# LIBERTY MATTERS

## LUDWIG VON MISES'S THE THEORY OF MONEY AND CREDIT AT 101

*1912 was the 100th anniversary of the publication of **Ludwig von Mises'** book Theorie des Geldes und der Umlaufsmittel (The Theory of Money and Credit). In this month's "Liberty Matters" online discussion our participants debate the importance of Mises' work as the next step in the application of Austrian economic insights into monetary theory, and in the formulation of a unique Austrian Theory of the Business Cycle (ATBC) which was further developed by Friedrich Hayek and Murray Rothbard. The soundness of his theory of money is tested against the recent emergence of a new form of currency known as Bitcoin.*

## LUDWIG VON MISES'S THE THEORY OF MONEY AND CREDIT AT 101

by Lawrence White

Related Links:

- Ludwig von Mises (1881–1973)
- Austrian School of Economics
- TMC
- Human Action

In *The Theory of Money and Credit* by Ludwig von Mises we have an intellectual treasure chest: a work filled with theoretical and applied economic insights that continues to be cited and debated a century after its initial publication (first edition 1912). Like Mises's *Human Action,* it is worth reading more than once. Each rereading yields gems of insight that were not fully appreciated on the previous reading.

Related Links:

- Knut Wicksell (1851–1926)

Over the years the book has attracted critical attention from prominent economists, especially since its translation into English in 1934. Soon after the first edition appeared the great Swedish economist Knut Wicksell responded to particular points on which it had criticized his view (see Festré 2003). In a review, John Maynard Keynes (1914) praised the book in some respects, but professed to find nothing original in it, presumably due to his later-admitted inability to grasp unfamiliar ideas when reading German (Keynes 1930, p. 199 n.2). In well-known works, J. R. Hicks (1935, p. 2) and Don Patinkin (1956, pp. 71–2) felt compelled to note their differences from Mises's analysis of the demand for money. More recently economists have cited Mises's book as anticipating modern analysis, including Nobel laureate Edmund S. Phelps (1968, p. 682) with respect to inflation expectations, and Nobuhiro Kiyotaki and Randall Wright (1989, p. 194) with respect to search-theoretic models of how money emerges.

Economists who have commented on *The Theory of Money and Credit* in some detail (Moss 1976, Rothbard 1976, Schuler 2012, Hülsmann 2013), and others who have commented in passing, have often called attention to three major contributions.

Related Links:

- Irving Fisher (1867–1947)

- Purchasing Power of Money

(1) It was the first work to use marginal-utility analysis to explain money demand and thence to explain the purchasing power of the monetary unit and its variation. In doing so it began with the preferences and expectations of individuals. By contrast Irving Fisher's near-contemporary *Purchasing Power of Money* (1911) propounded the "quantity theory of money," beginning not with individuals but with aggregative variables in the "equation of exchange."



**Irving Fisher**

(2) It marked a major departure in business-cycle theory by incorporating capital theory to explain real features of cycles. Mises combined (a) the Currency School's analysis of unsustainable business booms due to the injection effects of an unwarranted money expansion with (b) Wicksell's analysis of the effects of holding the market rate of interest below the equilibrium or "natural" rate of interest, and (c) Eugen von Böhm-Bawerk's capital theory, in which having the interest rate at its equilibrium level is the key to coordinating saving preferences with the intertemporal structure of production. From these elements he forged a novel monetary-malinvestment theory of the business cycle. As noted by Festré (2003), "While Wicksell focused on price level fluctuations [as did, we may add, Irving Fisher and other price-level stabilizationists], Mises built a theoretical framework in which business cycles are due to shifts in relative prices." Building on Mises, F. A. Hayek in a series of works spelled out in greater detail the theory of how a monetary expansion that holds the interest rate too low causes cyclical malinvestment, a systemic distortion in the economy's intertemporal structure of production that cannot be sustained (1931, 1933, 1939, 1941).

Related Links:

- Henry Thornton (1760–1815)

- David Ricardo (1772–1823)

(3) It provided a modern "purchasing power parity" theory of exchange rates, in the tradition of Henry Thornton (1802) and David Ricardo (1821), a few years before Gustav Cassel further developed and spread his own version of the theory (see Humphries 1979).

The second of these contributions has been widely discussed in the literature on Austrian Business Cycle theory. The third has been more or less absorbed into mainstream economics. In what follows I would like to further discuss the first one and to call attention to a fourth:

(4) It spelled out the role of competition among banks of issue in regulating the quantity of fractional-reserve bank-issued money (banknotes and checking accounts), putting the analysis and conclusions of the 19th-century Free Banking School on a firmer footing. In discussing this issue I will draw on an earlier but now inaccessible article of mine (White 1992) explicating Mises's free-banking views.

**The Purchasing Power of Money, the "Regression Theorem," and Bitcoin**

In seeking to explain the purchasing power of money (ppm) by reference to the marginal utility of money to individuals, Mises had to overcome the objection that such an explanation would necessarily be circular. The German economist Helfferich (see Mises 1981, p. 141) had reasoned that a unit of money is valued by any individual according to how much it can buy. Its valuation cannot then be used to *explain* how much it can buy. Mises showed that this vicious circle can be replaced

by a harmless helix (McCulloch 1980). An individual values a unit of money according to how much he *expects* it to buy, and the process of forming this expectation will almost unavoidably draw in part (which is not to say exclusively) on his knowledge of the ppm he has most recently observed. Given his expectation of today's ppm, the individual decides how many units he wants to hold, his quantity demanded. The market quantity demanded is the sum of individual quantities. Equilibrium ppm occurs where the market quantity demanded equals the market quantity supplied. As for all other goods, the exchange ratio for money is determined by the ratio "where both supply and demand are in exact quantitative equilibrium" (Mises 1981, p. 129). Where the quantity supplied exceeds the quantity demanded at the prevailing ppm, the ppm falls through the spending of excess balances. The ppm rises in the reverse case of an excess quantity demanded.

Under Mises's hypothesis about money demand, today's ppm draws on yesterday's, and yesterday's on that of the day before, and so on. Mises called this proposition "the regression theorem." How far back does the regression, or historical linkage, go? From a forward-looking perspective of explaining movements in the ppm from here on out, it really doesn't matter. But logically, Mises explained, for a commodity money it goes back to the day *before* the commodity first started being used as a medium of exchange. On that day it had an exchange value or purchasing power due only to the interaction of its supply with the demand for it as an ordinary commodity (for consumption or for use as a productive input) and not for use as a medium of exchange. For a fiat money like the U.S. dollar that became a fiat money by terminating the redeemability of what had been a claim to a commodity money (Federal Reserve Notes were once redeemable in silver or gold coin), the historical chain goes back to the day before termination, and thence back to the day before that commodity became a medium of exchange. Application of the logic to a new fiat money that was initially a redeemable claim for an established fiat money (like the Lithuanian talonas, redeemable 1:1 for the Russian ruble) follows straightforwardly.

George Selgin (1994) has insightfully applied Mises's historical-component hypothesis to explain why all governments that have successfully launched new fiat monies have first given them a fixed redemption value in terms of a commodity or an established fiat money. Donald Patinkin (1956, pp. 114–16, 573–75), on the other hand, has criticized the inclusion of a historical component in Mises's account of money demand on the grounds that the purchasing power of money (like the relative price of any good in Walrasian general equilibrium theory) can be derived completely ahistorically by thought experiment. Putting real money balances (in purchasing power units) into each agent's utility function, the economic theorist determines the real quantity of money the agent demands when faced with various hypothetical price vectors. The theorist then aggregates the individual real money demands, confronts them with the nominal supply, and solves at the market level for the ppm (and simultaneously for the vector of other relative prices) consistent with clearing all markets. Taken seriously, this approach suggests that a government might introduce a new fiat money merely by announcing its nominal quantity, leaving its purchasing power to be determined by the market-clearing requirement. Yet this has never been done, which casts doubt on the practical relevance of Patinkin's logical construct.



**George Selgin**

J. R. Hicks (1935), in a well-known article published one year after the English translation of *The Theory of Money and Credit,* and four years after the British pound had become irredeemable in gold, commented that the historical component in Mises's theory makes the value of any fiat paper money the "ghost of gold." But that is possibly a misleading metaphor. A fiat money is not a specter, but a living money. Its purchasing power evolves according to its own supply and demand changes, uninfluenced (or should we say unhaunted?) by the monetary unit's past definition. So it might be better to say that in Mises's theory a fiat standard is the descendent of a deceased commodity standard.

The historical component is only one component that goes into ppm expectations. Mises does not say that a money-demander will base his expected ppm for today *exclusively* on yesterday's ppm. Thus one should not characterize the regression theorem as saying that today's observed ppm by itself *determines* tomorrow's subjective value of money. The former rather *influences* the latter via the individual's ppm expectation. Mises says, quite reasonably, that the money-demander's ppm-expectation-formation incorporates his experience. Ppm expectations need not be static. In a fiat money economy with chronic inflation, the informed individual will make ongoing downward adjustments to his expected ppm. He can also factor in changes in the expected trend of inflation following central bank announcements or his own diagnosis of likely movements in supply or demand. In the case where a commodity standard is suspended (for the duration of a war, say) but resumption is expected, expectations of the future ppm and hence the path of the actual ppm will be accordingly constrained.

While all *governments* that have successfully launched new irredeemable monies have first given them a fixed redemption value, this is not true of the private irredeemable digital medium of exchange known as Bitcoin. A Bitcoin is basically a unique digital string of characters kept on one's hard drive or other storage medium. There are currently about 12 million Bitcoins in existence. The dollar value per unit changes daily. On December 1, 2013, Bitcoin traded at just above $958 per

unit, making the total stock equivalent to more than $11.8 billion. (The latest value of Bitcoin and other crypto-currencies can be tracked at https://coinmarketcap.com/.) As I understand it, the Bitcoin system produces new units at a preprogrammed diminishing rate, set to max out eventually at 21 million units, and awards new units to "miners" whose computers solve math problems generated by the system. Bitcoin units seem to have no ordinary commodity use (no value as anything other than a medium of exchange) and are not redeemable claims to anything. Bitcoin seems to have lifted itself by its own bootstraps from nothingness to a positive purchasing power.
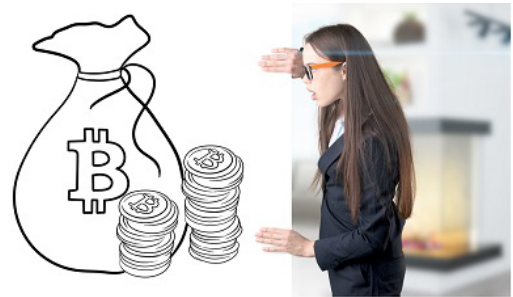


It is accordingly a challenge to account for Bitcoin (its acceptance as a medium of exchange with a positive purchasing power) using Mises's regression theorem. But, as Robert P. Murphy (2013a, 2013b) has argued, it would be a mistake to believe that if Bitcoin doesn't seem to fit the regression theorem then Bitcoin cannot possibly become money. Bitcoin is already a medium of exchange, that is, some people do trade for Bitcoins in order to use them to purchase goods and services. All that Bitcoin needs to become *money* (a commonly accepted medium of exchange) is wider acceptance, which the regression theorem does not rule out. (See also Šurda 2013.)

Two responses to the challenge seem possible. One is to say that the historical component posited by the regression theorem is *not* strictly necessary to explain the purchasing-power expectations people initially formed for Bitcoin. The historical component is important to the initial medium-of-exchange value of a good that *did* have a market value the previous day as an ordinary commodity, or as a redeemable claim, but it cannot be important to a new medium of exchange that had neither. In such a case purchasing-power expectations must arise

entirely from forward-looking speculation. Early adopters who paid positive numbers of dollars (or traded pizzas or devoted CPU time) to acquire Bitcoins did so because they believed that it might attain a higher dollar value in the future. In this account, the value of Bitcoin is basically a bubble, a self-feeding phenomenon unanchored by fundamentals. The trouble with a bubble story, of course, is that is consistent with *any* price path, and thus gives no explanation for a particular price path. Consistent with the bubble story, some Bitcoin-imitator crypto-currencies have crashed to zero after trying to launch into positive value.

The other possible response is to preserve the universal applicability of the regression theorem by saying that Bitcoin must have been a useful commodity to some people before it became a medium of exchange. As Murphy (2013b) puts a version of this case, it could be argued that "the very first people to trade for it did so because it provided them with *direct* utility because they knew there was at least a chance that it would serve to chafe the governments of the world with their printing presses.… [T]he early adopters of Bitcoin were doing it for ideological reasons, not for pecuniary reasons." Then, once it had an observable positive price, "it was off to the races in terms of standard Misesian theory." This scenario, however, does not deliver what the argument requires, namely, an account of how Bitcoins initially had a positive value *apart from their actual or prospective use as medium of exchange.* The value at every point in this scenario derives entirely from use or prospective use as a medium of exchange (only such use as a dollar competitor is what might "chafe the governments," not the existence of untraded digital character strings). The ideological-value story, like the bubble story, does not tell us what the value *per Bitcoin* might be but is consistent with any arbitrary value. It does not explain why, in the fabled first trade of Bitcoins for goods in 2010, the transactors settled on 10,000 BTC for two Papa John's pizzas (Mangu-Ward 2013), or why anyone else took that price seriously as a basis for forecasting the next day's Bitcoin purchasing power.

But perhaps there is no explanation for the exchange rate in the first Bitcoin transaction other than whimsy. (Did the transactors consult the electricity costs of mining one Bitcoin at that time?) Perhaps what allowed other traders to begin taking Bitcoin prices seriously was the observed wiliness of many people to engage in similar whimsies, including exchanging dollars for Bitcoins, which gave a historical component to expectations of a positive purchasing power for one Bitcoin.

**Mises's Understanding of Fiduciary Media and Free Banking**

In *The Theory of Money and Credit* (1981, p. 299) Mises clearly explained how it is not generally feasible to hold a fractional reserve behind redeemable claims to ordinary commodities, but it is feasible in banking. A firm that issues 1000 tickets reading "redeemable on demand for one loaf of bread" will have to have to be prepared to hand over 1000 loaves of bread, because people who want to eat bread will redeem them. Bread tickets cannot satisfy hunger without being redeemed. But trusted claims to silver or gold coin are different: they can take the place of coins as media of exchange. To the extent that sellers in the marketplace accept payment in a bank's notes (or checks), holders of the notes (or checking account balances) need not redeem them for coin before heading to the marketplace. Thus the trusted banker who issues 1000 banknotes can prudently count on only a fraction of them, and possibly a very small fraction, being redeemed on any given day. In Mises's words (p. 300), the trusted banker "is therefore in a position to undertake greater obligations than he would ever be able to fulfill; it is enough if he takes sufficient precautions to ensure his ability to satisfy promptly that proportion of the claims that is actually enforced against him." The banker does of course face the problem of accurately estimating the percentage of claims that will actually be redeemed, but "prudent and experienced" bankers "usually manage pretty well with it" (p. 362).

Mises understood and explained the limits on the volume of bank-issued money, or what he called "fiduciary media" (banknotes and checkable account balances in excess of specie reserves), much better than the 19th-century

Banking School or Currency School. In the tradition of Henry Thornton (1802) and other "Bullionists," he showed that the Banking School's "real bills doctrine" erred in supposing that the banking system could not over-expand by lending on the right kind of collateral. A banking system acting in unison can always lend more by lowering its interest rate on loans. Contrary to Banking School doctrine, "the quantity of fiduciary media in circulation has no natural limits. If for any reason it is desired that it should be limited, then it must be limited by some sort of deliberate human intervention—that is by banking policy." (p. 346).



**Ludwig von Mises**

But Mises immediately added that a natural limit is absent *only when a uniform interest rate policy is followed by all banks*. Otherwise the banks that expand by lending more at lower rates will be restrained by their losing reserves to banks that don't. This means that a natural limit is absent *only when the entire banking system is cartelized or directed by a central bank*. Under free banking competition, by contrast, any bank "will be able to circulate more fiduciary media only if there is a demand for them even when the rate of interest charged is not lower than that charged by the banks competing with it" (p. 347). Under

free banking there *is* a natural limit to the volume of fiduciary media.

Indeed, free banking competition compels the banks "to increase and decrease their circulation *pari passu* with the variations in the demand for money, so far as the lack of a uniform procedure makes it impossible for them to follow an independent interest policy." This has an unintended benefit: "But in so doing, they help stabilize the objective exchange value of money" (p. 347). The Banking School had vaguely recognized that the money stock is self-limiting in a competitive system, but they misunderstood the cause (it is gold redeemability working through the clearing system, not the properties of "real bills"). By failing to understand that the quantity of money is self-regulating only under free banking, the Banking School failed to grasp the danger of overexpansion of money under a central-banking system absent some deliberate policy constraint.

The costliness of expanding a bank's clientele, or the costliness of reserve losses for a bank that issues additional redeemable liabilities without expanding its clientele, means that it is incorrect to characterize fiduciary media as "essentially costless to produce" or to speak of credit as "gratuitous" under redeemability and competition. The quantity of fiduciary media lacks natural limits only under central banking (or, implausibly, a cartel that institutes a uniformly expansionary policy).

Mises's clear understanding of the natural limits under free banking means that we should regard his recommendations – in the last chapter of Part III of the book, and in the Part IV that was added in 1953 – for "legal limitation of the issue of fiduciary media" as second-best remedies for a polity that will not get rid of its central bank and reinstitute free banking. He writes there about the need for legal restrictions to limit the fiduciary issues of a central bank, not of any need – there is no such need in his view – to artificially limit the issues of a free banking system.

# MISES AND HIS FIRST-BEST OPTION

## by Jörg Guido Hülsmann

*he Theory of Money and Credit* reinvigorated the two central assertions of classical economics, that: (1) the wealth of nations depends on nonmonetary factors; and (2) it cannot be increased by stimulating the production of money through political interference. But the mainstream of 20th-century economics did not follow Mises. It followed Irving Fisher and John Maynard Keynes. This is precisely why it is still important and inspiring to read Mises today. Lawrence White is right on target in praising *The Theory of Money and Credit* as an "intellectual treasure chest" that is "worth reading more than once." He draws our attention to Mises's regression theorem and to his favorable assessment of the role of competition among banks of issue. We shall follow him onto these grounds, with some more emphasis on free banking.

**The Regression Theorem and Bitcoins**

Related Links:

- [Friedrich von Wieser (1851–1926)](#)

Mises developed the regression theorem based on the work of Friedrich von Wieser (Wieser 1929; Hülsmann 2007), who himself could rely on the 19th-century German literature (see Gabriel 2012). Contrary to Wieser, Mises argued in terms of demand and supply, and he later emphasized that the demand for money was not just derived from the subjective value of nonmonetary goods, but from the subjective value of the cash balances themselves.



**Friedrich von Wieser**

From an epistemological point of view, the regression theorem does not seem to be an element of Misesian praxeology. It does not concern an *a priori* causal relation (see Hülsmann 2006, North 2012). The subjective value of money depends on the expected future purchasing power of money (PPM), but these expectations are *not necessarily* based on the prior PPM (see Hülsmann 1996, pp.168–71). Moreover, as the case of Bitcoin shows, the subjective value of a medium of exchange need not be based on its expected future PPM at all.

On the question whether Bitcoin initially had a nonmonetary value, I therefore think that Robert Murphy (2013) has a better case than Lawrence White. By contrast, White correctly points out the difficulty of the quantitative determination of the initial Bitcoin value. He offers an initial-whim explanation. This is plausible. Indeed, whimsies are implied in the ideological nature of Bitcoin's nonmonetary value component.

This ideological component is essential not only for Bitcoin's initial value, but also for its survival. All media of exchange need a nonmonetary value component. Otherwise their future purchasing power is indeterminate, and they would be driven out of the market when that future purchasing power is – for whatever reason – widely expected to decline (Hülsmann 1996, pp. 263ff). The nonmonetary component of precious metals is well known. The nonmonetary component of fiat monies is the threat of violence (Kusnetzov 1997). The value of U.S. dollars ultimately derives from the fact, known to all users,

that U.S. citizens will be coerced into accepting dollars as legal tender for all debts public and private. This provides a rock bottom to the value of U.S. dollars. What is the rock bottom of Bitcoin? Presently it is antistatist ideology. If ever the ideology vanishes, something else will have to take its place. At present, it is not clear what that could be.

**Free Banking**

Contrary to mainstream monetary thought, Mises insists that changes in the supply of and demand for money do not benefit the economy as a whole. This central idea runs through his monetary thought from beginning to end.

In the third part of *The Theory of Money and Credit*, he refutes one by one the claims purporting to show that the creation of fiduciary media by fractional-reserve banks could be beneficial from an overall point of view. Mises showed that credit granted in the form of fiduciary media was *not* "true credit"; that the issue of fiduciary media was *not* "elastic" in the sense that it accommodated changes in the demand for money at a constant price level; and that the issue of fiduciary media was in principle unlimited. It was also in that third part of his book that he presented his new crisis theory. The point was that not only are fractional-reserve banks useless from an overall point of view, but they are also in fact harmful to the economy. In conclusion he adopted the policy recommendation of the Currency School (see Hülsmann 2000, Hülsmann 2012, Salerno 2012), arguing that any further creation of fiduciary media should be outlawed.

Starting with the second edition, Mises began to highlight the beneficial role that competition might play in limiting the issue of fiduciary media. I thoroughly disagree with Lawrence White that this was Mises's first-best policy, whereas the outlawing of additional fiduciary media was just a second-best option. It is exactly the other way round. Mises thought the outlawing of additional fiduciary media was the straightforward solution. But the government did not wish to go that way because it had an interest in perpetuating the practice of artificial money creation. It wished to control the banking system in order to channel funds into the public purse, and it imposed

central banks upon the market to facilitate the extension of fiduciary media. Compared to *that* policy, Mises preferred not to regulate fractional-reserve banks at all.

But even this stance was not categorical. After all, fractional-reserve banks have an interest in agreeing on uniform policies to facilitate credit expansion. They do not need governments and central banks in this regard (see Mises 1912, p. 426; 1953, p. 397). The long-run implication is patent: "The quantity of fiduciary media in circulation has no natural limits. If for any reason it is desired that it should be limited, then it must be limited by some sort of deliberate human intervention – that is by banking policy" (1912, p. 360; 1980, p. 346). Mises therefore qualified his endorsement of free banking, admonishing his readers not to conflate it with an endorsement of fractional-reserve banking. The prudent approach was to continue watching the banking industry and act accordingly: "If it should prove easier now for the credit-issuing banks to extend their circulation, then failure to adopt measures for limiting the issue of fiduciary media will involve the greatest danger to the stability of economic life" (Mises 1924, p. 410; 1980, p. 439).

Therefore, in spite of his favorable reconsideration of free banking, Mises still maintained the general conclusion of the first edition, in which he advocated the legal interdiction of any further issues of fiduciary media.

# MISES, THE REGRESSION THEOREM, AND FREE BANKING

## by Jeffrey Rogers Hummel

My thoughts about The *Theory of Money and Credit* by Ludwig von Mises are inevitably colored by the fact that my first introduction to the discipline of economics was at Grove City College in 1967 in a class taught by Hans Sennholz, a devoted student of Mises. All of us who studied under Sennholz were encouraged to read Mises, and I soon tackled *The Theory of Money and Credit*. The only

other book on monetary theory I had read before that was Murray Rothbard's *What Has Government Done to Our Money?* (1964). Consequently, Mises's delving into advanced and somewhat abstruse monetary controversies and his critiquing of other authors with whom I was entirely unfamiliar left me a bit lost, particularly since I was a history major. It is only with subsequent study over the intervening years that I came to understand fully Mises's contributions. I therefore find myself in general agreement with Larry White's appreciation of *The Theory of Money and Credit*. So I will focus my comments on two of the issues he discusses, offering a few elaborations, reservations, and unresolved questions.

**Fiat Money and the Regression Theorem**

Mises employed his Regression Theorem to explain the acceptance of fiat moneys as media of exchange. According to Larry, "[A]ll governments that have successfully launched new fiat monies have first given them a fixed redemption value in terms of a commodity or an established fiat money." Yet if we interpret the term "redemption" strictly, this is not quite historically accurate. Among the earliest fiat moneys in the West were those issued by the British colonies in North America. Rarely if ever were these "bills of credit," as they were called at the time, *directly and immediately* redeemable for gold or silver at a fixed rate. Instead, they usually were *indirectly* linked to commodity money at a fixed rate through taxes (and often also through legal tender laws that applied to debts enforced in the courts). In other words, unlike Federal Reserve notes, which initially could be exchanged for gold before they became fiat money, bills of credit were denominated in the colony's unit of account and could be used to pay taxes and other government levies in lieu of the monetary commodity. But bills of credit were rarely immediately redeemable, although they were often accompanied by a promise that at some future date they would become so (Michener 2011).



**Ludwig von Mises**

This reliance on taxation and future redeemability applies also to the Continental currency issued by the U.S. during the American Revolution, to the Greenbacks issued by the Union during the Civil War, and to the currency issued by the Confederacy during the same conflict. Indeed, Confederate currency, unlike Union Greenbacks, was not made legal tender in private transactions but only publicly receivable for taxes. George Selgin's extensive treatment of fiat money's emergence (1994, 816–21), which invokes Mises analysis, is somewhat difficult to interpret. On the one hand, he seems to require only a fixed exchange rate rather than strict redeemability for successful launching of fiat money, thereby encompassing the historical instances I have mentioned. And when George explicitly rejects taxes as a sufficient mechanism, he appears to be referring to a new fiat money using an entirely new unit of account, completely unrelated to and floating against the existing commodity money. However, he also claims that "[p]ublic receivability laws can confer value on a new money only" if "there be real resources at the disposal of government to be exchanged for the new money at administered prices," which was hardly the case with bills of credit, Greenbacks, and Confederate currency. Whatever George's position, Mises was clearly wrong when he stated that fiat money emerges exclusively "when the coins or notes in question have *already* been in circulation

as money-substitutes," by which he meant "perfectly secure and *immediately* convertible claims" (emphasis mine; 1953, 78).

These historical counterexamples are particularly interesting because Mises, in other passages, inadvertently describes them. He divided what he called "money in the narrower sense" (or what has come to be called *outside money* in modern monetary theory) into not just two categories: commodity and fiat money. He introduced a third category, which in the English translation was designated "credit money." *The Theory of Money and Credit* defined credit money as "a claim against any physical or legal person," but one that "must not be both payable on demand and absolutely secure.… Credit money … is a claim falling due in the future that is used as a general medium of exchange" (1953, 61–62). Mises continued to posit these three categories in the final edition of *Human Action* (1966, 429). He considered the notes of banks that had temporarily suspended redemption in specie (gold or silver coin) as one example of credit money, but irredeemable moneys launched through taxation fit the category equally well. In fact, Mises (1953, 153) referred to Confederate currency as a specific example of credit money. *The Theory of Money and Credit* went so far as to speculate that most forms of so-called fiat money might in reality be credit money: "Whether fiat money has ever actually existed is, of course, another question, and one that cannot offhand be answered affirmatively. It can hardly be doubted that most of those kinds of money that are not commodity money must be classified as credit money. But only detailed historical investigation could clear this matter up" (1953, 61).

Outside of Sennholz in his lectures, the only Austrian economist (to my knowledge) who has pursued Mises's distinction between fiat and credit money, albeit cursorily, is Joe Salerno (2010, 68–70, 586). Yet this distinction may be more than a terminological quibble. One recent development in monetary theory is the Fiscal Theory of the Price Level (FTPL), most ably advocated by John Cochrane (2005) of the University of Chicago. It is a highly mathematical attempt to formally integrate the impacts of both monetary policy and fiscal policy, and one of its crucial underlying assumptions is that the current value of fiat money depends on people's expectations of the government's future taxes. In other words, it explicitly treats fiat money as what Mises called credit money, and moreover contends that future expected taxes have a significant effect on money's velocity (that is, the reciprocal of the portfolio demand for money) and therefore on the price level. In this respect, the FTPL is building on an extensive literature arguing that expected taxes played the major role in determining the price level in the early fiat (credit) money issues in America. (Smith 1984,1985a, 1985b, 1988; Wicker 1985; Calomiris 1988a, 1988b; critiques of this literature include Michener 1988; McCallum 1992; and Sumner 1993; for surveys of the debate see Michener and Wright 2006 and Michener 2011.) It is also consistent with Michael Sproul's (2003) much cruder "backing theory of money," which denies the existence of fiat money altogether and about which Sproul (2013) is currently debating Kurt Schuler at the Free Banking blog.

The FTPL does not necessarily undermine the validity Mises's Regression Theorem as an explanation for the launching of fiat (or credit) money. Nor is it entirely inconsistent with a sophisticated interpretation of the quantity theory of money. Yet it has some far more intriguing implications. Tyler Cowen once emailed me that the FTPL "is formally correct but not so useful; just a way of re-explaining the traditional boxes," while Greg Mankiw in personal conversation similarly described it as merely "a new vocabulary." But I believe they are both mistaken. If correct, the FTPL implies that neither fiat nor credit money are true outside money in the sense of being assets only, with no offsetting liability. Instead they are really what current monetary theorists refer to as *inside money*, with future taxes representing the offsetting liability-side, making them much like shares of stock, whose value depends on an anticipated future income stream. Not only does this conclusion eliminate any real-balance effect that can result from fiat or credit money constituting net wealth (unlike commodity money), but it impinges on the long-standing debate over whether a pure inside-money economy would be feasible. Initiated

by Knut Wicksell (1936) and addressed by Don Patinkin (1965), this debate was part of the wide-ranging development during the late 1980s and early 1990s of the New Monetary Economics, with Leland Yeager (1997), Tyler Cowen and Randy Kroszner (1994), and George Selgin and Larry White (1994) major participants. I am not myself convinced that the FTPL is correct, but it deserves more attention and discussion than those influenced by Mises's *Theory of Money and Credit* have given it.

**Mises and Free Banking**

Larry credits *The Theory of Money and Credit* with "putting the analysis and conclusions of the 19th-century Free Banking School on firmer footing." Here he is reiterating his interpretation in an earlier article that coincidentally appeared in a Sennholz *festschrift* (White 1992). Larry was somewhat more cautious in that version, admitting that the first edition of *The Theory of Money and Credit* "endorsed free banking mainly by implication" before drawing upon Mises's subsequent writings to buttress his conclusion. Undoubtedly Mises, like Sennholz, favored unregulated banking and, by the last edition of *Human Action* (1964, 443), was skeptical of any legal imposition of 100 percent reserves, as advocated by Rothbard. But we must carefully distinguish between favoring free banking as a legal regime and predicting how it would operate in practice. I think Larry goes too far when he seems to imply that Mises had in mind the kind of free banking that he (1999) and George (1988) predict would emerge without regulation: that is, a system in which reserve ratios are extremely low and banks adjust the money supply to demand in a way that stabilizes velocity. As much as I may agree with their prediction, I can assure them that Sennholz repeatedly affirmed his belief that unregulated competition among banks would drive reserve ratios up very high and possibly close to 100 percent, and he left the impression that such was Mises's opinion as well.
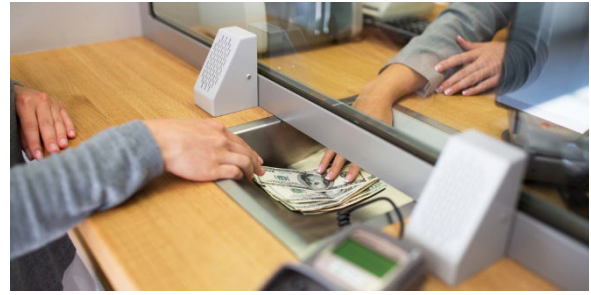


Mises himself was never entirely clear whether or not he advocated free banking as a means of approximating Rothbardian ends, neither in *The Theory of Money and Credit* nor in *Human Action*. Yet revealing is his division of "money substitutes" (i.e., inside money) into "money certificates," that portion 100 percent redeemable for outside money, and "fiduciary media," the remaining portion exceeding the amount of outside money. The passage that Larry cites where Mises (1953, 312 [note my pagination is different from Larry's because I am using a different printing of the same edition]) admits that free banking might "help stabilize the objective exchange-value of money" appears to be no more than a minor concession to the Banking School's penchant for currency elasticity rather than an expression of something Mises found economically desirable. And in *Human Action* (1966, 443) he evinced an unambiguous desire to keep fiduciary media tightly constrained: "Free banking is the only method available for the prevention of the dangers inherent in credit expansion. It would, it is true, not hinder a slow credit expansion, kept within very narrow limits, on the part of cautious banks…. But under free banking, it would have been impossible for credit expansion to have developed into a regular—one is tempted to say normal—feature of the economic system. Only free banking would have rendered the market economy secure against crises and depressions."

The key phrase, "credit expansion," did not appear in *The Theory of Money and Credit* until Mises added Part Four to the 1952 edition. But it relates to Austrian business cycle theory, which Mises had first sketched out in the original edition of that work. I have never been entirely comfortable with this theory (Hummel 1979), despite believing that it contains some penetrating insights. Mises defined credit expansion as any *net* increase in fiduciary

media in *Human Action* (1966, 434). Although he earlier had conceded that such increases through fractional-reserve banking had, as world output grew, prevented the "undesirable consequence" of a "tremendous increase in the exchange-value of money" (i.e., significant secular deflation), with its "additional harm" of increasing commodity money's resource cost (1953, 298–99); in the final edition of *Human Action* he is insisting that "the trade cycle" can arise in a "pure market economy" from *any* credit expansion (1964, 573–74). This tension simply highlights one major difficulty with Austrian business cycle theory, no matter which variant from its assorted advocates we examine. It is a theory that hinges on specifying two firm dividing lines: (a) between those financial instruments that constitute inside money and those comprising what Mises considered genuine manifestations of people's savings, and (b) between those increases in the money stock, however defined, that generate a self-reversing boom and those that do not. Alas, after myriad attempts, no consensus has emerged on either question among Austrian economists.

## MISES WAS LUKEWARM ON FREE BANKING

### by George A. Selgin

I first read *The Theory of Money and Credit* in the spring of 1981. I ought to have been working on a master's degree in resource economics at the University of Rhode Island. But I'd had my fill of Hotelling's Rule and was itching to broaden my economic horizons.

Having read many general economics classics while I was an undergrad, I decided to start taking on works dealing with specific fields. Inflation was in the headlines, and monetary theory sounded challenging, so my plan was to start with it and then move on to easier stuff. That I'd read a couple dozen books before yanking *The Theory of Money and Credit* off the shelf turned out to be a lucky break: had I read it too soon, I'd certainly have found it dense, if not impenetrable. Instead, comparing it to everything I'd managed to glean from the other books, I

never doubted that, despite its age, it towered over the rest. Indeed, though I've read hundreds of other books about money since, including some awfully good ones, none has had a greater influence upon my own thinking on the subject.
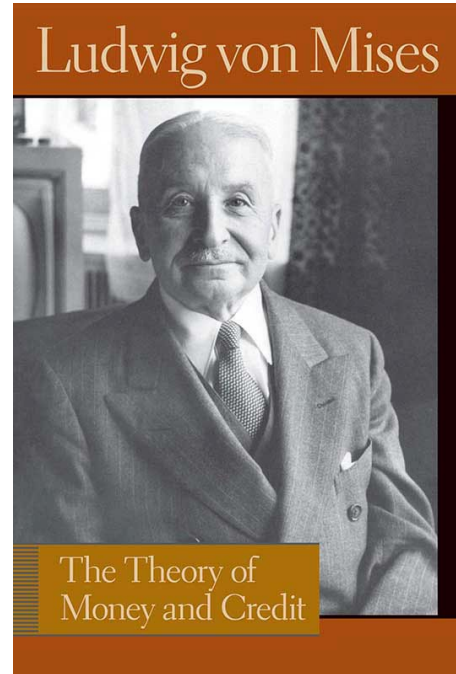


### The Regression Theorem

Like Larry, I regard Mises's treatment of the determination of money's purchasing power, and the "regression theorem" that plays the central part in that treatment, as one of his book's most important contributions. As Israel Kirzner makes clear in an excellent, though (so far as I'm aware) never-published, essay on the regression theorem, Mises's approach was unique, not so much because of the particular explanation he offered of how agents come to form their expectations regarding an inconvertible money's purchasing power, but because he realized that no theory of a fiat money's equilibrium value could be considered complete unless it accounted for the people's willingness to treat some particular paper medium, not as so many mere bits of paper, but as an economy's generally accepted medium of exchange, to which a positive exchange value might reasonably be assigned.[1]Many other theorists—and Don Patinkin, most notoriously of all— simply failed to recognize the challenge posed by the presence of what we

would today refer to as "multiple equilibrium solutions" to the value of a (potential) fiat money problem. One solution—the one taken for granted by Patinkin using the Walrasian "tâtonnement" approach—implies a positive equilibrium purchasing power.[2] The other is the one in which a would-be fiat "money" is not actually accepted as such, so that it commands no value at all. Patinkin assumes, in effect, that because agents might humor a Walrasian auctioneer by telling him that they would equip themselves with $n$ units of some would-be paper "money" *if* each unit could buy $q$ baskets of goods, and with $n+x$ units if each could buy $q$-$z$ baskets (and so on), those agents will in fact happily exchange valuable goods for the "money" in question at the determined equilibrium rate. To see the flaw in this approach, one has only to imagine the Walrasian polling being done using stage money. Evidently we need a theory that can account for the fact that one can buy things with actual fiat monies, but *not* with stage monies. Yet Patinkin didn't see this, and neither do most of his readers, even today.

### Bitcoin

The regression theorem itself constitutes, as I've said, but one particular solution—a solution that might now be labeled "backward-looking" expectations-formation. Actual fiat monies are valuable, while stage monies aren't, because the fact that the former have been valued historically makes the "Patinkin" equilibrium more salient, to backward-glancing agents, than the alternative, zero-value equilibrium. For a long time, as Larry points out, I also regarded this theorem as being the *only* practical solution. Consequently I saw in it the key, not only to the history of fiat money (and, in particular, to the "commodity money—redeemable money substitutes—fiat money" pattern of money's historical development), but also to the successful launching of new official monies.[3]

Bitcoin's success has, however, caused me to reconsider my previous understanding of the significance of the regression theorem; for that reason I think it only right that Larry should devote a substantial part of his retrospective to discussing a subject that some may think both esoteric in itself and at most tangential to the subject

of Mises's great work. Bitcoins aren't even "money," after all—not, at least, according to the standard definition, which holds that to be "money" a thing must be, not just an *occasionally* accepted, but a *generally* accepted, medium of exchange. But though Bitcoins aren't money, their story challenges Mises's theorem by suggesting that an otherwise valueless medium might become money despite not having a provenance linking it with some commodity progenitor.

Why, in that case, was anyone willing to be the first person to trade something having a well-established value, whether their labor or a pizza or some other good, for an as-yet useless medium? Here there was no question of a "friendly helix" of backward-looking expectations coming to the rescue. From a backward-looking perspective a Bitcoin was no better than a piece of stage money; the salient equilibrium, viewed from that angle, was the zero-value one, based on the prediction that nothing would change. Yet things did change, so that as of this writing a Bitcoin is worth about $1000. How was this possible?



In a post published at *Freebanking.org* a couple months before Bob Murphy's EconLib essay appeared,[4] I dubbed this question "the oyster problem":

> It is said[5] that the first person to eat an oyster had to have been exceedingly brave or exceedingly crazy or some of each. But that primordial mollusk eater had nothing on the first, equally anonymous person to receive bitcoins in exchange for valuable merchandise, in the hope of somehow fobbing them off in turn on others. The earlier pioneer might, after all, have simply taken his cue from a seagull or oyster-catcher. Unlike the rise of bitcoin's network, that of various past money commodities like tobacco, cowries, and salt poses no puzzle: whoever first toyed with accepting such commodities for

goods could count on the existence of persons who desired the commodities in question for their own sake, even if no one else was prepared to hazard their employment as exchange media…. The first person to accept bitcoins in exchange, in contrast, couldn't hope to smoke them, make them into a nice bracelet, or sprinkle them on his food, in case he couldn't trade them away: he (or she) could *only* hope that someone else would attempt a similar leap of faith, or face the consequences of trading some useful goods or service for so many units of digital dross.[6]

I went on to ask persons having intimate knowledge of Bitcoin transactions to submit their own explanations concerning how Bitcoin solved the oyster problem, and received a bunch of intriguing replies.[7] My own preferred theory invoked what I called "expressive exchange"—a counterpart to the "expressive voting" solution to the paradox of voting—a solution that treats voting as a source of direct satisfaction, like cheering on one's favorite sports team while watching them on TV. The person who accepted Bitcoins for that first pizza did it, in other words, because he liked the *ideas* Bitcoin stood for, and wanted to root for them.

As I said, I liked the theory. But now I realize I wasn't giving the Bitcoin team enough credit. Records show that a just a few persons took part in most early Bitcoin transfers, and especially in the larger-volume ones. My guess is that they all knew each other, and that those trades were more-or-less fictitious, with large values being traded and then traded back again, with the intent of enhancing the prominence of the positive-value equilibrium by drawing attention away from the much larger set of inactive Bitcoin markets. Bitcoin's inventors, I'm now almost certain, were making conspicuous leaps onto their own bandwagon, so as to encourage others to do so, whether to express themselves or to profit by doing so. In short, a clever marketing strategy, including a little strategic sleight-of-hand, can substitute for history in putting a positive sign on the expected value of an otherwise useless potential exchange medium.

**Criticisms**

Mises, of course, can hardly be faulted for not having anticipated a possibility that has come as a surprise even to those of us who have watched it unfold. *The Theory of Money and Credit* does, however, suffer, in my humble opinion, from some serious shortcomings. One of them stems from Mises's refusal to employ raw statistics, let alone econometrics of any sort, to bolster his claims regarding the merits or drawbacks of alternative monetary arrangements. Statistical measures of such things as money's purchasing power are, admittedly, far from perfect. Nor would they serve any use if alternatives could be judged and compared on strictly *a priori* grounds. Generally, though, *a priori* reasoning alone will not allow one to conclude that monetary arrangement A performs better than arrangement B. In particular, it cannot tell us whether a managed fiat standard is likely to have a more stable purchasing power than a gold standard. Whether it does or doesn't is an empirical matter, and as such it is one best settled by reference to the best available statistics, for such statistics, as crude as they may be, are at least better than mere assertions.

Alas, Mises's disdain for statistics, and especially for statistics purporting to represent "price level" (or the inverse of what Mises's called money's "outer objective exchange value"), caused him to rely upon mere assertions in assessing the relative merits of gold and fiat money, and to do so even when available statistics would have supported his case. This was unfortunate, both because it rendered Mises's particular policy recommendations less persuasive than they might otherwise have been, and because it almost certainly limited the overall appeal of *The Theory of Money and Credit* within a discipline that was becoming more-and-more statistically minded.[8]

Other significant shortcomings of *The Theory of Money and Credit* stem from Mises's failure to rid himself of certain Currency School prejudices: although Mises, unlike many of his contemporaries, was never a doctrinaire exponent of either Banking or Currency School views, he did not succeed, in my view, in completely resisting some Currency School fallacies. In particular, he endorsed the Currency School view that, under a gold standard, a

nation's money stock ought to vary in strict accord with its monetary gold reserves, while blaming business cycles on deviations from this strict pattern. Mises sets out his opinion most clearly in *Human Action*:

> Issuance of fiduciary media, no matter what its quantity may be, always sets in motion those changes in the price structure the description of which is the task of the theory of the trade cycle. Of course, if the additional amount issued is not large, neither are the inevitable effects of the expansion.[9]

The same Currency School prejudice is, however, also implicit in the terminology employed in *The Theory of Money and Credit*, where Mises distinguishes between "commodity credit" and "circulation credit," the first of which refers to credit based on actual savings.[10] It is not difficult to see how even such terminology, not to mention more explicit statements like that quoted above, have been understood by Rothbard and many others as embodying an implicit endorsement of 100-percent reserve "banking" as against any fractional alternatives, including free banking. At very least, it must be said (and here I'm afraid I disagree with Larry) that Mises's defense of free banking was a lukewarm one, based on his (mistaken) belief that free banking would offer no scope for any substantial creation of fiduciary media.

**Endnotes**

[1] Israel Kirzner, "A Note on the Circularity 'Bogey' in the History of the Marginal Utility Theory of Money." Ms., New York University, n.d.

[2] Don Patinkin, *Money, Interest, and Prices*, 2nd. ed. (New York: Harper & Row, 1965), pp. 115–16.

[3] George Selgin, "On Ensuring the Acceptability of a New Fiat Money," *Journal of Money, Credit, and Banking* 26 (4) (November 1994), pp. 808–26, and idem.; "Adaptive Learning and the Transition to Fiat Money," *Economic Journal* 113 (484) (January 2003), pp. 147–65.

[4] Robert P. Murphy, "The Economics of Bitcoin," *Library of Economics and Liberty*, June 3,

2013. https://www.econlib.org/library/Columns/y2013/Murphybitcoin.html.

[5] In fact the common statement misquotes Jonathan Swift, who observed, in dialogue 2 of his *Polite Conversation*, that "He was a bold man that first eat an oyster."

[6] "Bitcoin," *Freebanking.org*, April 22, 2013, http://www.freebanking.org/2013/04/22/bitcoin/.

[7] "A Challenge to the Bitcoin Community," *Freebanking.org*, May 2, 2013, http://www.freebanking.org/2013/05/02/a-challenge-to-the-bitcoin-community/.

[8] See George Selgin, "Ludwig von Mises and the Case for Gold." *The Cato Journal* 19(2) (Fall 1999), pp. 259–77.

[9] Ludwig von Mises, *Human Action* (Chicago: Regnery, 1949), p. 4439n.

[10] *Theory of Money and Credit*, part III, chap. 15, sections 10–12.

# A RESPONSE TO HÜLSMANN, HUMMEL, AND SELGIN

## by Lawrence H. White

I thank Professors Guido Hülsmann, Jeff Hummel, and George Selgin for their thoughtful commentaries. I am broadly in agreement with Hummel's and Selgin's comments, but not surprisingly have some serious differences with Hülsmann's views on banking theory and his interpretation of Mises. (See our previous exchange on free banking in Hülsmann 2003 and White 2003.)

Hülsmann writes that "All media of exchange need a nonmonetary value component." For fiat money, he says, this component is the "threat of violence," meaning the collection of taxes and the enforcement of legal-tender laws. This seems to me an unnecessary concession to the state theory of money. Some important evidence weighs against the view that state enforcement is a necessary

condition for the continued circulation of a fiat money once it has been launched. In particular, the Somali shilling continued to circulate in Somalia even after the state disappeared, ending the state's ability to collect taxes or enforce legal-tender laws (Luther and White 2013).

Interpreting Mises's argument in *The Theory of Money and Credit*, Hülsmann writes: "Mises insists that changes in the supply of and demand for money do not benefit the economy as a whole." In fact, this is true only of a change in the nominal quantity of fiat money. It was Mises's view (and it is my view) that a gold standard is different because monetary gold is costly to produce.

Mises explained how a reduction in the demand for monetary gold due to the development of fractional-reserve banking is beneficial under a gold standard. Consider the ordinary case of the global economy in which the stock of monetary gold continues to rise due to gold mining. Because there is no benefit to the economy from increasing the stock of monetary gold in the world as a whole, the labor and capital sacrificed to produce an increased stock is a cost without a benefit. Banking developments that reduce the demand for monetary gold accordingly benefit the economy by avoiding this cost. With respect to the development of fractional-reserve banknotes and checking accounts as substitute media of exchange in a specie economy, Mises accordingly wrote (*Theory of Money and Credit*, Book III, Chapter 17, para. 4): "If metallic money is employed, then the advantages of a diminution of the demand for money due to the extension of such other means of payment are obvious." Historically, given the growth of real economic activity,

> the tremendous increase in the exchange value of money, which otherwise would have occurred as a consequence of the extension of the use of money, has been completely avoided, together with its undesirable consequences. If it had not been for this the increase in the exchange value of money, and so also of the monetary metal, would have given an increased impetus to the production of the metal. Capital and labor would have been diverted from other branches of

production to the production of the monetary metal.... [In consequence] the welfare of the community would have suffered. The increase in the stock of precious metals which serve monetary purposes would not have improved the position of the individual members of the community, would not have increased the satisfaction of their wants; for the monetary function could also have been fulfilled by a smaller stock. And, on the other hand, a smaller quantity of economic goods would have been available for the direct satisfaction of human wants if a part of the capital and labor power that otherwise would have been used for their production had been diverted to mining precious metals.

Mises went on to note that this resource-cost-saving argument applies to a commodity money regime, but not to a credit money or a fiat money.



The statement just quoted comes from the third part of *The Theory of Money and Credit*. It is thus not correct to write, as Hülsmann does, that "In the third part of *The Theory of Money and Credit*, he [Mises] refutes one by one the claims purporting to show that the creation of fiduciary media by fractional-reserve banks could be beneficial from an overall point of view." In the above-quoted passage Mises makes an important claim of exactly this sort himself.

Likewise it is incorrect to attribute to Mises the (unwarranted) view that "not only are fractional-reserve banks useless from an overall point of view, but they are also in fact harmful to the economy." Compared to a system where payments can only be made in specie or in

transferable titles to specie issued by money warehouses, fractional-reserve banks – far from being useless – provide payment services at lower cost both from the individual transactor's point of view and from the overall point of view.

In my lead essay I credited Mises with improving economists' understanding of how adverse clearings limit the quantity of bank-issued money. Hülsmann, by contrast, attributes to Mises the view "that the issue of fiduciary media was in principle unlimited," which I take to be a paraphrase of Mises's statements (*Theory of Money and Credit* Book III, ch. 17, para. 26) that "[t]he circulation of fiduciary media ... is only elastic in the sense that it allows of any sort of extension of the circulation, even completely unlimited extension," and that "[t]he quantity of fiduciary media in circulation has no natural limits." I quoted the second statement and pointed out that we will miss Mises's key insight that it does not apply to a competitive free-banking system if one ignores, as Hülsmann does, the immediately following paragraph. There -- I quote it again in hopes that it will not be overlooked again -- Mises writes:

> Of course, all of this [lack of a natural limit to bank-issued money] is true only under the assumption that all banks issue fiduciary media according to uniform principles, or that there is only one bank that issues fiduciary media. A single bank carrying on its business in competition with numerous others is not in a position to enter upon an independent discount policy. If regard to the behavior of its competitors prevents it from further reducing the rate of interest in bank-credit transactions, then -- apart from an extension of its clientele -- it will be able to circulate more fiduciary media only if there is a demand for them even when the rate of interest charged is not lower than that charged by the banks competing with it. Thus the banks may be seen to pay a certain amount of regard to the periodical fluctuations in the demand for money. They increase and decrease their circulation *pari passu* with the variations in

the demand for money, so far as the lack of a uniform procedure makes it impossible for them to follow an independent interest policy. But in doing so, they help to stabilize the objective exchange value of money. To this extent, therefore, the theory of the elasticity of the circulation of fiduciary media is correct; it has rightly apprehended one of the phenomena of the market, even if it has also completely misapprehended its cause.

What is the referenced "theory of the elasticity of the circulation of fiduciary media" that has "completely misapprehended" the cause of the demand-elasticity of bank-issued money? It is the pair of erroneous Banking School doctrines known as the Fullartonian "law of the reflux" and the Real Bills Doctrine. The correct apprehension of the cause is the theory of adverse clearings, as partially understood by some Free Banking School writers in the 19th century, better understood by Mises, and as most thoroughly explained in recent years by George Selgin in *The Theory of Free Banking* (1988) and other works.

Under what conditions is it true that "the issue of fiduciary media [is] in principle unlimited"? The issue of *fiat money* is unlimited by economic forces, but "fiduciary media" refers to redeemable bank liabilities and not to fiat money. Redeemable liabilities are not gratuitously issued but are costly for any bank to keep in circulation in a competitive environment. The issue of fiduciary is unlimited only in the analytical limiting case (never historically realized) of a single world banking system with a single issuer of fiduciary media (or a perfect cartel among all issuers) and zero public demand to hold the medium of redemption (which, by the way, is inconsistent with gold being the medium of redemption), so that the issuing bank's risk of reserve loss is zero no matter how great the volume issued. Short of that case, the risk of adverse clearings or over-the-counter reserve losses strictly limits the ratio of bank-issued money to bank reserves to a finite number.

Hülsmann seems to think – despite all historical evidence (Dowd 1993, White 1995, White 2003) – that a perfect

cartel among all banks of issue is a reasonable approximation to historical systems in which banks were free to issue fiduciary media. He even cites Mises in a way suggesting that he thinks that this was Mises's position: "After all, fractional-reserve banks have an interest in agreeing on uniform policies to facilitate credit expansion. They do not need governments and central banks in this regard (see Mises 1912, p. 426; 1953, p. 397)."

This statement is mistaken in three respects. First, if banks could collude, their interest would lie, like any monopolist's or monopoly cartel's, in *restricting* industry output and thereby getting a higher price and a monopoly profit. In banking, this means that a profit-seeking bank cartel would seek to charge *higher* interest rates on loans (and to pay lower interest rates on deposits). To do so requires the cartel to move *up* the industry demand curve for bank loans to a *smaller* volume of loans, which implies a *smaller* volume of liabilities (holding constant banks' other interest-earning assets and non-deposit sources of funds) than under competition. Second, again like firms in any industry, there is no reason to think that banks can successfully collude without government (via the government central bank or otherwise) to enforce the cartel's prices. When the rest of the industry is charging loan rates above the competitive level (the interest rate on deposits plus the marginal cost of intermediation), any individual bank can profit by undercutting the cartel, lending more at a slightly lower loan rate, thereby undermining the cartel's attempt to fix the loan rate. Third, Mises quite clearly found it absurd to suppose that a banking industry could collude to bring about a uniform proportional expansion in every bank's liabilities, so as to avoid the adverse clearings that constrain any non-uniform expansion. Mises noted in *Human Action* that the self-interest of reputable banks lies in not cooperating with less responsible banks. He wrote (Mises 1966, ch. 17, para. 159):

> But, some people may ask, what about a cartel of the commercial banks? Could not the banks collude for the sake of a boundless expansion of their issuance of fiduciary media? The objection is preposterous. As long as the public is not, by

government interference, deprived of the right of withdrawing its deposits, no bank can risk its own good will by collusion with banks whose good will is not so high as its own.… Under free banking a cartel of banks would destroy the country's whole banking system. It would not serve the interests of any bank.

Let me turn to Jeff Hummel's contribution. Hummel notes that historically some governments have launched new monies without first giving them a fixed spot-redemption value, by promising future redemption in gold or receivability for taxes. These, he rightly notes, are "credit money" in Mises's terminology (as translated). He considers this a correction of my statement that the launching of new government fiat monies has always proceeded by ending redeemability of a previously spot-redeemable money. I do not dispute these cases, and I would have worded my statement differently if I had thought about them. However, Hummel and I are not in disagreement, because when I said fiat money, I meant fiat money, not credit money.

Hummel cautions that my essay "goes too far when he seems to imply that Mises had in mind the kind of free banking … system in which reserve ratios are extremely low and banks adjust the money supply to demand in a way that stabilizes velocity." Mises did of course emphasize the *restraint* that free banking imposes on the volume of bank-issued money. Hummel refers to Mises's "unambiguous desire to keep fiduciary media tightly constrained," and appropriately quotes *Human Action* (Mises 1966, p. 443): "Free banking is the only method available for the prevention of the dangers inherent in credit expansion."

I see no difference between Mises's view and my own view in that regard – I also desire a system that keeps the volume of money tightly constrained to its warranted volume.
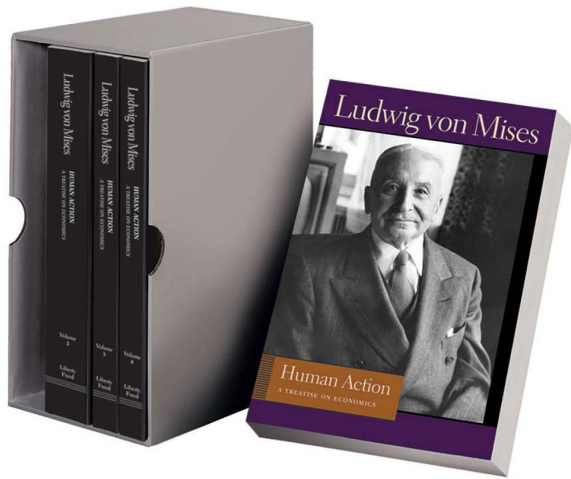
But Mises in *Human Action* (p. 446) does quote Cernuschi to the effect that free banking would have narrowed the use of banknotes considerably, and in other ways suggests that reserve ratios under free banking would be, as Hummel puts it, "up very high and possibly close to 100 percent." If that is Mises's prediction, then on this point I do depart from Mises. In my 1992 essay that Hummel cites, I criticized Mises for suggesting that free banking would produce reserve ratios close to 100 percent. The best historical evidence we have, from the Scottish free-banking system and other mature systems, shows reserve ratios below 10 percent. This appears to be another instance of the point Selgin made in his contribution: that an empirical question of magnitude cannot be settled by a priori reasoning. We need to look at the historical evidence.

Hummel highlights Mises's division of bank-issued money into "money certificates" and "fiduciary media." According to this conceptual scheme, if a bank issues 100 in banknotes against 10 in reserves and 90 in loans, the first 10 banknotes are "money certificates" and the remaining 90 are "fiduciary media." I am puzzled by this way of dividing things. It seems anti-subjectivist, because from the point of view of a banknote's holder, the "last" banknote issued by a bank with fractional reserves is identical to the "first." It makes more sense to distinguish inside money from outside (base) money, or equivalently, bank-issued money from reserve money.

Hummel goes on to quote Mises's statement that free banking "would, it is true, not hinder a slow credit expansion, kept within very narrow limits, on the part of cautious banks."

It isn't entirely clear what proposition Mises is advancing in this sentence. Under a constant set of parameters, the equilibrium quantity of bank-issued money is determined. A slow credit expansion requires a slow change in one or more of the parameters. But exactly what parametric change is Mises supposing? A slow secular decline in the cost of topping up low reserves on short notice? A slow rise in the fraction of the public prepared to hold bank-issued media? If one of these two secular trends is assumed, then I am in full agreement the proposition that a free banking system would respond with an appropriately slow expansion in bank-issued money.

Turning now to George Selgin's comments, I agree with most of what Selgin has to say about the regression theorem and Bitcoin. I would reemphasize the point I tried to make in my lead essay, that while Bitcoin's appeal to antistatists in undeniable, that link does not determine the *magnitude* of the real purchasing power of one Bitcoin in the same way as a "rock-bottom" (to use Hülsmann's term) purchasing power of gold is determined by the intersection of the supply curve with the nonmonetary demand curve for gold. To know where to draw the demand curve for real Bitcoins, we would have to suppose that the ideological desire is not merely to participate in holding a nonstate medium of exchange, but to hold *so many dollars-worth*.

Selgin's hypothesis that Bitcoin's founders were "making conspicuous leaps onto their own bandwagon, so as to encourage others to do so," and in so doing were pursuing "a clever marketing strategy," seems plausible, but awaits historical documentation. Since there is a public record of all Bitcoin transactions, it should be possible to discover whether there was an in-group pattern of offsetting trades in the early transactions. If true, we should expect to see the same pattern in the launchings of the 70 other cyber-currencies (listed at <https://coinmarketcap.com/>) that emulate Bitcoin, the largest of which is Litecoin.

Needless to say, given my own efforts (White 2013, 2014) to show that the gold standard has outperformed fiat

money as judged by price indices and other historical statistics, I agree with Selgin that "reference to the best available statistics" trumps "mere assertions."

Finally, I agree with Selgin that Mises was far too sweeping when he wrote that "Issuance of fiduciary media, no matter what its quantity may be, always sets in motion those changes in the price structure the description of which is the task of the theory of the trade cycle." As I noted in a footnote in my 1992 essay, an increase of bank-issued money that matches an increase in the demand to hold bank-issued money is not disequilibrating but the reverse. It prevents a liquidity spillover from the market for money balances to the market for loanable funds, and thereby prevents a disequilibrating rise of the market interest rate above the natural rate, rather than causing the market rate to fall below the natural rate.

## WHAT MISES SAID

### by Jörg Guido Hülsmann

In his response to my comment, Professor White focuses on two questions pertaining to Mises's theory of banking:

1. whether Mises thought there were advantages to the economy as a whole if the demand for monetary gold diminishes due to the development of fractional-reserve banking; and

2. whether Mises thought that the production of fiduciary media tends to be limited under fractional-reserve banking.

Lawrence White believes my interpretation of Mises to be wrong, at any rate as far as these two points are concerned. Let me therefore address them in turn.

(1) It is correct that the development of fractional-reserve banking tends to diminish the demand for base money under a gold standard. It is correct that Mises, in the second and third parts of *The Theory of Money and Credit*, highlighted that this tendency implied that more original factors of production could be devoted to the production of other goods. And it is also correct that, in the first

edition (1912), Mises considered this tendency to be beneficial from an overall point of view.

However, he thoroughly reconsidered his position in later works, especially in second edition of *The Theory of Money and Credit* (1924) and again in the fourth part added to the 1953 American edition. More precisely, while he still acknowledged that the development of fractional-reserve banking tends to diminish the production of gold, he no longer held this to be beneficial.

In the concluding chapter of the third part (chap. 20, III, sections 9 and 10) he now stated that the (fractional reserve) gold-exchange standard was pointless because it did not effectively rein in monetary interventionism. The only alternative was either to go the full way to fiduciary media or to return to the actual use of gold in daily exchanges. He clearly opted for the latter alternative. (See Mises 1924, pp. 403f; 1980, pp. 432f.)



**Ludwig von Mises**

He explains his reform plan in more detail in the fourth part added to the 1953 edition. But the objective is the same as in 1924, namely, the establishment of an effective gold-coin circulation. He wrote: "Gold must be in the cash holdings of everybody. Everybody must see gold coins changing hands, must be used to having gold coins in his pockets, to receiving gold coins when he cashes his paycheck, and to spending gold coins when he buys in a store." (1980, p. 493)

Let us now step back and consider what this means. Mises did not change his analysis of the resource-cost-saving *mechanism*. The development of fractional-reserve banking tends to diminish the production of gold, and this liberates labor and land resources that can now be devoted to other production projects. But this tendency no longer appears to be *beneficial*. The "resource-cost-savings" made possible by fractional-reserve banking now appear to be as foolish as the resource-cost-savings of building a car without breaks or shock absorbers.

In other words, Mises had come to the conclusion that the resource costs of gold production were worth the while. In *Human Action* he recognized that he had once been wrong in endorsing the gold-exchange standard in previous works. (See Mises 1949, chap. 31, section 3, p. 780.) In another chapter he stated: "If one looks at the catastrophic consequences of the great paper money inflations, one must admit that the expensiveness of gold production is the minor evil." (Mises 1949, chap. 17, section 6, p. 419)

Hence, Lawrence White's contention that Mises considered the resource-cost-saving induced by fractional-reserve banking to be beneficial from an overall point of view is correct only for the German-language first edition of 1912. In all other editions Mises revised this earlier stance, both in the third part of the *Theory of Money and Credit*, but also in the fourth part of the same book, as well as in *Human Action*.
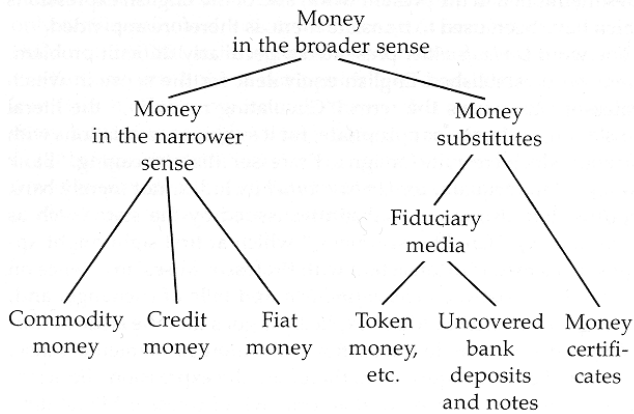
(2) According to Lawrence White, Mises thought that the production of fiduciary media tended to be limited under fractional-reserve banking. Professor White was kind enough to reiterate a lengthy quote from Mises that allegedly substantiates this contention. I shall reciprocate the favor by quoting this passage again, yet with two supplements: First, I shall also quote the few lines that immediately follow the said passage and that conclude section 4 of chapter 17. Second, I shall modify a few sentences of the translation (**highlighted**), because the English text is here not 100 percent covered by the German collateral:

> The circulation of fiduciary media is in fact not elastic in the sense that it automatically accommodates the demand for money to the stock of money without influencing the **inner** objective exchange value of money, as is erroneously asserted. It is only elastic in the sense that it allows of any sort of extension of the circulation, even completely unlimited extension, just as it allows of any sort of restriction. The quantity of fiduciary media in circulation has no natural limits. If for any reason it is desired that it should be limited, then it must be limited by some sort of deliberate human intervention -- that is by banking policy.Of course, all of this is true only under the assumption that all banks issue fiduciary media according to uniform principles, or that there is only one bank that issues fiduciary media. A single bank carrying on its business in competition with numerous others is not in a position to enter upon an independent discount policy. If regard to the behavior of its competitors prevents it from further reducing the rate of interest in bank-credit transactions, then—apart from an extension of its clientele—it will be able to circulate more fiduciary media only if there is a demand for them even when the rate of interest charged is not lower than that charged by the banks competing with it. **Thus we see that, up to a point, the banks pay regard to the periodical fluctuations in the demand for money**. They increase and decrease their circulation *pari passu* with the variations in the demand for money, so far as the lack of a uniform procedure makes it impossible for them to follow an independent interest policy. **But in doing so, they make an essential contribution to stabilizing the inner objective exchange value of money. In this regard, therefore, the theory of the elasticity of the circulation of fiduciary media is correct; it has rightly apprehended a tendency manifesting itself on the market, even if it has also completely misapprehended its cause. And precisely**

**because it has employed a false principle for explaining the phenomenon that it has observed, it has also completely closed the way to understanding of a second tendency of the market that emanates from the circulation of fiduciary media.** It was possible for it to overlook the fact that so far as the banks proceed uniformly, there must be a continual augmentation of the circulation of fiduciary media, and consequently a fall of the **inner** objective exchange value of money. [Mises 1912, pp. 360f; cf. Mises 1980, pp. 346f]

The crucial point is this: Mises held that the production of fiduciary media entailed *two possible tendencies*. If the banks agree on common procedures, the production of fiduciary media is in principle unlimited. If the banks do *not* agree on common procedures, the further production of fiduciary media is slowed down. He repeats this view in several passages of the third part of his book. (See for example Mises 1912, pp. 340, 420, 425f, 444.)

Money
in the broader sense

Money
in the narrower
sense

Money
substitutes

Fiduciary
media

Commodity
money

Credit
money

Fiat
money

Token
money,
etc.

Uncovered
bank
deposits
and notes

Money
certifi-
cates

Then why did he state that "quantity of fiduciary media in circulation has no natural limits"? I think the reason is quite straightforward. Mises believed that the second tendency dominates the first one. *Temporarily*, it is possible and even likely that fractional-reserve banks do not reach the agreement needed for *quick* credit expansion. But credit expansion is nevertheless the long-run tendency, for two reasons.

On the one hand, credit expansion does not necessarily have to be fast; it can also occur in a creeping trial-and-error process. Mises wrote (1980, p. 411): "So long as the

banks do not come to an agreement among themselves concerning the extension of credit, the circulation of fiduciary media can indeed be increased slowly, but it cannot be increased in a sweeping fashion. Each individual bank can only make a small step forward and must then wait until the others have followed its example."

On the other hand, the banks may eventually reach an agreement, in which case credit expansion can proceed at a much faster pace. This is actually the scenario that Mises envisioned in developing his business-cycle theory in chapter 19. He wrote:

> We know … that all credit-issuing banks endeavor to extend their circulation of fiduciary media as much as possible, and that the only obstacles in their way nowadays are legal prescriptions and business customs concerning the covering of notes and deposits, not any resistance on the part of the public. If there were no artificial restriction of the credit system at all, and if the individual credit-issuing banks could agree to parallel procedure, then the complete cessation of the use of money would only be a question of time.

This passages is intriguing for more than one reason. It not only shows that Mises believed that the "second tendency" – the one toward a continual augmentation of the circulation of fiduciary media – dominated the first tendency. It also highlights the fact that Mises had a different conception of the workings of free fractional-reserve banking, and of the collusion between such banks, than Professor White.

Mises held that *all* credit-issuing banks try to issue as many fiduciary media as possible. They do not need to be cartelized through government interventions (although that might possibly speed up the process), but have a self-interest in doing so. *In spite of* political interference, such as legal reserve ratios, however, the long-run tendency was for fractional-reserve banks to cartelize, first on the national level and eventually on a global level. This is what Mises purports to show in chapter 16, where he deals with the "evolution of fiduciary media." The driving

force of this process is the basic and perennial motivation of *all* banks to increase their issue of fiduciary media. At the time of writing *The Theory of Money and Credit*, he saw the biggest obstacle to the establishment of a world bank and the full cartelization of all fractional-reserve banks not in the inability of the *banks* to come to an agreement, but in the reluctance of *governments*. (See Mises 1912, pp. 339f; 1980, p. 329.)

It is true that, starting from the second edition (1924), Mises underscored the short-run benefits of competition between fractional-reserve banks, and in *Human Action* he downplayed the strength and even the existence of the "second tendency." Professor White gives a pertinent quote in which Mises (1949, chap. 17, p. 444) questions the likelihood of the establishment of a banking cartel without government support. However, even then Mises upheld his standard position. Free fractional-reserve banking is the second-best option. The first-best option is to stop any further production of fiduciary media. The very section in *Human Action* in which Mises rejects the scenario of a banking cartel ends with the following statement:

> Government interference with the present state of banking affairs could be justified if its aim were to liquidate the unsatisfactory conditions by preventing or at least seriously restricting any further credit expansion. In fact, the chief objective of present-day government interference is to intensify further credit expansion. This policy is doomed to failure. Sooner or later it must result in a catastrophe. [Mises 1949, p. 445]

These are Mises's views. Now we can discuss the question whether they are correct. Professor White does not think they are, but he will not be surprised that I think *he* is wrong. For the sake of brevity, however, I shall conclude with a few cursory statements pertaining to three of his errors.

First, Lawrence White claims that the "issue of fiduciary media is unlimited only in the analytical limiting case (never historically realized) of a single world banking system with a single issuer of fiduciary media," etc. It is

true that such a unified world banking system has never existed, but that does not mean that there is no tendency toward its establishment. I happen to think that such as tendency exists, or is at any rate much more plausible than the model of free fractional-reserve banking cherished in the writings of Professor White. Fractional-reserve banking without collusion not only is an "analytical limiting case (never historically realized)," but there is also not the slightest reason to think that it ever will come to be.

Second, it is not correct that collusion among banks could only aim at restricting industry output. The point of collusion is to increase the revenues of cartel members beyond the level that would be possible under competition. Usually that involves restricting output, but in the case of fractional-reserve banking it implies increasing output.

Third, is there really "no reason to think that banks can successfully collude without government … to enforce the cartels' prices"? I agree that this is improbable in most circumstances, but then again the fractional-reserve industry is special because it is built on the obfuscation and outright violation of property rights. And then there is also another consideration, highlighted by Professor White himself, who has argued that private central banks can evolve out of clearing house associations. (See White 1999, pp. 70ff.)

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# READING OFF THE PAGE

## by George A. Selgin

Although I suppose that I'm expected to reply to Larry's remark concerning my comment on his essay, it is to Guido Hülsmann's latest reply to Larry that I find myself tempted to respond. For in that reply Professor Hülsmann exhibits the tendency, all too common among 100-percent reserve types, to confuse Mises' opinions with their own.

For example, Hülsmann points out that Mises thought the gold *exchange* standard "pointless because it did not effectively rein in monetary intervention." He then states

that the only other options were "to go the full way to fiduciary media or to return to the actual use of coins in daily exchanges" and that Mises "clearly opted for the latter alternative." Finally, he says that "this means" that, despite its capacity to limit gold production, fractional reserve banking was not, in Mises's opinion, beneficial. If you cannot spot the *non sequitur* lurking in this chain of reasoning, then perhaps you have never heard of the *classical* gold standard -- an arrangement in which both actual gold coins and fractionally backed bank deposits and notes serves as exchange media. I do not doubt, however, that Professor Hülsmann has heard of it. So why does he overlook it in interpreting Mises? Could it be that he is tempted to do so because he must do so in order to portray Mises as having opposed fractional reserves?
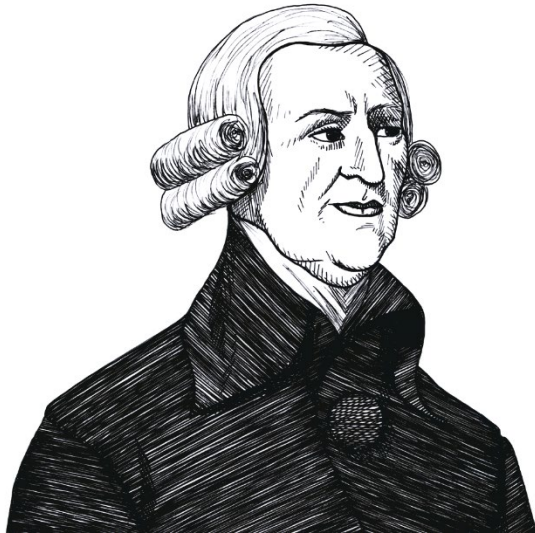


**Adam Smith**

By the way, Adam Smith, who believed no less than Mises did that "Everybody must be used to having gold coins in his pockets," and whose well-known opinion on this matter almost certainly informed Mises's stand, also wrote eloquently of the resource-cost savings to be had from fractional reserve banking. If in insisting on a gold-coin standard Adam Smith also meant to condemn fractional reserve banking, he has yet to be properly understood by anyone.

Further on in his reply, Hülsmann refers to Mises's claim that *if* banks manage to "agree on common procedures"

(meaning that they arrange to expand credit in unison), "the production of fiduciary media is *in principle* unlimited" (my emphasis). Next he brandishes Mises's claim that "all credit-issuing banks *endeavor* to extend their circulation of fiduciary media as much as possible" (my emphasis again), which he proceeds to reiterate: "banks *try* to issue as many fiduciary media as possible" (ditto), this being their "perennial *motivation*." Finally, he concludes, once again without clear warrant, that Mises did *not* think it unlikely that banks could succeed in expanding credit in unison, particularly by lobbying either for the establishment of a central bank or for "full cartelization" of the banking industry, and that Mises therefore regarded free fractional reserve banking as a second-best alternative, if that, to its outright prohibition.

All of which is, or ought to be, deeply unsatisfying to anyone conversant with the general thrust of economics, and of the theory of competition especially, since Adam Smith. *Of course* businessmen covet monopoly power; *of course* they'd like to be members of a cartel, if only they could hold one together; *of course* they may try to get the government to cater to their desires. The question is, what should economists to do about it? Should they conclude that competition is a bad thing, or a distant "second best" thing? Should they plead for *banning* any industry that *might* harm the public *if* the government makes either a monopoly or a cartel of it, instead of pleading for the government to stay out of it? I'm pretty sure that Hülsmann, asked to answer this question with reference to any industry other than banking, would offer the conventional answer. He is, in any case, entitled to be as inconsistent as he likes. But if he wishes to attribute the same inconsistency to Mises, he is a long way from making his case.

# FRACTIONAL RESERVE BANKING AND AUSTRIAN BUSINESS CYCLE THEORY

## by Jeffrey Rogers Hummel

As I feared, the discussion so far has descended to the interminable debate over the legitimacy and desirability of fractional-reserve banking. Larry, George, and I line up on one side, concluding that fractional-reserve banking, if unregulated and unsubsidized, provides important monetary and economic benefits that far outweigh any potential downside. Guido embraces Murray Rothbard's total opposition to anything but 100 percent reserves. This has spilled over into our interpretations of Mises's texts, with Larry, George, and me in fundamental agreement (with possibly some slight disagreements), while Guido tries to demonstrate that Mises ultimately came to the same position as Rothbard. These interpretive disputes are inevitable. The reason is that Mises, like all writers and thinkers, displayed occasional ambiguity and (as Guido does point out) was not perfectly consistent and unchanging across writings that spanned over half a century.



**Murray Rothbard**

To nudge the discussion in a more productive direction, I would like to explore how Mises's contributions impinge on an issue of current concern to economists of all stripes: the business cycle.

At the close of my initial contribution to the discussion, I mentioned two unresolved questions about Austrian business cycle theory. *The Theory of Money and Credit*'s first edition introduced this theory, which since then has evolved into four slightly different variants. (1) The original variant of both Mises and Friedrich Hayek (1931, 1933, 1939), despite minor differences in emphasis, primarily focused on how credit expansion --instigated either by government or the banking system -- causes self-reversing malinvestment in the economy's capital structure. (2) Rothbard's variant (1963) added his blanket hostility to fractional-reserves and a concomitant enthusiasm for deflationary bank panics that cleanse the economy. (3) Roger Garrison's variant (2001), building on his understanding of some passages in Mises, posits that malinvestment and the resulting correction drive the economy off its long-run production possibilities frontier, first outward and then inward. This permits the boom to simultaneously increase both consumption and investment. Garrison thereby implicitly incorporates the upward-sloping short-run aggregate supply curve that populates so many mainstream models of the macroeconomy. (4) George's (1988), Larry's, and Steve Horwitz's (1992) variant, building instead on Hayek, grants almost equal billing to velocity shocks along with monetary shocks as a source of business cycles and couples that with Leland Yeager's (1997) analysis of the disequilibrating effects of excess supplies and demands for money, making their version into a kind of Austrian-Keynesian-Monetarist amalgam.

Despite what they have in common, these four variants imply divergent answers to the two questions I raised in my previous comment. As I emphasized, Austrian business cycle theory "hinges on specifying two firm dividing lines: (a) between those financial instruments that constitute inside money and those comprising what Mises considered genuine manifestations of people's savings, and (b) between those increases in the money

stock, however defined, that generate a self-reversing boom and those that do not." The Rothbard variant denies that autonomous changes in velocity play any role in the cycle, contending that they merely represent changes in people's preferences about the demand for money. Not only does this differ from the Selgin-White-Horwitz position, which like Keynesian theory, views negative velocity shocks as a potential source of depressions. But it also differs from the orthodox Monetarist position, which denies the *empirical* significance of autonomous shifts in velocity. Changes in money demand, according to the Monetarist position, are primarily driven by monetary shocks. And Garrison, in some of his writings (2005, 491), has endorsed this conclusion: "An exogenous change in money demand is rarely if ever the source of a macroeconomic disruption. (Here, the Austrians fall in with the monetarists.)"

Rothbard, by eliminating any role for velocity, confines the cause of business cycles to only monetary fluctuations. This makes a precise definition of what constitutes inside money all the more critical. Yet even hardcore Rothbardians disagree about what financial instruments should be included. Should you count small time deposits (CDs), which are not negotiable like checking accounts but are redeemable *de facto* on demand at a penalty rate? If you do, then an increase in their quantity (without a change in any other component of the money stock) must induce malinvestment; if you do not count them, the increase becomes merely a change in time preference. Rothbard's definition of the money stock at one point (1978) included small time deposits at their penalty rate, whereas such Rothbardians as Joseph Salerno (2010, 115-30) and Frank Shostak (2000) both exclude them. Salerno's definition includes both savings deposits and money market deposit accounts, whereas Shostak excludes savings deposits. These may seem trivial, technical differences. You can quibble about them endlessly, and I could mention several others, or about the differences all three have with Larry's preferred definition (1986). But on top of creating ambiguity about what 100 percent reserves would look like, these disagreements make the entire concept of "credit expansion" vague and ill-defined.

George, Larry, and Horwitz get around this problem by arguing that unregulated free banking, with a commodity money base, would tend to stabilize $MV$ (which is money times velocity in the equation of exchange). Since $MV$ equals nominal GDP, stabilizing $MV$ would eliminate most business cycles. It also appears to conveniently obviate the need for a precise dividing line between inside money and what Mises considered genuine credit instruments. Any change in the quantity of something not defined as part of the money stock -- perchance, small CDs -- will necessarily be captured in velocity. But this only evades the problem. Surely credit expansions that are inconsistent with underlying time preferences are not the sole cause of changes in velocity. In developed economies with fiat money, nominal GDP is almost always rising, with fluctuations in its rate of growth. Since this means $MV$ is also always rising, how do you precisely identify periods that represent artificial booms generating malinvestments? And how can you determine if the cause is central-bank policy or something else? To reply that all such economies are *always* experiencing central-bank-induced credit expansion and therefore will at *some* unknown date in the future suffer another recession of *some* unknown magnitude, is not really much of a business cycle theory. Indeed, it reduces Austrian business cycle theory to an empty tautology, untestable and irrefutable. It also leads George (2008), Larry (2008), and other Austrians to a stubborn insistence that the Federal Reserve under Alan Greenspan must have caused the housing boom that preceded the financial crisis of 2007-2008, despite the fact that the growth rates of *all* the monetary measures -- the base (which the Fed directly controls), M1, M2, and MZM -- were steadily declining during the period (Henderson and Hummel 2008a, 2008b).

These are a few of the several, untidy issues that require more consideration, study, and discussion in order to develop Mises's insights into a more convincing and sophisticated understanding of the business cycle.

# FURTHER REMARKS ON HÜLSMANN AND HUMMEL

## by Lawrence H. White

As evidence that Mises "qualified his endorsement of free banking," Professor Hülsmann quotes the following statement by Mises (1980, p. 439): "If it should prove easier now for the credit-issuing banks to extend their circulation, then failure to adopt measures for limiting the issue of fiduciary media will involve the greatest danger to the stability of economic life." It is clear in the context of the surrounding sentences, however, that Mises was not here warning about any free-banking system, but was instead warning about the central-bank-dominated banking systems that actually prevailed at the time he wrote in 1924. A few sentences earlier in the same paragraph Mises wrote that the likelihood of enacting measures for restricting fiduciary media "depends upon the kind of credit policy that is followed in the immediate future by the banks in general and by the big central banks-of-issue in particular." I doubt that Mises would have disagreed with the proposition – although he does not here state it as clearly as one might like – that any effective measures for limiting fiduciary media in a nation must first and foremost limit the issues of the national central bank. (I also doubt that Hülsmann would disagree.) In the conditions of 1924, just as today, the central bank's liabilities were held as reserves by credit-issuing commercial banks, and the central bank's expansion thereby drove the banking system's expansion as a whole.

A slightly earlier paragraph in *The Theory of Money and Credit* is also of interest in this discussion. Mises observed that "the considerations … that are supposed to militate against the freedom of the banks" were discussed "two or three generations ago," meaning roughly 1850-1875. The supposedly decisive argument was "the currency principle," which held, in Mises's words, that "any note issue that is not covered by gold is dangerous, and so, in order to obviate the recurrence of economic crises, such issues must be restricted." Mises then rebutted this view by referring to his own findings earlier in the book: "We

have already shown that the dangers envisaged by the currency principle exist only when there is uniform procedure on the part of all the credit-issuing banks, not merely within a given country but throughout the world." That is, absent a world cartel of central banks, the Currency School view that "any note issue that is not covered by gold is dangerous," or in other words that every issue of fiduciary media is dangerous, is invalid.
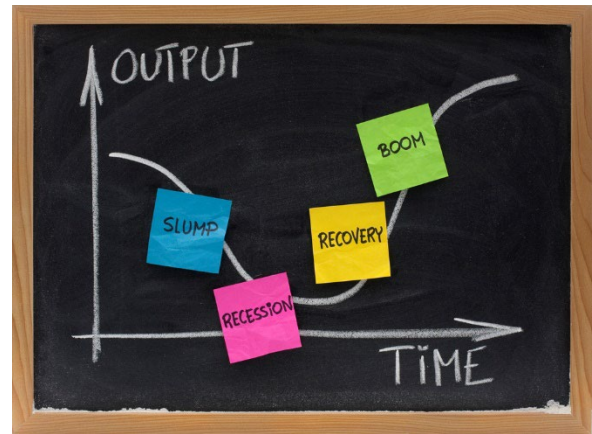


This last passage is also relevant to Professor Hummel's argument that Austrian business cycle theory "hinges on specifying two firm dividing lines: (a) between those financial instruments that constitute inside money and those comprising what Mises considered genuine manifestations of people's savings, and (b) between those increases in the money stock, however defined, that generate a self-reversing boom and those that do not." I certainly agree with Hummel on the need for the second distinction. As I have previously indicated, I (along with other modern free bankers) disagree with the proposition that every increase in the money stock is disequilibrating, because an increase that counteracts an incipient excess demand for money is on the contrary equilibrating. It does not lower the market interest rate below the natural rate, but prevents the opposite discrepancy. Which side was Mises on? Although Mises elsewhere unfortunately declared that every increase in the money stock sets the business cycle in motion (seemingly regardless of whether it creates an excess supply of money or not), the modern view follows from the passage of *The Theory of Money and Credit* that I have twice now quoted ("Thus the banks may be seen to pay a certain amount of regard to the periodical fluctuations in the demand for money. They

increase and decrease their circulation pari passu with the variations in the demand for money, so far as the lack of a uniform procedure makes it impossible for them to follow an independent interest policy"). Mises here pointed out that under free banking "periodical" (which I take to mean seasonal) variations in the volume of bank-issued money will match variations in the volume of demand to hold bank-issued money. Such variations can only be seen as equilibrating. And in the passage I quote in the paragraph just above, to repeat, Mises rejected the Currency School view that every increase in uncovered note-issue is dangerous (except in the limiting case of a world central banking cartel).

I am less sure about Hummel's first distinction. I don't see why an Austrian business cycle theory must insist that an instrument – in particular an interest-bearing checking account – cannot, under normal conditions, be both inside money and a genuine manifestation of people's savings. That is, a particular increase in the volume of checking-account balances can be a genuine manifestation of an increase in very short-term savings (whether at the expense of current consumption or at the expense of holding wealth in some other form, for example cash, consumer durables, or longer-term savings) and not a manifestation of an excess supply of money.

I want to conclude by quoting what I think is an admirably clear statement of Mises's favorable judgment on free banking, which appears soon after the two paragraphs I just quoted:

> If the arguments for and against state regulation of the bank-of-issue system and of the whole system of fiduciary media are examined without the etatistic prejudice in favor of rules and prohibitions, they can lead to no other conclusion than that of one of the last of the defenders of banking freedom: "There is only one danger that is peculiar to the issue of notes; that of its being released from the common-law obligation under which everybody who enters into a commitment is strictly required to fulfill it at all times and in all places. This danger is

infinitely greater and more threatening under a system of monopoly."

As the source of the quoted sentences Mises cites the German free-banking proponent I. E. Horn: "Horn, *Bankfreiheit* (Stuttgart, 1867), pp. 376 f." The text of Horn's book is available here: https://archive.org/details/bankfreiheit00horngoog. [Editor: It was first pubished in French the year before: J.-E. Horn, *La liberté des banques* (Paris: Guillaumin, 1866).]

# THE DEMAND FOR MONEY ALSO MATTERS

## by George A. Selgin

In his most recent contribution to our discussion, Jeff Hummel misrepresents my particular "take" on the Mises-Hayek theory of the business cycle, so I'd like to take this opportunity to correct him, while also pointing out what I consider to be some serious errors in his own reasoning.
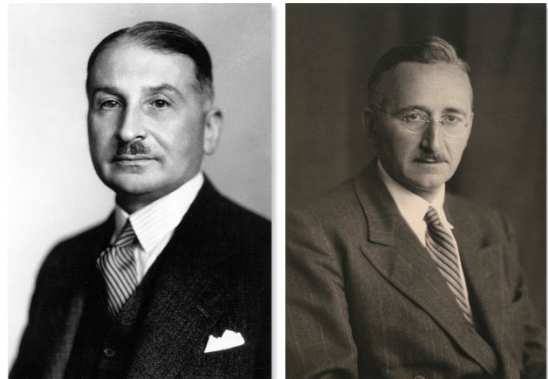


I've never intended, first of all, to revise the Mises-Hayek theory except by insisting that there are circumstances in which an increase in the nominal money stock, and especially in the stock of "fiduciary" media unbacked by high-powered money, instead of setting a Mises-Hayek type cycle in motion, merely serves to accommodate a like increase in the public's willingness to add to the total extent of bank-intermediated saving. Credit expansion serves in such cases not to drive lending rates below their "natural" levels, but to keep them from rising above

those levels. Expressed in the simple terms of the equation of exchange, the argument amounts to saying that, holding reserves constant, expansion of M is stabilizing rather than destabilizing so long as it serves to offset a like decline in V. The claim is essentially the same as Hayek's own theoretical stand that an ideal monetary policy is one that serves to stabilize the flow of spending, MV.

Given my desire to clarify this aspect of the theory, and also because I wished to show how a free-banking system tends to achieve the ideal in question, I naturally devoted a lot of attention to discussion of the implications of changes in money's velocity. But this theoretical emphasis doesn't at all mean, as Professor Hummel claims, that I meant to grant "almost equal billing to velocity shocks along with monetary shocks as a source of business cycles." In fact I took no stand in the writings in question concerning the historical importance of velocity shocks relative to shocks, policy-based or otherwise, to the nominal supply of money. To have taken such a stand would have meant delving much more deeply into the historical and statistical record than I ever intended to do in works mainly concerned with theory.

It follows that, although Hummel is perfectly correct in characterizing me as someone who "views negative velocity shocks as a *potential* source of depressions" (my emphasis), he errs both in claiming that there is something particularly "Keynesian" about my stand and in saying that it "differs from the orthodox Monetarist position, which denies the *empirical* significance of autonomous shifts in velocity" (emphasis in original). To claim that something is "potentially" important is of course not to insist that it is, or has been, important in fact.

Later in his remarks, Hummel recognizes that a stable MV ideal conveniently avoids the problem of deciding which among many measures of the money stock to treat as a policy instrument or target. But then he goes on to insist that it does so only by evading the crucial policy problem of "precisely" identifying "periods that represent artificial booms generating malinvestment." I confess that I don't understand this argument at all and

am indeed inclined to think it hopelessly muddled. The entire point of the MV argument, mine and Hayek's alike, is that the periods of excessive money creation are "precisely" those in which MV grows excessively rapidly (I shall come in a moment to clarifying "excessively"), for those are the times when money-stock growth exceeds concurrent growth in the demand for real money balances, thereby swelling the stream of payments.[11] When productivity is stagnant, the swelling translates into rising prices of both factors and final goods, which serve to restore monetary equilibrium by causing a proportional increase in the demand for *nominal* money balances. When productivity itself is improving, real money demand itself increases, so that final-goods prices needn't rise as much, if indeed they must rise at all.

"In developed countries," Hummel goes on to observe, MV "is almost always rising." He then wonders, first, how it is possible in that case to tell whether it is rising so much as to cause an artificial boom. The answer is that once people come to anticipate a pace of spending growth such as might otherwise have set a cycle in motion, the cyclical effects of the growth are muted or neutralized, and remain so until expectations are again exceeded. That the real consequences arising from any particular rate of credit expansion will depend upon the extent to which the rate comes as a surprise has been conventional wisdom since the rational-expectations revolution. It was, moreover, Mises own understanding long before then, as conveyed in his reply to Lachmann's posing of more or less the same question Hummel now raises.[12] That proponents of a stable MV ideal must recognize this truth hardly places them in a more awkward position than advocates of an M growth rate rule;[13] still less does it deprive them of an ability to offer meaningful policy recommendations by reducing their ideal to an "empty tautology." In *Less Than Zero*, for example, I offer perfectly concrete advice concerning the desirable target rate of MV growth, which elsewhere I have amended in light of high established growth-rate expectations only to the extent of allowing that the preferred target is best implemented gradually.[14]

Hummel next wonders how the stable-MV camp can possibly "determine if the cause [of a boom] is central-bank policy or something else." In fact, nothing could be easier, for if a central bank is running the show then it is responsible *ipso facto* for any undue expansion of spending. If the proposed MV growth rate target is 3 percent and spending grows 5 percent, the central bank has erred by allowing M to grow by two percentage points too many. (It matters not which M one refers to, so long as one has in mind the appropriate corresponding V.)

Hummel, in contrast, appears to subscribe to the jejune view that central banks are responsible only for keeping growth of their own balance sheets within certain limits, without reference to what is happening either to the real demand for high-powered money or to the various determinants of the money multiplier. His thinking commits him to claiming, for example, that in a fiat system a sharp increase in prices is to be regarded as the central bank's fault if the rise is associated with proportional growth in the monetary base, but not if it follows a sharp decline in the public's preferred currency ratio, or a sharp increase in money's velocity — as if central banks weren't also responsible for responding appropriately to such developments! Since the Great Contraction of the early 1930s occurred despite the Fed's having added to the stock of base money, perhaps Hummel is even willing to be so consistent as to insist that that debacle was caused not by "central-bank policy" but by "something else."
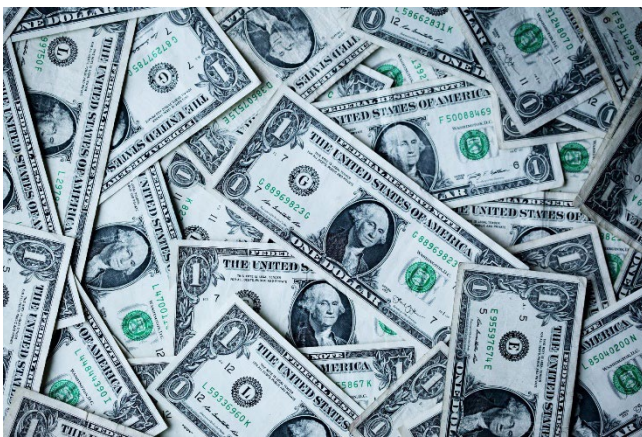


Although he might not be prepared to go so far as to absolve the Fed of blame for the Great Contraction,

Hummel doesn't flinch from denying that it played any part in fueling the recent housing boom and from portraying my and Larry's "stubborn insistence" to the contrary as proof of our having worked our way into a theoretical corner. But Larry and I (and plenty of other economists from a host of different schools of thought) are not pointing our fingers at the Fed simply because our theories prevent us from entertaining other possibilities. We do so because the actual acceleration of spending growth, the record-low levels at which the real federal funds rate was kept, and other evidence besides warrants it.

Hummel, on the other hand, is cocksure that the Fed did nothing wrong because "the growth rates of *all* the monetary measures ... were steadily declining" during the period in question. But a central bank's ultimate responsibility, as I have just said (and as I observed to Hummel and Henderson some time ago, without any apparent result[15] ), is *not* that of seeing to it that this or that monetary measure grows at such-and-such a rate; its ultimate duty is that of seeing to it that the supply of money grows only as much as is needed to accommodate prior growth in the real demand for money balances. It follows that, when the demand for real balances *declines,* a responsible central bank must allow the nominal money stock to decline no less rapidly, or else risk contributing to a monetary excess with all that that implies. The monetary statistics Hummel refers to show only that the nominal stock of money was declining, but not that it was declining as rapidly as it ought to have. That MV was in the meantime growing exceptionally rapidly proves, on the contrary, that the money stock wasn't declining rapidly enough.

**Endnotes**

[11] At one point Hummel observes, bafflingly, that "Surely credit expansions that are inconsistent with underlying time preferences are not the sole cause of changes in velocity." How he can read into any defense of stable MV an implicit assumption that changes in V must occur only in response to excessive credit expansion is utterly beyond me.

[12] Ludwig von Mises, "'Elastic Expectations' and the Austrian Theory of the Trade Cycle," *Economica* 10 (August 1943); Ludwig Lachmann, "The Role of Expectations in Economics as a Social Science." *Economica* 14 (February 1943).

[13] Or does Professor Hummel mean to suggest that the cyclical consequences of, say, a 10 percent annual growth rate of M2 will be the same if the public anticipates a 10 percent growth rate as they would be if it anticipated no growth at all?

[14] George Selgin, *Less Than Zero: The Case for a Falling Price Level in a Growing Economy* (London: Institute of Economic Affairs, 1997).

[15] "Guilty as Charged," *Mises Daily* (November 7, 2008), in reply to David R. Henderson and Jeffrey Rogers Hummel, "Greenspan's Monetary Policy in Retrospect," *Cato Briefing Paper* 109, November 3, 2008. Online at: https://mises.org/daily/3200

# ON THE STABILIZING EFFECTS OF FRACTIONAL-RESERVE BANKING

## by Jörg Guido Hülsmann

George Selgin believes that I tend to confuse Mises's opinions with my own. Clearly, all participants in this online discussion have been strongly influenced by Mises's writings, though each of us has driven this inspiration into somewhat different directions. It is therefore tempting indeed to conflate the master's opinions with one's owns. My impression is that this holds true for Professor Selgin, too, no less than for anyone else.

Since the discussion turned to Mises's business-cycle theory, I should like to comment on three specific issues:

1. the tautological nature of Mises's business-cycle theory;

2. the role of the money supply in that theory; and

3. the equilibrating effects of fractional-reserve banking.

(1) Professor Hummel thinks that Mises's business-cycle theory boils down to an "empty tautology." Mises would probably have agreed that it is a tautology. In *Human Action* (chap. II, sect. 3), he stated: "Aprioristic reasoning is purely conceptual and deductive. It cannot produce anything else but tautologies and analytic judgments. All its implications are logically derived from the premises and were already contained in them." But he went on to point out that tautologies are not always *empty*. Quite to the contrary, they can very well contain important insights into (causal) features of the real world.

Moreover, in *Human Action* he further elaborated the argument, already presented in the *Theory of Money and Credit*, that the propositions of economic theory concern ordinal rather than cardinal relations. This implies, for example, that business-cycle theory does not allow us to specify the precise date of future crises, and it also does not allow us to specify *ex ante* their magnitude. Rather, such specifications must be given in the light of "historical" judgments pertaining to the contingent future circumstances of time and place. As Mises underscores, these "historical" judgments always contain an element of subjectivity.

It is also worthwhile to recall that economic science, like any science, only provides *partial* insights (therefore the usual *ceteris paribus* clause). Mises's business-cycle theory highlights only *one* chain of causation, but there are many others that might play out simultaneously. Again it is historical judgment, not a priori theory, which might disentangle the relative weight of each factor that bears on a concrete situation.

(2) I agree with Professor Hummel that the definition of the components of the money supply is a critical issue of Mises's business-cycle theory. It is also correct that on this question there is no general agreement. However, this difficulty is, again, not a problem for *theoretical* analysis, but for *historical* analysis. The very point of Mises's typology of money is that the technical characteristics of various financial instruments are more than often irrelevant to deciding whether or not they

belong to the money supply. What counts is whether they can be redeemed on demand at par; and whether this is the case must be determined for each concrete historical context.

This being said, and since we are at it, I should like to mention for the record that I disagree with Mises on the central role of the money supply. I do not think there is an a priori causal relation between ("artificial") increases of the money supply and intertemporal disequilibria (see Hülsmann 1998). (3) Lawrence White claims that an increase in the money stock "that counteracts an incipient excess demand for money is … equilibrating. It does not lower the market interest rate below the natural rate, but prevents the opposite discrepancy." Similarly, George Selgin argues that in such cases, credit expansion serves "not to drive lending rates below their 'natural' levels, but to keep them from rising above those levels. Expressed in the simple terms of the equation of exchange, the argument amounts to saying that, holding reserves constant, expansion of M is stabilizing rather than destabilizing so long as it serves to offset a like decline in V."

I completely disagree with this view. Let me begin by highlighting that Professor Selgin conflates two very different meanings of stabilization. At the end of the passage I just quoted, he addresses the stabilization of aggregate demand (M x V). The production of fiduciary media by fractional-reserve banks may indeed, under certain circumstances, entail a stabilization of this aggregate. But so what? Why is such a state of affairs more beneficial than a shrinking aggregate demand or a rising aggregate demand? I know fairly well how a Keynesian economist might respond to this question. With Jeffrey Hummel, I wonder whether George Selgin's response is really any different in substance. But my point is that the stabilization of aggregate demand is not the same thing as intertemporal stabilization. It does not at all follow that the former implies the latter, or the other way round.

This brings me to the central question: Is it really the case that credit expansion, when it occurs simultaneously with an increase in the demand for money, does *not* drive interest rates below their natural levels, but prevents them from rising above those levels? The whole argument is premised on the notion that the increase in the demand for money, if unchecked by a corresponding increase in the money supply, would entail an intertemporal disequilibrium. But why should this be so? It is true that the increase in the demand for money would tend to entail a temporary increase of market interest rates (the latter would not necessarily be higher than before, but rather higher than they otherwise would have been). But why should we interpret this event as an increase of the interest rates above their natural level? Why is that temporarily higher level not itself the natural level? Why should the structure of production *not* be adjusted to interest-rate changes resulting from variations of the demand for money? As Dan Mahoney (2011) has recently pointed out, it is precisely when fractional-reserve banks prevent changes of the interest rates that they steer the structure of production away from the state in which it should be.

# REPLY TO GEORGE SELGIN ON AUSTRIAN BUSINESS CYCLE THEORY

## by Jeffrey Rogers Hummel

I appreciate George's clarifications, which help push our conversation into more interesting realms. I am a great admirer of his work on monetary economics, believing he has made major contributions. I certainly did not intend to misrepresent George's views. Nor do I have any fully developed business cycle theory of my own to offer as an alternative to his. I consider this to be the great unresolved issue in macoeconomics. While I believe, as I said before, that Austrian theory offers some penetrating insights, I do not find any of its variants to be entirely satisfactory.

George denies any intention to "revise the Mises-Hayek theory," and yet he then proceeds to discuss how he "clarified" (his word) one aspect of the theory.

Categorizing the varied theories of economists is inherently imprecise, especially when the differences are subtle and nuanced. I did concede that my four suggested variants of Austrian business cycle theory had elements in common, and that the goal of stabilizing $MV$ came from Hayek (although not from Mises). Whether the writings of George (along with Larry and Steve Horwitz) deserve to be classified as a separate variant or merely an elaboration on Mises and Hayek is a judgment call. George is free to minimize his originality; I think it merits greater recognition.

Moreover, there is another respect in which George has extended Austrian business cycle theory. As mentioned in my previous comment, he has incorporated from the Monetarist analysis of Leland Yeager (which in turn goes back to at least the work of Clark Warburton) a story about the inherent disequlibrating effects of *both* excess supplies and demands for money. But unlike Yeager (who rejects Austrian business cycle theory), George replaced price stability as the equilibrating optimum with his productivity norm and its (usual) secular deflation.

I did misinterpret George's explicit views about the *empirical* importance of velocity shocks, in part because a major portion of his 1988 book is devoted to explaining how an unregulated banking system would offset such shocks. That said, I still think George's acceptance of velocity shocks as a *potential* source of depressions introduces a Keynesian element. I should hastily add that this is not intended as a criticism. Unlike many other Mises-influenced economists, I do not believe that Keynesianism is utterly bereft of value, despite my adamantly rejecting its policy implications. The essential element of Keynesian business cycle theory is that autonomous falls in velocity (what Keynesians traditionally refer to as a falls in autonomous expenditures, which are equivalent to increases in money demand) cause economic downturns. George is right that Monetarists also accept this as a theoretical possibility. But it is also a depression scenario that requires no previous malinvestment boom, giving it a decidedly un-Austrian flavor.
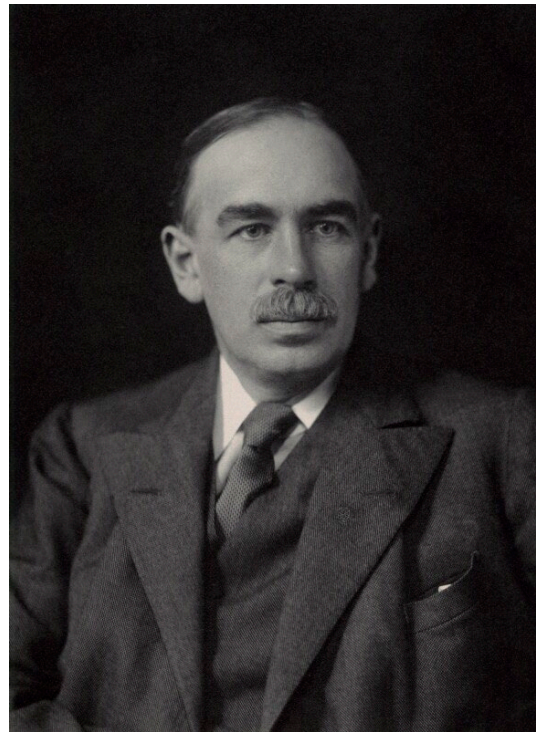


**John Maynard Keynes**

Moreover, one can make too much of the distinction between monetary and velocity shocks. Again as emphasized in my previous comment, how a shock is classified depends on what is included in the money stock. An extreme case is the Great Depression. Outside of Rothbardians, most proponents of Austrian business cycle theory accept that the Great Depression was made great by what Hayek came to call the "secondary deflation." This entailed an enormous collapse of the broader measures of the money stock from 1929 to 1933, driven mainly by a series of banking panics, as well documented by Milton Friedman and Anna Jacobsen Schwartz (1963). Yet over the same four-year period the monetary base, directly controlled by the Fed, ultimately rose. Thus, what was a huge negative monetary shock from the perspective of the broader monetary measures was a negative velocity shock to the monetary base.

In response to my query (and apparently Ludwig Lachmann's as well) about how looking at $MV$ permits one to "precisely identify periods that represent artificial booms generating malinvestments," George quite reasonably introduces expectations, viz.:

> …once people come to anticipate a pace of spending growth such as might *otherwise* have set a cycle in motion, *the cyclical effects* of the growth *are muted or neutralized, and remain so* until expectations are again exceeded [emphasis mine].

This sounds to me suspiciously like pure Monetarism, in which downward turning points in *MV* growth become the primary cause of depressions. I'm unclear whether George is making a comprehensive claim; i.e., that *any* growth rate of *MV*, if constant and fully anticipated, will *not* generate a self-reversing malinvestment boom, until the growth rate changes. Or to put the question in concrete terms, is he saying that if *MV* growth in the United States had continued to remain constant after the expansion of 1922 to 1929, there would have been no garden-variety depression in 1929, much less a Great Depression? I may be missing something, but if there is another way to interpret George's answer, I hope he will elaborate. To my question about how to determine if the central bank or something else causes a change in *MV* growth, George gives a straightforward answer. As long as there is a central bank that can target *MV*, then the central bank is responsible. But this is not really an answer to my question; it is a policy prescription. Indeed, it is a policy prescription that grants the Fed more ability to stabilize the macroeconomy, even if only in theory, than I think is warranted.

Again, consider the Great Depression. I concede that Friedman and Schwartz were right that the Fed, with a sufficient expansion of the monetary base, could have totally offset the deflation and even cut the bank panics short. But that still leaves open what triggered the panics. Friedman and Schwartz blamed a change in Fed policy, arguing that most of the bank failures were liquidity failures. Yet there are alternative explanations, including that of Charles Calomiris and others, who conclude that the banks were already suffering serious solvency problems. Surely this is an important economic question in and of itself, irrespective of whether the Fed, with a better policy, could have averted the subsequent deflation.

Perhaps because I was trained as a historian, I am at least as curious about the causes of depressions as about the proper policy. After all, depressions in the United States long predated the creation of the Fed. George and Larry have been in the forefront of economists revealing how numerous legal restrictions made the U.S. banking system peculiarly vulnerable to shocks and panics. Yet that still leaves unexplained the timing of the downturns. Were the initiating factors mainly domestic, or can we point the finger at the international factors, particularly the policies of the bank of England? Was it some combination, or do different cases require different stories?

Which brings us to the housing boom and the subsequent financial crisis. George and I agree that exceptionally low interest rates were a major cause, but we disagree as to why rates were so low. In a recent Econlib article (2013), I challenged the common but exaggerated notion of almost total Fed control over interest rates. Since many factors can affect interest rates, one must avoid the following circularity: "Why were interest rates so low? Because of Greenspan's expansionary monetary policy. How do you know Greenspan's policy was expansionary? Because interest rates were so low." In other words, to blame Greenspan, some independent variable must be invoked. If I understand George's reasoning (2008; but he can correct me if I am wrong), he relies (as do most other economists who blame the Fed) on some kind of Taylor Rule that specifies what the interest rate should have been to prevent the housing boom. But the Taylor Rule in all its versions makes the astonishing assumption that the underlying real interest rate (what Mises called the originary rate and Wicksell the natural rate) remains perfectly constant over long periods. I find this a particularly peculiar assumption for Austrian economists to accept.

The standard way the Fed is thought to affect interest rates is by changing its balance sheet, buying or selling securities and thus altering the monetary base. Yet the increase in the base during the housing boom was overwhelmingly dwarfed in size by the *net* inflow of savings from abroad. In one year alone, 2006, that annual inflow was reaching nearly $800 billion, far exceeding the

mere $200 billion increase in the base for the *entire half decade* from 2001 to 2006. Furthermore, it is widely recognized that the savings inflow was not entirely market driven by was heavily influenced by the policies of the Chinese government, which had coupled a pegged exchange rate with significant internal capital and exchange controls. I have never encountered a plausible explanation of how the Fed can significantly affect interest rates other than by manipulating its balance sheet, nor a plausible mechanism that would magnify any resulting impact on interest rates way beyond the impact of other changes of like monetary magnitude in the demand for or supply of securities. But I am open to being persuaded.

P.S. I must apologize to the readers and my fellow participants in this discussion for the delay in writing this reply. I hope that doesn't seriously inhibit possible responses.

## UNHELPFUL LABELS: REPLY TO HUMMEL

## by George A. Selgin

As Jeff Hummel wonders, in his last post, whether he has interpreted my views correctly, I hope I may be pardoned for getting in a last word for the sake of answering him.

My answer is that, although in general Jeff represents my views correctly (I shall come to some exceptions in a moment), his portrayal suffers from his insistence upon sorting various aspects of those views into "Monetarist," "Keynesian," and "Austrian" pigeonholes, together with his tendency to regard any admixture of ideas of the three schools as necessarily problematic.

Jeff claims, for example, that in treating a decline in money's velocity as a potential cause of depression, I introduce a "Keynesian" element into my analysis. And although he is quick to say that for him the epithet carries no opprobrium, he finds the "decidedly un-Austrian flavor" of this aspect of my thinking troublesome.

But what is so "Keynesian" about the notion that velocity sometimes declines, and that, other things equal, such a decline implies reduced spending and a temporary decline in profits and production? As Hummel himself recognizes (and as Leland Yeager has gone to some length to remind people[16]), this sort of thinking is straight "old-fashioned" Monetarism, and as such predates the *General Theory* by many decades. It forms as well, as Jeff also recognizes, part of Hayek's own understanding. Finally, as Larry makes clear in his contributions to this forum, it is at least implicit in some of Mises's arguments. The Keynes of the *General Theory*, on the other hand, far from offering a particularly clear and coherent statement of the possibility in question, obscured it by introducing the vague concept of "liquidity preference."

And even if it were true that my theory had a "Keynesian" flavor, it wouldn't follow, as Jeff suggests, that the flavoring amounted to any serious revision of the Austrian theory. It would merely indicate my own eclecticism, which I have never intended to disguise. The fact that I believe that a recession or depression can result from a collapse in spending, with no need for a prior boom, doesn't means that I either reject or desire to radically revise the Austrian cycle theory. It just means that I (like Hayek) think that there is more than one way in which downturns can happen. Indeed, I have publicly complained about the obnoxious tendency of both Monetarists and Austrians (to say nothing of Keynesians) to insist upon a "one theory fits all" approach to understanding business cycles.[17]

Later in his remarks Jeff, replying to my suggestion that the cyclical consequences of any particular spending growth rate will be "muted or neutralized" to the extent that the growth is anticipated, labels the suggestion "pure Monetarism," as if overlooking the fact that, in making it, I referred to Mises's own (1943) statement of the same view.[18]

Jeff also wonders whether I am "making a comprehensive claim ... that *any* growth rate of *MV*, if constant and fully anticipated, will *not* generate a self-

reversing malinvestment boom." Evidently I did not intend to go quite so far, or I would have written "neutralized" instead of "muted or neutralized." On the other hand, I did not limit my statement to cases of "constant" spending growth rates, for (as the rational-expectations revolution has taught us) any nonrandom growth pattern might be fully anticipated. (I know, I know: I have now added a New Classical "flavor" to my arguments. So sue me.) As for 1922-29, I can only say that the question Jeff raises concerning that period has me scratching my head, for if MV growth accelerated during the 20s, and the acceleration itself was not anticipated, that is all it would take to cause an Austrian boom-bust cycle of some (perhaps small) magnitude, regardless of what happened to spending afterwards.

Next Jeff claims that, in saying that central banks are responsible for any undesirable changes in spending growth, and not just those stemming from changes in the size of their own balance sheets, I am granting such banks, and the Fed in particular, "more ability to stabilize the macroeconomy" than is warranted. Fiddlesticks: I readily concede -- indeed, I've long argued, as Jeff knows very well -- that central banks aren't capable of managing money in such a way as to avoid or at least minimize cycles. When I say that central banks are to blame for undesirable changes in the flow of spending, I don't mean that they might do better. I mean that another arrangement entirely might do better.

Finally, Jeff claims that, to hold the Fed partly responsible for the easy credit conditions that helped to stoke the subprime boom, (1) I must assume that it had "almost total control over interest rates" and (2) I must be relying upon some version of the Taylor rule with its "astonishing assumption that the underlying real interest rate ... remains perfectly constant over long periods." Both suggestions are mistaken. To suggest, first of all, that the Fed was to blame for the very low interest rates that prevailed between 2001 and 2007 is not to claim that it had "almost complete control" over those rates. It is only to claim that it was able to influence them at the margin, and temporarily. To deny that central banks can have such an influence would of course be to

reject out of hand not only the Austrian cycle theory, but Wicksell's theory, and every other theory that holds central banks capable of influencing real interest rates to some nontrivial extent. Certainly neither Greenspan nor any other central banker ever believed central banks to be so powerless. On the contrary: most central bankers are inclined to exaggerate central banks' control over interest rates, except (of course) when it comes to defending themselves against accusations of irresponsible bubble-blowing.

Second, one needn't appeal to the Taylor rule at all to claim that the Fed kept interest rates too low. There are other ways for gauging where the federal funds rate stood relative to its "natural" or "neutral" value. My own assessment is in fact based on a comparison of the federal funds rate with a rough natural-rate estimate based on the growth rate of total factor productivity.[19]

Finally, neither my nor Taylor's assessment takes for grated a "perfectly constant" natural or neutral rate of interest; both merely assume the real natural or neutral rate varies around a constant mean.

### Endnotes

[16] 1. Leland B. Yeager, "New Keynesians and Old Monetarists." In George Selgin, ed., *The Fluttering Veil: Essays on Monetary Disequilibrium* (Indianapolis: Liberty Fund, 1997), pp. 281-302.

[17] "Booms, Bubbles, Busts, and Bogus Dichotomies." *Freebanking.org*, August 30, 2013. .

[18] Ludwig von Mises, "Elastic expectations and the Austrian Theory of the Trade Cycle," *Economica*, August 1943.

[19] George Selgin, David Beckworth, and Berrak Bahadir, "The Productivity Gap: Monetary Policy, the Subprime Boom, and the Post-2001 Productivity Surge." Unpublished working paper, September 2013. The paper includes a comparison of its own estimates with those based upon a Taylor rule.

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