GALLAWAY AND VEDDER ON STABILIZATION POLICY

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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)[1]:

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 recoordinating 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 recoordinating 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.

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.[2] 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[3]

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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[2]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.

[3]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.[4] 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.[5] They then assume a linear form for (1), yielding (3) L = b(PQ/W),[6] 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.[7]

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,[8] leaves only the price level as a

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[4]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).

[5]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.

[6]"b" is implicitly assumed to be constant.

[7]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.

[8]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.[9]

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.[10] 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,[11] or as L = (1/m) (PQ/W) which is G&V's (3), again, with m = 1/b.[12]

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 ≠ 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 ≠ W/bAP_L),[13] 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 ≠ 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 ≠ 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 ≠ 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

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[9]We thank a very thorough and insightful referee for this point.

[10]A markup of 1 sets P = U with nothing left for the complementary resources or profits, therefore, m > 1.

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

[12]This equation may be interpreted as either the demand for labor or an aggregate demand curve.

[13]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 an 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."[14] Then they convert from real to monetary values of output: (6) $PQ = PAK^aL^{(1-a)}$. Next these authors take the partial derivative[15] 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)[16] 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.

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[14]Their production function is written aK^aL^{1-a}. This obviously is a typo, so we have rewritten it AK^aL^{1-a}.

[15]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).

[16]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] ≠ 1-a.[17]

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[18]

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.[19] 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 (ε_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

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[17]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.

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

[19]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: $w_r/AP_L = ((1+1/E)/(1+1/e))(\varepsilon_L)$, 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 ε_L is the output elasticity of labor. This is quite a different relationship than the ARW= ε_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.[20]

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.[21]

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)[22] must be

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[20]It is hard to imagine a model more in conflict with Austrian methodology.

[21]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.

[22]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, α, 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.[23]

Moreover, the correct, general formulation of the ARW[24] 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., $((1+1/E)/(1+1/e))(\varepsilon_L)$. 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 $a = 0.5$,[25] 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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[23]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.

[24]This assumes the ARW is defined as w_r/APL, as per G&V.

[25]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.[26]

It availeth naught to reply that production functions are merely symbolic.[27] 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^{a}L$^{(1-a)}$] 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):[28]

> 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.[29] 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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[26]On this, see Barnett (2004).

[27]Again, we thank a very thoughtful referee for raising this issue.

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

[29]G&V (2000, p. 22) similarly links the "classical perception of the macroeconomy and the Austrian view."

(1) $D_L = f(w_r)$; (2) $S_L = S_0$;[30] 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 wr $\neq$ wr*, then $U \neq U^*$; $U \gtrless U^*$. "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).[31] 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).[32]

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[30]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 $\approx$85 million or $\approx$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.

[31]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.

[32]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.[33] 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, $\%\Delta ARW_t = f(ARW_{t-1} - \overline{ARW})$,[34] [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') $\%\Delta ARW_t = \alpha + \beta (ARW_{t-1} - \overline{ARW}) + \varepsilon_i$.[35] 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

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[33]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.

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

[35]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 Wr, 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.[36] 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 $\overline{ARW} = (\Sigma ARW_t)/150$, 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 $\overline{ARW} \neq (\Sigma ARW_t)/150$, 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., $\overline{ARW} = (\Sigma ARW_t)/T$, t = 1 . . . 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

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[36]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) %ΔARW$_t$ = f(ARW$_{t-1}$ - $\overline{\text{ARW}}$),” where $\overline{\text{ARW}}$ is the “‘normal’ or ‘natural’ or ‘equilibrium’ value for ARW,” depends on the period for which $\overline{\text{ARW}}$ 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, $\overline{\text{ARW}}$, is suspect. Second, according to their model, if ARW ≠ $\overline{\text{ARW}}$ 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, $\overline{\text{ARW}}$, 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”;[37] (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) %ΔARW$_t$ = f(ARW$_{t-1}$ -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 $\overline{\text{ARW}}$.

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[37]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) $\%\Delta\ \hat{ARW}_t = -0.007 - 0.135(ARW_{t-1} - \overline{ARW})$.[38] 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) $\%\Delta ARW_x$ $= \%\Delta ARW_a + 0.135(ARW_{t-1} - \overline{ARW})$. (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,"[39] 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).[40]

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, $\%\Delta ARW_n$, $\%\Delta ARW_t$, and $\%\Delta ARW_a$ 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

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[38]That there is no standard symbol over $\%\Delta RW_t$ to indicate that it is an estimated value is surely an oversight, therefore we have inserted a caret or "hat" (^) where appropriate.

[39]Perhaps they meant simplistic?

[40]The specific mechanism is very unAustrian because of its deterministic nature.

of eq. (13), $-0.135(ARW_{t-1} - \overline{ARW})$ (their estimate of %ΔARW_t) for %ΔARW_n in eq. (14). Note that the right hand side of eq. (13) is actually $-0.007 - 0.135(ARW_{t-1} - \overline{ARW})$, but they drop the constant term -0.007 because it is not "significantly" different from zero.[41]

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

That is, in *computing* the value of %ΔARW_x as the difference between the actual and estimated values of %ΔARW_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 correllogram 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;[43] 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

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[41]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.

[42]"Using the calculated values of the exogenous component in the adjusted real wage" (G&V 2000, p. 25).

[43]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 %ΔARW$_x$, there is no reason to analyze it.

<div align="center">INCOMPATIBILITY WITH AUSTRIAN ECONOMICS</div>

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,[44] 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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[44]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[45] and equations,[46] 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.[47] 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.[48]

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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[45]And that in quite simple form which captures none of the complexity of the real economy that is so central to Austrian economics.

[46]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.

[47]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."

[48]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,[49] 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.[50] And, this holds in

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[49]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).

[50]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).[51] 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."[52] Moreover, it seems that these authors have spilled ink merely to say that Austrians prefer $w_r/AP_L \neq$ (1-a) to the classicals' $w_r/AP_L =$ (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) $\%\Delta \hat{ARW}t = -0.007 - 0.135(ARW_{t-1} - \overline{ARW})$.[53]

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

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[51]See footnote 17, above.

[52]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.

[53]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[54] 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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[54]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.[55]

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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[55]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.[56]

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, recoordinating 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:[57]

> 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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[56]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 nineth Austrian Scholars Conference: "[You] cannot have honest accounting if you have dishonest money."

[57]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.[58]

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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[58]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.[59]

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.[60] 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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[59]These are the words of our referee.

[60]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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