Classical Monetary Theory and the Quantity Theory

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Some years ago, I proposed (Glasner 1985) what then seemed to be a novel interpretation of classical monetary theory. Relying on work by Laidler ([1972] 1975), Thompson (1974), Frenkel and Johnson (1976), Girton and Roper (1978), and McCloskey and Zecher (1976), I suggested that notwithstanding the conventional identification of classical monetary theory with the quantity theory of money—and a rather crude version of the quantity theory, at that—many leading classical theorists espoused a monetary theory very different from the quantity theory. The difference between the two theories, I argued, is that the quantity theory treats the stock of money as an exogenous variable to which prices adjust, whereas the other (antiquantity-theoretic) theory treats the absolute level of prices, fixed by the convertibility of money into a real commodity, as an exogenous variable to which the stock of money adjusts. I further argued that much of the history of classical monetary theory could be understood as a dialectic between these two clashing theories.

Until the early 1970s the notion that the quantity theory was the dominant, if not the only, monetary paradigm in classical economics

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was not even up for debate. The works cited above laid the foundation for my own challenge to the consensus. What role my 1985 article and a subsequent one (Glasner 1989b) had in upsetting the consensus about the quantity theory in classical economics is better left to others to sort out (see Skaggs 1999). But whatever its significance, my contribution to this reassessment of classical monetary theory elicited critical responses from two distinguished historians of economic thought, Mark Blaug (1995) and D. P. O'Brien (1995).

Blaug (1995, 32–33) charges that I misrepresented my antiquantity-theoretic version of classical monetary theory as the exclusive classical monetary theory when, in fact, the quantity theory was integral to classical monetary theory, and that I mistakenly included David Ricardo, Henry Thornton, and J. S. Mill in the antiquantity-theory camp. O'Brien (1995, 54) contends that the theory I ascribed to classical monetary theorists bears no likeness to classical monetary theory. Moreover, the theoretical model underlying my version of classical monetary theory, O'Brien argues, is based on a series of untenable theoretical assumptions that the classical economists would never have entertained.

In responding to these issues, I shall first address the less fundamental, though hardly trivial, criticism of Blaug. Blaug actually acknowledges the antiquantity-theoretic content of much of classical monetary theory, and in doing so he undercuts, at least in part, some of O'Brien's criticisms. In section 2, I summarize the model that I used to explain the classical antiquantity theory. O'Brien charges that that model, focused exclusively on long-run equilibrium conditions, not only ignores the short-run adjustment problems that chiefly concerned the classical monetary theorists but also improperly assumes that purchasing power parity always obtains. I demonstrate that my model does *not* exclude the possibility of short-run disequilibria and that the only assumption of the model related to purchasing power parity is a routine one—that the law of one price obtains. I consider in section 3 a model that explicitly allows for short-run variations in local price levels, in this case. It is only within such a model, according to O'Brien, that the issues that occupied the classical economists can be understood. In section 4, I discuss the nature of monetary adjustment when the money stock consists of the liabilities (convertible on demand into gold) of a competitive banking system. In section 5, I comment on the role of monetary policy in a model with an endogenous real business cycle, a circumstance that, in O'Brien's view, provides the rationale for the proposed monetary regime of the Currency School. Section 6 contains a few concluding thoughts.

1. Blaug on Classical Monetary Theory

Blaug discusses classical monetary theory in the context of the question (which also provided the title of his paper), Why is the quantity theory of money the oldest surviving theory in economics? In sketching the history of this theory, Blaug represents the views of the classical economists in what seems to me an eminently reasonable fashion. Having identified the exogeneity of the money stock as the key factual prerequisite for the quantity theory, Blaug recognizes the tension between the theory and an adherence to the gold standard. Consequently, he remarks on the anomalous nature of David Hume's famous thought experiment about the adjustment process that would follow the destruction of four-fifth's of Britain's domestic gold stock and why, notwithstanding the puzzlement of earlier commentators, notably Jacob Viner (1937), Adam Smith properly chose not to reproduce Hume's price-specie-flow analysis in his own account of international monetary adjustment. Only during the 1797 suspension of the convertibility of sterling into gold did the quantity theory come into its own as a policy-relevant analytical tool. With the British supply of money under the control of the Bank of England and with fluctuating exchange rates in terms of gold and other currencies, the preconditions for the quantity theory to hold were more nearly satisfied during the suspension than they had been before or, for a long time, would be again. After the resumption of convertibility, the Currency School derived their policy prescriptions from the quantity theory, whereas the Banking School reverted to a supposedly more general cost-of-production theory of the value of money.

This all seems to me to be on target, and Blaug (1995, 32) acknowledges that he is just reiterating what "has become almost a standard interpretation of classical monetary economics in recent years." However, Blaug concludes his account of the quantity theory in classical economics by alluding to "commentators who have taken historical revisionism one step further" and "argue that classical monetary theory has been misunderstood by just about everybody." Noting again that the Banking School denied that the monetary authorities could control the quantity of money produced by the banking system, Blaug attributes to me the thesis "that the victorious Currency School applied the quantity theory to a monetary regime for which it was, strictly speaking, inappropriate" (32). Quoting my assertion (Glasner 1985, 55) that misplaced quantity-theoretic propositions in the work of some classical economists "have fostered the misconception that the quantity theory was the essential classical monetary theory," Blaug comments (1995, 33) that if this indeed is a misconception it was widely shared by the classical writers themselves.

Blaug's discussion here raises two questions. First, as a matter of positive economics, is the quantity theory really incompatible with a monetary regime based on convertibility? Second, as a matter of doctrinal history, did classical monetary theorists really consider the quantity theory to be incompatible with a monetary regime based on convertibility? On the first question, Blaug concludes that the key issue is the degree to which the money supply is exogenously or endogenously determined. On this relevant question, I do not doubt that, for most of the classical period, the money stock was endogenously determined.[1] But the reasons for this endogeneity are more general than Blaug seems to recognize.

I shall return to this point in sections 3 and 4; the question of immediate interest is whether classical monetary theorists recognized the incompatibility of the quantity theory with a monetary regime based on convertibility. Blaug claims that in my 1985 and 1989 articles, I said that the quantity theory was not the essential classical monetary theory, and he takes objection to such a view. However, I do not believe that those articles denied that the quantity theory was an important part of classical monetary theory. My point was that there was a coherent antiquantity theory that was also espoused by an important group of economists within the classical tradition. To be sure, I took the liberty of calling that antiquantity-theoretic model "the classical monetary theory." Blaug calls my doing so a "trick," a way to confine "the label 'classical monetary theory' to those who either denied the exogeneity of the money supply, like the members of the Banking School, or who con-

1. In fact, endogeneity is not quite enough to lay the quantity theory to rest. In a pure commodity money economy, for example, the stock of money is endogenous but will exhibit a nearly proportional relationship between the stock of money and prices. For an elegant discussion of this point and Ricardo's recognition of it, see Cottrell forthcoming. Similarly, as an anonymous referee noted, the quantity theory could still hold if the money-supply function included endogenous variables as arguments and the money-supply function were independent of the demand for money.

fined their arguments essentially to inconvertible paper money, like Ricardo and Thornton." But what Blaug disparages as a "trick," I conceived as a rhetorical device to underscore the theoretical coherence of a particular set of views that, unlike the quantity theory, were created by and found widespread support among classical economists. In and of itself, employing such a rhetorical device did not deny that the quantity theory had a place in classical economics or even that it was often deployed to analyze convertible monetary regimes. Of course, the words Blaug used to disparage my use of the label "classical monetary theory" themselves constitute a rhetorical device to underscore his view that the quantity theory was an essential part of classical monetary theory—a position that I never disputed. So the issue between Blaug and me, it turns out, is rhetorical, not substantive.

However, Blaug (1995, 45n. 9) does raise a substantive disagreement in challenging my contention that classical economists like Thornton, Ricardo, and Mill would have "excluded the convertible money created by the banking system from the quantity of money that could be said to have an independent effect on prices" or that "from their point of view, the quantity of money produced by the banking system behaved passively" (Glasner 1989b, 226). I continue to believe that my interpretation of Ricardo is well supported. Indeed, subsequent work by Arnon (1984, 1991) on the evolution of Ricardo's monetary theory and his monetary-policy views generally upholds my earlier (Glasner 1985, 56–57, 61) interpretation of Ricardo. Blaug (1995, 31) seems even to concede as much in an earlier passage that also undercuts his rhetorical thrust against me.[2] Although Mill's tendency to be all-embracing in his theoretical discussions creates some ambiguities, his consistent espousal of Banking School doctrines, a number of which he seems to have been the first to articulate, has been amply documented by Skaggs (1994). Nor can I understand how one who denied that banknotes could be overissued as long as any coin remained in circulation (Mill [1848] 1909, 634; see also 544, 654) could have believed that the quantity of money produced by the banking system did not, under convertibility, behave passively.

2. Before criticizing my verbal "trick" of redefining classical monetary theory, Blaug himself espoused a strikingly similar definition: "When convertibility of paper money was restored in 1821, the members of what soon came to be known as the Currency School argued as if the quantity theory was relevant even for commodity money, while the members of the Banking School echoed the *truly classical*, pre-Ricardian cost-of-production theory" (1995, 31; emphasis added).

Even Thornton, who, of the three mentioned by Blaug, may have held views furthest from my version of the classical monetary model, espoused many propositions consistent with that model. Thornton believed, for example, that the value of a convertible currency was determined by the value of gold, and that country banks passively supplied the quantity of paper money consistent with the price level determined either by convertibility or, during the suspension, by the Bank of England. To be sure, elements of his account of the adjustment by country banks to the lead of the Bank of England suggest that the country banks had a limited and transitory impact on local prices that triggered an interregional analogue of the price-specie-flow mechanism. But Thornton's own version of the price-specie-flow mechanism was sufficiently differentiated from Hume's to suggest that the adjustment process that he envisaged did not require a very long time to run its course. Indeed, Thornton ([1802] 1939, 269–70) explicitly criticized Hume's price-specie-flow analysis precisely because it ignored the constraints of commodity-price arbitrage on spatial price differences.

On the other hand, Thornton's seminal articulation of the central-banking or lender-of-last-resort responsibilities of the Bank of England might be viewed as inconsistent with the spirit of the classical model that I described in earlier work, which emphasized the equilibratory role of competition in ensuring that banks supply just the amount of money demanded by the public. Although the classical model could provide a theoretical rationale for free banking and for withdrawing all monopoly privileges from the Bank of England, that policy conclusion, especially given the fragmented and undercapitalized character of the country banks, was not the only one consistent with the classical model. Even John Fullarton (1845) and Thomas Tooke (1844) and, a generation later, Walter Bagehot ([1873] 1962), although sympathetic to the idea of competition in banking, did not advocate stripping the Bank of England of its central-banking and lender-of-last-resort responsibilities. Moreover, Skaggs (1995) has shown that the widespread impression that during the nineteenth century Thornton's teaching fell into obscurity only to be resurrected by Friedrich Hayek and Viner is belied by the extent to which his views and policy prescriptions were incorporated into the Banking School doctrine and, through the Banking School, transmitted to Walter Bagehot, whose articulation of the central-banking and lender-of-last-resort responsibilities of the Bank of England defined British monetary orthodoxy toward the end of the nineteenth century. It is entirely plausible that Thornton had less in common with the straightforward quantity-theoretic analysis of the Currency School than with the antiquantity-theoretic tradition that inspired the Banking School.

One final reason for Thornton's particular relevance for this discussion is his concern with and early contributions to a theory of banking. The point of such a theory is to explain how the quantity of (bank-created) money is somehow determined by an interaction between the public's demand to hold bank-created money and the costs that banks incur in creating such money. Such a theory is totally absent from, and not easily reconcilable with, the quantity theory of money. In its cruder forms, the quantity theory takes the nominal quantity of money as an exogenous variable, and analyses of the banking system, if any, rarely go beyond a more or less mechanical rendition of the money multiplier.[3] In the classical period, the focus of the quantity-theoretic discussions of banking typified by, say, Robert Torrens (1858) was on a supposed propensity toward overissue and overexpansion, which occasioned policy measures to constrain the fluctuations in some subset of bank-created monetary instruments to mirror those imputed to an imaginary, full-bodied, purely metallic currency. So Thornton's concern with how the quantity of bank-created money might respond to fluctuations in the demand for it is sufficient, by itself, to distinguish him from the quantity-theoretic tradition in classical economics.

2. O'Brien on the Law of One Price, Purchasing Power Parity, and the Quantity Theory in Classical Economics

O'Brien faults my account of classical monetary theory for two reasons. First, my model, in his view, applies only to long-run equilibrium, whereas the classical monetary theorists were concerned mostly with short-run monetary control. Second, my model improperly assumes

3. As mentioned in footnote 1, the quantity theory may be consistent with a functional relationship between the nominal quantity of money and other endogenous variables as long as the supply function is independent of the demand function. The theoretical point is that in the quantity theory, discrepancies between the quantity of money demanded and the quantity supplied do not, as they do in the classical model, trigger a rapid adjustment in the quantity supplied. Instead, such discrepancies trigger adjustments in income and prices, which, through a circuitous process, ultimately bring the quantity of money demanded and the quantity supplied back into equality.

that the law of one price holds continuously when, in fact, it holds, if at all, only in the long run. This assumption leads to the notion that convertibility entails a uniform international price level. Together, these assumptions "produce an account of classical monetary theory" that is "hard to recognize" (O'Brien 1995, 51).

Interestingly, O'Brien holds up my model to illustrate a style of theorizing that, borrowing from Joseph Schumpeter, he calls the Ricardian Telescope. By this O'Brien means an inappropriate Ricardian preoccupation with long-run equilibrium conditions that distracts us from the possibility that these conditions may not always be satisfied and that deviations from long-run equilibrium conditions could be cumulative. In making this charge against my model, O'Brien does not seem to have grasped it quite correctly. In addition, he seems insensible of the paradox latent in a charge that an account of classical monetary theory could be unfaithful to the original because it too closely follows the theoretical style of the premier theoretician of classical economics.

O'Brien characterizes long-run equilibrium in classical monetary theory as a set of international prices and a distribution of gold that would occasion no international flow of gold, that is, that entails equilibrium in the balance of payments. Thus, O'Brien considers the adjustment of relative prices to be integral to international monetary equilibration. My argument, that under the gold standard national price levels could not deviate from the uniform international level fixed by the value of gold, clearly collides with O'Brien's understanding of the international adjustment mechanism.

What O'Brien fails to address is that, quite apart from the international distribution of specie under the gold standard, profitable arbitrage opportunities should (in theory) constrain price differences in internationally traded commodities at different locations within limits set by the costs of transporting them between those locations. Moreover, the constraints on price differences between internationally traded commodities at different locations would (in theory) indirectly constrain price differences between untraded commodities at those locations, especially insofar as untraded commodities were supplied competitively. The arbitrage constraints on price differences between similar internationally traded commodities would not necessarily eliminate all price differences. The point is just that the error-correction mechanism driven by commodity arbitrage is more direct than the one driven by international specie flows. That long-run equilibrium in the balance of payments under the gold standard imposed the only relevant constraint on international price deviations is hardly as self-evident as it appears to O'Brien. As Paul Samuelson (1980, 154–55) observed, "[Hume] made the mistake of supposing that the Quantity Theory of Money linked every price in a region to the money in that region alone. This overlooked the forces that keep the competitive prices of the same transportable good virtually the same in all regions."[4]

Indeed, O'Brien (1995, 54) explicitly commends the classical writers for not believing that the law of one price "holds *at all times*." In emphasizing the qualifier "at all times," O'Brien leaves it unclear over what time period he or the classical writers believe that the law of one price would not hold.[5] After appealing first to Nassau Senior and then to Adam Smith to support his contention that the classical economists rejected the law of one price and that national price levels could differ under the gold standard, even in the absence of balance-of-payments equilibrium, O'Brien (1995, 55) later concedes that Ricardo criticized Adam Smith for forgetting that the prices of internationally traded products like corn could not differ between countries. Having allowed the law of one price into classical economics through the backdoor, O'Brien shifts his focus to purchasing power parity, invoking the authority of Jacob Viner against the doctrine even as he quotes Viner's explicit distinction between the law of one price and purchasing power parity.

O'Brien cites Viner as the authority for the proposition that a shift in the terms of trade between two countries can, even under the gold standard, cause a divergence between their domestic price levels. Believing that this possibility refutes the following assertions of mine, O'Brien offers a detailed proof of the proposition. The fallacies (all drawn from my 1989 paper) are as follows:

4. Cesarano (1998) has argued that Hume did not make the mistake attributed to him by Samuelson and many others, including Thornton ([1802] 1939, 269–70). Cesarano makes a good case that Hume's analysis is not as misguided as it has been made to appear. But, even in Cesarano's interpretation, Hume's thought experiment is still problematical. At any rate, Samuelson's substantive analysis remains very much intact.

5. O'Brien (1995, 54) seems to make a substantial concession to my position by stating that if the law of one price did hold, then all the positions that I attributed to the classical economists, such as the validity of the Banking School position and the validity of Say's identity for a monetary economy, would follow. In fact, the law of one price is not a sufficient condition for these conclusions to follow. One must still assume that a competitive market mechanism governs the supply of money by the banking system.

1. "The price level in any country was fixed by the internationally determined value of the metal" (205).
2. "With the value of precious metals unchanged, the price level, under a metallic standard, would not change either" (207).
3. Adam Smith rejected the Humean price-specie-flow mechanism "because it incorrectly applied the quantity theory to determine the price level of a country with a metallic currency. A national price level depends on the international value of the metal used as money, not the quantity of money in the country" (208).

The proof supplied by O'Brien consists in showing that in a two-good, two-country model, an increase in country A's demand for country B's export raises the relative price of B's export compared to A's export, that is, the terms of trade shift in favor of B. If both countries are specialized in the production of their own export, the domestic price level of A falls and the domestic price level of B rises. So much for the idea that domestic price levels are fixed by convertibility.

The difficulty with this proof is not with its logic, which is unassailable, but its relevance, for it uses the term *price level* in a sense different from the one I attached to it. O'Brien understands *price level* to mean the equivalent of the GDP price deflator. Thus an increase in the price of the export of country A relative to the export of country B implies a corresponding increase in the GDP price deflator of A relative to that of B. But that was not what I meant. Rather, I had in mind a more comprehensive measure that includes all internationally traded products. Let the relative prices of the exports of the two countries change. Measured in terms of a common unit of account, their prices, even after the change, remain equal in both countries. Given this meaning, there is nothing wrong with saying that both countries share a uniform price level.[6] In fact, this equality of prices is inherent in the very diagrams that O'Brien uses to show that the GDP price deflators of the two countries diverge.[7]

6. Viner (1937, 311–18) explicitly discusses the absence of any statistical concept of the price level in the writings of the classical theorists. Instead, they viewed the price level as a vector of all money prices for all goods.

7. O'Brien (1995, 58–59) concludes that the "free-banking reinterpretation" of classical monetary theory is based on the proposition that, under all normal circumstances, the real exchange rate is constant. Since it is a commonplace of the pure barter theory of international trade that the terms of trade (to which the real exchange rate corresponds in a monetary model) can change, O'Brien appears to have inferred (incorrectly) from my assertion that a

O'Brien (1995, 59) concludes by attributing the "distinctly curious results" I reached to the combined confusion of assuming purchasing power parity and instantaneous adjustment to long-run equilibrium. However, the only confusions that I can detect are semantic: between the law of one price for similar tradable commodities and purchasing power parity and between two distinct meanings of the term *price level.* Moreover, the law of one price, as a staple of international trade theory, is an assumption that O'Brien cannot avoid in his analysis, even though he argues as if it were a condition of long-run equilibrium. Referring to my citation of Samuelson's (1980) analysis of balance-of-payments adjustment under the assumption that "prices for all goods are perfectly arbitraged across all markets" (Glasner 1989b, 209), O'Brien observes that this "is, of course, something which would never occur during an adjustment period, if it ever occurred at all" (1995, 59). Presumably, O'Brien is referring to the adjustment period during which balance-of-payments equilibrium is restored. But he neglects to explain how the arbitrage of price differences between internationally traded commodities is linked to the balance of payments.[8]

O'Brien (1995, 75–76n) goes on to assert that "despite misconceptions to the contrary . . . it [is not] necessary for price equalization to exist for the gold standard mechanism to correct the balance of payments. Indeed . . . one would not expect purchasing power parity—merely the adjustment of relative price levels to the point at which reciprocal demand would bring payments into balance." O'Brien suggests here what he calls "price equalization" is brought about, if at all, only through the restoration of balance-of-payments equilibrium. He asserts that Samuelson "simply dismisses" the balance-of-payments adjustment mechanism advanced by Viner and Gottfried Haberler and "assumes price equality in order to put forward a version of the modern monetary theory of the balance of payments." "It should hardly be

change in the quantity of money could not alter the real exchange rate that I deny that a real disturbance could do so.

8. O'Brien relies on an unpublished paper by I. Moosa (1993) to assert that, as a factual matter, the prices of goods were not equalized under the gold standard. Even accepting Moosa's study at face value, I do not see how it would affect an evaluation of the content of classical monetary theory. The law of one price was clearly accepted in some form by most classical economists. In some cases (e.g., Nassau Senior), they inconsistently framed quantity-theoretic arguments about international monetary equilibrium that were contradicted by the law of one price without explaining why the law of one price did not hold.

necessary to emphasize," O'Brien objects, "that one set of assumptions cannot invalidate an alternative set of assumptions" (1995, 76n. 3). But Samuelson did not simply assume price equality any more than O'Brien simply assumed it in showing that the terms of trade could shift in a two-good/two-country model. They both assumed that competition would eliminate price differences between similar products, because intracommodity arbitrage, derived from a deeper theoretical analysis than is the price-specie-flow mechanism, is woven into the very fabric of conventional economic analysis. A model of balance-of-payments adjustment cannot, in the absence of an explicit theoretical argument to the contrary, routinely proceed as if price differences subject to arbitrage were not eliminated.[9]

3. Local Price-Level Differences and the Gold Standard

I believe that I have shown that O'Brien's objections notwithstanding, my account of classical monetary theory is coherent and may even be true. Nevertheless, I would not summarily dismiss his objections as necessarily misguided. Something about my account clearly disturbs him. And he is too good an economist and too careful a scholar for his concerns to be dismissed even if he has failed to identify adequately the basis for his misgivings. I therefore explore in this section another possible basis for those misgivings.

The nub of O'Brien's objections is my notion of a uniform international price level. Although he chose to criticize this property of my model by attacking the law of one price, he could also have done so by introducing various frictions or transaction costs or simply by postulating the existence of nontradable goods. However, in challenging the idea of a uniform international price level, O'Brien focused exclusively on tradable goods, trying to prove that their domestic prices would not be tightly constrained under the gold standard. This approach was, I

9. O'Brien observes that Samuelson's model does allow for changes in the terms of trade between countries to induce departures from purchasing power parity. He suggests that this concession by Samuelson is somehow inconsistent with my account of classical monetary theory. As I have explained, such a shift in the terms of trade and the resulting violation of purchasing power parity does not contradict everything I have written, although it is true that I did not explicitly discuss this possibility in my 1985 article. On the other hand, I did discuss at length another possible departure from purchasing power parity in my 1989 *HOPE* article (222–25). See section 4.

think, bound to fail. I should now like to consider how the analysis would have played out had O'Brien chosen to couch his criticism in terms of a model that included nontradables.

Suppose that in addition to the two tradable goods (apart from gold) in our two-country model, each country has a third, nontradable one. Let the money stock in country A be increased exogenously. The resulting excess supply of money and excess demand for goods in A cannot initially raise the price of either of the two tradable goods, unless total world spending on both tradable goods rises, so that the relative price of gold falls and the world price level of tradable goods rises. Now suppose that the additional spending on the tradable goods in A is matched by a corresponding cutback on tradable-goods spending in B, with an unchanged division of spending between the two tradable goods. With the relative price between the tradable goods unchanged, there is no change, under the gold standard, in the price of either good. On the other hand, the additional spending on A's nontradable good raises its price (absolutely and therefore relative to the two tradable goods). The increased relative price of the nontradable good draws resources from A's export industry, reducing A's exports, which, together with an increase in imports from B, creates a balance-of-payments deficit in, and an outflow of gold from, A. Gold flows out of A until the excess supply of money is eliminated and the price of the nontradable good returns to its original equilibrium level.

Thus, if we allow for nontradable goods, the potential for price-level disturbances that troubles O'Brien could indeed arise under a gold standard. If so, it is fair to ask whether my account of international adjustment under the gold standard can survive the inclusion of nontradables in the analysis. In particular, is it possible that, under the gold standard, a banking expansion would increase the prices of nontradables, triggering an unsustainable internal boom and a balance-of-payments deficit—the scenario that so preoccupied the Currency School? And if not forestalled or promptly reversed by appropriate policy measures, would such a boom increase the risk of a financial panic owing to a contraction belatedly initiated by a banking system whose commitment to convertibility was threatened by a continuing outflow of gold?

These issues cannot be considered strictly in terms of the theory of international monetary adjustment that chiefly concerned us in this and the previous section. They must also be viewed in light of the classical theory of banking to which I referred in section 1. This theory, which originated with Adam Smith, was further developed by John Fullarton (1845) in his contribution to the Banking School–Currency School debate and inspired his famous law of reflux (Glasner 1992). Despite his explicit focus on the Banking School–Currency School debate and references to my enthusiasm for free banking, O'Brien is surprisingly reticent about this theory of banking.

4. The Classical Theory of Banking, the Law of Reflux, and the Quantity Theory

The key feature of what I call the classical theory of banking is the notion that there is an economic mechanism that induces profit-seeking bankers to supply an amount of banknotes (and deposits) that equals the amount demanded by the public. In contrast to David Hume (1955, 67–68), who, perhaps prejudiced by the debacle of John Law's system a few decades earlier, believed that banks were inherently disposed to overissue, Adam Smith ([1776] 1976) argued that the marginal cost of redeeming unwanted convertible notes would exceed the marginal revenue from issuing them, so that a private bank would never intentionally issue more of its notes than the public wanted to hold. Miscalculations might occur. But the resulting overissue would be unprofitable, and banks would try to avoid them. One way they could do so was to lend only on the security of real commercial bills. Hence the real-bills doctrine (Glasner 1992, 871–75).

Henry Thornton is often thought to have espoused a very different view of banking from that articulated by Adam Smith. Unlike Smith, Thornton emphasized the unique role of the Bank of England in the British monetary system and the need for the Bank of England to exercise judgment in discharging its special responsibilities. I do not wish to minimize these differences, which were insufficiently acknowledged in my earlier work. But notwithstanding his substantive differences with Smith, Thornton also believed that there was an automatic mechanism that equalized the quantity of paper credit supplied by the British banking system with the amount demanded. Thus Thornton criticized Smith for asserting that banks, in displacing coin from circulation by issuing paper money, would issue no more paper than the amount of coin displaced. Thornton ([1802] 1939, 95–96) observed that since the public would probably find it convenient to hold more paper money than they would have held in coin, banks would actually issue more paper than the quantity of coin displaced. But since they would not be issuing any more paper money than the public wanted to hold, banks would not thereby precipitate an inflationary overissue. Moreover, Thornton in both his *Paper Credit* and in the *Bullion Report* (Cannan 1925) absolved country banks of any responsibility for the inflation that occurred after the suspension of convertibility in 1797. Although no enthusiast for free banking, Thornton shared none of the Humean prejudice that banks were inherently predisposed to inflationary overissue, a view that, though not widely held in the Bullionist controversies, became a basic tenet of the Currency School.

John Fullarton further developed the classical theory of the supply of bank money. Observing that banks could not prevent the public from converting banknotes into deposits and that competition obliges banks to pay sufficient interest on deposits to eliminate any profit from creating deposits at the margin, Fullarton (1845, 92–93) argued that banks would issue no more notes than the public demanded. Any unwanted notes would be converted into deposits, which generate no profit at the margin (see also Glasner 1992, 877–82). Bank incentives to expand were governed by the marginal profitability of creating additional deposits, not the inframarginal profitability of notes.

In the previous section, I conceded that O'Brien could have grounds for concern about the potential for destabilizing monetary disturbances in a model with nontradable goods whose prices, within some limits, could fluctuate in response to monetary disturbances. In the view of the Currency School and O'Brien, such disturbances are potentially destabilizing. If, in contrast to the simple case in which all goods are tradable, such disturbances do have price-level effects, is the Banking School position that purely monetary disturbances are not a serious policy problem still defensible?

It will be useful first to recall that for a monetary disturbance to be destabilizing under a gold standard, the disturbance must induce responses that entail a cumulative movement away from equilibrium, until more or less violently checked by some less immediate constraint, say, a loss of gold reserves. If such cumulative movements away from equilibrium are at all likely, then the concerns of the Currency School —the prevention or mitigation of potentially cumulative deviations from an equilibrium path that are caused by monetary disturbances— quite plausibly determine the objectives of monetary policy.

However, even if domestic monetary disturbances, by affecting the prices of nontradable goods, could affect domestic price levels under the gold standard, it does not follow that such disturbances would trigger cumulative deviations from equilibirum. A cumulative deviation requires a persistent monetary expansion. But if, as many classical economists believed, there is an automatic mechanism that equates the amount of money supplied by a competitive banking system with the amount demanded by the public, then a cumulative process would not automatically follow from an overissue of bank money, even an overissue that did raise the prices of nontradables, unless that automatic mechanism ceases operation.

Suppose that the banking system mistakenly overissued bank money and that the reflux mechanism did not withdraw the excess money from circulation rapidly enough (i.e., banks did not realize their mistake soon enough to allow the money they had created to be extinguished in the normal course of accepting repayment of outstanding loans) to prevent the prices of nontradables from rising. If prices did rise, the nominal demand for money would increase accordingly to absorb the overissue so that, at least in the short term, the excess money would remain in circulation. The long-run outcome would depend on whether domestic banks, after the resulting balance-of-payments deficit causes an outflow of gold, choose to operate with a reduced level of gold reserves. If they do, then the world price level would adjust slightly as the relative price of gold fell and the earlier relative price ratio between tradables and nontradables was restored. If domestic banks try to restore their reserves, they would tend to restrict their issue of money, and domestic nontradables prices would recede toward their earlier levels. Thus although an overissue by the banking system is possible, the effect would seem to be limited, not cumulative, in the short run and might well be reversed in the long term.

To account for a cumulative effect, one must show not just that overissue is possible, but that a competitive banking system, as David Hume and the Currency School believed, is predisposed to overissue. But if, as Adam Smith and the Banking School believed, a bank cannot profit from overissue, so that overissues are usually unintended, then episodes of overissue would not be serially correlated and would not lead to a financial crisis.

One way to rationalize a supposed propensity of competitive banks toward overissue would be to invoke the Thornton-Wicksell distinction between the natural and market rates. Because the natural rate is unobservable, competitive banks would typically not immediately adjust their lending rates upon an (unobservable) increase in the natural rate. The divergence between the natural rate and the lending rate encourages investors to resort to relatively cheap bank credit to finance their investment projects, fueling a rapid expansion in bank lending and deposit creation.

Although Thornton ([1802] 1939, 253–56) not only recognized that, but explained how, such an expansion was possible, he qualified the argument by explicitly linking it to usury laws that prevented banks from raising their lending rates to match the market level. Thus it is not clear how serious a problem Thornton, at any rate, would have considered deviations between the natural and market rates in the absence of legal constraints on bank lending rates. Certainly, profit-seeking banks have a strong incentive to raise their lending rates after a real increase in the demand for bank finance. The natural rate may be unobservable, but there are observable market indicators that do respond to changes in the natural rate.

It may be worthwhile at this point to observe that standard renditions of the cumulative process triggered by a divergence between the natural and market rates typically exclude an essential element from the analysis. Focusing on the relationship between the natural rate and the bank lending rate, they ignore entirely the bank borrowing or deposit rate. This might have made some sense in Thornton's time, when deposits did not account for the bulk of total bank liabilities; it did not make sense by the fourth and fifth decades of the nineteenth century at the height of the Banking School–Currency School debates. Unless deposit rates are somehow held even further below market levels than are lending rates, why would banks expand their balance sheets by issuing additional liabilities? They would be more likely simply to allow the composition of their assets to shift toward holding the IOUs of private investors and away from holding other, perhaps less risky, assets. Unless banks expand their balance sheets and increase the total volume of their liabilities, it is not so clear that the natural-rate/market-rate analysis would have strong implications for the behavior of the price level. Of course, the composition of the balance sheets of banks is not devoid of macroeconomic significance. As they become more heavily weighted toward business loans, balance sheets may become more risky and less liquid, which could have serious consequences at the end of the investment boom. And although such balance-sheet effects could serve as a rationale for providing the banking system with some sort of lender-of-last-resort protection, they would have but a tenuous connection to the concerns that animated the Currency School.

Thus even within a model that allows for short-run deviations in national price levels under the gold standard of the kind O'Brien seems to envision, the endogenous mechanism tending to equate the supply of money with the demand for money would tend to limit, if not eliminate, the destabilizing effects of such short-run deviations. However, in questioning the importance of such deviations, I do not necessarily dismiss the importance of all monetary disturbances.

My conclusion that monetary disturbances are unlikely to be important within a model that includes nontradables as well as tradables follows from a particular theory about how a competitive banking system works. In contrast to a model with just tradables, in which the unimportance of monetary disturbances follows directly from the law of one price, that result is contingent, in the more general model, on a theoretically shallower model of competitive banking.

5. Monetary Policy in an Endogenous Cycle

In discussing the Banking School–Currency School debate, O'Brien argues that the policy prescriptions of the Currency School were calculated to smooth out an endogenous real business cycle. Citing Lord Overstone's description of the inherent regularity of the business cycle, which monetary policy could, at best, only mitigate but never eliminate, O'Brien (1995, 68–75) tries to prove the superiority of the Currency School regime to the noninterventionist policy of the Banking School. To do so, he deploys a mathematical model of an endogenous real cycle in which changes in the quantity of money positively affect prices and real income in the short run.

O'Brien's results follow straightforwardly from his assumptions. If there is an endogenous real cycle in real income and prices, and changes in the quantity of money cause corresponding short-run changes in real income and prices, it would be surprising indeed if reducing the quantity of money during the upswing and increasing it during the downswing did not reduce the amplitude of the cycle.

The Banking School shared the Currency School belief in an endogenous real cycle, though there were few who attributed to it the highly symmetrical character that Lord Overstone ([1857] 1972, 31) did. However, the Banking School held that underlying changes in economic activity—"the needs of trade"—elicited changes in the quantity of money. Banking School theorists doubted that the underlying cycle responded to policy-induced changes in the quantity of money, believing that those instruments (banknotes) that the Currency School wanted and those (deposits and bills of exchange) that they did not want to control were readily substitutable for each other. Thus a mildly restrictive stance during the upswing was unlikely to have much dampening effect. However, a strict quantitative limit on the total amount of banknotes could, and did, have a devastating psychological effect as soon as it was feared that the Bank of England would be statutorily barred from extending additional credit. Such fears, as Bagehot ([1873] 1962) described so well, proved to be self-fulfilling, and once they became widespread the ensuing financial panic subsided only after the Bank Charter Act was suspended.

Thus O'Brien's demonstration that the Currency School policy regime produced a cycle of lesser amplitude than did that of the Banking School is contingent on assumptions about the demand for money that the Banking School would not have accepted. If we allow for empirically important kinds of disturbances to the demand for money, the Currency School policy regime may well be a source of instability at least as dangerous as the passive Banking School policy in the face of an endogenous real business cycle. No mathematical model can resolve the underlying dispute about the character of the demand for money.

6. Conclusion

In previous work, I offered what I believe was a coherent view of a certain tradition in monetary analysis that occupied an important place in classical economics. To provide such a view, I proposed a simplified model of a competitive supply of convertible banknotes and deposits. Although the abstract form of the model may at first have seemed strange to classical monetary theorists, I do not think that, having grasped its content, they would have found its conclusions at all shocking. The tradition with which I have been concerned may be called anti-quantity-theoretic, because it recognized that when the money stock largely consisted of convertible paper produced by a competitive banking system, the quantity of money, even in the short run, was not an exogenous variable controllable by the monetary authority. Although the classical analysis of such a monetary regime was not wholly satisfactory, and though there were important analytical differences even among those classical writers who rejected a straightforward quantity-theoretic approach, this tradition seems worthy of continued interest and study. Such a judgment does not imply that the quantity theory had no role in classical economics or even that its role was subsidiary to the antiquantity theory. But it does, at least, require us to be more careful than we once were in specifying the theoretical presuppositions on which the propositions of classical monetary theory were based.

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