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## THE DUTCH MONETARY ENVIRONMENT DURING TULIPMANIA

DOUG FRENCH

When the economics profession turns its attention to financial panics and crashes, the first episode mentioned is tulipmania. In fact, tulipmania has become a metaphor in the economics field. Should one look up tulipmania in *The New Palgrave: A Dictionary of Economics*, a discussion of the seventeenth century Dutch speculative mania will not be found. Guillermo Calvo (1987, p. 707), in his contribution to the *Palgrave* instead defines tulipmania as: “situations in which some prices behave in a way that appears not to be fully explainable by economic ‘fundamentals.’”

Brown University economist, Peter Garber, is considered the modern tulipmania expert. In Garber’s view, tulipmania was not a mania at all, but is explainable by market fundamentals. The explosive increases in the price of tulip bulbs, Garber says, can be explained by supply and demand factors. Rare bulbs were hard to reproduce and in the greatest demand. Thus, rare bulbs tended to rise in price. However, this does not explain the price history of the common Witte Croonen bulb, that rose in price twenty-six times in January 1637, only to fall to one-twentieth of its peak price a week later (Garber 1989, p. 556).

Garber admits in more recent works (2000, p. 80) that the “increase and collapse of the relative price of common bulbs is the remarkable feature of this phase of the speculation.” Garber in his own words “would be hard pressed to find a market fundamental explanation for these relative price movements.”

In addition to his “fundamentals” argument, Garber (1990b, p. 16) points to the Bubonic Plague as a possible cause of tulipmania. “Although the plague outbreak may be a false clue, it is conceivable that a gambling binge tied to a drinking game and general carousing may have materialized as a response to the death threat.” This fatalistic extension of Keynes’s “animal spirits” hypothesis is less than convincing.

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Economic historian, Charles Kindleberger, in spite of referring to tulipmania as “probably the high watermark in bubbles” (1984, p. 215) gives the episode scant treatment in his *Manias, Panics, and Crashes: A History of Financial Crises* (1989).<sup>1</sup> Kindleberger’s view of tulipmania may be gleaned from a footnote on page seven of the second (1989) edition. “Manias such as . . . the tulip mania of 1634 are too isolated and lack the characteristic monetary features that come with the spread of banking after the opening of the eighteenth century.”

It is highly probable that in Kindleberger’s view the supply of money in 1630s Holland, did not undergo the sudden increase needed to create a speculative bubble. But this paper will present evidence to the contrary; the supply of money did increase dramatically in 1630s Holland, serving to engender the tulipmania episode.

After the fall of the Roman Empire, many different money systems prevailed throughout Europe. Kings were eager to strike their own gold and silver coins. These coins were typically made full legal tender, at a ratio of value fixed by the individual states. This supreme right of coinage was exercised and misused by every sovereign in Europe.

After the fall of Byzantium, coins struck with sacred images disappeared. These sacred images had kept the superstitious masses, not to mention states, from altering the coins. But, without these sacred images, those gold and silver coins underwent numerous alterations, to the point where it was difficult to follow either a coin’s composition or value. This “sweating,” “clipping,” or “crying” of coins continued right up to the beginning of the seventeenth century, with all of Europe’s various rulers being guilty. These kings quickly found that an empty state treasury could be filled by debasing the currency.

The powerful Charles V was among the most culpable for altering the value of money. These alterations in the Netherlands came by monetary decree. In 1524, Charles raised the value of his gold coins from 9 or 10, to 11 3/8 times their weight in silver coins. This manipulation created immense displeasure throughout the kingdom and in 1542, Charles returned to a ratio of ten to one, not by lowering the value of his gold coins back to their value before 1524, but by degrading his silver coins. Four years later, in 1546, Charles struck again suddenly raising the value of his gold coins to 13 1/2 times the value of silver coins. These actions served to first overvalue and undervalue gold in relation to its market value to silver,<sup>2</sup> with the result being that the overvalued money drove the undervalued money out of circulation. This is a phenomenon known as Gresham’s Law. A silver ducat went from 54

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<sup>1</sup>In the 1996 edition of *Manias, Panics, and Crashes*, Kindleberger added a chapter on Tulipmania. Garber (2000, p. 77) believes that Kindleberger added the chapter “to critique [Garber’s] view that the tulipmania was based upon fundamentals.”

<sup>2</sup>The ratio of silver to gold from 1524 to 1546, based on the average for Europe, fluctuated between approximately 10 1/2 and 11 (Rich and Wilson, eds. 1975, p. 459).

grains fine down to 35 grains fine (Del Mar 1969a, p. 345). Thus, with silver coins being the primary circulating medium of Holland, the new debased and overvalued silver coins drove the undervalued gold coins from circulation. This action raised the value of gold nearly 50 percent and by this device, Charles was able to replenish his dwindling treasury.

This transgression, in 1546, wrote Del Mar (1969a, p. 348) may have been “the straw that broke the patience of his long suffering subjects.” A revolution was then sparked in the Netherlands and although Charles was able to check any upheaval during his reign, with the accession of Phillip the Bigot, the smoldering revolutionary fires burst into intense flames. After the “Confederation of Beggars” formed in 1566, six years later the revolution was proclaimed.

One of the first measures instigated by the revolutionary government was “free” or “individual” coinage. Helfferich (1969, p. 370) explains:

The simplest and best-known special case of unrestricted transformation of a metal into money is that known as “the right of free coinage,” or “coinage for private account.” The State will mint coins out of any quantity of metal delivered to it, either making no charge to the person delivering the metal, or merely a very small charge to cover cost. The person delivering the metal receives in coin from the mint the quantity of the metal delivered up by him either without any deduction or with a very small deduction for seigniorage.

The idea of free coinage was brought to the Netherlands from the Dutch East Indians, who inherited the concept from the Portuguese. The practice was originated by the degenerate Moslem governments of India, and was copied by Mascarenhas in 1555 (Del Mar 1969a, pp. 344-51).

Free coinage was an immediate success. Possessors of silver and gold bullion obtained in America, “had vainly sought to evade the coinage exactions of the European princes; now the door of escape was open; they had only to be sent to Holland, turned into guilders and ducats, and credited as silver metal under the name of *sols banco*” (Del Mar 1969a, p. 351).

As the seventeenth century began, the Dutch were the driving force behind European commerce. With Amsterdam as capital of Holland, it served as the central point of trade. Amsterdam’s currency consisted primarily of the coins of the neighboring countries and to a lesser extent its own coins. Many of these foreign coins were worn and damaged, thus reducing the value of Amsterdam’s currency about 9 percent below that of “the standard” or the legal tender. Thus, it was impossible to infuse any new coins into circulation. Upon the circulation of newly minted coins, these new coins were collected, melted down, and exported as bullion. Their place in circulation was quickly taken by newly imported “clipped” or “sweated” coins. Thus, undervalued money was driven out by overvalued or degraded money, due to the legal tender status given these degraded coins (Smith 1965, p. 447).

To remedy this situation, the Bank of Amsterdam was originated in 1609. The Bank was to facilitate trade, suppress usury, and have a monopoly on all trading of specie. But the bank's chief function was the withdrawal of abused and counterfeit coin from circulation (Bloom 1969, pp. 172-73). Coins were taken in as deposits, with credits, known as bank money, issued against these deposits, based not on the face value of the coins, but on the metal weight or intrinsic value of the coins. Thus, a perfectly uniform currency was created. This feature of the new money, along with its convenience, security and the city of Amsterdam's guarantee,<sup>3</sup> caused the bank money to trade at an agio, or premium over coins. The premium varied (4 to 6 and 1/4 percent), but generally represented the depreciation rate of coin below its nominal or face value (Hildreth 1968, p. 9).

One of the services that the bank provided was to transfer, upon order from a depositor, sums (deposits) to the account of creditors, by book entry. This is called a giro banking operation. This service was so popular that the withdrawal of deposits from the bank became a very rare occurrence. If a depositor wanted to regain his specie, he could easily find a buyer for his bank money, at a premium, due to its convenience. Additionally, there was a demand for bank money from people not having an account with the bank (Clough 1968, p. 199). As Adam Smith (1965, pp. 447-48) related in the *Wealth of Nations*: "By demanding payment of the bank, the owner of a bank credit would lose this premium." The city of Amsterdam's guarantee, in addition to the requirement that all bills drawn upon or negotiated in Amsterdam, in the amount of six hundred guilders or more, must be paid in bank money, "took away all uncertainty in the value of the bills," and thus forced all merchants to keep an account at the bank, "which necessarily occasioned a certain demand for bank money."

Smith (1965, pp. 448-49) goes on to explain the mechanics of how the Bank of Amsterdam issued bank money. The Bank would give credit (bank money in its books for gold and silver bullion deposited, at roughly 5 percent below the bullion's then current mint value. At the same time as this bank credit was issued, the depositor would receive a receipt that entitled the depositor, or bearer, to draw the amount of bullion deposited from the bank, within six months of the deposit. Thus, to retrieve a bullion deposit, a person had to present to the bank: (1) a receipt for the bullion, (2) an amount of bank money equal to the book entry, and (3) payment of a 1/4 percent fee for silver deposits, or 1/2 percent fee for gold deposits. Should the six month term expire with no redemption, or without payment of a fee to extend for an additional six months, "the deposit should belong to the bank at the price at which it had been received, or which credit had been given in the transfer books." Thus, the bank would make the 5 percent fee for warehousing the deposit, if not redeemed within the six-month time frame. The higher fee

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<sup>3</sup>The City of Amsterdam was responsible for the coin or bullion's security while at the bank, against fire, robbery, or any other accident.

charged for gold was due to the fact that gold was thought to be riskier to warehouse, because of its higher value. A receipt for bullion was rarely allowed to expire. When it did happen, more often than not, it was a gold deposit because of its higher deposit fee.

This system created two separate instruments that were combined to create an obligation of the Bank of Amsterdam. As Smith (1965, p. 450) explains:

The person who by making a deposit of bullion obtains both a bank credit and a receipt, pays his bills of exchange as they become due with his bank credit; and either sells or keeps his receipt according as he judges that the price of bullion is likely to rise or to fall. The receipt and the bank credit seldom keep long together, and there is no occasion that they should. The person who has a receipt, and who wants to take out bullion, finds always plenty of bank credits, or bank money to buy at ordinary price; and the person who has bank money, and wants to take out bullion, finds receipt-salways in equal abundance.

The holder of a receipt cannot draw out the bullion for which it is granted, without re-assigning to the bank a sum of bank money equal to the price at which the bullion had been received. If he has no bank money of his own, he must purchase it of those who have it. The owner of bank money cannot draw out bullion without producing to the bank receipts for the quantity which he wants. If he has none of his own, he must buy them of those who have them. The holder of a receipt, when he purchases bank money, purchases the power of taking out a quantity of bullion, of which the mint price is five per cent. above the bank price. Theagio of five per cent. therefore, which he commonly pays for it, is paid, not for an imaginary, but for the real value. The owner of bank money, when he purchases a receipt, purchases the power of taking out a quantity of bullion of which the market price is commonly from two to three per cent. above the mint price. The price which he pays for it, therefore, is paid likewise for a real value. The price of the receipt, and the price of the bank money, compound or makeup between them the full value or price of the bullion.

The same system that Smith describes above, also applied to coins that were deposited with the bank. Smith (p. 451) does assert that deposits of coinage were more likely to “fall to the bank” than deposits of bullion. Due to the highagio (Smith indicates typically five percent) of bank money over common coin, the paying of the bank’s six-month storage fee created a loss for holders of receipts.

The amount of bank money for which the receipts had expired, in relation to the total amount of bank money was very small. Smith (p. 451) writes:

The bank of Amsterdam has for these many years past been the great warehouse of Europe for bullion, for which the receipts are very seldom allowed to expire or, as they express it, to fall to the bank. The far greater part of the bank money, or of the credits upon the books of the bank, is supposed to have been created, for these many years past, by such deposits which the dealers in bullion are continually both making and withdrawing.

The bank was highly profitable for the city of Amsterdam. Besides the aforementioned warehouse rent and sale of bank money for the *agio*, each new depositor paid a fee of ten guilders to open an account. Any subsequent account opened by that depositor would be subject to a fee of three guilders. Transfers were subject to a fee of two guilders, except when the transfer was for less than 600 guilders. Then the fee was six guilders (to discourage small transfers). Depositors were required to balance their accounts twice a year. If the depositor failed to do this, he incurred a 25 guilder penalty. A fee of 3 percent was charged if a depositor ordered a transfer for more than the amount of his account (Smith 1965, p. 454).

In the beginning, the Bank of Amsterdam did not perform a credit function; it was strictly a deposit bank, with all bank money backed 100 percent by specie. The administration of the Bank of Amsterdam was the charge of a small committee of city government officials. This committee kept the affairs of the bank secret. Because of the secretive nature of its administration, it was not generally known that individual depositors had been allowed to overdraw their accounts as early as 1657. In later years, the Bank also began to make large loans to the Dutch East India Company and the Municipality of Amsterdam. By 1790 word of these loans became public and the premium on the bank money (usually 4 percent, but sometimes as high as 6 and 1/4) disappeared and fell to a 2 percent discount. By the end of that year the Bank virtually admitted insolvency by issuing a notice that silver would be sold to holders of bank money at a 10 percent discount. The City of Amsterdam took the Bank over in 1791, and eventually closed it for good in December of 1819 (Conant 1969, p. 289).

The effects of free coinage combined with the stability of the Bank of Amsterdam, created the impetus that channeled the large amounts of precious metals being discovered in the Americas, and to a lesser degree in Japan, toward Amsterdam. The Bank was also prohibited from exporting uncoined precious metals; it had a duty to send its metal to the mint (Van Dillen 1964, pp. 92-93).

After Columbus came to America in 1492 and Cortez invaded Mexico in 1519, an influx of precious metals began to enter Europe, principally through Spain. The output of these fertile mines in the Americas reversed a trend of lower prices in Europe that had been caused by the combination of static metals production in Europe and rapidly expanding industry and commerce. Production in the New World was further increased after the discovery of Peru's Huancavelica mercury mine in 1572. The amalgamation process, which was invented in the mid-sixteenth century, depended heavily on mercury. This process greatly increased the efficiency of the silver production process (Hamilton 1929, pp. 436-43).

The Japanese silver mining industry was also expanding at the same time, but without the benefit of the mercury-amalgam process. The Dutch East India Company had a virtual monopoly on trade with Japan and of course access to their precious metals production from 1611 through the end of the



century. Del Mar (1969b, pp. 307-8) points out that, “from 1624 to 1853 the Dutch were the only Europeans permitted to trade with Japan,” managing “to obtain about one-half of the total exports of the precious metals from Japan.”

Flynn (1983, pp. 162, 164) indicates that:

American output of bullion, in conjunction with the output of Central European and Japanese mines, increased the world's supply of silver sufficiently to slowly drive its market value downward. That is, there was price inflation in the sixteenth century. American and non-American mines produced such an enormous quantity of silver that its market value dropped to a level below the cost of producing it in a growing number of European mines.

Francis Walker (1968, p. 135) validates this view: “the astonishing production of silver at Potosi began to be felt. From 1570 to 1640 silver sank rapidly. Corn rose from about two oz. of silver the quarter, to six or eight oz.” Walker (1968, p. 135) goes on to quote David Hume:

By the most exact computations that have been formed all over Europe, after making allowance for the alterations in the numerary value, or the denomination, it is found that the prices of all things have risen three, four, times since the discovery of the West Indies.

The table in exhibit one illustrates this large influx of precious metals into Europe. Bullion flowed from Spain to Amsterdam due to both trade and seizure of treasure. As Violet Barbour (1963, pp. 49-50) relates:

In 1628 occurred the famous capture of the Spanish treasure fleet by Piet Heyn, which netted 177,537 lbs. Weight of silver, besides jewels and valuable commodities, the total estimated to come to 11 ½ to 15 million florins. More important than such occasional windfalls was the share of Dutch merchants in the new silver brought twice a year to Cadiz from the mines of Mexico and Peru, a share which represented in part the profits of trade with Spain and through Spain with the New World. Just what that share was from year to year we do not know. Only a few fragmentary estimates for non-consecutive years in the second half of the century have come to light. According to these the Dutch usually carried off from 15 to 25 per cent of the treasure brought by the galleons and the flota, their share sometimes exceeding, sometimes falling below the amounts claimed by France or Genoa.

Del Mar (1969b, pp. 326-27) echoes this view:

The honest Abbe Raynal explains the whole matter in a few words: whilst the Portuguese robbed the Indians, the Dutch robbed the Portuguese. “In less than half a century the ships of the Dutch East India Company took more than three hundred Portuguese vessels . . . laden with the spoils of Asia. These brought the Company immense returns.” Much of eastern gold, which found its way to Amsterdam was proceeds of double robbery.

Further evidence of an exceptionally large increase in the supply of money in the Netherlands is provided by an excerpt from a table of the total mint output of the South Netherlands, 1598-1789, which is displayed in Figure 2. These figures point to the explosive increase in the supply of money from 1630-38, the later part of which tulipmania took place (1634-37).

Figure 3 displays the balances of the Bank of Amsterdam. Total balances more than doubled from less than four million florins in 1634 to just over eight million in 1640. More specifically from January 31st 1636 to January 31st 1637—the height of the tulipmania—Bank of Amsterdam's deposits increased 42 percent.

As the above evidence indicates, free coinage, the Bank of Amsterdam, and the heightened trade and commerce in Holland served to attract coin and bullion from throughout the world. As Del Mar (1969a, p. 351) writes:

Under the stimulus of “free” coinage, an immense quantity of the precious metals now found their way to Holland, and a rise of prices ensued, which found one form of expression in the curious mania of buying tulips at prices often exceeding that of the ground on which they were grown.

Del Mar (1969a, p. 352) goes on to discuss the end of Tulipmania:

In 1648, when the Peace of Westphalia acknowledged the independence of the Dutch republic, the latter stopped the “free” coinage of silver florins and only permitted it for gold ducats, which in Holland had no legal value. This legislation discouraged the imports of silver bullion, checked the rise of prices, and put an end to the tulip mania.

Del Mar concedes in a footnote that the mania had already been discouraged on April 27th, 1637 by a resolution of the States-General that canceled all contracts.

By 1636, a formal futures market had developed for the tulip market. Trading took place in taverns in groups, known as “colleges” where rules governed bidding and fees (Garber 2000, p. 44). As Garber (2000, p. 45) explains:

Neither party intended a delivery on the settlement date; only a payment of the difference between the contract and settlement price was expected. So, as a bet on the price of bulbs on the settlement date, this market was not different in function from currently operating futures markets.

The crash of tulip prices in 1637 left the growers of the bulbs to absorb the majority of the financial damage of the mania. With the government basically canceling all contracts,<sup>4</sup> growers could not find new buyers or recover money owed them by buyers supposedly under contract. As Simon Schama (1987, pp. 361-62) describes:

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<sup>4</sup>The Court of Holland judged the tulip sales to be bets under Roman law (Gelderblom and Jonker n.d.).

In any event, the magistrates of the Dutch towns saw niceties of equity as less pressing than the need to deintoxicate the tulip craze. Their intervention was hastened by the urgency of returning the genie speculation to the bottle from which it had escaped, and corking it tightly to ensure against any recurrence. To some extent, they could feel satisfied that the ineluctable operations of Fortuna had already punished the foolhardy by taking them from rags to riches and back again in short order. But they still felt impelled to launch a didactic campaign in tracts, sermons and prints against folly, since its special wickedness had been in leading the common people astray.

In spite of the short duration of the tulip craze, and assertions by other authors to the contrary, there is evidence of financial pain that resulted from Tulipmania. A chart depicting the number of annual bankruptcies in Amsterdam, Leiden, Haarlem, and Groningen from 1635-1800, presented by Messrs. van Houtte and van Buyten (1977, p. 102), reflects a doubling in the number of bankruptcies in Amsterdam from 1635 to 1637. It would be hard to imagine that only tulip growers made up this increase in the number of bankruptcies. I suspect some of the "foolhardy masses" were among this group.

The story of Tulipmania is not only about tulips and their price movements, and certainly studying the "fundamentals of the tulip market" does not explain the occurrence of this speculative bubble. The price of tulips only served as a manifestation of the end result of a government policy that expanded the quantity of money and thus fostered an environment for speculation and malinvestment. This scenario has been played out over and over throughout history.

Like other periods of heightened speculation, Dutch interest rates "declined sharply" in the seventeenth century according to Homer and Sylla (1996, p. 141) and tulip bulb futures could be traded with no margin required (Garber 2000, p. 44). The tulip trading clubs were "soundly organized" and "proved very effective in smoothing transactions" (Gelderblom and Jonker n.d.). Dash (1999, p. 110) writes that the masses speculated in the tulip trade as an outgrowth of "an increasingly feverish boom in the Dutch economy as a whole, which began in 1631 and 1632 and gathered pace toward the end of the decade and meant that in many cases there was more money around than ever before."

As more novice florists became interested in tulip speculation, professional growers introduced "an unusually large number of new varieties in 1634, which had the effect of depressing prices," but provided an avenue for commoners to participate in the mania (Dash 1999, p. 111).

But what made this episode unique was that the government policy did not expand the supply of money through fractional reserve banking which is the modern tool. Actually, it was quite the opposite. As kings throughout Europe debased their currencies, through clipping, sweating or by decree, the Dutch provided a sound money policy which called for money to be backed one hundred per cent by specie. This policy, combined with the occasional

seizure of bullion and coin from Spanish ships on the high seas, served to attract coin and bullion from throughout the world.

The end result was a large increase in the supply of coin and bullion in 1630s Amsterdam. Free coinage laws then served to create more money from this increased supply of coin and bullion, than what the market demanded. This acute increase in the supply of money served to foster an atmosphere that was ripe for speculation and malinvestment, which manifested itself in the intense trading of tulips.

Figure 1  
Spanish Imports of Fine Gold and Silver from America (in grams)

Period	Silver	Gold
1503-1510		4,965,180
1511-1520		9,153,220
1521-1530	148,739	4,889,050
1531-1540	86,193,876	14,466,360
1541-1550	177,573,164	24,957,130
1551-1560	303,121,174	42,620,080
1561-1570	942,858,792	11,530,940
1571-1580	1,118,591,954	9,429,140
1581-1590	2,103,027,689	12,101,650
1591-1600	2,707,626,528	19,541,420
1601-1610	2,213,631,245	11,764,090
1611-1620	2,192,255,993	8,855,940
1621-1630	2,145,339,043	3,889,760
1631-1640	1,396,759,594	1,240,400
1641-1650	1,056,430,966	1,549,390
1651-1660	443,256,546	469,430
Total	16,886,815,303	181,333,180

Source: Hamilton (1934; reprinted in Clough 1968, p. 150).

Figure 2  
Total Mint Output of the South Netherlands, 1598-1789 (in guilders)

	Gold	Silver	Copper	Total
1628-29	153,010	2,643,732	4,109	2,800,851
1630-32	364,414	8,838,411	6,679	9,209,503
1633-35	476,996	16,554,079		17,031,075
1636-68	2,917,826	20,172,257		23,090,083
1639-41	2,950,150	8,102,988		11,053,138
1642-44	2,763,979	1,215,645	47,834	4,027,458

Source: Jan A Van Houtte and Leon Van Buyten (1977, p. 100).

Figure 3  
Bank of Amsterdam (in florins)

Years (on the 31st of January)	Total balances	Metal Stock
1630	4,166,159	3,105,449
1631	3,784,047	2,976,742
1632	3,636,079	3,281,113
1633	4,272,224	3,866,890
1634	3,995,666	3,474,527
1635	3,860,342	3,416,112
1636	3,992,338	3,486,306
1637	5,680,522	5,315,576
1638	5,593,750	5,256,606
1639	5,802,729	5,446,002
1640	8,075,358	7,823,964
1641	8,056,232	8,356,437

Source: Van Dillen (1964, p. 117).

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# AN AUSTRIAN VERSION OF THE LUCAS CRITIQUE

TOBIAS BASSE

*If your idea's not defensible,  
don't make it comprehensible. . . .*

Kenneth Boulding

The Austrian School of economics is mainly interested in human action. Austrian economists acknowledge that individual human beings act and that interacting economic agents form a complex system called the economy. Consequently, Austrians try to understand economic phenomena by analyzing human action. Compared to mainstream economists the methodology applied by scholars classified as Austrian usually is less formal. Indeed, the Austrian School of economics tends to criticize econometric analysis and formal modelling of the general equilibrium type. Instead of relying on formal models, Austrian economists usually start with explanatory axioms and then use thought experiments to try to understand economic problems (Rothbard 1976, pp. 19–21; Selgin 1988, pp. 20–28; Herbener 1996, pp. 97–98; and Backhouse 2000, p. 32). There is ample evidence for an aversion to mathematics and econometrics in the writings of scholars of the Austrian School of economics. The most prominent example, of course, is Mises, who argues: “There is no such thing as quantitative economics. All economic quantities we know about are data of economic history” (Mises 1996, p. 351).

Scholars of Austrian economics argue persuasively that formal models are not able to capture the complex dynamics of market processes. In the eyes of Austrian economists the market is not only an abstract place of exchange between buyers and sellers of goods, but also a process that helps to generate knowledge by letting economic agents reveal their preferences in voluntary exchanges. Profit-seeking individuals continuously will try to detect chances

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to increase profit by observing market prices of all sorts of goods and redirecting factors of production to more profitable use. Thus, without any form of central planning, production will be adjusted to the preferences of economic agents. The market process is a sequence of discovery procedures helping to deploy resources in more adequate ways. From this point of view an aversion to mathematics and econometrics is absolutely understandable; in spite of recent advances in computer technology and the theory of complexity, mainstream economists still are not (and most probably never will be) able to adequately model the inherent dynamics of the sequence of discovery procedures called the market process.<sup>1</sup> But, of course, Austrian economists are not alone criticizing the use of econometric techniques. The proponents of the rational expectations hypothesis, for example, do also argue that there are severe problems when policymakers try to actively manipulate economic variables based on the results of econometric analysis. This is the famous Lucas critique which will now be examined.

#### STRUCTURAL BREAKS AND THE LUCAS CRITIQUE

The methods of econometrics and statistics cannot be applied unconditionally to all sorts of economic problems. In order to use these statistical techniques there has to be a somewhat constant relationship between the relevant variables in the period observed. Austrian economists, of course, argue that there are no constant relationships in the field of human action. At this point it would obviously be a big mistake not to quote Mises: "In the field of human action, however, there are no such constants. The equations of mathematical economics are therefore useless for all practical purposes" (Mises 1977, p. 99).

This critique is taken extremely seriously in the field of financial econometrics. The adherents of the Variable Beta Model, for example, argue that the factor BETA (which is the coefficient used to measure risk premia in regressions based on the concept of the Capital Asset Pricing Model) is not constant over time (Krueger and Rahbar 1995, pp. 36-42). They try to model the time variation of this coefficient of a regression model. But, of course, the approach requires that there is a quite stable relationship between the coefficient BETA and the exogenous variables used to explain BETA. Thus, the problems due to the lack of constants in economics are not really solved using a Variable Beta Model.

Mainstream economists might also note that, other things being equal, economic agents will behave in a more or less stable way. This point of view is understandable and may even be accepted by quite a number of Austrian economists (Hoppe 1997, pp. 72-73). As a consequence, the problems with

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<sup>1</sup>Kirzner (1996, p. 150) and Herbener (1996, p. 157), for example, note that advances in computer technology will not help to overcome the problems of formal modelling. See also Vedder (1997, p. 77).



the variability of human action mainly become problems with structural breaks forcing economic agents to change their behavior. Mainstream economists could go on to argue that *ceteris paribus* structural breaks are not very likely to occur; of course, oil price shocks and similar exogenous factors with major influence on the behavior of economic agents are exceptional events. Additionally, econometricians can reduce the number of data points observed in order to minimize problems with known structural breaks in time series analysis. But there is a more fundamental problem because the *ceteris paribus* assumption usually will not hold. Mainstream economists regularly use econometric analysis to justify government intervention. At this point the so called Lucas critique is crucial. Lucas argues that changes in economic policy can systematically affect the behavior of economic agents:

[G]iven that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any change in policy will systematically alter the structure of econometric models. (Lucas 1976, p. 41)

While Mises points out that there are no constant relationships in economics (so that econometric analysis can only be applied in the field of economic history) the Lucas critique goes even further. If there were constant relationships in advance of policy actions these relationships would have to break down after the introduction of new guidelines for economic policy. Profound changes in economic policy will in other words necessarily create structural breaks. Thus, careful econometric analysis of economic phenomena at a certain point of time cannot be used to investigate future effects of policy actions. Active economic policy generally is not compatible with econometric analysis and consequently econometric analysis cannot be used to justify policy actions. This problem has also been observed by a number of econometricians (Christ 1993, pp. 77-78; Granger 1999, p. 54; Boivin and Giannoni 2002, p. 105; and Granger 2003, p. 71).

The arguments put forward by Lucas have also spurred more general criticism of quantitative economics only loosely tied to the Lucas critique. Mainstream economists and econometricians have begun to concede that there is no true econometric model that can perfectly describe an economy (Christ 1993, pp. 74-75; Eichenbaum 1996, pp. 22-23; Leamer 1994, pp. 66-69; and Pagan 2002, pp. 2-3). Because of the fact that models always are at least partly wrong a number of mainstream economists even questioned whether models actually can be tested (Eichenbaum 1996, p. 22 and Leamer 1994, pp. 66-67). Leamer for example argues:

A model is a powerful device for organizing our thoughts; it is not literally true; indeed it derives its power from the very fact that it is not literally true. Thus there is no reason to test it. Instead of testing, we should be determining its accuracy and its usefulness. We should attempt to identify

empirical circumstances in which the model is useful and other circumstances in which it is misleading. (Leamer 1994, p. 66)

Thus, one surely has to conclude that the arguments presented by Lucas have been extremely influential. Lucas and his followers challenged the whole profession of quantitative economists. Econometricians simply had to accept this serious challenge and began to strike back. In fact, McCallum notes that the emergence of vector autoregression (VAR) models in the 1980s was a direct response to the Lucas critique (McCallum 1982, pp. 9-10).<sup>2</sup> It is well known that VAR models are nonstructural and try to explain variables as linear combinations of lagged values of itself and lagged values of all other variables observed. Thus, the structure of a VAR model is not based on theoretical considerations but on the dynamics of the data examined in the model. It could be argued that VAR models pose a challenge to the Lucas critique by not relying on particular structural relationships. But this challenge is a rather dubious one. Considering VAR models and the Lucas critique McCallum argues:

[T]hey consist of a set of reduced-form equations in which lagged values of the system's variables are used to explain current values, with all variables treated as endogenous. Consequently, VAR systems would seem to be even more vulnerable to the critique than the traditional models that Lucas considered. (McCallum 1982, p. 10)

Consequently, VAR models cannot help econometricians to discard the Lucas critique. This is, for example, also acknowledged by Sargent (1979, p. 8). Discussing VAR models he notes:

Further, while the techniques were developed partly in response to criticisms of standard simultaneous equation macroeconomic models, they are not intended to remedy all the defects in the standard models pointed out by critics like Lucas. (Sargent 1979, p. 14)

Additionally, Austrian economists (although being extremely critical concerning the positivist approach to philosophy favored by Popper) will most probably also agree that nonstructural econometric VAR models seem to be even more questionable than traditional econometric models because it is impossible to use econometrics to falsify theoretical concepts that already have been fitted to the historical evidence. Moreover, VAR models are not completely atheoretical because appropriate variables have to be selected in advance of estimating the model (Leamer 1985, p. 286). It seems to be obvious that the selection of adequate variables will somehow be based on economic theory. Thus, Leamer concludes: "[C]an causal inference be made in the context of vector autoregressions without relying on a priori theory? The answer is quite clearly no" (Leamer 1985, p. 256).

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<sup>2</sup>Also, see Sargent (1979, p. 14).

Challenging the Lucas critique mainstream economists could also argue that most policy actions take place within a given framework. Using economic policy instruments within this given framework can be interpreted as normal policymaking. One might think that all policy actions of the normal policymaking type will not result in structural breaks. From this point of view the Lucas critique will only apply to substantial changes of policy regimes. But what does the word substantial mean in this context? Moreover most mainstream economists that try to challenge the Lucas critique favor discretionary policy regimes. Actually their opposition to the Lucas critique mainly stems from their aversion to policy regimes based on clear rules (as advocated by Lucas). But without clear rules there is no given framework for economic policy. Consequently the private sector will have enormous difficulties distinguishing between “normal policymaking” and “changes in policy regimes.” Not being able to rely on clear rules economic agents will be forced to change their behavior more frequently when policy action is observed.

There is also more fundamental criticism of the Lucas critique. The traditional Lucas critique is based on the rational expectations hypothesis. Thus, it is assumed that economic agents will use all information currently available to forecast future events. Consequently, there will be no systematic forecast errors and the private sector will predict all systematic policy actions based on economic planning. Due to rational expectations economic agents will adjust their behavior and systematic economic policy actions will consequently have no real effects. Only unforeseen random shocks originating from economic policy or exogenous events will not be anticipated. But no reasonable mainstream economist will advise government institutions to randomly select policy actions without economic planning. Some economists argue that policymakers have more information and thus may be able to forecast random exogenous shocks and then respond to these shocks. But it is of course impossible to forecast random shocks and even if government agencies had information advantages bureaucrats certainly would not be able to respond timely using the additional information. More profound criticism of the rational expectations hypothesis and the Lucas critique comes from the scholars of the Austrian School of economics.<sup>3</sup> Hoppe, for example, argues persuasively that it would be feasible for interested observers to provide a list of all possible actions of economic agents if expectations were indeed formed according to the rational expectations hypothesis (Hoppe 1997, p. 56). Needless to say that there is no list of this kind because nobody is capable of writing down all actions economic agents possibly could think of. Additionally, the rational

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<sup>3</sup>See, for example, Garrison (1986, pp. 94-95) and Hoppe (1997, pp. 56-59). But Yeager argues that there are certain similarities between Austrian economists and the proponents of the rational expectations approach. See Yeager (1987, p. 6). Similar comments also come from Garrison.

expectations theory assumes that all human beings have identical knowledge (p. 57). This assumption is also not very realistic.

One further important argument against the rational expectations hypothesis seems to be that gathering information is costly. Thus, it is not always rational for economic agents to form rational expectations. This very simple but devastating argument against the rational expectations hypothesis clearly shows a major weakness of contemporary economic theory: Economists still do not exactly know how expectations are formed. Most probably distinct economic agents use different methods to form their expectations. The forecasts of human individuals surely are influenced by their experiences and subjective beliefs. In spite of the fact that the exact way expectations are formed is a mystery it is very likely that individual economic agents somehow will adapt to changes in their environment. They will not stick to their original forecasts if significant changes take place. Thus, it seems to be certain that expectations in changing environments are not formed according to the static expectations hypothesis. Assuming that successful behavior by individual economic agents will be imitated seems to be reasonable. Thus, in the long run the economy probably will arrive at the equilibrium solution suggested by the rational expectations theory. There is, of course, an additional problem because of the fact that rational expectations models may have multiple equilibria. As a consequence investigating how expectations are formed can (but not necessarily will) help to select an appropriate equilibrium (Bullard 1991, pp. 51, 57). In any case the way to a new equilibrium surely is more painful than suggested by the elegant rational expectations approach.

At this point an important question arises: Does the Lucas critique really rely on the rational expectations hypothesis? The answer to this question quite clearly is no. As long as forecasts are not generally formed based on static expectations the Lucas critique is indeed valid because individual economic agents will react to profound changes in economic policy by altering their expectations. Disengaged from the rational expectations hypothesis this new version of the Lucas critique is even more devastating for economic policymakers and econometricians. In the long run the solutions suggested by the rational expectations approach may be correct because successful behavior of individual economic agents will be imitated by others. Thus, economic policy has to be ineffective in the long run. But the elegant rational expectations approach completely disregards painful adjustment processes. Resources will be wasted by responding to the new settings of economic policy. Without further knowledge of the way expectations are formed mainstream economists will also not be capable to construct formal models that show how a new equilibrium can be reached.

The Phillips curve discussion is an excellent example for the new form of the Lucas critique. The idea of this curve originated from econometric research that was conducted by Phillips using British wage and price data starting in 1861 (Phillips 1958, pp. 283–99). According to the data there was a trade off between unemployment and wage growth in this period. High wage

growth seemed to be related to low unemployment rates. This is hardly a surprise for most economists. But mainstream economists soon began to transform the original wage-change version of the Phillips curve presenting a price-change version relating inflation to unemployment. Based on this interpretation of the results of Phillips's econometric analysis some mainstream economists concluded that policymakers could rather freely choose between the two evils of unemployment and inflation. But of course in large parts of the period observed by Phillips money creation in Great Britain was controlled by the rules of the gold standard and so there was no active monetary policy (Glasner 1989, p. 150). Thus, following the Lucas critique econometric analysis of this period cannot help to predict the effects of active monetary policy. Economic agents in the good old times of the gold standard simply did not have to expect that inflation could rise due to a monetary expansion triggered by a central bank freed from specific rules. In the end monetary authorities produced stagflation by unsuccessfully trying to manipulate the unemployment rate in accordance to the principles of the Phillips curve. This historical example clearly indicates that the Lucas critique has to be taken seriously.

Historical experience also shows that moving from one equilibrium to another equilibrium was a very painful experience. The fatal experiment now called stagflation period illustrates that monetary policy induced considerable uncertainty because higher inflation rates are known to also increase the variability of inflation (McClure and Willet 1988, pp. 180-81; and Golob 1994, p. 29). As a consequence, the ability of market prices to co-ordinate the allocation of resources was obstructed. Economic agents needed time to adopt their behavior to the new situation of high inflation rates. In this process of trial and error resources were wasted (for example to produce financial innovation) but in the end the equilibrium suggested by the rational expectations approach prevailed. The trade off between inflation rates and unemployment rates observed under very different conditions broke down completely and active monetary policy produced higher inflation rates without lowering unemployment rates. Thus, the historical evidence from the stagflation period in the 1970s and early 1980s clearly suggests that it is not the elegant rational expectations version of the Lucas critique (suggesting a rather painless ride from equilibrium A to equilibrium B) but the new version of the Lucas critique presented here that makes econometrics a very dangerous tool.

#### A RESEARCH AGENDA

The proponents of the Austrian School of economics could use the new version of the Lucas critique presented above as an additional argument to explain their aversion to formal modelling and econometrics. Most probably some mainstream economists will accept this point of view. But without empirical evidence the Austrian version of the Lucas critique certainly will not convince many mainstream economists from the limitations of econometric

analysis. Consequently, a somewhat different research strategy will be advocated here. Austrian economists should examine historical events and empirically test the validity of the Lucas critique. Chow breakpoint tests could be used to show that changes in economic policy have produced structural breaks in relevant macroeconomic time series. Thus, econometric research should be applied in a more Austrian way to convince mainstream economists from the possible dangers of using econometric methods. Many Austrian economists probably will argue that this research strategy is not very reasonable. There are indeed some difficulties to be faced. Following Mises, econometric analysis of economic phenomena only can be an investigation in economic history. But Austrian economists regularly present discussions of historical evidence to illustrate their findings and most Austrians will accept that statistical techniques can be used to understand history. Hoppe, for example, argues:

[A]lthough questions of this nature may easily degenerate into idle semantic quibbles such as whether a glass of water is half empty or half full, empirical questions—disagreements on matters of fact—are accessible to empirical research and can, in principle, be decided upon based on the observation of the facts. (Hoppe 1997, p. 74)

Some problems remain because there are no constants in the field of human action. But economic agents will, other things being equal, most probably behave in a quite stable way. This argument has already been discussed above. As a consequence, it seems reasonable to assume that the coefficients of regression models describing the behavior of economic agents in the absence of structural breaks are also more or less stable. This assumption can be tested with tests for parameter constancy. A regression model clearly is not useful when these tests show signs of parameter instability. Otherwise, the model may be applied as a tool to examine historical events. Thus, it is not clear why some proponents of the Austrian School of economics seem to think that using empirical tests to analyze historical evidence is not acceptable at all and why they are opposed to empirically check whether their theoretical considerations can explain past events.<sup>4</sup> As a matter of fact, it would be very helpful if econometric tests of the new Lucas critique could finally convince the majority of mainstream economists that government intervention cannot be justified by examining the past. While the traditional Lucas critique certainly has been very influential its general acceptance has been hampered by its reliance on the theory of rational expectations. Without tying the Lucas critique to the questionable assumptions of the rational expectations hypothesis mainstream economists favoring government intervention will have a hard time to discard the Austrian version of the Lucas critique. Experience clearly shows that the scholars of the Austrian School of economics should try to

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<sup>4</sup>Very similar arguments regarding formal modelling are presented by Backhouse (2000, p. 40).

avoid every risk of not popularizing their more powerful version of the Lucas critique; failing to provide empirical evidence to convince mainstream economists surely would be risky.

Both versions of the Lucas critique prohibit the use of econometrics to justify government intervention of any kind because intervention necessarily leads to structural breaks. Ironically, mainstream economists regularly use econometric methods and are very likely to not follow the guidelines given by the Lucas critique. Austrian economists, on the other hand, are on principle opposed to government intervention and thus will not apply econometric analysis to justify economic policy actions but usually shy away from using econometric techniques. From this point of view an Austrian economist is the perfect econometrician. Austrians should always stress that econometric analysis is only helpful examining the past and thus cannot be applied to forecast future events. This critical attitude towards econometrics can be very helpful to produce ingenious econometric research. Austrian economists, for example, surely never would dare to run a regression without prior thinking about economic theory. Econometric research performed by Austrians clearly would also help to show that numerous proponents of active government intervention try to hide their questionable ideas behind batteries of cryptic test statistics; examining the research activities of some mainstream economists one can easily get the impression that Kenneth Boulding is not exaggerating with his ironic comments quoted above.

As a matter of fact, econometric analysis can be a very powerful research tool when certain basic rules are obeyed. Austrians definitely should not accept that this powerful tool is used solely by mainstream economists who almost certainly will disobey one of the rules by justifying policy action with their empirical work. Today many econometricians seem to be ready to understand the Austrian message. As noted above, numerous econometricians have begun to question how econometric research was performed in the past. In this context it has to be noted that there is a relatively new technique in econometrics called cointegration analysis. This technique is based on the idea that in the long run there are more or less stable relationships between certain variables. In the short run variables can move away from such long run equilibria. But if two variables are indeed cointegrated there will be a mechanism (or process) brining about a move towards equilibrium. Cointegration analysis even provides a new and probably more Austrian interpretation of equilibrium situations (Granger 1991, p. 68). Thus, the techniques of cointegration analysis are an interesting tool to examine historical data when studying market processes. It is, for example, very likely that the prices of one good in two different markets are cointegrated. The prices of two very similar goods in one market also are most probably cointegrated. These ideas seem to be very Austrian because the scholars of the Austrian School of economics have always argued that economic actions take place in time and that the neoclassical idea of equilibrium without any reference to time is not very helpful. Garrison for example notes: "Regaining a healthy respect for the temporal element

requires that we look at the market process that transforms a sequence of short runs into a long run" (Garrison 1986, p. 90).

Cointegration analysis interprets disequilibria in the short run as phenomena that will lead to processes of adjustment. Thus, cointegration analysis is an investigation in the temporal elements of market processes and can be seen as a modelling strategy that combines short-run dynamics and long-run equilibria. As a consequence, the techniques of cointegration analysis even may enable Austrian economists to apply methods of econometric research without having to sacrifice their central themes.

### CONCLUSION

Lachmann once suggested that Austrian economists should actively participate in the discussion between mainstream economists to rehabilitate the ideas of the Austrian School of economics (Lachmann 1976, p. 220). This article tries to follow Lachmann by presenting an Austrian version of the famous Lucas critique. The new version of the Lucas critique is more powerful than the traditional one. It is not based on the rational expectations hypothesis and thus cannot be discarded by showing that the rational expectations approach is questionable. Moreover, while the traditional Lucas critique implies that active economic policy is ineffective (but free of costs) the Austrian version of the Lucas critique shows that active economic policy generates uncertainty and wastes resources. But it is also argued that Austrian economists should not use this new version of the Lucas critique to generally dismiss econometrics. The Lucas critique only shows that the consequences of government intervention cannot be analyzed by using econometrics. Applied in the right way econometrics in fact can be a very helpful research tool for economists studying historical events. Especially the relatively new techniques of cointegration analysis seem to be an appropriate way of studying market processes by explicitly modelling learning and the adjustment towards a new equilibrium.

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# RESTRUCTURING BEFORE PRIVATIZATION— PUTTING THE CART BEFORE THE HORSE: A CASE STUDY OF THE STEEL INDUSTRY IN ROMANIA

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The Eastern European countries have been going through a transition phase since the liberalization of their economies with the collapse of communist regimes in the early 1990s. The attempts at transition from a centrally planned economy to a market based one has provided a fascinating laboratory for research in both theory and practice. The problems of how to make the transition are not fully resolved. Economists are still debating various strategies. While the neoclassical economists focus on the macroeconomic issues of the transition process, the Austrian School puts the focus on the microeconomic issues.<sup>1</sup> Part of the ongoing debate among neoclassical economists is whether state owned industries need to be restructured before they can be privatized.

Romania is an interesting case that has not received much attention.<sup>2</sup> Among the Eastern bloc countries, Romania stands out as having been virtually gutted after forty-two years as a “people’s republic.” The terror and megalomania that characterized Ceausescu’s regime since the early 1960s left the country completely unprepared for the transition to capitalism. This paper

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<sup>1</sup>Rothbard (1992) has provided insight into an Austrian perspective on these issues.

<sup>2</sup>Hefner and Woodward (1999) discuss the role of foreign direct investment in developing entrepreneurial activity and successful business practices in Romania. Coyne and Leeson (2004) more recently discuss the role of three types of entrepreneurship, productive, unproductive, and evasive, in underdeveloped countries using Romania as a case study.

examines the progress of the Romanian economy during the decade of transition while focusing on the restructuring debate illustrated by the Romanian steel industry.<sup>3</sup> The steel industry is particularly interesting since it is often viewed as one of the major industries in centrally planned economies. The steel industry has been analyzed by Wagner (1998), Kotas and Markus (1999), and Kotas and Price (2000). All have argued that the Eastern European steel industry needs to reorganize and restructure before it privatizes to be successful in the transition.

In the next section we present an overview of what may be termed the “neoclassical approach” to the transition process. We then describe the history of the Romanian plan for transition that has been implemented. Section four uses Austrian theory to critique and offer an alternative approach. This section is followed with a case study of the Romanian steel industry during the transition. The last section offers concluding remarks on whether or not restructuring is a necessary condition for successful transition to a free market.

#### THE NEOCLASSICAL APPROACH: THE ROAD TO FREEDOM?

According to Parker et al. (1997) the neoclassical transition process typically contains the following four reforms: macroeconomic stabilization, enterprise and price liberalization, elimination of foreign trade and investment barriers, and privatization of state-owned enterprises. Bruno (1994) suggests that in implementing these steps, there are several questions that neoclassical economists find themselves asking. Are initial price shocks necessary? Is an output collapse unavoidable? Can fiscal balance be achieved? How should large state-owned enterprises be privatized? “The problem is how to instill market-oriented behavior in management and workers without unduly delaying the privatization stage” (Bruno 1994, p. 45).<sup>4</sup>

##### *Gradualism versus Shock Therapy*

There are two main perspectives on how the transition process and privatization should occur: (1) a gradual process or (2) a “big-bang” or “shock-therapy” approach to the establishment of property rights. Bruno (1994, p. 27) states, “the key question to be asked at the outset of any major reform is whether to adopt the big bang approach or gradualism.” He continues by claiming that “The answer to this question is not at all clear.”

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<sup>3</sup>Moving from a soviet style command economy to a truly free-market is a complex process involving the establishment and evolution of institutional arrangements that support a private property rights system. The complete analysis of this process from the neoclassical or Austrian perspective is beyond the scope of this paper. Instead we focus our attention on one aspect of this process; namely the debate as to whether to restructure state run enterprises before they are made private.

<sup>4</sup>Hefner and Woodward (1997) present two case studies in Poland that analyze the managerial problems in the transition.

Cohen (1993) argues that gradualism is the key for the success of the transition process in Eastern Europe. He claims that a rapid transition will fail and the outcome will be massive unemployment, bankruptcies, and social distress, ultimately requiring the intervention of the state. In this view, the development of proper institutional structures, such as tax laws, financial intermediaries, and capital markets, must precede the formation of property rights.

Economists who favor rapid transition are concerned with many of the same problems, but claim the key is macroeconomic stability. Macroeconomic stability for them consists of low inflation, economic growth, low unemployment, and stable fiscal and monetary policy. Sachs (1997) argues that monetary growth, due to budget deficits, additional government spending, and credit expansion, is the main cause of inflation. He favors the elimination of fiscal deficits and the creation of a tax base, which promotes monetary stability and confidence in the currency and the government. Although he does not favor price controls, he argues that wage controls can be useful in short-run for state-owned enterprises. Bruno (1994) on the other hand, argues that the monetary overhang and price liberalization that resulted in unstable prices throughout Eastern Europe could have been less severe, had price controls been left in place longer and the monetary devaluation smaller. He further argues that the declines in output should be gradual to reduce the harm to firms and employees. He suggests that the state offer subsidies and loans to weak firms.<sup>5</sup>

According to Sachs (1992), privatization of enterprises is important for a successful transition. However, due to a process he refers to as spontaneous privatization, whereby managers liquidate assets, citizens have been left with a negative impression of privatization. Thus, he claims that commercialization should be the first step toward privatization. Commercialization converts the enterprise into a treasury owned joint-stock company.<sup>6</sup> While Lipton and Sachs (1990) favor a rapid approach they doubt that it will produce immediate increases in productivity or managerial efficiency.

After commercialization, the next step requires the restructuring of the enterprises. Although a logical course of action, this policy prevents the rapid implementation of reforms within individual enterprises. Boycko and Shleifer (1993) argue that the prerequisite for this step is the depoliticization of firms.

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<sup>5</sup>It is worth noting that social spending, such as unemployment benefits and the design of programs that soften the negative impact of economic reform, is a recurring issue in the neoclassical literature. See Milner (1991), Sachs (1997), and Bruno (1994); however, they all argue that state intervention in social protection programs should be limited.

<sup>6</sup>Lipton and Sachs (1990, p. 294) define privatization as "the creation anew of the basic institutions of a market financial system, including corporate governance of managers, equity ownership, stock exchanges, and a variety of financial intermediaries, such as pension funds, mutual funds, and investment trusts."

The firms must change their objective from meeting the desires of politicians to maximizing profits. They argue that restructuring reduces the amount of inefficiency, creates product competition, improves corporate governance, and eliminates political control of capital allocation. Their claim is that restructuring is a critical strategy of depoliticization, and hence privatization.

In contrast, Blanchard et al. (1994) question whether traditional Western stabilization methods can be effective in an economy where state ownership prevails. They raise the question of whether or not the lessons of the West can be directly translated to the Eastern bloc countries. In addition, they question the logic of the stages of transformation inquiring whether privatization should come first.

The neoclassical view of transition involves stabilizing the macroeconomy and attempting to liberalize foreign and domestic markets before addressing the issue of private property rights. In the neoclassical paradigm, the establishment of private property involves three major steps in the following order: commercialization, restructuring, and privatization.<sup>7</sup> The main difference between the gradual and rapid approach in the neoclassical transition process is not the agenda of issues, but the pace at which this process is implemented. With the gradual approach everything is done in stages, the “big-bang” approach attempts to address these issues simultaneously. However, with the logical order suggested by neoclassicals both processes would be gradual. The gradual and big bang approach under the neoclassical paradigm involves the state directing the process at every stage, which from an Austrian perspective is the fatal flaw in this policy. While the neoclassicals focus on the macroeconomic issues and debate the timing of the process they miss the larger implication that the logic of their steps to establish private property rights and attain a market is at best backwards.

#### THE NEW ROMANIAN “PLAN”

During the socialist era, Eastern Europeans were denied political and economic freedoms. The “parasitic mentality” that ruined the civil society and crippled economic life, was a result of the lack of separation between individuals and the state, since “all economic and social decisions became political decisions and everybody became a ward of the state” (Dorn 1994, p. 440). The Communist Party in Romania maintained the tightest control over the

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<sup>7</sup>An issue of taxonomy may be of use here. The terms commercialization and restructuring are noted above and the neoclassical definition of privatization, at least according to Lipton and Sachs, is explained in the previous footnote. We accept the neoclassical definition of commercialization and restructuring, throughout this paper. However, the definition of privatization does not conform with the Austrian notion of private capital or factors of production remaining in the hands of capitalists (see Mises 1998). Therefore, when comparing and contrasting these positions we will use the Austrian definition of privatization.

economy and did not realize the impossibility of regaining tight control after moving towards freedom.<sup>8</sup> The population could no longer tolerate the deteriorating condition of their lives, which led to the revolution of December 1989 and the overthrow of communism to adopt democracy.<sup>9</sup>

Examining Romania's transition process reveals that the new government has followed the neoclassical paradigm. It is also clear that after a decade of reform Romania's attempt at a big bang approach has, as predicted, evolved into gradualism. The concept of planning in this context is important, because after years of central planning, the Romanian government is still planning. Only now the government is trying to plan a free market. What follows is a description of the legislation and government intervention that has occurred, and the neoclassical explanation as to why this process has been so slow.

Daianu (2001), Stan (1995), Earle and Sapatoru (1993), and Demekas and Kahn (1991) all claim that the Romanian economy, under the rule of Ceausescu, was the most Stalinist regime in Eastern Europe. The inheritance left by Ceausescu's regime was one of central planning, strict autarky, isolationism, and industrial policy choices that ignored comparative advantages making reform a difficult process (Stan 1995). Therefore, the Romanian economy had more to overcome than other Eastern European countries.

The 1990 interim government's main economic goal was improving living standards. Populist measures were taken that resulted in a boom in consumption, a dramatic decrease in industrial production and investment, and a rise in imports, all of which were financed by foreign exchange reserves. In May 1990, the interim government won the general elections and began to implement its transition plan.

The issue of stabilizing the macroeconomy took precedence. Controlling inflation, unemployment, and output were all primary concerns. Daianu (1994) discusses the disequilibria in the Romanian economy and argues that the proper use of macroeconomic tools would reduce both the external and internal imbalance in Romania's command economy. Consequently, the changes in unemployment, inflation, and output that occurred, led some economists to push for a more gradual approach. Van Frausum and Gehman (1994) and Bruno (1994) note that among the previously centrally planned economies, the decline in output and increase in unemployment were among

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<sup>8</sup>The Soviet leader, Mikhail Gorbachev realized the need for reform, and his policies of *glasnost* and *perestroika* reveals the intention to allow for greater personal, economic, and political freedom, and encouraged the surrounding communist countries to adopt such reforms. The reforms of the 1980s, as presented above, reveal Nicolae Ceausescu's intention to follow the path of openness started by the Russian leader. For a detailed analysis of these Soviet reforms, see Boettke (1993).

<sup>9</sup>Boettke (1994a) argues that democracy is neither a necessary nor a sufficient condition of economic reform. However, democracy in the sense of an electoral process was viewed as the first step to transformation in all of these countries. Similarly, Dorn (1991) warns that democracy could turn Russia into a large welfare state.

the greatest in Romania. Various fiscal and monetary policies were initiated to stabilize the economy. A period of high inflation 1990–1993 occurred during, which Daianu (2001, p. 449) attributes to the destruction of the old institutional framework “without a rapid build up of market-based institutions.” The period 1993–1994 was characterized by an interest rate shock. The banking system attempted to control inflation by trying to raise real interest rates to positive levels. Inflation returned during the 1995–1996 period due to government budget deficits. As of 1999, the Romanian economy still had not achieved macroeconomic stability.

The government’s inability to accomplish macroeconomic stability led the government to turn to the International Monetary Fund (IMF) and the World Bank for financial support of its economic reform program. With the pledge of \$1 billion from the IMF to promote macroeconomic stability in Romania, the Romanian government agreed to the unrealistic standards of the IMF (Gregory 1999). As Gregory argues, even though the Romanian government did not always agree with the IMF’s macroeconomic conditions, they had to follow the IMF’s recommendations, which follow the neoclassical paradigm, in order to obtain the promised financial support. In addition, in 1999 the World Bank approved a \$550 million loan package, which gives them the power to control the decisions of the government (Gregory 1999). This aid created a new level of bureaucracy, which continued to distort the situation. The plans of the IMF and the World Bank failed to improve the transition and were simply a bailout at the national level.

In July 1990, Law No. 15/90 recognized state-owned enterprises as commercial companies. Initial price liberalization occurred in three rounds: November 1990, April 1991, and July 1991 and removed price controls from approximately 80 percent of goods and services. In July 1991, new legislation was adopted on the privatization of commercial companies. The new government envisioned a two-stage process of reform: first a reform of the existing economic entities and second a modernization phase. The government’s report stated, “the period of transition should be reduced to the possible minimum” (Demekas and Kahn 1991, p. 15). Thus, the idea of a rapid transformation appeared to be the favored point of view. Enterprises in strategic sectors, such as mining, defense, or transportation were to become *regies autonomes* (RA), and later transformed in whole or in part into commercial companies allowing them to be privatized at a later date (Earle and Sapatoru 1993).

The National Agency for Privatization (NAP) became the government body in charge of the privatization process (commercialization). In 1991 the Romanian government created five Private Ownership Funds (POF) and one State Ownership Fund (SOF). The POFs were established in August 1992 and functioned as the holders of equity shares equivalent to 30 percent in the newly formed commercial companies. One of the goals of the POFs was to accelerate the commercialization of shares from the SOF by allocating them to the POFs. The SOF was established to transfer into private ownership the



70 percent of shares it had in each commercial company. The SOF was to create a commercialization program to sell 10 percent of the shares each year. Stan (1995) states, in the first five years of Romania's transition that the "mass privatization" process failed to occur, since the NAP and the SOF "privatized" only 8 percent of the enterprises planned. There exists confusion over whether these institutions are controlled by the state or private sector. Earle and Sapatoru (1993, p. 158) emphasize "the lack of understanding of the proper boundary between state and private." Political appointees govern these funds showing the continuity of a command system rather than a system that relies on market-type mechanisms (Earle and Sapatoru 1993).

Following commercialization, the Romanian government implemented restructuring of the state owned enterprises (SOE).<sup>10</sup> Ianos (2001) confirms that the dominant view of economic reform was that industrial restructuring should precede privatization. In 1993, the Agency of Restructuring was created with the objective of reorganizing unprofitable enterprises through technical and financial support. Political pressure caused the program to yield no improvements in the performance of these companies, and only further delayed the privatization process. This program's failure leads one to further question the feasibility of designing successful restructuring programs under the auspices of the government. Ianos (2001, p. 194) states, "the involvement of the state in this process followed a classical path of subsidizing domestic and export items, supporting loss-making industrial activities, and setting prices by overrating the exchange value of the national currency."

In June 1995 the government attempted another fast paced commercialization process, and in September began the Mass Privatization Process (MPP). More legislation followed in December 1995 designed to accelerate the restructuring of RA and SOE. In 1997 the government made still another attempt at a rapid commercialization. The results were, as expected, more declines in output and rising unemployment. The 1995 laws and the creation of the Ministry of Privatization in December 1997 demonstrate the failure to increase the pace of the commercialization process and the government's intention to continue planning.

Bilsen and Konings (1998) assess the restructuring process in terms of job creation, destruction, and firm level growth in Romania. They conclude that employment growth is lowest in the SOE, slightly higher in private enterprises, and is higher still in the *de novo* private firms, firms created after December 1989. The successes of these firms demonstrate the importance of entrepreneurship and establishing property rights. Daianu (1994, 2001), van Frausum and Gehman (1994), and Stan (1995) all acknowledge that institutional fragility and undefined property rights have combined to discourage

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<sup>10</sup>The commercialization and the restructuring processes overlap, since the process initiated by the POFs converting ownership certificated into shares for more than 100 commercial companies was not finalized until April, 1995.

foreign investment in SOEs and thus contributed to the delay of private ownership. It is clear from the information presented that even Romanian neo-classical economists realize that their “plan” has failed to achieve its desired goal. The next section evaluates the logical progression to a market economy using Austrian theory.

#### THE AUSTRIAN APPROACH TO PRIVATIZATION

While neoclassical economists debate the merits of a gradual versus a rapid approach to transition, Austrian economists agree on a quick transformation. In fact, if an Austrian approach is followed, the transformation can only be quick. It should be noted that the use of the term shock therapy is somewhat misleading. The goal is not to shock the economy, but rather move it forward as quickly as possible. A rapid transition will result in shocks to the economy, but these shocks are necessary.<sup>11</sup>

In justifying a rapid transformation to a free market economy it is helpful to examine the Austrian arguments for transforming former socialist states. Boettke (1994b) explains that there is more to the transformation process than speed. The neoclassical arguments for rapid versus gradual transformation are not all right or wrong. He explains that Sachs is correct in stating that incentives matter and providing the correct incentives quickly is important, but Cohen (1993) is also correct in stating that culture and history matter. According to Boettke, the error is in thinking that these arguments are mutually exclusive.

One approach is to treat Romania as a developing economy rather than an emerging market. Bauer (1957) argues that the first function of government in a developing country is the maintenance of law and order. Governments have to safeguard the lives and property of their citizens. This type of government structure did not exist in Romania during the socialist regime. The drastic changes in laws, governmental structure, and development of resources are therefore in many ways closer to a developing country. Bauer warns that in a developing country resistance to economic change will exist and governments need to avoid monopolization of resources and power to implement these

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<sup>11</sup>The shock approach has yet to be attempted in Eastern Europe. The Czech Republic has come the closest to using shock therapy and one can see the influence of the Austrian method in its transition (see Hazelett 1995). Several historical examples of successful rapid transformations exist. One such case is the German miracle from 1948 to 1961, under the influence of Ludwig Erhard, the German economy made a drastic improvement. Erhard started with a currency reform and moved to a market reform; he abolished all price and wage controls and allowed almost complete freedom of movement, trade, and occupation, thus radically expanding the rights of private-property owners (Hoppe 1991). Robert Higgs (1977) notes that in the U.S. the federal government did not institute new policies to aid the newly emancipated slaves; however, property rights were upheld and blacks were able to achieve economic success.

changes. Strong government involvement in these facets of the economy is likely to retard economic growth.<sup>12</sup>

Boettke (1994b) reveals that the neoclassical methodology of development theory explains why Romania and other former socialist states are not treated as developing countries. The formalist and positivist revolution in neoclassical economics undermined the emphasis on institutional infrastructure. The rise of socialism, and the Keynesian revolution in macroeconomics changed government's focus from designing rules to directing the economy. With the development of modern macroeconomics capital theory was removed, and the underpinnings of microeconomics in macroeconomic theory were lost. According to Garrison (2001), this left two points of view in mainstream macroeconomics: the Walrasian world of general equilibrium with no market complications and the Keynesian view that markets cannot generate solutions. Austrian economics relies heavily upon capital theory in its macroeconomic analysis acknowledging that markets experience disruptions, but maintains the microeconomic foundations of market analysis.

The Austrians however place more emphasis on the elements of market process, exploring the ways in which the discoordination of markets occurs and how markets respond. Gallaway and Vedder (2000) claim that at the macroeconomic level discoordination among wages, prices, and the average product of labor is common, and that the macroeconomy is in a continual state of flux. Thus, creating economic policies that generate macrostability in the neoclassical sense is extremely difficult. They propose that the most appropriate short-run macroeconomic stabilization policy is to give entrepreneurs and workers maximum freedom to adjust to potentially disordinating shocks to the macroeconomy.

Rothbard (1992) touches upon an important issue in recognizing the struggles associated with desocialization and the establishment of free markets. He makes several recommendations on how not to desocialize and emphasizes that markets cannot be planned. First, governments should not phase in free markets. Second, underground markets should not be illegal. The third recommendation is do not inflate the money supply. Finally, do not raise taxes. Neoclassical economists have ignored all of these recommendations in the policies advocated in the transition process, and it is clear from the earlier section that the Romanian politicians favored the neoclassical view.

The Austrian perspective on the privatization process opposes the neoclassical view by emphasizing the establishment of property rights for state-owned resources as the primary step. Only after property rights have been realized can successful commercialization and restructuring occur. Boettke (1993) argues that difficulties in economic policies are not unique to Eastern countries. Western democracies have been faced with difficult economic problems as well. Austrian economics has shown that while socialism will always

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<sup>12</sup>One could argue that the political interference noted in the last section could be used as evidence of government hampering economic development.

lead to chaos, capitalism is an economic possibility. Today Eastern and Western economies are quasi-mercantilist, rent-seeking societies. The problems and paradoxes of reform all revolve around the nature and logic of economic institutions and the vested interests of the old regime. The basis of economic reform is private property rights that are protected by a rule of law (Boettke, 1994b).

Rather than command and control, the agenda for studying economic development that I have alluded to appears to suggest that state action be limited to the establishment of rules which cultivate economic experimentation and competition. Property rights need to be clearly defined and strictly enforced. Economic progress is a function of the “rules of the game.” Legal, political and cultural institutions combine to provide the effective rules within any society, and as such determine whether that society will progress or stagnate. (Boettke 1994b, p. 18)

Furthermore, Boettke (1994a) warns that conventional neoclassical models of politics and economics can result in two traps that can stall real reform. Neoclassical theory is preoccupied with (1) “getting the price right” and (2) maintaining economic stability in these transition economies.<sup>13</sup> Boettke (1994b) explains that Mises and Hayek, in their critiques of socialism, outlined why the institutional requirements of private property rights are essential for a free market economy. Property rights create: (1) A legal environment that provides credibility and certainty that encourages investment. (2) The right incentives for responsible decision making on behalf of property owners. (3) An environment of social experimentation that spurs progress. (4) The means for economic calculation, which allows the formulation of price, profit and loss signals, that can reasonably guide resources. Boettke and Leeson (2003) argue that in the former soviet states that if property rights are correctly established entrepreneurship will lead to new industry and competition. Entrepreneurs involved in the market process will be able to evaluate prices, and create a dynamic market economy. Restructuring existing industries will only retard this transformation.

Consequently, in order to establish a market economic system, there are certain steps that have to be taken. Boettke (1993b), Mises (1998), and Rothbard (1956, 1977) argue that market competition requires the separation of economic forces from political forces. Governments cannot understand the data of economics within the context of the chooser. Preferences and value scales cannot exist separate from the action of the individual. Coyne and Leeson (2004) demonstrate how entrepreneurship has been perverted by Romanian officials.

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<sup>13</sup>A third trap could be noted which is to favor the creation of democracy over markets (see footnote 9). Also Zakaria (2003) explains that democracy will not necessarily generate a desirable outcome. We are thankful to an anonymous referee for this point.

Praxeology explains why privatization schemes such as vouchers or auction models (commercialization) cannot value assets without a market, and a reliable market cannot exist without private property. However, if government manages the reform process the outcome will be one of economic distortions and political instability. Boettke (1994a) emphasizes that Austrians understand that economic and political freedoms are intertwined and that institutions are important.

Planned economic reform is not distinctly different from the communist system of monopoly that removed individuals from the decision making process of public life. Zygmunt (1994) explains that privatization can be viewed as a threat when citizens depend on a single enterprise for their livelihoods. Gradualism and government restructuring is favored by many, because citizens think the state can minimize failures. The neoclassical approach delays the privatization process and does not result in the liquidation and reallocation of assets that is necessary to create capitalism.

If establishing property rights is the first logical step what is the best process? Rothbard (1992) recommends returning property to the original owners or their heirs when possible. In lieu of this possibility he argues for a homesteading approach.<sup>14</sup> Consistent with the homestead approach is Zygmunt's (1994) view of liquidation or spontaneous privatization, Boettke (1994), and Boettke and Leeson's (2004) concept of *de facto* property rights. The argument is that in former communist countries, the business executives and factory managers seek to retain control through privatization of what they have already laid claim to as their *de facto* property. These individuals have been responsible for this property and have placed a value upon it establishing implicit property rights that should now be recognized.

Boettke (1994) argues that Hayekian economics and politics provides the most viable answer to the neoclassical traps by offering an alternative perspective that respects cultural traditions and institutions, while providing a methodology for critical analysis. Information under liberalization can freely flow to provide the incentives that neoclassical economists desire so that individuals can best discover and use the information. This information will then develop and emerge within the institutional arrangements of the economy rather than being imposed. The concept of *de facto* property rights, if established with certainty and credibility that these property rights will be protected, will necessarily lead to a commercialization and restructuring process that is based on entrepreneurial expectations. Boettke and Leeson (2003, p. 36) note that as important as the successful privatization of former state owned firms may be, the real test of a successful reform is marked by "the

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<sup>14</sup>Homesteading is the act of mixing one's labor with land and capital that is otherwise unclaimed and thereby creating private property. For a detailed discussion of the homesteading principle see Rothbard's *Man, Economy, and State* (1993) and *The Ethics of Liberty* (1998).

establishment of institutional conditions such that private newly created firms come to dominate the economic scene.”

The idea of commercialization and restructuring being the result of a private property system is lost in the neoclassical theory because of their frictionless models of economics and politics. The frictionless model of competition in neoclassical theory is the outcome and not the process. It is an ending, and not an explanation of how markets function. Traditional neoclassical arguments use the competitive model to avoid the problems of cost and uncertainty. The arguments of Mises (1975, 1998) and Hayek (1945, 1984), which incorporate private property, the rule of law, and uncertainty into their theory are the most appropriate for these former soviet states. Western advisors approach commercialization and restructuring with a careful and detailed plan and envision a process of phasing in reforms. Even if one claims that these reforms are no longer a socialist system, the mere act of government establishing institutions to encourage successful business practices, without first privatizing the property, is at best a major government intervention in the economy.

Austrians focus on liberalism and the power of social cooperation, thus arguing against interventionist attempts by the government to improve the economic order. Mises extended his work on the calculation debate to the theory of interventionism. Ikeda (1994) explains that Mises's and Hayek's arguments establish a theory of interventionism by connecting the impossibility of economic calculation and use of knowledge, to demonstrate how interventionism is inherently related to the centrally planned economy. Rothbard (1993) extended this work explaining that social utility can never be improved for a society when government intervenes.<sup>15</sup>

The Austrian perspective of privatization opposes the neoclassical view by emphasizing the establishment of property rights for state-owned resources as the primary step. Only after property rights have been realized can institutions, entrepreneurship, market prices, and profit and loss signal emerge all of which are necessary for successful commercialization and restructuring to occur. Finally, Austrian economists realize that planned economic reform, even if done in the name of markets, is still economic planning.

#### RESTRUCTURING THE STEEL INDUSTRY

The steel industry provides a case study of the neoclassical view of restructuring. The steel industry in Romania was developed during the industrialization of the 1960s. In 1989, the industry consisted of thirty-three enterprises.<sup>16</sup> The

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<sup>15</sup>For a detailed analysis of the categories of intervention and the theory of social utility see Rothbard *Man, Economy, and State* (1993). Also, see Salerno (1993) for the further development of this theory suggesting that government intervention can reduce social utility.

<sup>16</sup>In addition, four research and development institutes exist in the steel industry.

Romanian steel industry was a large exporter of steel products in the Council of Mutual Economic Assistance (CMEA), always registering positive net exports.<sup>17</sup>

The attempt at commercialization and restructuring has not resulted in the anticipated economic benefit, primarily because the policies outlined for eliminating the SOE failed to occur. Many of the steel companies continue to be state-owned, and as of 2002, all thirty-three enterprises are still in existence.

Since 1989, steel output has continuously declined due to the fall in internal consumption of steel, reaching a low 4.5 million tons per year in 2000. The decline in output has also decreased employment. In 1989, 207,856 people were employed in the steel industry. In 2000, 77,000 people were employed in the industry. According to the restructuring strategy, developed by the Ministry of Industry, the number of steel workers is expected to decrease to approximately 54,000 by 2010. Net exports are another indication of macrostability in the neoclassical model. Exports of steel products have declined from 1,069 thousands tons in 1989 to 505 thousand tons in 1999, while imports have fallen from 844.5 thousand tons to 487 thousand tons during the same period. Net exports remained positive, but are decreasing.<sup>18</sup>

The first stage in Romania's efforts to transfer the state-owned enterprises into "private ownership" was the process of commercialization, initiated in July 1990. Enterprises were to be transformed into either joint-stock or limited liability companies that were to be subsequently privatized. Shares of steel companies were to be divided into the POF and SOF. Over a decade later the SOF has not accomplished its stated goal still holding large percentages of steel enterprises. The SOF eventually changed its name to the Authority for Privatization and Management of State Ownership. The main objectives of the "new" institution are the restructuring of enterprises, providing financial assistance as needed in the revamping, managing the companies' portfolio, and the sale of the state's shares to the private sector. These objectives are "following the governmental strategy," which reveals the continuity of a command system with constant political intervention in the economic realm.

The restructuring strategy of the Romanian steel industry for the years 2002 to 2005 focuses mainly on the industrial segment. The government plan recommends: specialization in each enterprise in order to avoid competition as much as possible at the national level, maintaining the internal market demand, maintaining and developing the export markets, development of intermediate products, and investments in technology. The restructuring program concentrates on increasing the economic efficiency of enterprises by

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<sup>17</sup>The Council of Mutual Economic Assistance (CMEA) was a trading bloc consisting of Communist bloc countries.

<sup>18</sup>Government of Romania (2002).

trying to increase productivity, reduce input consumption, and increase quality to achieve European Union standards. The total explicit cost of the industrial and social restructuring of the steel industry amounts to 590 million dollars. These funds are being supplied from governmental resources, strategic investors, and external sources.<sup>19</sup>

The Romanian government is not alone in favoring restructuring before privatizing. Kotas and Markus (1999) argue that the objective of the restructuring process in Romania and Central Europe is necessary for the long term viability of the steel industry. They define viability as business profitability, the ability to fully meet customer needs, and the ability to compete with international steel manufacturers. They argue that privatizing first involves the risk that the new shareholders will not restructure plants. If the government continues to privatize first and restructure subsequently, there is a high risk of viability never occurring (Kotas and Markus 1999). The Austrian perspective argues if these entities, which are the result of planning, should even continue to exist; it can only be decided under private ownership. Although modernization of some plants has occurred, there is no guarantee of viability, and cannot be as long as the steel industry continues to be operated by the government. The investments in restructuring have been funded by loans from state-owned banks without consideration for the business strategy, which has left steel firms with large debts (p. 7). This outcome should not be surprising as it is understood that economic calculation by the government cannot occur, so no "business strategy" could possibly occur. Kotas and Markus claim that the privatization of Romanian banks would lead to investment decision makers placing a greater emphasis on business viability. While the banking industry being completely private is a worthwhile goal it is not enough to generate true entrepreneurial decision making for the resources of the steel industry.

One problem with obtaining viability in the steel industry is the source of inputs. Kotas and Price (2000) urge the Romanian government to exploit its domestic resources rather than to import materials, even if the costs of production are higher and mining operations are not viable.<sup>20</sup> The steel industry cannot obtain any kind of meaningful viability or success if the government continues to control and intervene directly in these companies, and indirectly through input markets. This results in the steel industry continuing to operate without economic calculation. Specifically, it is the lack of market prices in the input markets, such as mining and financial markets, that prevent economic calculation.

The privatization in the Romanian steel industry has been extremely slow. More than a decade after the fall of the Communist regime, there still exist 14 enterprises that are SOE's. A true market process that signals profits and

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<sup>19</sup>Government of Romania (2002).

<sup>20</sup>Kotas and Markus fail to consider the theory of comparative advantage and the law of association in this analysis.



losses has yet to be realized. Furthermore, many of these companies have been incurring major losses on a yearly basis, and yet none of the 33 companies have been liquidated (Government of Romania, 2002). Kotas and Price (2000) think that the costs to turn these enterprises into profitable businesses may run into billions of dollars per company; however, the likelihood of funding these turnarounds from the state and private sectors is small. Some of these companies have to be liquidated; however, as long as the government owns both the banking system and these companies, the liquidation process is not likely to occur. It is in the interest of politically appointed managers in these companies, to follow the government imposed strategy. This restructuring strategy offers the government complete control over the economic situation of these enterprises. Furthermore, it is important to highlight the impact of unions on the steel industry transition. According to Aurel Radi President of the "METAROM" National Trade Union, unions are independent from the management of the enterprises, as well as the political power.<sup>21</sup> However, due to the legislation passed in 2002, unions are consulted in the commercialization process, and a social contract will be drawn between the union and the buyer; disregarding this contract represents a clause to annul the contract.

Further compounding the difficulties of establishing a private property based market economy, the Authority for Privatization and Management of State Ownership nationalized (i.e., reverted ownership to the state) of two previously commercialized steel companies (Ciobanui, June 2002). This sudden reversal of property rights raises the issue of the government's credibility of protecting private property. The investors' property rights are being taken away at governmental discretion. Private investors run a great deal of risk when investing in the Romanian steel industry. The fragility of private property rights retards the market process, which leaves enterprises in the possession of the government. Wagner (1998) underlines the importance of having a successful steel industry rather than a Romanian owned steel industry. To the extent that markets dictate the profitability of this industry this can only be accomplished by the establishment of true property rights based on sound legal foundations.

## CONCLUSION

With the collapse of the Soviet System in 1989 many envisioned economic and political innovations in the former communist countries. Western economic advisors rushed to aid in the development of these new institutions, but a lack of understanding of economics and history has stalled this process to date.

Neoclassical economists did not understand the significance of economic calculation as developed by the Austrian School. With their concern for establishing equilibrium prices and macrostability they ignored underlying

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<sup>21</sup>Reported to one of the authors in an interview with Aurel Radi in July 2002.

principles: the macro economy is always in a state of flux and prices can only emerge under a private property rights order. The result has been a slow process with a mix of government relinquishing and re-taking control.

The Austrian insights lie in the understanding that private property rights in all aspects of a market are what allow individuals to evaluate resources. Furthermore, the Austrians understand the important role of institutions and that they cannot be planned. Privatization will be a shock, but there is a lot to be corrected from years of misallocation. New institutions must emerge and not be forced.

The steel industry is a microcosm of the attempt at transition by neoclassical economists and Romanian officials. Property rights have not been established in the industry or its resource markets. Despite the attempts to stabilize the economy the industry has experienced all the negative shocks that economists are trying to minimize. The key to the restructuring of the steel industry, in Romania from the Austrian perspective relies upon the important role that the Austrians place on the entrepreneur. The entrepreneur must assess the market and the resources available to him to determine which industries he expects to yield profit. However, entrepreneurship in and of itself is not sufficient. As Coyne and Leeson (2004) note, destructive forms of entrepreneurship may develop depending on the institutional structures in place.

Notions of restructuring prior to privatization are merely delaying the process of sorting out resources and the necessary liquidation and reorganization of assets. If central planning failed then why do we continue to witness politicians and economists continue to advocate centralized decision making in the transition process? Finally, it is worth noting that Lange (1937, p. 134) argues that most socialists preferred a gradual transition to a socialist state, but he claims that a swift change is best any time you have a radical change. "The very existence of a government bent on introducing socialism is a constant threat to this security. Therefore, the capitalist economy cannot function under a socialist government unless the government is socialist in name only." We claim the same argument can be made in reverse, that a radical change to capitalism should be rapid, and that the neoclassical gradualist approach toward capitalism that has been visible in Romania is one in which the government is capitalist in name only.

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# AUSTRIAN BUSINESS CYCLE THEORIES IN THE REVIEWS OF THE FEDERAL RESERVE SYSTEM

GREG KAZA

References to the works of economists and economic schools of thought are a relatively recent development in texts of reviews<sup>1</sup> published by Federal Reserve System member banks. Text references in reviews were rare in the central bank's first half-century (1914-1964) of operation. Explicit references to economists and schools of thought began appearing in the late 1950s and early 1960s. The essays in the late 1960s and early 1970s featured references to Keynes and Milton Friedman (Friedman and Schwartz 1963) on the importance of fiscal versus monetary policy. Outstanding examples were in reviews of New York and St. Louis banks.<sup>2</sup>

## THE LIMITS OF EVOLUTION: THEORIES OF THE BUSINESS CYCLE

The business cycle is a frequently cited topic among references to Austrian economists. There are 11 text references to Austrians in Fed reviews that refer to the business cycle. Austrian business cycle theory (ABCT) identifies the central bank as the cycle's major actor due to its interference with the market and the natural rate of interest. The failure to consider one ABCT variant—the Misesian theory—illustrates the limits of evolution in Fed reviews.

One exception is Formaini (2001, Dallas). The essay does not use the term “malinvestment” but briefly presents a Schumpeterian view of ABCT:

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<sup>1</sup>The 12 Federal Reserve member banks are Atlanta, Boston, Chicago, Cleveland, Dallas, Kansas City, Minneapolis, New York, Philadelphia, Richmond, St. Louis, and San Francisco. For a reference to An Austrian at a Federal Reserve Conference see Paul A. Samuelson, “Summing Up Business Cycles: Opening Address,” at Boston Conference Series No. 42: “Beyond Shocks: What Causes Business Cycles?”

<sup>2</sup>For New York see Davis (1968, 1969) and Corrigan (1970). For St. Louis see Brunner (1968), Andersen and Jordan (1968), Fand (1970), and Andersen and Carlson (1970).

Schumpeter's entrepreneurs are the causes of business cycles because their actions create dislocations that can come in waves. Cyclic downturns are characterized by what Ludwig von Mises (1881–1973) called a “cluster of errors” as most entrepreneurs suddenly guess wrong. Why? Schumpeter suggests three reasons: (1) innovative ways of applying existing inventions and resources immediately trigger emulation by others; (2) the extra demand that financial backing gives to these undertakings is financed by credit-expanding activities that banks can engage in under a fractional reserve system; (3) the new undertakings generate “spillover effects” and trigger similar dislocations in other industries (Schohl 1999).<sup>3</sup>

Schumpeter emerged from the Austrian tradition, and his business cycle theory as well as his ideas about entrepreneurs were influenced by previous work in that tradition.<sup>4</sup>

The essay notes the key role that fractional-reserve systems—central banks and the *credit structure* (Mises 1912)—play in Austrian theory on economic fluctuations and the business cycle. But elsewhere in Fed reviews ABCT is rarely presented and only within narrow parameters. Hayekian and Misesian cyclical theories are virtually ignored. The most common text citation is to the Austrian Haberler<sup>5</sup> (1937) in Dotsey and King (1988, Richmond); Kydland and Prescott (1990, Minneapolis); Diebold and Rudebusch (2001, San Francisco); and Ludvigson, Steindel and Lettau (2002, New York).

Dotsey and King (1988) cite Haberler as a “classic interwar survey of business cycle theory.” They discuss “the role of expectations . . . [which] also constitute an independent source of shocks in “psychological” theories of the business cycle.

Kydland and Prescott (1990) cite Haberler for “an extensive overview” of alternative views of business cycles but also ignore the Austrians. Rather, they present the cycle in light of established neoclassical growth theory. “The study of business cycles,” they write, “flourished from the 1920s through the 1940s” but “ceased to be an active area of economic research” in the 1950s and 1960s.

Now, once again, the study of business cycles, in the form of recurrent fluctuations, is alive. At the leading research centers, economists are again concerned with the question of why, in market economies, aggregate output and employment undergo repeated fluctuations about trend.

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<sup>3</sup>Schohl concludes Schumpeter's (1951, p. 155) claim to examine “how firms rise and decline . . . and how this rise and decline affects the aggregates” is still “a neglected issue on the profession's research agenda.”

<sup>4</sup>Formaini cites other Austrians on entrepreneurship. These are Carl Menger, Friedrich von Wieser, Friedrich A. von Hayek, Murray N. Rothbard, Ludwig Lachman, and Israel Kirzner.

<sup>5</sup>Craver (1986) and Salerno (1999) identify Haberler with the Austrian school. Haberler studied under Wieser and Mises at the Univ. of Vienna, where he earned doctorates in law and economics. For a review of Haberler (1937) see Ellsworth (1940). Footnote references to Haberler in the era include Cantor and Wenninger (1993, New York), Walsh (1986, Philadelphia); and Kumar and Whitt (1992, Atlanta).



Diebold and Rudebusch (2001) cite Haberler on “maladjustments” in their essay on five unanswered questions about the business cycle:

The notion of the increasing fragility of an aging expansion had wide currency among business cycle theorists in the prewar era. Gottfried Haberler's [1937] classic synthesis of prewar business cycle theory devotes an entire section to this topic with the title “Why the Economic System Becomes Less and Less Capable of Withstanding Deflationary Shocks After an Expansion Has Progressed Beyond a Certain Point.” Additionally, there is a section entitled, “Why the Economic System Becomes More and More Responsive to Expansionary Stimuli After the Contraction Has Progressed Beyond a Certain Point.” In both sections, Haberler finds the reasoning, which is based on the inelasticity of the supply of money and of the factors of production, compelling. Indeed, the fact that an economic expansion or contraction gave rise to “maladjustments in the economic system (counterforces) which tend to check and reverse” itself was usually accepted by early writers as “dogma, at least so far as the expansion is concerned.”

They conclude:

As should be evident, although much has been done, many questions about business cycles remain unanswered. For example, a basic question is, What are the causes of business cycles? Can we formulate an explanatory model of economic fluctuations, instead of just a statistical forecasting description of business cycles? In our judgment, there has been very little success in the literature in forging a consensus about the nature of such an explanatory model.

Ludvigson, Steindel, and Lettau (2002) cite Haberler on stabilization in periods of economic fluctuation:

The wealth channel has deep roots in the literature on monetary policy and economic stabilization, reaching back at least to the earliest literature stimulated by Keynes' *General Theory*. Early on, Gottfried Haberler and A.C. Pigou noted that changes in consumer spending generated by countercyclical changes in the real value of the money stock could help provide an automatic stabilizing force to any economy subject to inflationary and deflationary forces.

The parameters of debate in Fed reviews are broad enough to consider Keynesian, monetarist, psychological, and real growth theories of the business cycle. A criticism is that they are narrowly written in avoiding Hayekian and Misesian theories and any references to malinvestment.

One example is Fuhrer (1994, Boston) who cites the “possibility that output fluctuations affect long-run growth,” noting it is “an idea that dates back to Schumpeter (1939).” Contractions might provide opportunities for firms to make structural adjustments that enhance productivity. Yet John Taylor, cited by Fuhrer, finds the “link from fluctuations to growth unpersuasive, since a

good deal of restructuring (through ‘job destruction’) occurs during years when output is at or above potential.”

Hayek is termed a “liquidationist,” not an Austrian, by Wheelock (1992, St. Louis), in his survey of classical and Keynesian interpretations of the Great Depression. He writes,

During the Depression, proponents of the liquidationist view argued against increasing the money supply since doing so might reignite speculation without promoting an increase in real output. Indeed, many argued that the Federal Reserve had interfered with recovery and prolonged the Depression by pursuing a policy of monetary ease. Hayek (1932), for example, wrote:

It is a fact that the present crisis is marked by the first attempt on a large scale to revive the economy . . . by a systematic policy of lowering the interest rate accompanied by all other possible measures for preventing the normal process of liquidation, and that as a result the depression had assumed more devastating forms and lasted longer than ever before.<sup>6</sup> (Wheelock 1992, p. 130)

Wheelock observed “the liquidationist theory of the business cycle was commonly believed in the early 1930s,” yet “died out quickly with the Keynesian revolution, which dominated macroeconomics for the next 30 years.” The Federal Reserve’s “failure to respond vigorously to the Great Depression,” he concludes, “probably cannot be attributed to a single cause. Each of the explanations discussed in this article clarifies certain points about Fed policy during the Depression.”

Exceptions in reviews that cite Austrians are Humphrey (1982 and 1991, Richmond) in which he argues Austrian economists Ludwig von Mises and Friedrich von Hayek relied on “the classical doctrine of forced saving to explain the upswing phase of their monetary overinvestment theory of the cycle.” Humphrey avoids the Misesian *malinvestment*, uses the term “overinvestment,” and credits Henry Thornton (1982) for introduction of this doctrine, which “refers to the potential rise in the rate of capital accumulation and hence long-term economic growth owing to the inflation-induced redistribution from wages to profit.” Thornton, he writes, “anticipated a key feature of those modern neoclassical monetary growth models that treat investment as a function of the monetary growth rate.”

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<sup>6</sup>According to Wheelock, “Several key officials shared Hayek’s views. For example, the minutes of the June 23, 1930, meeting of the Open Market Committee report the views of George Norris, Governor of the Federal Reserve Bank of Philadelphia:

He indicated that in his view the current business and price recession was to be ascribed largely to overproduction and excess productive capacity in a number of lines of business rather than to financial causes, and it was his belief that easier money and a better bond market would not help the situation but on the contrary might lead to further increases in productive capacity and further overproduction.

Another exception is Leeper (1995, Atlanta). Some economists, he writes, believe

erratic monetary policy plays a substantial role in generating business cycle fluctuations. Among writers before World War II, Irving Fisher (1931), R.G. Hawtrey (1934), Friedrich A. Hayek (1934), Ludwig von Mises ([1934] 1980), and Lionel Robbins (1934) were important contributors to this view.

Leeper observes “monetary policy effects are neither well understood nor easily predicted.”

Humphrey (1984, Richmond) rebuts the Austrian School’s contention “that monetarists invariably ignore relative price and real output effects in the monetary mechanism.” He writes “monetarists, like Austrians,” stressed these effects.

[M]onetarist and Austrian theories of the business cycle share many of the same or similar characteristics. Because of this, the two approaches should be seen as complementary rather than as competing. The similarity between the two views also casts doubt on the notion of a unique Austrian view of the monetary mechanism.<sup>7</sup>

One cannot assume the Fed authors who cite Haberler are familiar with Austrian cyclical theories. Humphrey should be credited for presenting an Austrian view although he rejects it. One is forced to conclude that evolution—and intellectual diversity—still has its limits at the Fed. This becomes more evident when comparing Fed reviews with papers written by economists employed by the Bank for International Settlements and International Monetary Fund that examine Austrian cyclical theories.<sup>8</sup>

#### CONCLUSION: THE CONTINUING EVOLUTION OF A HUMAN INSTITUTION

In 1964, as the Federal Reserve System marked its first half-century, the flagship New York bank’s *Monthly Review* published a series of triumphalist essays celebrating a central bank that could do little wrong. One example was the March 1964 essay, “Fiftieth Anniversary of the Federal Reserve System—

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<sup>7</sup>For an Austrian reply see Clark and Keeler (1990).

<sup>8</sup>Oppers (2002) cites Mises: “Austrians find the influence of central banks on monetary aggregates potentially troubling. Attempts of the monetary authority to manipulate the interest rate, they argue, will effect the market for loanable funds, inevitably rendering the plans of consumers and entrepreneurs intertemporally inconsistent” (p. 6). Borio and White (2003) describe “a much older tradition in business cycle theory” and observe, “This tradition takes root in work by Pigou (1929), Fischer (1932) and the Austrian tradition (eg., Mises (1912), Hayek (1933) and Schumpeter (1939) among others” (p. 26).

Immediate Origins of the System,” which recounts the panics of 1873, 1884, 1893, 1901, 1903, and 1907:

In the decades prior to the establishment of the Federal Reserve System, it became increasingly apparent that the country's financial system failed to meet fully the needs of a growing economy. These shortcomings were most dramatically revealed in fairly frequent “money panics.”

Later that year, ex-New York President Allan Sproul (1941–56), dismissed monetarism in the *Monthly Review* (November 1964). Sproul attacked Friedmanite critics “who would substitute an invariable formula for fallible human judgment or weak human resolve” in monetary policy's conduct:

I am willing to wait at least until we have more persuasive arguments that a rigid invariable formula can ride through the continuing changes in the economic environment, without the benefit of human judgment and without causing major errors instead of minor ones.

Others were unwilling to wait and by decade's end a debate had broken out in the central bank's reviews around the conduct of monetary policy. *The Federal Reserve Bank of St. Louis Review* advanced a monetarist critique within the central bank in the late 1960s. St. Louis Fed President Darryl R. Francis spoke for many monetarist sympathizers when he told the Arkansas Bankers Association at Hot Springs in May 1969:

The recent record of national economic stabilization policy has left much to be desired. For almost five years we have had an accelerating inflation which we have not arrested either for lack of will or lack of knowledge as to how to do it. Uncertainty about the role of the Federal budget and about monetary policy has prevailed. Did the inflation come from the Federal spending the budget deficit, monetary expansion or from some combination? Is the cure for the inflation to be found primarily in budget policy or in monetary policy?

To err is human. The monetarist critics were willing to admit the Federal Reserve, a human institution, was capable of error. Defenders of Fed policy such as Sproul acknowledged the Fed was human but drew the wrong conclusion. The Keynesian theory they defended could no longer provide a plausible explanation for events. That theory collapsed in the double-digit inflation and unemployment of the mid-to-late 1970s.

Parameters were widened, debate occurred in Fed reviews, and a new theory emerged. A decade later, another Fed bank president stepped forward to again critique monetary policy. Minneapolis Fed President Mark H. Willes told a meeting in Atlanta, Georgia in December 1979:

Nobody is very happy with the conduct of monetary policy. The economy has performed badly, particularly in terms of inflation and large costs that go with it. And the near-term outlook for the performance of the economy is grim. Many critics accuse the monetary authorities of failing to deal

effectively with the problems we have faced, and virtually every policymaker admits that, in hindsight, we have made some mistakes that have added to our economic woes.

President Willes concluded: “Because we know so little, economists and policymakers should be considerably humbler in their policy prescriptions.”

The rational expectations critics were willing to admit the Fed, a human institution, was capable of error. The *Federal Reserve Bank of Minneapolis Quarterly Review* championed the rational expectations critique within the central bank in the late 1970s (Lucas and Sargent 1979; Sargent 1980). Parameters widened, debate occurred in Fed reviews, and a new theory emerged. The St. Louis and Minneapolis reviews were unique for promoting new theories (monetarism and rational expectations) termed controversial, even radical by internal critics.

It is possible another humble quest could lead Fed economists to examine ABCT impartially. Fed economists cited Hayek when it was in their self-interest to search for an answer to inflation. It is not in their self-interest to question the *idea* of a central bank. And even if they did it is highly unlikely the Fed would ever embrace ABCT as it did Keynesianism, monetarism, and rational expectations. These schools of thought offered the Fed formulas to promote itself as a stabilizing factor in the economy. ABCT, in contrast, implies that the central bank’s very existence is the root cause of the problem of business cycles.

Pride does not lead to humility and a willingness to examine new ideas. Pride leads to triumphalism and sets the stage for human institutions to fail. The idea that Fed monetary policy errors were a contributing factor to the twentieth Century’s Great Contraction—the Great Depression (Friedman and Schwartz 1963)—was once widely rejected within the central bank with the rest of Friedmanite economics. It is quaint today to read Davis (1969, New York):

The view that “only money matters” or, perhaps more accurately, that “mainly money matters” was the province of an obscure sect with headquarters in Chicago. For the most part, economists regarded this group—when they regarded it at all—as a mildly amusing, not quite respectable collection of eccentrics. The number of serious attempts to grapple with the Friedman view on the role of money until recently has been remarkably small.

In this age of Alan Greenspan as Economic Oracle it has been easy for many to forget the Federal Reserve is a human institution capable of error. Throughout their history, Fed reviews have undergone a process of continuing evolution. John Maynard Keynes, Henry George, and Thorstein Veblen were among the scant few economists mentioned in Fed reviews in the central bank’s first half-century. In the modern era it is also possible to read about Friedman, Robert E. Lucas, Jr., Thomas J. Sargent *and* Schumpeter, Hayek, Mises and other Austrians. One no longer has to search only footnotes. Ideas are presented in the texts of Fed reviews. The evolutionary

process can be seen in the emergence of alternative theories in Fed reviews, including monetarism and rational expectations. Yet despite these developments, the process is stunted and incomplete. Which Fed review is willing to consider nonmainstream theories that question the conduct of monetary policy *today*? Austrian business cycle theory (ABCT) identifies the central bank as the major source of economic fluctuations. To examine ABCT in Fed reviews is to once again acknowledge the central bank is a human institution capable of error. To willfully continue to ignore ABCT is to suggest pride, not humility, reigns.

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# GALLAWAY AND VEDDER ON STABILIZATION POLICY

WILLIAM BARNETT II AND WALTER BLOCK

In an article in this journal, “The Fraud of Macroeconomic Stabilization Policy,” Gallaway and Vedder (2000; hence G&V) state that their: “arguments can be summarized with the following propositions (pp. 31-32)<sup>1</sup>:

1. All major macroeconomic paradigms have as their centerpiece the productivity-adjusted real wage rate.
2. The productivity-adjusted real wage rate has the property of being a trendless stationary time series.
3. Variations in the productivity-adjusted real wage are of two broad types: (a) Exogenous shocks and (b) an endogenous re coordinating mechanism.
4. The exogenous shocks are random in character and generate cycles in the productivity-adjusted real wage rate (and unemployment and economic growth) in the fashion suggested by Slutsky (1927 and 1937).
5. The endogenous re coordinating dampens the amplitude of these cycles, reducing the variance in the productivity-adjusted real wage rate in the United States by 42 percent over the period 1959.1 through 1996.2
6. Consequently, short-term economic forecasting is a rather dubious proposition.

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<sup>1</sup>The ideas in G&V (2000) are largely a reprise of those in G&V (1987) and Vedder and Gallaway (Vedder and Gallaway 1997).

7. *Ex post* attempts at implementing stabilization policy are *destabilizing*, not stabilizing.
8. Therefore, the notion of short-run contracyclical macroeconomic policy-making is an exercise in futility."

Certainly, their propositions 1, properly understood, and 6-8 do not come as a surprise to Austrian economists.<sup>2</sup> However, what sets Austrians apart from mainstream economists is methodology and consequent analyses. The first section contains an analysis of their methods, which are found wanting. Although G&V "tip their hats" to Austrian economics, in section two, "Incompatibility with Austrian Economics," we challenge their claim that their analysis is compatible therewith. In the conclusion we make some final statements.

#### METHODOLOGY

##### *Excessive Aggregation*<sup>3</sup>

G&V's models are highly aggregated. They use the money wage ( $W$ ), general price level ( $P$ ), real output of goods and services ( $Q$ ), and [total] employment ( $L$ ) to construct the real wage ( $w_r = W/P$ ) and the average product of labor ( $Q/L$ ), hereinafter  $AP_L$  (i.e.,  $AP_L = Q/L$ ), and, in turn, "the productivity adjusted real wage for labor in the economy" ( $ARW = w_r/AP_L$ ). This latter, the ARW, is the central concept in their paper. Virtually their entire contribution revolves around deviations of the ARW from its "natural" or "normal" or "equilibrium" value.

Their use of artificially constructed, highly aggregated variables such as "the money wage," "the general price level," "the real wage rate," "real output of goods and services," "[total] employment," and "the average product of labor," negates the Austrian project from the start, as it precludes any analysis involving changes in relative prices and wages, their attendant changes in

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<sup>2</sup>This refers to mainstream-type "countercyclical policies." It is not to deny that there are policies not commonly regarded as countercyclical that should be undertaken immediately on their own merits regardless of whether there is a cycle, or that may be useful in mitigating the downturn if there is a cycle, regardless of the stage. Such policies include *inter alia*, elimination of anticompetitive regulations. For more on this, see section II - 8, *infra*.

<sup>3</sup>G&V's note 2 is tacit acknowledgment of the aggregation problem when they note that Austrians are "more oriented toward considerations of the structure of wages rather than their overall level." However, after then maintaining that both the level and structure are important, they then proceed to ignore the structure of wages entirely. Their rationalization for this is a quote from Mises (1966, p. 578). Certainly, Mises himself did not think in terms of aggregates, whether of wages are any other economic phenomena. Rather, the distortion in the structure, not level, of wages and prices was, to Mises (2000, p. 75), "the main source of the social consequences of inflation." Moreover, it is not at all clear that wage rates in the passage G&V single out refer to nominal-wage, and not real-wage, rates.

allocation of resources and resource productivities, and resulting changes in the composition of output. This problem is not unrelated to that of the analysis done with these variables. However, in what follows, the problems involved with aggregation are ignored, *arguendo*, and we follow G&V (2000) in their use of aggregate variables and analysis in order to demonstrate the difficulties with their presentation even when we abstract from this problem.

G&V analyze the ARW in three models: Keynesian, Classical, and Austrian.

### *Models*

#### *Keynesian*

G&V (2000, p. 20) begin with a “pure” Keynesian model in which (1)  $L = f(AD)$ , where  $L$  is employment and  $AD$  is aggregate demand.<sup>4</sup> They define  $AD$  as  $PQ$  and invoke Keynes’s suggestion that the money wage,  $W$ , should be the unit of account, transforming the right-hand-side of (1) into (2)  $AD = PQ/W$ .<sup>5</sup> They then assume a linear form for (1), yielding (3)  $L = b(PQ/W)$ ,<sup>6</sup> which they convert to (4)  $W/P = b(Q/L) = bAP_L$ , with no justification given for the linear form, though there is a rationale available, as will be shown below. Moreover, the same reasoning makes clear that  $0 < b < 1$ , although G&V do not vouchsafe us this information.<sup>7</sup>

Rewriting (3) or (4) yields  $P = W/bAP_L$ . G&V state that: “within the pure Keynesian framework, money wage rates are assumed to be rigid, which, holding the average productivity of labor constant,<sup>8</sup> leaves only the price level as a

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<sup>4</sup>Of course, Keynes’s concept of aggregate demand is quite different from that of standard or modern economics, where demand,  $Q$ , aggregate or otherwise, is in terms of real phenomena. For Keynes, the aggregate demand function,  $D = f(N)$ , is in term of nominals, not reals, with “ $D$  be[ing] the proceeds entrepreneurs expect to receive from the employment of  $N$  men” (Keynes 1936, p. 25).

<sup>5</sup>It should be noted that this converts Keynes’s nominal aggregate demand function, denominated in monetary units, say dollars (\$), into a real aggregate demand function, denominated in labor units, say man-hours (man-hrs). Let output be denominated in units of  $X$ , some composite good;  $P$  be the price level in terms of \$ per unit of  $X$ ,  $\$/X$ ; and,  $W$  be the wage rate in \$ per man-hour,  $\$/man-hr$ . Then for Keynes, aggregate demand is  $PQ$ , and its units are  $(\$/X)(X) = \$$ ; whereas for G&V, aggregate demand is  $PQ/W$ , and its units are  $(\$/X)(X)/(\$/man-hr) = man-hr$ . For these two concepts to be equivalent, there must be a constant relationship between the quantity of employment and the monetary value of output. This necessitates that the real-wage rate be constant, but with  $W$  assumed constant that requires that  $P$  also must be a constant.

<sup>6</sup>“ $b$ ” is implicitly assumed to be constant.

<sup>7</sup>In this equation “ $b$ ” is the fraction of the average product of labor that labor receives as real wages, the rest going for interest, rent, and profits. The absolute maximum  $b$  could take on would be 1, if labor got the entire amount of output; in any case it has to be greater than 0, or else labor gets nothing and starves to death.

<sup>8</sup>However, though a constant  $AP_L$  means that the ratio of  $Q$  to  $L$  is constant, the levels of  $Q$  and  $L$  are indeterminate.

variable that may adjust or be adjusted to alter levels of employment.” However, this is incorrect. There are only four terms in the relevant equation, however written; to wit:  $W$ ,  $P$ ,  $b$ , and  $AP_L$ . Because three of them ( $W$ ,  $AP_L$ , and  $b$ ) are assumed constant, the fourth,  $P$ , necessarily is also. Therefore,  $P$  may *not* “adjust or be adjusted to alter levels of employment,” or anything else for that matter.<sup>9</sup>

Moreover, the key equation, (3)  $L = b (PQ/W)$ , in V&G’s Keynesian framework is but the standard Keynesian/Post-Keynesian markup pricing model (Snowdon et al. 1994, p. 372), where price is taken to be a markup,  $m$  ( $m > 1$ ), over unit labor expense,  $U$ ; i.e.,  $P = mU$ .<sup>10</sup>  $U$ , in turn, is equal to  $W$  divided by the  $AP_L$ ; therefore,  $P = mW/AP_L$ . This equation may be rewritten as  $W/P = (1/m) AP_L$ . This is G&V’s (4) with  $m = 1/b$ ,<sup>11</sup> or as  $L = (1/m) (PQ/W)$  which is G&V’s (3), again, with  $m = 1/b$ .<sup>12</sup>

If prices are, or if they are assumed to be, a constant markup over unit labor expense; i.e., if  $m$  in the equation  $P = m (W/AP_L)$  is constant, then G&V’s result that  $ARW$ , the ratio of the real wage to the  $AP_L$ , is constant, must hold; i.e.,  $ARW = b = 1/m$ . And, a fortiori, if, for whatever reason, at some point in time  $ARW \neq b = 1/m$ , and, as G&V assume,  $m$ ,  $W$ , and  $AP_L$  are constant, then *of course* only  $P$  can adjust to eliminate the disequilibrium. That is, in their Keynesian model  $ARW$  is a constant because the model is in reality a constant-markup-over-unit-labor-cost pricing model in which the markup,  $b$ , is a “parametric value;” i.e., a constant.

Moreover, if what G&V mean is that in a *disequilibrium situation* (i.e.,  $P \neq W/bAP_L$ ),<sup>13</sup> given the assumption that every term on the right-hand-side is constant, so that the entire right-hand-side is constant, only  $P$  can adjust to alter levels of employment, they are certainly correct, mathematically. If  $A \neq B$ , and  $B$  cannot change, then the only way that  $A$  and  $B$  can become equal is for  $A$  to adjust. However, we are entitled to ask, from the perspective of economics, if at some point in time  $A = B$ , and surely it must have in the case in hand, then how does it come about that  $A \neq B$ ? That is, if we start with  $P = W/bAP_L$ , where  $W/bAP_L$  is a constant, we must be told how we arrive at a point where  $P \neq W/bAP_L$ ; i.e., how it came to be that  $P$  became either too low or too high? But this issue is ignored by G&V, and probably with good reason. Perhaps it is because in this model, it is virtually impossible for  $P$  to change

<sup>9</sup>We thank a very thorough and insightful referee for this point.

<sup>10</sup>A markup of 1 sets  $P = U$  with nothing left for the complementary resources or profits, therefore,  $m > 1$ .

<sup>11</sup>Note that, given  $m = 1/b$ , the constraints  $m > 1$  and  $0 < b < 1$  are identical.

<sup>12</sup>This equation may be interpreted as either the demand for labor or an aggregate demand curve.

<sup>13</sup>It should be noted that in a disequilibrium situation, the link between G&V’s real model and Keynes’s nominal model is broken, as the link is dependent on a constant real-wage rate; see footnote 4.

from an initial equilibrium value:  $P$  does not change in response to a change in  $W$ , as the latter is constant; nor does  $P$  change in response to  $b$ , as that also is constant; nor does  $P$  change in response to a change in  $AP_L$ , as that also is constant; nor does  $P$  change in response to a change of equal proportion (necessitated by the assumption that  $AP_L$  is constant) in both  $Q$  and  $L$ . We are left to wonder how given the assumptions of the model it could ever be in disequilibrium. Furthermore, given the assumptions of the model,  $P \neq W/bAP_L$  cannot be some sort of unemployment equilibrium, else (2), (3), and (4) are not equations, and should not have been written with equal signs.

### *Classical*

G&V (2000, pp. 20-21) build their classical model by utilizing a Cobb-Douglas production function, (5)  $Q = A K^a L^{(1-a)}$ , where  $Q$  is as before, and  $K$  and  $L$  are “capital and labor inputs, respectively.”<sup>14</sup> Then they convert from real to monetary values of output: (6)  $PQ = PAK^a L^{(1-a)}$ . Next these authors take the partial derivative<sup>15</sup> with respect to  $L$ , to arrive at the [value of] the marginal product of labor: (7)  $\partial PQ/\partial L = P(1-a)(Q/L)$ . Whereupon because of the classical assumption “that the money wages will be equal to the money version of the marginal product of labor” (the value of the marginal product under competitive conditions)<sup>16</sup> they (G&V, 2000, p. 21) set up (8),  $W = P(1-a)(Q/L) = P(1-a)AP_L$  and (9)  $W/P = (1-a)(AP_L)$ . Finally, they offer us (10),  $w_r/(AP_L) = 1-a$  or  $w_r/AP_L = ARW = 1-a$ .

Because, as with  $b$ , G&V (2000, p. 21) assume  $a$  to be a parametric value, (4) and (9) are equivalent. That is, the Keynesian  $ARW = b$  and the classical  $ARW = 1-a$ , though arrived at differently, are mathematically the same. The difference is that, with  $a$ ,  $b$ , and  $AP_L$  held constant in both models, but  $W$  held constant only in the Keynesian model, in the classical world it is possible for  $W$  and  $P$  to get misaligned, in which case either or both can adjust, whereas in G&V’s Keynesian model  $P$  is necessarily a constant, also, and nothing can get misaligned. However, if somehow,  $P$  did become misaligned, only  $P$  could adjust.

<sup>14</sup>Their production function is written  $aK^aL^{1-a}$ . This obviously is a typo, so we have rewritten it  $AK^aL^{1-a}$ .

<sup>15</sup>Although they say that they differentiate with respect to  $L$ , actually they partially differentiate, treating  $a$ ,  $A$ , and  $K$  as constants. Moreover, they implicitly assume “perfect competition” in the real world, because they treat  $P$  as a constant. Had they taken the partial derivative without assuming perfect competition, the result would have been the marginal revenue product,  $(P+Q\partial P/\partial Q)(1-a)(Q/L)$ ; i.e.,  $MR(1-a)(Q/L)$  instead of their  $P(1-a)(Q/L)$ .

<sup>16</sup>By “competitive conditions” they mean perfect competition as per the mainstream concept thereof. They also imply perfect competition in the labor, as well as the output, market, else they would have set the “money version of the marginal product of labor” equal to  $W+L\partial W/\partial L$ ; i.e., the marginal factor expense.

*Austrian*

As to the Austrian version of the ARW, G&V (2000, p. 22) state that:

By contrast, the Austrian perspective places greater emphasis on the elements of market process, exploring the way in which discoordination of markets occur and how markets respond to discoordination. In general, Austrians are more interested in expression (10) when it reads (11)  $w_r/AP_L$  [= ARW]  $\neq 1-a$ .<sup>17</sup>

Certainly the former statement is correct. However, the latter is misleading, to say the least. On this, see section two on “Incompatibility with Austrian Economics” especially “G&V on the similarities between classical and austrian macroeconomics and equilibrium.”

*Reality v. perfect competition and average v. marginal productivity; Cobb-Douglas (CD) v. constant elasticity of substitution (CES); and dimensional analysis*<sup>18</sup>

*Reality v. perfect competition and average v. marginal productivity*

But certainly this model, though in keeping with Keynesianism, is not consistent with either classical or Austrian thinking. Let us derive (9)–or (10), which, save for a change in symbols, is identical to (9)–in a different, yet more standard, way. Let  $p$  be profits. Then  $p = PQ - WL - rK$ , where  $r$  is the rental price on capital inputs.

First maximize  $p$  with respect to  $L$  assuming, as G&V do, perfect competition in all markets, and then maximize without this special assumption.<sup>19</sup> Maximization of  $p$  requires that we set  $\partial p/\partial L = 0$ , and solve. With  $P$  and  $W$  constant,  $\partial p/\partial L = P\partial Q/\partial L - W = 0$ , or  $w_r/MP_L = 1$ . Therefore, G&V’s ARW =  $w_r/AP_L = (MP_L/AP_L)$ ; i.e., their definition of the ARW *requires* that it be equal to  $MP_L/AP_L$ , which is the output elasticity of labor ( $\epsilon_L$ ).

Next, maximizing  $p$  with respect to  $L$  *without* assuming perfect competition yields:  $\partial p/\partial L = (P + Q\partial P/\partial Q)(\partial Q/\partial L) - (W + L\partial W/\partial L) = 0$ ; or,  $(MR)(MP_L) = MFE_L$ , where  $MR$  is “aggregate marginal revenue,”  $MP_L$  is the “aggregate marginal product of labor,” and  $MFE_L$ , the “aggregate marginal factor expense of labor.” Then,  $(MFE_L/MR)/(MP_L) = 1 = ARP$ . This is “the correct equation” for the (productivity) Adjusted Real Wage. That is, the appropriate ARW is the

<sup>17</sup>In the published version (11) is actually given as  $w_r \neq 1-a$ , not  $w_r/AP_L \neq 1-a$ . This is obviously a typo. Moreover, these authors somehow infer from this that Austrians would be interested in their eq. (11):  $w_r/AP_L = (1-a)$ . One wonders what Mises and Rothbard would have made of this curious claim.

<sup>18</sup>The mathematics supporting this section may be found at: <http://www.mises.org/journals/scholar/Barnettblock.pdf>

<sup>19</sup>Of course, it is both a theoretical truth *and* an empirical fact, that markets, whether individual or aggregated, are not perfectly competitive. Had G&V allowed  $P$  and  $W$  to fluctuate, as they maintain they *do* fluctuate to adjust the labor market to equilibrium, their eq. (10) would have been, instead:  $(MFE_L/MR)/AP_L = 1-a$ .

ratio of marginal factor expense of labor to the marginal revenue of output, relative to the marginal product of labor, not the real wage relative to the average product of labor. And, as is to be expected in economics the relevant relationships involve *marginals*, not *averages*.

To make the correct equation comparable to G&V's equation, we must convert from  $(MFE_L/MR)/MP_L$  to  $w_r/AP_L$ . This results in:  $w_r/AP_L = ((1+1/E)/(1+1/e))(\epsilon_L)$ , where  $e$  is the elasticity of labor supply with respect to the (nominal) wage rate,  $E$  is the elasticity of output demand with respect to the price thereof, and  $\epsilon_L$  is the output elasticity of labor. This is quite a different relationship than the  $ARW = \epsilon_L$  formulated and posited by G&V. Indeed, instead of setting  $ARW = w_r/AP_L$  they should have set  $ARW = w_r/MP_L$ . However, in order to estimate a model based on the  $MP_L$  one needs data thereon, whereas to estimate G&V's model "all" one requires is data on the  $AP_L$ , which, according to V&G (1997, p. 32) are "readily available."

#### CD v. CES

Even if we accept arguendo G&V's (implicit) assumption of perfect competition, their results still depend critically on another assumption: that the economy as a whole can be characterized by a one good-two resources, CD production function.<sup>20</sup>

G&V use a constant-returns-to-scale, CD function,  $Q = AK^aL^{(1-a)}$ . In this case, the  $MP_L = \partial Q/\partial L = (1-a)AK^aL^{-a}$  and the  $AP_L = AK^aL^{-a}$ . Therefore,  $MP_L = (1-a)AP_L$ . Consequently, profit maximization requires  $w_r = (1-a)AP_L$ . G&V then rearrange terms to arrive at  $w_r/AP_L = 1-a$  or  $ARW = 1-a$ . The point is, this constitutes no more than a special case based upon their implicit assumptions of perfect competition and a one good-two resources CD production function. G&V's analysis depends critically on these assumptions. Specifically, they appear to have chosen the CD function because the  $MP_L = AP_L$  multiplied by the exponent of the labor term.<sup>21</sup>

Consider, next, the results had G&V used, with their  $AP_L$  based ARW, a different commonly used function, say the constant-elasticity-of-substitution (CES) function, in its simplest form:  $Q = (cK^{-\alpha} + (1-c)L^{-\alpha})^{-1/\alpha}$ ,  $\alpha > -1$ ,  $0 < c < 1$ . Then,  $MP_L = AP_L/((c/(1-c))(L/K)^{\alpha} + 1) \Rightarrow ARW = (1-c)/(c(L/K)^{\alpha} + (1-c))$ . Compare that with G&V's CD based ARW,  $ARW = 1-a$ . For these two formulations to have the same equilibrium values, the capital to labor ratio  $(K/L)$ <sup>22</sup> must be

<sup>20</sup>It is hard to imagine a model more in conflict with Austrian methodology.

<sup>21</sup>Although G&V chose the exponents of  $K$  and  $L$ ,  $a$  and  $1-a$ , respectively, such that the production function is one of constant-returns-to-scale, this result holds for any exponents of  $K$  and  $L$ , say  $a$  and  $b$ , where  $a > 0$ ,  $b > 0$ .

<sup>22</sup>Of course, such a ratio is inherently confused, and incapable of being measured, much less be expected to be constant. This is but one manifestation of the aggregation problem; i.e., in this case, the impossibility of aggregating heterogeneous capital and labor inputs. On the other hand, if one tries to measure them in monetary terms, then the problem manifests itself in that the monetary value of the physical inputs can alter without any change in the inputs; i.e., the measured value of the inputs diverges without any transformation in the outputs.

a constant such that:  $K/L = ((c - ac)/(a - ac))^{1/\alpha}$ . Therefore, even if at some point the equilibrium values of the CD and CES ARWs were identical; i.e.,  $(1-a) = (1-c)/(c(L/K)^\alpha + (1-c))$ , a change in  $K/L$  would cause them to diverge. Whether, then, they deviate, and if so, to what extent, is an empirical matter depending upon the specific values of  $a$ ,  $c$ ,  $\alpha$ , and  $K/L$ , which given the limits on the parameters ( $0 < a, c < 1$ ;  $\alpha > 1$ ) are potentially large, but see footnote 22. Therefore, there is no way of knowing this, save on the basis of consistent, empirical estimates.<sup>23</sup>

Moreover, the correct, general formulation of the ARW<sup>24</sup> is equal to the afore-shown ratio involving the price elasticity of demand for output, the wage elasticity of supply for labor, and the output elasticity of labor; i.e.,  $((1+1/E)/(1+1/e))(\epsilon_L)$ . There is no reason to expect any of these elasticities to be constant, or a fortiori, for the ratio involving them to be constant. Therefore, we conclude that the appropriate real wage-productivity relationship is the ratio of the real wage to the marginal product of labor; to wit:  $w_r/MP_L = (1+1/E)/(1+1/e)$ , and that this ratio would be constant for any reasonably long time period only by coincidence.

### *Dimensions*

Regardless of the production function used, the mathematics is untenable as soon as dimensions are introduced. For example, using G&V's CD function,  $Q = AK^aL^{1-a}$ , if we measure  $Q$  in units of widgets per year (wid/yr),  $K$  in units machine-hours per year (cap-hr/yr), and  $L$  in units of man-hours per year (man-hr/yr) then attaching the units (in brackets) to the variables, yields:  $Q[\text{wid/yr}] = A(K[\text{cap-hr/yr}])^a (L[\text{man-hr/yr}])^{1-a}$ . Note that the exponent  $a$  attaches not just to the magnitude of  $K$ , but also to its units. But as  $1 > a > 0$ , say  $a = 0.5$ ,<sup>25</sup> the units of  $K$  are the square root of the ratio of machine-hours to time. Similarly, for  $L$  the units are the square root of the ratio of man-hours

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<sup>23</sup>On the issue of consistent estimates, and also the magnitudes involved, see Huang and Whalley (2003, p. 3):

This study includes a section on skilled unskilled labour substitution elasticities. While Abrego and Whalley use an elasticity estimate of 1.25, behind this is a key table in Hamermesh reporting 19 estimates. 5 of these are of the wrong sign, 1 is below 0.3 and 2 are above 5. While 1.25 may be a defensible central tendency value for their purposes, how best to use this additional information in their model procedures is not clear, as also is whether one can make useful statements as to the likelihood of certain model outcomes in light of this range of estimates. (Emphasis added)

See also Table 1 (Huang and Whalley 2003, pp. 13-14) and accompanying text.

<sup>24</sup>This assumes the ARW is defined as  $w_r/APL$ , as per G&V.

<sup>25</sup>In a more general CD function, one without constant returns to scale; i.e., one in which the exponents of  $K$  and  $L$  do not sum to one (1), the units become even more problematic.



to time. And, the units of  $A$  are the ratio of widgets to the product of the square roots of machine hours and man hours. Surely, all this is intellectually unacceptable.<sup>26</sup>

It availeth naught to reply that production functions are merely symbolic.<sup>27</sup> That is: If 5 men using 5 hammers can produce 45 widgets, writing “45 widgets = 5 men + 5 hammers” is just symbolic and one can’t take the incommensurability of the three units seriously. The reason it naught availeth is that soi-disant (mathematical) economists do not use equations in the manner suggested above; i.e., as a form of shorthand. No, they put such ideas in forms they can and do use mathematically.

There would be no problem if economists used mathematical notation solely in symbolic fashion, save for potential confusion by numerate people who had not been initiated into economists’ ways of using mathematical forms solely in a symbolic way and who came across some economics involving the use of mathematics as shorthand. That, however, is not the case. Rather, such formulations are subjected to various mathematical operations. One very relevant example is G&V (2000, p. 21): “Differentiating (6) [ $PQ = PAK^aL^{(1-a)}$ ] with respect to  $L$  to produce the marginal product of labor produces (7)  $dPQ/dL = P(1 - a)(Q/L)$ .” And, truthfully, were that the worst offense, one might be tempted to look the other way. However, it is not, so one should not.

In sum, G&V’s attempt to establish their regression equations based on a constant relationship between the real wage and the  $AP_L$ , in turn based upon the concept of perfect competition in all markets and a CD aggregate production function, is unjustified.

#### *Disequilibrium Real-wage Rates and Implications of Perfectly Inelastic Labor Supply*

This issue arises because of the following statement by G&V (2000, p. 24):<sup>28</sup>

Given the . . . statistical relationship between variations in the ARW term and employment, unemployment, and . . . rates of growth in . . . output, the variations of the ARW . . . are quite meaningful. Higher levels of ARW are associated with higher unemployment and lower rates of economic growth. (Vedder and Gallaway 1997, app. B)

V&G (1997, p. 31) state: “neoclassical and Austrian economists essentially believed that unemployment varies directly with” the ARW.<sup>29</sup> V&G (1997, pp. 31–32, 313) assert an inverse relationship between the ARW and unemployment. The model they develop is:

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<sup>26</sup>On this, see Barnett (2004).

<sup>27</sup>Again, we thank a very thoughtful referee for raising this issue.

<sup>28</sup>As G&V (2000) explicitly refer to, and rely on, Vedder and Gallaway (1997) some of our analysis concerns the latter.

<sup>29</sup>G&V (2000, p. 22) similarly links the “classical perception of the macroeconomy and the Austrian view.”

(1)  $D_L = f(w_r)$ ; (2)  $S_L = S_0$ ;<sup>30</sup> and, (3)  $S_0 = D_L = f(w_r)$ .

Equation (3) determines the equilibrium real-wage rate,  $w_r^*$  and level of employment,  $N^*$ , at which there is an equilibrium level of unemployment,  $U^*$ .  $w_r$  is the actual real-wage rate and  $U$  is the actual level of unemployment. If  $w_r \neq w_r^*$ , then  $U \neq U^*$ ;  $U \geq U^*$ . "A rise in prices lowers real-wage rates, *enabling* employers to hire workers whose marginal product previously was lower than the real wage they would have commanded. Thus unemployment falls below  $U^*$  and  $(U - U^*)$  becomes negative" (V&G, 1997, p. 314; emphasis added).<sup>31</sup> Furthermore, "[k]eeping in mind our assumption of a fixed supply of labor, we may write (13) " $U - U^* = f(w_r^*) - f(w_r) = f(w_r^* - w_r)$  where (14)  $d(U - U^*)/d(w_r^* - w_r) < 0$ " (V&G, 1997, p. 315).<sup>32</sup>

<sup>30</sup>We note that Vedder and Gallaway (1997, p. 313) assume that: "the supply of labor ( $S_L$ ) [is] a fixed proportion of the population; i.e., perfectly inelastic. . . . For purposes of simplifying the analysis, we will assume an invariant population over time." The former assumption may be unobjectionable, but surely, in a work that uses 37½ years of quarterly data, a period in which the population increased ≈85 million or ≈50 percent from less than 180 million to more than 265 million, the latter is unacceptable. Vedder and Gallaway's perfectly-inelastic labor supply puts us in a Henry Georgian situation re labor, not raw land, thus we offer "a modest proposal:" eliminate the income tax, et al., in favor of a single tax on labor.

<sup>31</sup>One can model a labor market such that in disequilibrium the difference between the actual and natural rates of unemployment is negative; i.e., the actual rate is less than the equilibrium rate. For example, by making the demand for labor dependent upon the actual real wage and the supply of labor dependent upon the expected real wage; however, such a model requires, at a minimum, in addition to variables for the actual, nominal wage and actual price level, an expected, nominal-wage variable. Vedder and Gallaway have no such variable—in fact, expectations do not appear in their model at all. A model can be constructed without an explicit labor market, in which  $U$  can be less than  $U^*$ , but this entails including a variable for expectations re either price level or inflation. Again, Vedder and Gallaway have no such variables.

Vedder and Gallaway (1997, p. 314) state: "A rise in prices lowers real wage rates, enabling employers to hire workers whose marginal product previously was lower than the real wage they would have commanded. Thus unemployment falls below  $U^*$ , and  $(U - U^*)$  becomes negative." However, although employers may wish to hire more workers (an increase in the quantity demanded), G&V have provided no reason why more workers would be willing to work (an increase in the quantity supplied) in the face of a decrease in the real-wage rate. In a standard labor market, as in any standard supply and demand model, the quantity exchanged is at most the equilibrium quantity. At any real wage greater than or less than the equilibrium real wage, the quantity exchanged is less than would be exchanged at equilibrium, being equal to the either the quantity demanded or the quantity supplied, respectively. The quantity is determined by the lesser of the quantity demanded or the quantity supplied. Therefore, G&V's model, contrary to their statements about it, does not allow for a decrease in the actual unemployment rate below the natural rate.

<sup>32</sup>This is an odd formulation as, other than the function  $f(x) = x$ , we are unaware of any function such that  $f(a) - f(b) = f(a - b)$ . However, this is obviously not a relevant function for the matter at hand. Again, had dimensions/units been clearly specified (Barnett 2004) it is likely this problem would have been avoided.

If, as V&G assume, the supply of labor is perfectly inelastic, for real-wage rates at or above the equilibrium level, no change in their analysis is necessary. However, the decrease in the volume of employment and attendant increase in the amount of unemployment would be less, *ceteris paribus*, with a perfectly inelastic supply of labor than in the normal case of an upward-sloping supply curve of labor.<sup>33</sup> However, when the real-wage rate is below the equilibrium rate, employers may *desire* to employ more labor, but most emphatically they are not *able* to do so, or at least not in a society of free men. Therefore, there would be no change whatsoever in the levels of employment or unemployment when compared with their respective rates at the equilibrium real-wage. And, of course, this means that eq. (14),  $d(U - U^*)/d(w_r^* - w_r) < 0$  is incorrect, or, at least, incomplete, in that it only applies if  $w_r \geq w_r^*$ .

#### *The Lack of Clear Documentation of Data Sources*

Because G&V (2000) did not directly provide the source(s) of their data, we assume that it was the same as in their book (V&G 1997). However, in trying to figure out if the money-wage rate included in the ARW took into account nonwage compensation, which, of course, it should, we were led to Table B.1, in V&G (1997, pp. 324-27). Although they provide the data used, they do not offer adequate citations to the sources thereof, or at least not sufficiently for a nonspecialist. The sources are cited as: "U.S. Department of Labor, U.S. Department of Commerce, and authors' calculations" (V&G 1997, p. 327). Because the only wage-type variable in the table is "Compensation per Hour," and not "Wages per Hour," we assume that the compensation variable includes more than wages and, therefore, that the money-wage rates they used to calculate ARW included nonwage compensation as well.

#### *Statistics, and "Normal" or "Natural" Values; and, Averages and Human Action*

Finally,  $\% \Delta ARW_t = f(ARW_{t-1} - \overline{ARW})$ ,<sup>34</sup> [eq. (12)], is critical. It says that the percentage change in the ARW from period t-1 to period t is a function of the divergence between the ARW in period t-1 and natural or trend value of the ARW. G&V estimate a linear form of (12), (12')  $\% \Delta ARW_t = \alpha + \beta (ARW_{t-1} - \overline{ARW}) + \varepsilon_t$ .<sup>35</sup> Although they never tell us how to calculate  $\overline{ARW}$ , it must be done in order to estimate (12'). Assuming that a bar over a variable has the standard meaning, the mean value of the variable, this presents a problem from the point of view of Austrian economics. This school of thought is based upon

<sup>33</sup>In a normal market with an upward-sloping, labor-supply curve, regardless of whether the real-wage rate is above or below the equilibrium rate, actual employment will *always and necessarily* be less than equilibrium employment.

<sup>34</sup>In their eq. (12) G&V write  $ARW_{t-1} - ARW$ . We assume that the absence of a bar over ARW is a typo.

<sup>35</sup>Eq. (12) is not in their paper, but it is implicit, as it is the equation that must have been estimated to arrive at (13).

praxeology—the study of human action. To make sense, we must understand (12) to mean that a divergence between the ARW in period  $t-1$  and the equilibrium or natural value thereof means that the labor market is in a state of discoordination. That is, in retrospect, some individuals consider their behavior to have been suboptimal and make adjustments thereof. Specifically, because the ARW in  $t-1$  was too high or too low, interpreted to mean above or below, respectively, the natural ARW, the economic actors made suboptimal decisions. Assume, as G&V do, that  $AP_L$  is constant; this means that entrepreneurs have to adjust  $W_r$ , either by adjusting either  $W$  or  $P$ , or some combination of both. Therefore peoples' behavior is affected by the value of  $\overline{ARW}$ . However, this is antipraxeological. Consider, their model uses quarterly data from 1959.1 through 1996.2, some 150 quarters.<sup>36</sup> Let us number them from 1 (1959.1) to 150 (1996.2). Consider now the implications for human action of eq. (12), picking a data point at random, for example, 100. Then, eq. (12) reads:

$\% \Delta ARW_{100} = \alpha + \beta (ARW_{99} - \overline{ARW})$ . However  $\overline{ARW} = (\sum ARW_t)/150$ ,  $t = 1-150$ . Therefore, people are basing their decisions in period 100 on an average value that depends on information ( $ARW_t$ ,  $t = 100-150$ ) that that is not yet available! And this is true for every period from 1 through 150, as even when people are acting during period 150, they are only creating data for that period. It will not be fully available until period 151. It is simply impossible to reconcile this procedure with Austrian or indeed with any rational economics.

The argument might be made that we have misinterpreted  $\overline{ARW}$ , that in fact  $\overline{ARW} \neq (\sum ARW_t)/150$ ,  $t = 1-150$ , but, rather, that in considering deviations from the average, we use not the average for the entire period, but the average value of the ARW from the beginning up to and including the period for which we are calculating the deviation, i.e.,  $\overline{ARW} = (\sum ARW_t)/T$ ,  $t = 1 \dots T$ , where  $T$  is the period whose deviation is being calculated, or, perhaps, the immediately prior period. If so, then a different, but no less fatal, problem arises. In that case the calculated value of the independent variable ( $ARW_{t-1} - \overline{ARW}$ ) depends critically upon the length of the time series used, a theoretically arbitrary length, though, perhaps not arbitrary from the point of view of data availability. Let us use our previous example, but assume that they had used only 100 data points, from 1 (1971.3) to 100 (1996.2). Using the same randomly chosen data point, formerly #100, now # 50; (1983.4 in either case) the calculated value of the independent variable then becomes  $ARW_{1983.4} - \overline{ARW}_{1971.3-1983.3}$  in the case of 100 data points and  $ARW_{1983.4} - \overline{ARW}_{1959.1-1983.3}$  in the case of 150 data points. Moreover, not only will the value of the independent variable vary with the number of data points, but so, in consequence, will that of the dependent variable. That is, the strength of their “simple

<sup>36</sup>Amazingly, they assume the stock of capital goods is constant (see footnote 15, above) and then use data for 37½ years.

endogenous reCOORDINATING mechanism for the macroeconomy (12)  $\% \Delta ARW_t = f(ARW_{t-1} - \overline{ARW})$ ,” where  $\overline{ARW}$  is the “‘normal’ or ‘natural’ or ‘equilibrium’ value for ARW,” depends on the period for which  $\overline{ARW}$  is calculated.

(G&V, 2003, p. 24) imply this choice is unimportant because:

In the data set under consideration, the initial value of ARW is 99.92 (1992 = 100) and the terminal value is 100.63. In between, the maximum value is 104.14 (in 1980.2) and the minimum is 96.24 (in 1965.4). This indicates a time series that is essentially trendless with a variation of approximately four percent, plus or minus, about its mean value (= 99.72). This is quite consistent with the notion of an underlying normal or equilibrium value. A unit root test of this data series reveals that it is a stationary series.

This is problematical for two reasons. First, the price, wage and productivity data used to calculate ARW and, a fortiori,  $\overline{ARW}$ , is suspect. Second, according to their model, if  $ARW \neq \overline{ARW}$  then it will revert, and the greater the deviation the stronger the response. However, the only factor that affects the rate of reversion is the deviation of the actual ARW from the mean ARW, and time. This (implicitly) assumes there are no sectoral differences in adjustment speeds or, if such exist, that sectoral weights do not change over time. Such assumptions are highly unrealistic, from the Austrian or any other perspective. Finally,  $\overline{ARW}$ , is an *average of averages*. Such aggregated measures and most especially any supposed relationship between their values as cause, and human action as effects, is certainly incongruous with Austrian economics.

#### *Exogenous Shocks: Inherent in the Data or Artifacts of the Calculations*

In a section on “The Sources of Discoordination,” G&V suggest three possibilities: (1) an endogenous “mechanism”;<sup>37</sup> (2) systematic exogenous shocks; and, (3) random exogenous shocks. They then “attempt to identify the nature of the changes in the ARW variable.”

[G&V] begin by defining a simple endogenous reCOORDINATING mechanism for the macroeconomy: (12)  $\% \Delta ARW_t = f(ARW_{t-1} - \overline{ARW})$ , which is to argue that this period’s percentage change in ARW is systematically related to last period’s deviation from the “normal,” or “natural,” or “equilibrium” value for ARW, denoted by  $\overline{ARW}$ .

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<sup>37</sup>Because Austrian economists understand economics to be about purposeful human action, the use of terms such as “mechanism” are best avoided, as their use tends to divert our attention from the actions of individuals to a search for some type of mechanical process, particularly one that can be expressed in a mathematical model, which can then serve as the basis for statistical estimation of the relevant parameters, themselves assumed to be constants, of which there are none in the real world of human action. No doubt “process” would have been a better choice for a paper that purports to have an Austrian flavor.

They then convert it to a linear form and estimate the parameters to arrive at: (13)  $\% \Delta \hat{ARW}_t = -0.007 - 0.135(\hat{ARW}_{t-1} - \overline{ARW})$ .<sup>38</sup> They provide regression statistics, including  $R^2 = 0.067$  and a t-value of 3.24 for the independent variable. They then state:

Clearly, there is a statistically significant endogenous adjustment mechanism. However, it accounts for only a small proportion of the variation in percentage changes in the ARW measure and the value of the regression parameter that describes it seems, at first glance, to be small. More will be said about this later.

They then go on to use this equation in their derivation of (16)  $\% \Delta \hat{ARW}_x = \% \Delta \hat{ARW}_a + 0.135(\hat{ARW}_{t-1} - \overline{ARW})$ . (16) is the basis for an extensive discussion which leads to their (G&V 2000, p. 31) proposition: “(4) The exogenous shocks are random in character and generate cycles in the productivity-adjusted real wage rate (and unemployment and economic growth) in the fashion suggested by Slutsky (1927 and 1937).”

Unfortunately, their discussion and proposition are based on a derivation that is unsatisfactory. Specifically, (G&V 2000, p. 24) assume: (1) an endogenous reCOORDINATING mechanism and exogenous shocks; (2) “that the [endogenous reCOORDINATING] mechanism described in (13) represents the sole source of endogenous variation in movements in ARW”; and, (3) that because “the constant term in 13 is not significantly different from zero,” it may be disregarded.

The concept of exogenous shocks is reasonable in certain contexts. However, when dealing with the entire economy it makes much less sense. One is entitled to ask what sort of shock(s) might be exogenous to the whole economy? Moreover, G&V (2000, pp. 21-22) assume that when the entire economy is disCOORDINATED, manifested by variations in ARW, the essence of the full adjustment mechanism for the whole economy is captured in one eq. (13), which mechanism, by their own admission is “simple,”<sup>39</sup> taking the form of a predetermined, invariant response “that systematically relates this period’s percentage change in ARW to last period’s deviation from the ‘normal,’ or ‘natural,’ or ‘equilibrium’ value for ARW” (G&V 2000, p. 24).<sup>40</sup>

Consider now their derivation, annotated, and the consequences thereof. The key is to see that they have used three different symbols for the same variable. No doubt this was inadvertent, nevertheless it is critical to understanding the results of their statistical analysis. For G&V,  $\% \Delta \hat{ARW}_n$ ,  $\% \Delta \hat{ARW}_t$ , and  $\% \Delta \hat{ARW}_a$  are one and the same. It is obvious that the former two are the same when we note that their eq. (15) arises from substituting the right-hand-side

<sup>38</sup>That there is no standard symbol over  $\% \hat{ARW}_t$  to indicate that it is an estimated value is surely an oversight, therefore we have inserted a caret or “hat” (^) where appropriate.

<sup>39</sup>Perhaps they meant simplistic?

<sup>40</sup>The specific mechanism is very unAustrian because of its deterministic nature.

of eq. (13),  $-0.135(\text{ARW}_{t-1} - \overline{\text{ARW}})$  (their estimate of  $\% \Delta \text{ARW}_t$ ) for  $\% \Delta \text{ARW}_n$  in eq. (14). Note that the right hand side of eq. (13) is actually  $-0.007 - 0.135(\text{ARW}_{t-1} - \overline{\text{ARW}})$ , but they drop the constant term  $-0.007$  because it is not “significantly” different from zero.<sup>41</sup>

That  $\% \Delta \text{ARW}_a = \% \Delta \text{ARW}_t$  may be understood through examination of eq. (16), itself a rearrangement of the terms in eq. (15). The values of  $\% \Delta \text{ARW}_x$  are calculated.<sup>42</sup> Obviously eq. (16),  $\% \Delta \text{ARW}_x = \% \Delta \text{ARW}_a + 0.135(\text{ARW}_{t-1} - \overline{\text{ARW}})$ , was used for this purpose. As we know the data used for  $(\text{ARW}_{t-1} - \overline{\text{ARW}})$ , the only question is that of the data used for  $\% \Delta \text{ARW}_a$ . The answer is obvious, the only data they had was that for  $\% \Delta \text{ARW}_t$ , the *actual* data for each time period,  $t$ , which must be what they used; i.e.,  $\% \Delta \text{ARW}_a = \% \Delta \text{ARW}_t$ . The actual value of  $\text{ARW}_t$  is equal to its estimated value plus the error term; i.e.,  $\% \Delta \text{ARW}_t = \% \Delta \text{ARW} + e_t$ . Therefore,  $\% \Delta \text{ARW}_a = -0.007 - 0.135(\text{ARW}_{t-1} - \overline{\text{ARW}}) + e_t$ . If we then substitute  $-0.007 - 0.135(\text{ARW}_{t-1} - \overline{\text{ARW}}) + e_t$  for  $\% \Delta \text{ARW}_a$  in eq. 16, that equation reads:  $\% \Delta \text{ARW}_x = -0.007 - 0.135(\text{ARW}_{t-1} - \overline{\text{ARW}}) + e_t + 0.135(\text{ARW}_{t-1} - \overline{\text{ARW}})$ , or,  $\% \Delta \text{ARW}_x = e_t$ .

That is, in *computing* the value of  $\% \Delta \text{ARW}_x$  as the difference between the actual and estimated values of  $\% \Delta \text{ARW}_t$ , they calculated the error term and labeled it the percentage change in the exogenous element. They plot this data series in their figure 1, and state: “The visual pattern is suggestive of a ‘white noise’ series with a zero mean. This is confirmed by a correlogram analysis” (G&V 2000, p. 25). Of course, it looks like “white noise.” They plotted the data for a variable assumed to be random, with zero mean and constant variance;<sup>43</sup> i.e., the data for the error term from an OLS regression. Is it any wonder, therefore, that the “graphic representation of this data series . . . is suggestive of ‘white noise’?” What would have been noteworthy would have been if the plot did *not* look like white noise. Certainly, then, their calculations re the exogenous part of variations in the ARW cannot support G&V’s (2000, p. 31) conclusions.

Because the analysis in the section “The Sources of Discoordination” subsequent to eq. (16), as well as in the subsequent section, “The Impact on

<sup>41</sup>The use of “insignificant” in this context is infelicitous. We say that because the context is statistical analysis, and in that context significance refers to the probability that the estimated parameter is different from the hypothesized value. Because the t-statistic they provide for the constant term is 0.11, they may mean that the estimated value of the constant terms is not statistically significant from the hypothesized value, (implicitly) zero. Or, they may mean that the difference between the estimated value and zero is a very small number; to wit 0.007.

<sup>42</sup>“Using the calculated values of the exogenous component in the adjusted real wage” (G&V 2000, p. 25).

<sup>43</sup>As G&V did not mention the estimation technique used, we assume they employed the standard regression method, OLS, and that the data are such that the assumptions of that statistical model regarding the error term hold; i.e., that the error term is a randomly distributed variable of zero mean and constant variance.

Contra-Cyclical Macro-economic Policy,” is dependent on the calculations of  $\% \Delta \text{ARW}_x$ , there is no reason to analyze it.

#### INCOMPATIBILITY WITH AUSTRIAN ECONOMICS

G&V identify their own work with that of Austrianism, despite the fact that it is patently no such thing. Let us consider a few such instances,<sup>44</sup> and demonstrate that they are mistaken not only in this regard, but also substantively erroneous in some of these cases.

##### *Relative-wage Rates and Unemployment*

According to their model, provided the *average* real-wage rate is “correct,” no misalignment of relative money-wage rates or of relative real-wage rates can cause unemployment. Certainly, this is not compatible with Austrian economics.

##### *“Money Wage Rates Don’t Matter”*

G&V (2000, p. 19) maintain that “Keynes set the spiritual tone for the next half-century with his remark, ‘It can only be a foolish person who would prefer a flexible wage policy to a flexible money policy,’ [which] translate[d] into the notion that ‘money wages don’t matter.’” They then purport to “Contrast these views with those of Ludwig von Mises (1998, pp. 577-78),” obviously intended as a representative of the Austrian viewpoint:

[it is] vain to justify a new credit expansion by referring to unused capacity, unsold . . . stocks and unemployed workers. . . . The belief of the advocates of credit expansion and inflation that abstention from further . . . expansion and inflation . . . would perpetuate the depression is utterly false. The remedies these authors suggest . . . would merely upset the process of recovery. (footnote omitted)

But this quote of Mises’s they cite has little or nothing to do with wages, whether they “matter” or not. This is more than passing curious, in that they *could* have cited Mises (1998, p. 577) to the following effect just a few pages away: “If commodities cannot be sold and workers cannot find jobs, the reason can only be that the prices and wages asked are too high.”

##### *Dissimilarities with Mises*

G&V (2000, p. 20) offer the following as the main thesis of their article: “This fundamental disagreement between what would become the mainstream view with respect to macroeconomic policy and Mises is the focus of this article.” An examination of G&V (2000), compared to any of the writings of

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<sup>44</sup>Apart from these gratuitous citations of Austrians, there is nothing in G&V (2000) that would be at all remarkable were it to appear in the *American Economic Review* or any other such mainstream scholarly periodical, or, for that matter, the *Journal of Post Keynesian Economics*.



Mises will demonstrate the falsity of this claim: The former relies heavily on mathematical argumentation<sup>45</sup> and equations,<sup>46</sup> which, for the Austrian, could only be relevant for economic history, not theory, as it is for G&V. A more thorough reading of G&V, as for example that offered in Part I of this present paper, will show in greater depth the sheer enormity of the intellectual chasm between these two sets of authors, G&V on the one hand, and Austrians in general on the other.

*G&V on the Similarities Between Classical and Austrian Macroeconomics and Equilibrium*

In the view of G&V (2000, p. 22, material in brackets inserted): “Both [classical and Austrian views on macroeconomics] are rooted in the marginalist tradition and both entertain the possible existence of a unique equilibrium position given appropriate *ceteris paribus* conditions.”

However, this is somewhat misleading. First, terminology. The “classical period,” insofar as microeconomics is concerned, is widely believed to end with the marginal and subjectivist revolutions usually dated 1870 or there about; however, with respect to macroeconomics, the classical period is thought, at least by Keynes, to end with the Keynesian revolution usually dated around 1936. Second, “macroeconomics” refers to economic analysis that involves in a fundamental way relationships among aggregated variables. Certainly, before 1890, there really was no “classical perception of the macroeconomy” in any meaningful sense.<sup>47</sup> Therefore, the period of “classical macroeconomics” was roughly four and one-half (4½) decades in length, from roughly 1890–1936. (This is not to deny the development of a variety of ideas as to how the overall economy functioned during that period and prior thereto.) Thus, it is questionable whether the term “macroeconomics” is appropriate pre-Keynes.<sup>48</sup>

Second the neoclassical microeconomics which dominated that period was thoroughly grounded in marginalism. So also was Austrian theory, including the Austrian theory of the cycle, that can be said to date from Mises’s *Theorie des Geldes und der Umlaufsmittel* in 1912 (translation: *The Theory of Money and Credit* (Mises 1981). However, it is at best questionable to what

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<sup>45</sup>And that in quite simple form which captures none of the complexity of the real economy that is so central to Austrian economics.

<sup>46</sup>There are no fewer than 17 equations in 15 short pages, but this sort of “reasoning” is virtually unprecedented in the publications of Mises or other Austrians.

<sup>47</sup>Keynes (1936, p. 7), for example, said that, “Professor Pigou’s Theory of Unemployment [is] the only detailed account of the classical theory of employment which exists.”

<sup>48</sup>According to the OED, “macro-economic” and “macro-dynamic” were first used in 1939 (OED 1) and 1933 (OED 2), respectively. <http://dictionary.oed.com/cgi/entry/00299035>, <http://dictionary.oed.com/cgi/entry/00299033>.

extent the various pre-Keynesian analyses of cycles can be said to be grounded in marginalism,<sup>49</sup> though this and similar claims seem to have arisen because of Keynes's (1936, pp. 5-7) largely successful attempt in the *General Theory* to define a simplistic pre-Keynesian "macrotheory"; i.e., to set up a straw man as a foil for his theory. To wit [bold emphasis added]:

**The** classical theory of employment—supposedly simple and obvious—has been based, I think, on two fundamental postulates, though practically without discussion, namely:

- I. **The** wage is equal to the marginal product of labour. . .
- II. **The** utility of **the** wage when a given volume of labor is employed is equal to the marginal disutility of that amount of employment.

However, even though such analysis is "marginalist" in some sense, it is not marginalist from the Austrian perspective. That is, it is some sort of analysis of the relationships among aggregate variables at their margins. But, relationships, at the margin or otherwise, among aggregate variables are alien to praxeological economics.

Third, this statement is highly problematic as it misconstrues the different positions re equilibrium within Austrianism itself, as well as in classical economics. There are two positions on this issue within what might be called the broad Austrian camp. The more popular one, exemplified by the writings of, among others, Mises (1998, pp. 245-51) and Rothbard (1962, p. 467), is that the notion of equilibrium, or the "evenly rotating economy" is merely a heuristic device, an attempt to hold fast onto *ceteris paribus* conditions, the better to be able to make sense of a complex world. Yes, in this vision, there are always forces pushing markets in the direction of equilibrium, but no one (apart from a neoclassical) seriously contemplates ever reaching such a state of affairs. But, in this perspective, the concept does have some positive value, and not only for introducing sophomores to the problems attendant upon price ceilings and floors. In contrast, in what might be called the minority Austrian view (e.g., Lachmann 1976) the equilibrium concept is not only not of positive benefit, it is actually an impediment to clear thinking.

In sharp contrast to these Austrian views, the pre-Keynesian tradition, whether the partial equilibrium of Marshall or the general equilibrium of Walras, what is true or not true in equilibrium states is of the keenest interest. Their methodology is predicated to a surprisingly large degree upon this claim, as are their public policy recommendations.<sup>50</sup> And, this holds in

<sup>49</sup>Many were based on such concepts as overproduction, underconsumption, the denial of "Say's Law of Markets" and/or the "Equation of Exchange" and the "Quantity Theory of Money" (Schumpeter 1954, pp. 738-50, 1074-135).

<sup>50</sup>In particular, the (limited) support given by the mainstream economists to anti trust legislation is strongly related to their view that the real world rarely attains the characteristics of their perfectly competitive equilibrium model. Any deviation between the two serves as a presumptive case for "competition" legislation.

spades for modern mainstream macroeconomics; e.g., real business cycles and New Keynesianism.

*G&V on the Similarities Between Classical and Austrian Market Adjustment Mechanisms*

Consider the views of on market adjustment mechanisms:

Where does the Austrian perception of the macroeconomy fit in this paradigmatic description? We begin by noting that there are similarities between the classical perception of the macroeconomy and the Austrian view. Both are rooted in the marginalist tradition and both entertain the possible existence of a unique equilibrium position given appropriate *ceteris paribus* condition. However, they differ in one critical point, the nature of the adjustment toward . . . equilibrium. The classical view is susceptible to being interpreted as arguing for instantaneous adjustment to equilibrium (Boettke 1997). In fact it is often characterized in this fashion, perhaps unfairly. Such a depiction leads to the rather obvious point that business cycle fluctuations, especially of the magnitude of the Great Depression of the 1930s, are evidence of the shortcomings of the classical model, and by implication, market adjustment mechanisms in general.

By contrast, the Austrian perspective places greater emphasis on the elements of the market process, exploring the ways in which discoordination of markets occur and how markets respond to that discoordination. In general, Austrians are more interested in (10) when it reads (11)  $w_r/AP_L \neq (1-a)$ .<sup>51</sup> G&V (2000, p. 22)

It is difficult to find support in Boettke (1997) for G&V's statement that "the classical view is susceptible to being interpreted as arguing for instantaneous adjustment to equilibrium."<sup>52</sup> Moreover, it seems that these authors have spilled ink merely to say that Austrians prefer  $w_r/AP_L \neq (1-a)$  to the classical's  $w_r/AP_L = (1-a)$ . In fact, all of this seems designed to justify their eq. (12)  $\% \Delta ARW_t = f(ARW_{t-1} - \overline{ARW})$  and (13)  $\% \Delta ARW_t = -0.007 - 0.135(ARW_{t-1} - \overline{ARW})$ .<sup>53</sup>

The obvious retort to this claim is that the Great Depression was hardly characterized by a legal regime that allowed for free markets at all, let alone "market adjustment mechanisms in general." After all, this was the decade of

<sup>51</sup>See footnote 17, above.

<sup>52</sup>Boettke (1997, p. 49) did state: "For involuntary unemployment to be a myth, the New Classical economists posited economic actors who adjusted their behavior so quickly (for all practical purposes, instantly) that equilibrium would be achieved at all points." However, there is a world of difference between the classicals and the New Classicals, and, surely, G&V were not referring to the New Classicals in the quoted statement.

<sup>53</sup>The caret is missing from the LHS of G&V's eq. (13); we assume that this was an oversight. And, in the same equation, G&V have a bar over  $ARW_{t-1}$  as well as over  $ARW$ . As the average of a single value is identically itself, i.e.,  $\Sigma ARW_{t-1}/1 = \overline{ARW}_{t-1}$ , makes no sense, we assume this is a typo.

the Smoot Hawley tariff, price supports for agriculture, as well as the cartelization of industries (NRA) and labor (NLRA) to maintain prices and wages, respectively, the CCC, WPA, PWA, and a host of other “alphabet soup” agencies, and many other accoutrements of what became known under FDR as the “New Deal.” That is, it is a myth that because the Great Depression continued on for at least a decade<sup>54</sup> the market system failed; in reality the market process was not allowed to function.

### *Wage levels*

Here is footnote 2 of G&V (2000, p. 22):

It might be objected that Austrians are more oriented toward considerations of the structure of wages rather than their overall level. . . . However, both are important. Mises recognizes the significance of the level of wages in *Human Action* (1966, p. 578) when he remarks: “Out of the collapse of the boom there is only one way out [sic]. Wage rates must drop.” See also Rothbard (1983) regarding the role of wage levels in the Great Depression.

G&V have misconstrued the views of Mises on this issue. Compare the truncated quote of Mises in their footnote with what he actually said (we italicize what G&V kept in this quote, to demonstrate the enormous importance of the context dropped by these authors):

*Out of the collapse of the boom there is only one way* back to a state of affairs in which progressive accumulation of capital safeguards a steady improvement of material well being; new saving must accumulate the capital goods needed for a harmonious equipment of all branches of production with the capital required. One must provide the capital goods lacking in those branches which were unduly neglected in the boom. *Wage rates must drop*; people must restrict their consumption temporarily until the capital wasted by malinvestments is restored. Those who dislike these hardships of the readjustment period must abstain in time from credit expansion. (Mises 1998, pp. 575-76)

Based upon the mangled G&V quote one would think that Mises would support their focus on average wage rates, when this was really no part of his intention. Mises was concerned primarily with relative prices. Moreover, *if, arguendo*, we interpret Mises in the manner to which G&V wish us to, he was only speaking roughly. It is simply not true that *all* wage rates must drop. Very much to the contrary, during the cleansing bust phase of the depression, only

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<sup>54</sup>Higgs (1997, p. 563) maintains that:

In light of the foregoing observations, we may justifiably adopt the following chronology: Great Depression, 1930 to 1940; transition to the war economy, 1940 to 1941; war-command economy, 1942 to 1945; demobilization, reconversion, and decontrol (the true Great Escape [from the Great Depression], 1945 to 1946; postwar prosperity, 1946 and beyond.

those wage rates in the higher or earlier or more interest rate sensitive orders of production (a concept that appears nowhere in G&V, and yet is highly relevant to their concerns) must drop. This is because there was *over* investment in these areas of the economy, which need to be written down or off. However, and this cannot possibly be overemphasized, wage rates must *increase*, at least relatively, in the lower or later or less interest rate sensitive orders of production, because, previously, during the boom, they were decreased, at least relatively. We must never forget that the Austrian Theory of the Business Cycle is *neither* an overinvestment nor yet an under-investment theory; rather, it is a *mal*investment theory. But the malinvestments take the form of *over* investment in the capital goods sector (this is why not only must wages fall there, but so must other prices) and *under* investment in the lower orders of production, preeminently in non durable consumer goods (this is why not only must wages *rise* there, but so must other prices).

And what of Rothbard? G&V (2000, p. 22, n.2) purport to find support by him for their distinctively non-Austrian views, when they aver in this context: "See also Rothbard (1963) for a discussion of the role of wage levels in the Great Depression?" We do not think so. Rothbard (1983, p. 238) supportively offers the position of Hugh Bancroft, publisher of *Barron's*: "it was particularly necessary for wage rates to decline in the producer goods' industries in view of the great decline in prices there." Elsewhere (1983, p. 51) Rothbard states: "Business as we have pointed out, depends for its profitability on price *differentials* between factor and selling prices, not upon general price levels." Because a wage is a price, we see that Rothbard, in contradistinction to G&V, was concerned with wage rates relative to other prices and not with their levels.<sup>55</sup>

#### *Exogenous or Endogenous*

In Keynes's vision of the economy, the source of the business cycle is instability in investment. "Any fluctuation in investment not offset by a corresponding change in the propensity to consume will, of course, result in a fluctuation in employment" (Keynes 1936, p. 314). Further, "But I suggest that a more typical, and often the predominant, explanation of the crisis is, not primarily a rise in the rate of interest, but a sudden collapse in the marginal efficiency of capital" (p. 315). For Keynes, the downturn is caused by a "market failure:" the collapse of investment demand because of a sudden deflation of businessmen's "animal spirits," themselves irrational to begin with. Only government intervention, appropriate both as to type and magnitude, can save the day. Austrians, in contrast, see the bust as the necessary consequence of the false boom, itself stemming from prior inflationary monetary policy, that interfered with market signals, preeminently the interest rate. For them, there is no other way to explain the cluster of entrepreneurial errors that constitute

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<sup>55</sup>Rothbard (1963, pp. 154-55) is concerned with wages in *manufacturing*, a proxy for the higher, earlier or more interest rate sensitive orders of production, not with *all* wages, or the wage *level*.

the business cycle. To the question, “Why do entrepreneurs, whose very survival and prosperity is predicated upon successful anticipation of prices, act like lemmings rushing into the sea in the depression?” only the Austrians answer that they operate on the basis of market signals, and these are perverted by governmental monetary policy.<sup>56</sup>

How do G&V (2000, p. 27) deal with this issue? In their view, there are two

sources of stability. One, of course, is the modest, but important, reordinating mechanism we have described. The second is less evident, but certainly significant. It takes the form of a phenomenon articulated by Slutsky in the 1920s. In a paper published in Moscow (1927) he demonstrated that random perturbations in a basic data series are capable of generating cycles in the data.” (footnote omitted)

Say what you will about this theory, one thing is certain, it is *not* an Austrian perspective. But further, it is a highly problematic one, as it does not even address itself to the structure of production or interest rate sensitivity, essential elements of the Austrian business cycle theory (ABCT). Nor does it even broach the cluster of errors phenomenon, much less answer it.

*Contracyclical macroeconomic policy-making is an exercise in futility*

According to point 8 of G&V (2000, p. 32), “the notion of short run contracyclical macroeconomic policy making is an exercise in futility.”

At first glance, there are good and sufficient reasons for taking this stance. Rothbard (undated, pp. 25–26) himself has stated:

What is the governmental role in the cure of depression? . . . what the government should do, according to the Misesian analysis of the depression, is absolutely nothing. It should only from the point of view of economic health and ending the depression as quickly as possible, maintain a strict hands off, “*laissez faire*” policy. Anything it does will delay and obstruct the adjustment process of the market.

It is hard to see how the public policy advice could be more definitive. And yet, let us take another look at the matter, for it is our contention that Rothbard is, as are G&V, incorrect in this assessment, and is corrected by no less an expert than, paradoxically, Rothbard himself. For he *also* says (undated, p. 26) after giving a long list of things government should not do:<sup>57</sup>

The government must do nothing to encourage consumption, and it must not increase its own expenditures, for this will further increase the social

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<sup>56</sup>Cycles are misallocations of resources caused by inaccurate calculations (of expected profits) based on “dishonest prices,” themselves a consequence of “dishonest money” that is lent into existence during a governmental policy induced monetary-credit expansion. Thanks to Tony Deden who stated, at the ninth Austrian Scholars Conference: “[You] cannot have honest accounting if you have dishonest money.”

<sup>57</sup>It should not inflate, prop up failing businesses, promote wage increases or prohibit decreases through law.

consumption/investment ratio. In fact, cutting the government budget will improve the ratio. What the economy needs is not more consumption spending but more saving in order to validate some of the excessive investments in the boom.

That is, Rothbard in effect contradicts himself. First he maintains the state should do “absolutely nothing”; then, that it should reduce its budget, which is certainly doing *something*. Rothbard is too much of a libertarian to call for the state to *do* anything other than cutting back its scope of activity, but surely to do this is to *act*. Reading in between the lines of Rothbard (undated), we may infer that were government to subsidize saving and penalize consumption, he would not think it all to the bad, insofar as this is *precisely* the medicine implied by ABCT for the cure of the depression. Of course, whether or not the state could *succeed* in any such operation is greatly to be doubted, given the wealth of libertarian, Austrian and even Public Choice analysis to the contrary. The point is, *arguendo*, that if credence could be placed in central planning, which it most certainly cannot, then, at least, this would be the direction indicated for intervention, at least insofar as interventions intended directly to affect saving and/or consumption are concerned. Moreover, cutting governmental expenditures, cutting tax rates, and reducing or eliminating anticompetitive regulations would also hasten the recovery.<sup>58</sup>

#### *The centrality of the productivity-adjusted real wage*

G&V (2000, p. 31) maintain: “All major macroeconomic paradigms have as their centerpiece the productivity-adjusted real wage.” But either Austrian economics is not a major macroeconomic paradigm or else this proposition is false. The essence of ABCT is an unsustainable misallocation of resources induced by a fiat credit expansion; it is not at all the productivity-adjusted real wage. This term *never* even appears in either *Human Action* or *Man, Economy, and State*. Nor is there an entry for “real wage(s)” in the index of either of these treatises. Further, one looks in vain in the index of Garrison’s (2001) book on ABCT for either “productivity” or “real wage(s).” Certainly, then, this concept is not central to ABCT. Of course, that may merely mean that ABCT is not a major macroeconomic paradigm.

#### CONCLUSION

In their very last paragraph they (G&V 2000, p. 32) state: “it is clear that Mises’s vision of the nature of the macroeconomy is substantiated by our findings.” We totally disagree.

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<sup>58</sup>It should be noted that Keynesians would agree that cutting tax rates would be countercyclical in a downturn, they would not agree that cutting governmental expenditures in the same circumstances would be countercyclical; rather, they would think such a policy would exacerbate the downturn.

Austrians ought not to get involved with aggregation, simple/simplistic math models, and nonsensical statistical models/estimations. The very few valid points in G&V can be made without these.

While it is, no doubt, possible to construct economic aggregates and to test them empirically in such a way that their conclusions appear consistent with the general vision of Mises, this is a method that is entirely and profoundly antithetical to his conception of economics.<sup>59</sup>

There may be a (very weak and limited) sense in which G&V can be interpreted as compatible with Austrianism, apart from the foregoing. They mention Austrians in their paper.<sup>60</sup> But this simply is not good enough. Mere lip service paid to a few leaders is not sufficient to warrant membership in a distinct school of thought. It is as if an atheist were to mention “God,” and “explain” how his position on morals is compatible with His moral code, and, on that basis, a religious order were to seize upon such a statement as indicating the atheist to be a believer.

There was a period of time during which the very existence of the Austrian School was threatened by a dearth of followers (Rockwell 1995). Did such a possibility constitute a legitimate excuse for this sort of thing in those dark days of Austrianism? That question calls for tactical and strategic insights, to say nothing of an ethical analysis. Were Austrians under those conditions to accept as praxeological that which is patently no such thing, it would constitute a sort of intellectual fraud. Whether it would have been worth doing, were this the *only* way to keep the tradition alive, is a question beyond the scope of the present paper.

The Austrian School is now so well established that it ought to be well beyond any temptation to characterize as Austrian obviously mainstream work that merely condescends to mention its name and some of its leading lights. Nevertheless, that is exactly the case regarding G&V. We conclude that there is every need for a paper such as this present one, which strips away the supposed veneer of Austrianism from a publication which is no such thing.

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<sup>59</sup>These are the words of our referee.

<sup>60</sup>They also discuss marginalism. But this is hardly unique to Austrians. There are three economists typically credited (*The Concise Encyclopedia of Economics*) with the birth of this idea: Menger, Jevons, and Walras. The first was indeed an Austrian; but the others most certainly were not.



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*MODERN MACROECONOMICS: ITS ORIGINS, DEVELOPMENT, AND CURRENT STATE.* BY BRIAN SNOWDON AND HOWARD R. VANE. NORTHAMPTON, MASS.: EDWARD ELGAR, 2005.

Brian Snowdon and Howard R. Vane's *Modern Macroeconomics* is a description of each of the different major schools of thought relating to macroeconomic stabilization policy. The book had originally been intended to become the second edition of Snowdon et al. (1994), but the rewriting of the text became so extensive that the authors instead decided to publish it as an entirely new work (p. xiv). Although the book also covers economic growth theory and political macroeconomics, most of the text is about what has been called "the warring schools of macroeconomics" (Samuelson and Nordhaus 2004, chap. 33).

The book distinguishes seven different schools of macroeconomic thought: orthodox Keynesianism, orthodox monetarism, the New Classical School, real business cycle theory, new Keynesianism, Post Keynesianism, and the Austrian School. As the authors themselves acknowledge, their classification differs slightly from some of those made by other scholars (e.g., Phelps 1990), but it should not prove to be particularly controversial. Each school of thought receives one chapter, and most chapters conclude with an interview by the authors with a leading representative of the school (e.g., Milton Friedman on orthodox monetarism, Robert E. Lucas, Jr. on the New Classical School, Edward Prescott on real business cycle theory, N. Gregory Mankiw on new Keynesianism). The chapters on Post Keynesianism and on Austrian economics do not include such interviews, because they have been contributed by leading scholars of those schools (Paul Davidson and Roger W. Garrison, respectively) rather than by Snowdon and Vane.

What becomes clear upon reading through the chapters on stabilization policy is that after all these many years, the essence of the debate is still about the efficacy of government intervention (pp. 7-8). Keynesians argue that government can and therefore should intervene to correct what they consider deficiencies in the free-market system. Classical economists, on the other hand, believe that even to the extent that such deficiencies exist, it is not clear that government can and therefore should attempt to rectify them. Thus presented, Austrian economics would fit in the "classical" category. This is not to deny that there are, of course, substantive differences between Austrian and classical economics, just as there are critical differences between, say, the monetarist analysis of the business cycle and that by real business cycle theorists. But although different schools of thought within each of the two main groupings make different arguments to support their case, to some degree they nonetheless arrive at roughly similar macroeconomic policy recommendations.

In addition to the chapters on stabilization, there is one chapter on “the new political macroeconomics” and one on macroeconomic growth theory. The chapter on political macroeconomics, which examines the impact of the political context on the macroeconomic policy-making process, feels somewhat out of sequence with the rest of the book. Although the treatment is thorough, the literature associated with the public-choice research program receives scant attention (approximately half a page in a 62-page chapter). This is perhaps odd if for no other reason than that public choice theory was recognized by the Nobel Memorial Committee in the awarding of its 1986 economics prize to James M. Buchanan for his contributions in this field. Considering the length of the present volume (712 pages of text plus 100 pages of bibliography and indices), it would perhaps have been better to have published this chapter as a separate book that included more material on the public choice literature.

The authors point out that in the long run, economic growth has a much greater impact on living standards than do most attempts at stabilization policy (pp. 32–33). The fact that stabilization nonetheless receives the bulk of the attention in the book reflects the current state of orthodox economic science. But if the authors are correct, the proportion of space devoted to stabilization vs. growth may well be reversed in future editions. The core of the chapter on economic growth, the longest in the book, is a discussion of three types of growth models (Harrod-Domar, Solow-Swan, and Romer-Lucas’s endogenous growth models). In spite of the thorough treatment and extensive references, for some reason the well-known contributions by Ramsey (1928) and Phelps (1961) to growth theory are not mentioned in the text of the chapter.

The main overall contribution of the book is the diligence and fairness of its presentation of each of the different macroeconomic schools of thought of stabilization policy. The treatment is so exhaustive that unless you have some degree of familiarity with the subject matter, it may be easy to lose the forest for the trees (e.g., in the 30-page description of all the different new-Keynesian explanations for wage and price rigidity, pp. 366–96). But the contributions of each school of thought have been nicely summed up and assessed by the authors at the end of each chapter. In addition, the final chapter presents six remarkably detailed propositions about which the authors believe there is a consensus that, although perhaps not quite “unanimous,” is nonetheless “widespread” among mainstream economists (pp. 703–05). In terms of presenting a comprehensive overview of the history and current state of macroeconomic thought, the book is outstanding and deserving of the accolades it has received.

As a reader you are afforded the opportunity to make up your own mind regarding the merits of each of the different schools, in the unlikely event you are coming to this text as an agnostic. Representatives of several different schools clearly receive their due. Although the authors exhibit Keynesian leanings, the influence of Milton Friedman’s contributions, particularly of Friedman and Schwartz (1963) and of Friedman (1968), is heralded without qualification (pp. 165, 175). So is that of Robert E. Lucas, Jr. (pp. 220–21). A minor point of criticism would be that Friedman (1962) is missing from the discussion on central bank independence, which furthermore is oddly broken into two different sections in the book (pp. 257–62 and 549–54). Perhaps as a result, both sections lack focus: the pros and cons of different institutional monetary arrangements (automatic commodity standard, independent monetary authority, rules enacted by the legislature) could have been presented much more sharply.

The book contains an extensive bibliography, spanning 83 pages with more than 1,300 entries. Perhaps an obvious omission is David Ricardo's *On the Principles of Political Economy and Taxation*. Of course, this work was first published in 1817, or prior to Keynes's *General Theory* (1936), which the authors take as their starting point for macroeconomics. But most other pre-Keynesian works in the historical canon of economics (e.g., Adam Smith's *Wealth of Nations*, John Stuart Mill's *Principles of Political Economy*, Alfred Marshall's *Principles of Economics*) have all been included. Ricardo also does not appear in either the author index or subject index, although he is mentioned in the main text in several places. The subject index, incidentally, is somewhat brief. For example, the book discusses Friedman's time lags that contribute to the inefficacy of fiscal stabilization policy, but these lags do not show up in the subject index. The word interest, to randomly pick another one, also does not appear in the subject index.

Notably, the book includes a chapter on Austrian economics, written by Professor Roger W. Garrison. There are advantages to the chapter having been written by a representative of the Austrian School rather than by Snowdon and Vane themselves. The description of Austrian economics is as fair and accurate as any Austrian could hope for. Indeed, throughout the entire volume not a disparaging word is said about Austrian economics, or it would have to be the label "radical" (p. 698), which is equally applied to Post Keynesianism. Indeed, it is to the authors' credit to have recognized the merits of Austrian economics in the first place with the allocation of an entire chapter. Still, there are limitations that arise from the Austrian chapter being presented as a stand-alone entity written by a separate author.

Throughout the rest of the book, there are many instances where different schools of thought are compared and contrasted, but the Austrian viewpoint is usually not included in these comparisons. For example, the debate about the neutrality of money is discussed in several chapters, but nowhere is mentioned the Austrian view that money is not neutral because "changes in the quantity of money can never affect the prices of all goods and services at the same time and to the same extent" (Mises 1998, p. 396). The book contains a very good explanation of the Lucas critique (pp. 264-67) of the practice of macroeconometric modeling, namely that it tends to fail to account for parametric changes following changes in government policy. It would have been instructive to at least mention in this context the reasons for the Austrian aversion to mathematical modeling of the economy. Elsewhere (p. 174), the authors discuss the common ground between orthodox Keynesianism and Milton Friedman's monetarism. They conclude that "[a]lthough orthodox monetarism presented itself as an alternative to the standard Keynesian model, it did not constitute a radical theoretical challenge to it" (p. 357). This discussion would have been more complete if the Austrian case in that regard (e.g., Garrison, 1992) had been included.

The book frequently brings up the debate as to whether macroeconomics requires microeconomic foundations. Austrians would argue that it is a selling point that their macroeconomic theories are indeed based on a foundation of microeconomics, and that the split between micro- and macro- in orthodox economics is arguably meaningless and indicative of its shortcomings (cf. Horwitz 2000). But, although relevant, there is no mention of this in the text outside of Professor Garrison's chapter.

There are other examples of the lack of integration of Austrian economics in the main text. Friedrich von Hayek is acknowledged as an influence on the New Classical School's early emphasis on information and expectations (p. 223). But the point

is not developed, and the reader is left guessing to which of Hayek's publications the authors are referring.

As a final example, it would have been helpful to see the Austrian view on growth debated in the chapter on economic growth theory. The Austrian view that growth is not a maximand, that instead growth should merely reflect economic agents' intertemporal preferences as expressed in their consumption and saving decisions (p. 482), would appear to be highly controversial to orthodox economics.

As the authors themselves point out (p. 5), intense academic debate is conducive to arriving at truth. For this reason alone, it would have been appropriate if the Austrian School had been engaged in debate more fully throughout the text. Roger Garrison's chapter, mirroring the approach of his pathbreaking work *Time and Money* (2001), does just that, but his propositions go unanswered. Considering the authors' fair and reasonable treatment of each of the other different schools of thought, it would have been interesting to hear their assessment of the Austrian School.

For all their fairness and impartiality, the authors exhibit clear Keynesian leanings. As noted, they take Keynes instead of Smith as their starting point. If economics began with Smith, macroeconomics began with Keynes (pp. xv, 32; p. 698 claims that there is "no dissent" about this). But although the influence of Keynes on macroeconomic theory is universally recognized even by Austrians, in a volume such as this it would have been appropriate to have included more about the historical development of economics prior to the 1930s, not all of which was strictly about price theory, or what would subsequently come to be called microeconomics. Certainly Austrians such as Mises (1912), Hayek (1929, 1931), and Haberler (1932) had been thinking about monetary economics and business cycles long before the publication of Keynes's *General Theory* in 1936.

Four of the eight chapters dealing with stabilization are about Keynesianism ("Keynes v. the 'old' classical model," "The orthodox Keynesian school," "The new Keynesian school," "The Post Keynesian school"). This seems excessive, especially since the authors (p. 197) agree with Mankiw (1989) that what has come to be known as the new Keynesianism might as well have been called the new Monetarism (e.g., the preference for monetary over fiscal stabilization policy; cf. Modigliani's [1977] statement that "we are all monetarists"; cf. also DeLong 2000). In the introductory chapter, the authors unabashedly state that "[t]he most plausible explanation of the Great Depression is one involving a massive decline in aggregate demand" (p. 14), even though the different schools of macroeconomic thought to this day remain very much divided on that point.

The authors cite and agree with Vercelli (1991) that "the work of Keynes remains the 'main single point of reference, either positive or negative, for all the schools of macroeconomics' " (p. 29). As such, the book reflects perhaps the centrality of Keynesian theory to the development and current state of macroeconomics. But this is unfortunate. Volumes such as the present one more or less codify the present state of macroeconomic science. When they adopt Keynesian centrality, it becomes that much more difficult for the science to move on and get beyond Keynesianism.

Still, the book is highly suitable as a textbook for an undergraduate course in intermediate or advanced macroeconomics with lower-level macro courses as a prerequisite. Alternatively, the book could be used in a first-semester survey course in a graduate program. The book assumes a certain familiarity with basic concepts in

macroeconomics, such as the IS-LM model (p. 102), although it frequently points out where in the literature students can go in order to brush up on those basic concepts.

All in all, the thoroughness and fairness of the exposition of the different schools of thought make *Modern Macroeconomics* a worthwhile addition to the academic literature. Austrians should relish the fact that the book treats their theories better than most comparable economic textbooks do. After an absence of almost 70 years, Austrians may have begun regaining their seat at the table, even if at the same time *Modern Macroeconomics* makes clear that they still have work left to do in convincing their colleagues about the merits of their views.

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*DEFLATION: CURRENT AND HISTORICAL PERSPECTIVES.*  
EDITED BY RICHARD C.K. BURDEKIN AND PIERRE L.  
SIKLOS. CAMBRIDGE: CAMBRIDGE UNIVERSITY PRESS,  
2004.

The book *Deflation: Current and Historical Perspectives*, dedicated to the history and economics of the phenomenon of falling prices, is composed of 11 contributions by 20 economists from two major conferences on this scarcely investigated topic that were held in April 2000 at Claremont McKenna College and in 2002 at the XIIIth International Economic History Congress in Buenos Aires.<sup>1</sup> The editors have organized the articles, all of which examine different historical periods of deflation, into four parts. They have also provided a scholarly introduction that aptly combines an exposition of a few theoretical elements, such as Keynes's and Fisher's analyses of the impact of continuously decreasing prices, with some general historical evidence<sup>2</sup> and an overview of the different contributions.

Part one, "Fears of Deflation and the Role of Monetary Policy," contains three contributions that deal with individuals' past reactions to situations of deflation in the days of the Great Depression, in Britain during the period 1870-1939 and in Sweden during the deflations of 1921-1923 and 1931-1933. Part two, "Deflation and Asset Prices," contains two analyses of the proper way decreasing asset prices should be integrated in the conduct of monetary policy. Part three provides three "International Perspectives on Deflation." The first examines the relation between deflation and output in Canada and the United States of America during the classical gold standard.<sup>3</sup> The second article re-examines the policy of the strong lira in Italy's interwar period, while the third article reviews the causes and consequences of the recent deflation in Japan. The three contributions in part four are dedicated to "Stock Market Adjustments to Deflation." The comparative study of the responses of the stock exchanges in London, New York, Paris, and Berlin to the financial crises of the 1890s is followed

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<sup>1</sup>Three of the contributions have already been published as working papers by the NBER, as indicated where relevant.

<sup>2</sup>For a quick introduction to the history of deflation at the end of the nineteenth and at the beginning of the twentieth century, including a substantial amount of data, cf. Bordo et al. (2004).

<sup>3</sup>This paper by Bordo and Redish was published in March 2003 as NBER Working Paper 9520. It has already received a comprehensive discussion and very important comments by Thornton (2003) in the pages of this journal.

by an analysis of Germany's stock market during the alternating periods of inflation and deflation in the period 1913–1926. Finally, an article on the role of gold stocks as a hedge against deflationary pressures in the past and today closes this volume of 359 pages rich with empirical data.

One may rightly observe that this organization is arbitrary and does not ease the communication of the main ideas to the reader. One may also note that some of the articles do not really deal with the analysis of deflation strictly speaking. For example, what ultimately interests Martin Bohl and Pierre Siklos in “The Stock Market and the Business Circle in Periods of Deflation, (Hyper-) Inflation, and Political Turmoil: Germany, 1913–1926” is whether the present value model of asset price determination applies to periods of decreasing prices of assets as well as to periods of increasing prices. Richard Burdekin and Marc Weidenmier's “Deflationary Pressures and the Role of Gold Stocks: 1929, 1987, and Today” only establishes that in the past gold stocks did not need a climate of general inflation in order to perform well, and that in the aftermath of 1929 and of 2000 they served as a refuge but did not in the market decline in 1987. To give only one other example, Hugh Rockoff's analysis of “Deflation, Silent Runs, and Bank Holidays in the Great Contraction” examines the consequences of regional conflicts in monetary policy for the transmission of a banking crisis through inter-bank capital outflows, rather than the impact of deflation upon the economy.<sup>4</sup> The wide heterogeneity of the main research topics that are dealt with, and the subsequent lack of general unity of the volume, makes reading the book quite difficult.

However, despite these weaknesses and a few more to which we shall turn in a while, the volume has two great virtues that cannot be overemphasized. First, it is explicitly admitted that deflation is not always bad and that “good” deflation is theoretically quite possible. Second, the contributions that treated the relation between deflation and economic performance, contain data that clearly show that (1) deflations in the past were not disastrous and that (2) bad consequences, in terms of decreased output and higher unemployment, resulted from factors other than decreasing prices themselves. Thus, from the outset, one can hope that this volume will contribute to demystifying the current fears about deflation among the economic profession. From the point of view of Austrian economists, such an endeavor on behalf of mainstream economists can only be welcomed.

#### “GOOD” VERSUS “BAD” DEFLATION

In their opening chapter the editors allow for “good” deflation, deflation being defined as a “general fall in some aggregate price level,” under one very specific condition only (p. 7). A decrease in prices is good if it is driven by a positive supply shock, due to productivity or technological improvements for instance. In the jargon of the aggregate demand-aggregate supply model, they state:

Falling goods prices could be triggered by any number of factors, including not only a drop in asset prices but also positive supply shocks that

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<sup>4</sup>This contribution was published by the NBER in March 2003 as working paper 9522.

shift the economy's aggregate supply curve to the right, thereby putting downward pressure on prices *even as output increases*. Although this allows for the possibility of "good" deflation rather than "bad" deflation, it is still true that sustained deflation is only possible when the rate of money growth falls beyond the rate of growth of output and money demand. (p. 6; emphasis added)

The rationale for this kind of deflation being good is that real wealth *is* actually increasing.

On the other hand, deflations are bad whenever they are triggered by self-fulfilling expectations of decreasing prices or by "poor policy choices" (p. 10). The rationale for this kind of deflation being bad is that it *leads to* a decrease in aggregate demand, and hence in aggregate output according to their argument. These negative consequences on production are explained on the basis of three different channels. One channel is that of the intertemporal substitution effects. Expectations of continuous deflation supposedly imply a reduction in present-day consumption, i.e., in current aggregate demand, and therefore a lower output. A second channel concerns the ability of enterprises to pay off debts when nominal prices of output and of existing assets are decreasing. The bankruptcies that ensue in this case lead, again, to higher unemployment. With the third channel, falling output prices are particularly bad if nominal wages are rigid in the economy: "Deflation may be "worse" than inflation because of firms confronting rising real wages should nominal wage rigidity prevail" (p. 1). This again brings about lower employment and output.

Note that this distinction between "good" and "bad" deflation is not a genuine one, for the distinctive criterion is not the same in both cases. The goodness of a deflation is based on its causes (a supply shock), while its badness is defined relative to its consequences (a decrease in output and employment). Thus, according to this taxonomy, nothing prevents a deflation that is initially good from becoming bad later on. This is what should actually be considered the most probable outcome in the framework adopted by the editors, for they suppose that deflation brings about self-fulfilling expectations. In final analysis, the theoretical case for good deflation that the editors make is an ephemeral concession, for any deflation is destined to eventually become bad.

Within the framework of this analysis, which takes the relation between decreasing prices and bad consequences in terms of output and employment for granted, the question that naturally emerges next is, What is the monetary policy that can prevent deflation? Michael Bordo and Olivier Jeanne's contribution "Boom-Busts in Asset Prices, Economic Instability, and Monetary Policy" deals with exactly this issue.<sup>5</sup> The authors construct a small model suggesting that the optimal monetary policy does not take the form of a mechanical linear rule, such as the Taylor rule (p. 158). Rather, a monetary policy that takes into account the risk of bringing about a credit crunch, and therefore a future fall in real activity, shall be loose or tight according to the level of *market exuberance*. However, even though the model has the virtue of explicitly introducing expectations and individual reactions into the analysis, the basic hypothesis behind it still remains that a tightening of the credit market leads to a fall in real production.

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<sup>5</sup>This paper too was published by the NBER in May 2002 as working paper 8966.

This last hypothesis, as well as the other channels that are used to depict deflation as implying bad consequences, are all unacceptable.<sup>6</sup> To begin with, bankruptcies—whether caused by a higher debt-burden or by a deficient demand—lead only to a redistribution, and in no case to the destruction, of property. The reallocation of goods takes time and necessarily impoverishes some individuals while making others better off. However, there is no reason why aggregate output should decrease. We can even make the contrary case that debt-induced bankruptcies, because they transfer goods from the more indebted and hence less thrifty to the less indebted and hence more thrifty individuals, increase aggregate savings, and therefore real output in the future. Independent from this highly beneficial effect of deflation, this redistribution of wealth is not qualitatively different from the redistribution of wealth that follows any voluntary exchange on the market.

On similar grounds, the bad consequences of a decreasing aggregate demand triggered by expectations of lower future prices have to be discarded. First of all, lower and lower prices, rather than delaying purchases, may incite some individuals to buy goods that they now consider cheap enough given their own preferences. But even if we concede, for the sake of argument, that deflationary expectations lead to a lower aggregate demand, this does not imply that real activity will be reduced. It is true that in this case some exchanges will not be realized (at least until prices decrease enough), but this does not mean that the quantity of goods decreases in the present or in the future. Again, only the distribution of goods will be different among individuals, and this because of the fact that some individuals refrain themselves from buying and/or selling. To say that deflation has negative consequences because at current prices some exchanges are not realized ultimately means that any exchange has negative consequences. As a matter of fact, at any moment there are exchanges that are necessarily curtailed precisely because of the current conditions of action. It follows then that the negative consequences of decreasing prices per se have to be denied.

Forrest Capie and Geoffrey Wood's "Price Change, Financial Stability, and the British Economy, 1879-1939" does not defend such a categorical rejection of the channels through which deflation negatively impacts the economy. However, the authors show that these channels did not operate in the case of the two deflations the British economy experienced in the late nineteenth century and in the interwar years. Through an econometric estimation of expectations of change in prices, their analysis suggests that deflation was largely expected and that therefore individuals acted in such a way that no adverse effects can be discovered through the channels of Keynes's and Fisher's models: "We, therefore, conclude on this basis that deflation transmitted no adverse effects to the real economy through the channels suggested by these models" (p. 86). The conclusion that deflation is not always disruptive to the economy, but that unbalances are due to other factors within a deflationary context, can certainly be considered as quite a revolutionary finding in mainstream economics. The authors also make a very important comment, namely that if there was an adverse effect on employment, it is imputable not so much to decreasing prices per se, but to nominal rigidities: "It is true that, in the deflation of 1929 to 1932, falling

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<sup>6</sup>For a much larger list of myths about deflation, and their systematic debunking, cf. Hülsmann (2003a).

prices *and rigid money wages* did produce unemployment. Yet, even that was relatively short-lived, and there followed a great boom from 1932 to 1937-1938" (p. 86; emphasis added).

As a matter of fact, that is the only condition under which decreasing prices can be linked to lower output and employment. If costs of production are held constant while revenues are decreasing, then some processes of production become unprofitable and are discontinued, thereby freeing capital and labor. The important consideration here, however, is that no reallocation of the unemployed resources is possible, precisely because the rigidity of some of the prices prevents it. In other words, within the analytical distinction that the volume makes between bad and good deflation, it is the bad type that should have been considered the exceptional situation because price adjustments are being hampered. Unfortunately, this is not the conclusion conveyed by *Deflation*. The theoretical case for good deflation, although explicitly admitted in the volume, appears weak and will certainly not produce the much needed appeasement of the fears about deflation.

#### INSTITUTIONAL FACTORS AND ECONOMIC PERFORMANCE

Klas Fregert and Lars Jonung's "Deflation Dynamics in Sweden: Perceptions, Expectations, and Adjustment During the Deflations of 1921-1923 and 1931-1933" considers cases of deflation as rare events that can only be understood qualitatively, through a study of individuals' expectations of and reactions to the economic events. The authors take into account three groups: economists, policy makers, and wage-setters. Their article contains a vivid sketch of the economic debates between the members of the Stockholm School and of their interaction with policy announcements and actions which are very well detailed.

The deflation of 1921-1923 in Sweden was necessary in order to return the highly depreciated krona, which had lost some 60 percent of its purchasing power, to its pre-war gold parity because of the war inflation. This policy measure was supported by economists, all of whom "stressed the good performance of the gold standard before 1914 in comparison with the paper standard during the war," and by policy makers and by the general opinion (p. 98). The deflation process went on quite quickly and the 60-percent fall in the wholesale prices was realized by the end of 1922 (p. 97). But how did nominal wages and the general economic performance evolve?

For present-day standards, something unthinkable happened: employees accepted nominal wage reductions of about 50 percent over the period 1921-1923.<sup>7</sup> The real contribution of Fregert and Jonung is to make explicit the timing of these reductions, which helps us understand why unemployment increased sharply during the adjustment process and remained throughout the 1920s at double its pre-deflation level. As

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<sup>7</sup>One reason for the actuality of this wage flexibility, so high for modern standards, is certainly rooted in the economists' attitude toward price rigidities. Rather than trying to build a neverending list of explanations of rigidities, which finally contributes to their legitimization, economists at the beginning of the twentieth century never doubted flexible prices were the standard case, as the authors remind us through an aptly chosen quotation from Knut Wicksell: "The idea that the workers could not be persuaded to lower their claims, even if prices go down, is pure fancy" (p. 99).

a matter of fact, the wage decreases occurred mainly after the deflation was over: "Most of the price deflation took place in 1921, whereas wage deflation took place primarily in 1922" (p. 101). This delayed adjustment, and not deflation per se, explains why unemployment skyrocketed in 1921 to 25 per cent, before reaching its rather permanent level of 10 per cent in 1923–1924.<sup>8</sup> The authors themselves attribute this higher unemployment to the insufficient wage adjustment: "It is difficult not to attribute the permanently higher unemployment rate at about 10 percent, which lasted for the rest of the 1920s, to this real wage shock." (p. 104).

But then, the next question is why did employees not accept a quicker and larger adjustment, in which case the temporary effects on production and the permanent effects on employment could have been avoided? The authors detail the negotiations between employers and labor unions, and concluded that "Overall, the existence of collective agreements slowed down the adjustment process, as most wage reductions did not occur until 1922 when the deflation was almost over" (p. 105). But why did collective agreements play such an adverse role? The answer, although absent from Fregert and Jonung's contribution, can easily be inferred from a declaration made by the chairman of the Sweden Trade Union Federation in 1921 that they report but do not comment upon: "If there once is a significant reduction in consumer prices, then wages may become affected, but such a decrease has not happened, and it cannot be right that workers should pay before an expected but not actual decrease in the expense of consumption has happened" (p. 100). In other words, quarrels about who should first bear the distribution effects brought about by the decreasing prices are responsible for the delayed and partial adjustment. It is unfortunate that none of the contributions to the volume even raises this most important question.

The data concerning the second deflationary episode also confirm that wage rigidities are responsible for delayed adjustments in the context of deflation, even though the authors give a different interpretation. They report the support of economists for abandonment of the gold standard in September 1931, who argued that this was the way to insulate Sweden from the ongoing world deflation (p. 113). Why these same economists that defended the gold standard ten years earlier changed their mind remains however unexplained. The authors themselves express a favorable opinion of this policy measure: "The great advantage of the paper standard was that Sweden became isolated from the 'destructive' deflation that was taking place in countries still on gold" (p. 118). Now, despite this new weapon against deflation, results turn out to be rather mitigated, if not worse, for "The 1930s depression was about as deep as in the 1920s, *but it lasted longer*" (p. 123; emphasis added). What can be the explanation of this failure? The data that the authors report in their Figure 4.7 clearly show that real wages increased by 10 percent from 1930 to 1933, simultaneously with increases in unemployment, before both decreased afterwards. The negotiations that are again very well detailed show that nominal wages decreased much less, that the actual reductions were half what was contractually agreed to, and that "Wages decreased very little during the crises and then only gradually began to grow" (p. 125). In other words,

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<sup>8</sup>Although this behavior of employment is the main factor that explains the temporary decrease in Sweden's output, one should not neglect also the fact that during the adjustment process the number of lost working days in conflicts had increased substantially (p. 104).

wage rigidities were the most plausible explanation of the adverse effects upon the Swedish economy, even though Fregert and Jonung's interpretation of the data on this second deflationary period does not follow this direction.

A similar bias toward monetary interventionism can be discovered in Michele Fratianni and Franco Spinelli's "The Strong Lira Policy and Deflation in Italy's Interwar Period." One of their theses is that the Italian economy performed better than the American economy because of a looser monetary policy through an inventive channel of the lender of last resort. The authors also detail the achievements and the reasons for the abandonment of Mussolini's strong lira policy and derive some insights about the suitability of fixed exchange rates for domestic stabilization programs. Despite the overt pro-inflation favoritism of their contribution, the discussion of the relation between deflation and depression is highly instructive. They retain only two causal factors for depressing real economic activity: the collapse of international trade and nominal wage rigidities (p. 236). Furthermore, the reason why depression in Italy was less severe than in the United States is rooted in the relatively more flexible nominal wages there:

There is a significant difference between Italy and the United States concerning the relationship between price deflation and output growth. During the 1927-1933 period, the two variables are highly and positively correlated for the United States and *uncorrelated for Italy*. These patterns suggest that nominal rigidities were more of a force in the United States than in Italy. (pp. 236-37; emphasis added)

In the final analysis, the authors should have concluded that the data from Italy support the finding that there is no relation whatever between deflation and depression. Then, they should have looked for institutional factors in the Italian economy that could account for the 12 percent decrease in industrial production in the 1929-1934 period.

Special attention is paid to the institutional framework and its impact upon the economy in Lance Davis, Larry Neal, and Eugene White's "Deflation, the Financial Crises of the 1890s, and Stock Exchange Responses in London, New York, Paris, and Berlin." The authors undertake a comparative study of the evolutions on these major stock exchanges with the view to establish how different political and legal arrangements alter the effects of deflation on the economy. The research project is praiseworthy, and the article synthesizes extremely interesting and curious pieces of information. Thus, it appears for example that by the 1890s the volume of business on the *Coulisse*, the unofficial stock exchange in Paris, was more than 50 percent greater than that in the official market (p. 287). The authors show how political forces and self-protective measures by stockbrokers curtailed competition on financial markets respectively in France and Great Britain. Free-market competition on the NYSE is considered as instrumental in allowing self-interested innovations to prevent financial activity from declining despite the decrease in asset prices. The lesson that the reader is invited to draw is that in the long-run the economies with more competitive financial markets behaved better than the others. This last conclusion, however, if not to be taken as self-evident, is not supported by any evidence. Even though the authors are very successful in showing that financial markets did develop even during deflation, they unfortunately fail to substantiate their claim that less political and legal interventionism was comparatively better for the real economy.

This last criticism is not unimportant. Many of the contributions to the volume contain interesting data and give vivid sketches of episodes of economic and financial history. However, they are badly in want of a solid theory that would allow the authors to arrive at a justifiable and correct interpretation of past events. Even though empirical elements that demystify deflation do exist in the collection, they are scattered, not always exploited, and will probably not have the impact on the economic profession one could hope for. The unsatisfactory usage of historical data is due to theoretical weaknesses.

### DEFLATION AND THE FUNDAMENTALS OF MONETARY THEORY

What *Deflation* really suffers from is a neglect of the most basic propositions of monetary theory. The first and foremost monetary law states that the economy can adapt itself to any quantity of money, and therefore one cannot speak of an optimal quantity of money that would correspond to any particular set of circumstances. Even though this adaptation takes time and brings about a redistribution of property, no causal relation whatever can be established between changes in the quantity of money and changes in aggregate production. On the other hand, monetary prices do change in such an asymmetrical way, spatially as well as temporally, that monetary neutrality is never possible.<sup>9</sup>

The second most essential conclusion of monetary theory establishes the differences between monetary competition and government monopoly in the production of money. Monetary competition does not only mean that a bunch of commodity monies are competing between themselves, but that their producers have to compete with the producers of any other good in the economy in order to attract the much needed labor and capital. This implies that the quantities of the various free-market monies are ultimately determined by consumers' preferences. Under monetary competition nothing dictates that the various monies be physically used in exchanges—rather they can be represented in actual exchanges by money substitutes. Government intervention in the production of money dramatically changes these basic characteristics of the monetary framework. An increase in a monopolistically produced paper money, protected by legal tender legislation, is not subject to consumers' choice and therefore depends exclusively upon the discretion of the monetary authority. Banks are no longer required to keep one hundred percent reserves and they may engage in a fraudulent production of fiduciary media that only partially represent money. Bank credit, which does not imply a previous act of saving and is hence fundamentally different from real or commodity credit, becomes possible and plays a crucial role for the occurrence of booms and busts.

The articles in *Deflation* ignore all this economic knowledge and often present contrary ideas without even an attempt at a justification. Thus, Capie and Wood assert that "New gold discoveries in the 1890s helped alleviate the *gold shortage*, and prices began to rise again" (p. 68; emphasis added). But how could the quantity of gold money have ever been insufficient if money's utility stems from its purchasing power and prices can always adjust in order to satisfy any demand for real cash balances? Rockoff seems to have fallen prey to another common fallacy when he contends that

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<sup>9</sup>For demonstrations of this proposition, cf. Mises (1998, pp. 416-19), Rothbard (2004, pp. 764-66), and Hülsmann (2003b).



a chart depicting gains and losses of deposits by federal reserve district in 1929–1930 “illustrates the tendency of deposits to move from the smaller, *and therefore* weaker, banking systems to larger, *and therefore* stronger, systems” (p. 45; emphases added). The author does not explain how exactly the total volume of assets of a bank implies this inverse consequence upon the likelihood of its going bankrupt. The economics of fractional reserve banking establishes that such an implication is unsustainable.

Another economic error is uttered by such a keen scholar of the gold standard as Michael Bordo who is convinced, together with Olivier Jeanne, that “Restricting monetary policy implies immediate costs in terms of lower output and inflation” (p. 131). The authors must be considering this “implication” for a trivial truth, otherwise they would have cared to sketch a justification of it. The quest for such an explanation would have revealed that the positive relation between output and quantity of money, though not impossible, is certainly not inevitable. This same false belief of the existence of a Phillips curve is what ultimately accounts for the *insulation* virtues that two of the contributions ascribe to depreciation and inflation.<sup>10</sup> Fregert and Jonung are persuaded that “The policy measure that most significantly influenced the rapid expansion of the Swedish economy during the 1930s compared with countries that remained on gold was the depreciation of the krona” (p. 114). Commenting upon the same historical episode, Michael Hutchison in his otherwise well-documented and complete “Deflation and Stagnation in Japan: Collapse of the Monetary Transmission Mechanism and Echo From the 1930s” supposes that the abandonment of the gold standard was causally connected to the recovery of the economy: “Sweden was similarly adversely affected by maintaining the gold standard, and recovery was enhanced when it was abandoned” (p. 262). Another fallacious implication that we cannot omit mentioning here is the relation that Bordo and Redish see between growth and instability, with a certain Marxist flavor: “the United States was much more developed than Canada and, *hence*, was *more* exposed to the business cycle” (p. 192; emphasis added).

This is not an exhaustive list of the erroneous theoretical elements on which most of the contributions are resting. We only intend to show here that the scattered bits of economics in *Deflation* are unsatisfactory and that the volume essentially lacks a sound theoretical framework. One understands now why the collection is encapsulated in the simplistic and contradictory distinction between “good” and “bad” deflation as presented in the first section above, neglecting Salerno’s recent contribution to a sophisticated and coherent taxonomy of deflations (Salerno 2003). Salerno’s taxonomy makes it clear that one cannot fully understand deflations unless a distinction is made between a market-driven deflation and an intervention-imposed deflation. Ultimately then, it is the absence of an analysis of the nature and consequences of government intervention in the production of money that is the greatest weakness of *Deflation*. Even though the empirical content of the volume is quite significant, as portrayed mainly in the second section above, we doubt that the book will manage to

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<sup>10</sup>The *insulation* thesis assumes that an appropriate domestic monetary management can protect an economy against an external shock, to such an extent as to make domestic economic activity insulated, i.e., independent, from economic conditions in other countries. For a recent presentation of the insulation thesis, and a systematic and convincing exposition of the error it contains, cf. Glavan (2005).

make economists think differently about deflation given its theoretical flaws. The editors' warning in the Preface that "the study of deflation is, in many ways, in its infancy" could not have been more accurate.

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