4	23.3	26.3	26.5
Trade balance	0.0	-4.0	-4.9	-2.2	1.1	6.2
—with Brazil	2.1	2.1	1.7	0.8	1.0	1.5
Current account	-6.8	-12.2	-14.5	-11.9	-9.0	-3.9
Capital account	11.8	16.8	19.0	14.6	8.0	-14.8
Nominal GDP	272	293	299	284	284	269
Share of world exports (%)	0.44	0.47	0.49	0.41	0.41	0.43
Export volume (1990 = 100)	162	187	208	207	212	221
Export prices (1990 = 100)	119	115	103	91	100	97

Notes: The table uses 1990 as the base year because it was the last full year before the convertibility system began. The trade balance uses cost, insurance, and freight (c.i.f.) numbers for imports rather than lower free on board (f.o.b.) numbers. The trade balance with Brazil is calculated from each country's statistics for imports from the other.

Sources: IMF, *Direction of Trade Statistics* database, August 2004 (trade balance with Brazil); *International Financial Statistics* database, August 2004 (other statistics).

Taking a longer-term view, Sebastian Edwards (2002a) commented, “During the last decade two factors have contributed to Argentina’s poor export performance and [poor] productivity growth: an overvalued exchange rate; and membership in Mercosur, the regional trading bloc that includes Chile, Bolivia, Brazil, Paraguay and Uruguay.” Anne Krueger (2002c) likewise spoke of Argentina’s “weak export growth relative to other Latin American countries.” The fact is that under the convertibility system, Argentina’s share of world exports of goods made its first sustained rise since the 1940s. From 1990 to 2001, exports in current U.S. dollars rose 115 percent. Argentina’s growth in exports was close to Chile’s 118 percent and Peru’s 117 percent. Mexico was the only major Latin American economy where exports rose substantially faster: 289 percent, as a result of the North American Free Trade Agreement. Argentina outperformed Brazil, Colombia, and Venezuela, as well as Canada and the United States.¹² A look at the destination of Argentina’s exports suggests that Mercosur did not artificially inflate overall exports. Mercosur came into effect at the start of 1995. The annual rate of growth of exports to the Mercosur bloc was much higher from 1990 to 1994 than from 1994 to 2001, while combined exports to other countries showed the opposite pattern.¹³

In summary, economists who criticized the convertibility system for making Argentina uncompetitive neglected the supporting data on trade. The enormous nominal and real depreciation of the peso since January 2002 has not resulted in unusually high growth, measured in dollars. The value of exports grew 9.1 percent a year from 2001 to 2004, versus 7.2 percent a year under the convertibility system. However, most of the recent growth was the result of higher world prices for Argentina’s agricultural

¹² Statistics are from the IMF’s *International Financial Statistics* database, August 2004. Data for exports of goods and services (rather than goods alone) yield similar results. Perry and Servén (2003, working paper version, 19) purport to show that Argentina’s export performance was mediocre by using data on exports expressed in constant 1995 U.S. dollars, from the World Bank’s *World Development Indicators* database. They compare Argentina with a group of seven other major Latin American economies. Group exports are simply the sum of each country’s exports. The group looks as if it performed strongly, growing an average of 9.1 percent a year for the years 1992-2001, versus 7.6 percent a year for Argentina. However, the result is driven by Mexico, which had 36 percent of the group’s exports in 1991 and 48 percent in 2001. Giving equal weighting to each country instead of to each dollar of exports, the seven major economies grew an average of 7.3 percent a year. (Servén kindly provided me with a spreadsheet copy of the source data and calculations.)

¹³ See CEI statistics (<http://www.cei.gov.ar/estadistica/mercosur/cuadro14.xls>). Chile became an associate member of Mercosur in October 1996. The statements in the main text hold whether or not one counts it as part of Mercosur. Argentina had a prior trade agreement with Brazil, ratified in 1989 (Law 23.695).

exports. The *volume* (quantity) of exports grew only 4.1 percent a year from 2001 to 2004, versus 7.5 percent a year under the convertibility system. Argentina's share of world exports of goods has fallen every year since the convertibility system ended. Official projections for 2005 indicate a further decline, to 0.36 percent, although if the rapid pace of growth in the first four months of the year continues, Argentina's share will remain nearly unchanged at 0.39 percent. Argentina's trade balance with Brazil was in deficit in 2004 and will probably remain so in 2005, compared to surpluses from 1995 to 2003.¹⁴

Only a handful of economists who commented on the competitiveness of exports showed signs of having examined the trade data in some detail. I noted the growth of exports under the convertibility system (Schuler 2001b, section "Is the peso overvalued?"). David Feldman (2002a) considered the peso overvalued, but remarked that Argentina's exports had been growing. (Feldman 2002b, however, called Argentine goods "overpriced.") Feldman correctly understood that uncompetitiveness does not follow automatically from overvaluation. Earlier, Feldman (2001b) had advocated dollarization at one peso per dollar as preferable to devaluation. After Argentina devalued, Feldman was again notable for his correct minority view that Argentina's competitiveness, as measured by exports, was unlikely to improve substantially. Jiri Jonas (2002a, 26-27) analyzed import and export data and mentioned the changes in Argentina's share of world exports. Carolina Díaz Bonilla and others (2004, 7-8) noted the growth in Argentina's share of world exports under the convertibility system. Like Feldman, they distinguished between export competitiveness and what they considered to be an overvalued peso. Steve Hanke (2002f, 5) made the same distinction, though he did not consider the peso overvalued.¹⁵

¹⁴ Historical statistics are calculations from data on INDEC's Web site. The conjecture about the trade balance with Brazil in 2005 is based on the same source. The official projections underlying my calculation of Argentina's share of world exports in 2005 are a forecast of Argentina's exports by its Ministry of Economy (reported in Quiroga 2005) and a forecast of world exports of goods by the IMF (2005, 230).

¹⁵ An anonymous referee has suggested a more charitable interpretation of claims by many U.S. economists that a devaluation of the peso was advisable to restore Argentina's export competitiveness. He has proposed that the claims were a shorthand for addressing the divergence between the prices of tradable goods, which rose little under the convertibility system, and prices of nontradable goods, which rose substantially. Inefficient government and government-granted monopoly privileges to some privatized government agencies, such as telephone companies, hindered competitiveness insofar as even tradable goods have some nontradable inputs. It can be argued that devaluation was a blunt but necessary tool for

WAS DOLLARIZATION TECHNICALLY FEASIBLE? YES

On January 15, 1999, during Brazil's currency crisis, a key staff official working for Argentine president Carlos Menem announced that Menem was considering replacing the peso with the dollar. Dollarization had been suggested for Argentina before: economy minister Domingo Cavallo had mentioned it in March 1995, during a previous currency crisis. Never had it received such favorable consideration at the highest level, though. After the announcement of Menem's interest in dollarization, a number of economists analyzed the desirability and feasibility of dollarization in Argentina. Most who expressed an opinion considered dollarization feasible, but the doubters included some economists who were in or had only recently left key policy-making positions. I will leave the reader to decide the question of desirability. The question of technical feasibility is less a matter of judgment and more a matter of fact.

One question is whether dollarization would have been feasible at the rate of one peso per dollar that prevailed under the convertibility system. An important factor was the availability of foreign reserves. In an article published on November 12, 2001, Michael Mussa (2001), former director of research at the IMF, claimed that "the resources necessary to sustain the currency board are not available. Financing of fiscal deficits and deposit outflows have driven Argentina's foreign currency reserves below the monetary base." The balance sheet of the Argentine central bank from Friday, November 9, the last date Mussa could have seen, indicated that foreign reserves were 17.583 billion pesos, while monetary liabilities were 16.030 billion pesos and the monetary base was 11.858 billion pesos. Months later, Kristin Forbes (2002) similarly claimed that before the devaluation of the peso, there were not enough reserves to dollarize. Mussa and Forbes seem to have relied on figures from the IMF, which are compiled using accounting conventions different from those of the central bank. According to IMF statistics, *gross* foreign reserves exceeded the

overcoming nominal rigidities in the prices of nontradable goods. Argentine economists debated these ideas, especially in 2001, when minister of economy Domingo Cavallo proposed measures to attack the "Argentine premium" in prices (*costo argentino*). My discussion reflects the crude treatment most U.S. economists offered, rather than the more subtle points Argentine economists considered.

monetary base in September and October 2001, the last of the IMF's monthly figures Mussa could have seen. *Net* foreign reserves (foreign assets minus foreign liabilities) were below the monetary base, according to the IMF, but that need not prevent dollarization.

In a press conference of January 2002 shortly after Argentina abandoned the convertibility system, a questioner asked Anne Krueger, the IMF's first deputy managing director, "What about dollarization, is that included or excluded?" Krueger (2002a) replied, "Well, my understanding at the moment is that that is technically unfeasible. So I don't think the authorities are thinking about it, I don't think we are thinking about it. They have already said they are going to a floating rate regime, and we are just accepting that." An advisory panel of former central bank officials sent by the IMF to Argentina in July 2002 also discouraged dollarization without serious analysis (see Alejandra Gallo 2002). Even after abandoning the convertibility system, Argentina could have dollarized at one peso per dollar as long as it had or could obtain gross reserves (not necessarily net reserves) equal to the monetary base.

Another question is whether dollarization was feasible at a rate other than one peso per dollar. Even with small reserves, at a suitably depreciated exchange rate, dollarization is *always* technically feasible. At a rate of a billion pesos per dollar, the dollars many an Argentine carries in his wallet would have been more than sufficient for dollarization. Steve Hanke (2002c) requested that Krueger and the IMF reveal their reasoning for calling dollarization technically infeasible, but no IMF official ever responded. Hanke and I (for example, Schuler and Hanke 2001/2002, 13) were the only economists who made explicit reference to the balance sheet of Argentina's central bank, as posted on its Web site, in discussions of the feasibility of dollarization. Although I consider that in this case the majority view was correct, few U.S. economists who held it referred to data that might have supported that view.

**A CASE STUDY IN CARELESSNESS:
PAUL KRUGMAN**

The case of Paul Krugman illustrates in detail how U.S. economists failed to do the work necessary for understanding Argentina's situation accurately. Krugman is one of the best-known active economists. His twice-weekly column in the *New York Times* is read across the United States and, through syndication and translation, around the world. He specializes in international economics and is highly regarded by many economists. He wrote more than a dozen articles on Argentina's economic crisis.

In his book *Pop Internationalism*, Krugman (1996b, vii-ix, 118-19) decried people who comment publicly on international economic affairs without even knowing the basic ideas to be found in textbooks on international economics. But perhaps people do not consult the textbooks because textbook writers too often fail to check their assertions against the "book of the world." A reader of the widely used undergraduate textbook on international economics that Krugman wrote with Maurice Obstfeld will find this about currency boards:

Argentina's 1991 monetary law requiring 100 percent foreign exchange backing for the monetary base made it an example of a **currency board**, in which the monetary base is backed entirely by foreign currency and the central bank therefore holds no domestic assets. . . .

In a currency board regime, a note-issuing authority announces an exchange rate against some foreign currency and, at that rate, simply carries out any trades of domestic currency notes against the foreign currency that the public initiates. The currency board is prohibited by law from acquiring any *domestic* assets, so all the currency it issues automatically is fully backed by foreign reserves. In most cases the note-issuing authority is not even a central bank: its primary role could be performed as well by a vending machine.

Currency boards originally arose in the colonial territories of European powers. By adopting a currency board system, the colony effectively let its imperial ruler run its

monetary policy, at the same time handing the ruling country all seigniorage coming from the colony's demand for money. . . .

More recently, the automatic, vending machine character of currency boards has been seen as a way to import anti-inflation credibility from the country to which the domestic currency is pegged. Thus Argentina, with its experience of hyperinflation, mandated a currency board rule in its 1991 Convertibility Law in an attempt to convince a skeptical world that it would not even have the option of inflationary policies in the future. . . .

Since the currency board may not acquire domestic assets, it cannot lend currency freely to domestic banks in times of financial panic (a problem Argentina encountered frequently, as we have seen). . . .

Since a currency board typically may not acquire government debt, some argue that it can discourage fiscal deficits, thus reducing a major cost of inflation and devaluation (although Argentina's experience in this area provides a counter-example). (Krugman and Obstfeld 2003, 695-696)

Krugman and Obstfeld's theoretical definition of a currency board is reasonably accurate, and in a footnote, they observe, "Strictly speaking, Argentina's currency board involved a slight fudge. A limited fraction of the monetary base could be held in the form of U.S. dollar-denominated Argentine government debt." On most other points of fact they are wrong. As we have seen, the Convertibility Law (presumably the "1991 monetary law" they mention) allowed reserves to consist entirely of Argentine government bonds denominated in foreign currency, although later legislation imposed restrictions. The central bank engaged in sterilized intervention, held extensive domestic assets, lent to domestic banks, and acquired government debt. By Krugman and Obstfeld's own definition, then, the convertibility system lacked the automatic, vending machine

quality they correctly understand as being characteristic of currency boards.¹⁶

In a section of their textbook called “Argentina’s 2001-2002 crisis,” Krugman and Obstfeld (2003, 694) write, “Argentina’s rigid peg of its peso to the dollar proved increasingly painful as the dollar itself appreciated in the foreign exchange market. As panel (a) of Figure 22-3 [on page 680 of their text] shows, the peso’s real exchange rate remained high despite high domestic unemployment, and the country’s current account deficit remained high.” The graph shows data only through 1999. The notes to the graph explain that the real exchange rate is based on the “domestic price level”—apparently the consumer price index at the end of each year—calculated bilaterally against the United States. As has been mentioned, the bilateral real exchange rate based on consumer price indexes peaked in 1995 and was falling thereafter. Calculations based instead on wholesale or producer prices and updated to 2001 would show that in 2000 and 2001 the real exchange rate was perhaps *undervalued* (below the levels existing shortly before the convertibility system began). Making a plausible argument that the peso was overvalued according to Krugman and Obstfeld’s criteria requires referring to the multilateral real exchange rate, which they do not discuss, rather than to the bilateral rate with the United States. Even then, only the multilateral real exchange rate based on *consumer* prices offers much support for the claim of overvaluation.

¹⁶ Krugman and Obstfeld’s summary of currency boards is defective in other respects as well. Currency boards arose specifically in *British* colonies; there are only isolated instances of currency boards among colonies of other countries. Colonial currency boards did not hand seigniorage to “the ruling country,” but to the local colonial administration, which in many cases had elements of local representation. Nor were currency boards in British colonies a completely captive market for British government securities, because they were typically allowed to hold securities issued by other colonies and sometimes by independent countries within the sterling area. Krugman and Obstfeld claim that “Hong Kong has a currency board” and that “Similarly [to Argentina’s lack of monetary credibility in the early 1990s], Estonia and Latvia, with no recent track record of monetary policy after decades of Soviet rule, hoped to establish low-inflation reputations by setting up currency boards after they gained independence.” None of these systems fits their definition of a currency board. The Hong Kong Monetary Authority in August 1998 went so far as to buy about US\$15 billion of equities in the local stock market, an unorthodox operation even for a central bank. Its purchases of equities exceeded the monetary base. Krugman and Obstfeld confuse Latvia with Lithuania. The Estonian and Lithuanian central banks hold little in domestic assets, but their foreign assets considerably exceed 100 percent of the monetary base and hence give them freedom for discretionary monetary policy that an orthodox currency board would not have.

While Argentina's crisis was unfolding, Krugman used his *New York Times* columns to castigate the supposed currency board as the main problem: "So why is Argentina's economy depressed? Basically it comes down to the currency board, which pegs the value of the peso at one dollar and ensures (technicalities aside) that each peso in circulation is backed by a dollar in reserves" (Krugman 2001a). But it is hardly a technicality to observe that the convertibility system differed from Krugman's textbook description of a currency board not just on one or two points, but on most points. Nor does the record support Krugman's claim that "Argentina was an experiment in doing away with monetary activism. After generations of mismanagement, Argentina returned to a colonial-era monetary system, a 'currency board,' which took government out of the loop" (Krugman 2001f). A quick look at the central bank's Web site reveals that the central bank issued 210 regulations of a supposedly durable character in 2001 alone; they touched all aspects of the financial system.¹⁷ Steve Hanke (2002x, 212) identifies eight of these as being among 24 important policy decisions by Argentina's government in 2001 (and another three in early 2002) that contributed to the undoing of the convertibility system. The government was firmly in the loop.

Krugman termed the peso "severely overvalued" as early as 1995 (Krugman 1995a) and claimed after Brazil's devaluation of January 1999 that "Argentine producers find themselves priced out of world markets" (Krugman 2000b). Despite his skepticism about the exchange rate of one peso per dollar, in June 2000 he thought that persisting with it was the lesser of two evils (Krugman 2000a). By July 2001, though, the continuing contraction had changed the balance for Krugman, who wrote, "Some Wall Street analysts believe that the Argentine government will default but try to keep the peso pegged at one dollar. Maybe—but that would be a bizarre strategy, choosing the worse of two evils" (Krugman 2001a). He suggested pesifying dollar assets and liabilities: "Simply issue a decree canceling the indexation" in dollars (Krugman 2001c).

When Argentina's economic decline accelerated in 2002 after the government did what Krugman had proposed, he fell silent. To date (July 2005), he has published no analysis of why the decline was so severe, though he has restated some earlier points in Krugman (2003a).

¹⁷ Banco Central de la República Argentina (<http://www.bcra.gov.ar>>Enter>Regulations>Communications). The figure of 210 refers to the "A" communications issued (though not all published) in 2001, numbered 3208 to 3417. Over the whole life of the convertibility system there were 1596 such communications, numbered 1822 to 3417. There were also thousands of "B" and "C" communications, which were generally less important.

ARGENTINA LISTENS TO THE ECONOMISTS —AND SUFFERS

The consensus view among economists about the convertibility system and its effects on Argentina influenced the policy of the IMF and U.S. Treasury Department. During the crisis and afterwards, officials or former officials of both institutions made a number of statements repeating elements of the consensus view. Many of the officials were economists, and some of their statements appear in Appendix 2. Shapers of opinion in other professions have since faithfully repeated the consensus view among economists. For example, in an article that is in effect a supplementary chapter to his standard history of Argentina, David Rock (2002, 79), a professor of history at the University of California-Santa Barbara, identified the convertibility system as currency board. In a book looking back at Argentina's crisis, Paul Blustein (2005, 20), a business reporter for the *Washington Post*, did likewise. Moisés Naím (2005), the editor of *Foreign Policy* magazine and a former Venezuelan minister of trade and industry, claimed in a review of Blustein's book that the peso was not maintained "at a level that would stimulate exports." Electronic searches readily yield many similar instances.

In early 2002, the new government of president Eduardo Duhalde did what the consensus view recommended. Effective January 6, the government devalued the peso from the previous rate of one per dollar to 1.40 per dollar and ended the convertibility system. On January 9, the government "pesified" dollar assets and liabilities, forcibly converting them at 1 peso per dollar. On February 9 it floated the peso. These measures were part of a package that included other steps not necessarily urged by the consensus, such as converting dollar bank deposits into pesos at a differential rate of 1.40 pesos per dollar; confiscating the dollar reserves of banks and paying them only 1 peso per dollar; suspending bankruptcy proceedings; and doubling penalties for employers who dismissed workers.

The peso depreciated to nearly 4 per dollar in April 2003 before recovering somewhat. (Sebastian Edwards 2001 and David Hale 2001 were notable for correctly predicting a much larger depreciation of the peso than most observers expected.) As of July 2005, the central bank is managing the exchange rate to remain close to 2.90 pesos per dollar. For the year 2002,

the dollar value of exports fell 4.5 percent, though volume (quantity exported) rose 0.7 percent. Real GDP, which had fallen 5.5 percent in 2001, fell a record 16.3 percent in the first quarter of 2002 and 13.5 percent in the second quarter, on a year-over-year basis. The fall in real GDP for 2002 as a whole was 10.9 percent, the worst in more than a century. The unemployment rate rose to 23.6 percent in 2002 from 18.3 percent the year before, and the proportion of Argentines below the officially defined poverty line jumped to 57.5 percent from 38.3 percent the year before.¹⁸ Since 2002 the economy has recovered, though GDP per person is still below the earlier peak.¹⁹

In an economy supposedly being depressed by a currency board maintaining an overvalued currency, one would have expected a rapid increase in exports, and a rebound or at least a slowing rather than an acceleration of economic decline in 2002. To my knowledge, no advocate of devaluation or pesification before the fact has explained why Argentina's economy shrank so much in 2002 after devaluing, or why Argentina's share of world exports has been lower since the convertibility system ended than it was during the last seven years of the system. A possible explanation is that the Duhalde government was clumsy. Still, advocates of the consensus

¹⁸ Statistics are from INDEC's Web site.

¹⁹ U.S. economists have written little on Argentina's recovery; Forbes (2005) is among the few exceptions. One reason for the silence is that the recovery is still in progress; another is that economists have moved on to other subjects now that Argentina no longer so regularly appears on the front pages of U.S. newspapers. In my view, the recovery results from four factors. (1) It started from a low base because the policies of the Duhalde government in early 2002 destroyed considerable wealth. After some months, though, the destruction ceased and the economy bottomed out. (2) The big depreciation of the peso in nominal and real terms in early 2002 made import-competing industries more competitive, though not necessarily more efficient. Imports fell from US\$20.3 billion in 2001 to \$9.0 billion in 2002; in 2005, at the year-over-year growth rate of the first four months, they will be about \$30 billion, while exports will be \$40 billion. (3) Government finances have improved. Inflation and default on foreign debt reduced federal spending as defined by the IMF from 17.8 percent of GDP in 2001 to 14.6 percent in 2002. Higher revenues from the economic recovery sent the government budget into surplus in 2003, for the first time since current IMF statistics began in 1973. The budget has remained in surplus since, so deficits no longer create political pressure for policies that discourage growth, such as the three big tax increases the De la Rúa government imposed. (4) International conditions have been very favorable. The world economy has grown fast, world interest rates have been low, and commodity prices have risen sharply. Such major Argentine export commodities as petroleum, soybeans, beef, and wheat rose more than 50 percent in dollar terms from 1999 to 2004. The depreciation of the peso approximately tripled the weight of the export sector in the economy, to about 23 percent of GDP, and amplified the effect of the rise in commodity prices.

view should have considered the possibility that the cures they were proposing might be administered so clumsily as to cause great harm. Ricardo Hausmann (2001, 2002c), after first proposing to upend contracts on a massive scale by forcibly converting dollar-denominated claims into pesos, later implied his disapproval of Argentina's implementation of the idea by calling it a violation of property rights.

ONE SWALLOW DOES NOT MAKE A SUMMER

Argentina's economic crisis has led to the search, typical of such spectacular cases, for the supposed lessons of its experience. Among the conclusions that some writers have drawn are that "neoliberal" reforms were disastrous (DeLong 2002a), that allowing extensive foreign participation in the banking system was inadvisable (Stiglitz 2002a), and that currency boards and fixed exchange rates are very risky (Daseking and others 2004, 43-4; Edwards 2002e, 241). Forget for a moment that Argentina grew fastest when it undertook free-market policies most vigorously, in the early 1990s; that when banks in Argentina were almost all locally owned, the country suffered one of the most expensive banking crises on record, costing an estimated 55 percent of GDP from 1980-82; that the convertibility system was not a currency board; and that the exchange rate was not a fixed rate (in the sense Milton Friedman, Robert Mundell, and some other economists quoted in Appendix 2 use the term, to mean a rigid rate lacking sterilized intervention). The broader point is that where a large body of relevant experience exists, generalizing from a single case is supremely unscientific.

In the period 1999-2001, when analysis placing Argentina's crisis within a wider historical, political, or cross-country context could have done the most good, only two U.S. economists provided it. One was Manuel Pastor, Jr.: with Carol Wise, a specialist in international relations, he wrote a long article that discussed the roots of Argentina's political and economic deadlock (Pastor and Wise 2001b). The other was Steve Hanke, who provided a running commentary on events in a series of articles in the U.S. and Argentine press. Since the start of 2002 there have been many analyses that have brought historical and political insights to bear on analysis of the crisis, but they were too late to be of use in preventing the crisis. (One of my own efforts [Schuler 2003] falls into this category.)

CONCLUSIONS

Economics is often derided as mere armchair analysis. Today, though, armchair analysis need not mean uninformed commentary. Through the Internet, researchers can gain instant access to official statistics, read local newspapers, and correspond easily with local contacts. The gap between the knowledge available to somebody on the scene and somebody thousands of miles away has greatly diminished. By 2001, extensive official information on Argentina was available for free. Argentina's central bank had on its Web site the texts of the Convertibility Law, the law of the central bank, recent balance sheets, annual reports stretching back a few years, and monetary statistics back to 1989. Most of that material was available in English translation as well as in Spanish. The texts of laws and decrees relevant to the convertibility system were available in Spanish on the Web site of Infoleg, a service of the Ministry of Economy. INDEC, Argentina's national statistical agency, had extensive statistics on its site, though at the time only in Spanish. The IMF also made available considerable information. Its country reports were available for free on its Web site. The published volumes and database of the IMF's *International Financial Statistics* were available only by subscription, but could be found in many university libraries.

Almost no U.S. economists showed signs of having researched the relevant legal and statistical information in depth. Jan Kregel (2003, 21 n27) and Steve Hanke and I (for instance, Hanke and Schuler 1999b, 3) were apparently the only economists who cited any Argentine law or decree by number. A handful of economists, including Michael Mussa (2002d, 46-47), examined data from the central bank, though apparently not the whole range available. A somewhat larger group, including Mark Weisbrot and Dean Baker (2002a) and Werner Baer, Pedro Elosegui, and Andrés Gallo (2001), used data from the national statistical agency and other parts of the Ministry of Economy. Many economists seem to have looked at *International Financial Statistics*, but several made statements that revealed they had consulted no statistics.

Mistakes can arise from many sources: reliance on inaccurate data, our incomplete understanding of complex situations, even something as elementary as errors of transcription. In my own writings I have at times committed each of these kinds of error, so I do not criticize my fellow economists simply for having made mistakes. My criticism is that as a

group, economists were slaves to assumptions that they failed to check against readily available facts. Scattered individual errors would have been regrettable, but unimportant. The problem was systemic: economists failed to exercise the individual diligence and mutual scrutiny that is supposed to prevent elementary errors from becoming conventional wisdom. The “experts” assumed that the consensus of opinion was correct and did not bother to spend a few minutes checking the sources that would have confirmed or rejected it.²⁰

Failure to check the facts resulted in egregious errors by many of the most prominent economists in the United States. I have already mentioned some choice examples, but the prize goes to Maurice Obstfeld (2000, 21). In the quarterly review of the National Bureau of Economic Research, he wrote, “Argentina, in the wake of hyperinflation in 1991, wrote into its constitution a currency board system under which all base money is backed by foreign reserves and domestic pesos are convertible into dollars at a 1:1 rate.” Obstfeld packed three major errors into one sentence: the convertibility system was part of statutory law, rather than being written into Argentina’s constitution; the system was not a currency board by the definition Obstfeld himself has offered in various editions of his textbook with Paul Krugman (for instance, Krugman and Obstfeld 2003, 695); and all base money was not necessarily backed by foreign reserves.

Too often, economists also failed to define the terms they were using. They proceeded as if it were obvious what was meant by a currency board, or an overvalued exchange rate, or uncompetitive exports. Even where they did offer definitions, in many cases the definitions were bad or cursory. Few discussions went below the surface to explore, for example, whether different measures of overvaluation yielded different results, and what the differences might imply about Argentina’s situation.

The Argentine episode, though but a few years old, has already passed from current events into mythology, skipping the intermediate stage, history. The bald one- or two-page summaries of Argentina’s experience found in textbooks bid fair to make immortal the errors of analysis we have reviewed. Argentines paid a high price for following the consensus of U.S. and other economists: deepening depression; unemployment among nearly a quarter of its working population; and poverty among more than half its people. Let us hope that the next time scores of economists offer advice to a country in trouble, they will first seek a solid body of facts instead of basing their analysis mainly on unfounded assumptions.

²⁰ Zarazaga (2003) also explores flaws of the consensus view.

[Appendix 1:
Link to full references.](#)

[Appendix 2:
Link to detailed quotations.](#)

[Appendix 3:
Link to spreadsheet of central bank balance sheet data.](#)

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Brackets around an economist's name indicate that a reference is about him or is an interview with him, rather than being written by him. The references here are keyed to Appendix 1, so a reference such as DeLong (2002c) means that DeLong (2002b), which is missing here, is there. Online links in this article were current as of May 6, 2005.

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DO ECONOMISTS REACH A CONCLUSION?

Do Economists Reach a Conclusion on Free-Banking Episodes?

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[Abstract, Keywords, JEL Codes](#)

WHAT DO WE MEAN BY FREE BANKING

IN THIS PAPER WE SURVEY THE LITERATURE ON HISTORICAL episodes of “free banking”—more accurately lightly regulated banking—and distinguish areas in which a consensus has been reached from areas in which more research needs to be done. It is important to recognize at the outset that the term “free banking” as we use it is an historical term. It was applied in the nineteenth century to banking systems that in fact were regulated along many dimensions, and were very far removed from true laissez-faire banking systems. To some extent, as we will show below, it was a fair term to use in the sense that the systems that were referred to as “free banking” were subject to fewer regulations than the systems they replaced. But the term “free banking” was also used at times, we suspect, because the word free, especially

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in the early part of the nineteenth century, created a positive aura—the banking version of labeling a frozen island Greenland.

There are numerous dimensions on which banking freedom can be measured. Any particular historical example might be freer than comparable systems on some dimensions and less free on others. The significance of particular cases, moreover, cannot be understood, therefore, without understanding their institutional and historical background. For these reasons we will not attempt to summarize all of the cases that have been studied and perform a meta-analysis. Instead, we will focus on six cases: the three best known, Scotland, the United States, and Canada, two additional cases that allow us to look at interesting institutional differences, Sweden and Switzerland, and one country on the periphery, Chile. We will then attempt to formulate some generalizations based on those case studies, generalizations that seem to us to be consistent with the remainder of the literature.

In principle the idea of free banking or *laissez-faire* banking is very simple. A true *laissez-faire* banking system would be one in which individuals were free to make any arrangements they wanted with respect to means of payment, media of exchange, and borrowing and lending. Any limitations on voluntary arrangements imposed by government would be a departure from *laissez faire*.

Of course, there is much more to be said. Two discussions, one by Friedrich Hayek and one by Adam Smith, are particularly important. The Hayekian concept of free banking emphasizes the role that competition could play in controlling the supply of high-powered money and thus inflation, while the Smithian concept, which is the basis of the historical examples we discuss, refers to free banking that exists within a monetary system in which government plays an important role. Friedrich Hayek laid out his concept of free banking in *Denationalization of Money*, first published in 1976. As one might guess from the date, the dominant economic issue at the time was inflation, and as one might guess from the author, the proposed solution was private competition.

Hayek imagined a system in which governments steered completely clear of money. The government would not define the basic legal tender or even the basic unit of account. Money would be completely “denationalized.” Hayek imagined private firms issuing competing monies in units of their own choosing: Mengers, Ducats, Florins, Talents, and so on (1978, 53). In principle, these issuers could (and Hayek thought would) issue pure fiat monies—paper monies unbacked by any promise to pay in gold, silver, or other commodities. What would prevent them from overissuing, that is from simply adding zeros and collecting the seignorage? The reputation of

the issuer would be enough, Hayek argued, to prevent overissue. If the government overissues a monopoly legal tender, the usual cause of inflation, there is little that the ordinary person can do. But in Hayek's world a brand of money that was depreciating in terms of commodities would be abandoned in favor of another brand that was not depreciating. Private competition, in other words, would solve the problem of inflation.

Hayek's vision has produced a heated debate. If there were several competing monies, would transaction costs be high? Would high transactions costs lead to the adoption of one currency? In other words, is the unit of account a natural monopoly? If one firm emerged with a monopoly would it be tempted to overissue just as a government monopoly issuer would? This concern has been raised by a number of writers including Milton Friedman (1959, 6-7), Eugene Fama (1983, 13), Lawrence Summers (1983), George Selgin and Lawrence H. White (1994), and Antoine Martin and Stacey L. Schreft (2003).

Is there historical evidence that could be brought to bear on Hayek's vision? Undoubtedly there is, albeit indirect evidence. Note that while Mengers are a purely imaginary unit of account, the Ducats, Florins, and Talents that Hayek mentions were the names of real coins. And, although they did not possess all the characteristics that Hayek wanted, they did circulate widely in a world of competing monetary units. Indeed, the idea that monies issued by one state should not be allowed to circulate within the borders of other states belongs to the late nineteenth and twentieth centuries, a newcomer. As late as 1857 Mexican Pesos, and a number of other foreign coins, were legal tender in the United States.

Nevertheless, while considerable historical research has been done on this type of monetary competition, most of the studies that have explicitly used the term free banking have fallen within what we call the Smithian concept of free banking, after Adam Smith, and these are the cases we will examine here.

The Smithian concept of free banking starts a long way from *laissez faire*.¹ It assumes a standard form of "high-powered money" that defines the monetary unit, usually a gold or silver coin, or both (bimetallism). Banks are then legally bound (whether by law or the terms of their charters) to convert their notes on demand into standard money. This was the accepted

1. When it came to banking Adam Smith's departures from *laissez faire* were substantial, as emphasized by Edwin G. West (1997). Nevertheless, it makes sense to refer to "Smithian free banking" because the regulations Smith favored were common to the historical episodes that have gone under the term "free banking."

monetary framework of the nineteenth and early twentieth centuries, and was generally presupposed by advocates and opponents of “free banking.” As usual, Adam Smith gave the clearest statement of this version of free banking.

If bankers are restrained from issuing any circulating bank notes, or notes payable to the bearer, for less than a certain sum; and if they are subjected to the obligation of an immediate and unconditional payment of such bank notes as soon as presented, their trade may, with safety to the publick, be rendered in all other respects perfectly free. (Smith 1979 [1776], 329)

As this quotation also makes clear, Smith favored two additional restrictions on note issue: a ban on delayed redemption (some Scottish banks had issued notes containing “option clauses,” discussed below), and a second, sometimes neglected restriction, a limitation on the size of notes that could be issued. Smith favored this second restriction because he believed that allowing the issue of small denomination notes would encourage “beggarly bankers,” whose failure would bear heavily on the poor. Smith also believed that it was important to keep some coin in circulation to prevent the complete breakdown of the monetary system if the home offices of the banks were forced to close because of war (a reference to the ‘45?).²

These restrictions on *laissez faire*, as important as they were, left plenty of room for freedom or additional regulation. An idea of the many legal restrictions that were sometimes imposed on “free banks” during the free-banking era can be obtained by looking at the checklist in table 1. We need to consider these possibilities in detail.

2. The evolution of denomination restrictions in the United States has been explored by Eugene White (1995) and Howard Bodenhorn (1993). Smith favored a minimum note of £5, a substantial sum.

Table 1. A Checklist of Banking Restrictions

<p>I. Freedom to issue bank notes</p> <ul style="list-style-type: none"> A. Were banks allowed to issue bank notes (paper money) or only deposits? B. Was it required that notes and deposits be redeemable in high-powered money? C. Was it required that redemption of notes and deposits be instantaneous, or could a bank delay redemption of one or both? D. Were there restrictions on the denomination of notes? For example, were small notes prohibited?
<p>II. Freedom to lend</p> <ul style="list-style-type: none"> A. Did notes have to be backed by government bonds? B. Were banks required to hold a minimum reserve of high-powered money? C. Could banks invest in long-term real assets such as real estate or corporate stocks? Were banks limited to short-term nominal debts secured by real assets (the real bills doctrine)? D. Was bank lending subject to usury laws? E. Were banks required to make their balance sheets public?
<p>III. Freedom of entry</p> <ul style="list-style-type: none"> A. Could potential bankers start a bank at the time or place of their choosing by following a standard procedure, or did bank charters require legislative action? B. Could banks open branches? C. Could a potential banker choose limited liability?
<p>IV. Freedom from regulation by (or help from) a central bank</p> <ul style="list-style-type: none"> A. Was there a government owned or controlled central bank that regulated the banks or acted as lender of last resort? B. If there was no government owned or controlled central bank, was there a privileged private bank that played a similar role?

Freedom to Issue Notes

For the average person, indeed for many economists, privately issued bank notes are the strangest feature of free banking. Although some private banks still issue notes—in Scotland, Northern Ireland, and Hong Kong—many people have never seen a privately issued note.³ Unlike debates over the private provision of pensions, health care, or electricity, the debate over the private provision of paper money concerns an alternative that most people have never experienced. Americans have not had privately issued notes in their pockets since the Great Depression.

Smith wanted notes to be redeemable on demand, but this is not the only possibility. Some Scottish notes, prior to legislation in 1765, contained an “option clause” under which a bank that issued a note could delay redeeming it for a period of time provided that the bank paid interest on the note until it was redeemed.⁴ Deposits also may be demand deposits (redemption is instantaneous) or time deposits (redemption may be subject to possible delays).

The convertibility of the free-banking note into specie (an old term for gold or silver coins) meant that the rate of growth of the stock of money over the long run, and hence the rate of inflation, was governed by the rate of growth of the stock of specie. There was, to put it somewhat differently, a golden or bimetallic anchor to the monetary system that limited the potential for inflation. A process of adverse clearings, the so-called “law of reflux,” limited the ability of individual banks to overissue. If one bank separately attempted to place more notes into circulation than were needed in its local area, it would soon be punished by a reflux of notes coming back for redemption. In the *Wealth of Nations* Adam Smith gives a good account of the law.

The late multiplication of banking companies in both parts of the United Kingdom, an event by which many people have been much alarmed, instead of diminishing, increases the security of the public. It obliges all of them to be more

3. Traveler's checks or bank checks are the closest analogs for Americans.

4. The option clause has been discussed as a possible mechanism for reducing the potential for bank runs. See Gherity (1995) and Selgin and White (1997). The same problem exists with respect to deposits: requiring instant redemption on demand encourages bank runs during periods of distress. It was common in the United States for certain classes of deposits to allow banks to delay redemption.

circumspect in their conduct, and, by not extending their currency beyond its due proportion to their cash, to guard themselves against those malicious runs which the rivalry of so many competitors is always ready to bring upon them. It restrains the circulation of each particular company within a narrower circle, and reduces their circulating notes to a smaller number. By dividing the whole circulation into a greater number of parts, the failure of any one company, an accident which, in the course of things, must sometimes happen, becomes of less consequence to the public. This free competition, too, obliges all bankers to be more liberal in their dealings with their customers, lest their rivals should carry them away. In general, if any branch of trade, or any division of labour, be advantageous to the public, the freer and more general the competition, it will always be the more so. (Smith 1979 [1776], 329)

An alternative way to make the same point is to note that convertibility prevents solvent bank notes from being traded far below their par value. Notes from a bank that overissues (and thus has too few specie reserves as compared with its notes) would be priced below par and redeemed. To be sure, there is a cost to converting notes into specie. So—as in the antebellum United States—notes could sell at a small discount, one that would grow as the note migrated farther from the point of redemption. But these discounts were similar to the small fluctuations in exchange that could occur under the gold standard between the “gold points.”

Hand to hand currency was, of course, more important in the monetary systems of the nineteenth century than it is today, although it occasionally plays a significant role, especially in less developed countries. In the United States in 1850, for example, bank notes made up about 35 percent of the total M2 money supply (Friedman and Schwartz (1970, 322-23). Today currency makes up nearly 50 percent of the M1 money stock in the United States, but only about 7 percent of the M3 measure.⁵

5. The current figures are for January 2004 and are from the Federal Reserve website, www.federalreserve.gov, accessed April 5, 2005. Currency includes coins and travelers checks. A considerable amount of U.S. currency is used in black markets and outside the United States.

A demand deposit is like a bank note in that it is a liability on the bank that issued it, but a demand deposit differs from a note in two important ways. With notes there is at least the hope for the issuing bank that the note will pass from hand to hand without creating a demand for the bank's reserves. When checks are deposited and cleared the issuing bank will lose reserves. Even if the system as a whole expands, reserves will be lost to cash. The bank that increases its issue of deposits by making additional loans, in other words, must expect to engage the interbank clearing system. There is also a legal difference between notes and deposits. When a shoe seller accepts a bank note the seller's basic contract, as a practical matter, becomes a contract with the bank. If the bank issuing the note fails, the shoe buyer who paid with the bank note is off the hook. On the other hand, when someone accepts a check, the basic contract remains a contract between the seller and the writer of the check. Even if the bank in which the deposit was lodged fails, the shoe seller can go back to the shoe buyer for payment.

Freedom to Lend

One would think that a minimum condition for free banking would be the freedom of a bank to lend the funds as it saw fit. Many "free-banking" systems, however have been subject to restrictions of one sort or another. The American free banks were required to back their notes with government bonds (usually bonds issued by the state where the bank was located).

Funds derived from deposits or capital could be invested more freely. However, two other restrictions favored by Smith, real bills and usury laws, applied to these investments. The "real bills doctrine" held that the best asset for banks to hold, given their demand liabilities, would be short-term business loans secured by real assets, for example, a loan to a bakery to purchase flour (Smith 1979 [1776], 304). For the most part, real bills was regarded as a conservative norm for bankers to adopt if they were wise. This may well be the way Smith regarded the doctrine. But it could be written into law, for example, by restricting the freedom of banks to own land, a frequent restriction in bank charters and free-banking laws in the United States.

Bank lending, moreover, was frequently restricted by usury laws. Adam Smith favored usury laws, arguing that the laws channeled funds into safer investments, which he thought was good for long-run economic growth (Smith 1979 [1776], 356-57). Jeremy Bentham famously took Smith to task

for this obvious departure from *laissez faire*. A movement to repeal the usury laws paralleled the movement to establish free banking (Rockoff 2003).

Freedom to Enter the Business of Banking

Pure freedom would mean one could start a bank at the time and place of one's choosing. Indeed, in the American case the term "free banking" referred mainly to this right. Before free banking the right to charter new banks was the privilege, sometimes jealously guarded, of state legislatures. But the general right to enter the field could be restricted in various ways. Under some laws would-be bankers were required, for example, to provide a minimum amount of capital, or to limit in various ways their note issues or lending.

One restriction that was important in the United States, although not in other countries, was the limitation on the right to establish branches. Although free-banking laws in the United States allowed entrepreneurs to establish a bank with one office within a particular state, branching was often prohibited, and branching across state lines was ruled out because the chartering of banks was state law and states prohibited banks with charters from "foreign" states from establishing branches. As we will see below, there is a strong consensus that prohibitions on branch banking caused a great deal of trouble in the United States.

Across free-banking systems, the obtainable corporate charter varied. Perhaps the most important issue was whether the organizers of a bank could limit the liability of shareholders. A recent contribution by C.R. Hickson and J.D. Turner (2004) argues that unlimited liability was often a key characteristic of successful free-banking systems. Double liability was a compromise between limited liability and unlimited liability: if the bank failed the shareholders not only lost their initial investment, but they were also liable again for an amount equal to the nominal value of their shares.

Freedom from Regulation by (or Help from) Central Banks

Some free-banking systems have been subject to more or less control by a government owned central bank or privileged private bank. At the time Adam Smith outlined his system of banking the Bank of England had important legal privileges not available to other English banks. In Scotland, only three banks (the Bank of Scotland, the Royal Bank of Scotland, and

the British Linen Company) were allowed the advantage of limited liability. The government could bestow a number of other privileges that would allow a private bank to achieve a leading position in terms of size and strength within the banking system. Perhaps the most important privileges were those of holding the government's deposits without paying competitive interest on them and managing the government's debt. Even the mere appearance of a special relationship with the government might reassure depositors and create a competitive advantage. With privileges came obligations: governments might call on the privileged bank to monetize government debt.

A privileged private bank or central bank could influence its smaller rivals in a number of ways. In normal times it could control individual banks by collecting and returning notes for redemption. In times of financial distress it could lend reserves to faltering rivals, in other words it could act as a lender of last resort. Walter Bagehot's classic, *Lombard Street* (1873), was essentially a demand that the Bank of England, then a privileged private bank, acknowledge its responsibility to act as lender of last resort.⁶ Why would a private bank take on these responsibilities? One motive might be self-interest: when aggressively redeeming notes a privileged private bank might be hoping to increase its share of the bank note market; when acting as lender of last resort a privileged bank might be hoping to profit by making high interest loans to troubled rivals. The motives, however, might be more complicated. A privileged bank might assist other faltering institutions to maintain the system within which it enjoys privileges, to provide stability that it conceived would rebound to its own advantage, or from a genuine sense of responsibility to the financial community.

Some economists, for example Milton Friedman and Anna J. Schwartz (1986), Hugh Rockoff (1986a), and Charles Goodhart (1988) argue on the basis of historical evidence that a lender of last resort is necessary to assure stability in a Smithian system of banking. Smithian banking systems will be fractional reserve systems, and there will always be some possibility of a panic. Other economists, such as Vera Smith (1936) and Richard Timberlake (1993), argue that there is no need for a privileged

6. Bagehot described a system of Smithian free banking, including the absence of a privileged private bank operating as a lender of last resort, as an ideal system. However, he argued that since the English system had evolved over a long period with the Bank of England playing a central role, it was impractical to consider a change of regime. The practical course was to make the existing system work better.

bank or state run central bank to regulate the system of note issuing banks or to serve as lender of last resort.

And economic historians can point to cases, such as Canada, the Suffolk system, or the New York Clearing House Association in the United States⁷ that did well with self-regulated systems instead of a formal central bank.

HOW DO YOU EVALUATE CASES OF FREE BANKING?

It is obvious, given the many dimensions along which banking freedom can range, that evaluating free banking will be difficult. One would like to arrange cases of free banking on a continuum ranging from total freedom to total regulation. Any such arrangement, however, will be somewhat arbitrary. In the American case banks were highly restricted, for example, in terms of the assets they could hold, but they were free to issue shares with limited liability, and were free of any oversight by a central bank. In the Scottish case, banks enjoyed far more freedom in terms of the assets they could hold, but their liability was unlimited (except for the older chartered banks) and they operated in an environment in which, arguably, there were heavyweight banks—the Bank of Scotland, the Royal Bank of Scotland, and the Bank of England—standing in the background. Comparing these cases inevitably requires judgments about how important various restrictions were in practice.

Nor is it easy to decide whether a banking system, however free it may have been, performed well or badly. Perhaps the greatest fear about free banking is that if banks are free to issue paper money they will be tempted to put too much in circulation. Partly, this fear is based on a thought experiment. What if a conservatively managed bank changed course and put a large amount of paper into circulation? Would not the notes, at least for a time, simply pass from hand to hand? What if all banks became reckless at one time, would not this produce an inflationary issue of paper? Free banking, to put it more dramatically, will end in an orgy of inflation and wildcat banking.⁸ This

7. See, Timberlake (1984), Gorton (1985), Gorton and Moullineaux (1987), Calomiris and Kahn (1996)

8. For our purposes it seems sufficient to define a wildcat bank loosely as a bank that appears to be a risky venture set up with the intention of profiting in the short-run from the wide circulation of its notes. Of course, what appears to be a risky or even foolish venture to one observer may appear to be the soul of propriety to another. In earlier work one of us

scenario is something that investigators must check for in every historical case. However, when we go beyond this relatively clear criterion we find that there is a wide variety of criteria for evaluating banking systems.

On the microeconomic side we can ask the set of questions we would ask about any financial system. How profitable was the banking system? Did it provide good service to its customers? Were bank offices conveniently located? Were loans available to worthy borrowers at low cost? More generally, we can ask whether free banking contributed to economic growth by channeling funds to sound investments. Another issue is corruption. Does free banking encourage corruption by providing an opening for shady characters to fleece the public, or does it discourage corruption by removing an incentive for people to bribe the legislators and regulators for privileges? Does free banking generally promote efficiency in the banking system, including efficient levels of assurance?

On the macroeconomic side there are also a number of questions. To be sure, under the Smithian system convertibility assured reasonable price stability in the long run. An increase in the supply of precious metals, as after the discovery of gold in California in the middle of the nineteenth century or in South Africa at the end of the century, could produce a mild inflation. Only fiat paper money regimes, however, could produce high inflations or hyperinflations (Rolnick and Weber 1997).

On the other hand, Smithian banking systems could fall prey to banking panics. They were, after all, fractional reserve systems. Free-banking systems may have been more prone to banking panics (as the fear of a wildcat banking suggests) or less prone, but we cannot rule out a priori the possibility that the public would lose confidence in the system, and that there would be a scramble for high-powered money. Whether free-banking systems are more prone to banking panics is a subject where no consensus has been reached among scholars. Consider the case of Australia, a case that, according to Kevin Dowd (1992) was a unique experience of a major banking collapse (1893) within a free-banking system⁹. Rather than being a consequence of a lightly regulated banking system, Dowd argues that this crisis was the result of a real supply shock and Government intervention. A quite different explanation is proposed in two recent papers by Hickson and Turner (2002, 2004). They argue that the crisis could have been avoided if the banks had been more

attempted to provide a more precise definition, and the result was to rule out some cases that contemporary observers classified as wildcat banking.

9. Dowd (1992) compares the scope of this crisis to the one occurred during the 1930s in the United States.

regulated. The lack of regulation resulted in low capital and cash and banks over-holding risky assets.

THE HISTORICAL EXPERIENCE

Given the plethora of systems that have been referred to as “free banking,” and the plethora of criteria that can be used to judge the performance of a banking system, it is not easy to come to a summary judgment about the lessons of history. Nevertheless, we believe that we can identify some issues on which economists have reached a consensus, along with, of course, many issues on which they have not. This means that we will be following the crowd: focusing on the restrictions that economic historians have stressed, and adopting their implicit weighting of various criteria for success.

We do not attempt an exhaustive survey. Such a survey would require at least one substantial volume, perhaps several. Many of the less well-known cases, moreover, have attracted relatively few scholars, so conclusions have not been tested. Besides, conclusions reached in recently documented relevant experiences like the case of Australia are still highly controversial and probably require further research. While scholars agree that this was a true case of free banking, they differ in evaluating how well the system worked, especially regarding its stability. Historical cases of free banking, moreover, tend to attract students with strong ideological priors. It is probably true that free banking has attracted more scholars predisposed to free markets than to regulation. In part, this may reflect the interest of Hayek and other leading free market scholars in free banking. The attraction of this issue may also reflect the relative success of a number of free-banking systems. Advocates of free markets, like advocates of regulation, are drawn to cases that appear to confirm their priors.

These considerations, in other words, suggest to us that a meta-analysis that attempted to find a consensus by treating all of the existing studies within a common framework would not be convincing. Here we discuss six cases in detail: Scotland, the United States, Canada, Sweden, Switzerland, and Chile.¹⁰

10. Among the episodes that we omit four of the most intriguing and deserving of further research were in Australia, China, Colombia, and France. Kurt Schuler (1992a) in the overview essay for Kevin Dowd (1992) surveys 60 historical cases. This volume includes

These are all examples of systems that included the basic Smithian restrictions: private notes and deposits were convertible on demand into high-powered money coined or at least defined by the government. They are all drawn from the nineteenth century. This is, of course, not an historical accident: the prestige of the gold standard and *laissez faire* were at their peak. Although limited, we believe that our survey covers the main cases in the sense of the ones that have drawn the most attention from economic historians and that span the full range of institutional structures that have gone under the name free banking

Free Banking in Scotland

The story of banking in Scotland begins in 1695 with the Scottish Parliament's authorization of the Bank of Scotland. A second bank, the Royal Bank of Scotland, was chartered in 1727 (by the British Parliament), and a third, the British Linen Bank, originally intended to finance the linen trade, in 1746. All three were limited liability banks located in Edinburgh. The demand for banking operations in other cities, Glasgow in particular, led to the establishment of note-issuing banks organized as partnerships or, after 1810, as joint stock companies, with unlimited liability. This sector of the banking industry, as well as the older limited liability sector, expanded rapidly, and by the beginning of the nineteenth century Scotland was served by a dense, vigorously competitive network of banks and bank branches.

The limited liability banks were bigger than the other banks and remained, as shown in Table 2, a major force in the banking industry during the industrialization of Scotland.

The liability rules were altered several times. But the key legislation, passed in 1879, permitted banks with unlimited liability to choose limited liability, a step that most of them took in 1882.

In 1802 the three limited liability banks issued 56.2 percent of all notes and 41.5 percent of all deposits. Legislation in 1845 froze the note issue of all banks except for increases backed 100 percent by specie, thus locking in a major role for the limited liability banks. In 1850 the limited liability banks were issuing 31.8 percent of all notes and 32.5 percent of all deposits. Notes

essays on the four episodes mentioned above -- Lawrence White on Australia, George Selgin on Foochow, Adolfo Meisel on Colombia, and Philippe Nataf on France -- as well as several others. Some of these episodes, of course, have been studied by other scholars. Eugene White (1994), for example, has explored free banking in France during the Revolution.

by that time, however, had become a relatively small part of the total Scottish money supply, as shown in the last column of Table 2.

Table 2 The Scottish Banks

	Banks with Limited Liability	Banks with Unlimited Liability	Notes of Banks with Limited Liability as a share of Total Notes	Deposits of Banks with Limited Liability as a share of Total Deposits	Notes as a share of the sum of Notes and Deposits
	number	number	percent	percent	Percent
1772	3	28	16.1	28.8	42.9
1802	3	28	56.2	41.5	33.7
1825	3	33	33.0	39.9	18.3
1850	3	14	31.8	32.5	8.4

Source: Checkland 1975, 237, 240, 424, 426.

The Scottish banking system has had many admirers. Adam Smith advocated several restrictions on banking based on the Scottish experience, but he concluded:

That the trade and industry of Scotland ... have increased very considerably during this period [the decades preceding the *Wealth of Nations*], and that the banks have contributed a good deal to this increase, cannot be doubted. (Smith 1979 [1776], 297)

Rondo Cameron (1967) deserves much of the credit for the high reputation that the Scottish system enjoys among the current generation economic historians. Cameron noted that Scottish economic growth compared favorably with growth in England and France, two countries with far better endowments of natural resources. Cameron then argued that Scotland's banking and educational systems were the only plausible explanations for Scotland's comparative success. Cameron concluded his

survey of Scottish banking by quoting A. W. Kerr (1884), an early historian of Scottish banking:

In Scotland, banking was permitted to develop as the country advanced in wealth and in intelligence. Nay, it was even enabled to lead the nation on the path of prosperity, and to evolve, from practical experience, a natural and healthy system of banking, which would have been impossible under close state control similar to that followed in other countries. (Kerr quoted in Cameron 1967, 99)

The major controversy over the Scottish system is not over the quality of banking services provided by Scotland's banks, or even their contribution to the economic development of Scotland, as most students of Scottish banking are positive on both issues, but rather over how free the system was. In addition to the limitations on small notes and option clauses mentioned above, two further limitations, restrictions on the availability of unlimited liability, and "central banking" by the large Scottish banks or the Bank of England, have been discussed at length.

During the phase of free banking in Scotland, bankers were free to organize banks, but their partners or shareholders bore unlimited liability for the debts of the banks. Jack Carr and Frank Mathewson (1988) and Carr, Sherry Glied, and Mathewson (1989) argued that unlimited liability acted as a barrier to entry and was a significant restraint on the growth of banking. Moreover, it is possible that by increasing confidence in notes and deposits unlimited liability increased confidence in the security of bank liabilities, much like bond collateral requirements or deposit insurance did in the United States. We know from S. G. Checkland's magisterial history of Scottish banking (1968, 149-150, and 1975, 275, 288-292, 440-442), that the Public banks (as the three limited-liability banks were known) were opposed to the extension of the privilege of limited liability to the remaining banks, indicating that the Public banks benefited from their special status. Lawrence H. White (1990), however, has argued forcefully, and in detail, that this constraint was not binding. Nevertheless, the jury is still out on whether this constraint might have created positive externalities for the system.

In addition, there is the possibility that the two largest Scottish banks, the Bank of Scotland and the Royal Bank of Scotland, acted as "central banks" for the Scottish system, or that the Bank of England did so at one remove. This contention is hard to evaluate in part because the idea of central banking was evolving toward its modern form in the course of the

nineteenth century. Bagehot's *Lombard Street*, which demanded that the Bank of England acknowledge its duty to be the lender of last resort, one of the defining functions of a central bank, appeared in 1873.

Certainly the Public banks were given privileges often associated with central banks. The Public banks managed the government's funds. Moreover, only their notes were received by the Customs and Tax Office (Checkland 1975, 186, 204; Munn 1981, 12; and Cowen and Kroszner 1989, 226). And the pattern of reserves pointed to a central role for the Public banks. Checkland (1975, 186) notes that "it became the custom of other banks . . . to hold part of their reserves in the notes of the public banks, rather than hold cumbersome gold," a point on which Frank W. Fetter (1965, 34) concurred.

One can find stories in Checkland (1975) that sound like a central bank looking after its flock. For example, Checkland (1975, 175) tells us that the Bank of Scotland acted as a "note issue policeman" by collecting and presenting for redemption the notes of banks that it thought were issuing excessive amounts. In 1762 the Bank of Scotland and the Royal Bank restricted credit during a balance of payments crisis (Checkland 1975, 108-111; Hamilton 1953).

The notion that the Bank of England at one remove provided liquidity also has strong supporters. One can begin with Adam Smith (1979 [1776], 304) who when describing the situation in the 1760s noted that "Whatever coin therefore was wanted to support this excessive circulation both of Scotch and English paper money, whatever vacancies this excessive circulation occasioned in the necessary coin of the kingdom, the Bank of England was obliged to supply them." And Checkland concluded that by 1810 "the automatic principle of dispersed banks, each with an immediate liquid reserve in its coffers, was now highly unrealistic. The principal and ultimate source of liquidity lay in London, and, in particular, in the Bank of England" (1975, 432).

One can also find stories, as pointed out by Tyler Cowen and Randall Kroszner (1989, 228) and Dow (1996, 704-705), that sound like lender-of-last-resort operations by the Bank of England—occasions when the large Scottish banks received credits from the Bank of England during times of stress. Whether these stories add up to "central banking," as argued by Cowen and Kroszner and by Dow or merely to actions taken by bankers for ordinary business motives, as argued by Lawrence H. White (1990, 532-534), is not easy to decide. Reading the minds of nineteenth century bankers is no easy task.

It appears, to sum up, that there is considerable agreement that lightly regulated banking was a success in Scotland. Disagreement remains, however,

over whether some residual constraints—the classical Smithian restrictions, unlimited liability, or the presence of large privileged banks acting as quasi-central banks—contributed in some measure to that success.

Free Banking in the United States

The free-banking era in the United States, the two and one half decades preceding the Civil War, followed the demise of the Second Bank of the United States. During all of this time; indeed up to 1913 there was no formal central bank or privileged private bank to regulate commercial banks or to serve as their lender of last resort.¹¹ The states regulated banking, and they tried a wide variety of systems ranging from state owned banks to absolute bans on banking. The best known of these experiments was the so-called “free banking law.” Under this law an individual could enter the business of banking, including the issue of notes, provided that those notes were backed by government bonds (usually in-state bonds, but sometimes Federal, municipal, or other-state bonds).

Freedom of entry was what gave the system its name. It was a sharp contrast with the older system in which banks were chartered one by one by the state legislature. Freedom of entry may have been especially important in an economy that was expanding rapidly into new areas that required new banking facilities. Entry, however, was not completely free. Typically the bank was restricted to a single office—branching was prohibited. And banks from other states were not allowed to set up branches.

American free banks were also free in the sense that they were limited liability corporations. Indeed, Richard Sylla (1985) argues that the American free-banking laws were the first example of laws that established explicit rules by which private firms could obtain charters to function as limited liability firms.

Banks deposited the bonds with a state banking authority. If the bank failed to redeem even one of its notes in legal-tender coins the note holder could take the note to the banking authority who would then sell the bonds and redeem all the notes issued by the bank. The law seemed to be a desirable

11. A possible exception was in New England. Here the Suffolk Bank of Boston, with the support of the other Boston banks, maintained the value of country bank notes at par. The Suffolk has been described as a quasi-central bank.

compromise: freedom of entry provided that the note holder was protected. The American experience has been a major hunting ground for scholars seeking lessons about free banking and has contributed in some measure to the interest in the field. Partly this is because of the colorful stories about the prevalence of “wildcat banking” on the American frontier. An earlier generation of economic histories of the United States delighted in recounting these stories. Broadus Mitchell and Louise Pearson Mitchell, for example, included a section on “wildcat banking” in their textbook on American economic history. Here they informed students that

The weakness, ignorant management, or dishonesty of large numbers of the state banks earned for their notes such opprobrious names as “shinplasters,” “wild cats,” “red dogs,” and “stump tails.” The banks issuing these were called “rag mills” and “fly by nights.” (Mitchell and Mitchell 1947, 390)

It is not always clear whether these stories were about free banks or banks created under some other law. However, Mitchell and Mitchell go on to report the complaint of the governor of Indiana about the behavior of some of the bankers operating under Indiana’s free-banking law.

The speculator comes to Indianapolis with a bundle of bank-notes in one hand and the stock [government bonds for backing the notes] in the other; in twenty-four hours he is on his way to some distant point of the Union to circulate what he denominates a legal currency, authorized by the legislature of Indiana. He has nominally located his bank in some remote part of the State, difficult of access where he knows no banking facilities are required, and intends that his notes shall go into the hands of persons who will have no means of demanding redemption. (Mitchell and Mitchell 1947, 392)

Many of the wildcat stories concerned free banking in Michigan.¹² Here one of the regulations was that banks hold a reserve of specie. Wildcatters tried to fool the Bank inspectors by displaying a reserve and then whisking it

12. See Rockoff (1986b) for a more detailed discussion of the Michigan experiment.

to another bank before the inspectors arrived. In describing their attempt to examine the banks, the inspectors rose to poetic heights.

The singular spectacle was presented of the officers of the state seeking for banks in situations the most inaccessible and remote from trade, and finding, at every step, an increase of labor by the discovery of new and unknown organizations. . . . Gold and silver flew about the country with the celerity of magic; its sound was heard in the depths of the forest: yet like the wind one knew not whence it came or whither it was going. (Quoted in Hammond 1948, 6)

The implicit conclusion of the earlier textbooks was that lightly regulated banking was a disaster. To be sure, better-informed writers distinguished between the failure of the free-banking law in the West and the success of the free-banking law in New York. Perhaps, they thought, the free-banking law could work in a more sophisticated financial environment. Nevertheless, the overall impression left by these stories was that the free-banking law, and by extension all forms of lightly regulated banking, was a disaster waiting to happen.

A long line of research beginning (one of us likes to think!) with Rockoff (1974, 1975) has reversed this impression. Rockoff investigated the cases of wildcat banking suggested in the earlier literature and concluded that they were quantitatively unimportant. Moreover, the bond-security provision, Rockoff concluded, caused most of the problems. Each free-banking law had to specify the amount of government bonds backing notes. Should it be one dollar in bonds for one dollar in notes? Or \$1.10? Or \$.90? And how were the value of the bonds to be measured? Should they be valued at market price? Or at face value? Too much security made it uneconomic to start banks under the law; too little security encouraged risky or unscrupulous banking practices by creating highly profitable opportunities for bankers while misleading the public about the security of the notes they were accepting. Rockoff followed a strategy of constructing an upper bound on the amount of bad banking that had occurred under the free-banking laws. Most subsequent writers have lowered that bound even further.

Arthur J. Rolnick and Warren E. Weber (1983) investigated free-banking laws in four states: New York, Wisconsin, Indiana, and Minnesota—in detail. They found some evidence of problems. For example, nine of the 16 banks set up under Minnesota's law of 1858 failed by 1862. Nevertheless, they concluded that losses on bank notes were relatively low under “free banking.”

and that generally Rockoff had, if anything, been unduly pessimistic. In a subsequent paper (1984) they addressed the question of why free banks in some of their states failed in large numbers. They concluded that falling bond prices produced many of these failures, and that wildcat banking was seldom the problem. Rockoff (1991) expressed some doubt that the two hypotheses were as distinct as Rolnick and Weber suggested. After all, opening and closing banks that were required to hold bonds influenced bond prices. Nevertheless, Rolnick and Weber's stress on other factors that could produce bank failures helped to further lower the upper bound on the amount of fraudulent or foolish banking that had been produced by the free-banking laws.

Andrew J. Economopoulos (1988) took up the case of Illinois, another apparently bad experience marked by the rapid creation of many banks followed by many failures and heavy losses. Economopoulos found some suspicious evidence. While Chicago, the commercial center with a population of about 100,000, had only nine banks, 10 free banks were set up in towns with populations under 200 (where in fact it was illegal to setup a bank). Altogether, 43 were set up in towns with populations under 1000 (Economopoulos 1988, 260-261). Nevertheless, Economopoulos concluded that true wildcat banking was a rare phenomenon, and that the evidence is consistent with the idea that most of the banks were sound business propositions caught by unfortunate movements in the prices of the bonds backing the notes.

Iftekhhar Hasan and Gerald P. Dwyer (1994) investigated episodes in New York, Indiana, and Wisconsin in which large number of free banks closed. For the most part, they ruled out wildcat banking (a la Rockoff) and falling bond prices (a la Rolnick and Weber), and concluded that to some extent the problems may have been the result of forces beyond their control. In Indiana, for example, the cascade of banks closings began, according to Hasan and Dwyer, when Ohio authorities decided to force notes from certain banks out of circulation.

Thus the upper bound on bad banking under the American version of free banking set by earlier writers has been progressively lowered. Indeed, it seems possible to us that the attempt to lower the upper bound on the amount of unscrupulous and foolish banking may have gone too far. The critics have succeeded in showing that Rockoff's simplest and most extreme scenario of wildcat banking was rare. That doesn't mean, however, that every remaining bank was a model of propriety. Even when bond prices exceeded the amount of notes that could be issued on their basis (so that bankers had to put some capital into their enterprises) high profits may have encouraged reckless banking. Indiana is an example. It is always

possible that the game could have gone on indefinitely with Ohio using notes redeemable in small towns in Indiana—a long ride away in horse-and-buggy days. However, most people will view this as a distortion from what they would regard as a desirable system for supplying bank notes in Ohio. They will sympathize with the distaste expressed by the governor of Indiana, quoted above, for the speculator who “comes to Indianapolis with a bundle of banknotes in one hand and the stock in the other” and who in 24 hours “is on his way to some distant point of the Union to circulate what he denominates a legal currency.”

James A Kahn (1985) and Kenneth Ng (1988) put the enthusiasm for the American version of free banking in perspective. Kahn showed that on the whole free-banking states suffered from more failures and more costly failures than non-free-banking states, and Ng (1988) showed that the amount of banking services and the number of banks remained the same, or declined after the introduction of free banking. Both results, however, might be traced to the peculiarities of the bond-security system, rather than free entry.

Most students of American free banking, moreover, going back at least to Bray Hammond (1936), have pronounced New York a successful case of “free banking.” Bank failures were rare, losses on bank notes rarer still, and New York emerged as the nation’s financial center, as pointed out, for example, by Robert King (1983). Kahn (1985) and Ng (1988), who were not enthusiastic about American free banking in general, also acknowledged the success of the New York System.

Even the adoption of a relatively successful form of free banking in New York, however, could not prevent a financial panic: the panic of 1857 hit New York hard. Charles Calomiris and Larry Schweikart (1991) investigated the response of different banking systems in the United States to the panic and found that some—those that allowed branch banking or interbank cooperation—survived the crisis in better shape. Nevertheless, the crisis suggests the potential for panic that probably exists in any system that combines fractional reserve banking with bank notes and deposits that are legally redeemable on demand.

Does the abandonment of the antebellum version of free banking in favor of the National Banking system, moreover, prove that contemporaries viewed free banking, whatever its modern apologists might claim, as a failure? One issue that may have loomed large to a contemporary that has not been dealt with in detail by modern students is the complexity of the system. Merchants had to consult “Bank Note Detectors” when presented with an unfamiliar note, or send customers to dealers in out-of-town notes. The detector showed the discounts that applied to notes that had wandered far

from home and described potential counterfeits. Granted, merchants today have similar worries; checks, for example, can bounce. And we have all paid a fee to get cash from an ATM machine, a fee that resembles the discount paid by a note holder who wanted to convert a “foreign note” into “current money.” Gary Gorton (1996) and Michael Hauptert (1996), moreover, showed that the discounts on out-of-state bank notes reflected the reputations of the banks that issued the notes. Still, using a single note acceptable everywhere was more convenient.

However, the view that the antebellum version of free banking was abandoned because it was perceived to be a failure is simplistic. The National Banking Act, adopted during the Civil War (1863, 1864), was in fact a free banking law. It permitted entrepreneurs to establish banks as long as explicit conditions were satisfied and provided for the issue of convertible bank notes backed by government bonds.¹³ Salmon Chase, the secretary of the Treasury who proposed the National Banking system was himself the former governor of a free-banking state, Ohio, and probably had Ohio’s relatively successful system in mind as he proposed the national system. The National Banking Act was a compromise between those who favored a continuation of the pre-Civil War state-based system and those who favored the complete abandonment of private banking in favor of the greenback, a irredeemable currency issued during the war. The greenback was popular, and provided a uniform currency, but legislators feared that a permanent fiat currency might be overissued. The National Banking Act also solved the problem of providing a currency for western states that had seen notes backed by southern bonds disappear from circulation. And perhaps most importantly for its passage, the act strengthened the market for government debt during the war.

The history of the National Banking system, then, can provide further evidence on how well the American version of free banking worked in practice. On the one hand, advocates of free banking have pointed to the rapid economic growth in the United States. It was during this era that the United States became the world’s leading industrial power. On the other hand, critics of free banking can stress that the system fell prey from time to time to panic: there were major banking panics in 1873, 1893, 1907, and of course, 1930-33. Defenders of free banking attribute those panics to the restrictions on National Bank note-issues and to the restrictions on branch banking, not to laissez-faire, noting that there were no panics in the

13. Howard Bodenhorn and Michael Hauptert (1995, 1996) compared note issue under the National Banking system and the free banking systems.

Canadian banking system, which was not limited in these ways. A full summary of the vast literature on this period, however, is beyond the scope of this paper.¹⁴

The states continued to charter banks during the National Banking era. These banks were effectively barred from issuing notes, but regulations on the deposit side were up to the states. John James (1976) shows that the convergence of regional interest rates at the turn of the nineteenth century, a sign of the integration of regional capital markets, was produced by increased competition among state banks that in turn was produced by the adoption of “free-banking” laws. Illinois adopted “free banking” in 1887, many newly formed western states adopted “free banking” in their state constitutions, and many southern states adopted general banking laws near the turn of the century (James 1976, 896).

All in all, several conclusions about the American experience seem fairly secure. First, the stories about wildcat banking that dominated early accounts, although not baseless, were exaggerated by an earlier generation of economic historians. Second, the difficulties that did emerge appear to have been the result of restrictions imposed on the American free banks—restrictions on branch banking and the peculiar bond security system—rather than the result of freedom of entry. Third, the American system provided several cases—including New York, the most important state system—where the American version of “free banking” worked well. Finally, free banking American style does not seem to have conferred immunity to financial panic.

Free Banking in Canada

Banking began in Canada with the founding of the Bank of Montreal in 1817. Modeled loosely on the Bank of the United States it received a government charter in 1824. Charters for new banks soon followed in other provinces. “By the middle of the 1830s,” according to Kurt Schuler (1992b, 80), “an unwritten rule had emerged in the provinces with banks that almost all parties able to raise a certain minimum of capital would be granted a charter.” In other words, Canada soon had de facto free banking. It is part of the conventional wisdom shared by banking and monetary historians that from the mid-1830s until the post world war II period, the Canadian system of

14. The literature on the performance of the banking system under the National Banking Act is summarized in Richard Timberlake's (1993) classic *Monetary Policy in the United States*. The contrast between the U.S. and Canada is the focus of Bordo, Redish, and Rockoff (1996).

free banking was stable and efficient, especially when contrasted with ongoing difficulties in the United States.

An important dimension of freedom in Canadian banking was the right of banks to issue notes based on the general assets of the banks. In 1850 legislation created the possibility of starting “free banks” that could issue notes collateralized by government bonds, similar to the popular American system. Only five such banks were set up, however, and by 1860 all had failed or converted into chartered banks. The 1850 law was repealed in 1866.

The notes of the Canadian banks had to be convertible into gold or “dominion notes.” The latter were gold convertible notes issued by the government, both in large denominations to serve as reserves and in small denominations to serve as “change-making notes.”¹⁵ In this respect the Canadian system reflected Smith’s idea that bank notes should not be issued in low denominations.

The flexibility of a system in which notes were issued based on general assets provided measurable benefits. Interest rates in Canada did not follow the distinct seasonal pattern, rising in the fall and winter and falling in the spring and summer, which prevailed in the United States. The reason, as Selgin and White (1994a) show, is that in Canada the banks could adapt to the heavy demand for notes during the crop-moving season by issuing more notes relative to deposits. In the United States this kind of adjustment was hampered by the bond collateral system, and the seasonal pattern of interest rates prevailed, a constant irritant to farmers who found that interest rates were highest when they ran short of funds. This pattern lasted until the establishment of the Federal Reserve System.

Another difference between Canada and the United States was the freedom of Canadian banks to establish branches, a freedom that was restricted in the United States. In the U.S. banks could not branch across states lines, and in some states, so called “unit-banking” states, banks were not allowed to establish branches even within the state that chartered the bank. As a result Canadian banks were larger than their American counterparts were, and the Canadian banks were better able to diversify the risks related to particular regions. The resulting difference in the stability of the two systems was dramatic. The sharpest contrast appears during the Great Depression. Both countries suffered similar declines in GDP. For Canada this was inevitable because the Canadian economy was thoroughly entangled with the

15. The phrase is from Walker (1894, 248). Walker and Holladay (1934) provide useful snapshots of the system.

economy of its much larger neighbor. However, while hundreds of banks failed in the United States, and while a series of state bank holidays in the United States led to a nationwide bank holiday, no banks failed in Canada. Bank branches were closed, but the banking system continued to function effectively. The contrast between the stability of the Canadian system and the instability of the U.S. system, however, was present long before the Great Depression. By 1900 American reformers were drawing attention to the difference in stability and calling for reforms of the American system to make it more like the Canadian system.

Canada's private system did so well that a central bank was not established until 1935. The reason for establishing the bank at the time was not a concern about the stability of the banking system, but the hope that by producing inflation the Bank could alleviate the depression (Bordo and Redish 1987). The Canadian system, like the Scottish system and parts of the American system, was clearly a successful case of lightly regulated banking. However, as in the Scottish case it can be argued that Canada did have large private banks that may have served as proto-central banks. Bordo and Redish (1987, 408) note that the Bank of Montreal "emerged very early as the government's bank performing many central bank functions." They suggest that it may have been the rivalry of the Bank of Montreal with other large banks, such as the Royal Bank, that prevented the Bank of Montreal from becoming a full-fledged central bank. Its rivals had enough political muscle to limit the privileges extended to the Bank of Montreal.

Free Banking in Sweden

First established during the 1830s, the Swedish private banks, the Enskilda banks, had the right to issue notes until the Banking Act of 1897 conferred a note issuance monopoly on the Bank of Sweden (Riksbank). The transition to a complete monopoly took place between 1901 and 1904.

This experience has received increasing attention from economists and economic historians. Lars Sandberg (1978), one of the first scholars in recent years to look at the relationship between finance and economic growth in Sweden, argued that Sweden was an "impoverished sophisticate," a country with a financial and educational system far in advance of its level of per capita income. Was this due to the private sector and the Enskilda banks? Charles Kindleberger (1982), going back to the famous Swedish economist Eli Hecksher, argued that before 1895 the history of the Swedish banking was largely limited to that of the Riksbank. But a number of

authors (Selgin 1988, Selgin and White 1987, Dowd 1996) have viewed the Swedish case as an example of a successful lightly regulated banking system. Lars Jonung (1989), in an influential paper, pointed out that the Enskilda notes competed effectively against the notes of the Riksbank, demonstrating the superiority of the private system. In a recent work, Anders Ögren (2003), although questioning the extent to which Sweden's system fits the free-banking model, has highlighted the importance of the Enskilda in the development of the credit market and the provision of the means of payment.

As in Canada, entry in Sweden was regulated: banks were chartered by the Swedish Parliament. In 1824 the monetary monopoly of the Riksbank was abolished, and the first private charter was granted in 1831. Until the 1860s the number of banks remained limited; there were only six chartered banks during the 1840s and eight during the 1850s. The Banking Act of 1864 simplified the procedure for obtaining a charter, and an automatic renewal of charters was allowed. As a result, the number of private note-issuing banks grew rapidly, approaching thirty by the end of that decade. The number then remained stable.

Perhaps the main restriction on the Enskilda banks was unlimited liability. On the other hand, there was no required minimum specie reserve until 1874. At that time, when Sweden switched from a silver standard to the gold standard, banks were required to hold 10 percent of their paid in capital in gold. However, the law stated that the bank note issue should be fully backed by the sum of securities held by the bank as part of its equity capital, specie and legal tender money, and the claims of the bank up to an amount not to exceed 50 percent of the bank's equity capital. During the first half of the nineteenth century lending rates were limited to six percent. This constraint was removed by the Banking Act of 1864. The Act also established an option clause, allowing banks to delay the conversion of its notes for 6 months subject to the payment of an annual interest rate of 6 percent *per annum*. And the law established the payment of a tax upon issuance ranging between 0.2 percent and 1 percent of total notes issued.

One of the most distinctive aspects of the Swedish experience is that note supply was provided not only by the Enskilda banks, but also by the Riksbank. Riksbank notes were legal tender and redeemable in specie. The Riksbank could issue notes up to its reserves (specie and foreign exchange) plus a fixed amount that varied between 35 and 45 million Crowns. (In practice, this amount was larger than the Riksbank reserves.) Until 1859 Riksbank notes represented more than half of total note supply; a number

that fell to 45 percent afterwards. After 1880 the Riksbank had a monopoly in the issue of five-crown notes.

With the move to the gold standard, a law passed in 1874 obliged the Enskilda banks to redeem their notes afterwards into specie only. But in practice, according to empirical evidence reported by Ögren (2003), Enskilda notes continued to be mainly redeemed into Riksbank notes. Indeed, about two-thirds of the Enskilda reserves were held in the form of Riksbank notes. Since Riksbank notes were receivable for paying taxes, they had a legal tender status. The latter meant that there was no difference if Enskilda banks held specie or Riksbank notes. It would appear, therefore, that the supply of Enskilda notes issuance was at least partially dependent on the supply of Riksbank notes, giving the Riksbank some control of the total money supply. The link between the State and the banking system was reinforced in 1869 when the Riksbank began to accept Enskilda banknotes at par.

By law the Riksbank was not supposed to give help to any private bank in case of financial difficulties. So a controversial point among Swedish scholars is related to the role as lender of last resort that the Riksbank supposedly played in practice. Empirical research provided by Ögren (2003) makes the case for this view. He shows that the Riksbank provided liquidity to the banking sector during at least two episodes of financial distress: 1857-1858 when it helped the Skåne Enskilda bank (which was the biggest Enskilda bank) and 1878-1879 when it supplied liquidity to the Stockholm Enskilda bank.

The Enskilda developed one of the essential elements that should emerge in any free-banking regime, namely an inter-bank clearing system. The practice of mutual acceptance of notes can be dated at least to the 1840s. During the 1850s, the Stockholm Enskilda bank formally took over this clearing function, but during the 1860s, the bank began to face increasing competition from a non-issuing bank (the Skandinaviska Kreditaktiebolaget). These two clearing institutions continued until 1897. The inter-bank clearing procedure was reinforced by the spontaneous development of an ingenious mechanism for the redemption of notes located in distant locations: the so-called "postal bank bill". A postal bill was purchased at the nearest bank and sent by ordinary mail. It was payable on demand at par, allowing its recipient to cash the bill at any conveniently located bank.

Although one could debate the relative contributions of the Riksbank and the Enskilda banks, it is clear that the combination of the two maintained convertibility and provided an efficient means of payment for

the Swedish economy. The Enskilda, moreover, benefited from their relative freedom in commercial activities, particularly after 1864 when usury laws were abolished and when bank entry restrictions were relaxed. Indeed, the Swedish credit market expanded strongly after the 1860s and private note issuance became secondary to deposits creation. According to Ögren (2003), one of the major achievements of private banks during this period was the development of liquid financial markets. The latter suggests that liberalism in the Swedish banking regulation has contributed to the expansion of local credit markets and, by this means, to economic development. Indeed, as argued by Lars Sandberg (1978), Swedish economic success before World War I, can be explained by the early development of a sophisticated banking system.

In 1897, as noted above, a law was passed that provided for the monopolization of the note issue by the Riksbank. This law also gave the Enskilda the right to discount at the Riksbank, and phased out private lending by the Riksbank, bringing the Riksbank into line with then current ideas about central banking. The transition period, during which the Enskilda notes were transferred to the Riksbank, occurred between 1901 and 1904.

In a series of papers Per Hortlund (2005a, 2005b, and 2005c) compared the system before monopolization with the system afterwards. Hortlund found that the money cycle decreased after the monopolization, while the credit cycle increased. He also argued that the Enskilda system had done a good job of meeting seasonal variations in the demand for notes, but found little difference between the free-banking system and the monopoly system. The violent disturbances to the world monetary system that occurred after 1904, however, make it hard to compare those two periods.

Free Banking in Switzerland

Swiss banking, of course, enjoys a worldwide reputation for quality and stability, not to mention discretion. It is therefore of some importance to our story that between 1826 and 1907, the Swiss free-banking era, the Swiss money supply was provided by private and cantonal banks. Although contemporaneous with the classical cases, the Swiss experience differs from them in several important dimensions. First, until 1848, Switzerland lacked a national currency and an official unit of account. Second, because of its political organization, the Swiss system was organized around both private

banks and cantonal banks depending on the local government. Finally, at least after 1881, the system was more restricted than others.

The beginning of the Swiss free-banking experience is linked to the liberal revolutions in the first half of the nineteenth century that removed the prevailing cantonal aristocratic governments. The liberal revolutions allowed several Swiss cantons to begin deregulating their banking systems by chartering new banks and, more importantly, allowing them to issue notes. After winning the civil war of 1848, the liberal forces were able to transform Switzerland from an association of independent States into a federal republic. The new government reformed the Swiss currency system, replacing the large number of cantonal monetary units with a single national currency, the Swiss Franc.

There were three types of note-issuing banks: private commercial banks, cantonal banks ruled by the cantonal governments, and local saving banks that had private and municipal ownership. Before the creation of the national currency, banks were free to choose both the unit of account of their notes and the outside money in which these notes could be redeemed. As a result, banknotes were denominated in the more accepted foreign currencies in Switzerland at this time; the French Franc, the Reichsgulden, and the Brabanterthaler. Thus, prior to the liberal revolution, Swiss banking was closer in some respects to Hayek's approach of free banking than to Smith's.

If foreign notes were accepted in Switzerland, what was the comparative advantage (or transaction costs reduction) of local banknotes labeled in the same currency unit they were supposed to replace? The truth is that until the establishment of the Swiss Franc, banknotes played a marginal role, thus suggesting that foreign notes were largely preferred to local banknotes.¹⁶ Indeed, according to Lars Jöhr (1915), only two private banks (Zurich and Saint Gallen), one cantonal bank (Bern), and a single deposit bank (Bern) were issuing notes during the 1840s; the total amount issued being below 2 million Swiss Francs. After the establishment of the Swiss Franc as the single unit of account, this began to change. By 1860 there were seven private banks, five cantonal banks, and five deposit banks circulating notes, amounting to nearly 15 million Swiss Francs. In 1880, the notes of the seven private banks, fourteen cantonal banks and fifteen saving banks amounted to nearly 100 million Swiss Francs.

16. See Weber (1992, 188-90, Table 10.1).

One important feature of the Swiss system was that cantonal banknotes were usually granted a series of privileges denied the notes of private banks. The cantonal governments guaranteed them, exempted them from taxes and fees that private banks were forced to pay, and accepted them at par for tax payments and other transactions with the cantonal government, although they were not a legal tender. Because of these advantages the cantonal banks were the main Swiss note suppliers, with a total market share ranging from 50 to 65 percent.

In 1881, a Federal banking law imposed several new restrictions. Note issuance was limited to cantonal and incorporated banks; the issue of notes by individual private bankers was forbidden. The banks were forced to back their notes by a mixture that was 40 percent specie and 60 percent authorized domestic and foreign government bonds. In addition, the paid-in capital had to account for at least one third of banknotes in circulation. Note issuance was charged with federal and cantonal taxes and fees. The federal government, moreover, was in charge of providing all banks with standardized notes ranging in face value from 50 to 1000 Francs. The notes differed only regarding the name and signatures of the issuer. In light of these restrictions, Weber (1992, 196) is probably right in arguing that “the free issue of paper money had ended in Switzerland by 1881.”

An important remaining provision of the Law was that banks were obliged to accept other banknotes at par. From a theoretical point of view, this restriction was important. To some extent, it implied fixing the exchange rate among the different banknotes. The latter means that note differentiation was no longer very important for banks when competing for gaining the public’s favor. The Swiss banking historian Jöhr (1915) noted that “the ordinary man, in the course of the years, ceased to differentiate between the notes of the various banks. If the notes carried the name and signatures of this or that bank, it was no longer taken into consideration.”¹⁷

Free-banking theory holds that note-brand discrimination is a crucial aspect of any competitive note issuance system (Selgin 1988). In fact, the *reflux* mechanism, which prevents the overissuance of notes, cannot operate properly if the public cannot distinguish among the notes of different banks. Thus, one might guess that overissuance could have existed after the Law of 1881. And in a recent paper Neldner (2003) presents evidence, based on foreign exchange data and the upper gold and silver points of the

17. Quoted in Neldner (1998, 291).

Swiss bimetallic monetary regime, that strongly supports the case for an overissuance of notes after the mid-1880s.

During the 1890s there arose clear signals pointing to a future nationalization of the Swiss note issue. Indeed, in 1891 a referendum authorized the federal government to establish a central bank. As a result, several important commercial banks began to abandon voluntarily the note-issuing business while maintaining their other banking activities. When the Swiss National Bank was established in 1907, the federal government became the sole issuer of Swiss bank notes. The creation of the Swiss central bank was supported unanimously by a commission of experts appointed by the Financial Department. As pointed out by Neldner (2003), it is interesting to note that the majority of the commission members were representatives of the issuing banks. In this case it is interesting to note that the final switch from a private to a centralized system of note issue does not appear to have resulted from an attempt by the government, or private interests allied with it, to appropriate the seignorage from issuing notes.

Because of the many particularities, the Swiss experience with private note issuing banks offers many interesting lessons. First, this experience suggests that the development of a flourishing system of note-issuing banks requires the existence of a single (national) unit of account. As we have seen, both the number of banks and money creation only expanded after the establishment of the Swiss Franc as national currency. Second, at least after the federal banking law of 1881, the Swiss experience seems to have been less free than other experiences in many important dimensions such as the existence of privileged cantonal banks and restrictive collateral requirements for private banks. Finally, this experience suggests that overissue can arise when the capacity of the public for differentiating notes is weakened by the law. The same kind of problem arose in the Chilean case, to which we now turn.

Free Banking in Chile

The classic examples of Free Banking, the United States, Scotland, and Canada, were all wealthy English-speaking countries. Sweden and Switzerland broaden the picture somewhat, but still leave us within the European orbit. Chile allows us to look at a less developed country.

In 1860, under the influence of French economist Courcelle-Seneuil,¹⁸ Chile approved a law allowing private banks to issue notes almost freely. What has been called the *Chilean free-banking era* ended in 1898 when the State monopolized the issue of notes. Besides formal freedom to issue notes, the Chilean case is a good example of how a private money regime can fulfill some dimensions of a free-banking system while not fulfilling others.

Among the historical experiences, there is probably no other country that adopted such a liberal banking law (even if the reality, as we will see, the system did not always function as intended)¹⁹. Besides a minimal capital provision, private banks were almost free to issue notes and to perform all remaining banking activities. The following excerpt from the message of President Montt when presenting the new banking law to the Congress in 1860 illustrates the intentions of the framers of the banking law:

[In the US free-banking system] the main part of banking provisions imposes some heavy restrictions on banks. They forbid bank branching, they limit the type of bank operations, they restrict the kind of documents that can be discounted by banks, they decide whether banks can or cannot pay interest on their deposits and they force banks to guarantee their notes by means of State bonds. The law I am submitting does not include any of these restrictions and therefore leaves us with a wide range for freedom. (Montt quoted in Ross 1886, 43; author's translation.)

The main features of the free-banking law of 1860 can be summarized as follows:

1. Notes had to be convertible into gold or silver on sight and on demand.
2. Anyone could establish a note-issuing bank.

18. He was hired in 1855 by the Chilean Government as Finance Minister Adviser and Professor of Political Economy at the Universidad de Chile.

19. Former deputy and banker Nicomedes Ossa asserted in 1886: "... it is an honor to have passed twenty five years ago one of the more free banking laws of the world" (Nicomedes 1886, 377; author's translation).

3. Banks were not required to hold any minimum percentage of specie as a backing.²⁰ Instead notes could not exceed 150 percent of the paid in capital.
4. Banks were required to present monthly balance sheets. This information was revised by the Ministry of Finance and was published every month in the newspapers. Loans to the banking administration had to be presented in a special account on the balance sheet.

Regarding notes, however, the Chilean system was not as free from state intervention as one might have thought. Despite free entry, the State favored certain banks. By financing State budget deficits, a small group of banks obtained the right for their notes to be received by the State at par. This guarantee gave the privileged banks an important competitive advantage over any new entrant. In fact, over 50 percent of all circulating notes were issued by the most privileged bank, the Bank of Chile. This bank was granted the monopoly of receiving all fiscal deposits as well as managing the State's international financial operations. In practice it practically was a State bank.

When the law of 1860 was launched it was hoped that a private system of note issue would remove the temptation for the government to monetize fiscal deficits. The links between banks and the State, however, undermined this advantage. In 1878, a big loan to the government was arranged and the amount of bank notes that could be received by the State at par implied a doubling of the circulation. This loan, of course, created strong incentives for the private banks to increase their issues and undermined convertibility. A deep financial crisis occurred in 1878, after which the government decreed the inconvertibility of bank notes and thus the depreciation of the local currency (Briones 2004).

Starting in 1879, the State began to issue its own inconvertible notes. These notes were legal tender and represented between 60 percent and 85 percent of total circulating notes. In principle, the convertibility of bank notes into gold was resumed in 1880. But when pressed the banks simply converted their notes into the government's depreciated legal tender paper. Consequently, returning to gold convertibility was impossible until the retirement of the State's notes.

20. For example, for the U.S. free banking experience, in Indiana and New York banks were required to hold 12.5 percent in specie reserves (Rolnick and Weber 1984, 269, table 1).

In 1895 Chile returned to the gold standard when the State began to convert its notes into gold at a rate of 18 pennyweights of gold per Chilean peso. The banking system was allowed to postpone conversion of its notes until 1897. The delay was not sufficient to avoid a heavy monetary contraction which pushed annual real interest rates up to 35 percent. Liquidity problems became unsustainable and, in 1898, a bank run occurred. As a result the State decreed a return to a paper money regime, and forbade private issuance of notes.

Evidently, the Chilean system, despite its trumpeted start deviated from some important principles of what we have called the “Smithian” version of free banking. As we have seen, before 1878, private banks provided the notes, but unavoidable political links and favoritism resulted in a strong group of privileged banks. After 1878, the note supply was not determined by banks but mainly by the State and the convertibility principle was abandoned during most of the time.

Is this evidence enough to reject the case of Chile as an example of a free-banking system? As soon as one takes into account freedom in the remaining banking intermediation activities, the answer is no. Free entry was granted, branching was not restricted, and no minimum capital was required when performing common banking activities. Furthermore, there were no restrictions on the sort of assets a bank could hold, no maximum interest rates were imposed, and no restrictions were placed on the volume and composition of loans and deposits.

The number of banks and branches increased consistently over time while the market share of deposits in privileged banks was declining. From 11 banks and 19 branches in the mid-1870s, the numbers increased to 25 and 45 in 1895. At the end of the 1880s, British and German banks began to enter into the local markets as well. Banking deposits rose by more than 300 percent between 1878 and 1898 (in real terms), representing nearly 15 percent of GDP. Besides, banking capitalization increased in real terms by more than 200 percent during the same period of time. The ratio of specie to deposits exhibited an average value of 15 percent during the gold standard period showing that, despite freedom, banks were in a relatively strong financial position.²¹

As in the US case, it was commonly believed that minimally regulated banking had produced many bank failures in Chile. The data, however, reveal that between 1880 and 1898, cumulated losses for depositors

21. This figure is high when compared to an average value of near 10 percent in the United States, Canada, United Kingdom, and France.

represented just 2.5 percent of total deposits. This figure is below the one registered in the well documented New York case, generally regarded as a successful “free-banking” experience (see King 1983).

Freedom also allowed private banks to perform an important role in developing a long-term credit market such as the mortgage market. Although at the end of the 1870s mortgage operations were performed mainly by a State institution, by the mid 1890s private banks had a market share of 60 percent. During the same period the market value of mortgage obligations in real terms rose by more than 350 percent and represented nearly 20 percent of GDP in 1898.

It is beyond the scope of this section to establish a definitive link between Chilean financial development and economic growth during the period. Nevertheless, one can reasonably imagine that the rapid financial and banking development should have had a positive impact. Indeed, the expansion of banking and financial intermediation activities coincided with a period of rapid economic growth.

The Chilean experience highlights the importance of political variables on the performance of a banking system. With a small ruling elite and concentrated economic power, Chile had great difficulty creating note-issuing banks that were completely independent of the government. This is the main reason why, regarding note supply, the Chilean banking industry did not achieve the degree of freedom promised by the 1860 law. We need additional research to determine whether Smithian free banking is more likely to be viable in a decentralized political system. The Chilean experience also suggests that even in an unfavorable political environment a free-banking law can create freedom in common banking activities and be successful in developing the financial and banking industry.

WHAT DO WE KNOW, AND WHAT DO WE NEED TO KNOW?

The term “free banking” has been applied to a wide variety of banking systems. This makes classification and evaluation of the historical experience difficult. The term “free banking” moreover, tends to provoke strong ideological responses. Nevertheless, we believe that we can discern some areas of agreement among students of the six cases we have examined in detail here, a set of conclusions that we believe is generally supported by the weight of the other cases discussed in the literature.

(1) “Free banking,” as the term was used in the nineteenth century, referred to banking systems that were far removed from *laissez faire*. In all of the historical cases examined here banks were subject to numerous regulations. Most importantly, it was required that notes issued by banks had to be redeemable on demand in high-powered money. At times we have used the term “Smithian free banking” to describe the common features of these systems because Adam Smith advocated freedom for banking provided that this basic restriction, as well as some secondary restrictions such as a limitation on low denomination bank notes, were in place. A number of so-called free-banking systems were subject, moreover, to additional restrictions. Clearly, the term “lightly regulated banking” is more accurate than “free banking” if not faithful to the historical language.

(2) It appears that wildcat banking, if it existed at all, was at most a very rare phenomenon. Some of the few cases of overissue and wildcat banking that have been reported resulted from legislation that undermined the ability of the public to distinguish among the notes issued by individual banks. There were, of course, bank failures under free banking, especially in the United States where restrictions on branch banking weakened the system. Corruption and foolishness occurred in lightly regulated banking systems, and as in any private industry, failure was an important way of disciplining the system.

(3) A lot of very good banking was done in lightly regulated banking systems. Examples include Scotland, New York under the system that prevailed in the United States before the Civil War, Canada, Sweden, Switzerland, and Chile in the nineteenth century, and many less well studied cases. Evidently, there were a variety of lightly regulated banking systems that could serve as models for sound banking systems.

(4) Some students of these episodes attribute the success of these systems to the residual restrictions. An advocate of lightly regulated banking who wished to include additional forms of protection for note-holders or depositors, such as extended liability or collateral requirements, could find support within the literature.

(5) Perhaps the question on which scholars disagree the most is whether a lightly regulated banking system can dispense with a lender of last resort. While some students of lightly regulated banking argue that a central bank is unnecessary; others maintain that the potential for a banking panic exists in any fractional reserve system, so that some institutional arrangement is needed to deal with the problem. The Smithian system, which outlaws option clauses for notes may well increase the need for a central bank. Even such staunch advocates of freedom as Milton Friedman and Anna J. Schwartz (Friedman and Schwartz 1986, Schwartz 1993) have expressed doubts about dispensing altogether with a lender of last resort. The jury is still out.

(6) Lightly regulated banking was possible in a world characterized by considerable confidence in *laissez faire* and limited and decentralized government. The chance of a return to free banking with respect to the issue of notes appears to be low. After all, most people in the world today have never seen a privately issued bank note. Knowledge that the private issue of notes worked well in the nineteenth century is confined to a small group of monetary and financial historians. Governments, moreover, would be forced to sacrifice some seignorage in the process of returning to privately issued notes. On the other hand, further relaxation of restrictions on deposit banking, by far the more important component of the money supply, may well be in the offing. In recent decades the consequences of liberalizing branch banking in the United States, for example, are in line with what students of nineteenth century banking concluded long ago. The story of free banking, moreover, provides a cautionary tale about judging the regulation of banking on the basis of anecdotes rather than evidence.

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INVESTIGATING THE APPARATUS

The Federal Reserve System's Influence on Research in Monetary Economics

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[Abstract](#), [Keywords](#), [JEL Codes](#)

THE FEDERAL RESERVE SYSTEM IS NOT ONLY THE SUBJECT OF research by American monetary economists it is also a major sponsor of their research. The Fed (the Board of Governors plus the twelve regional Reserve Banks) employed about 495 full-time staff economists in 2002. That year it engaged more than 120 leading academic economists as consultants and visiting scholars, and conducted some 30 conferences that brought 300-plus academics to the podium alongside its own staff economists. It published more than 230 articles in its own research periodicals. Judging by the abstracts compiled by the December 2002 issue of the *e-JEL*, some 74 percent of the articles on monetary policy published by US-based economists in US-edited journals appear in Fed-published journals or are co-authored by Fed staff economists.¹ Over the past five years, slightly more than 30 percent of the articles by US-based economists published in the *Journal of Monetary Economics* had at least one Fed-based co-

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1. Here “articles on monetary policy” consists of articles falling in *e-JEL* subcategories E42 and E50-59. Details appear below.

author. Slightly more than 80 percent had at least one co-author with a Fed affiliation (current or prior Fed employment including visiting scholar appointments) listed in an online *vita*. The corresponding percentages for the *Journal of Money Credit and Banking* were 39 percent and 75 percent. The editorial boards (editors and associate editors) of these journals are even more heavily weighted with Fed-affiliated economists (9 of 11, and 40 of 46, respectively).

The stated goals of the Fed's research program are improvements in monetary policy and banking regulation. These goals are unobjectionable. But we should consider how the Fed's sponsorship may influence the character of academic research in monetary economics.² One possible influence is a simple "crowding out" effect: unless the supply curve of monetary economists is flat, incentives to study policy design within the context of the *status quo* monetary regime will crowd out research on alternative monetary regimes. But the influence may be more systemic than that. If academic research is subject to network effects—meaning that the larger the community of researchers who investigate a particular topic or take a particular approach, the greater the professional rewards to other researchers for doing likewise—then even those researchers outside the Fed's direct sphere of influence will be indirectly influenced by its program. They know, for instance, that their research must pass muster with Fed-affiliated journal editors and referees.

Federal Reserve officials themselves proclaim that the Fed's research has an impact. Anthony M. Santomero, President of the Federal Reserve Bank of Philadelphia (and a University of Pennsylvania emeritus professor) remarked to a research conference that "our Philadelphia Fed Research Department . . . continues to make substantial contributions to the field of economics and to set the standard for economic research" (2002, 2). He advised the conference's academic participants of the direction he would like their research to take: "The payments system has not received the time and attention it deserves on the academic research agenda. I encourage your interest and involvement to fill the void in this critical area of research" (2002, 3).

2. Mayer (1999) discusses the reverse question of the extent to which academic research influences the makers of monetary policy. On the convergence between academic and Fed researchers with respect to "concerns and techniques" see McCallum (1999). In a fashion parallel to my methods here, Klein and DiCola (2004) investigate the development-agency ties of the authors and editors of the *Journal of Development Economics*.

The size of the Fed's research program and its possible *status quo* bias have attracted little scholarly attention, though a few economists have made pertinent remarks in passing. Edward J. Kane (1980) has observed that Fed officials naturally promote the kind of staff research that they consider useful, particularly the better quantification of monetary policy linkages. Such research, he noted, tends to regard the Fed as a social-welfare optimizer. We might add that it takes current institutional arrangements for granted. Kane (1993, 290) has likewise noted that "by manipulating the size of staff and the activities for which they are rewarded or penalized, Fed officials help to shape the agenda of contemporary economic research on monetary policy."³ Robert D. Auerbach (1985, 52), in an account of political influences on Fed policy-making, has commented that "censorship is present in a significant portion of the Federal Reserve research departments' publications. Therefore, this voluminous research, distributed at little or no charge, should not be disguised as the work of an independent think tank." Eugenia Toma and Mark Toma (1985) have argued that the Board of Governors used budgetary allocations to penalize two Reserve Banks (St. Louis and Minneapolis) whose research departments were relatively critical of the Fed's policy-making.⁴

Milton Friedman, as reported in a Minneapolis Fed magazine article on the Shadow Open Market Committee (Fettig 1993), "maintains that since the Federal Reserve Board and its district banks hire a large number of economists in the field of money, the central bank has a sort of oligopoly on monetary opinion. In other words, if you want to advance in the field of monetary research, according to Friedman, you would be disinclined to criticize the major employer in the field. 'This problem with the Fed is why the Shadow is so relevant,' says Friedman." (The Shadow Open Market Committee is a panel of economists who critically review Fed policy actions from a Monetarist perspective.)

Here I examine the size and scope of the Fed's research program—the extent of its "oligopoly" in research—by detailing the various ways in which the Fed generates and influences research in monetary economics. I

3. The *Federal Reserve Research Roundup*, a newsletter of the Financial Markets Center (an independent think tank), makes a similar observation at the outset of a recent issue (4th quarter 2002): "As one of the world's largest employers of economists, the Federal Reserve produces an unparalleled volume of research. ... This huge program provides the Fed ongoing opportunities to shape lines of inquiry and schools of thought throughout the economics profession."

4. Rolnick (1985) challenged their claim with regard to the FRB Minneapolis.

provide some measures of the Fed's efforts, its inputs into the process of producing professional research, and some measures of the output of those efforts in the professional journals. But while we can count publications by Fed-employed and Fed-affiliated economists, we cannot observe the counterfactual world where the Fed does not exist or spends nothing on research. We thus cannot directly measure how far the Fed's research program shapes the character of monetary economics, although we can try to judge the direction of influence by observing the types of research the Fed publishes. It must of course be left to the reader to evaluate whether marginal research of those types has benefits in excess of its opportunity cost of foregone research in other directions.⁵ To be clear, my aim is to draw attention to the institutional incentives and filtering mechanisms at work in shaping Fed-sponsored research; it is certainly not to imply that researchers employed by the Fed are disreputable or act in bad faith.

MAKE OR BUY?

The Federal Reserve System faces a "make or buy" decision with respect to economic research: "make" it in-house or "buy" it from outside economists. In practice the Fed does some of each. The Fed's "making" of research comprises (1) employment of staff economists, and (2) in-house publishing of books, periodicals, and working papers. To avoid double-counting, expenses allocated to the second category would exclude the salaries of the staff economists who have written for the publications.

The system's research departments seldom interact with business economists and forecasters other than to share the stage with them at FRB regional "business outlook" conferences. Accordingly the Fed's "purchase" of research is mostly from academic economists. It most importantly includes (1) visiting scholar programs and consulting arrangements, (2) conferences and seminars where academics present papers (alongside Fed economists), and (3) sponsorship of out-of-house publications such as special issues of academic journals.

5. One Fed staff economist's reaction to a draft of this paper was the suggestion that recent academic research on money has been largely useless from the perspective of those who make monetary policy.

While the distinction between internal and external production is useful for organizing discussion, some Fed expenditures on research straddle the line. When a Reserve Bank's research department brings in an academic economist for a stint as a visiting scholar, or to present a working paper at a department seminar, the expense may contribute both to that academic's research and to the research of the staff economists by keeping them abreast of work in the field. When a visiting scholar co-authors with a staff economist, the visit's expense contributes to internal as well as external production. When a visiting scholar is expected to produce an article for the Bank's research periodical, part of the expense of her visit can be allocated to in-house publishing.

Staff Economists

Table 1 details the distribution of the Fed's 495 full-time staff economists between the Board of Governors (220) and the twelve regional Reserve Banks (275). It also reports the numbers of listed Visiting Scholars, whose role is discussed later. To put the number of Fed staff economists in context, the top 50 Ph.D.-granting US economics departments together employ about 390 economists in macroeconomics, monetary economics, and banking.⁶ That is, the Fed employs full-time about 27 percent more macro/money/banking economists than the top 50 US academic economics departments put together. (Note also that most of the economists in those departments have been visiting scholars at Federal Reserve banks.) Although some Fed economists pursue research in other areas, this is at least as true of the academic economists counted, who typically list macro or money or banking as one of several interests.⁷

6. The 390 figure is extrapolated from my own examination of 20 departmental website listings. The "top ten" US economics departments list 99 faculty who name macroeconomics, money, or banking as a research interest, while departments "41-50" list 57 such faculty. Combining the twenty departments gives an average of 7.8 macro/money/banking economists per school or an estimated 390 in the top 50 departments. I used a departmental ranking by Dusansky and Vernon (1998), based on publications in eight journals, but there is no reason to think that the count of macro/money/banking economists would change much using a different ranking. The Dusansky-Vernon list is available with hyperlinks to departmental sites at <http://edirc.repec.org/usa-top.html>.

7. I have not tried to estimate the percentages of research time spent on topics other than money/macro/banking by either academic or Fed economists.

Comparative head counts raise the question of whether a Fed staff economist devotes as much time to research as an academic economist. Fase and Vanthoor (2000, 37), who interviewed research directors at each of the twelve Reserve Banks and at the Board, indicate that staff economists spend “half of their time on basic research and the rest on policy and briefing activities.”⁸ While Fed staff economists thus have duties other than research, the same is true for academic economists who teach. My own discussions with staff economists indicate that the typical staff economist at a Federal Reserve Bank has at least as much time for research as the typical economist at a research university who teaches three or four courses per year. Jansen (1991, 735) offers a similar estimate.⁹

Table 1: Staff Economists and Visiting Scholars

	Staff Economists	Visiting Scholars
Board of Governors	220	17
FRB-Atlanta	21	3*
FRB-Boston	24	7
FRB-Chicago	36	8
FRB-Cleveland	19	16
FRB-Dallas	26	2
FRB-Kansas City	17	9
FRB-Minneapolis	7	25
FRB-New York	49	14
FRB-Philadelphia	15	16
FRB-Richmond	16	6
FRB-Saint Louis	22	11
FRB-San Francisco	23	13
Total	495	132

8. The quote from Fase and Vanthoor (2000, 37) appears in the summary of their discussions at the FRB San Francisco. Economists at the FRB Kansas City reported the same time split to them (45). The reported range across staff economists at the FRB Chicago (31) runs from 10 percent to 100 percent of time on basic research. Discussions at other Banks yielded no reported time split.

9. Jansen (1991) ranks FRB and Board research departments against each other, and against academic departments, by tabulating publications in academic journals in 1978-83 and 1983-88. My measures of Fed-produced output appear larger than his because (1) I count articles in Fed journals where he counted only articles in academic journals, and (2) FRB economists are publishing much more in academic journals now than they did 20 years ago.

Notes on Table 1: Latest year reported by each institution (2001, 2002 or 2003). Tallied 1/21/03 from Board and FRB web sites, except: Kansas City visitors for 2002 by email from Robert Hampton, Manager, Economic Research Department, FRB-Kansas City; St. Louis visitors for 2002 by email from Daniel Thornton, Visiting Scholar Coordinator, FRB-St. Louis. St. Louis staff number includes one listed as “mathematician”. “Visiting scholars” includes external economists listed as such or as “consultants” (Chicago and Minneapolis), “associates” (Dallas) or “visitors” (Minneapolis). The numbers exclude intra-Fed and inter-central bank visitors (the Board had 1 visitor from the FRB-Richmond and one from the European Central Bank.) The 132 visiting-scholar total is less than the column sum because it excludes multiple counting of scholars who visited more than one Bank.

*See text footnote 12.

In-house Research Publications

Each of the 12 Reserve Banks, and the Board of Governors, publishes one or more research periodicals. Some are formatted and bound like academic journals. Others look more like newsletters. Fed staff economists write most of the articles, but academics also contribute as authors or co-authors. All of the principal research publications, listed in Table 2, are fully available online, free of charge. All are also available free of charge in hard copy, except the FRB-Dallas *Economic and Financial Policy Review* (which has been online-only since the end of 2001) and the *Federal Reserve Bulletin* (which is \$25 per year in hard copy). I examine below the question of how large the output of these periodicals looms in monetary economics, together with the publications of Fed-affiliated economists in academic journals.

No article appears in any of these periodicals without first being reviewed by staff of the Board of Governors.¹⁰ A well-known academic researcher once told me that as a visiting scholar at a Federal Reserve Bank he contributed an article to the Reserve Bank’s journal, only to have the Board’s reviewers blue-pencil a passage for removal because it criticized the System’s policy-making during an episode *fifty years earlier*. Most contributors, one expects, have learned to self-censor potential criticism of the Fed’s policy-making or institutional structure. Where a Fed-published article does mention a criticism, the author is typically reporting the

10. Toma and Toma (1985, 181) briefly relate the origins of this review policy. Auerbach (1985, 52) reports that “the practice at the [Federal Reserve] Bank where I worked was to clear research through the Board of Governors and to ‘persuade’ economists to delete material that the Board or the Bank officials did not like,” adding “[n]ot all the research is changed.” Fase and Vanthoor’s (2000, 32) discussions with FRB economists confirm the review policy’s continued existence.

argument of some set of economists, which he then balances against the contrary argument of others.

Table 2: Fed Research Periodicals

	Journal Name	# of Articles, 2002
Board of Governors	<i>Federal Reserve Bulletin.</i>	14
FRB-Atlanta	<i>Economic Review.</i>	17
FRB-Boston	<i>New England Economic Review</i>	22
FRB-Chicago	<i>Economic Perspectives</i>	16
FRB-Cleveland	<i>Economic Commentary</i>	20
FRB-Dallas	<i>Economic and Financial Policy Review</i>	6
FRB-Kansas City	<i>Economic Review</i>	14
FRB-Minneapolis	<i>Quarterly Review</i>	7
FRB-New York	<i>Economic Policy Review</i>	23
FRB-Philadelphia	<i>Business Review</i>	18
FRB-Richmond	<i>Economic Quarterly</i>	16
FRB-Saint Louis	<i>Review</i>	29
FRB-San Francisco	<i>Economic Review</i>	4
	<i>Economic Letter</i>	38
Total		244

Notes: The FRB-Cleveland *Economic Review* ceased publication 4Q 2001; the *Economic Commentary* series contains the sort of articles that appear in the research journals of the other FRBs. Article count includes transcribed speeches but excludes what appear to be introductions or comments.

There have been a few conspicuous in-house criticisms of Fed policy over the years, but they are exceptions to the general pattern. The Monetarist critique of Fed discretion found its way into some Fed periodicals, most notably those of the FRB-St. Louis. Today the current president and research director in St. Louis (Robert Poole and Robert Rasche) are former members of the Shadow Open Market Committee, but

the research staff no longer produces such distinctly Monetarist work.¹¹ At the FRB-Cleveland, where recently retired president Jerry L. Jordan favorably cited laissez-faire monetary theorists (Friedrich A. Hayek, Ludwig von Mises, Benjamin Klein, George Selgin) in proposing a greater future role for competitive markets in the global monetary order (Anonymous 1995; Jordan 1999), only one staff economist (Ed Stevens) has published kindred views.

Fed journals have published a few articles that highlight the benefits of pre-commitment to a monetary policy rule, a la Kydland and Prescott (1977), but such articles are relatively rare. Lee Hoskins (1993, 50), former President of the FRB-Cleveland, once commented:

It still puzzles me that volumes of research have been published on central bank operating procedures and management of monetary aggregates, yet relatively little research has been published on the value of a credible precommitment to a price-stability objective. My intuition tells me that the latter is far more important than the former in terms of economic welfare. (Hoskins 1993, 50)

Four economists then working for the FRB-New York (Bernanke, Laubach, Mishkin, and Posen, 1999) published a book arguing for “inflation targeting,” but theirs was a proposal for “constrained discretion” (6), not for a rule. Robert Hetzel (1997) is a rare example of a Fed-employed economist explicitly setting forth an overall judgment favoring the legislative imposition of a rule to direct Fed policy.

By my count, twice as many pro-discretion articles (12) as pro-rules articles (6) have appeared in Fed publications over a recent five year period (1998-2002 inclusive).¹² Thus the preponderance—among those infrequent

11. Hafer and Wheelock (2001) trace the rise and subsequent decline (after the velocity of standard monetary aggregates became erratic in the 1980s) of proposals for money-growth rules at the St. Louis Fed, 1968-86.

12. A list with full citations is available from the author. Admittedly, personal judgment enters into any such sorting. The articles I counted as pro-discretion were by Chang (Atlanta 1998), Haubrich (Cleveland, 2000), Miller (Cleveland, 2002), Cecchetti (New York, 1998), Meyer (St. Louis, 2001), Chatterjee (Philadelphia, 1999 and 2001), Santomero (Philadelphia, two in 2002), Judd and Rudebusch (San Francisco, 1999), Anonymous (San Francisco, 1999), and Walsh (San Francisco, 2001). The articles I counted as pro-rules were by Kydland and Wynne (Dallas, 2002), McCallum (Richmond, 2000), Hetzel (Richmond, 2000), Broadbuss (Richmond, 2001), Wolman (Richmond, 2001), and Poole (St. Louis, 1999).

articles that broach the topic—lies on the side of the status quo. As an example, visiting scholar Carl E. Walsh (2001) writes in the FRB-San Francisco *Economic Letter*:

There is a long tradition of trying to take discretion out of monetary policy—Milton Friedman's proposal that the Fed should just ensure a constant annual growth rate for the money supply was an example of a policy designed to remove the role of the individual policymaker. While economists have identified broad principles to guide policymakers, making policy is not a science. Good policy will probably always require good policymakers, as it requires combining the science of the economist with the art of the practitioner. (Walsh 2001, page)

Federal Reserve Board chairman Alan Greenspan (2002) occasionally makes guardedly favorable comments about the gold standard as a monetary regime. A recent article by Kydland and Wynne (2002) is the only recent Fed-published article to do likewise (and they are even more guarded than Greenspan).

I have not found a single Fed-published article that calls for eliminating, privatizing, or even restructuring the Fed. Research on “free banking” has been limited to evaluations of the antebellum state banking regulatory systems that went by the name. With one exception, the notion of laissez-faire banking has not been discussed.¹³

In addition to their journals, the research departments at the Board and at each of the Reserve Banks make staff working papers available free of charge, and publicize their availability widely. For example, the FRB-New York annually mails out, free of charge, a twenty-four page summary of its *Publications and Other Research*. The website *Research Papers in Economics* (www.ideas.repec.org) lists ten different working paper series from the Board of Governors alone.

This list excludes articles specifically concerned with regime choice for *other* countries (i.e. floating, pegging, dollarization, or currency boards).

13. The exception is a brief article by British economist Kevin Dowd (1993) in a symposium of contrasting views on deposit insurance.

Other Periodicals: Newsletters, Magazines

In addition to research-oriented periodicals and working papers series, the Reserve Banks publish magazines and newsletters for business and general audiences. For example, the FRB-Minneapolis publishes *The Region*, a quarterly magazine of articles and interviews that “explores banking and economic policy issues that relate to Federal Reserve activities.” The FRB-New York publishes *Current Issues in Economics and Finance*, which it describes as “a newsletter-style publication focusing on economic and financial topics,” and *Second District Highlights*, “a regional supplement to *Current Issues* covering financial and economic developments in the Federal Reserve System’s Second District.”

Articles in these less-technical Fed periodicals are often assigned to students. As an example, consider the supplemental reader to Frederic S. Mishkin’s market-leading money and banking textbook. Articles and excerpts from Federal Reserve publications make up 31 of 32 readings in Eaton and Mishkin’s *Readings to Accompany The Economics of Money, Banking, and Financial Markets*, 4th ed. (1997).¹⁴ Nine of the 31 are from Fed periodicals other than the research journals listed in Table 2, namely the FRB-Richmond *FRB Cross Sections*, FRB-Dallas *The Southwest Economy*, FRB-St. Louis *The Regional Economist*, FRB-New York *Current Issues in Economics and Finance*, and the *Chicago Fed Letter*. One is from an FRB-Richmond monograph. Perhaps the primary reason that Fed articles are so popular with Eaton and Mishkin is that they are free to reprint. But it may also be noted that from 1994 to 1997 Mishkin was Executive Vice President and Director of Research at the Federal Reserve Bank of New York.

Books

In addition to conference proceedings (see below), occasionally one of the Federal Reserve Banks publishes under its own imprint a book directed at economists or economics students. Particularly noteworthy is the FRB-New York’s supplemental textbook on *U. S. Monetary Policy & Financial Markets* by Ann-Marie Meulendyke (1998). (Previous editions appeared in 1982 and 1990.) The FRB-New York makes the text available

14. The 4th edition was published in book form. Mishkin’s current reader is online only, password protected. The one non-Fed article in the 4th edition was a piece on the “Big Mac Index” reprinted from *The Economist*.

free of charge. A Google search finds it as assigned reading on the money and banking syllabi at 17 colleges. One syllabus declares: “This book is free and will be distributed in class.”

Fed-sponsored academic conferences

In 2002 the Federal Reserve System sponsored or co-sponsored at least 29 conferences where 328 US academics made presentations (the number of unique academic participants was somewhat smaller due to a few participating in multiple conferences). At these conferences academic economists typically made presentations on panels alongside Fed staff economists (and sometimes economists from foreign central banks). There is no reason to believe that 2002 was an unusually active year.

Visiting scholars and consultants

As Table 1 above reports, the most recent available lists name a total of 132 academics as visiting scholars at one or more Federal Reserve Banks during the year.¹⁵ Occasionally an academic visitor may spend several continuous months in residence, but more commonly the visitor stays a week, or (if teaching nearby) drives in a dozen or so days during a semester. The FRB-Philadelphia website explains:

Each year, the Research Department hires several academic researchers as visiting scholars. These scholars visit the Bank to interact with our staff economists, to present seminars, to further their own research agendas, and to advise our staff economists on their research. Several arrangements are possible—some scholars spend one day a week at the Bank for a semester or longer, and

15. At least one FRB website list seriously understates the number of visiting scholars. The FRB Atlanta site lists only 3 visiting scholars, but Gerald Dwyer, head of the Bank’s financial research group, reports (personal communication) that 26 scholars each visited the Research Department for 5 or more days in 2002. It seems likely that other FRB websites— especially those where the visitor list is fewer than 10 names—also understate the actual numbers of visitors. Twelve economists have the distinction of being simultaneously listed as visitor by two or more Fed institutions.

others visit for more extended periods. (FRB-Philadelphia 2005)

Those who visit an FRB for an extended period are commonly expected (or required) to contribute an article to the Bank's research journal. Often a visiting scholar is co-authoring with a staff economist. In cases where no such demands are made, the visitor pursues his or her own research under the Fed's roof rather than the home university's.

The prospect of being a visiting scholar at a Federal Reserve Bank or the Board of Governors presumably encourages some economists to spend more time on research considered interesting and relevant by those in charge of invitations at the Bank's or the Board's research department. (If that were not a criterion for invitation, it would be hard to see why visiting scholars are given time to pursue their own research during their visit, rather than being expected to spend all day interacting with staff economists). By the same token, the prospect discourages work that would be frowned upon at the Fed. The prospect of potential future Fed employment works in the same direction. Precisely *how much* the incentive reorients research in the profession is, for reasons noted above, an elusive empirical question.

PUBLISHED OUTPUT

To evaluate the size of the Fed's impact on professional research, we would like to know *what share of all US publications in monetary economics* is published in Fed periodicals, or authored by the Fed staff economists (wherever published), or by Fed staff plus Fed visiting scholars. It is infeasible to make the denominator the total output of all US-based economists in monetary economics (and the numerator the output of all Fed-based economists) during a year. Even if (counterfactually) the manpower for such a count were available, two important problems would arise. (1) There is no obviously correct scheme for assigning relative weights to (say) an article in the *Journal of Political Economy*, an article in the FRB-Richmond *Quarterly*, and a chapter in an NBER conference volume. (2) There would be many judgment calls as to which articles should be counted as work in monetary economics (e.g. should work in growth theory be counted?).

I offer two alternative measures that sidestep these two problems. First, the listing of “Articles in Current Periodicals” in a recent issue of the *e-JEL* (December 2002), under the subject classification “E-Macroeconomics and Monetary Economics,” provides information on more than 600 recent articles in the field from a very wide array of journals. Relying on the *e-JEL* database means implicitly assigning a zero weight to publications (books, book chapters, articles in unrecognized periodicals) not abstracted there, and accepting the authors’ self-categorization of their articles. The *e-JEL* lists each article under one or two author-chosen two-digit sub-classifications. I took note of the cross-listings so as not to double-count articles within any one-digit class. For each article, the *e-JEL* lists the journal of publication (with a link to the publisher’s website) and the authors’ self-reported affiliations. Although the *e-JEL* lists some articles published in languages other than English (provided they have English summaries), I eliminated those from consideration as negligibly important for the academic discussion in the United States. I then sorted the journals into four mutually exclusive groups by institutions and countries of origin. The groups are: Federal Reserve (both Board of Governors and Reserve Bank), Government (e.g. IMF, Bureau of Labor Statistics), US academic (including journals published by think tanks), and foreign.¹⁶ I likewise sorted the articles by their authors’ affiliations as reported by the *e-JEL*.¹⁷

Appendix One summarizes the numbers, by subject classification. I focus on the articles in Fed and US academic journals (the first and third groups), and by Fed and US academic authors, as representing the most important literature and participants in the US discussion. For each category I compute two ratios: (1) a relatively narrow *ratio of Fed production*, namely the share of US journal articles either published by the Fed or written by authors who identify themselves as employed by the Fed, and (2) a relatively

16. The rules for sorting journals between US and foreign were: (1) the official journal of a nationally identified non-US organization (e.g. Austrian Academy of Sciences) was assigned to the foreign category; (2) any other academic journal was considered a US journal if the publisher lists *any one* of its editors as affiliated with a US institution. Thus almost all Kluwer and Elsevier journals, though published in Europe, were counted as US journals. The sorted list of journals is available from the author on request.

17. Here the rules were: (1) the article was assigned to the Federal Reserve category if the *e-JEL* reports Fed employment for any co-author of the article; (2) otherwise the article was assigned to the government category if the *e-JEL* reports for any author employment at a US institution (all but a few were at universities). Employment affiliations reported by the *e-JEL* were those the authors self-reported in the original publication source. A few authors reported employment as a Fed visitor for an article in the Fed-published category, but not for another article in one of the other three categories.

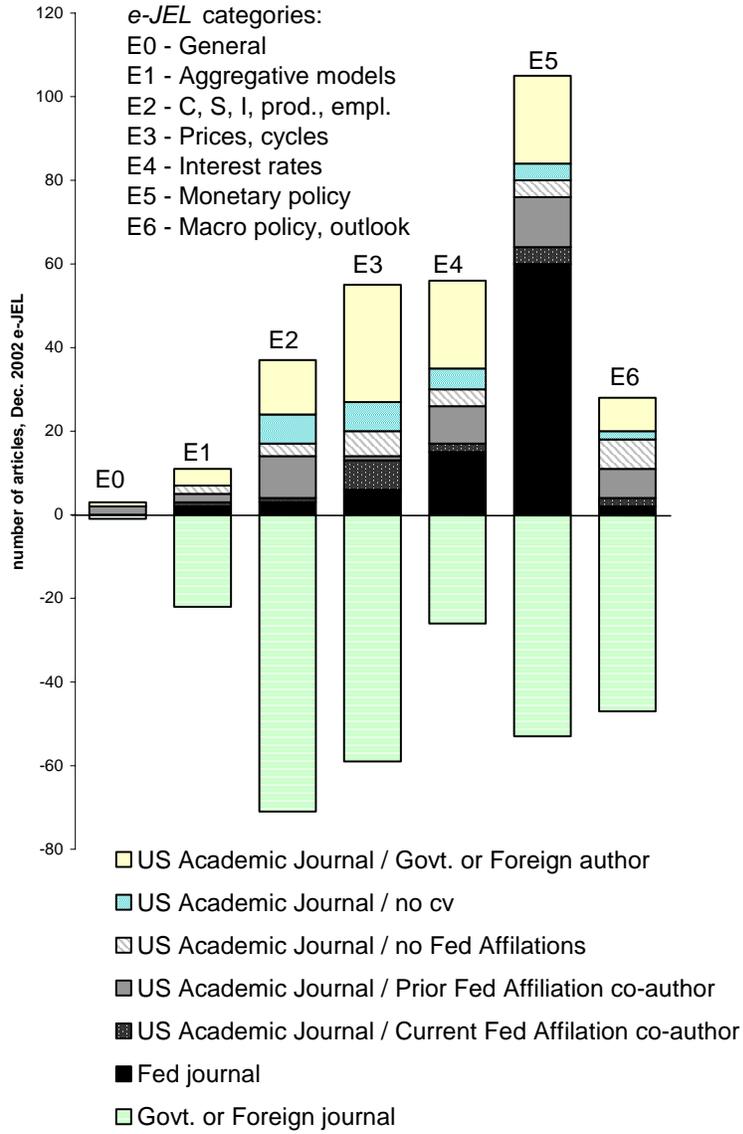
broad *ratio of Fed affiliation*, namely, the share of US-co-authored articles in broadly US journals either published by the Fed or, where the authors have online *curricula vitae*, co-authored by an economist ever employed by the Fed (current or previous staff economist; current or previous visiting scholar, consultant, or advisor).

Both ratios omit articles by government and foreign economists from the denominator, in that way increasing the reported ratios of Fed influence in the broadly US journals, but they equally omit articles by government and foreign central bank economists from the numerator. Because the Fed rarely hires visitors and consultants from foreign universities or from other government agencies, the “Fed affiliated” ratio may be viewed as answering the following question: of the articles in monetary economics in broadly US journals, any of whose co-authors *might* have worked (full-time or visitor) at the Fed, what percentage have a co-author who *has* worked at the Fed? The same numbers are shown graphically in Figure 1.

The Fed-produced and Fed-affiliated ratios are highest in *e-JEL* category “E5-Monetary Policy, Central Banking, and the Supply of Money and Credit” because that is where Fed staff economists focused their efforts. Whereas the numbers of US academic-produced articles were rather evenly distributed across categories E2, E3, E4, and E5 (respectively 20, 18, 20, and 18), the numbers of Fed-produced articles were quite skewed toward the E5 category (14, 13, 17, 64). Consistent with the hypothesis that the Fed’s incentives lead researchers to avoid considering alternatives to the institutional status quo, Fed production in subcategory “E42-Monetary Systems-Standards-Regimes-Government and the Monetary System” amounted merely to 4 articles (of 8 US articles), of which only two dealt with monetary standards or regimes (the other two concerned the retail payment system), while Fed production in subcategory “E52-Monetary Policy (Targets, Instruments, and Effects)” was 55 articles (of 68).¹⁸

18. These article counts support Kane’s (1993, 290) view that the “bureaucratically approved issues” for Fed staff research “focus on the control subsystem,” e.g. the usefulness of various intermediate targets, “rather than on the broader principal-agent conflicts comprised in the information and incentives subsystems of monetary policy-making.”

Figure 1: Fed-published and Fed-affiliated articles, by e-JEL category



As an alternative to the *e-JEL* snapshot, Appendix Two offers counts of the articles appearing in three leading academic monetary and macroeconomic journals (the *Journal of Monetary Economics*, the *Journal of Money, Credit, and Banking*, and the *Journal of Macroeconomics*) for a recent five-year period (1998-2002), apportioning the authorship of their articles in the same fashion. The numbers are shown graphically in Figure 2.

FEDERAL RESERVE AFFILIATIONS AMONG JOURNAL EDITORS

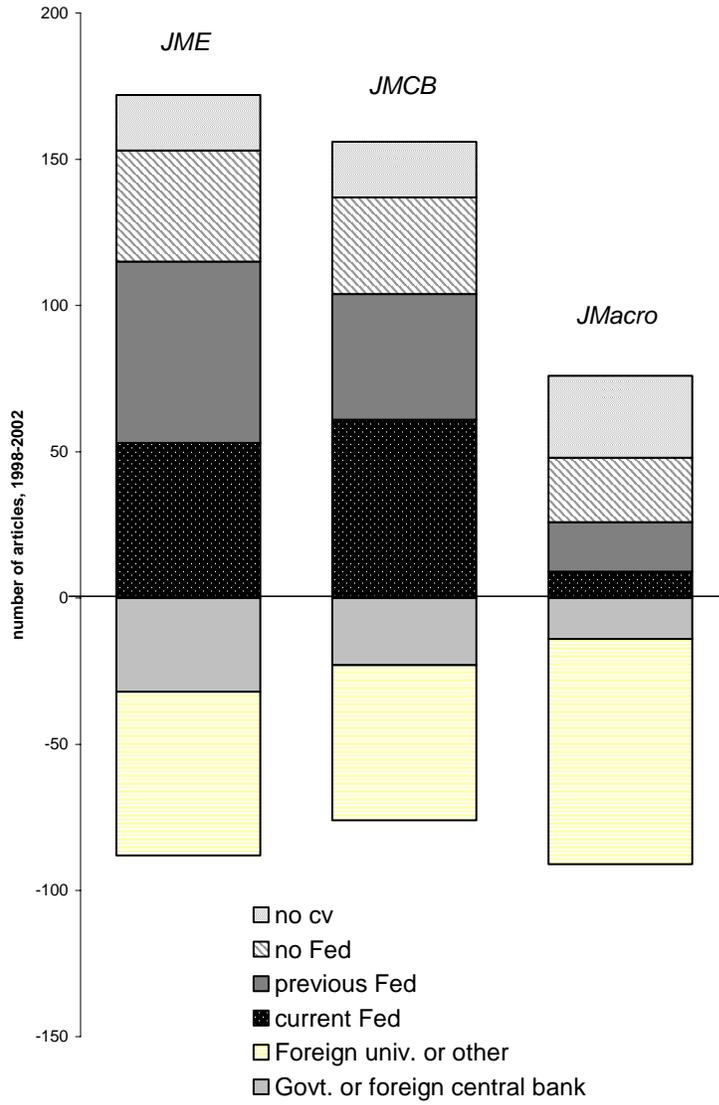
The Federal Reserve Banks draw their visiting scholars and consultants (and sometimes their research directors), as one would expect, from among the most productive academic monetary economists. So do the professional field journals in choosing their editors. Current or prior affiliation with the Fed is therefore pervasive among editors of the journals concerned with money and banking. The appearance is created that becoming a journal editor increases one's probability of being invited to become a Fed visiting scholar (or the reverse). As a result of the overlapping choices of the Fed and the journals, scholars who want to publish in the field of monetary economics must pass through a gateway controlled largely by editors affiliated with the Federal Reserve System. The overlap is personified by two cases. The FRB New York hired Stephen G. Cecchetti, who was and continued to be editor of the *JMBC*, for a two-year stint as its research director, 1997-99.¹⁹ Ben S. Bernanke, who became a member of the Board of Governors of the Federal Reserve System in August 2002, continued simultaneously to serve as the Editor of the *American Economic Review* (a position to which he was appointed in July 2001).²⁰

At the *Journal of Monetary Economics*, 1 of the 2 current editors and 8 of the 9 associate editors have been visiting scholars, advisors, or consultants

19. When Cecchetti took the FRB-New York post, he and Paul Evans (also of Ohio State U) were the *JMBC*'s editors, with Allan Berger (Federal Reserve System Board of Governors) listed as co-editor. In its next issue (November 1997), the *JMBC* began listing five editors: two from the Fed (Cecchetti and Berger), and three from Ohio State (Evans, Peter Howitt, and Nelson G. Mark).

20. According to a Board staff reply to my email inquiry, as *AER* editor he "recuses himself in rare instances that raise conflict of interest questions."

Diagram 2: Author affiliations for *JME*, *JMCB*, *JMacro* articles



at one or more Federal Reserve banks or at the Board of Governors. At the *Journal of Money, Credit, and Banking*, 3 of the 3 current editors and 37 of the 43 associate editors have Fed positions on their resumes. Of the 37 Fed-affiliated *JMCB* associate editors, 8 are currently full-time staff economists with the Federal Reserve System. Of the 6 associate editors who have no Fed affiliation, two work outside the US (one as a Bank of England staff economist, the other a Canadian-based academic and recent Bank of Canada special advisor).

Appendix Three lists the Fed affiliations for *JME* and *JMCB* editors, largely from the listed individuals' online *curricula vitae*.

CONCLUSION

It is relatively straightforward to document how the Federal Reserve System's research program pervades American monetary economics. It is a more subtle problem to evaluate what impact the Fed's research program has on the character of US academic research in monetary economics. Possibly every paper written by a visiting scholar before, after, and during his time at the Fed is exactly the paper that he would have written anyway—in which case the marginal research product of his visit would lie entirely in its indirect contribution to research by the staff economists.

Although the research departments of the regional Reserve Banks seek to establish their own reputations,²¹ their incentives would seem to steer them away from research that would challenge the monetary regime *status quo* favored by the Board of Governors. By contrast, Fed economists are not reluctant to recommend sweeping changes in other government financial institutions, such as Fannie Mae or the Federal Deposit Insurance Corporation (for an example see Eisenbeis and Wall 2002). By extension, an academic economist who values the option to someday receive an offer from the Fed, either to become a staff economist or a visiting scholar, faces a subtle disincentive to do regime-challenging research. To repeat Fettig's (1993) characterization of Milton Friedman's view: "if you want to advance

21. See the interviews with FRB research directors summarized by Fase and Vanthoor (2000). Economists at the Reserve Banks appear to have less career attachment to the Fed, moving back and forth to academia more commonly, than do the economists at the Board of Governors.

in the field of monetary research . . . you would be disinclined to criticize the major employer in the field.”

These incentives and filtering mechanisms may produce a result *as if* the Federal Reserve were deliberately subsidizing research that takes the institutional *status quo* for granted. This should not be surprising, nor is it scandalous. We naturally expect the research that any organization sponsors to tend to promote rather than to undermine that organization’s interests. When (say) the insurance industry sponsors a report on the advisability of federal subsidies for terrorism insurance, the sponsorship alerts cautious readers to scrutinize the research methods and findings for pro-industry bias. Raising the question of the Fed’s *status quo* bias alerts us that the same sort of scrutiny is appropriate to monetary policy research, to avoid employing a double standard. The Fed has an institutional interest in preserving the legal restrictions that generate its seigniorage revenues and the privileges that give it discretionary monetary policy and regulatory powers. Fed-sponsored research generally adheres to a high level of scholarship, but it does not follow that institutional bias is absent or that the appropriate level of scrutiny is zero.

APPENDIX ONE

Summary ratios of Fed influence for articles in the *e-JEL* category “E - Macroeconomics and Monetary Economics”, December 2002 issue, by subcategory

E0 - General (4 articles)

0 in Fed journals
 3 in US academic journals, of which authorship was
 0 Fed
 2 US academic, of which
 2 had previous Fed affiliation
 1 other (foreign-based or government-employed; this residual category is not reported below)

Fed produced: 0/2
 Fed affiliated: 2/2.

E1 - General Aggregative Models (33 articles)

2 in Fed journals
 9 in US academic journals, of which authorship was
 1 Fed
 4 US academic, of which
 2 had previous Fed affiliation
 2 had no Fed affiliation

Fed produced: 3/7
 Fed affiliated: 5/7

E2 - Consumption, Saving, Production, Employment, and Investment (105 articles)

3 in Fed journals
 34 in US academic journals, of which authorship was
 1 Fed
 20 US academic, of which
 10 had previous Fed affiliation
 3 had no Fed affiliation

7 had no cv online
 Fed produced: 4/24
 Fed affiliated: 14/17

E3 - Prices, Business Fluctuations, and Cycles (114 articles)

6 in Fed journals
 49 in US academic journals, of which authorship was

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7 Fed
14 US academic, of which
 1 had previous Fed affiliation
 6 had no Fed affiliation
 7 had no cv online

Fed produced: 13/27
Fed affiliated: 14/20

E4 - Money and Interest Rates (82 articles)

15 in Fed journals
41 in US academic journals, of which authorship was
 2 Fed
 18 US academic, of which
 9 had previous Fed affiliation
 4 had no Fed affiliation
 5 had no cv online

Fed produced: 17/35
Fed affiliated: 26/30

E5 - Monetary Policy, Central Banking, and the Supply of Money and Credit (158 articles)

60 in Fed journals
45 in US academic journals, of which authorship was
 4 Fed
 20 US academic, of which
 12 had previous Fed affiliation
 4 had no Fed affiliation
 4 had no cv online

Fed produced: 64/84
Fed affiliated: 76/80

E6 - Macroeconomic Policy Formation, Macroeconomic Aspects of Public Finance, Macroeconomic Policy, and General Outlook (75 articles)

2 in Fed journals
26 in US academic journals, of which authorship was
 2 Fed
 16 US academic, of which
 7 had previous Fed affiliation
 7 had no Fed affiliation

2 had no cv online

Fed produced: 4/22

Fed affiliated: 11/18

Notes:

“Fed journal” = *e-JEL*-abstracted Federal Reserve periodicals (listed in Table 2), plus the August (part 2) issue of the *JMCB*, which is sponsored by the FRB Cleveland and consists of papers presented at an annual FRB Cleveland / *JMCB* conference

“US academic journal” = *e-JEL*-abstracted journal, not published by the Fed or a government agency, with one or more co-editors based at a US academic institution (a categorized list of journals is available from the author)

“Fed” authorship = *e-JEL* article abstract reports Fed employment (staff economist or visiting scholar) for at least one co-author

“US academic” authorship = *e-JEL* article abstract gives a US institutional affiliation (e.g. a university or think tank), other than the Fed or a government agency, for at least one co-author

“US academic” authorship articles are divided into three mutually exclusive and jointly exhaustive subsets:

“previous Fed affiliation” = Fed staff or visiting scholar position is reported on at least one co-author’s online c.v. or elsewhere

“no Fed affiliation” = a sufficiently complete online c.v. is available for at least one US academic co-author, and reports no previous Fed position

“no cv online” = no online *curriculum vitae* is available for any US academic co-author

The “Fed produced” ratio:

Numerator (Fed produced articles) = Articles in Fed journals + Articles in US academic journals with Fed authorship

Denominator (total US articles): Numerator + Articles in US academic journals with US academic authorship

The “Fed affiliated” ratio:

Numerator (broadly Fed-affiliated articles) = Articles in Fed journals + Articles in US academic journals with Fed authorship + Articles in US academic journals with previous-Fed-affiliated authorship

Denominator (total US articles for which previous Fed affiliation or its absence could be determined) = Numerator + Articles in US academic journals with “no Fed affiliation” authorship

(Both numerator and denominator exclude articles for which no US academic co-author’s c.v. is available online.)

APPENDIX TWO

Ratios of Fed influence for articles in the *JME*, *JMCB*, and *JMacro*, 1998-2002

Journal of Monetary Economics (260 articles)

Authorship was
53 Fed
32 Government bureau or foreign central bank
56 foreign university or other
119 US academic, of which
 62 had previous Fed affiliation
 38 had no Fed affiliation
 19 had no cv online
Fed produced: 53/172
Fed affiliated: 115/143

Journal of Money Credit and Banking (232 articles)

61 Fed
23 Government bureau or foreign central bank
53 foreign university or other
95 US academic, of which
 43 had previous Fed affiliation
 33 had no Fed affiliation
 19 had no cv online
Fed produced: 61/156
Fed affiliated: 104/137

Note: in this count, articles in the Fed-sponsored August (part 2) issues were not counted as Fed-produced unless an author listed the Fed as employer.

Journal of Macroeconomics (167 articles)

9 Fed
14 Government bureau or foreign central bank
77 foreign university or other
67 US academic, of which
 17 had previous Fed affiliation
 22 had no Fed affiliation
 28 had no cv online
Fed produced: 9/77
Fed affiliated: 26/48

APPENDIX THREE

Fed affiliations among current *JME* and *JMCB* editors*Journal of Monetary Economics**Editors:*

Robert G. King, Boston U

Advisor, FRB-Richmond,
July 1984-present.
Visiting Scholar, FRB-
Minneapolis, Jan.-June
1985

Charles I. Plosser, U Rochester

(No Fed affiliation known)

Associate Editors:

David K. Backus, NYU

Economist, FRB-
Minneapolis, 1988-1990.

Marianne Baxter, Boston U

Visiting Scholar, FRB-
Richmond, Summer 1997
Visiting Scholar, Board of
Governors, 1987

Mark J. Bilal, U Rochester

Visiting Scholar, FRB-
Minneapolis, 1984-1985

Ricardo J. Caballero, MIT

(no Fed affiliation known)
Visiting Scholar / Consultant,
Federal Reserve Board,
“multiple occasions”

Janice Eberly, Northwestern U

Visiting Scholar, “several” FRBs and
Board of Governors

Martin Eichenbaum, Northwestern U

Senior Consultant, FRB-Chicago

Sergio Rebelo, Northwestern U

Consultant to Board of Governors

Richard Rogerson, U Pennsylvania

Visiting Scholar, FRB-Minneapolis

Steven Williamson, U Iowa

Economist, FRB-Minneapolis, 1987-
1989Visiting Scholar, Board of
Governors, 2002Visiting Scholar, FRB-Richmond,
2002

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Journal of Money, Credit, and Banking

Editors

Paul Evans (Managing Ed.), Ohio State U	Visiting Scholar, FRB-San Francisco, Sept 1980-June 1981
Mark J. Flannery, U Florida	Research Adviser, FRB-Philadelphia, 1981-1984 Senior Economist, FRB-Philadelphia, 1980 Summer Research Associate, FRB-Boston (1973, 1974), and Board of Governors (1975)
Kenneth D. West, U Wisconsin	Visiting Scholar, FRB-Kansas City, 1998 Visiting Scholar, Board of Governors, 1988-90, 1992, 1993, 1996

Associate Editors

George J. Benston	Visiting Scholar, FRB Atlanta
Mark Carey	staff, BoG
Todd Clark	staff, FRB KC
Mario Crucini	ex staff, FRB Minneapolis
Wouter den Haan	Visiting scholar, FRB Chicago, 1997; BoG, 1994
William Dupor	Consultant, FRB Minn, June 1999
Martin Eichenbaum	Senior Consultant, FRB-Chicago
Robert P. Flood	ex BoG staff
Timothy S. Fuerst	visiting scholar, FRB Cleveland
Jeffrey C. Fuhrer	staff, FRB Boston
Michelle R. Garfinkel	ex staff, FRB St. Louis
Marvin Goodfriend	staff, FRB-Richmond Advisor, FRB Richmond, 1988-92; Visiting Scholar, BoG, 1991, 1992, 1994, and 1996; FRB New York, 1995
Iftexhar Hasan	visiting scholar, FRB Atlanta 2002
Patric H. Hendershott	ex staff, BoG
Donald D. Hester	ex consultant, BoG (dates unknown)
Joel F. Houston	ex staff, FRB Philadelphia 1986-87
Peter N. Ireland	ex staff, FRB Richmond, 1991-1994

George C. Kaufman	consultant, FRB Chicago
Kenneth N. Kuttner	staff, FRB NY (non-US-based; visiting economist and advisor, Bank of Canada)
Karen K. Lewis	ex staff, BoG, 1987, 1991, 1995
Deborah J. Lucas	Advisory Board, FRB New York
Loretta J. Mester	staff, FRB Philadelphia
Frederic S. Mishkin	ex Research Director, FRB-NY, 1994-97
Don Morgan	staff, FRB NY
Charles R. Nelson	Consultant, BoG, 1990-96
Edward Nelson	(staff, Bank of England)
David H. Papell	(U Houston; no Fed affiliation known)
Jonathan A. Parker	visiting scholar, FRB Minneapolis, 2002
Joe Peek	Visiting Economist, FRB Boston, 1985-2000
George Pennacchi	Research Associate, FRB Cleveland, 1995-96, 98-02
Paolo Pesenti	staff, FRB NY
Manju Puri	(Stanford U; no Fed affiliation known)
David Romer	ex visitor, BoG
Stephanie Schmitt-Grohe	ex staff, BoG, 1994-98
Anna J. Schwartz	(NBER; no Fed affiliation known)
Phillip E. Strahan	ex staff, FRB New York, 1993- 2001
Stephen J. Turnovsky	(U Washington; no Fed affiliation known)
Gregory F. Udell	Visiting Economist,, BoG, 1988- 1989; Consultant 1991-1992
Martin Uribe	ex staff, BoG, 1994-98
Christopher Waller	Visiting Scholar, FRB St.Louis, 1994-1995
Carl Walsh	Visiting Scholar, BoG, 1994, May ex staff, FRB SF, 1985 – 1987; current consultant

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Economics in Practice

Decline in Critical Commentary, 1963-2004

PHILIP R.P. COELHO, FREDERICK DE WORKEN-ELEY III,
AND JAMES E. MCCLURE*

[Abstract, Keywords, JEL Codes](#)

IN THE LAST FEW DECADES, THE SPACE DEVOTED TO CRITICAL commentary has declined sharply at top economic journals. We inspected economics journals to collect data on trends in critical commentary—defined as articles classified as comments, replies, rejoinders, and the like. For the period from 1963 to 2004, data were collected for: *The American Economic Review* (AER), *The Economic Journal* (EJ), *The Journal of Political Economy* (JPE), *The Quarterly Journal of Economics* (QJE), and *The Review of Economics and Statistics* (REStat).

In 1989 an editor of the *American Economic Review*, Orley Ashenfelter, explained the decline in critical commentary as follows:

Although the number of articles has now stabilized at about its 1984 level, our publication of notes, comments and replies has decreased steadily since 1985. Both I and my co-editors believe this is a desirable editorial change. Our goal is to increase the number of major, important research papers in the *Review*, and we expect this to come mainly at the expense of our publication of brief notes and comments. (Ashenfelter 1989, 405-406)

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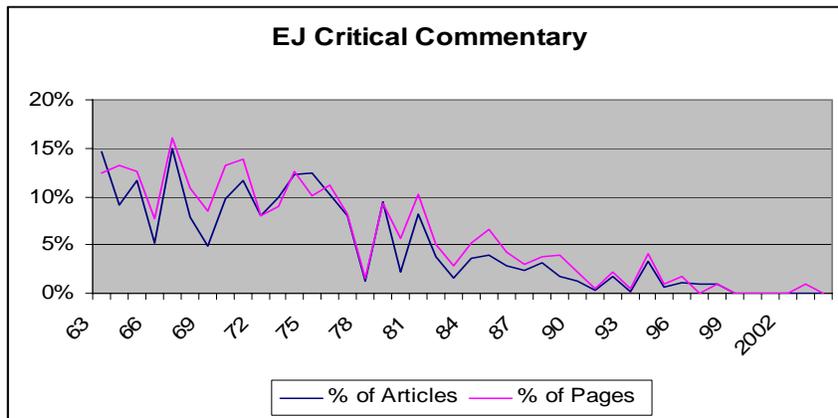
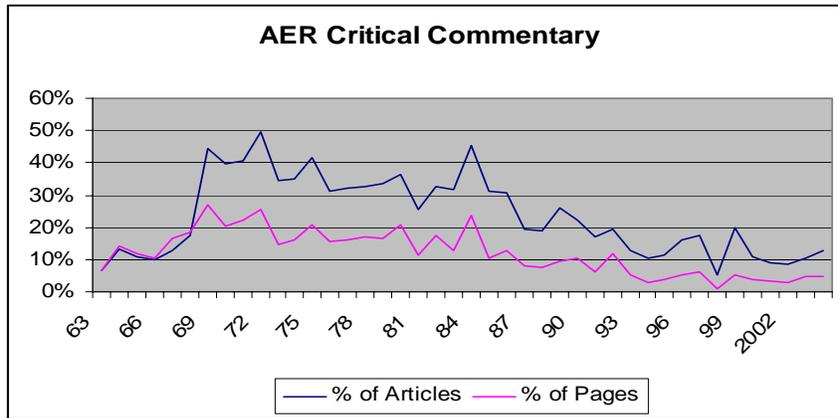
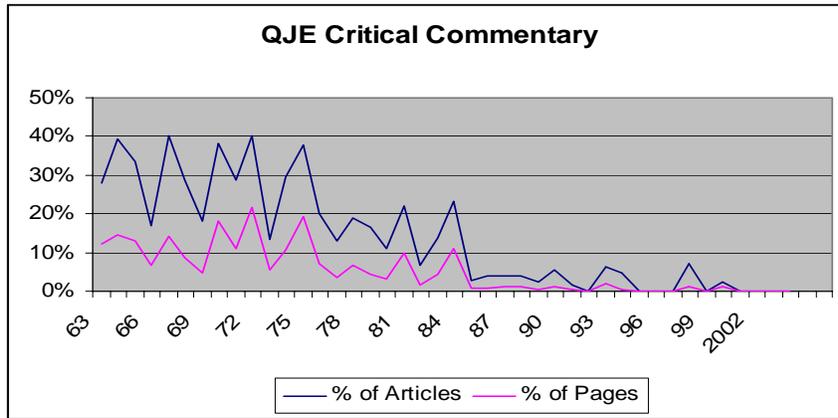
Clearly Ashenfelter and his co-editors believed that a prejudice against critical commentary was desirable. Beyond this, an *AER* co-editor advocated active animosity.

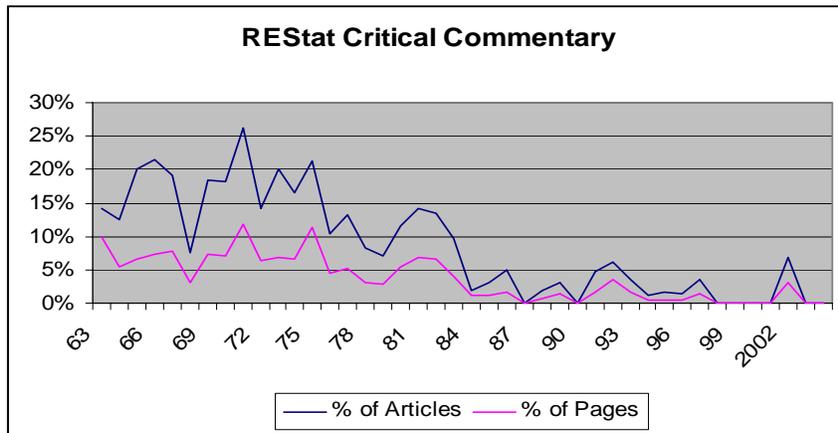
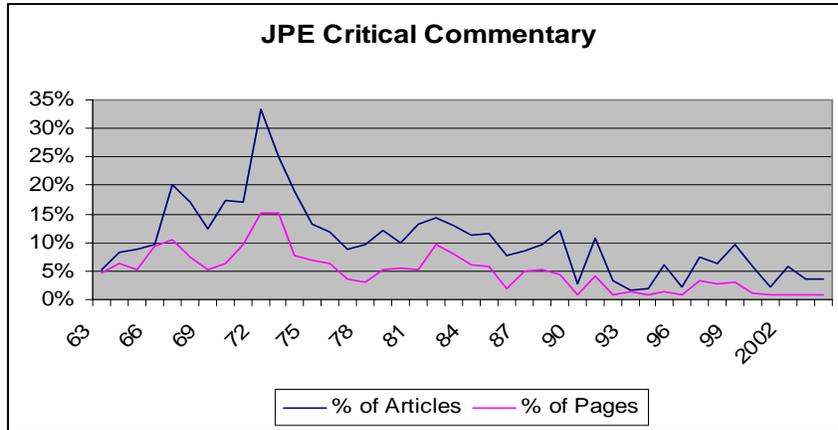
As a matter of policy, the *Review* is intentionally hostile to comments and notes, for overall the readership for comments and notes tends to be restricted to the readers of the original article (see Ashenfelter's editorial statement in any recent *Proceedings of the AER*). It is my personal opinion that many of the comments and notes published in the *Review* historically actually belonged in *Economic Letters*. (R. Preston McAfee 1996)

Laband, Tollison, and Karahan (2002) compiled proxies of authors' inputs (e.g., co-authorship and acknowledgements to referees) and journal-supplied quality controls (e.g., submission fees and editorial board size) and used them as explanatory variables in a time-series analysis of the amount of space devoted to commentary in the *AER*. Among their findings are that commentary is negatively and statistically significantly related to the increasing size of the editorial board, and with the increasing fraction of papers in which colleagues were thanked in the acknowledgement notes that begin papers.¹

Laband et al. provide two explanations for the decline in commentary. The first is that critical commentary has become less and less warranted over time. "We find that there is empirical evidence in favor of the idea that these trends [of declining critical commentary] are mostly due to more *ex ante* investment by authors in their papers. . . . In summary, there appears to be evidence in support of the notion that increasing editorial and author attention to papers before they get published has led to a decline in quality control provided by 'the market' in the form of post-publication commentary" (322).

¹ Laband et al. (2002) also considered contents of articles in terms of usage of tables, equations, figures, appendices, and references. Another statistically significant finding is that the number of equations per article is positively correlated with space devoted to commentary; their interpretation is that formal methods make assumptions more explicit and riper for commentary. Alternatively, this result could have arisen from the reductions in the costs of doing empirical work, which has given rise to a general trend in economics toward empirical publications which typically show fewer equations.





These figures show the time paths, by journal, of critical commentary as a percentage of numbers of articles and the percentage of journal pages. Appendix 1 provides the hyperlink to the Excel file containing the complete data and figures (as well as data on book reviews).

A second explanation provided by Laband et al. is that because *AER* papers have less and less to say over time there is less and less reason to publish an exchange about what is published. “The decline in critical commentary may also be associated with, perhaps in large measure, a long-term decline in the relevance/importance of significant and encompassing ideas in economics. . . . A long-run decline in the relevance/importance of

economic research would be consistent . . . with a decline in critical commentary over time” (329). This rationale dovetails with McAfee’s statement that, “the readership for comments and notes tends to be restricted to the readers of the original article.”

Coelho and McClure (2005) show that over the past three decades there has been a trend in top economics journals toward complex, non-operationalized theories. Meanwhile, empiricism in economics has been rising. Empirical research has advanced with the availability and low prices of computers, data sets, and software. The data may be manipulated to test hypotheses (“forced to confess”). These manipulations are, more often than not, neither transparent nor obvious. Empirical methods and research are more reliable when scrutinized and subjected to debate. It is strange that the profession eschews debate now when it is even more critical to scholarship.

IN PRAISE OF CRITICISM

The benefits of critical commentary are:

1. Errors and limitations are prominently publicized, reducing the likelihood that other scholars will repeat or build on the errors.
2. Readers and the researchers achieve a broader and deeper comprehension of the matter at hand. Readers may be presented with conflicting interpretations.
3. Self-serving behavior by editors is constrained.²
4. The interest of readers is piqued. Not only is criticism inherently interesting, but because publication in such journals is highly valued, readers will be encouraged by the prospect of uncovering errors and writing comments.³

² For empirical estimates of editorial “favoritism” in “full articles” in economics see David N. Laband and Michael J. Piette (1994).

³ Glenn Ellison (2002, 959-960) indicates that “the weighted fraction of pages in the *AER*, *QJE*, and *JPE* written by authors from the top eight schools [suggest] an increase in school-level concentration, both between the 1970s and the 1980s and between the 1980s and the 1990s.” A difficulty is that Ellison’s sample excluded comments, replies and rejoinders. The reliability of a number of Ellison’s results is undermined by this sampling bias (because it is

An editorial posture that eschews critical commentary subjugates the spirit of scientific inquiry. As Michael T. Ghiselin observes, “Error is eternal, and wisdom consists in living with it, not letting our vanity tell us that it has been transcended” (1974, 13).

[Appendix 1:
Link to the Excel file containing
the complete data and figures.](#)

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highly unlikely that the population of authors writing comments, replies and rejoinders has the same representation at top eight schools as the population of authors writing full articles).

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CHARACTER ISSUES

The Role of Economists in Ending the Draft

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[Abstract, Keywords, JEL Codes](#)

ON JANUARY 27, 1973 CONSCRIPTION OF MEN INTO THE U.S. military ended, and on June 30, 1973 the law authorizing conscription expired. Conscription, henceforth referred to as the draft, had been in existence since 1940, except for a period from March 1947 to June 1948. Richard Nixon had made ending the draft an issue during his presidential campaign against Hubert Humphrey, a strong proponent of the draft. Nixon even went so far as to devote a whole radio address to his proposal to end the draft on CBS on October 17, 1968. It was around that time that I started following the issue closely, reading extensively in the popular and academic literature about the politics, economics, and history of the draft. Despite President Nixon's apparent commitment to volunteerism, as it was then called, almost no one I talked to between 1968 and 1970 believed that the draft would end soon. A powerful coalition of southern Democrats, in both the Senate and the House of Representatives, along with some Republicans and northern Democrats, strongly supported the draft. Political opposition to the draft seemed scattered, consisting mainly of various statist and conservative Republicans and some statist Democrats. Yet, less than five years after Nixon's radio address, the draft was over.

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Thirty-two years later, it has not reappeared, despite a serious effort in 1979 to bring it back.

How did it end so quickly? I believe there are two reasons. First, 1972 was the first year in which people aged 18 to 20 were allowed to vote. President Nixon probably sensed a base of support among these new voters, and many of the males in this group would presumably appreciate not being drafted. The second and, I believe, more important reason is that economists in the mid to late 1960s did extensive work documenting the tremendous human cost of the draft and laying out a systematic and strong case against the draft and for an all-volunteer force. Economists from across the political spectrum, but especially from the classical-liberal, pro-market end, were very active in this work.

A caution to the reader: Those who share the late George Stigler's view that economists have nothing to tell policymakers that they don't already know will not, most likely, be persuaded that economists have influenced draft policy. I have no way of proving, beyond a doubt, that economists have, in fact, had such an effect. However, my contention that they have is based on my close observations of the draft debate that has taken place over many years. What follows is not a rigorous test of the effect of economists on policy, but rather a story about their involvement, along with some documentation.

One of the first empirical studies of the economics of the draft and of ending the draft was done by Walter Oi (1967a, 1967b), an economics professor then at the University of Washington and later at the University of Rochester's Graduate School of Management. In his study published in the *Sol Tax* volume (Oi 1967b), Oi distinguished clearly between the budgetary cost of military manpower and the economic cost. Oi granted the obvious, that a military of given size could be obtained with a lower budgetary cost if the government used the threat of force to get people to join—that is, used the draft. But, he noted, the hidden cost of this was the loss of well-being among draftees and draft-induced volunteers. Using some empirical methods that were sophisticated for their day, Oi estimated the loss to draftees and draft-induced volunteers and found it quite high—between \$826 million and \$1.134 billion. While this number might seem low today, Oi's data were in mid-1960s dollars. Inflation-adjusted to 2005, the losses would be \$4.8 billion to \$6.6 billion.

Oi's study was one in a long line of articles by economists who used empirical methods to estimate the labor supply of first-term enlistees. Other economists who contributed to the literature at the time were Stuart Altman (1969), the late David Bradford (1968), Alan Fechter (Altman and Fechter

1967), Anthony C. Fisher (1969), and W. Lee Hansen, and Burton Weisbrod (1967). Their articles appeared in such prestigious economics journals as the *American Economic Review* and the *Quarterly Journal of Economics*, which, in this author's view, were more open to publishing articles on important policy issues than they are today. Although it was sometimes not clear whether the economists who did the empirical studies were against the draft, their evidence made a strong case that an all-volunteer force was quite viable, even in the midst of a fairly intense war such as the Vietnam War. One had the feeling also that the economists who wrote about it were, to a man, against the draft.

In my opinion, just as important as the academic-journal articles, were the activities by economists making the case against the draft to a general audience. The most famous economist in this category was Milton Friedman. In December 1966, various prominent and less-prominent academics, politicians, and activists were invited to a four-day conference at the University of Chicago. Papers were commissioned and the people who wrote them gave summaries, after which the discussion was open to all. Fortunately, the discussion was transcribed. The papers and discussions appear in the Sol Tax volume referenced earlier, which came out the following year. The invitees included two young anti-draft Congressmen, Robert Kastenmeier (D-Wisconsin) and Donald Rumsfeld (R-Illinois), and one pro-draft Senator, Edward Kennedy (D-Massachusetts). Also attending were pro-draft anthropologist Margaret Mead and anti-draft economists Milton Friedman and Walter Oi. Friedman gave the general economic and philosophical case for a voluntary military in his presentation, "Why Not a Voluntary Army?" Reading through the whole Sol Tax volume, with all the papers and transcripts of the discussion, I had the sense that there was a coalescing of views over the four-day conference, as people from various parts of the ideological spectrum found that they shared a strong antipathy to the draft and that the economists had a surprisingly strong economic case against it. Friedman's speech and his various comments at the conference still make compelling reading. One of his best rhetorical flourishes was his criticism of the charge that those who advocate ending the draft are advocating a "mercenary" army. Friedman said:

Now, when anybody starts talking about this [an all-volunteer force] he immediately shifts language. My army is "volunteer," your army is "professional," and the enemy's army is "mercenary." All these three words mean exactly the same thing. I am a volunteer professor, I am a

mercenary professor, and I am a professional professor. And all you people around here are mercenary professional people. And I trust you realize that. It's always a puzzle to me why people should think that the term "mercenary" somehow has a negative connotation. I remind you of that wonderful quotation of Adam Smith when he said, "You do not owe your daily bread to the benevolence of the baker, but to his proper regard for his own interest." And this is much more broadly based. In fact, I think mercenary motives are among the least unattractive that we have. (quoted in Tax 1967, 366)

Next to this passage in my dog-eared version of the Sol Tax book, which I purchased over 30 years ago, I wrote one word: "Wow!" This is rhetoric at its best, a tight argument passionately stated.

Two of Friedman's comments about this conference are worth noting. Writing some 30 years later, Friedman noted that the 74 invited participants "included essentially everyone who had written or spoken at all extensively on either side of the controversy about the draft, as well as a number of students" (Friedman and Friedman 1998, 377). Friedman's other comment is worth citing:

I have attended many conferences. I have never attended any other that had so dramatic effect on the participants. A straw poll taken at the outset of the conference recorded two-thirds of the participants in favor of the draft; a similar poll at the end, two-thirds opposed. I believe that this conference was the key event that started the ball rolling decisively toward ending the draft. (Friedman and Friedman 1998, 378)

Other economists were also making the philosophic and economic case for ending the draft. One notable group was a group of students and newly-minted graduates from the University of Virginia Ph.D. program in economics. They were studying, or had studied, under James Buchanan, Gordon Tullock, the late G. Warren Nutter, Leland Yeager, and other notable economists at that university at the time. This group included James C. Miller III, later the chairman of the Federal Trade Commission and director of the Office of Management and Budget, Mark V. Pauly, now a prominent health economist at Wharton, and Robert Tollison and Cotton

M. Lindsay, two well-published economists at Clemson University.¹ This young group of economists took the initiative of putting together a book of essays, *Why the Draft? The Case for the Volunteer Army* (1968). This was one of the first books on the draft that I read. I picked it up at a used book store in Winnipeg in the late 1960s, read it on the bus to and from the University of Winnipeg, and came away realizing that the U.S. tradition was one of volunteerism rather than conscription, and also understanding, for the first time, the strong economic arguments against the draft. The book, edited by James C. Miller III, had a preface written by Edward Brooke, at the time a Republican U.S. Senator from Massachusetts. Miller told me once that the book sold out its 30,000-copy print run and that Penguin, the publisher, dragged its heels and never printed more, despite the obvious demand.

Another important group that worked on the nuts and bolts of the draft, and of the transition to an all-volunteer force, consisted of the various economists working for Nixon's Gates Commission—that is, the President's Advisory Commission on an All-Volunteer Force. Usually when presidents appoint commissions, they do so for one of two reasons: (1) to “punt” on a controversial issue by buying time and hoping it will go away or (2) to get a blue-ribbon group to come out in favor of something on which there is a consensus among experts, but on which the public is divided. But President Nixon's commission fit neither of these. First, had Nixon been trying to punt, he would not have formed the commission on March 27, 1969, less than three months after his inauguration. Second, there was not a consensus among the experts: there was a division of views within academia, within the military, and within the political establishment. As Milton Friedman, one of the 15 members of the Commission noted, the members of the Commission were evenly divided: five were avowed proponents of the draft; five were avowed opponents; and five were uncommitted.² This division of views came as a surprise to William H.

¹ The other two were Thomas D. Willett, later a deputy assistant secretary of the U.S. Treasury and now an economist at Claremont and Joseph M. Scolnick, Jr., a political scientist.

² The members were Chairman Thomas S. Gates, Jr., former secretary of defense; Alfred Gruenther, former supreme Allied commander, Europe; Lauris Norstad, former supreme Allied commander, Europe; W. Allen Wallis, an economist and president of the University of Rochester; economist Alan Greenspan; Milton Friedman; Thomas Curtis, former Republican congressman from Missouri; Frederick Dent, president of Mayfair Mills; Crawford Greenewalt, chairman of the finance committee of E.I. DuPont de Nemours and Co.; Stephen Herbets, a student at Georgetown University Law Center; Theodore Hesburgh, president of the University of Notre Dame and chairman of the U.S. Commission on Civil Rights; Jerome Holland, president of the Hampton Institute; John Kemper, headmaster of

Meckling, the executive director of the Gates Commission and an economist who was dean of the University of Rochester's Graduate School of Management. In a speech in early 1979 in which he reminisced briefly about his time on the Gates Commission, Meckling said, "I thought I would be estimatin' supply curves."³ In other words, Meckling thought he would be planning how to end conscription. His thought was understandable, as President Nixon's charge to the Commission read, in part, as follows:

I have directed the Commission to develop a comprehensive plan for eliminating conscription and moving toward an all-volunteer armed force. The Commission will study a broad range of possibilities for increasing the supply of volunteers for service, including increased pay, benefits, recruitment incentives and other practicable measures to make military careers more attractive to young men. (Nixon's statement of March 27, 1969.)

Instead, Meckling found himself not just overseeing a large research project, but also having to make the case with some Commission members for getting rid of the draft. Meckling did the job well, and one of his favorite passages of the Commission's report—which he wrote and enjoyed quoting—was the opening paragraphs of Chapter 3, which was titled, "Conscription is a Tax." Here are those paragraphs:

Any government has essentially two ways of accomplishing an objective whether it be building an interstate highway system or raising an army. It can expropriate the required tools and compel construction men and others to work until the job is finished or it can purchase the goods and manpower necessary to complete the job. Under the first alternative, only the persons who own the property seized or who render compulsory services are required to bear the expense of building the highway or housing project. They

the Phillips Academy; Jeanne Nobel, a professor at New York University and vice-president of the National Council of Negro Women; and Ray Wilkins, executive director of the NAACP.

³ Meckling, who was my boss at the time, loved to drop his "g's" at the end of "ing" words.

pay a tax to finance the project, albeit a tax-in-kind. Under the second alternative, the cost of the necessary goods and services is borne by the general public through taxes raised to finance the project.

Conscription is like the first alternative—a tax-in-kind. A mixed force of volunteers and conscripts contains first-term servicemen of three types—(1) draftees, (2) draft-induced volunteers, and (3) true volunteers. Draftees and draft-induced volunteers in such a force are coerced into serving at levels of compensation below what would be required to induce them to volunteer. They are, in short, underpaid. This underpayment is a form of taxation. Over 200 years ago, Benjamin Franklin, in commenting on a judicial opinion concerning the legality of impressments of American merchant seamen, recognized the heart of the issue, and even estimated the hidden tax. He wrote:

“But if, as I suppose is often case, the sailor who is pressed and obliged to serve for the defence of this trade at the rate of 25s. a month, could have £3.15s, in the merchant’s service, you take from him 50s. a month; and if you have 100,000 in your service, you rob that honest part of society and their poor families of £250,000. per month, or three millions a year, and at the same time oblige them to hazard their lives in fighting for the defence of your trade; to the defence of which all ought indeed to contribute, (and sailors among the rest) in proportion to their profits by it; but this three millions is more than their share, if they did not pay with their persons; and when you force that, methinks you should excuse the other.

“But it may be said, to give the king’s seamen merchant’s wages would cost the nation too much, and call for more taxes. The question then will amount to this; whether it be just in a community, that the richer part should compel the poorer to fight for them and their properties for such wages as they think fit to allow, and punish them if they refuse? Our author tells us it is *legal*. I have not law enough to dispute his authority, but I cannot persuade myself it is

equitable.” (Report of the President’s Commission 1970, 23-24)

During its ten months of work, the Gates Commission hired a number of economists to estimate supply curves for officers and enlistees, the effects of bonuses on retention, the effect of various factors on reenlistments, determinants of labor turnover costs in the military, the size of the conscription “tax,” and productivity of the U.S. military recruiting system, to name a few. The roster of people working on the Commission studies included Walter Y. Oi, Robert J. Barro, Larry A. Sjastaad, Harry Gilman, Alan Fechter, Stuart Altman, Ronald N. Hansen, and J. Huston McCulloch. The four directors of research for the whole project were Stuart Altman, Harry J. Gilman, David Kassing, and Walter Y. Oi. The three consulting organizations involved in the project were the Center for Naval Analyses, the Institute for Defense Analyses, and the Rand Corporation.

The Commission also examined non-economic issues. Excellent studies on the U.S. historical experience with volunteerism and the draft (done by John L. Rafuse) and on conscription and constitutional law (by David M. Stigler) are among the non-economic issues written up in a logical and compelling way. After more than 30 years of having read dozens of government reports and the studies behind them, I can safely say that the quality of these studies is about the highest I’ve seen. That’s a tribute both to the economists and others who did the studies and to the four economists who supervised them.

So, as you can see, Meckling was not completely off the mark in thinking that he would be estimating supply curves. But he also engaged in debate. I was not able to interview Meckling for this article, but I did interview his widow, Becky Meckling, a few months after his death in 1998.⁴ One debate from her husband’s days stood out in her mind—one, she said, that had delighted Bill. The Commission had held hearings and what had Meckling beaming, she said, was an exchange between Milton Friedman and one of the witnesses, General William Westmoreland, who had been the commander of the troops in Vietnam and was, at the time, Chief of Staff of the U.S. Army. I had heard Bill tell this story a few times, and the day stood out in Milton Friedman’s mind too. Friedman reports the dialogue in his memoirs, *Two Lucky People*, co-authored with his wife, Rose:

⁴ I got to know Bill and Becky Meckling in the late 1970s, when Bill was the dean of the business school at the University of Rochester and I was an assistant professor there.

In the course of his testimony, he made the statement that he did not want to command an army of mercenaries. I stopped him and said, 'General, would you rather command an army of slaves?' He drew himself up and said, 'I don't like to hear our patriotic draftees referred to as slaves.' I replied, 'I don't like to hear our patriotic volunteers referred to as mercenaries.' But I went on to say, 'If they are mercenaries, then I, sir, am a mercenary professor, and you, sir, are a mercenary general; we are served by mercenary physicians, we use a mercenary lawyer, and we get our meat from a mercenary butcher.' That was the last that we heard from the general about mercenaries. (Friedman and Friedman 1998, 380).

In those same memoirs, Friedman reminisced about the Gates commission's meetings and effectiveness:

Tom Gates was a splendid, open-minded, even-handed chairman, who gradually shifted his position to become a convinced supporter of an all-volunteer army. The same thing happened to the other two men from the military, Al Gruenther and Lauris Norstad. Though evenly split at the outset, we ended by submitting a unanimous report, save only for Roy Wilkins, who was prevented by illness from participating in the last three months of our proceedings and therefore decided to abstain. (Friedman and Friedman 1998, 379)

One economist particularly worth noting is Martin Anderson, a long-time senior fellow at the Hoover Institution, who joined Richard Nixon's campaign for president in 1967. Anderson wrote the anti-draft speech that Nixon gave on CBS radio during the 1968 election. As an adviser to President Nixon, Anderson helped choose the members of Nixon's commission on the all-volunteer force. My educated guess is that without Anderson, the Commission would not have happened. And I'm quite confident that without Anderson, it would not have had, as members, so many prestigious economists.

One other economist worth noting, both for his work on the Commission and for his public opposition to the draft, is the aforementioned Allen Wallis. Wallis had been a top adviser to President Eisenhower and was later to be the Undersecretary of State for Economic

Affairs under President Reagan's Secretary of State, George P. Shultz. On November 11, 1968, while president of the University of Rochester, Wallis was invited to give a speech in Rochester to the Monroe County branch of the American Legion on the occasion of its 50th anniversary. His speech is a model of how to approach an audience when you have an important, controversial message to give and you think your audience might disagree with you. Wallis spent the first half of the speech connecting with his audience, telling them his reminiscences of being a young child at the end of World War I, telling them his thoughts about war and foreign policy, and generally assuring them that although he wished for peace, he was not a pacifist. Then he turned on a dime, saying:

There is one measure we can take and should take immediately that would do much to resolve the dilemma that arises because, on the one horn, one of America's most fundamental—and also most admirable—characteristics is repugnance for war and, on the other horn, the ability to wage war is essential to the preservation of freedom.

The measure I propose will, I fear, shock some of you. I respectfully request that you nevertheless hear me out and think over my proposal carefully, rather than reject it out of hand. It is not a view that I have come to lightly or recently, but one I have held for over twenty years. It is not original with me, nor is it without strong support from many respectable citizens of unquestionable patriotism.

A step that would do much toward resolving our dilemma is to abolish the draft—abolish it completely, lock, stock, and barrel; abolish it immediately, with no ifs, ands, or buts. (Wallis 1976, 46)

Wallis went on to advocate getting rid of the draft within 90 days of incoming President Nixon's inauguration. He spent the last half of the speech laying out the various arguments against the draft. He also dealt with the "mercenary" issue that Milton Friedman was later to address, quoting his labor economist colleague Harry J. Gilman. Gilman had asked, "Why . . . are officers who are encouraged to enter and to remain in the service by reasonably high levels of pay called 'dedicated career men' but privates who

would volunteer when they too received higher levels of pay called ‘mercenaries?’” Wallis’s speech, incidentally, was covered positively in *The Nation*, a major left-wing publication (The Nation 1968).

When I read the speech in 1976, I wondered how the audience had reacted, but I didn’t get my answer until a day in 1979, when I had lunch with Wallis. I expected him to say, “I got some polite applause.” Polite applause would have been a victory with such an explosive issue in 1968, the peak of the Vietnam War. Instead, he answered that he had received a standing ovation. Wallis later gave me a copy of a letter he had written to a Republican friend, John A. Perkins, telling of the reaction. The following are excerpts from that letter:

You ask about the reception [given the speech.] It was considerably better than I anticipated. I am not sure that “standing ovation” is quite accurate as a description of their response, but it is a fact that they all stood up while they clapped. The chairman of the meeting turned to me and said, “I agree with you completely: I have sons.” One member of the audience shook my hand as I was leaving and said, “I admire your courage!” Two Negroes stopped me, clasped my hands warmly, looked deep in my eyes for more than five but less than ten seconds, seemed to convey a feeling of deep emotion, but said scarcely anything. (Wallis 1969)

Whenever the military has had trouble recruiting, advocates of the draft, such as Northwestern University sociologist Charles Moskos, come out in favor of bringing back the draft. When they start to push for the draft, the front line of defense against them is still economists. This was evidenced in 1979 and 1980, when high inflation caused a serious drop in real pay and consequent increase in difficulty meeting recruiting quotas. Of all the threats to bring back the draft in the last 32 years, the threat in 1979-1980 was the most serious. Senator Sam Nunn (D-Georgia) held hearings with the goal of building support for the draft and, at least, registration for a future draft. Various economists fought the good fight against this move, including Stanley Horowitz, Sam Kleinman, and Chris Jehn, then economists at the Center for Naval Analyses, Lee Mairs, a Navy Commander, at the time the chief manpower economist for the U.S. Navy, and John T. Warner, an economics professor at Clemson University. Martin Anderson organized an important conference on the draft at the Hoover

Institution in November 1979 and invited the top proponents and opponents of the draft (Anderson 1982). What impresses me about literally all of them is their moral certitude in opposing the draft, and that none of them opposed the draft just because they happened to be vulnerable to it themselves or had a son who would be. Some of them, such as Hoover's Martin Anderson, have no children. Chris Jehn, an assistant secretary of defense in the first Bush administration, later an associate director of the Congressional Budget Office, and now a vice-president at Cray Inc., has only a daughter. Walter Oi has two daughters.

Also, many other economists signed the following "Economists' Statement in Opposition to the Draft," which I wrote and circulated in 1980:

We, the undersigned, oppose moves toward the reimposition of the draft. The draft would be a more costly way of maintaining the military than an all-volunteer force. Those who claim that a draft costs less than a volunteer military cite as a savings the lower wages that the government can get away with paying draftees. But they leave out the burden imposed on the draftees themselves. Since a draft would force many young people to delay or forego entirely other activities valuable to them and to the rest of society, the real cost of military manpower would be substantially more than the wages draftees would be paid. Saying that a draft would reduce the cost of the military is like saying that the pyramids were cheap because they were built with slave labor. (Economists' Statement 1981, 2)

The statement was published as a full-page ad in *Libertarian Review*, *Inquiry*, and *The Progressive*. Among the prominent economists who signed were Kenneth Boulding, Harold Demsetz, Milton Friedman, Alan Greenspan, David Friedman, Donald McCloskey, William Meckling, Allen H. Meltzer, James C. Miller III, William A. Niskanen, Mancur Olson, Sam Peltzman, Murray Rothbard, Jeremy J. Siegel, Vernon Smith, Beryl W. Sprinkel, Jerome Stein, and James L. Sweeney.

In the years since 1980, whenever the draft issue has arisen, economists have been there to fight it. Such economists include Paul Hogan, a specialist in military manpower who has worked for various

manpower research groups, Steven Cylke, an economist with the U.S. Navy, and the aforementioned Chris Jehn and John Warner.

What can we learn from the push by economists to end the draft? Public-choice economists emphasize the role of special interests, and their views should not be dismissed lightly. But ideas and ideals matter, too. Before the draft was eliminated, most Americans had trouble imagining a country without one, given that it had been in existence for 31 of the previous 33 years. And yet we have now gone without a draft for 32 years. Ironically, one of the people who had trouble believing that ideas would conquer special interests was the late William Meckling. In one of the last lengthy conversations I had with him, in 1979, Meckling was playing “ain’t it awful,” lamenting what he saw as the inevitable decline of liberty. Although I agreed with him about his facts, I argued that because of good, intellectually powerful people like him taking action in the public arena, the decline was not inevitable.

Meckling: What do you mean?

Henderson: Look back at your whole life’s work since you became an economist. What were the two policy issues on which you weighed in and spent substantial time?

Meckling: Well, that would be in the late 1950s and early 1960s, when I wanted to make sure the federal government didn’t get a monopoly on satellites in space and in the late 1960s, when I was with the Gates Commission.

Henderson: And what happened?

Meckling (looking sheepish): Well, the government did allow commercial satellites and we got rid of the draft.

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David R. Henderson is an associate professor of economics in the Graduate School of Business and Public Policy, Naval Postgraduate School in Monterey, California and a research fellow with the Hoover Institution. He was previously a senior economist with President Reagan's Council of Economic Advisers. Henderson is the editor of *The Fortune Encyclopedia of Economics* (now *The Concise Encyclopedia of Economics*), the first, and still the only, economics encyclopedia aimed at a lay audience. He also wrote *The Joy of Freedom: An Economist's Odyssey* and is co-author of the forthcoming book, *Making Great Decisions in Business and Life*. Besides publishing in academic journals, Henderson has published over 100 articles in the *Wall Street Journal*, *Fortune*, *Red Herring*, the *New York Times*, *Los Angeles Times*, *Christian Science Monitor*, *San Francisco Chronicle*, *Chicago Tribune*, and *Reason*. He has appeared on *The O'Reilly Factor*, the *Jim Lehrer Newshour*, and *CNN*.



CORRESPONDENCE

Editors,

A recent paper of Binmore appears to contain a fundamental logical error.

At the beginning of his contribution to the recent symposium on information and knowledge, Binmore ([Why the Distinction Between Knowledge and Belief Might Matter EJW April 2005](#)) adduces the following motivating example:

Alice is a perfectly rational decision-maker who values her own safety. She therefore won't step in front of a car when crossing the road. I am so sure of my facts that I attribute probability one to this assertion. But what was my reasoning process in coming to this conclusion? I have to contemplate Alice comparing the consequences of stepping in front of a car with staying on the kerb. But how can Alice or I evaluate the implications of the former event, which we know is impossible? In mathematical logic, anything whatever can be deduced from a contradiction.

The entire seven-page article ensues in this spirit.

With all our respect and admiration for Ken Binmore, we are dumbfounded by his analysis. The assertion in question is, "Alice won't step in front of a car when crossing the road." Let's call this p . Binmore became convinced of p by a reasoning process involving several elements, including Alice's concern for her safety. He then asks, "What was my reasoning in coming to this conclusion?" That is, he wishes to review the reasoning leading to p . To do so he—and Alice—contemplate the consequences of $\neg p$ (the negation of p); namely, that she does step in front of a car. Considering the consequences of $\neg p$ in seeking to establish p is a universally accepted

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procedure both in everyday thought and in formal logic; it is called reasoning “per absurdum,” or “indirectly.” So far, so good.

Now Binmore asks, “How can Alice or I evaluate the implications of the former event [i.e. $\neg p$], which we know is impossible? In mathematical logic, anything whatever can be deduced from a contradiction.” But Ken, that you know $\neg p$ to be impossible is only because you previously convinced yourself of p . You are now reviewing the reasoning leading to that previous conclusion, i.e., to p . In this review, surely you cannot assume p itself—you cannot assume what you wish to prove!

Robert Aumann
Hebrew University of Jerusalem

KEN BINMORE RESPONDS:

Dear Bob,

I don’t think you will find many takers for the claim that it is a logical error to say that the statements:

Alice never acts irrationally
Alice has acted irrationally

contradict each other. I guess that you mean that these statements should not be allowed to arise simultaneously when working out what is rational for Alice. However, *after* you have worked out what is rational for Alice in some situation—perhaps using your own favored definition of rationality—then doubtless you do agree that it becomes meaningful to pose the counterfactual:

What if a perfectly rational Alice were to act irrationally?

Such counterfactuals seem to me of the essence in discussions of backward induction, because what keeps rational players on the backward-induction path is their prediction on what would happen if someone were to stray. You defend a theory in which such counterfactuals don’t need to be interpreted in considering what common knowledge of rationality implies—

and which I think therefore can't possibly be right (Binmore 1997).

My own position is that it is not possible to say what is rational in a game without specifying how it makes sense to interpret the relevant counterfactuals. In the example given in my note, I differ from Reinhard Selten (1982) on how best to interpret counterfactuals only in allowing my "trembles" to be correlated with each other. In such cases, it is easy to see that rational play need not always follow the backward-induction path. If one makes the trembles independent of each other, then rational play will always follow the backward-induction path.

I do not like the conclusion that rational play depends on the context in which a game is being played any more than you, but at least it makes game theory a more interesting subject.

Ken Binmore
University College London

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R. Selten and U. Leopold. 1982. Subjunctive Conditionals in Decision Theory and Game Theory. *Studies in Economics: Volume 2 of Philosophy of Economics*, ed. Stegmüller, Balzer, and Spohn. Berlin: Springer-Verlag.

Editors,

Regarding Warren Gibson's thought-provoking piece ([The Mathematical Romance: An Engineer's View of Mathematical Economics, EJW April 2005](#)):

At the end of his significant contribution, Gibson asks: "What if real answers to urgent problems could be delivered in plain English?"

But if mathematical economics (particularly, the model-building side) is a science like any other, isn't its mathematical nature part of its scientific character? It follows that model-building economics is to be assessed in this respect with the other sciences. So the following counter-questions arise:

1. Which genuine science uses plain English? All the sciences we know use mathematics at their core. Why should economics alone be different?
2. All the other sciences are significantly mathematical *and* highly successful. Gibson, however, feels that economics—*because* it is mathematical—does *not* discuss serious problems! There's something wrong here.
3. Furthermore: The overwhelming majority of economists are there because they wish to pursue a science. Again, how can a science be conducted in plain English? To be scientific is to be mathematical.

In sum: those who complain about mathematical modeling, etc. in economics should explain how a science can be *non*-mathematical.

Of course, one could argue, that economics is not a science. But then it could not be neoclassical economics—it would have to be something else.

Sudha R. Shenoy
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Editors,

Three remarks about Daniel Klein's [The PhD Circle in Academic Economics \(EJW April 2005\)](#): First, the data on Chicago contains at least one mistake—Szentes is from Boston University, not Boston College. Second, it would probably be best to exclude emeritus, and other very elderly profs, because I believe that PhD granting was more concentrated 50 years ago, in large part because the market scale was so much smaller. On the other hand, and third, I have done this kind of analysis for just Asst profs (my concern was quantifying Chicago's placement), and found that Harvard, MIT, and

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Chicago PhDs (all 3 in about equal numbers) were sitting in most of the Asst Prof spots at top 15 depts, so maybe your results would be similar if you use only Asst profs.

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Department of Economics
University of Chicago

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