ARTICLES

GARRISON ON KEYNES

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ABSTRACT: This paper examines Roger W. Garrison's interpretation of John Maynard Keynes. Garrison has given economists a useful way to illustrate Keynes's theory, but there are two fundamental problems with Garrison's interpretation. First, the shape of the Hayekian triangle cannot be fixed in Keynes's theory. Second, Garrison's interpretation contradicts the IS-LM model. The demand constraint is derived from the IS-LM model and the IS-LM demand constraint is used to illustrate Keynes's theory.

KEYWORDS: John Maynard Keynes, Roger W. Garrison, IS-LM model, Hayekian triangle, capital-based macroeconomics, Keynesian demand constraint, socialization of investment

JEL CLASSIFICATION: E12, E22, E32, E43, E52, P20, B22

INTRODUCTION

Roger W. Garrison's capital-based framework is an outstanding contribution to macroeconomics. The capital-based framework illustrates the Austrian vision of sustainable and unsustainable growth. Furthermore, Garrison's framework can be used to compare the Austrian theory with the theory of John Maynard Keynes.

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Garrison compares the Austrian theory and Keynes's theory with the Hayekian triangle and the Keynesian demand constraint. Garrison has given economists a useful way to illustrate Keynes's theory, but there are two fundamental problems with Garrison's interpretation. This paper examines Garrison's interpretation of Keynes and suggests how Garrison's framework can be extended.

GARRISON'S INTERPRETATION OF KEYNES

According to Garrison, the shape of the Hayekian triangle is fixed and cannot change in Keynes's theory.[1] "The triangle can change in size but not in shape." (Garrison, 2001, p. 135). How does Garrison justify that the shape of the Hayekian triangle is fixed for Keynes? Garrison's justification is the last sentence in Chapter 4 of the *General Theory*: "if we can assume that, in a given environment, a given aggregate employment will be distributed in a unique way between different industries, so that N_r is a function of N, further simplifications are possible" (Keynes, 1936, p. 45). N_r is employment in a single firm, industry, or stage of production. N is employment in the entire economy. Therefore, Keynes makes employment in each stage of production a function of employment in the entire economy. To Garrison, this means that the shape of the Hayekian triangle is fixed: "The structure of capital was assumed fixed, the extent of its actual utilization changing in virtual lockstep with changes in the employment of labor" (Garrison, 2001, p. 18).

Garrison (2001, p. 136) developed the Keynesian demand constraint to show how consumption changes with investment in Keynes's theory. The Keynesian demand constraint shows that "Investment and consumption are positively related" (Meltzer, 1988, p. 153). Investment and consumption must move in the same direction. If investment increases, then consumption increases too. If investment falls, then consumption also falls. In figure 1, the Consumption-Investment curve (CI curve) is the demand

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[1] Garrison's linking of the Keynesian cross and Hayekian triangle is an important pedagogical innovation. Shackle only alludes to this connection: "If one draws a diagram of what Keynes says about capital in the *Treatise*, there will appear a Hayekian triangle of the stages of production" (Shackle, p. 516).

constraint. The CI curve illustrates the "positively sloped, linear relationship between investment and consumption" (Garrison, 2005, p. 510). The point where the CI curve intersects the production possibilities frontier is the point of full employment. There is unemployment if the economy is located inside the frontier. The demand constraint shows that the economy cannot move along the frontier: "The market economy in [Keynes's] view is incapable of trading off consumption against investment" (Garrison, 2005, p. 512).

Figure 1. The Labor-Based Framework
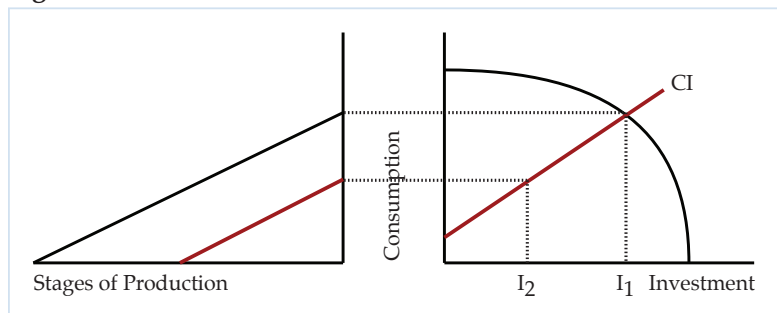
Keynes (1936, p. 143) argues that the business cycle is caused by fluctuations in the marginal efficiency of capital. Investment is unstable because of the uncertainty underlying investors' cash flow expectations. A sudden collapse of the marginal efficiency of capital starts the cycle. Figure 1 shows that investment falls from I_1 to I_2. Investment and consumption must move in the same direction, so the sudden collapse of investment means the amount of consumption also falls. The economy spirals downward along the CI curve. The Hayekian triangle depicts structural fixity. A collapse of the marginal efficiency of capital "reduces the triangle's size without changing its shape" (Garrison, 2005, p. 512). The Hayekian triangle shrinks, but the shape of the Hayekian triangle is constant. The fixed shape of the Hayekian triangle means that the slope of the Hayekian triangle's hypotenuse is fixed. The Hayekian triangle shows "the constant slope associated with Keynes's structure of industry" (Garrison, 2001, p. 135).

PROBLEMS WITH GARRISON'S INTERPRETATION

There are two fundamental problems with Garrison's interpretation of Keynes. First, the shape of the Hayekian triangle cannot be fixed in Keynes's theory. The slope of the Hayekian triangle's hypotenuse represents the price spread between the stages of production. In the Austrian theory the interest rate is the price spread between the stages of production: "The slope of the hypotenuse of the Hayekian triangle reflects the market-clearing rate of interest" (Garrison, 2001, p. 50). However, the interest rate is not the price spread between the stages of production in Keynes's theory. Keynes made "a rigid analytical distinction between the concepts of the MEC and the rate of interest" (Salerno, p. 44). Keynes accuses Mises and Hayek of "confusing the marginal efficiency of capital with the rate of interest" (Keynes, 1936, p. 193). The marginal efficiency of capital is the price spread between the stages of production in Keynes's theory. For Keynes, the slope of the Hayekian triangle's hypotenuse reflects the marginal efficiency of capital. The hypotenuse becomes flatter when the marginal efficiency of capital falls, and the hypotenuse becomes steeper when the marginal efficiency of capital rises. The marginal efficiency of capital is fixed if the slope of the Hayekian triangle's hypotenuse is fixed.

Garrison holds the shape of the Hayekian triangle constant because of Keynes's simplifying assumptions from the early chapters of the *General Theory*. Garrison argues that the shape of the Hayekian triangle is fixed because Keynes assumes that "income to all factors bears a constant ratio to income to labor…. non-labor income is constrained to move in proportion to labor income" (Garrison, 2001, p. 134). Keynes (1936, p. 55 n.2) does assume that "factor cost bears a constant ratio to wage cost" early in the *General Theory*. Still, it is important to consider whether Keynes maintains this assumption. After all, the justification for the fixed structure assumption is at the beginning of the *General Theory*, but Keynes's business cycle theory and main policy recommendation are at the end of the *General Theory*.

The fixed structure assumption is problematic because Keynes relaxes his simplifying assumptions about labor and factor costs later in the *General Theory*. Keynes only assumes that factor costs bear a constant ratio to wage costs while he is developing the building

blocks of his theory: "we shall assume that the money-wage and other factor costs are constant per unit of labour employed. But this simplification, with which we shall dispense later, is introduced solely to facilitate the exposition" (Keynes, 1936, p. 27). Many interpreters recognize that Keynes's theory does not depend on his early assumptions, including such diverse authors as Leijonhufvud (1968, p. 161), Patinkin (1976, pp. 101–102), Moggridge (1976, p. 92), Meltzer (1988, p. 164), and Davidson (2007, p. 182). Garrison acknowledges that Keynes does not maintain his early assumptions after Chapter 18: "Keynes presented his arguments on the assumption of fixed prices and wages, and then (after his stocktaking in Chapter 18) he offered qualification that derived from the fact that, to some extent, prices and wages can and do change" (Garrison 2001, p. 133). Keynes presents his business cycle theory in Chapter 22, so Keynes is not operating under the assumption of a fixed structure of production when he presents his business cycle theory. Also, the structure of production is not fixed when Keynes makes his main policy recommendation in Chapter 24. Keynes's theory of cyclical unemployment, theory of structural unemployment, and main policy recommendation do not depend on structural fixity.

The second fundamental problem with Garrison's interpretation is that it contradicts the IS-LM model. The IS-LM model is the standard interpretation of Keynes's theory. To Garrison, the IS-LM model describes "neither the actual workings of the economy nor Keynes's understanding of them" (Garrison, 2001, p. 125). Since the labor-based framework contradicts the IS-LM model, it is important to examine Keynes's role in the development of the IS-LM model and whether Keynes accepted the IS-LM model after the *General Theory* was published.

Keynes played a more significant role in the development of the IS-LM model than any other economist. Keynes created the first version of the IS-LM model: "a four-equation IS/LM model first appears in a lecture by Keynes in December 1933" (Dimand, 2010, p. 99). The mid-1934 draft of the *General Theory* has a similar version of the IS-LM model.[2] Keynes also collaborated with the authors of the

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[2] See Rymes (pp. 122–128) and Dimand (2007) for more on Keynes's 1933 version of the IS-LM model. See Keynes (1973a, pp. 424–456), Patinkin (1976, pp. 73–79), and Meltzer (1988, p. 143–144) for more on the mid-1934 draft of the *General Theory*.

earliest IS-LM papers. David Champernowne (1936) and W. Brian Reddaway (1936) published the first IS-LM papers. Keynes taught and tutored Champernowne and Reddaway at Cambridge, and both attended Keynes's 1933 lectures. Champernowne submitted his paper for publication before the *General Theory* was published. Champernowne admits that his IS-LM paper "was based on Keynes' lectures and supervisions" (quoted in Young, 1987, p. 83). Roy Harrod (1936) published the third IS-LM paper. Young (p. 87) shows that Harrod's version of IS-LM emerged out of a correspondence between Keynes and Harrod during the summer of 1935. Keynes was the first person to present a version of the IS-LM model and he was the key collaborator with the authors of the earliest IS-LM papers.

The *General Theory* does not include a formal version of IS-LM. However, all of the elements of the IS-LM model are in the *General Theory* and "an informal version of the model was there to be found" (Laidler, p. 4). According to Keynes (1936, pp. 246–247), the factors that determine income are the consumption function, the investment demand function, the money demand function, and the quantity of money. These are the factors underlying the IS-LM model. Keynes identifies the elements of the IS-LM model in the *General Theory*, "But Keynes never brought all the elements together" (Hansen, 1953, p. 147). Still, Keynes does suggest how the elements of the IS-LM model can be combined to determine income: "if we have all the facts before us, we shall have enough simultaneous equations to give us a determinate result" (Keynes, 1936, p. 299). Keynes argues that the saving function and investment demand function alone cannot determine the interest rate, "if, however, we introduce the state of liquidity-preference and the quantity of money and these between them tell us that the rate of interest is r2, then the whole position becomes determinate" (Keynes, 1936, p. 181). There is no formal version of the IS-LM model in the *General Theory*, but the IS-LM model can be derived from the *General Theory*.

Keynes accepted the IS-LM interpretations after the *General Theory* was published.[3] Keynes approved of Harrod's statement

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[3] Murray N. Rothbard, like Alvin Hansen, views the IS-LM model as the only possible correct interpretation of Keynes's theory: "That Keynes was a Keynesian—of that much derided Keynesian system provided by Hicks, Hansen, Samuelson,

of the IS-LM model: "I like your paper (may I keep the copy you have sent me?) more than I can say. I have found it instructive and illuminating, I really have no criticisms. I think that you have re-orientated the argument beautifully" (Keynes, 1973b, p. 84). After reading the IS-LM paper by James Meade (1937), Keynes told Meade "it was a true representation of the *General Theory*" (quoted in Young, 1987, p. 37). Garrison (1993) is aware that "Keynes himself … endorsed John R. Hicks's early interpretation of the *General Theory*". Finally, in the *Economic Journal* Keynes endorsed a presentation of the IS-LM model by Oskar Lange (1938): "The analysis which I gave in my *General Theory of Employment* is the same as the 'general theory' explained by Dr. Lange" (Keynes, 1973b, p. 232 n. 1). The claim that the IS-LM model is an incorrect interpretation of Keynes's theory is unjustifiable given the overwhelming evidence that Keynes accepted the IS-LM model after the *General Theory* was published.[4]

Keynes created the first version of the IS-LM model and Keynes endorsed the IS-LM interpretation after the *General Theory* was published. Why did Keynes leave a formal version of the IS-LM model out of the *General Theory*? Keynes did not include a formal version of the IS-LM model in the *General Theory* because "Keynes himself did not truly understand his own analysis" (Samuelson,

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and Modigliani—is the only explanation that makes any sense of Keynesian economics" (Rothbard, 1992, p. 196). Milton Friedman (1997) and Allan Meltzer (1988) use the IS-LM model to explain Keynes's theory because "Keynes accepted the IS-LM interpretation" (Meltzer, 1992, p. 160). Don Patinkin shows that Keynes expressed "consistent approval of the IS-LM interpretation of the *General Theory*" (Patinkin, 1990, p. 214). Even the Post Keynesian economist John E. King admits that "Keynes never once repudiated the IS-LM interpretation of the *General Theory*. On the contrary, he endorsed it warmly" (King, 2003, p. 31).

[4] Interpreters of Keynes who reject the IS-LM interpretation tend to overlook Keynes's endorsements of the early IS-LM papers by Champernowne, Reddaway, Harrod, and Meade. It is a myth that Hicks invented the IS-LM model: "Hicks's failure to acknowledge both Harrod's and Meade's papers in his own, gave the initial impression that he discovered the IS-LM approach independently and alone" (Young, 1987, p. 171). Hicks did not start writing his IS-LM paper until he had read Harrod's and Champernowne's IS-LM papers. Hicks (1937) never mentions Harrod's paper, but Hicks uses Harrod's equation system. Hicks's contribution was the IS-LM diagram. Keynes's endorsement of Lange's paper is especially important because it appeared in print and it appeared after Keynes's famous 1937 article in the *Quarterly Journal of Economics*. Lange (n. 1) acknowledges that his system of equations is similar to Reddaway's, Hicks's, and Harrod's.

1946, p. 188). More specifically, Keynes did not understand that the pure liquidity preference theory is flawed.

Keynes rejected the classical (and loanable funds) theory of the interest rate. Keynes (1936, pp. 180–181) argues that the classical theory cannot determine the interest rate: "Keynes attacked the classical theory of interest on the ground that it is indeterminate…. we cannot know what the rate of interest will be unless we already know the income level. And we cannot know the income level without already knowing the rate of interest" (Hansen, 1953, p. 140). The level of saving cannot be known until the level of income is known, but level of income cannot be known until the interest rate is known. For Keynesians the classical theory cannot determine the interest rate, but the classical theory can be used to derive the IS curve. "The one diagram that we do find in the *General Theory* (p. 180) is logically equivalent to the IS curve" (Patinkin, 1990, p. 224).

Keynes needed to introduce another interest rate theory because he rejected the classical theory. Keynes developed the liquidity preference theory of the interest rate. According to the pure liquidity preference theory, the interest rate is determined by the supply and demand for money. However, the supply and demand for money cannot determine the interest rate. Keynes (1936, p. 199) made the demand for money a function of income, and this left his liquidity preference theory indeterminate: "Keynes did not, however, see that his own interest theory was equally indeterminate" (Hansen, 1953, p. 147). The demand for money cannot be known until the level of income is known, but the level of income cannot be known until the interest rate is known. On Keynes's own grounds the pure liquidity preference theory is indeterminate. For Keynesians the liquidity preference theory can be used to derive the LM curve, but the liquidity preference theory cannot determine the interest rate. Keynes did not include a formal version of the IS-LM model in the *General Theory* because he did not realize that the pure liquidity preference theory is indeterminate.

Keynes expressed a purely monetary theory of the interest rate in the *General Theory*. He denied that saving and investment play any role in determining the interest rate: "Keynes gives the misleading impression that the demand for and supply of money determine the rate of interest independently of the saving and investment schedules" (Meltzer, 1988, p. 149). Keynes forgot

that he made money demand a function of income. He did not realize that the pure liquidity preference theory is indeterminate. "Keynes had failed in his attempt to fashion a 'purely monetary' theory of interest … he had been forced to recant his revolutionary creed" (Fletcher, 1987, p. 124). Keynes could recant by admitting that one of his elements was wrong, or Keynes could recant by reverting to the IS-LM model. Keynes recanted by returning to the IS-LM model: "by supporting Hicks's interpretation of his theory, Keynes went a good way back towards the Robertsonian view that productivity and thrift help determine the rate of interest" (Presley, 1979, pp. 185–186). By accepting the IS-LM model after the *General Theory* was published, Keynes admitted that saving and investment influence the interest rate.

The labor-based framework contradicts the IS-LM model.[5] The labor-based framework ignores how the interest rate changes when investment changes. For example, a collapse of investment reduces the interest rate in the IS-LM model. In contrast, a collapse of investment does not reduce the interest rate in the labor-based framework. For Garrison, after investment collapses "the old rate of interest still clears the market for loanable funds…. the rate of interest remains unchanged" (Garrison, 2001, pp. 146–147). This is a problematic. Keynes made the demand for money a function of income. Lower income must reduce the demand for money, and hence reduce the interest rate. Similarly, the labor-based framework ignores how the interest rate changes when saving changes. The interest rate falls when saving increases in the IS-LM interpretation, but the interest rate does not fall when saving increases in the labor-based framework. After saving increases "The initial interest rate is, once again, the market-clearing rate" (Garrison, 2001, p. 162). The labor-based framework is problematic because it contradicts the IS-LM model when saving or investment changes.

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[5] The labor-based framework does not contradict the IS-LM model if the economy is in a liquidity trap. In this sense, Garrison might be grouped with interpreters like Friedman who "put great emphasis on highly elastic liquidity preference" (Friedman, 1972, p. 928). However, the term 'liquidity trap' does not appear in *Time and Money* and Keynes (1936, p. 207) did not believe that the economy was usually in a liquidity trap.

CYCLICAL AND SECULAR UNEMPLOYMENT

Keynes accepted the IS-LM interpretation, so the demand constraint must be derived from the IS-LM model. "It is possible to derive the demand constraint for the IS-LM relationship by shifting the investment schedule and tracking the equilibrium values of investment and consumption" (Garrison, 1995, n. 2). The IS-LM demand constraint can be derived with equation 1 and equation 2.[6] In figure 2, the upward-sloping CI curve summarizes the relationship between consumption and investment. The CI curve is the IS-LM demand constraint.

$$(1) \quad Consumption = \frac{a(de+f)+b(cf+dM)}{f(1-b)+de}$$

$$(2) \quad Investment = \frac{(1-b)(cf+dM)-ade}{f(1-b)+de}$$

In terms of the IS-LM model, Keynes's main concern is autonomous investment (c). According to Keynes, "There is no reason to suppose that there is 'an invisible hand', an automatic control in the economic system which ensures of itself that the amount of active investment shall be continuously of the right proportion" (Keynes, 1982, pp. 386–387). Keynes believed that autonomous investment is unstable and chronically low: "The weakness of the inducement to invest has been at all times the key to the economic problem" (Keynes, 1936, pp. 347–348). A collapse of the marginal efficiency of capital means autonomous investment collapses. In figure 2, the amount of investment falls from I_1 to I_2. The amount of consumption also falls because "consumption and investment always move in the same direction" (Garrison, 1995, n. 2). The economy spirals downward along the CI curve.

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[6] Where a is autonomous consumption, b is the marginal propensity to consume, c is autonomous investment, d is the interest sensitivity of investment, e the is sensitivity of money demand to income, f is the sensitivity of money demand to the interest rate, and M is the real money supply. Keynes main concern was autonomous investment (c).

Figure 2. Cyclical and Secular Unemployment
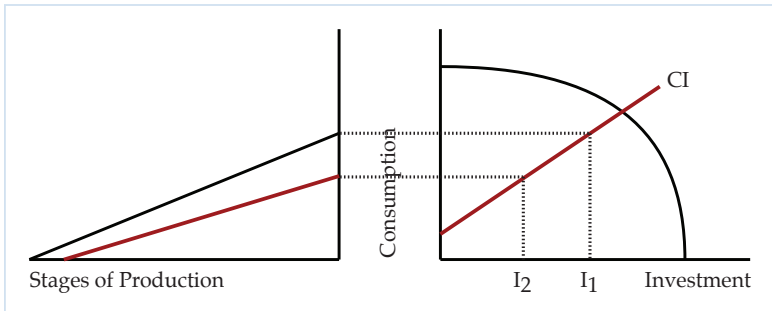
A sudden collapse of autonomous investment reduces the size of the Hayekian triangle. The horizontal leg and the vertical leg both shrink. In the labor-based framework the shape of the Hayekian triangle does not change, but it is necessary to eliminate the fixed structure assumption. For Keynes, the slope of the hypotenuse represents the marginal efficiency of capital. A collapse of the marginal efficiency of capital means there is a drop in the price spread between the stages of production. The Hayekian triangle changes in size and shape. The Hayekian triangle's hypotenuse becomes flatter.[7]

The business cycle is not the main focus of Keynes's theory. Keynes's primary concern is secular unemployment, not cyclical unemployment. "The central thesis of the *General Theory* is that a capitalist economy operating on the principles of laissez-faire fluc-tuates around a stable equilibrium at which there is less than full use of resources" (Meltzer, 1988, p. 123). Keynes's theory is a theory of chronic stagnation. To Keynes the free market economy operates "in a chronic condition of sub-normal activity" (Keynes, 1936, p. 249). According to Keynes, "We oscillate … round an intermediate

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[7] To Keynes, the consumer goods industries fluctuate more than the capital goods industries. This feature of Keynes's theory is inconsistent with the observation that the "capital-goods industries fluctuate more widely than do the consumer-goods industries" (Rothbard, 1963, p. 9). Robertson argued that "More pronounced cycles will take place in construction good industries with consumer good industries being less affected" (Presley, p. 19). Hansen recognized that "the most salient characteristic of cyclical movements of business is the fluctuation in the production of capital goods" (Hansen and Tout, p. 119).

position appreciably below full employment" (Keynes, 1936, p. 254). The economy is normally located inside the frontier because autonomous investment is chronically low. Keynes's business cycle theory is actually a corollary of the stagnation thesis. Figure 2 shows that "oscillations of the economy play themselves out inside the PPF" (Garrison, 2001, p. 177). When autonomous investment collapses, the economy spirals down from a point inside the frontier to a point even deeper inside the frontier. The free market economy fluctuates inside the frontier because the level of investment is unstable and chronically lower than full investment.

In Austrian terminology, Keynes's stagnation thesis means that the Hayekian triangle is chronically smaller than the social optimum. For Keynes, the utilization of the capital stock is normally suboptimal in a free market economy. The suboptimal size and shape of the Hayekian triangle represents the underutilization of society's productive capacity. The amount of goods flowing from the structure of production is persistently below the amount that society is capable of producing. The economy is capable of producing more goods, but labor and capital are underworked. The Hayekian triangle could be larger, but some of society's labor and capital are idle because investment is chronically low. To Keynes, the size and shape of the Hayekian triangle is chronically suboptimal in a free market economy.

KEYNES'S POLICY RECOMMENDATIONS

According to Keynes, the fundamental flaw with the free market economy is chronically low investment. Unemployment is chronically high because investment is chronically low. Therefore, Keynes's most important policy goal is increasing investment. "As for the preferred method of achieving full employment, Keynes consistently maintained his view of the 1930s that it was desirable to concentrate on the stimulation of investment" (Moggridge, 1976, p. 132). To Keynes there are two practicable ways the government can increase the amount of investment: "investment is stimulated either by a *raising* of the schedule of the marginal efficiency or by a *lowering* of the rate of interest" (Keynes, 1936, p. 193). In terms of the IS-LM model, government can increase the amount of investment by increasing autonomous investment or increasing the money supply.

Keynes's main policy recommendation is socializing investment. Keynes believed that socializing investment is the only way to achieve permanent full employment: "a somewhat comprehensive socialisation of investment will prove the *only* means of securing an approximation of full employment" (Keynes, 1936, p. 378).[8] Uncertainty about the future cash flows from investment projects causes chronically low autonomous investment, so "the duty of ordering the current volume of investment cannot safely be left in private hands" (Keynes, 1936, p. 320). By socializing investment, the government can push the economy up the demand constraint to the frontier. Furthermore, the government can ensure that the economy stays on the frontier. Keynes recommended a permanent program of managing investment to pin the economy to the frontier: "The object of Keynesian policy, of course, is to drive the economy to some point on the frontier and keep it there" (Garrison, 2001, p. 44). The government can make sure that the size and shape of the Hayekian triangle always corresponds to the social optimum. Socializing investment is the only way to guarantee that the amount of consumer goods flowing from the structure of production always equals the maximum amount that society is capable of producing.

Garrison (2001, p. 154) also uses the labor-based framework to explain Keynes's view of monetary policy. Monetary policy can increase the amount of investment in Keynes's theory: "An increase in the supply of money will necessarily raise total income … Admittedly it follows from this theory that you may be able to increase employment by direct inflation" (Hicks, 1937, pp. 150–151). Increasing the money supply increases the amount of investment by reducing the interest rate. In Figure 3, an increase in the money supply increases the amount of investment, from I_2 to I_3.

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[8] Keynes described himself as a liberal socialist: "By his own admission, Keynes lay at the 'liberal socialist' end of the broad spectrum of political and social thought that runs to Ludwig von Mises and Hayek and successors such as Milton Friedman at the other" (Moggridge, 1976, p. 38). According to O'Donnell (1999, p. 165), "It is *clear*, explicit and unambiguous; he used the term socialism to characterise his own views". For different views on Keynes's recommendation of socializing investment see Garrison (2001, p.180–187), Meltzer (1988, pp. 189–192), Rothbard (1992, p. 191–192), Salerno (1992, pp. 48–58), Patinkin (1976, p. 136–137), Tobin (1983, p. 8), Fletcher (pp. 173–178), and Davidson (2007, pp. 65–68). For more on Keynes's political philosophy, see O'Donnell, Brunner, Lambert, Dostaler, and Raico.

Figure 3. Monetary Policy in Keynes's Theory
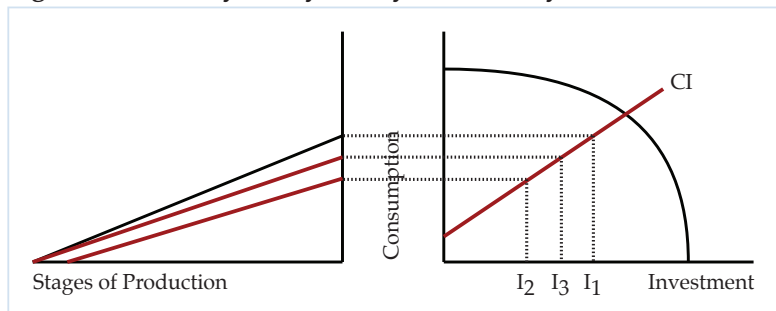
However, it is not possible to totally offset the collapse of the marginal efficiency of capital by reducing the interest rate: "fluctuations in the market estimation of the marginal efficiency of different types of capital … will be too great to be offset by any practicable changes in the rate of interest" (Keynes, 1936, p. 164). Increasing the money supply can push the economy up the demand constraint, but it cannot restore the amount of investment to its original level. Figure 3 shows that increasing the money supply raises the amount of investment from I_2 to I_3, but it does not raise the amount of investment back to I_1. For Keynes monetary policy plays a secondary role: "Full employment, then, in all likelihood, cannot be re-established by monetary policy alone…. monetary policy is the best solution to a secondary problem" (Garrison, 2001, pp. 154–155). Monetary policy has benefits, but monetary policy plays a secondary role for Keynes because it cannot totally counteract the business cycle.

Increasing the money supply causes the Hayekian triangle to grow in Keynes's conception of the Hayekian triangle. However, increasing the money supply does not change the shape of the Hayekian triangle. "Keynes distinguishes between the schedule of the marginal efficiency of capital and the prevailing rate of interest" (Meltzer, 1988, p. 128). The marginal efficiency of capital is completely determined by investors' cash flow expectations. Increasing the money supply does not change investors' cash flow expectations, so the slope of the hypotenuse does not change. Increasing the money supply cannot restore the Hayekian triangle to its original size and shape after a collapse of the marginal

efficiency of capital. The marginal efficiency of capital, not the fixed structure assumption, rules out the market mechanisms featured in the Austrian theory.[9]

More importantly, monetary policy cannot solve the problem of chronic stagnation. Keynes's primary policy objective, full investment, cannot be achieved with monetary policy alone. Increasing the money supply cannot solve the structural problem of chronically low autonomous investment: "no practicable reduction of the rate of interest would be great enough to encourage firms to increase their investments sufficient to generate full employment" (Patinkin, 1976, p. 137). Monetary policy can push the economy up the demand constraint, but monetary policy cannot push the economy to the frontier. Moreover, monetary policy cannot guarantee that the economy is permanently located on the frontier. Monetary policy can increase the size of the Hayekian triangle, but monetary policy cannot ensure that the size and shape of the Hayekian triangle always corresponds to the social optimum.

Keynes's key point is that the fundamental problem with the free market economy is chronically low autonomous investment. Monetary policy cannot increase autonomous investment, so monetary policy cannot solve the fundamental problem with the free market economy. According to Keynes (quoted in Meltzer, 1988, p. 131), "It is not quite correct that I attach primary importance to the rate of interest. What I attach primary importance to is the scale of investment and [I] am interested in the low interest rate as one of the elements furthering this. But I should regard state intervention to encourage investment as probably a more important factor than low rates of interest". Monetary policy can only increase the amount of investment indirectly by reducing the interest rate. Keynes's main policy recommendation is to directly increase autonomous investment. Full investment is Keynes's main

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[9] For the Austrians, an increase in the supply of loans by fractional reserve banks affects the shape of the Hayekian triangle. For Keynes, the shape of the Hayekian triangle does not change when fractional reserve banks increase the supply of loans. This feature of Keynes's theory is significant because it rules out the Austrian Business Cycle Theory. "The most important error Keynes commits is to consider investment determined by the marginal efficiency of capital" (Huerta de Soto, 1998, p. 555). See Fuller (2013) for more on the marginal efficiency of capital.

priority, and socializing investment is the only way to achieve full investment. Socializing investment is the only permanent solution to Keynes's stagnation thesis.

CONCLUSION

Roger W. Garrison has given economists a tremendously useful way to illustrate Keynes's theory. For illustrating Keynes's theory, the demand constraint diagram is superior to the Keynesian cross. The demand constraint diagram is simpler than the Keynesian cross. Unlike the Keynesian cross, the demand constraint diagram isolates investment. By isolating investment, the demand constraint diagram highlights the key issue of chronically low investment in Keynes's theory. However, Garrison's demand constraint is incomplete. The demand constraint must be derived from the IS-LM model because Keynes accepted the IS-LM interpretation of his theory. Following Garrison, the IS-LM demand constraint is an elegant way to illustrate Keynes's theory.

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AUSTRIAN BUSINESS CYCLE THEORY: EVIDENCE FROM KANSAS AGRICULTURE

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ABSTRACT: The popularity of the Austrian Business Cycle Theory (hereafter ABCT) continues to grow in both the popular press and the mainstream of the economics profession. That the ABCT is increasingly subjected to conventional empirical analysis is a testament to its intuitive appeal. In first-world economies, the agriculture sector is characterized by investment in expensive and highly-specialized equipment. While some agricultural products are "close to consumption," the network of highly specialized processing and transportation equipment necessary for the functioning of modern agriculture indicates that this sector is characterized by more roundabout production processes. Since the ABCT is primarily a theory of malinvestment in the more roundabout stages of production, analysis of the agricultural sector of the economy is relevant to the study of ABCT. This paper examines data for the production agriculture industry to determine whether business cycles in industry are consistent with the ABCT. Time series analysis using vector autoregression and other methods is conducted. Results are mixed, but strong arguments in favor of ABCT effects in agriculture are made.

KEYWORDS: agriculture, Austrian Business Cycle Theory, VAR

JEL CLASSIFICATION: Q14, E3, B53

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INTRODUCTION

Though it is considered a "heterodox" school of economics, Austrian Economics is one of the fastest growing schools. One of the best-known elements of Austrian theory is the Austrian Business Cycle Theory (hereafter ABCT). This theory has received increased attention (whether positive or negative) in the popular press (a Google search of "Austrian Business Cycle Theory" under the "News" tab returned 1,990 results on June 25, 2014) and in academic studies (Laidler, 2011; Bordo and Landon-Lane, 2013).

ABCT is a theory of malinvestment (De Soto, 2009, p. 375). The central proposition is that the market rate of interest is driven below the rate of time preference that prevails in society (Garrison, 2001, ch. 4). This is accomplished by an increase in the supply of money. The time preference that prevails in society is known as the "natural rate of interest" and was developed by Wicksell (Wicksell, 1962). Since interest rates are driven below equilibrium, the quantity demanded of loanable funds is now higher and the quantity supplied of loanable funds now lower than they otherwise would have been. Investment is now unsustainably higher than its equilibrium level. At the same time, consumption is higher because the incentive to save is lower. This constitutes the overinvestment portion of the theory. As to malinvestment, the increase in the money supply has so-called Cantillon effects on the economy (Garrison, 2001, ch. 4). The structure of capital in the economy is changed by the unsustainable increases in investment and consumption. This structure of capital can be conceived of as the various complementary relationships between various capital goods (Garrison, 2001, ch. 4; Lewin, 2011, p. 122). Investments are made which would not otherwise be made, since the costs of those investments are below equilibrium levels. Since the Austrian view conceives of the economy as a complex structure instead of a series of aggregates, the Cantillon effects are important. Some industries enjoy increases in output prices which are higher than others. These changes in relative prices result in unsustainable investment which affects some industries more than others. Overall, during the boom, investment and consumption are high. Prices in consumer goods industries and industries farthest removed from consumption in the structure of production are higher than prices in industries in the intermediate stages.

The onset of the bust comes when interest rates begin to increase and converge toward the natural rate. This condition is met either when the monetary authority sees fit to influence rates higher or when rates on the market begin to rise, as expectations of inflation are brought to bear (De Soto, 2009, p. 375). Investments are liquidated, but since some capital investments are, to varying degrees, specific to certain production processes, this liquidation can take time. Unemployment results, as skill sets are specific to certain production processes. To the extent that retooling and retraining are hindered or costly, the bust will persist. Output as a whole declines as liquid funds from divested capital are reorganized into productive investments. Once the structure of production is again consistent with resource availabilities and tastes and preferences, the economy can resume sustainable growth (Garrison, 2001, ch. 4). The recession of 1920/21 is often cited as a prime example of ABCT (Woods, 2009).

Though there are few studies claiming to test or illustrate ABCT via econometric means relative to other theories, such studies have largely found favorable results. Wainhouse (1984) used Granger causality tests on output data from 1959 to 1981 to determine whether a monetary origin of the business cycle existed. Results were generally favorable, suggesting that ABCT had empirically-demonstrated explanatory power.

Bismans and Mougeot (2009) used a panel regression approach with data from France, Germany, UK, and USA from 1980 to 2006 to determine whether effects consistent with ABCT could be found in the data. The study focused on changes in the term spread of interest rates (a proxy for the difference between natural and market interest rates) as a driver of changes in GDP. The authors did not explicitly account for changes in monetary policy, relying instead on Bernanke (1990) to indicate that monetary shocks explain 55 percent of the variation in the term spread.

Recent work in the econometric examination of ABCT (Keeler, 2001; Bismans and Mougeot, 2009; Mulligan, 2006) used observed changes in the term structure of interest rates as a proxy for changes in the difference between the Wicksellian natural and market interest rates. The use of the term structure as a proxy for this difference was criticized by Carilli and Dempster (2008). They suggested that the use of the term structure of interest rates

was based on the Expectations Theory of the term structure, which was suspect. Further, they suggested that a measure of the difference between the natural and market rates of interest should be independent of monetary policy actions. In the place of the term structure of interest rates, Carilli and Dempster (2008) used both the real growth rate of GDP and the ratio of savings to consumption as proxies for the natural rate of interest and the federal funds rate as the market rate.

The present work purports to test for ABCT effects using output data from the production agriculture industry (defined as the use of arable land to grow crops or to raise livestock) as a proxy for an early-stage industry. We defend the selection of production agriculture as an early stage industry on the basis that 1) it is a capital-intensive industry, 2) its assets are highly specialized, and 3) its products are relatively distant from final consumption.

Previous work has examined ABCT effects in early-stage industries. Mulligan (2002) examined early-stage industries from a capacity utilization standpoint and Young (2005) used employment statistics to test the Hayekian version of ABCT. This study differs in that it focuses on the net production of the agricultural sector. In this way, it is similar to other studies which focus on net aggregate production of final goods (Carilli and Dempster, 2008; Bismans and Mougeot, 2009). Thus, the use of output statistics in an early-stage industry is a contribution of this study to the existing literature.

DATA

To specify the variables used in this study, six data series were used. The time series data included information from 1973 to 2010. To approximate the changes in reserves resulting from monetary policy, annual data on money at zero maturity (MZM) was obtained from the St. Louis Federal Reserve FRED database. Money at zero maturity is defined as the M2 money supply less time deposits plus money market funds.

The gap between the natural rate and the market rate of interest (GAP) was also approximated with data from FRED. The market rate of interest is specified as the annual effective federal funds rate. Since, as Carilli and Dempster (2008) and Murphy (2003) indicate,

liquidity preference is a key determinant of interest rates, the present authors believe that the use of the real growth rate of GDP as a proxy for the natural rate of interest is suspect. Thus, following Carilli and Dempster (2008) and Rothbard (2001), we specify the ratio of savings to consumption as a proxy for the natural rate. To approximate output in Kansas agriculture (OUTPUT), annual data on net farm income and value of farm production (gross margin) were obtained from the Kansas Farm Management Association dataset. Output is specified as the ratio of net farm income to value of farm production. This is done to eliminate the effect of prices on output.

The authors use profit to measure *net* output. This ensures that the econometric analysis is focused on the contribution of this particular stage (production agriculture) to the total output of the economy. Due to the stage- and location-specificity of the data, the authors used value of farm production to net out the effects of changes in the value of the dollar rather than conventional price indices. Conventional price indices would not accurately account for changes in agricultural product prices since the specific types and quality of agricultural output has changed drastically over the period of the study. All data series from FRED were converted to real values using the chain type price index on personal consumption expenditures.

To determine whether each series is stationary, Augmented Dickey Fuller tests were conducted. The results can be found below in Table 1. All three series were nonstationary in levels, so it was necessary to difference them. The percentage change was calculated for MZM and OUTPUT. For GAP, the first difference was taken.

METHODS

To determine whether output statistics from production agriculture are consistent with the ABCT, the complex theory was distilled into two propositions: that changes in reserves impact the interest rate gap, and changes in the interest rate gap impact output of production agriculture. Recall that GAP is defined as the difference between the natural rate of interest and the federal funds rate. If, *ceteris paribus*, the federal funds rate is pushed down (pushed up), or if the natural rate rises (falls), GAP increases (decreases). Further, it was necessary to find an endogenous turning

point in the data where the interest rate gap indicates, first, a rise in output followed by a fall in output. This was done to differentiate between the claims of the ABCT and the claims of the Monetarists (namely that policymakers can influence output when inflation expectations are high). These two models are approximations of those used in Carilli and Dempster (2008).

To estimate the first model, a structural vector autoregression (SVAR) was estimated. The SVAR was used because it allows for relationships between contemporaneous values of the regressors whereas standard VAR analysis does not. This is a departure from Carilli and Dempster (2008). To determine the number of lags, the Akaike information criterion was used. The results are found in Table 2. A lag length of 3 was chosen based on this test.

To determine whether the causal relationships elucidated in the first model were a feature of the data, Granger causality tests were conducted. Granger causality is not a test of causation in the conventional sense; it merely shows whether or not there is significant evidence that lagged values of one variable improve the forecasts of another variable. Still, it is important in deciphering whether or not changes in MZM are leading indicators of changes in the interest rate gap and whether or not changes in the interest rate gap are leading indicators of changes in agricultural output.

There was not statistically significant evidence of a Granger-causal relationship between MZM and GAP (Table 3). That is, lags of MZM do not improve forecasts of GAP. However, there was a statistically significant relationship between changes in GAP and changes in OUTPUT. Lags of changes in the interest rate gap improved forecasts of changes in output. This result indicates that some statistically significant relationship exists between the interest rate gap (and therefore interest rate policy) and output in agriculture. Further tests are needed to explore this result in greater depth.

The next step in the analysis was to specify the coefficient matrix for the contemporaneous values of the regressors in the SVAR. To specify this matrix (Table 4), assumptions based on theory were necessary. Since there were three variables, it was necessary to specify three assumptions. For the equation with the percentage change in MZM as the left hand side variable, it was assumed that the other variables do not impact MZM in the current year. Since the Federal

Open Market Committee influences market rates via manipulation of bank reserves, it is unlikely that interest rates would impact reserves in the same period. Even if such effects exist, there are lags associated with monetary policy that would push these effects off to a later period. It is unlikely that production agriculture is large enough to have an impact on total reserves contemporaneously as well. Output in production agriculture may impact reserve levels if managers, overall, reduce or increase their debt loads in a relatively short period of time. However, this effect is likely to be delayed, since even short-term operating loans are secured before the production year. The third and final assumption was that output will not impact the interest rate gap in the same period. Market interest rates may be impacted if farmers change their debt loads, but again, this decision is made after that output is observed.

To further determine the impacts of changes in MZM on GAP and the impacts of changes in GAP on changes in OUTPUT, impulse response analysis and forecast error variance decompositions were estimated. This analysis will paint a more detailed picture of the relationships between these variables. The impulse response analysis (IRA) shows how an exogenous shock to one variable impacts other variables over time. This was important for determining whether the ABCT effects were features of the data. The forecast error variance decomposition (FEVD) gives the percentage of the forecast error variance of a given variable that is explained by exogenous shocks to all the variables over time. The results of this analysis will help to understand how much each variable was responsible for changes in the others from a forecasting standpoint.

The next element of the analysis was to estimate a polynomial distributed lag model. The purpose of this analysis was to determine whether or not an endogenous turning point exists in the data. That is, whether or not lags of GAP have a relationship to OUTPUT such that earlier lags were positively related and later lags were negatively related. The question being answered is whether or not the business cycle (in this case, increases followed by decreases in the output of a sector relatively distant from consumption) was a function of this gap. The polynomial distributed lag model estimated will be quadratic so as to capture the potentially-nonlinear relationship between GAP and OUTPUT.

Finally, the Diebold-Mariano (D-M) test was conducted. This test was designed to determine whether one of a pair of variables was better at forecasting a third. For the purpose of this study, the two predictor variables being compared were changes in MZM and changes in GAP. The findings will indicate to what degree the interest rate gap was necessary in the causal chain proposed above to predict OUTPUT.

RESULTS

Impulse Response and Forecast Error Variance Decomposition Analysis

To determine the relationships between MZM, GAP, and OUTPUT, impulse response analysis (IRA) was conducted on the SVAR coefficients (Table 5). Since GAP is the difference between the natural rate of interest and the federal funds rate, it should rise as MZM increases. The IRA (found in Table 6) displays some interesting results. An exogenous, one unit shock to the change in MZM results in a large increase in the change in the interest rate gap, as expected. This change eventually becomes negative at 4 steps ahead and returns to a positive (albeit small) value in period 7.

The initial positive effect of MZM on GAP which turns negative after 4 years indicates that changes in the money supply can only temporarily drive rates below their natural level. There is an endogenous turning point; an increase in the money supply will drive rates down in the near term, but rates must rise later because the pool of saved resources has not increased. This endogeneity differentiates ABCT from the claims of the Monetarists.

At 8 steps ahead, there is still a small, positive level effect on the change in the interest rate gap. In other words, a change in the money supply tends to drive a wedge between the natural rate and the market rate even after 8 years have passed. However, these results are suspect, as the Granger causality test found no evidence to support the notion that a change in MZM is a leading indicator of changes in the gap. The change in MZM also has an initially positive effect on the change in output. At 6 years ahead, this effect becomes negative and remains so through 8 years ahead.

The impulse response function analysis indicates that the ABCT effects on output may be a result of shocks to changes in MZM. The impacts of a shock to the change in the interest rate gap have very little effect at all on changes in output. It is necessary to be humble about all the results presented on the IRA because the confidence bands are extremely broad. This is likely a result of the small sample size.

The FEVD analysis (Table 7) further indicates that MZM is a relatively more powerful predictor of OUTPUT. Nearly all the variation in the forecast errors is a function of exogenous shocks to the change in MZM. That is, shocks to the change in the interest rate gap are not responsible for hardly any of the variation in the forecast errors for the change in output. This suggests that perhaps changes in MZM in this model have the most predictive power for the variables of interest. This is a somewhat strange result, as the Granger causality test for changes in the interest rate gap as a leading indicator of the changes in output was significant at the 10 percent level. More work is needed to decipher these seemingly conflicting results.

Polynomial Distributed Lag Function Analysis

The results of the polynomial distributed lag model (Almon, 1965) show, perhaps, the strongest evidence for ABCT effects in production agriculture. Lags of GAP are regressed on OUTPUT to determine whether effects predicted by ABCT exist. The model was estimated with a polynomial of degree two. According to the results (Table 8), the p-values on the linear and quadratic terms were both significant at the 5 percent level. The polynomial may be of a higher order, but it is at least quadratic.[1]

The lagged values exhibit features consistent with the ABCT and demonstrate the existence of an endogenous turning point. This endogenous turning point differentiates the Austrian theory from the Monetarist theory in that it demonstrates that interest rate manipulation creates mal-investments and overconsumption in the short run which must be liquidated and reduced in the long run.

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[1] The Durbin-Watson test indicates white-noise errors. This indicates that the lag length selection is not problematic.

(Carilli and Dempster, 2008). The five earliest lags have positive coefficients (though they are not statistically significant at the 10 percent level) and the final three lags have negative coefficients. This implies that an innovation in GAP, which occurs when the market rate of interest is driven below the natural rate, initially raises OUTPUT for four years, after which OUTPUT falls. This result, coupled with the Granger causality tests, indicates ABCT effects in the data which are distinct from effects consistent with Monetarist theory. Only two of the coefficients for this model are significantly different from zero statistically. Again, this may be a problem of a small sample size.

Diebold-Mariano Test

The Diebold-Mariano test (Table 9) for differences in the forecast errors of two models was also conducted. The first model is the change in MZM predicting the change in OUTPUT. The second is the change in GAP predicting the change in OUTPUT. The null hypothesis is that the expected value of the difference between the squared errors is zero. If the null hypothesis is rejected, it indicates that the forecasting ability of the two models are different. If we fail to reject the null, it indicates that the forecasting ability of the two models are not statistically different. If forecasts in the two models are not statistically different, it indicates that the interest rate gap may not be the conduit through which changes in the money supply affect agricultural output.

A squared loss function was used to compute z-scores to determine if there is a statistically significant difference between the forecasting power of the two models. Recursive, pseudo-out-of-sample forecasts were estimated for the models starting in 1988. Forecasts for 1, 2, and 3 steps ahead were calculated and a squared loss function was used. As the z scores indicate, the difference between the forecast errors is not significantly different from zero.

Since changes in MZM and changes in GAP are both equally good leading indicators of changes in OUTPUT, it may be that changes in output are not explained very well at all by either. This indicates that neither model is better than the other at predicting changes in output. These findings contradict the results of the FEVD analysis. However, it is important to note that this paper

makes use of annual data and that it may be difficult to distinguish statistically between innovations and, MZM and GAP.

CONCLUSION

The purpose of this study was to determine whether the observable data on the US monetary system and Kansas production agriculture are consistent with ABCT. The findings in this study are mixed. The Granger causality test and the polynomial distributed lag analysis indicate that changes in the interest rate gap are a good leading indicator of changes in agricultural output, and therefore that ABCT effects exist in the data.

Specifically, the results indicate that downside deviations in the interest rate gap have a nonlinear effect on output such that output is increased in the short run and decreases after a period of time. Since this nonlinear effect of the interest rate gap on output has an endogenous turning point, we suggest that this is evidence of the ABCT and not of Monetarist theories which do not predict an endogenous turning point. (Carilli and Dempster, 2008)

However, the IRA, FEVD, and D-M test analyses indicate that Federal Reserve policy is a better predictor of changes in agricultural output and that ABCT effects do not exist in the data according to the model presented. While monetary policy clearly has an effect on the interest rate gap, it is not clear based on the findings of these tests whether monetary policy affects the output of production agriculture through its effect on the interest rate gap. Additional research is needed to determine whether these results can be reconciled or whether more robust results can be found with similar data.

One of the primary difficulties with this analysis is determining whether MZM is a good indicator of reserves. Part of the problem here is that many Austrian business cycle theorists speak of the supply of money rather than reserves as the variable that is manipulated by the monetary authority. We have followed the method used by Carilli and Dempster (2008) in an earlier version of their paper. However, MZM was not used in the final version of their paper. More work is needed to determine the proper variable to specify the measure spoken of in the theory.

Another problem with this analysis is a lack of data. Future analysis will include finding a suitable proxy for production agricultural output to enhance the number of observations available. Another appropriate extension would be to use other specifications of the natural rate of interest.

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APPENDIX

Table 1. Augmented Dickey-Fuller Tests

	type	t-stat	5% t-crit
MZM	trend	-0.083	-3.588
GAP	trend	-2.239	-2.994
OUTPUT	trend	-2.067	-3.588

Table 2. Lag Length Selection

	AIC
0	4.728
1	4.283
2	4.371
3	4.138*
4	4.390

* Minimum AIC indicates appropriate lag length

Table 3. Granger Causality Tests

Lags of:	Improve forecasts of:	p-value
MZM	GAP	0.302
GAP	OUTPUT	0.053*

* indicates significance at the 10% level

Table 4. SVAR Matrix of Coefficients on Contemporaneous Values

	MZM	GAP	OUTPUT
MZM	1	0	0
GAP	22.321	1	0
OUTPUT	1.767	0.036	1

Table 5. SVAR Estimation Results

Equation: MZM	Lag	Coefficient	P-value
MZM	1	0.925	0.000***
GAP	1	0	0.93
OUTPUT	1	-0.03	0.000***
MZM	2	-0.433	0.057*
GAP	2	-0.007	0.084*
OUTPUT	2	0.027	0.010***
MZM	3	0.074	0.62
GAP	3	-0.002	0.568
OUTPUT	3	-0.021	0.032**
Constant		0.013	0.252

Equation: GAP	Lag	Coefficient	P-value
MZM	1	12.499	0.209
GAP	1	-0.232	0.286
OUTPUT	1	0.033	0.925
MZM	2	-5.391	0.618
GAP	2	0.097	0.63
OUTPUT	2	0.441	0.363
MZM	3	-5.028	0.495
GAP	3	-0.298	0.061*
OUTPUT	3	-0.162	0.718
Constant		-0.279	0.622

Equation: OUTPUT	Lag	Coefficient	P-value
MZM	1	5.215	0.313
GAP	1	0.102	0.367
OUTPUT	1	0.066	0.719
MZM	2	2.205	0.696
GAP	2	-0.164	0.127
OUTPUT	2	-0.053	0.832
MZM	3	6.951	0.079*
GAP	3	0.14	0.090*
OUTPUT	3	0.109	0.643
Constant		-5.82	0.058*

* Indicates significance at the 10% level
** Indicates significance at the 5% level
*** Indicates significance at the 1% level

Table 6. Impulse Response Analysis

Impulse MZM	Response GAP	OUTPUT
1	22.321	1.767
2	7.369	7.617
3	6.893	4.206
4	-13.728	11.653
5	-5.930	4.201
6	-4.472	-1.482
7	6.057	-5.884
8	3.325	-2.830

Impulse GAP		Response OUTPUT
1		0.036
2		0.105
3		-0.190
4		0.120
5		-0.116
6		-0.048
7		-0.077
8		0.017

Table 7. Forecast Error Variance Decomposition

MZM	MZM	GAP	OUTPUT
1	1	0	0
3	0.999	0.000	0.001
6	0.999	0.000	0.000
8	0.999	0.000	0.000
GAP	MZM	GAP	OUTPUT
1	0.998	0.002	0.000
3	0.998	0.002	0.000
6	0.998	0.002	0.000
8	0.998	0.002	0.000
OUTPUT	MZM	GAP	OUTPUT
1	0.757	0.000	0.243
3	0.986	0.001	0.013
6	0.995	0.000	0.005
8	0.996	0.000	0.004

Table 8. Polynomial Distributed Lag Function

	Degrees of Polynomial Coefficient	p-value
Intercept	0.014	0.943
Constant	-0.025	0.856
Linear term	-0.288	0.005***
Quadratic term	-0.190	0.032**

Lags of GAP	Lag Distribution Coefficient	p-value
0	0.039	0.616
1	0.078	0.197
2	0.095	0.111
3	0.090	0.164
4	0.064	0.345
5	0.016	0.808
6	-0.054	0.365
7	-0.145	0.010**
8	-0.258	0.000***

Dubin-Watson Test Statistic
D-W = 1.752
*** indicates significance at the 1% level
** indicates significance at the 5% level
Dependent variable: OUTPUT

Table 9. Diebold-Mariano Test

steps	z-score
1	0.214
2	0.213
3	0.213

THE PLUCKING MODEL, THE GREAT RECESSION, AND AUSTRIAN BUSINESS CYCLE THEORY

RYAN H. MURPHY

ABSTRACT: This brief note points out that Milton Friedman's "Plucking Model" has not held following the Great Recession. Friedman argued that the Plucking Model offered evidence against theories like Austrian Business Cycle theory; the bust was what needed explanation, not the boom. But as many economists have pointed out, the years leading up to the Great Recession fit many of the stylized predictions of the Austrian Business Cycle. Given their observations, it is of interest that the bust in recent years has not followed the Plucking Model.

KEYWORDS: Austrian Business Cycle, Plucking Model, Great Recession

JEL CLASSIFICATION: B53, E32

Milton Friedman's "Plucking Model" (Friedman, 1993) has been used to argue against the relevance of theories of the "boom" preceding economic downturns, such as Austrian Business Cycle Theory (ABCT). According to Friedman, output data show that economies follow a trend, with recessions being temporary setbacks prior to a return to a trend approaching the economy's maximum

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feasible output. Economies do not substantially go over trend during a boom; they collapse and then return to trend. Recessions "pluck" output downwards, but booms do not have similar effects in the opposite direction, as shown in Figure 1. Therefore, busts are what is to be explained, not the boom. While defenders of ABCT have objected to this interpretation (e.g. Garrison 1996), it remains an effective rhetorical point among macroeconomists.

Figure 1. Friedman's Plucking Model
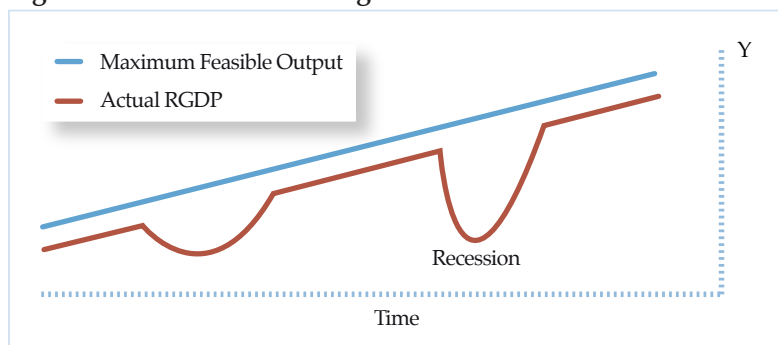
However, despite the unemployment rate in the United States falling to 5.8 percent as of October 2014 following the Great Recession, RGDP has not returned to its "potential," i.e., Friedman's maximum feasible output. This contradicts the historical record which Friedman highlighted as fatal for ABCT. Figure 2 provides actual and potential RGDP in the United States since 2002 (data from FRED). From 2008–2009, RGDP fell off significantly in comparison to its potential, and while it has grown since then, it did not snap back to potential as predicted by the plucking model.

Figure 2. Actual RGDP Versus Official Potential RGDP
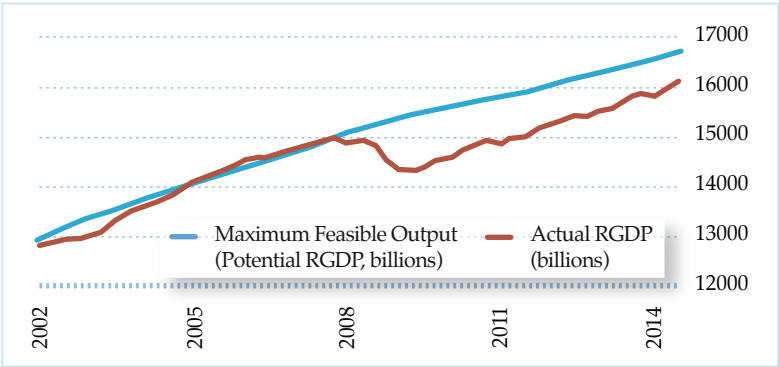
These data for potential RGDP assume that the economy was not overheating during the boom. If we grant the possibility that an ABCT boom was happening in the middle of the first decade of the millennium, then an altogether different picture emerges. Figure 3 compares actual RGDP against the assumptions that potential RGDP increased 1.5 percent, 1.75 percent, and 2 percent per year, starting in 2002. Under any of those assumptions (especially the middle case), the boom is clear and the modest growth rates we experience now are merely reflective of what is possible today given the US economy's fundamentals.

Figure 3. Actual RGDP Versus Alternative Potential RGDPs
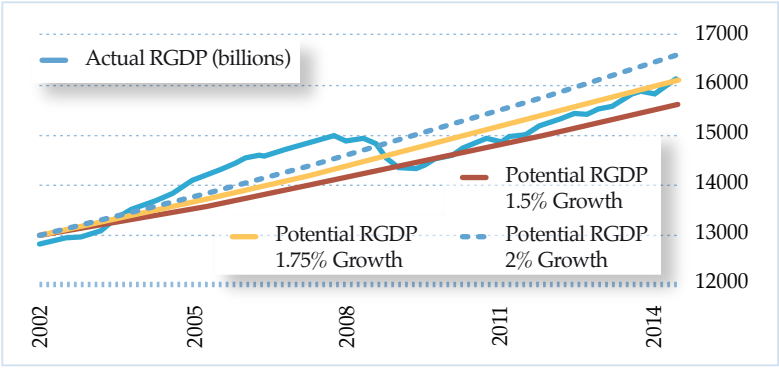
In the context of the plucking model, recent data do not reject ABCT. If anything, the data behave exactly as ABCT predicts—or at least how Friedman conceived of ABCT predicting. The reason for the importance of this point is the revival of interest in ABCT from mainstream economists, as documented by Cachanosky and Salter (2013). These mainstream economists, including Caballero (2010), Diamond and Rajan (2009), and Taylor (2009) each have made the argument that, of all recessions of recent memory, the Great Recession most clearly fit the stylized predictions of ABCT.

If these economists were correct, that this recession was a departure from the patterns of others, then it stands to reason that economies would cease behaving in accordance to the plucking model, which is precisely what has happened. This is not in any way the "Texas sharpshooter fallacy," where after the fact the "test" of a theory is defined so that the theory passes; the plucking model has been one of the primary rationales as to why ABCT was dismissed by macroeconomists.

It may be a coincidence that the first time in nearly a century mainstream economists recognized how well the stylized predictions of ABCT fit a recession was also the first time in recent memory that the plucking model failed to hold. Perhaps in the coming years the US will experience rapid growth, and the pluck of the Great Recession was just a very large, slow pluck. Or it is perhaps the case that the Great Recession is the clearest example of an Austrian Business Cycle we have on record, and the data bear that out.

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AUSTRIAN ENVIRONMENTAL ECONOMICS REDUX: A REPLY TO ART CARDEN AND WALTER BLOCK

EDWIN G. DOLAN

ABSTRACT: In March 2014, I presented a paper on Austrian Environmental Economics in Auburn, Alabama, as the F.A. Hayek Memorial Lecture at the Austrian Economics Research Conference, sponsored by the Ludwig von Mises Institute, which was subsequently published in the *Quarterly Journal of Austrian Economics* (Dolan 2014b), along with comments by Art Carden (2014) and Walter Block (2014). This short paper replies by emphasizing the importance of an institutional framework for environmental mass torts, without which a strict application of libertarian ethics leads to corner solutions in which there is a coordination failure.

KEYWORDS: Austrian school, economics, libertarianism, environmentalism, tradable emissions permits

JEL CLASSIFICATION: Q0, A11, A12, B25

INTRODUCTION

I thank the QJAE for the opportunity to reply to Walter Block's (2014) and Art Carden's (2014) comments. Carden's short

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comment, which is supportive of my position on many points and skeptical on others, contains a number of constructive suggestions (including suggestions for further reading) that I will take to heart. Block's comment is much longer and more polemical. Nevertheless, when considered substantively, with rhetorical flourishes left to one side, his remarks confirm the validity of several of my major points, as I will explain.

AUSTRIAN PRAXEOLOGY AND LIBERTARIANISM

Both Carden and Block take me to task, and rightly, for failing to distinguish carefully enough between praxeology, as the value-free methodology of Austrian economics, and libertarian ethics. It is a pity that neither of them was at the conference to make the point from the floor, in which case I could have addressed it on the spot. I do agree that paying more heed to the distinction between praxeology and ethics would strengthen my arguments, and I will try to correct that shortcoming in what follows.

However, while agreeing that praxeology and libertarian ethics are two different things—one a method, the other a system of values—I would like to point out that they are closely related. We can see the relationship with the help of a simple table:

	Statist values regarding ethics and policies	Libertarian values regarding ethics and policies
Neoclassical economic method	Abundantly attested	Significant minority
Austrian economic method	Empty	Universal

The point here is that whereas we can find mainstream practitioners of neoclassical economic methods who profess statist ethical values and support interventionist policies, and others who profess libertarian values and support free-market policies, the box that combines Austrian praxeology with statist ethics and policies is, for all practical purposes, empty.

That is no coincidence. In Chapter 6 of his book *Power and Market*, one of Block's heroes, Murray Rothbard (1962) undertakes what he

calls a "praxeological critique of ethics." He argues that although praxeological methodology itself is value free, it can be applied to refute the logical validity of any and all value systems that oppose free markets. That reasoning suggests that the box is empty because the notion of a statist praxeologist is a logical impossibility. Perhaps it is the absence of any non-libertarian variant of Austrian economics that makes it easy for Austrian writers—even Block himself—to fall into the habit of failing to note when they slip from a praxeological perspective to a libertarian one, or vice-versa.

Furthermore, when it comes to policy analysis, the relationship between praxeological and libertarian reasoning is more complex than Block makes it out to be. Speaking of policies that seek to improve environmental coordination through the better definition and enforcement of property rights, he writes,

> The dismal science *qua* dismal science is limited to exploring and explaining economic reality. Economists, but not in their official capacity, along with everyone else, may then use these findings to "resolve" things. But as value free social scientists, they are precluded from making public policy recommendations. There are libertarian policies: those compatible with the non-aggression principle and private property rights. But there are no, there can be no, Austrian policies. (p. 227)

In practice, though, when Austrian economists set out to analyze any policy proposal, whether it has to do with the environment, banking, antitrust, or anything else, they engage in a two-step process. One step is normative. It asks, "are the means and ends of the proposal consistent with libertarian ethical standards, such as nonaggression?" The other step is positive: it applies value-free praxeological reasoning to the question, "*if* this policy were implemented, *would it then* produce the results claimed by its proponents?" Since all Austrian economists are, in practice, also libertarians, the policy proposal will be rejected if it fails either test, or both of them. Only if it passes both tests is it not rejected.

Yes, I suppose we could say that there is some sense in which a declaration by an Austrian/libertarian economist that a certain policy passes both tests does not make it an "Austrian policy,"and a sense in which rejection of a policy because it fails one or both tests is not a "policy recommendation," but that would be splitting hairs. An ordinary person reading what self-professed Austrian

economists say in self-professed Austrian forums would see statements that could easily be taken as Austrian policy recommendations, whatever name we give them.

ENVIRONMENTAL MASS TORTS AND THE COORDINATION PROBLEM

As I note in my QJAE paper (Dolan 2014b) and at greater length elsewhere (Dolan 2014a), the Austrian approach characteristically views environmental issues as coordination problems. Following that reasoning, environmental problems arise when two or more parties have conflicting uses for a given resource. In an example I use, a hypothetical Vermont farmer, Nancy Norman, wants to use certain land for growing maple trees, while certain Midwest power plants want to use the airshed in which that land is located for disposal of gases and particles that are harmful to maple trees. The problem is how to coordinate these conflicting uses in a way that allows the realization of potentially mutually beneficial arrangements. For example, in return for certain payments or voluntary exchanges of property rights, the parties might agree to use the airshed exclusively for waste disposal, or exclusively for growing trees, or for waste disposal up to a certain threshold that they negotiate, based on their subjective valuations of the costs of pollution abatement and any resulting harm to the trees.

In cases where the parties are unable to achieve coordination, we can often trace the failure, in the words of Roy Cordato (2004), to "a lack of clearly defined or enforced property rights." If so, then one path to improving coordination would be to develop better rules for defining and enforcing said property rights.

And by the way, when writers like Cordato talk of property rights as the key to resolving the environmental coordination problem, I think they have something different in mind from what Block apparently does. Block says, "while property rights are indeed the key to resolving environmental problems, this is a basic element of libertarianism, a normative pursuit, not economics, a positive one." On the contrary, I construe Cordato's proposition as a positive one: *if* we improve the clarity and enforceability of property rights, *then* it will be possible better to coordinate the competing plans of the affected parties. On a normative plane,

I have no doubt that Cordato believes the clear definition and enforcement of property rights to be consistent with libertarian ethics, but the practical importance of what he has to say lies in his positive, not his normative analysis.

What would "clearly defined and enforced property rights" look like? Austrian writers have, from time to time, addressed this question. As an example, I use a set of normative legal principles for environmental property rights that is based on the principle of homesteading, advanced by Rothbard (1982) in a piece that is well known in Austrian circles.

When he comes to this point in my paper, Block correctly zeroes in on the following passage, which presents one of my central propositions:

> The property rights approach works best when the number of parties involved in environmental dispute are few and proximate. When they are many and remote, neither face-to-face bargaining nor common law litigation works well. Many of the most important environmental issues of our times fit this pattern, including urban smog, acid rain, ozone depletion, ocean acidification, and anthropogenic climate change. I will refer to this class of problems as *environmental mass torts*. (Dolan, 2014b, p. 204)

Block critiques my application of this proposition to the Nancy Norman example as follows:

> First, if the midwestern power plants polluted first, our girl Nancy is "coming to the nuisance." Thus, she should not win her case against them. But what is wrong with that?
>
> …Second, "Norman would have to prove actual damage. In any legal action, she would have to bear the cost of expert testimony regarding the science of acid rain, and would have to rebut defendants' testimony that some other agent, say a fungus, might be harming her maple trees. The testimony would have to establish her contentions beyond a reasonable doubt." But what, pray tell, is wrong with that? If I accuse Dolan of stealing my car, I would have to prove this claim before any court, even an extant one, would award me damages. And proving this might be expensive to me. But surely Dolan would not want the court to compel him to pay me under any other circumstances. (pp. 231, 232)

Unfortunately, Block muddies the water here by not following his own admonition to distinguish between the value-free Austrian method of praxeology and libertarian ethics. Let me spell it out.

If Block's "But what is wrong with that?" is construed as meaning "But what is wrong with that as a matter of libertarian ethics?" then the answer is "nothing." I agree that as a matter of ethics, or of normative jurisprudence, no court should find a person guilty of a crime or tort unless the accuser is able to produce evidence of guilt in accordance with the proper legal procedures. To use a noneconomic example, many outsiders who followed news accounts of the O. J. Simpson trial felt sure that the defendant had murdered his wife. Yet, as a matter of judicial ethics, we would not have wanted the judge in the case to conclude it by saying, "Despite the inability of incompetent prosecutors to present the evidence in a way that was convincing to the jury, I sentence the defendant to life in prison."

However, the essence of my argument about the property rights approach to environmental mass torts is not about ethics at all. Rather, it is a value-free if-then proposition that is fully consistent with Austrian praxeology: *If* there are many pollution victims and many sources of pollution and *if* property rights are defined and enforced as outlined by Rothbard, *then* the parties to the dispute will fail to coordinate their conflicting uses for the resource in question. "What is wrong here" is not that the outcome of the property rights approach is unjust, but that as a matter of positive economics, it offers no help in resolving the very coordination problem that it professes to address. The property rights approach fails not the first, but the second of the two tests that I outlined above.

Interestingly, Rothbard himself acknowledges that individual pollution victims would have little chance, under his proposed rules, of prevailing in a tort action against multiple, remote polluters. The reason is that whether there is a causal relationship between emissions and harm to the victim or not, the presence of other emitters or of natural sources is likely to mask the evidence of causation in a way that would make it impossible for the victim to prevail in court. Here is how he puts it in a passage that I quote in my paper:

> The prevalence of multiple sources of pollution emissions is a problem. How are we to blame emitter A if there are other emitters or if there are natural sources of emission? Whatever the answer, it must not come at the expense of throwing out proper standards of proof, and conferring unjust special privileges on plaintiffs and special burdens on defendants. (Rothbard, 1982, p. 87.)

Instead, he says, if the burden of proof is insurmountable, then pollution victims "must assent uncomplainingly."

TRADABLE EMISSION RIGHTS

Another issue that I address in my paper is the Austrian animus against tradable emission rights, or TERs as Block calls them. I maintain that there are no grounds in libertarian ethics for a blanket condemnation of TERs. Rather, I point out that as long as the rights being traded are legitimately held to begin with, the process of trading them is also legitimate. The example I use is an exchange on which trading takes place in rights to noise emission that have been legitimately established by Rothbard's homesteading method.

This proposition seems to throw Block aback. He cannot deny it, so he quickly shifts his target. Immediately after quoting my proposal for trading of homesteaded noise easements—rights that he concedes are legitimately owned by the seller, he writes "But this is not at all what tradable emissions is all about. Rather there is no recognition in mainstream depictions of this phenomenon." He then continues to criticize what he construes as a "mainstream" version of TERs that involves trading of rights that are not legitimately owned.

Block covers his shift of target by contending that "Dolan is extrapolating from a case where the rights to emit noise, or whatever, was licitly owned, to one where it most certainly is not," but that is simply not the case. The "extrapolation" is Block's own, not mine. He and I are in complete agreement that TERs cannot be ethically justified unless the rights in question are licitly owned.

I am not quite sure why Block opposes TERs so fervently, when logically, he should endorse them as applied to legitimately owned emission rights. Once TERs pass the first test, that is, once we establish legitimate ownership, then it is self-evident that they also pass the second. It is fully consistent with positive praxeology to say that voluntary trading of legitimately owned property will advance the cause of coordination (in my example, the coordination of the plans of the owners of the airport and the owners of surrounding homes). If it did not do so, then the parties in question would simply not trade.

Although Block does not say so, my own suspicion is that he may not believe that there are any cases—at least none that fit the profile of environmental mass torts—in which we can, in practice, identify a group of emitters who legitimately own the rights they claim and distinguish them from otherwise similar emitters who do not legitimately own their claimed rights. If there are such cases, I invite Block to specify them and then to explain why a TER approach would not be helpful in facilitating coordination for that case. If there are no such cases, then Block has provided a further validation of my contention that the property rights approach, while conceptually sound, is of little practical use in cases of environmental mass torts.

A PENCHANT FOR CORNER SOLUTIONS

Part of the value of comments is that they often help us to see things more clearly than we did at the outset. Block's comment has helped me in just that way with regard to a point that I did not focus on sufficiently in my original paper: The tendency of the Austrian approach to environmental economics to produce what economists call "corner solutions."

In the case of a coordination problem, a corner solution can be defined as one in which one party gets everything and the other gets nothing. Some situations, for example certain sports and games, have a zero-sum character that makes corner solutions inevitable, but we rarely encounter such situations in economics. If the participants in an economic situation start from a corner solution, they will almost always find it mutually beneficial to move away from the corner through some kind of voluntary exchange.

For example, suppose the legitimate owner of a parcel of land finds that it contains the only known deposit of a new and useful rare earth, "misesium." We would not expect the owner of the deposit to hoard it all, nor to sell the rights to a single user. Rather, we would expect the owner to initiate a process of trading, the likely outcome of which will be to bring quantities of the substance under the ownership of many parties, each of whom has his or her own ideas about how it can best be put to use.

When, following Cordato and others, we formulate environmental issues as problems of coordinating conflicting human purposes for

resources like air, land, and water, we naturally envision a process of free, mutually beneficial adjustments among the plans of numerous parties. In our airport case, that might, for example, involve purchase of land by the airport for a buffer zone; offers of monetary compensation to settle victims' noise complaints; payments by landowners and developers to induce the airport to invest in noise abatement; or even a purchase of the airport by a consortium of landowners with the intention of closing it. Even if no outsider can determine the optimal end point of the coordination process, we can say confidently that voluntary transactions such as those just listed would result in movements toward better coordination.

When the parties are few and proximate, for example, in conflicts over water rights or land use, we can find cases where the property rights approach does enable such compromises. However, in the case of environmental mass torts, property rights approach would, in practice, be more likely to produce all-or-nothing outcomes where one party gets everything and the other gets nothing.

In one kind of corner solution, the polluter gets everything and the victim gets nothing. Block's "What is wrong with that?" response to the plight of my hypothetical Vermont farmer is a case in point: The polluter emits at will and the farmer gets no relief at all. Rothbard appears to agree that this is a likely result when he tells us that, if the nature of an environmental mass tort makes it impossible for pollution victims to meet the required burden of proof, they must "assent uncomplainingly."

We less often encounter the opposite kind of corner solution, in which the pollution victim gets everything and the polluter nothing, but at the very end of his comment, Block helpfully provides an example: At one point in my paper, I raise the issue of climate change. I note that all too often, Austrian writers, when dealing with climate change, remove both their praxeological and libertarian hats in order to put on a third one, that of the amateur climatologist. Rather than do the hard slog of showing how market mechanisms and property rights could resolve the coordination issues raised by climate change, they take the easy route of saying we don't have to worry, because there is no such thing as anthropogenic climate change. In my paper, I admonish Austrians, instead, to approach the topic in the spirit of "What if Chicken Little is right this time?"

Block, to his credit, accepts the challenge, responding as follows:

> [I]f Chicken Little is right, and underarm deodorants, aerosols, refrigerants, etc. really cause global warming, which in turn leads to cancer and other dread diseases, then by gum and by golly, the libertarian would prohibit them at the point of a gun. Using these products would under these wild-eyed assumptions be akin to shooting howitzers up into the air, with no consideration of where they may land. But the point is, libertarians have already responded to this "spirit" called for by Dolan. And the answer is clear. Then, they would be NAP [non-aggression principle] violations. (p. 243)

And, if I may ask, what is wrong with that? Nothing, in terms of libertarian ethics. Everything in terms of coordination. To put it as a value-free, if-then statement, *if* greenhouse gases cause climate change, and *if* we prohibit all such emissions at the point of a gun, *then* there will be no coordination, no balancing of the conflicting interests of polluters and victims. But that result would be the opposite of a free-market solution. It would be literally a *market-free* solution, a solution in which there is no trading and no mutually beneficial adjustments of conflicting plans take place. It would be completely unlike the kind of coordination we get in a market economy when, say, one party wants to use tin to can salmon while another wants to use tin to solder electrical wires, and the prices of tin, salmon, and electrical devices all adjust to reflect the interplay of conflicting interests.

THE BOTTOM LINE

Let me close by again thanking Carden and Block for their helpful comments. Despite their differing styles of presentation, I find that both are generally supportive of my main conclusions:

- To date, the property rights approach, as expounded by writers who are both libertarians and Austrian-school economists, has produced some elegant conceptual analysis of environmental issues but little of practical value for facilitating the coordination in cases of environmental mass torts.

- The missing link is a practically workable institutional framework that would allow us to distinguish which claimed property rights are valid and which are not, and to enforce

those that are valid. Without such an institutional framework, strict application of libertarian ethics inevitably leads to corner solutions in which justice is served but coordination is not.

- If the institutional issues of legitimizing and enforcing property rights could be resolved, then the objection to mechanisms like TERs would vanish and we could begin to move away from the corners toward a genuine resolution of the pressing environmental problems of the day.

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THE MARGINAL EFFICIENCY OF CAPITAL: REJOINDER

EDWARD W. FULLER

ABSTRACT: This paper is a rejoinder to "The Marginal Efficiency of Capital: Comment" by Lucas M. Engelhardt. What Engelhardt calculates in his comment is not the Marginal Efficiency of Capital. Engelhardt incorrectly ranks investment projects by Present Value instead of Net Present Value. Engelhardt does not prove that Keynes has a wealth maximizing theory of investment, so his comment is not a successful defense of Keynes's theory.

KEYWORDS: John Maynard Keynes, marginal efficiency of capital, net present value

JEL CLASSIFICATION: E12, E22, E52, E58

INTRODUCTION

This paper is a rejoinder to "The Marginal Efficiency of Capital: Comment" by Lucas M. Engelhardt. Engelhardt attempts to show that Keynes's Marginal Efficiency of Capital (MEC) and the Net Present Value (NPV) always give identical rankings if factor prices are flexible. Engelhardt is unsuccessful. Therefore,

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Engelhardt does not prove that Keynes has a wealth maximizing theory of investment.

ENGELHARDT'S EXAMPLE

Engelhardt uses an example to illustrate that Keynes's MEC and the NPV always give identical rankings if factor prices are flexible. However, his example is problematic. What Engelhardt (p. 522) represents as the MEC is not Keynes's MEC. Engelhardt calculates something much different: "I assume that the project's startup cost is equal to the greater of the two present values. Then I calculate the interest rate that would be required to make the Net Present Value of each project zero" (Engelhardt, p. 521). This is not the definition of the MEC. Keynes's MEC is the "rate of discount which would make the present value… equal to *its* supply price" (Keynes, p. 135 emphasis added). The calculation of Keynes's MEC does not involve two investment projects. Keynes's MEC only involves one project, but Engelhardt's calculation involves two projects. Since Engelhardt does not use Keynes's MEC, he does not prove that the NPV and Keynes's MEC always give identical results if factor prices are flexible.

In table 1 of his comment, Engelhardt (p. 521) incorrectly ranks investment projects according to the Present Value (PV) instead of the Net Present Value (NPV): "we calculate the Present Values (not the Net Present Values)… Fuller's NPV was really just present value, but subtracting an arbitrary constant" (Engelhardt, p. 520). This is a fundamental error. Investors do not rank projects by PV. Investors rank projects by NPV. The NPV is the investor's entrepreneurial profit, and "business always tends to adopt those practices and that scale of activity which maximize profits" (Rothbard, p. 1118). According to Mises (1922, p. 119),

> the motive force of the whole process which gives rise to market prices for the factors of production is the ceaseless search on the part of the capitalists and the entrepreneurs to maximize their profits by serving the consumers' wishes…. It is only the prospect of profit which directs production into those channels in which the demands of the consumer are best satisfied at least cost.

Engelhardt's Internal Rate of Return (IRR) rankings are identical to his PV rankings. However, PV rankings are not wealth maximizing

rankings. Therefore, Engelhardt's IRR rankings are not wealth maximizing. Even if Keynes had used Engelhardt's IRR, his theory would still be flawed. Engelhardt's IRR is not a substitute for the NPV or Keynes's MEC. Engelhardt does not clarify the difference between the Austrian and Keynesian approaches to economic calculation because he does not use the NPV or Keynes's MEC.

CLARIFICATIONS

According to Engelhardt (p. 520), "the result that the two approaches result in different rankings will only hold if factor prices are held constant." This is not true, and Fuller (p. 393) does not assume that factor prices are constant to show that the MEC contradicts the NPV. Economic calculation must take place at a certain point in time. Balance sheets and NPV calculations "describe as well as possible the state of affairs at an arbitrarily chosen instant while life and action go on and do not stop" (Mises, 1949, p. 215). Prices are always changing, but the investor must use *current* market prices at the time he is ranking projects. Given factor prices at the time of economic calculation, Keynes's MEC contradicts the NPV when the interest rate is below the crossover rate. Factor prices can change in the future, but what matters is that the MEC contradicted the NPV at the time of the investor's economic calculation.

Engelhardt does not distinguish between independent projects and mutually exclusive projects. This is clear in table 3 of his comment. Engelhardt (p. 523) says, "if the interest rate is less than 23.38 percent, then both Project 1 and Project 2 are undertaken." Engelhardt does not realize that he is dealing with mutually exclusive projects. According to Mises (1949, p. 694), "there is the embarrassing multitude of producers' goods and the infinite variety of procedures that can be resorted to for manufacturing definite consumers' goods." Furthermore,

> it can very easily be decided which kind and what number of consumption goods should be produced. No one has ever denied that. But once this decision has been made, there still remains the problem of ascertaining how the existing means of production can be used most effectively to produce these goods in question. In order to solve this problem it is necessary that there should be economic calculation (Mises, 1922, p. 123)

The wooden bridge and the steel bridge are mutually exclusive projects. The investor-entrepreneur can only build one bridge. He cannot build both bridges in the same place. The investor in Fuller (p. 385) uses the NPV to decide which bridge to build. It is not a simple accept or reject decision as Engelhardt (p. 522) suggests. Engelhardt's example in table 3 does not prove that the MEC and NPV give identical results because Engelhardt does not distinguish between independent and mutually exclusive projects.

CONCLUSION

Fuller (p. 393) shows that Keynes has a flawed theory of investment. To Keynes, investment is the fundamental problem with the free market economy: "The weakness of the inducement to invest has been at all times the key to the economic problem" (Keynes, pp. 347–348). Keynes's theory of investment is absolutely essential to his critique of Say's Law and the free market economy. According to Alvin Hansen (p. 34),

> The slope of the consumption function… is indeed a necessary pillar for the overthrow of Say's law. But it is not sufficient. In addition, it must also be shown that there is no reason to suppose that the price system will operate in a manner so that investment outlays will automatically tend to fill the ever-widening gap, in absolute terms, between consumption and output.

Keynes's critique of the free market economy is problematic because Keynes's theory of investment is flawed. Moreover, since the MEC is not a wealth maximizing theory of investment, the MEC rules out the Austrian Business Cycle Theory.

One great advantage of Roger Garrison's capital-based framework is that it is a comparative framework. It can be used to compare the Austrian theory with Keynes's theory. However, there is no place for the individual entrepreneur conducting economic calculation in the capital-based framework. The NPV diagram injects the individual entrepreneur into the capital-based framework. The NPV diagram is also a comparative framework that can be used to compare the Austrian theory and Keynes's theory. The NPV diagram shows that "Keynes's internal rate of return [MEC] did not give an investment demand function according

to the maximum present wealth criterion of choice by investors" (Alchian, p. 941). The NPV diagram illustrates that Keynes's critique of the free market economy is problematic because Keynes had a flawed theory of investment. Engelhardt does not prove that Keynes has a wealth maximizing theory of investment. Engelhardt has not successfully defended Keynes's theory.

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THE MARGINAL EFFICIENCY OF CAPITAL: REPLY TO FULLER'S REJOINDER

LUCAS M ENGELHARDT

ABSTRACT: This is a brief reply to "The Marginal Efficiency of Capital: Rejoinder." I explain that I never intended to defend Keynes against Fuller's (2013) criticism. Rather, I intended to highlight that Keynes's conclusions rest on a key shortcoming in Keynes's theory: the assumption of sticky factor prices.

KEYWORDS: John Maynard Keynes, marginal efficiency of capital, net present value

JEL CLASSIFICATION: E12, E22, E52, E58

INTRODUCTION

I hesitate to write a reply to a rejoinder, as it seems likely that marginal utility in such discussions can very quickly diminish. However, after reading Fuller's response, I thought some clarifications of my own position were in order. Rather than defend Keynes against Fuller's (2013) criticism, my purpose was to emphasize the flawed assumption of sticky factor prices upon which Keynes's conclusions rest.

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MY EXAMPLE

In some sense, I agree with Fuller's rejoinder that my MEC is not Keynes's MEC—but for reasons different than Fuller's rejoinder suggests.

Fuller contends that the problem is that, unlike Keynes, I use the supply price of an alternative project to calculate the MEC of a specified project. This is not really a problem since I assume that the two projects "would utilize the same resources" (Engelhardt, 2014)—and so have the same supply price. (This is one way in which my example differed from Fuller's wood and steel bridges—which, presumably, use different resources.)

The way in which my MEC is different from Keynes's is that I assume that the supply price adjusts so that there's a "tendency for the net present value of an investment project to equal zero" (Fuller, 2013). So, it is perfectly accurate to say that my internal rate of return (IRR) is "incompatible with Keynes's [business cycle] theory." (Fuller, 2015) Actually, I think Fuller (2015) makes a very strong point that I neglected to notice. As Fuller says: "Keynes argues that the business cycle is caused by fluctuations of the MEC. However, Engelhardt's IRR cannot fluctuate." This is exactly right. By my assumptions, any collapse in expected cash flow will lead supply prices to fall proportionally so that the IRR is unaffected. This is not true in Keynes—and it is not true in Keynes precisely because Keynes assumes sticky factor prices. This is also why my comment should be seen as a critique—not a defense—of Keynes's theory.

CLARIFICATIONS

A more serious charge is that my comment "contains basic misunderstandings about economic calculation." In particular: because I rank projects by present value (PV) rather than net present value (NPV). I agree that, in general, this would be incorrect—however, my example assumes "that the two projects are competing ways of using the same set of resources" (Engelhardt, 2014)—and therefore have the same startup costs. So, the PV ranking and NPV ranking will be the same in this case. So, while NPV may, in general, be the

correct criteria, if we are dealing with a given set of resources, NPV rankings and PV rankings will give the same result.

I am also accused of not distinguishing between independent projects and mutually exclusive projects. To this I plead guilty—because in that regard, I was following Keynes's lead. Since I was intending to highlight Keynes's sticky price assumption, I left part of his theoretical apparatus in place. According to Keynes: "the actual rate of current investment will be pushed to the point where there is no longer any class of capital-asset of which the marginal efficiency exceeds the current rate of interest." (Keynes, 1936) This was intended to further highlight that "Austrian theory is primarily about *which* investment projects get chosen, while Keynesian theory is driven by the question of *how many* projects get chosen." (Engelhardt, 2014) I agree that, if we want to do theory well, ranking is the correct approach, since in reality—unlike in Keynes—resources are scarce.

CONCLUSION

The example I gave in my comment involved three key assumptions: (1) two projects that would both utilize the same resources, (2) multiple entrepreneurs considering each project, and (3) factor markets that adjust so that the net present value of the winning project is zero. Under these assumptions, MEC (or, alternatively, IRR) and NPV calculations give the same ranking—so Fuller (2015) is correct that "An investor can always get the correct answer by using the NPV, so Engelhardt's IRR is superfluous." In order for MEC and NPV rankings to differ we require, as in Fuller (2013), that prices do not fully reflect present values. Under those conditions, Fuller (2013) is correct. At the heart rests a bigger issue: whether ranking is relevant. In his use of the MEC, Keynes (1936) does not appear to believe that ranking matters—as investors do not choose between projects. They simply invest in a project or do not based on a comparison of the MEC with the going rate of interest. However, I agree with Fuller (2013, 2015) that, in reality, ranking is relevant—that is, which projects are chosen matter. Choosing is always a choosing between alternatives—and, at heart, Austrian business cycle theory is about when investors choose their projects poorly.

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BOOK REVIEW

THE ORIGINS, HISTORY, AND FUTURE OF THE FEDERAL RESERVE: A RETURN TO JEKYLL ISLAND

MICHAEL D. BORDO AND WILLIAM ROBERDS
CAMBRIDGE: CAMBRIDGE UNIVERSITY PRESS, 2013, 439 PP.

PATRICK NEWMAN

Recently the Federal Reserve reached its one hundred year anniversary. This milestone provided a nice occasion for economists to analyze the Fed's performance in the past century. As a result in the past several years there have been many conferences, special journal issues, and books that have centered on the Fed's centennial and its record. The present book is a collection of essays from a 2010 conference dedicated to that task. The conference marked the centennial of the famous Jekyll Island meeting (1910), a private gathering of U.S government officials and bankers dedicated to the formation of a central bank. It was significant

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because what came out it, the Aldrich Plan, eventually became the body of the Federal Reserve Act. The 2010 conference, which was held in the same location as the 1910 meeting, included many monetary heavyweights, such as Fed chairmen Ben S. Bernanke, Alan Greenspan, and Paul A. Volcker along with Michael D. Bordo, Charles W. Calomiris, Barry Eichengreen, Alan H. Meltzer, Ellis W. Tallman, David C. Wheelock, Eugene N. White, and more. Anyone interested in either monetary economics or macroeconomic history will undoubtedly have heard of these names, as they have been very influential in these fields.

The conference itself included three sessions of papers and a concluding panel. The first session focused on the background of the Federal Reserve and its initial impact. The second session focused on various parts of the Fed's track record in the past 100 years while the third focused on its future. The panel included a video presentation by Paul Volcker and a discussion between Alan Greenspan, Ben Bernanke, and Gerry Corrigan (former president of the Federal Reserve Banks of New York and Minneapolis). Given my own interest and relative (yet meager) expertise, I will concentrate this review on three papers presented in the first two sessions as well as the comments to the papers. The first, "'To Establish a More Effective Supervision of Banking': How the Birth of the Fed Altered Bank Supervision" is by Eugene White and is commented by Warren Weber, the second "The Promise and Performance of the Federal Reserve as Lender of Last Resort 1914–1933" by Michael Bordo and David Wheelock and commented by Ellis Tallman, and the third "Volatile Times and Persistent Conceptual Errors: U.S. Monetary Policy 1914–1951" by Charles Calomiris and commented by Allan Meltzer.

White's paper provides a useful economic history of the National Banking System and the early Federal Reserve System from the generally overlooked angle of bank regulation and supervision. He describes the pre-Fed regulatory apparatus, juxtaposes it with the initial Fed changes, and concludes with a brief analysis of the New Deal overhaul to the financial sector. The theses of the paper, as succinctly put by Weber, are that the National Banking System's flaws were not due to its regulatory framework and that the Federal Reserve System did not substantially improve it. The main defects of the pre-Fed era were the prohibition on branch banking and the

lack of a lender of last resort central bank (to mitigate the "inelasticity" of national bank notes). Although briefly mentioned by White (p. 32), I was glad to see Weber emphasize another important flaw of the National Banking System: the pyramiding of reserves among the different layers of banks.[1] The "pyramiding," which referred to the fact that many of the national banks could keep part of their legal reserves as interest earning deposits in other banks (particularly central reserve city banks in New York), allowed for a greater expansion of credit and an undue concentration of reserves in New York. The New York banks were often heavily invested in call loans in the stock market, so when other banks withdrew money from their New York balances it led to multiple contractions of deposits and could cause financial pressure on the stock market, which possibly led to (or exacerbated an already existing) panic.[2]

Closures of national banks caused by the panics and other events were surprisingly not very costly to the economy. In fact, the costs hold up very well compared to estimates of some of the notable disasters during the Federal Reserve era. White estimates that the total losses from national banks during the period 1865–1913 totaled roughly $44 million, or the equivalent of 0.3–0.6 percent of GDP. Compare this to the 2.4 percent of GDP lost during the 1929–33 contraction, the 3.4 percent of GDP during the Savings and Loan crisis in the 1980s, or the whopping 11.6 percent of GDP from the 2008–09 financial crisis. Interestingly enough, the losses of the National Banking System compare well to the losses of the "free banking era" (1838–60) which totaled 0.01 percent of GDP (p. 30). Given the even laxer regulation and greater approximation (though by no means perfect) of the banking system to a free market, this comparison suggests that an unregulated banking system can do well in minimizing depositor and shareholder losses.

This is not to say that White thought the panics that occurred during the period had no major economic consequences. On the contrary, White argues that they had severe macroeconomic effects and that the pre-Fed era had greater volatility in various economic aggregates and shorter expansions, although the lengths of recessions were

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[1] This point was also described in greater depth in the second essay by Bordo and Wheelock (pp. 64–66).

[2] For more, see Klein (1982, 180–182).

similar. Without getting into too much of a discussion on various modern GDP/Industrial production estimates of macroeconomic performance in the pre-Fed era, it can be argued that the evidence White provides is from too small a sample (the late 1880s onwards, which includes the particularly rough 1890s). A legitimate argument can be made that the macroeconomic volatility and frequency and duration of recessions in the entire pre-Fed era (e.g. after the Civil War) has been overstated, and that the Federal Reserve has not noticeably improved economic performance.[3]

The prohibition on branch banking caused by federal and state legislation led to an uneconomical amount of small single "unit" banks that were often undiversified in their loan portfolios and artificially propped up from an absence of competition, making them very susceptible to business failures and panics. This, along with the lack of a central bank, is what distinguished the U.S banking system from other European countries. White also mentions the Canadian banking system during this time, which allowed branch banking but did not have a central bank until 1935 and did not suffer from bank runs and panics. This leads White to cogently suggest that Congress' first reform should have been to allow for branch banking and then worry about the need for a lender of last resort. The comparison also suggests that perhaps a government-instituted lender of last resort was not needed at all.

After discussing some of the changes brought by the Federal Reserve with regard to moral hazard and regulation (such as the discount window), White concludes by describing the New Deal regulatory overhaul in the early 1930s. White argues that these changes were largely harmful, and "the New Deal swept aside [the] successful regime and imposed a radically different one that sharply increased moral hazard and risk taking" (p. 45). The reforms were driven mainly by small unit bankers and investment bankers and replaced the competitive market environment with a "loosely organized government cartel" that had many regulations on entry and pricing (p. 46). Many of these regulations gave quasi-monopoly grants to banks and hampered economic performance.

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[3] For more, see Selgin, Lastrapes, and White (2012). However also see Miron (2012) who challenges their conclusions.

White notes that it took decades for all of the consequences of the regulations to be shown, such as the moral hazard spurred on by the Federal Deposit Insurance Corporation (FDIC). White briefly describes the increased moral hazard brought by the FDIC through its termination of the "double liability" policy, an old National Banking System regulation that was imposed on shareholders in order to protect depositors (pp. 24–25). A more general and widespread inducement to moral hazard, however, was the main feature of the FDIC itself that insured depositors up to a certain amount against bank failure. With such a guarantee, depositors are less likely to monitor banks because they know the government will cover some of their deposits if the bank fails.

Bordo and Wheelock's paper also provides an analysis of the National Banking era but concentrates more on the performance of the Federal Reserve during its initial years (1914–33). They talk about the rationale behind the founding of the Fed, which was to serve as an effective lender of last resort to the banking system, and enumerate reasons why they were unable to do so as envisioned by the creators of the system, particularly during 1929–33. These include the conventional reasons such as the system's decentralized structure, its harmful allegiance to keeping the U.S dollar on the gold standard and its execution of the "Riefler-Burgess" doctrine but also its inability to recreate the essential features found in other European central banks and defects in the discount window apparatus (p. 83).

Bordo and Wheelock document the steps taken in the drive for a U.S central bank, such as describing the National Monetary Commission, the Warburg plan, the famous meeting at Jekyll Island, the Aldrich bill which was based off of that meeting, and the eventual Federal Reserve legislation. They properly state at the beginning that "the Federal Reserve Act of 1913 resembled the Aldrich bill in many respects" and later that "the act almost completely replicated the key monetary and international policy provisions of the Warburg plan and the Aldrich bill" (pp. 60, 71). However, they also write that the Federal Reserve Act and the Aldrich bill differed largely in terms of organizational structure. This seems to buttress the argument found in the introduction to the book by Michael Bordo and William Roberds:

> Over years, the clandestine nature of the meeting has often been criticized as allowing undue Wall Street influence over the founding of the U.S. central bank. However, the meeting itself was just one step in the process that led to the creation of the Federal Reserve, and many details of Aldrich's original design were changed in the legislation that was eventually passed (p. 2).

The purported "differences" in the central bank systems would seem to allow for a convenient agnosticism of the special interest banking involvement in the legislation. From a historical perspective as well as to answer the ever important question of "cui bono?" or "who benefits?" from a piece of legislation, what matters is that private bankers wanted to form a central bank to benefit themselves and that their main plan formed the basis for the Federal Reserve System (Rothbard, 1984, pp. 89–103).[4]

Bordo and Wheelock also take the standard interpretation of Federal Reserve monetary policy during the 1920s. They largely follow Friedman and Schwartz (1993) and say the Fed's performance largely avoided the problems found in the National Banking System and that economic activity and the price level were stable. Movements in Federal Reserve credit were a matter of seasonal accommodation and more or less automatic (p. 76). The interpretation provided by Friedman and Schwartz, as well as later monetary historians, argues that the Fed's policy can be seen as deflationary and neutral to the economy. This view might be misleading overall because it can be argued that the Fed's policy during this era should be viewed as inflationary and disruptive. Movements in member bank reserves, which were mostly responsible for the increase in the money supply, experienced three sharp jolts upwards in 1922, 1924, and 1927 and were caused by heavy purchases of government securities and acceptances from the Federal Reserve. Changes in Federal Reserve credit outstanding is an inaccurate measurement of Fed policy, because it includes the uncontrolled factor of bills repaid into the system.[5] One could also argue that the apparent stability of economic

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[4] For a study that argues that the organization drive for the Fed and its public benefits was made possible through additional private benefits (particularly international) concentrated to New York bankers, see Broz (1997).

[5] For more, see Rothbard (2008, pp. 85–167) and Newman (forthcoming).

indicators was merely an illusion and that the Fed's expansionary policies promoted an economic boom that eventually burst at the end of the decade.[6]

Another problematic interpretation of their 1920s analysis appears when they discuss the defects of the discount window. They say that one of its weaknesses was that member banks were reluctant to borrow (p. 84). Despite the Fed's attempts to dissuade banks from borrowing and "reminding" them that they were reluctant to lend, banks did often borrow continuously from the Federal Reserve. I was glad to see Tallman challenge them on this point and mention that member banks were able to effectively borrow from the Federal Reserve (pp. 104–105). Data found in White's paper also show that many banks in the 1920s did borrow for profit for significant periods of time (p. 43).

Calomiris also reviews the early years of the Federal Reserve (1914–51), but from a broader viewpoint and not specifically on the lender of last resort policies as described in the previous essay. The bulk of the paper looks at five key issues concerning the late 1920s and 1930s that monetary economists have analyzed in recent decades. They are: the Fed's involvement in the 1929 October stock market crash, the apparent conflict between remaining on the gold standard and expanding the money supply from 1929–33, Friedman and Schwartz' classification of banking panics, the liquidity trap question in the 1930s, and the connection between the Fed's increase in reserve requirements in the mid-1930s and the subsequent recession from 1937–38. While all of Calomiris' discussion is very insightful, I will concentrate on his points made in the first, third, and fifth topics of interest.

Calomiris notes that in the stock market boom and bust, stocks more than doubled in value amidst an explosion in technological innovation that occurred throughout the 1920s. Opinions on the cause of the rise in stock prices were mixed (including contemporary research) as some thought that the high prices were based off of expectations of past revenue growth and were sustainable, while others thought they were symptoms of an unsustainable bubble that was not grounded in fundamentals. The Fed initially tried to

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[6] On the "business cycle" consequences, see below.

deal with this through a policy of "moral suasion," or attempting to restrict loans to banks that would be made for speculative purposes while maintaining credit for legitimate activity. This policy was abandoned in favor of one of outright contraction at the end of the decade. Calomiris notes that there is evidence on both sides for whether a bubble in stocks existed, but seems to slightly hint that the growth in the market was sustainable (pp. 187–188, 203).

While understandably not being mentioned in the essay, Austrian business cycle theory (ABCT) sheds important light on the stock market bubble question in the late 1920s. In a nutshell, this theory says expansionary monetary policy by a central bank leads to a boom in intensive capital goods industries that is unsustainable and inevitably turns into a bust. The rapid expansion in the stock market was simply a reflection of the higher-order boom as stocks are titles to capital goods. And the growth in these capital goods industries was not based on fundamentals, as their profitability was artificially exaggerated by the increase in bank credit created by Fed policy. The October stock market crash and subsequent downturn was not caused by tight monetary policy in the late 1920s, but was a lagged adjustment to the downturn in those industries which began earlier in the middle of 1929.[7] And the downturn in those industries was caused by the end of the boom that the Fed engineered throughout the 1920s. Rather than pinpointing the Federal Reserve's mismanagement of monetary policy starting in the late 1920s, ABCT pushes the timeframe back to the early 1920s.[8]

With regard to the third issue, Calomiris challenges the view taken by Friedman and Schwartz (1993) that the bank failures which gripped the country in the early 1930s were nationwide panics driven mainly by illiquidity (positive equity but not having enough reserves to meet depositor withdrawals) and not insolvency problems (negative equity). According to Friedman and Schwartz, there were four major banking panics during this period: one in late 1930, two in 1931, and one from late 1932 to early 1933. Calomiris presents evidence which suggests that these panics were not purely exogenous instances of illiquidity issues and were more regional in

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[7] This is not to say that ABCT explains the severity of the 1929–33 downturn.

[8] For more on ABCT and its relationship to the 1920s, see Rothbard (2008).

nature (except for the last one). He notes that the bank failures were a continuation of previous bank failures in troubled agricultural regions in the prior decade, as roughly half of the 15,000 bank disappearances between 1920–33 came from before 1930, and there were high numbers of bank failures that occurred outside the panics (pp. 192–197). Perhaps the most succinct statement of the issue can be found earlier in the essay when Calomiris writes that "the high bank failures of the early 1930s are best seen as a continuation of the rural bank failures that had begun in the 1920s" (p. 180). Calomiris concludes by saying "it is probably not correct to argue, then, that the Fed failed to detect avoidable national liquidity crises and prevent waves of bank failures in 1930 and 1931" (p.196).[9] This insolvency analysis nicely fits in with what was argued by Vedder and Galloway (1997, pp. 112–127), who state that the rigid wage policy promoted by the Hoover administration led to a profit squeeze that reduced the value of bank portfolios and caused worried customers to withdrawal deposits.

Lastly, Calomiris argues that the Federal Reserve's increase in reserve requirements from 1936–37 did not have a contractionary effect on the economy and did not contribute to the recession of 1937–38, contrary to the views of Friedman and Schwartz (1993). Calomiris writes that one must look elsewhere for an explanation of the recession and suggests two explanations: the gold sterilization or contractionary fiscal policies taken at this time. Another explanation also deserves mentioning, namely the fact that the pro-union 1935 National Labor Relations Act (the "Wagner Act") was upheld as constitutional by the Supreme Court in 1937, leading to an enormous jump in wages that caused a rapid increase in unemployment and decline in business activity.[10]

Overall, despite some of my criticisms, the essays in the book are extremely well researched and important. They are all sources of a wealth of information about U.S economic history and they cite an impressive amount of both old and contemporary research

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[9] It must be stressed though that Calomiris did not think there were any illiquidity problems or that the Fed did the right policy in the early 1930s, on the contrary, he argues that the Fed should have vigorously expanded to prevent the money supply from failing.

[10] For more, see Salerno (2010, 437-38).

to buttress their arguments. They provide up to date commentary on key macroeconomic and monetary policy questions in contemporary research, and deserve to be read by anyone interested in these fields. Readers will find that the writers' arguments and the cited sources can be used for future research and will want to be well acquainted with the papers.

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BOOK REVIEW

SWEDEN AND THE REVIVAL OF THE CAPITALIST WELFARE STATE

ANDREAS BERGH
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PER L. BYLUND

*S*weden and the Revival of the Capitalist Welfare State is the third updated (and first English) edition of Andreas Bergh's well-researched book on the rise, fall, and return of the world-renowned Swedish welfare state. The work importantly traces the origins of the Swedish wonder, in which Sweden rose from being one of Europe's very poorest to being the world's fourth richest country in a period of 100 years. Bergh further discusses how this trend came to a halt and, in this third edition, analyzes how Sweden rediscovered its previous path and remained financially strong through the recent financial crisis.

The book consists of seven topically distinct chapters and an introduction. The first half of the book delineates the country's

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fascinating economic history over the past 150 years, from being a marginalized and backwards country in the outskirts of Europe and untouched by industrialization. Chapter two tells the story of how Sweden underwent rather radical free market reform that initiated and resulted in a full century of strong economic growth 1870-1970. These "golden years," as Bergh calls them, made Sweden prosperous in an unsurprising way: through the establishing of and continued respect for strong, market-supporting capitalist institutions.

Chapter three discusses how and why the until then highly successful "capitalist welfare state" stumbled for a quarter-century (1970–1995) as the state became progressively interventionist. Perhaps surprisingly, we learn that it was not failed welfare state policies that caused the country's decline, but failed macroeconomic such. They culminated in a severe economic crisis—Sweden's own depression—in the early 1990s, which necessitated a political change of course. Chapter four delineates the return of the capitalist welfare state on the Swedish political scene, and how it was revived through a series of extensive and politically significant reforms.

The second half of the book discusses the peculiar nature of Swedish politics and political economy. Chapter five targets the spirit of consensus that has characterized the country's political leadership under democracy; even right-wing and classical liberal parties have fundamentally been in support of the development of the welfare state and taken part in its expansion. It also introduces several areas of important recent reforms toward increasing competition within the realm of the welfare state.

Chapter six discusses the theoretical and systemic implications of these reforms, and focuses on how the Swedish welfare state exploits market mechanisms for efficiency in the system. Bergh here evaluates the consequences of recent deregulation, competitive procurement, the national school voucher system, and more. But whereas the welfare state has indeed seen reform such that "competition has increased on many markets and sectors, there is still a substantial degree of intervention in most markets." Bergh even notes that "it is more correct to speak of re-regulation rather than de-regulation" (p. 91).

The following chapter focuses on two politically and historically important areas that have remained unaffected by reform: labor market regulation and rent control. The discussion is an interesting exposition of core undertakings of the Swedish welfare state that provides an intriguing glimpse into the Swedish culture and mindset. Chapters six and seven jointly produce a balanced view of Sweden's political landscape and the extensive reform that caused the revival of the capitalist welfare state.

The historical review is well worth telling, as is the discussion on areas of reform *contra* sacred-cow issues in Swedish realpolitik. It properly serves as an illustrative example of what policies have worked, and what policies have not, along with plausible reasons for these outcomes. It is also interesting that Bergh doesn't shy away from discussing future scenarios and what to expect from each of them. This is done in chapter eight, which concludes the book with a discussion on the future of high-tax states such as Sweden and a sum-up of the welfare state's remaining challenges. While Sweden has overcome many problems and in a sense disproved pessimistic predictions about its unsustainability, it is wise to note that there are quite a few challenges ahead. Bergh prudently notes that "the welfare state is not yet off the hook" (p. 109).

Overall, the book is a great and informative read, and it provides a fact-based and balanced view of the world-renowned Swedish welfare state, its development and evolution. Yet while the vast amount of supportive data, presented throughout the book's chapters as well as in five appendices, is a major strength, it is also the book's main shortcoming: Bergh follows the data, but refrains from attempting explanations that do not follow directly from them. As a consequence, many a stone is left unturned.

To this reviewer, some interpretations of events seem a bit lacking and ahistorical. While the storyline appears consistent, it is told without much regard to the fact that the data may not tell the whole truth. The story would likely benefit from a deeper contextualization and a discussion attempting to make sense of the data from a more theoretical point of view.

For instance, Bergh offers no explanation or even context for the far-reaching free market reforms of the mid-19th century that put capitalist institutions in place and ensured a full century of

economic growth. As Bergh notes almost in passing, Sweden had at the time a vibrant and highly influential classical liberal movement. But classical liberalism and free market thinking was widespread in Sweden and the Swedish provinces much earlier (Norberg, 1998).

Consider, for instance, how the Swedish monarch Gustav III, strongly influenced by Voltaire and Enlightenment thinking, enacted wide-ranging economic and social reform a century earlier. At about the same time, in 1765, Anders Chydenius, a priest and Member of Parliament from the Swedish province Ostrobothnia (now Finland), published the highly influential free market treatise *Den Nationnale Winsten* (The National Gain).

Also, if history books are to be trusted, Sweden was not the normal agrarian country but has a rather unique history that may play into the free market reforms that begin Bergh's story. Swedish peasants were primarily freeholders and therefore always an independent force to reckon with in national politics. It is easy to imagine that the culture and political rule of a property-owning populace could turn out very differently than in societies where a large part of the population suffered serfdom.

How historical peculiarities such as these influenced the political climate, and whether this helps explain how Sweden was ripe for a free market revolution in the 1860s, is not entirely clear. But it is possible that they can help explain why, for instance, "the development towards an egalitarian distribution started over 200 years ago" (p. 16) rather than with the 20th century welfare state.

Historical explanations fall outside the scope of Bergh's analysis, but may still be important to gain a deeper understanding for the turn of events. It is also interesting how Bergh emphasizes domestic events to such extent that he largely refrains from including international economic and political influences to understand the small and export-dependent country's development.

A possible reason for this may be found in the author's statement that "Sweden's economic expansion [is] a case study illustrating the importance of institutions" (p. 8). Bergh wants to tell the institutional story, and this motivation permeates the book. There is undoubtedly good reason to focus on the institutions: they played a central role in Sweden's development, and willingness to keep

them intact has been core to Swedish politics for a long time. Yet pointing to institutions takes us only so far. While they may be a major *explanans* for economic development, the shift toward adopting capitalist institutions remains largely unexplained.

Even so, there is more to the story than productive institutions. That Sweden remained neutral and unharmed during two World Wars, both of which played out in Europe and had a severe effect on Sweden's neighboring countries, should have had a significant effect, at least temporarily, on the country's relative economic development. Indeed, whereas Europe's productive capacity was largely destroyed or directed toward military production, Sweden's productive apparatus was essentially unaffected by the wars and consequently well positioned to exploit post-war demand.

Naturally, this should have been a great boon for the country's development, and as should be expected Sweden's economy grew quite rapidly. This seems to corroborate Bergh's storyline, which holds the "golden years" of economic growth continued through 1970. But as the post-war position is taken into account, the causal link between market-supporting institutions and economic growth is no longer as obvious. Is it not possible that a country with comparatively poor institutions would have been able to grow its economy had it enjoyed Sweden's unique position after the wars?

The storyline is further muddled as we follow the story past 1970 and into the "not quite so golden" years 1970–1995. Bergh notes that "there is some agreement in academic and political debate that Sweden made a number of mistakes which led to an aggravation of problems" (p. 36). He continues to discuss how in the mid-1970s Sweden started taking drastic measures to stay afloat. "Problem-stricken industrial sectors" were heavily subsidized and the Swedish krona was repeatedly devalued "to keep the industry competitive" (p. 37). The Swedish government set a new course: "the macroeconomic strategy from 1976 onwards was to use devaluations to postpone dealing with the fundamental economic problems" (p. 39). The interventionist welfare state was born.

Yet it is difficult to see how this constitutes a real explanation of what caused the emergence of the interventionist welfare state in the 1970s and an abrupt end to the institutions of the "golden years." Industrial sectors do not turn "problem-stricken" overnight, and

even the 1973 international oil crisis (not in the book) would not have caused what Bergh refers to as "fundamental economic problems."

It may be possible to formulate an alternative explanation that places greater emphasis on the international events and the distortive effects of domestic progressive politics and incessant macroeconomic meddling. While the institutions of the capitalist welfare state may have been relatively unchanged, post-war Sweden's policies stand out as something of a socialist experiment. As Bergh notes (p. 26), "Sweden did not … phase out rent control after the Second World War," "[t]he capital market was regulated after the Second World War in order to be able to direct investments into socially prioritized areas," and "[f]rom 1955 the state directed company investments with the aim of stabilizing business cycles." And overall, "[a]fter the Second World War and up until the mid-1970s, Sweden pursued Keynesian stabilization policies" to "manage household demand" (p. 37).

Rather than focusing on the formal institutions, an alternative explanation could point to Sweden as a country that was exceptionally well positioned to ride the post-war boom from beginning to end. Domestic production for virtually insatiable export markets generated sufficient economic growth to mask and postpone the real economic effects of the highly distortive, interventionist policies. As the problems amassed and the post-war demand for Swedish exports petered out, time caught up with the experiment. At this point, the government responded with intervening with the institutional framework instead of (which would have been more prudent) rolling back the experiment.

If this is a plausible explanation, then the real "golden years" ended with the Second World War (if not sooner) and was followed by a period of economic growth and political interventionism. This doesn't appear to support the institutional story, and Bergh doesn't dwell on the post-war period but only notes that the core capitalist institutions were intact. He quickly moves on to the 1970s and the "not quite so golden" years, during which "the fundamental productivity and structural problems remained unsolved: too many actors in the Swedish economy essentially did the wrong things in the wrong way" (p. 37). We are not offered an explanation for the reason why "too many" busied themselves doing "the wrong things in the wrong way."

The macroeconomic policies and institutional interventionism in the 1970s were, as we might expect, utter failures. They ultimately led to Sweden suffering its own depression in the early 1990s, which prompted a brusque political awakening that brought about a new consensus around extensive reform as well as a return to respecting market principles.

The Swedish welfare state from 1995 has consequently retreated from interventionist policies to such degree that Bergh notes that "the Swedish model as it appeared in 1980 is probably dead" (p. 67). So great is the difference that "Sweden after the reforms is considered to be a competitive economy with a good business climate" (p. 64). This does not mean, however, that the Swedish state is small. No, "Sweden increased economic freedom without substantial reductions in size of government" (pp. 65-66); the changes constituted "liberalization without welfare state retrenchment" (p. 66).

Bergh truly does a great job at dispelling myths about the Swedish welfare state, its rise and presumed effects and achievements. He is perhaps too dedicated to the institutional explanation and a little too reluctant to speculate on possible explanations, but there should be no doubt that this book is a very nice contribution to our understanding of the reality of the welfare state in contrast to progressive mythology. The book is money well spent for anyone interested in contemporary politics and political economy.

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BOOK REVIEW

THE FORGOTTEN DEPRESSION

GEORGE BRAGUES

For as long as every living economist has been plying their trade, a single historical episode has been taken as an *experimentum crucis*. Latin for "crucial experiment", it is what Isaac Newton used to call an observed outcome significant enough, by itself, of determining the validity of a theory. The event serving this function in present-day economics is the Great Depression. And it was John Maynard Keynes and his followers that originally established that as the *experimentum crucis* by arguing that the Great Depression conclusively refuted the classical view that markets are self-correcting and that, therefore, the government has a necessary role to play in countering economic slumps through increased expenditures. Even the critics of the Keynesian school ended up accepting the 1930s as pivotal. Famously illustrating

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this was Milton Friedman with his thesis that blame for the Great Depression ought to be laid at the Federal Reserve for running an overly tight monetary policy. Not just in the U.S., but throughout the developed economies, both these interpretations of the 1930s, traditional Keynesian and monetarist, have come to undergird public policy amidst the various economic stresses that have engulfed the globe since the financial tsunami of 2008. Central banks the world over have resorted to the monetary tap known as quantitative easing. Governments have bolstered their social insurance regimes and poured money into public works.

In his latest book, *The Forgotten Depression*, James Grant proposes that another moment in economic history be treated as an *experimentum crucis*. The former *Barron's* columnist, current editor of an influential financial markets newsletter, and regular media commentator, points us instead to the downturn of 1920–1921. That was the last time, according to Grant, that the U.S. government did not prescribe the now standard cure for economic slumps consisting of fiscal stimulus and easy money. Grant's purpose is to test the efficacy of this medicine by checking what happened when it was not administered. If its absence did not give rise to a prolonged sickness, then one must conclude that state intervention is not required treatment for a fall in economic activity. Indeed, one might then legitimately suspect that the government is worsening matters by hindering forces operating in the market naturally tending towards recovery. This is precisely the conclusion that Grant draws from the 1920–1921 experience—and he reaches it both engagingly and convincingly.

Now anyone looking to use that occasion as an instructive case study is immediately faced with the problem of delineating the extent of the decline. In the early 1920s, the U.S. government had not yet erected a huge statistics collection apparatus with a view to managing the economy. Few doubt the pronouncement of the National Bureau of Economic Research, the authority on business cycles dates, that the decline in economic activity began in January 1920 and subsequently lasted 18 months before bottoming in July 1921 (NBER, 2015). However, gross measures of economic performance, whether GDP or its predecessor GNP, were not calculated at the time. The US Federal Reserve had only recently begun to estimate the nearest equivalent to this, the industrial

production index. That fell by 31.5 percent during the 1920–1921 slump. While less devastating than the 51.7 percent drop from 1929–1933 in the throes of the Great Depression, the 1920–1921 period represents the third biggest decrease since the Fed started publishing the statistic in 1919 (Federal Reserve, 2015). Grant is well aware that Christina Romer (1994), as part of her over-all contention that pre-World War II business cycles were both shorter and less volatile than commonly thought, has put forward estimates indicating that the output loss during 1920–1921 was 6.6 percent, short of the 10 percent threshold informally used by economists to classify a given decline in production as a depression. Not being solely absorbed with macroeconomic aggregates, Grant counters this more modest assessment by detailing the various pieces of the economy. He tells us, for example, that automobile production fell by 23 percent, hourly manufacturing wages by 22 percent, and agricultural income by a whopping 56.7 percent, at the same time that the number of bankruptcies tripled with the debt associating with these quintupling. In this way, Grant substantiates that the 1920–1921 downturn was severe enough to offer a revealing empirical trial of the thesis that government is needed to resuscitate a slowing economy.

As with every slowdown in the industrial era, the lead up to 1920–1921 was an unsustainable boom. And as with every such boom, an overabundance of money fueled the ephemeral rise in fortunes. As Grant recounts the story, World War I had just ended when fear of economic collapse gripped observers pointing to the consequences of military production suddenly being wound down. Defying these predictions, consumer demand, long pent-up by the conflict, surged with the return of the soldiers from the European battlefield. To finance the war, however, the Woodrow Wilson administration had enlisted America's newly established central bank to augment the money supply. With the U.S. still nominally on the gold standard, the Fed anticipated that this liquidity injection could be quickly mopped up once hostilities ceased, given that the monetary base was mostly made up of short-term debt instruments that were self-liquidating. But government officials, particularly at the Treasury Department, were in no mood to put the post-war expansion in jeopardy and the Fed, barely five years into its existence, lacked the institutional clout to resist the

politicians. Thus, it accommodated the swelling demand, leaving consumers flush with money to spend on goods, which led to a general rise in prices. Not until January 1920 did the Fed summon the will to tighten monetary policy, raising its benchmark interest rate by 1.25 percent to 6 percent. The New York Fed, in a move followed by most other regional branches, then raised it one more time to 7 percent in June 1920.

Several factors combined to preclude any fiscal or monetary response to the ensuing tumble in the economy. Though the White House was occupied by a progressive enthusiast of government activism, Woodrow Wilson was fixated on securing entry of the U.S. into the League of Nations. Cementing what Grant calls an accidental policy of laissez-faire from the executive branch was the stroke that the President suffered in late 1919. In Congress, meanwhile, the dominant factions in both political parties saw the government as having no proper role in steering the economy. When Warren G. Harding subsequently assumed the Presidency in March 1921, both he and his Budget Director, Charles Dawes, brought a more deliberate policy of laissez-faire into the White House, cutting government spending and defeating a campaign in the Senate to offer bonuses to World War I veterans. The economics profession had not yet started advising the political classes to stabilize prices; such arguments were still incubating in the writings of Irving Fisher (1922). Very critical, too, in Grant's telling is that the two most powerful figures within the Fed, W.P.G. Harding, chair of the board, and Benjamin Strong, governor of the New York Fed, both stood against a loosening of monetary policy that would compromise the necessary liquidation of ill-judged investments made during the boom. In this cause, they had to deal with John Skelton Williams, the Comptroller of the Currency, who waged a strident battle for lower interest rates. Such were the emotions that Williams' crusade evoked that W.P.G. Harding once lunged at him during a hearing of the Joint Agricultural Commission.

While this bureaucratic fracas continued, prices fell. Nowadays, such a deflationary outcome is widely viewed with utter horror; it is precisely that which the Great Depression has taught policy-makers to avoid at all costs lest the economy go into a downward spiral. Lower prices are thought to be dangerous because investors and consumers are apt to wait until they get lower still, thereby

lowering demand for goods and services. This, in turn, is said to cause firms to lay off workers in an effort to cut costs, the resulting increase in unemployment prompting demand to drop further such that prices fall again to reinforce the caution among investors and consumers. On the contrary, Grant observes that prices did not keep plummeting into infinite depths, but eventually found a base once consumers saw there were good deals to be had in the stores and investors spotted the prospect of higher returns on capital projects thanks to lower costs for wages and materials. Left alone, the price system worked to restore the economy back to health by mid-1921. The foundations were thus set for a robust expansion that marked the rest of the decade in the U.S., an era that made for a telling contrast to the economic stagnation that went on to plague Britain, where prices, especially that for labor, had become sticky with the rise of unionism. As Grant encapsulates his account of 1920–1921: "the hero of my narrative is the price mechanism, Adam Smith's invisible hand" (p. 2).

For those who rather put their faith in the visible hand of the state, two lines of attack are open against Grant. One of them is to reject the causal framework of his story. This argument holds that market forces were actually not allowed free sway, that the government was a significant player, and that its central bank arm both caused and ended the 1920–1921 downturn, first by tightening monetary policy in 1920 and then by easing in 1921 (Economist, 2014). Yet Grant does not deny that the government instigated the slump. It did so, however, by fomenting the prior boom with artificially low interest rates. Unfortunately, he puts less emphasis on the malinvestments than he does on the infla-tionary dynamic this policy created, but Grant is right to insist that the Fed cannot be faulted for tightening monetary conditions in the face of escalating prices. Such a move, after all, is part of the currently accepted playbook for monetary policy. It does become thornier to disentangle cause from effect with respect to the ending of the 1920–1921 downturn, inasmuch as the New York Fed began to lower its benchmark rate in May 1921, two months before the economy hit bottom. Admittedly, this is an intriguing coincidence, but it is universally acknowledged that monetary policy involves a lag between its implementation and its impact on the economy. This lag is typically estimated to be around a year, not two months.

With respect to the second line of possible attack, Grant is more vulnerable. At a conference not too long ago, I recall a central banker urging a version of this by first conceding that the market can be relied upon to cure itself of a slowdown through a downward adjustment in prices. But, he added, there is inevitably going to be much suffering along the way; many will be forced into unemployment. Better, he said, for the central bank to intervene with monetary stimulus so as to smooth the necessary adjustment in the economy while minimizing the blow on people's livelihoods. In other words, doing nothing is cruel. Grant provides fodder for this charge by observing that the unemployment rate, though no one can be sure exactly how much it increased, reached double digit levels at the nadir of the 1920–1921 decline.

His answer to the cruelty charge, though, is that any attempt to cushion the required correction in the economy will only serve to prolong the malaise. He points out that is what happened with both Herbert Hoover's efforts to avert a decline in wages at the onset of the Great Depression as well as the larger economic rescue operation launched by Franklin D. Roosevelt's New Deal. Grant calls our attention as well to the anemic recovery that has followed the trio of deficit spending, zero interest rates, and quantitative easing adopted to combat the Great Recession of 2008–2009. Still, this is to suggest a trade-off between the duration of pain and its magnitude. When the business cycle turns negative, it seems, we must either choose between a quick, but more painful resolution to the imbalances generated by the preceding boom or a lengthier but less painful experience. If that is the case, then Grant needs to demonstrate why the first option is superior, which would necessarily entail grappling with the sorts of value judgments that are the province of moral and political philosophy. If that trade-off is more apparent than real, then he has to show that the state's endeavor to ameliorate the pain of a downturn will not just delay the recovery, but ultimately come to nothing.

No incident from the economic past can really be treated as an *experimentum crucis*. When it comes to human affairs, any particular sequence of events one happens to isolate will inevitably embody a unique configuration that renders it impossible to draw lessons applicable to every other analogous circumstance. An economic theory can only be as empirically good as the range

of historical situations it can explain. While highly illuminating, 1920–1921 cannot serve as the final word in the contest between the laissez-faire and interventionist approaches to the fluctuations of economic life. But neither can the 1930s as conventionally understood. In this enlarging of historical perspective lies the chief benefit of Grant's book.

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