

FREE BANKING IN SCOTLAND: REPLY TO A DISSENTING VIEW

Lawrence H. White

I appreciate Larry J. Sechrest's critical attention to the historical record of free banking in Scotland, and to my 1984 book in particular. He has identified some issues that bear further study. I fear, however, that many of his statements confuse rather than clarify matters.

My book advanced the thesis, certainly not original with me, that Scottish banking experience from 1716 to 1844 provides important evidence on the features and behavior of a "free banking" system. In the academic discussion of free banking over the last few years, several economists including Sechrest have criticized or questioned aspects of the thesis that Scotland had a reasonably close approximation to free banking. I have replied to a number of these criticisms in two recent pieces (White 1990, 1991).

I am pleased to see that my 1991 piece has persuaded Sechrest to withdraw his earlier argument (Sechrest 1988, p. 255) that the Bank of England acted as a central bank for the Scottish banks. I am disappointed that Sechrest finds it necessary to repeat in the *Cato Journal* several of the other arguments he and other critics have made elsewhere. Evidently I have not yet answered these remaining criticisms persuasively enough, or acknowledged them frankly enough, to satisfy Sechrest. Nonetheless I refer the interested reader to my previous replies for a broader defense of the thesis. I apologize for the repetition necessary in what follows.

Sechrest quotes my earlier statement (White 1984, p. 137) that Scottish experience "provides unique evidence on the workability of monetary freedom." I would no longer use the word "unique"

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The author is Associate Professor of Economics at the University of Georgia. He thanks George Selgin and Kevin Dowd for comments, and the George Edward Durell Foundation for research support. They should not be held accountable for the views expressed here.

here (see White 1991, p. 59, n. 2), because recent studies have turned up relevant evidence from the experiences of several other countries. Kurt Schuler (1991) counts Scotland as one of some 27 known historical cases of plural note-issue subject to no more than mild legal restrictions. Thus the case for free banking no longer needs to be “intimately tied,” as Sechrest puts it, to the Scottish example. But I still contend that the Scottish case provides useful evidence on the workability of monetary freedom. That is, it represents a relatively (though not completely) unrestricted banking system, as well as a successful banking system.

Sechrest disputes both the freedom of the Scottish system and its success. He disputes its success, as he has done before (Sechrest 1988, pp. 251–52), by recomputing bank failure rates. He disputes its freedom, as he has done before (Sechrest 1988, pp. 248–51, 254–55), by raising questions about note inconvertibility, the usury ceiling, and the privileges of the chartered banks. He has withdrawn the charge that the Bank of England acted as a central bank, but has added the question, previously raised by other authors, of restrictions on small-denomination notes.

Bank Failures

I acknowledged but did not address Sechrest’s argument about bank failure rates in my 1991 piece. Given that he has reiterated the argument, I am glad to have the chance to address it here.

Let me first clear up a matter of minor importance. In a footnote Sechrest finds discrepancies between the columns of figures and the column averages given in my table of English and Scottish bank failures, 1809–30 (White 1984, p. 48). For the column of English bankruptcies, the discrepancy is easily explained. My result of 18.1 (English bank failures per year per 1,000 banks) was reached by dividing the average number of bankruptcies per year (311/22, or approximately 14.1) by the average of annual numbers of banks (17,198/22, or approximately 781.7), and multiplying by 1,000. Sechrest evidently reached his result of 17.54 by averaging the figures in my column of yearly bankruptcies-per-1,000. My procedure weights all failures equally, whereas his procedure weights less heavily a failure that took place in a year with more banks. Because more than half the English failures occurred in years with more than 781.7 banks, his result is slightly smaller. Either procedure is defensible.

The discrepancy in the Scottish figures is due to a similar difference in procedure, plus the appearance of a regrettable typographical error in my column of figures (but not in my average). The 9 (nine)

in my 1815 row should have been a 0 (zero). That is why, unlike my other non-zero figures, it is not followed by a decimal point and a digit to the right. I thank Sechrest for helping to bring this to light.

More serious than these discrepancies is Sechrest's claim that a longer time series produces nearly identical failure rates in England and Scotland. My table was limited to the years 1809–30 because I relied on Gilbert's (1837, p. 110) figures, which were limited to those years. Sechrest proposes extending the series to earlier years, and recomputes the average annual failure rates for 1772–1830. As is indicated by his reporting unequal numbers of Scottish and English observations, Sechrest has actually been able to extend only the Scottish series as far back as 1772. His English series has no observations for years before 1784, for 1785–92 inclusive, for 1795, or for 1799.

Extending the series backwards is certainly unobjectionable in principle. But the year 1772, at which Sechrest begins his series, happens to be the year of the Ayr Bank crash, by far the most spectacular crash during the free banking period. By S. G. Checkland's count (1975, Table 2, pp. 134–35) that year saw the failure not only of the Ayr Bank, a large provincial banking company, but also the related failures of two of the "larger" private (non-issuing) bankers, and 11 of the "lesser" private bankers. In total, counting all private bankers as the equivalent of other banking firms,¹ 14 of 31 existing banks failed in 1772. Had Sechrest begun his series with 1773, he would have reported an average of 7.3 Scottish bank failures per year per 1,000 banks. By including 1772 he was able to report the result of 14.88 failures per year.

But why stop at 1772? In Sechrest's words, "making such a comparison over the longer time period surely conveys a more accurate picture." Checkland's tables make it possible to extend the data series back to 1716, the year the Bank of Scotland's statutory monopoly lapsed.² Going back to 1716, accepting Sechrest's annual figures

¹A footnote to my original table (White 1984, p. 48) stated that I excluded non-issuing banks from counts of both Scottish banks and bank failures, because non-issuing English banks were not included in the English figures taken from Gilbert. In fact, I evidently counted the failure of David Patterson in 1813, a private banker who probably did not issue notes. The footnote also explained that I had attempted to adjust the Scottish bank numbers to match the way in which English banks were (over-) counted. Sechrest's figures indicate that he has not followed me in either regard in his revisions, though he has retained most of the annual Scottish failure rates from my table as is.

²Sechrest (1988, p. 251) seems to think that I define the Scottish free banking era as beginning in 1765, but this is a misinterpretation. My book (White 1984, esp. p. 38) identifies the starting point as 1716. The earlier years of course display a less well-developed free banking system.

for 1772–1830, and using his column-averaging procedure, yields a result of 7.98 for average annual Scottish bank failures per 1,000 over the entire 1716–1830 period.³ Revising Sechrest's figures for 1809–30 to make them more consistent with Checkland lowers the result for 1716–1830 to 7.29, less than half the figure Sechrest reports for 1772–1830.⁴

Is the difference between 7.29 (Scotland 1716–1830) and 14.90 (Sechrest's figure for England 1800–30 plus six earlier years) significant? In comparing his two series for the 1772–1830 period, Sechrest concludes that "the rate for Scotland is thus *not statistically different* [his emphasis] from that for England at the 99 percent confidence level." He reports the numbers of observations and standard deviations, and uses these numbers to compute a Z statistic, on which he bases his conclusion about the difference between mean failure rates. It might seem appropriate to re-run Sechrest's test for statistical significance using the longer Scottish series just mentioned. In fact this would not be appropriate, because tests for statistical significance are simply out of place here.

The statistical method Sechrest uses is appropriate for testing the hypothesis that the means of two *random samples* with known standard deviations are equal (see Harnett and Murphy 1980, pp. 357–59;

³Checkland records several closings, but none labelled failures, before 1772.

⁴In addition to extending the time period covered, Sechrest has revised my 1809–30 series for Scotland. Though he does not detail the reasons, I believe that they can be inferred from Checkland's Tables 3 and 9 (1975, pp. 177–79, 320–21). I thank Sechrest for compelling me to check my table against Checkland's tables, thus bringing to light a few discrepancies regarding the dates of Scottish bank failures. I find, unfortunately, that there are also discrepancies between Sechrest's revised column and Checkland's tables. Sechrest has added a failure in 1810, consistent with Checkland's recording the failure of Allan and Stewart, a firm Checkland groups among lesser private bankers. Sechrest has added two failures in 1821, although Checkland records only one: the Galloway Banking Company was "wound up," which we may count as a failure; the Kilmarnock Banking Company also ended in 1821, but through merger rather than failure (Checkland 1975, p. 321; contrast also Sechrest 1988, p. 254). My column indicates a failure in 1820, though Checkland records none. I surmise that my 1820 figure was meant to reflect the one 1821 failure, as my table indicates no failure in 1821. I apologize for the misplaced figure. Sechrest does not reduce my 1820 figure to zero to accord with Checkland, however. In sum, I believe Sechrest has overcounted total failures in the 1809–30 period by two. He also has retained the erroneous "9" in my 1815 row, though Checkland records no failures in that year. Adjusting for these discrepancies by restoring the 1815 and 1821 rows to zero yields the figure of 7.29 reported in the text.

For the sake of completeness, I should also note an error of omission in Checkland's Table 9. Its list of Edinburgh private bankers lacks an entry #4 (it jumps from #3 to #5). From Table 3 (p. 177) and the text (p. 404) it is evident that Checkland meant to include Scott, Smith, Stein, and Company, a firm that ended (Checkland does not say failed) in 1812. Checkland provides no founding date.

or presumably any other introduction to statistical analysis). That is, the test tells us how likely it is that the *true* population means are equal, and that the observed difference in sample means could have been due to random sampling variations. But in the present instance we are not dealing with random samples. We have the *entire population* of annual failure-rate figures for Scotland, and thus we can observe the true population mean. For England we have the entire population of figures for 1800–30, plus a *non-random* sample of figures from earlier years.⁵

Consequently the question to ask is whether the difference between 7.29 and 14.90 is economically significant, not whether it is statistically significant. This difference looks “big” to me. The difference between 4.46 and 17.54, Sechrest’s Scottish and English averages for 1809–30, is even bigger.⁶ The second contrast is fairer, because the first draws failure rates from different sets of decades. If both systems became less failure-prone over time, it is unfair to contrast Scotland’s earlier-plus-later performance with England’s later-only performance.

The length of my discussion of this issue, I fear, puts more emphasis on raw bank failure rates than is warranted by their intrinsic importance. A bank failure may involve anywhere from large losses to zero losses for bank customers (note and deposit holders), and anywhere from large losses to small losses for bank shareholders. Most Scottish bank failures (including even the Ayr Bank failure) involved zero losses for customers because shareholders had unlimited liability, and the number of shareholders (or partners) was not restricted. In England the number of bank shareholders was limited to six before 1826. Total losses per capita from all bank failures appear to have been *much* smaller for Scottish than for English bank customers in the years before 1844 (White 1984, p. 41, citing Aytoun 1844, p. 678). An effort to better measure *this* difference between the two systems would be genuinely useful.

Convertibility

As I have said elsewhere (White 1991, p. 60, n. 9), I am troubled by Checkland’s and Fetter’s statements to the effect that Scottish

⁵Furthermore, even if the failure-rate data had represented random samples from larger populations, they are not normally distributed around their means, as would be required for valid use of the significance test Sechrest performs. Most of the Scottish figures are zeros, and none in either series is less than zero.

⁶Using unadjusted Scottish bank numbers, Scotland had 8.8 bank failures per thousand banks per year over the period 1809–30.

banks discouraged or rebuffed customers who requested specie. I would have thought that one of the ways banks competed for note-holding customers was to make notes easier to redeem. I had assumed that customers were sufficiently concerned about easy specie redeemability that providing it in any amount without hesitation was a cost-effective way of attracting customers. If Checkland's statements are correct, then one must question this assumption.⁷ Perhaps other qualities, no more costly to provide, were more important to Scottish bank customers than the ability to redeem unusually large batches of notes for specie without question or hassle.

Alternatively, one might question (as Sechrest apparently does) the premise that the banks were competing. I would find this premise much harder to discard, because there is ample evidence of vigorous competition among the banks (White 1991, pp. 45–47; 1990, pp. 530–31). Checkland (1975, p. 185), in a sentence just before the longer passage that Sechrest quotes, states that “the banks separately, and *in competition with one another* [emphasis added], had developed the means of maximising their issues and minimising the specie they paid out.” Competitive bidding for loans and deposits kept the spread between loan and deposit rates commonly as narrow as the 1 percent spreads earned by money market funds today, and sometimes even narrower. Checkland (1975, pp. 384–85) comments that “banking in Scotland [before 1850] had traditionally involved competition in terms of both rates. Between 1810 and 1828, profits on deposits had seldom exceeded 1%, the difference between an average deposit rate of 3% and a lending rate of 4%, or between respective rates of 4% and 5%.” Charles W. Munn (1981, pp. 147–48) adds:

Throughout the 18th century and into the 19th the interest allowed on deposits of all kinds was 4 per cent, i.e. 1 per cent below that charged on advances. Some companies, notably the Thistle Bank, were of the opinion that this was an insufficient margin to afford them an adequate profit. They were nevertheless aware that unilateral action on their part to change the rate would almost certainly lead to an outflow of deposits to banks which kept the old rates. . . . Graham of the Thistle Bank did not give up hope of [collusive] action to reduce deposit rates but nothing came of his endeavours.

I cannot imagine how these facts could be reconciled with any thesis other than the thesis that Scotland had a competitive banking

⁷Unfortunately the late Professor Checkland cannot now tell us what evidence he had for the statements Sechrest quotes. His book does not specifically cite sources for them. Checkland (1975, p. 184, n. 3), following a paragraph concerning a number of policies banks used in competing for shares of the note circulation, does cite “Sundry Memorandums, September 1789, *Innes of Stowe Papers*, SRO.” I have not been able to consult these papers.

industry. I will return to this issue below in the section on privileged banks.

Sechrest finds curious my citation of Kevin Dowd in rebuttal to the claim that Scottish notes were not in practice redeemable on demand. Dowd (1989, p. 155) quotes with emphasis the same sentence in Fetter (1978, p. 122) that Sechrest quotes: "Redemption in London drafts was the usual form of paying noteholders." This sentence means that, even accepting Checkland's statements at face value, the Scottish notes *were* still redeemable on demand. The usual redemption medium was drafts on London, rather than specie. Attenuation of *specie* redeemability, supposing that it can be documented, does not equal lack of *any* redeemability. Notes and deposits were still debt claims redeemable for a reserve money, banks still faced adverse clearings, and marginal liquidity costs associated with holding reserves still constrained overissues.

Small-Denomination Notes

Sechrest brings up the Act of 1765 almost as if to suggest that I was unaware of it. Yet he clearly knows I have discussed it, for earlier he quotes my statement (White 1984, p. 30) that the act "left Scotland with free banking." He neglects to quote the qualifying clause that makes up the rest of the sentence: "though it raised an entry barrier against very small-scale banks of issue." His repeating the entry-barrier point does not pose any difficulty for my analysis. I recognize that Scotland did not have absolutely pure *laissez faire*. As my 1991 piece discusses in more detail, the free banking thesis does not require such a fact not in evidence. It only requires that the legal restrictions in place were relatively mild, did not prevent effective competition, and did not seriously distort institutional development. The small-note ban put in place in 1765 did not appreciably slow the pace of entry into the Scottish banking industry.

Though I am not averse to ascribing negative consequences to the ban on small notes, I frankly find it difficult to make much sense of Sechrest's hypothesis that the small-note ban promoted inflationary overissues. (Many proponents of such a ban have believed that it would have the opposite effect.) By lopping off the quickest-turnover notes at the bottom end of the spectrum, the ban may well have raised the circulation period averaged over the entire spectrum of denominations, but that hardly seems relevant. The question is whether banks were now more able to overissue the denominations of notes they could still issue. I do not see that the small-note ban would have raised the average period of circulation, and thereby

attenuated the reflux of £1, £5, or other denominations of notes remaining in circulation.

To support the hypothesis that Scotland suffered an inflationary monetary expansion after the Act of 1765 banned small notes, Sechrest quotes Adam Smith's (1981, p. 302) statement that in Scotland "the circulation has frequently been overstocked with paper money." The text surrounding Smith's statement, however, in no way indicates that Smith was describing events *after* 1765. Sechrest's attempt to invoke Smith's authority here is rather ironic. Smith (1981, pp. 322–24) *defended* the small-note ban (as a paternalistic measure to protect poor people from possible defaults by "beggarly bankers"). At the same time Smith (1981, p. 324) denied, and provided direct evidence against, the hypothesis that Scotland had suffered price inflation on account of an overissue of banknotes: "The proportion between the price of provisions in Scotland and that in England, is the same now as before the great multiplication of banking companies in Scotland." To support his hypothesis Sechrest would have to do more than cite the behavior of selected Scottish prices. He would have to show that Scottish prices were not tracking world prices.

Interest Rate Ceilings

My 1984 book neglected to discuss the possible effect of the usury ceiling on Scottish banks. I thank Sechrest for pointing this out, and for citing some evidence regarding when the ceiling might have been binding. The evidence he cites seems to indicate that, apart from the 1797–1819 Restriction period, the ceiling was seldom binding. If so, then my omission was perhaps not all that serious. Sechrest reports that British consol rates were above the 5 percent ceiling on other rates in only three years before the Restriction period (1781, 1782, 1784). He also reports that short-term rates were usually above 5 percent in 1836–41, but notes that this was after the usury ceiling had been removed from bills of exchange and promissory notes.

On the other hand, we have the statements of Munn and Checkland, quoted above, that Scottish banks in many periods charged the legal maximum rate on advances (see also Checkland 1975, pp. 184, 192). The ceiling may often have constrained the banks in those periods, forcing them to engage in nonprice rationing of credit, though we do not know just how often or by how much.⁸ I am not

⁸Checkland (1975, p. 192) comments that the ceiling was binding "when, in times of pressure, interest rates on deposits rose in Scotland, [because] a bank had no means of compensating by raising the cost of borrowing to its customers beyond 5%, but was thus obliged to accept a diminished net income." Checkland calls this effect, by the way, "seriously *inhibitive*," not "seriously *prohibitive*" as Sechrest renders it. The same slight misquotation occurs elsewhere (Sechrest 1988, p. 254).

aware of any evidence showing that the ceiling seriously distorted banking institutions. Further study of this question may be warranted.

Privileged Banks

Finally, Sechrest reiterates points he (1988, pp. 250–51) and others (Carr and Mathewson 1988; Carr, Glied, and Mathewson 1989; Cowen and Kroszner 1989) have elsewhere made concerning the privileges of the chartered banks. My earlier pieces respond at length (White 1990, 1991). To summarize my reply: yes, there were deviations from *laissez faire*. The chartered banks did receive some rents worth protecting because only their notes were accepted for customs duties. The limitation of new entrants to unlimited liability might in principle have been a binding legal constraint. But neither item seems to have been an important limitation on competition in practice.

As Sechrest recognizes, unlimited liability did not prevent entry. Before 1810 the chartered banks were larger than the newer banks, perhaps because a charter provided a cost advantage in raising capital. Even so the chartered banks competed among themselves, and their branches competed with the provincial banking companies. After 1810, the year in which the Commercial Bank of Scotland entered, the size difference disappeared. Unlimited liability did not prevent large joint-stock banks from entering and surviving in the industry on a scale equivalent to that of the chartered banks. The three chartered Scottish banks clearly faced strong head-to-head competition from other banks after 1810. It is for this reason that my 1991 reply-to-critics piece emphasizes the 1810–1844 period, to answer the pointed question Sechrest raises in his footnote 3. I do not mean to exclude the earlier period, though I am willing to grant that the privileges of the three chartered banks may have affected the relative sizes of banks during the earlier period.

Competition among the chartered and joint-stock banks was vigorous in all important aspects of banking: note-issuing, deposit-taking, lending and discounting, letters of credit, inland exchange. Here the evidence on narrow interest spreads, and the inability to collude successfully, is again relevant.

Conclusion

Sechrest has pointed out, as others have, that Scottish banking before 1844 was not absolutely and purely free of legal restrictions. Of course, my book did not claim that it was. The question, then, is

whether each of the restrictions present was severe or mild, i.e., whether its impact was seriously distortive or not. I share what I take to be Sechrest's concern that we should not draw general conclusions, concerning the practical features of free banking systems, from any aspects of the Scottish system that were seriously distorted by legal restrictions. I do not believe that he has actually shown any serious distortions. He has identified some relevant areas for inquiry, particularly with regard to the impact of the usury law. His claim that the failure rate for Scottish banks "was not lower than that for English banks" does not hold up, especially when we compare rates across the same decades.

If there were serious distortions in the Scottish system, how could that be shown? It could be shown by investigating other countries even closer to *laissez faire*, or at least without the specific legal restrictions present in Scotland, and finding that important features of the monetary and banking system were significantly different in ways not attributable to legal restrictions in the other countries.

Until such findings come to light, I will continue to believe that the free banking model is relevant to Scottish banking, and that Scottish banking before 1844 provides useful evidence on free banking. Scotland offers a closer approximation to *laissez faire* than most other countries with plural note issue. The case for free banking can therefore continue to benefit from the Scottish experience.

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SECHREST ON SCOTTISH FREE BANKING

Kevin Dowd

Larry Sechrest offers a thoughtful and stimulating perspective on Scottish free banking, which makes a very useful contribution to the free banking controversy. I would like to focus my comments on three main points.

The first is the relative failure rate between English and Scottish banks, which presumably sheds some light on the crucial issue of whether the Scottish free banking system was more stable than its regulated English counterpart. I do not believe that the data support Sechrest's claim that the Scottish failure rate was as high as the English one. Let X be the difference between the English and Scottish failure rates. We wish to discriminate between Sechrest's null hypothesis that X has a mean of zero, and White's alternative hypothesis that the mean of X is positive. Sechrest estimates the means of the English and Scottish failure rates using different observations for each and finds that these means are so close that his null hypothesis cannot be rejected. However, this procedure is only valid if each of the English and Scottish failure series can be assumed to be random samples from populations that have trendless means and variances, and these requirements rule out any systematic (or trend) behavior in bank failure rates.

A more defensible procedure would be to suppose that the difference between the failure rates is a random sample with the required properties—systematic influences on bank failures would then be allowed provided they did not affect the relative failure rate. A t-test on Sechrest's 37 observations of the difference between the failure rates then gives a test statistic of 1.52, which has a probability value between 10 percent and 5 percent. This result gives considerably

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The author is a Lecturer in Economics at the University of Nottingham and a Fellow of the Durrell Foundation. He thanks Larry Sechrest and Lawrence H. White for permission to use their data.

less support to Sechrest's null hypothesis than his own test purports to give, but we would still accept his null hypothesis if we adopted the conventional 5 percent decision rule. To assess how robust this conclusion might be, I dropped the first two observations—1784 and 1793—and repeated the exercise with the remaining data. The test statistic then came out to be 2.34, which has a probability value of around 2.5 percent and indicates that we would now reject Sechrest's hypothesis in favor of White's. We also get much the same result if we use Sechrest's data over the 1809–1830 period that White covers, and we get a much stronger rejection of Sechrest's null hypothesis if we use White's data for the same period. (The first exercise gives us a probability value of around 2.5 percent; the second gives one that is negligible.) Sechrest's conclusion is thus acutely sensitive to the choice of data set as well as to the period covered.

These calculations lead me to the conclusion that the balance of evidence so far favors White's hypothesis over Sechrest's, but there is an obvious need for further work to focus on the derivation of the data and on the sensitivity of the results to plausible modifications in the inference method, the period covered, and the data set itself. I would also stress that the ultimate issue is not about bank failure rates per se, but about the relative stability of the two banking systems, and there are other indicators of relative stability besides the comparative failure rate. The relative volatilities of interest rates and bank lending come readily to mind, and these factors also need to be looked at before we can pass a reasonably definitive judgment on the relative stability of the Scottish banking system.

Turning now to the convertibility issue, let me begin by outlining what I understand free banking theory to predict. Competitive pressures would force banks to make their issues convertible, but in a mature free banking system we would not expect it to be efficient for banks to redeem their issues with the same commodity as that whose banknote price they peg (i.e., the medium of redemption would differ from the medium of account). The currency would therefore be "indirectly" convertible—banks might use financial instruments to redeem their issues, for example, but those financial instruments would have a given value in, say, gold. We might also expect bank issues to involve an "option clause" that would give the bank the choice of deferring redemption for some period provided it compensated the holders later on. (It was unfortunate that I described option clause convertibility as "imperfect" in my book [Dowd 1989, p. 156]. I am grateful to Sechrest for making me see how misleading that label is.) It follows that we would not expect free banks on a gold standard—such as the Scottish banks—to use gold to redeem their

issues, and we must also reckon with the possibility of an option clause.

The evidence Sechrest presents demonstrates only that the Scottish banks did not issue a “directly” convertible currency. This point is a useful one to make—I tried to make it myself in Dowd (1989, pp. 156–57)—but I see it as *confirming* rather than rejecting one of the predictions of free banking theory. It certainly does not establish, as Sechrest seems to suggest, that the Scottish currency was inconvertible as such, and there is other evidence to suggest that the inconvertibility hypothesis is rejected. The fact that the Scottish banks resumed their normal redemption policies after the Bank of England suspended specie payments in 1797 indicates how competitive pressures compelled the banks to maintain convertibility. And the apparent absence of any major discrepancy between the value of the Scottish pound note and gold for most if not all the free banking period would seem to confirm the convertibility of the Scottish currency. (Let me suggest, however, that the issue can be settled reasonably definitively by compiling a series on the price of gold in terms of Scottish pound notes. The inconvertibility hypothesis predicts that the series should be non-stationary, i.e., trended. I believe that this prediction would be rejected.)

Let me make one last point. Sechrest presents evidence that the small-notes ban, usury laws, and privileges of the larger banks meant that the Scottish banking system departed in significant ways from the theoretical ideal of a *laissez-faire* banking system. I readily acknowledge that there were (unfortunately) important departures from the *laissez-faire* ideal, but the key issue is not how “pure” Scottish free banking actually was, but the extent to which these departures invalidate specific conclusions we may draw from a free banking interpretation of the Scottish experience (such as White 1984, 1990, 1991). I see no reason to believe that these departures invalidate the important conclusions (a) that the Scottish banking system was more stable than the English one, or (b) that it issued a convertible currency.

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