picture, even to the extent of assuming away accumulated stocks of capital goods, and therefore of any age structure of distribution of such goods.[6] Since production is magically "synchronized," there is then no necessity for land and labor to receive any advances from capitalists. As Schumpeter writes:

> There is no necessity [for workers or landowners] to apply for any "advances" of present consumption goods. . . . The individual need not look beyond the current period. . . . The mechanism of the economic process sees to it that he also provides for the future at the same time. . . . Hence every question of the accumulation of such stocks [of consumer goods to pay laborers] disappears.

From this bizarre set of assumptions, "it follows," notes Schumpeter, "that everywhere, even in a trading economy, produced means of production are nothing but transitory items. Nowhere do we find a stock of them fulfilling any functions." In denying, further, that there is any "accumulated stock of consumer goods" ready to pay laborers and landowners, Schumpeter is also denying the patent fact that wages and rents are always paid out of the accumulated savings of capitalists, savings which could have been spent on consumer goods but which laborers and landowners will instead spend with their current incomes.

How can Schumpeter come to this conclusion? One reason is that when workers and landowners exchange their services for present money, he denies that these involve "advances" of consumer goods, because "It is simply a matter of exchange, and not of credit

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one that Schumpeter took from John Bates Clark, which in turn led to the famous battle in the 1930s between the Clark-Knight concept of capital and the Austrian views of Hayek, Machlup, and Boulding. See ibid., p. 6*n*. Also see F.A. Hayek, "The Mythology of Capital," in Fellner and Haley, eds., *Readings*, pp. 355–83.

[6]In Khan's words, for Schumpeter "capital cannot have any age structure and perishes in the very process of its function of having command over the means of production" (Khan, *Schumpeter's Theory*, p. 48). Schumpeter achieves this feat by sundering capital completely from its embodiment in capital goods, and limiting the concept to only a money fund used to purchase those goods. For Schumpeter, then, capital (like interest) becomes a purely monetary phenomenon, not rooted in real goods or real transactions. See Schumpeter, *Economic Development*, pp. 116–17.

transactions. The element of time plays no part." What Schumpeter overlooks here is the profound Böhm-Bawerkian insight that the time market is not merely the *credit* market. For when workers and landowners earn money now for products that will only reap a return to capitalists in the future, they *are* receiving advances on production paid for out of capitalist saving, advances for which they in effect pay the capitalists a discount in the form of an interest return.[7]

In most conceptions of final equilibrium, net savings are zero, but interest is high enough to induce gross saving by capitalists to just replace capital equipment. But in Schumpeter's equilibriums interest is zero, and this means that *gross* saving is zero as well. There appear to be neither an incentive for capitalists to maintain their capital equipment in Schumpeterian equilibrium nor the means for them to do so. The Schumpeterian equilibrium is therefore internally inconsistent and cannot be maintained.[8]

Lionel Robbins puts the case in his usual pellucid prose:

> If there were no yield to the use of capital . . . there would be no reason to refrain from consuming it. If produced means of production are not productive of a net product, why devote resources to maintaining them when these resources might be devoted to providing present enjoyment? One would not have one's cake rather than eat it, if there were no gain to be derived from having it. It is, in short, *an* interest rate, which, other things being given, keeps the stationary state—the rate at which it does not pay to turn income into capital or capital into income. If interest were to disappear the stationary state would cease to be stationary. Schumpeter can argue that no accumulation will be made once stationary equilibrium has been attained. But he is not entitled to argue

[7]See Schumpeter, *Economic Development*, pp. 43–44.

[8]Clemence and Doody attempt to refute this charge, but do so by assuming a zero rate of time-preference. Capitalists would then be interested in maximizing their utility returns over time without regard for when they would be reaped. Hence, capital goods would be maintained indefinitely. But for those who believe that everyone has a positive rate of time-preference, and hence positively discounts future returns, a zero rate of return would quickly cause the depletion of capital and certainly the collapse of stationary equilibrium. Richard V. Clemence and Francis S. Doody, *The Schumpeterian System* (Cambridge, Mass.: Addison-Wesley, 1950), pp. 28–30.

> that there will be no *de*cumulation unless he admits the existence of interest.[9] (emphasis added)

To return to Schumpeter's main problem, if the economy begins in a Walrasian general equilibrium modified by a zero rate of interest, how can any economic change, and specifically how can economic development, take place? In the Austrian-Böhm-Bawerkian view, economic development takes place through greater investment in more roundabout processes of production, and that investment is the result of greater net savings brought about by a general fall in rates of time-preference. Upon such a fall, people are more willing to abstain from consumption and to save a greater proportion of their incomes, and thereby invest in more capital and longer processes of production. In the Walrasian schema, change can only occur through alterations in tastes, techniques, or resources. A change in time-preference would qualify as a very important aspect of a change in consumer "tastes" or values.

But for Schumpeter, *there is no* time-preference, and no savings in equilibrium. Consumer tastes are therefore irrelevant to increasing investment, and besides there are *no savings* or interest income out of which such investment can take place. A change in tastes or time-preferences cannot be an engine for economic change, and neither can investment in change emerge out of savings, profit, or interest.

As for consumer values or tastes apart from time-preference, Schumpeter was convinced that consumers were passive creatures and he could not envision them as active agents for economic change.[10] And even if consumer tastes change actively, how can a

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[9]Emphasis added. In the excellent critique of Schumpeter's zero-interest equilibrium by Lionel Robbins, "On a Certain Ambiguity in the Conception of Stationary Equilibrium," *Economic Journal* 40 (June 1930): 211–14. Also see Gottfried Haberler, "Schumpeter's Theory of Interest," *Review of Economics and Statistics* (May 1951): 122ff.

[10]Thus, Schumpeter wrote: "It is not the large mass of consumers which induces production. On the contrary, the crowd *is mastered and led by the key personalities in production*" (italics are Schumpeter's) in "Die neuere Wirtschaftstheorie in den Vereinigten Staaten" ("Recent economic theory in the United States") *Schmollers Jahrbuch* (1910), cited in Schneider, *Joseph A. Schumpeter*, p. 13.

mere shift of demand from one product to another bring about economic development?

Resources for Schumpeter are in no better shape as engines of economic development than are tastes. In the first place, the supplies of land and labor never change very rapidly over time, and furthermore they cannot account for the necessary investment that spurs and embodies economic growth.

With tastes and resources disposed of, there is only one logically possible instrument of change or development left in Schumpeter's equilibrium system: technique. "Innovation" (a change in embodied technical knowledge or production functions) is for Schumpeter the only logically possible avenue of economic development. To admire Schumpeter, as many economists have done, for his alleged realistic insight into economic history in seeing technological innovation as the source of development and the business cycle, is to miss the point entirely. For this conclusion is not an empirical insight on Schumpeter's part; it is logically the only way that he can escape from the Walrasian (or neo-Walrasian) box of his own making; it is the only way for any economic change to take place in his system.

But if innovation is the only way out of the Schumpeterian box, how is this innovation to be financed? For there are no savings, no profits, and no interest returns in Schumpeterian equilibrium. Schumpeter is stuck: for there is no way within his own system for innovation to be financed, and therefore for the economy to get out of his own particularly restrictive variant of the Walrasian box. Hence, Schumpeter has to invent a *deus ex machina*, an exogenous variable from outside his system that will lift the economy out of the box and serve as the only possible engine of economic change. And that *deus ex machina* is inflationary bank credit. Banks must be postulated that expand the money supply through fractional reserve credit, and furthermore, that lend that new money exclusively to innovators—to new entrepreneurs who are willing and able to invest in new techniques, new processes, new industries. But they cannot do so because, by definition, there are no savings available for them to invest or borrow.

Hence, the conclusion that innovation is the instrument of economic change and development, and that the innovations are financed by inflationary bank credit, is *not* a perceptive empirical generalization discovered by Joseph Schumpeter. It is not an empirical

generalization at all; indeed it has *no* genuine referent to reality. Suggestive though his conclusion may seem, it is solely the logical result of Schumpeter's fallacious assumptions and his closed system, and the only logical way of breaking out of his Walrasian box.

One sees, too, why for Schumpeter the entrepreneur is always a disturber of the peace, a disruptive force *away* from equilibrium, whereas in the Austrian tradition of Mises and Kirzner, the entrepreneur harmoniously adjusts the economy in the direction of equilibrium. For in the Austrian view the entrepreneur is the main bearer of uncertainty in the real world, and successful entrepreneurs reap profits by bringing resources, costs, and prices further in the direction of equilibrium. But Schumpeter starts, not in the real world, but in the never-never land of general equilibrium which he insists is the fundamental reality. But in the equilibrium world of stasis and certainty there are no entrepreneurs and no profit. The *only* role for entrepreneurship, by logical deduction, is to innovate, to disrupt a preexisting equilibrium. The entrepreneur cannot adjust, because everything has already been adjusted. In a world of certainty, there is no room for the entrepreneur; only inflationary bank credit and innovation enable him to exist. His only prescribed role, therefore, is to be disruptive and innovative.

The entrepreneur, then, pays interest to the banks, interest for Schumpeter being a strictly monetary phenomenon. But where does the entrepreneur-innovator get the money to pay interest? Out of profits, profits that he will reap when the fruits of his innovation reach the market, and the new processes or products reap revenue from the consumers. Profits, therefore, are *only* the consequence of successful innovation, and interest is only a payment to inflationary banks out of profit.

Inflationary bank credit means, of course, a rise in prices, and also a redirection of resources toward the investment in innovation. Prices rise, followed by increases in the prices of factors, such as wages and land rents. Schumpeter has managed, though not very convincingly, to break out of the Walrasian box. But he has not finished his problem. For it is not enough for him to break out of his box; he must also get back in. As a dedicated Walrasian, he must return the economy to *another* general equilibrium state, for after all, by definition a real equilibrium is a state to which variables tend to return once they are replaced. How does the return take place?

For the economy to return to equilibrium, profits and interest must be evanescent. And innovation of course must also come to an end. How can this take place? For one thing, innovations must be discontinuous; they must only appear in discrete clusters. For if innovation were *continuous*, the economy would never return to the equilibrium state. Given this assumption of discontinuous clusters, Schumpeter found a way: When the innovations are "completed" and the new processes or new products enter the market, they outcompete the old processes and products, thereby reaping the profits out of which interest is paid. But these profits are made at the expense of severe losses for the old, now inefficient, firms or industries, which are driven to the wall. After a while, the innovations are completed, and the inexorable imputation process destroys all profits and therefore all interest, while the sudden losses to the old firms are also ended. The economy returns to the unchanging circular flow, and stays there until another cluster of innovations appears, whereupon the cycle starts all over again.

"Cycle" is here the operative term, for in working out the logical process of breakout and return, Schumpeter has at the same time seemingly developed a unique theory of the business cycle. Phase I, the breakout, looks very much like the typical boom phase of the business cycle: inflationary bank credit, rise in prices and wages, general euphoria, and redirection of resources to more investment. Then, the events succeeding the "completion" of the innovation look very much like the typical recession or depression: sudden severe losses for the old firms, retrenchment. And finally, the disappearance of both innovation and euphoria, and eventually of losses and disruption—in short, a return to a placid period which can be made to seem like the state of stationary equilibrium.

But Schumpeter's doctrine only *seems* like a challenging business cycle theory worthy of profound investigation. For it is not really a cycle theory at all. It is simply the only logical way that Schumpeter can break out and then return to the Walrasian box. As such, it is certainly an ingenious formulation, but it has no genuine connection with reality at all.

Even within his own theory, indeed, there are grave flaws. In the Walrasian world of perfect certainty (an assumption which is not relaxed with the coming of the innovator), how is it that the old firms wait until the "completion" of the innovation to find suddenly

that they are suffering severe losses? In a world of perfect knowledge and expectations, the old firms would know of their fate from the very beginning, and early take steps to adjust to it. In a world of perfect expectations, therefore, there would be no losses, and therefore no recession or depression phase. There would be no cycle as economists know it.

Finally, Schumpeter's constrained model can only work if innovations come in clusters, and the empirical evidence for such clusters is virtually nil.[11] In the real world, innovations occur all the time. Therefore, there is no reason to postulate any return to an equilibrium, even if it had ever existed in the past.

In conclusion, Schumpeter's theory of development and of business cycles has impressed many economists with his suggestive and seemingly meaningful discussions of innovation, bank credit, and the entrepreneur. He has seemed to offer far more than static Walrasian equilibrium analysis and to provide an economic dynamic, a theoretical explanation of cycles and of economic growth. In fact, however, Schumpeter's seemingly impressive system has no relation to the real world at all. He has not provided an economic dynamic; he has only found an ingenious but fallacious way of trying to break out of the static Walrasian box. His theory is a mere exercise in equilibrium logic leading nowhere.

It is undoubtedly at least a partial realization of this unhappy fact that prompted Schumpeter to expand his business cycle theory from his open-cycle model of the *Theory of Economic Development* of 1912 to his three-cycle schema in his two-volume *Business Cycles* nearly three decades later.[12] More specifically, Schumpeter saw that one of the problems in applying his model to reality was that if the length of the boom period is determined by the length of time required to "complete" the innovation and bring it to market, then how could his model apply to real life, where simultaneous innovations occur, each of which requires a different time for its completion? His later three-cycle theory is a desperate attempt to encompass such real-life

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[11]See Simon S. Kuznets, "Schumpeter's Business Cycles," *American Economic Review* (June 1940).

[12]Joseph A. Schumpeter, *Business Cycles: A Theoretical, Historical, and Statistical Analysis of the Capitalist Process*, 2 vols. (New York: McGraw-Hill, 1939).

problems. Specifically, Schumpeter has now postulated that the economy, instead of unitarily breaking out and returning to equilibrium, consists of three separate hermetically sealed, strictly periodic cycles—the "*Kitchin,*" the "*Juglar*," and the "*Kondratieff*"—each with the same innovation-inflation-depression characteristics. This conjuring up of allegedly separate underlying cycles, each cut off from the other, but all adding to each other to yield the observable results of the real world, can only be considered a desperate lapse into mysticism in order to shore up his original model.

In the first place, there are far more than three innovations going on at one time in the economy, and there is no reason to assume strict periodicity of each set of disparate changes. Indeed, there is no such clustering of innovations as would be required by the theory. Second, in the market economy, all prices and activities interact; there therefore can never be any hermetically sealed cycles. The multicycle scheme is an unnecessary and heedless multiplication of entities in flagrant violation of Occam's Razor. In an attempt to save the theory, it asserts propositions that cannot be falsifiable, since another cycle can always be conjured up to explain away anomalies.[13] In an attempt to salvage his original model, Schumpeter only succeeded in adding new and greater fallacies to the old.

In the years before and during World War II, the most popular dynamic theory of economic change was the gloomy doctrine of "secular stagnation" (or "economic maturity") advanced by Professor Alvin H. Hansen.[14] The explanation of the Great Depression of the 1930s, for Hansen, was that the United States had become mired in permanent stagnation, from which it could not be lifted by free market capitalism. A year or two after the publication of Keynes's *General Theory*, Hansen had leaped on the New Economics to become the leading American Keynesian; but secular stagnation, while giving

[13]This does not mean that all propositions must be falsifiable; they can be self-evident or deduced from self-evident axioms. But no one can claim that the alleged Kitchin, Juglar, and Kondratieff cycles are in any sense self-evident.

[14]See Alvin H. Hansen, *Fiscal Policy and Business Cycles* (New York: W.W. Norton, 1941). For a clear summary statement of his position, see Hansen, "Economic Progress and Declining Population Growth," in *Readings in Business Cycle Theory,* Gottfried Haberler, ed. (Philadelphia: Blakiston, 1944), pp. 366–84.

Keynesianism a left-flavor, was unrelated to Keynesian theory. For Keynes, the key to prosperity or depression was private investment: flourishing private investment means prosperity; weak and fitful investment leads to depression. But Keynes was an agnostic on the investment question, whereas Hansen supplied his own gnosis. Private investment in the United States was doomed to permanent frailty, Hansen opined, because (1) the frontier was now closed; (2) population growth was declining rapidly; and (3) there would be hardly any further inventions, and what few there were would be of the capital-saving rather than labor-saving variety, so that total investment could not increase.

George Terborgh, in his well-known refutation of the stagnation thesis, *The Bogey of Economic Maturity*, concentrated on a statistical critique.[15] If the frontier had been "closed" since the turn of the century, why then had there been a boom for virtually three decades until the 1930s? Population growth too, had been declining for many decades. It was easy, also, to demolish the rather odd and audacious prediction that few or no further inventions, at least of the labor-saving variety, would ever more be discovered. Predictions of the cessation of invention, which have occurred from time to time through history, are easy targets for ridicule.

But Terborgh never penetrated to the fundamentals of the Hansen thesis. In an age beset by the constant clamor of population doomsayers and zero-population-growth enthusiasts, it is difficult to conjure up an intellectual climate when it seemed to make sense to worry about the *slowing* of population growth. But why, indeed, should Hansen have considered population growth as *ipso facto* a positive factor for the spurring of investment? And why would a slowing down of such growth be an impetus to decay? Schumpeter, in his own critique of the Hansen thesis, sensibly pointed out that population growth could easily lead to a fall in real income per capita.[16]

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[15]George Terborgh, *The Bogey of Economic Maturity* (Chicago: Machinery and Allied Products Institute, 1945).

[16]Schumpeter, *Business Cycles*, p. 74.

Ironically, however, Schumpeter did not recognize that Hansen, too, in his own way, was trying to break out of the Walrasian box. Hansen began implicitly (not explicitly like Schumpeter) with the circular flow and general equilibrium, and then considered the various possible factors that might change—or, more specifically, might increase. And these were the familiar Walrasian triad: land, labor, and technique. As Terborgh noted, Hansen had a static view of "investment opportunities." He treated them as if they were a limited physical entity, like a sponge. They were a fixed amount, and when that maximum amount was reached, investment opportunities were "saturated" and disappeared. The implicit Hansen assumption is that these opportunities could be generated only by increases in land, labor, and improved techniques (which Hansen limited to inventions rather than Schumpeterian innovations). And so the closing of the frontier meant the drying up of "land-investment opportunities", as one might call them, the slowing of population growth, the end of "labor-investment opportunities," leading to a situation where innovation could not carry the remaining burden.

And so Hansen's curious view of the economic effects of diminishing population growth, as gloomily empirical as it might seem, was not really an empirical generalization at all. Indeed, it said nothing about dynamic change or about the real world at all. The allegedly favorable effect of high population growth was merely the logical spinning out of Hansen's own unsuccessful variant of trying to escape from the Walrasian box.

15

Professor Rolph on the Discounted Marginal Productivity Theory

Of current schools of economic thought, the most fashionable have been the econometric, the Keynesian, the institutionalist, and the neo-classic. "Neo-classic" refers to the pattern set by the major economists of the late nineteenth century. The dominant neoclassical strain at present is to be found in the system of Professor Frank Knight, of which the most characteristic feature is an attack on the whole concept of time preference. Denying time preference, and basing interest return solely on an alleged "productivity" of capital, the Knightians attack the doctrine of the *discounted* MVP and instead advocate a pure MVP theory. The clearest exposition of this approach is to be found in an article by a follower of Knight's, Professor Earl Rolph.[1]

Rolph defines "product" as any *immediate* results of "present valuable activities." These include work on goods that will be consumed only in the future. Thus, "workmen and equipment beginning the construction of a building may have only a few stakes in the ground to show for their work the first day, but this and not the completed structure is their immediate product. Thus, the doctrine that a factor receives the value of its marginal product refers to this immediate product. The simultaneity of production and product does not

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Originally a discussion in *Man, Economy, and State* (1962; Auburn, Ala.: Ludwig von Mises Institute, 1993), vol. 1, app. B, pp. 431–33.

[1]Earl Rolph, "The Discounted Marginal Productivity Doctrine" in *Readings in the Theory of Income Distribution*, W. Fellner and B.F. Haley, eds. (Philadelphia: Blakiston, 1946), pp. 278–93.

require any simplifying assumptions. It is a direct appeal to the obvious. Every activity has its immediate results."

Obviously, no one denies that people work on goods and move capital a little further along. But is the immediate result of this a *product* in any meaningful sense? It should be clear that the product is the end product—the good sold to the consumer. The whole purpose of the production system is to lead to final consumption. All the intermediate purchases are based on the expectation of final purchase by the consumer and would not take place otherwise. Every activity may have its immediate "results," but they are not results that would command any monetary income from anyone if the owners of the factors themselves were joint owners of all they produced until the final consumption stage. In that case, it would be obvious that they do not get paid immediately; hence, their product is not immediate. The only reason that they *are* paid immediately (and even here there is not strict immediacy) on the market is that capitalists advance present goods in exchange for those *future* goods for which they expect a premium, or interest return. Thus, the owners of the factors are paid the *discounted* value of their marginal product.

The Knight-Rolph approach, in addition, is a retreat to a real-cost theory of value. It assumes that present efforts will somehow always bring present results. But when? In "present valuable activities." But how do these activities *become* valuable? Only if their *future product* is sold, as expected, to consumers. Suppose, however, that people work for years on a certain good and are paid by capitalists, and then the final product is not bought by consumers. The capitalists absorb monetary losses. Where was the immediate payment according to marginal product? The payment was only an investment in future goods by capitalists.

Rolph then turns to another allegedly heinous error of the discount approach, namely, the "doctrine of *noncoordination of factors*." This means that some factors, in their payment, receive the *discounted* value of their product and some do not. Rolph, however, is laboring under a misapprehension; there is no assumption of noncoordination in any sound discounting theory. As we have stated above, *all* factors—land, and capital goods—receive their discounted marginal value product. The difference in regard to the owners of capital goods is that, in the ultimate analysis, they do not

receive any *independent* payment, since capital goods are resolved into the factors that produced them, ultimately land and labor factors, and to interest for the time involved in the advance of payment by the capitalists.[2] Rolph believes that noncoordination is involved because owners of land and labor factors "receive a discounted share," and capital "receives an undiscounted share." But this is a faulty way of stating the conclusion. Owners of land and labor factors receive a discounted share, but owners of capital (money capital) receive *the discount*.

The remainder of Rolph's article is largely devoted to an attempt to prove that no time lag is involved in payments to owners of factors. Rolph assumes the existence of "production centers" within every firm, which, broken down into virtually instantaneous steps, produce and then implicitly receive payment instantaneously. This tortured and unreal construction misses the entire point. Even if there were atomized "production centers," the point is that some person or persons will have to make advances of present money along the route, in whatever order, until the final product is sold to the consumers. Let Rolph picture a production system, atomized or integrated as the case may be, with no one making the advances of present goods (money capital) that he denies exist. And as the laborers and landowners work on the intermediate products for years without pay, until the finished product is ready for the consumer, let Rolph

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[2]Rolph ascribes this error to Knut Wicksell, but such a confusion is not attributable to Wicksell, who engages in a brilliant discussion of capital and the production structure and the role of time in production. Wicksell demonstrates correctly that labor and land are the only ultimate factors, and that therefore the marginal productivity of capital goods is reducible to the marginal productivity of labor and land factors, so that money capital earns the interest (or discount) differential.

Wicksell's discussion of these and related issues is of basic importance. He recognized, for example, that capital goods are fully and basically coordinate with land and labor factors *only from the point of view of the individual firm*, but not when we consider the total market in all of its interrelations. Current economic theorizing is, to its detriment, even more preoccupied than writers of his day with the study of an isolated firm instead of the interrelated market. Wicksell, *Lectures on Political Economy* (London: Routledge and Kegan Paul, 1934), vol. 1, pp. 148–54, 185–95.

exhort them not to worry, since they have been implicitly paid simultaneously as they worked. For this is the logical implication of the Knight-Rolph position.[3]

[3]Rolph ends his article, consistently, with a dismissal of any time-preference influences on interest, which he explains in Knightian vein by the "cost" of producing new capital goods.

16

Professor Kirzner on Entrepreneurship

Since I admittedly know more about Austrian economic theory than about Richard Cantillon, I would like to focus my comments on the Austrian aspects of Professor Hébert's paper, in particular his discussion of entrepreneurship. Hébert is correct in his discussion of the differences between Mises's and Kirzner's concept of the entrepreneur and in his critique of the Kirzner approach.

Mises conceives of the entrepreneur as the uncertainty-bearer, who receives profits to the degree that he can successfully forecast the future, and suffers losses to the extent that his forecasting goes awry. One evident case of rewards in proportion to the success of forecasting is the stock or commodity market. The stock or commodity speculator, furthermore, clearly suffers losses to the extent that his forecasting is significantly less accurate than that of his fellow speculators. But Mises points out that the market as a whole is in the same situation as the stock or commodity market. The entrepreneur who buys raw material and hires labor, and who thereby incurs costs in order to produce a future product, is expecting that he will be able to sell the product to customers for a revenue greater than the costs. Just as the stock speculator purchases stock in the hope and the expectation that it will rise in price, so the employer incurs costs in the expectation that he will be able to sell the product at a greater price.

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To Kirzner, on the other hand, entrepreneurship becomes reduced to the quality of *alertness*; and uncertainty seems to have little to do with the matter. In his lectures, Kirzner likes to stress the analogy that the entrepreneur is a person who, upon seeing a $10 bill in front of his nose, is alert to the existence of the money and leaps to grab it. The alert man will grab the $10 note rapidly; the less alert will take longer to see his opportunity and to take advantage of it. One problem, as Hébert mentions, is that it is difficult to account for actual *losses*; for the worst that can happen to the non-alert sluggard is that he misses his opportunity for gaining $10. But how then does it ever come about that he actually *loses* ten or more dollars? Moreover, by stressing alertness, Kirzner is emphasizing a quality of perception, of perceiving an opportunity that virtually exists, as a real *thing* out there. In reality, however, any profit opportunity is uncertain, and rather than be a real existing entity, it must always be subject to uncertainty. It is never as simple as mere alertness.

Take the case of perhaps the best fictional portrayal of the entrepreneurial function, Somerset Maugham's short story, *The Verger*. In this story, the illiterate verger of a church in London is fired for not being able to read or write. Walking down the street looking for a cigarette for consolation, he observes that he cannot find a tobacconist in the neighborhood, and so he decides to invest his severance pay in setting up a tobacconist shop. This comes close to the Kirzner model of "perceived opportunity," of being alert to a gap in the services provided by the market. But even here, matters were not that simple. The verger, after all, had to forecast costs and revenues, and he could well have suffered losses if his forecasting had erred greatly. The need for a tobacconist could have withered from a change of smoking habits, from a new store entering the neighborhood at the same time, or whatever.

Even Kirzner's best case, the arbitrageur, is subject to uncertainty, a point which Hébert overlooks. The arbitrageur can perceive that a product sells for one price at one place and at a higher price somewhere else, and therefore buy in the first place to sell in the second. But he'd better be cautious. The transactions are not instantaneous, and something might occur in the interim to change the seemingly certain profits into losses. It is, after all, possible that the other entrepreneurs, far from purblind to the profit opportunity lying await for arbitrage, know something which our would-be arbitrageur

does not. At any rate, he might be better advised to look before he leaps. Surely, *some* arbitrageurs in the history of the world have suffered losses.

As Hébert points out, Mises applies the concept of entrepreneur to all cases of uncertainty-bearing, and since laborers face uncertainty in deciding where to move or what occupation to go into, laborers are also entrepreneurs. But the most important case of entrepreneurship, the driving force in shaping the actual structure and patterns of production in the market economy, are the capitalist-entrepreneurs, the ones who commit and risk their capital in deciding when, what, and how much to produce. The capitalists, too, are far more subject to actual monetary losses than are the laborers.

Kirzner's entrepreneur is a curious formulation. He need not, apparently, risk anything. He is a free-floating wraith, disembodied from real objects. He does not, and need not, possess any assets. All he need have to earn profits is a faculty of alertness to profit opportunities. Since he need not risk any capital assets to meet the chancy fate of uncertainty, he *cannot* suffer any losses. But if the Kirznerian entrepreneur owns no assets, then how in the world does he earn profits? Profits, after all, are simply the other side of the coin of an increase in the value of one's capital; losses are the reflection of a loss in capital assets. The speculator who expects a stock to rise uses money to purchase that stock; a rise or fall in the price of stock will raise or lower the value of the stock assets. If the price rises, the profits are one and the same thing as the increase in capital assets. The process is more complex but similar in the purchase or hiring of factors of production, the creating of a product and then its sale on the market. In what sense can an entrepreneur ever make profits if he owns no capital to make profits on?

For example, I might have a brilliant idea on how to make a profit on the market. I might be keenly alert to a profit opportunity virtually lying at my feet. I may have a sure tip on the stock market. But if I haven't got any money to invest, the profits, perceived opportunity or not, will simply not be made. Entrepreneurial ideas without money are mere parlor games until the money is obtained and committed to the projects.

One Kirznerian reply to such criticisms is that the entrepreneur need not own any assets, need not be a capitalist, if he can induce other people with money to invest in his idea.

But this reply is unsatisfactory. Let us consider two possible such cases. In one example, I, with a brilliant entrepreneurial idea, sell that idea to someone with money; we invest in that project, with him putting up all the money and letting me be a junior partner because I contributed the idea. He keeps, say, 80 percent of the shares, and gives me the other 20 percent. But the Kirznerian Concept is now contradicted. In the first place, the moneyed man, risking his own assets in the firm, has thereby *become* an entrepreneur. The employer who spends his capital and hopes for a profitable return *is* an entrepreneur, an uncertainty-bearer, and he is also to the same extent a capitalist, since that is the extent of assets that he is risking. But there is more to the problem than this. For I might have begun as a free-floating wraith, as a man with an idea and no assets. But because of my contract with the moneyed investor, I have now *become* a capitalist, since I now own assets to the amount of 20 percent of the firm. In other words, there are here two fundamental and fatal flaws in Kirzner's notion of the alert idea man as the entrepreneur: one, that the capitalist is *also* an entrepreneur, and two, that the pure idea man has, willy nilly, become a capitalist.

The second possible case of the entrepreneur financing his project at first blush looks more favorable for Kirzner's doctrine. The pure idea man induces a capitalist to *lend* him all the money he needs to invest in his idea. The entrepreneur takes the loaned funds and sets up his business, investing in the new idea, and hoping for profits. But, once again, the Kirzner concept is contradicted. For the idea man has still become a capitalist-owner; for he now owns all the assets of the new company, even though they may be mortgaged to the hilt in loans from his backer.

The former idea man has once again, willy nilly, become an asset-owner, a capitalist. He owns the equipment and the raw material, he owns the product before sale, and he owns the money acquired from sale. He will suffer losses if the revenues do not meet expectations. It is true that he will have to share any profits with the lender by paying him interest. But the lender, though his interest return is fixed, is still partly an entrepreneur. For while his return is fixed, it is by no means certain, and if the idea fails and the firm goes bankrupt, the capitalist's money has been lost. So that he, too, still shares the entrepreneurial function with the idea man.

It might be said that, in this case at least, the idea man can lose no money because all the money was loaned to him by the capitalist. But, as in the first case where he received assets as a gift from his partner, the entrepreneur, by borrowing money, soon *became* a capitalist and asset owner. The man who borrows $1 million and then buys $1 million worth of assets is now someone risking that million, and he loses his share of the assets if he suffers insolvency. Furthermore, his interest payment is now a net loss to him. Aside from the interest due, it is true that he will not be monetarily worse than he was at the beginning, when he had the idea. But he will be monetarily poorer than he was while he owned the new plant. An employer-entrepreneur must be a capitalist; *at what time* he became a capitalist and asset owner is irrelevant to the theory.

If I may engage in a bit of sociology of knowledge, I think I can explain why Kirzner has deviated so sharply from the main Misesian line. In the first place, there is a certain uncharacteristic lack of clarity in Mises's discussion of entrepreneurship. While Mises basically links the capitalist and entrepreneur together in uncertainty-bearing, there are passages in his *Human Action* which treat the entrepreneur as an entirely separate entity, and not just as the forecasting aspect of the activities of the capitalist or laborer. In other words, there is a certain amount of textual justification in Mises for the Kirzner turn—justification which did not exist in Böhm-Bawerk, where the entrepreneur is clearly the capitalist and there is no possibility of such separation. On the other hand, Böhm-Bawerk did not develop the theory of profits, losses, and uncertainty to any extent, which had to wait for Mises, who grounded himself on Frank Knight as well as the other Austrians.

But, second and I think more important, Kirzner developed his theory of entrepreneurial alertness I believe in reaction to the opposite deviation from main-line Misesianism introduced into the Austrian arena by Ludwig M. Lachmann. Becoming a disciple of G.L.S Shackle, Lachmann, and following him other younger Austrians, maintains not only that uncertainty is pervasive on the market, but also that we cannot even say that the market contains a tendency toward equilibrium, a tendency fueled by the profit-and-loss signals of the market. To Lachmann, expectations and therefore actions on the market are random, rather than responsive to market signals. It is one thing to say, with Mises and his followers, and in contrast to

the neoclassical economists, that equilibrium does not and can never exist on the market. It is quite another thing to say that the market does not even harbor equilibrating tendencies.

The upshot is really the scrapping of economic theory altogether, and the Lachmannian economist becomes a mere institutionalist and historian, recording past choices and trends. There is no question that Mises would have called such a doctrine *anti*economics. I believe that it was in horrified reaction to this Lachmannian nihilism that Professor Kirzner sought a way to downplay uncertainty and to make his entrepreneur a more tangible and objective entity earning tangible profits on the market. In the dialectic of the history of thought, it is a common occurrence for one deviation from the main line of theory to give rise to a deviation in the opposite direction. Since I believe the Mises-Hayek mainline position to be the correct one on this issue, I can only hope that these deviations will in effect cancel each other out and that Austrian thought will return to its own mainstream position.

Next, Professor Hébert mentions Schumpeter's theory of entrepreneurship, and contrasts it to the Misesian position. But while it is true that Schumpeter was trained in Böhm-Bawerk's seminar in Vienna at the same time as Mises, he early shifted to a Walrasian position. Being a Walrasian, Schumpeter had to believe that general equilibrium is a living reality, an existing state of affairs, at least part of the time. But if the world is in general equilibrium, how do business cycles or growth and development emerge?

Schumpeter's *Theory of Economic Development* was a fascinating, though ill-conceived, attempt to derive a theory of the business cycle and economic growth from a Walrasian general equilibrium starting-point. According to Walras, tastes, technology, and resources were given in general equilibrium. If we begin with the economy in that equilibrium state, therefore, any change from that state must occur in at least one of these variables. To Schumpeter, as to other neoclassical economists, tastes could not be the changing element. Tastes he regarded as basically fixed; certainly they could not be the driving force of economic change. Total supply of resources didn't change very frequently either. So Schumpeter was left with innovation in technology as the only possible motor force for any change, be it business cycles or economic development. But then Schumpeter was confronted with a problem: how would these innovations be financed?

Not out of new savings, since tastes were given, and since by definition net savings are zero in equilibrium. Not out of profits, since by definition profits are zero in equilibrium. One way out might have been finance out of interest returns, since according to Austrian theory, savings, the result of positive time preference, are positive even in equilibrium. But Schumpeter had rejected the concept of time preference, so he was left with interest and profits both being zero in equilibrium. The result was that Schumpeter had trapped himself in a Walrasian box: the only conceivable way by which new investment, which had to be in innovations, could be financed was by the creation of new money. This meant that only inflationary bank credit could finance economic development.

In short, because Schumpeter believed in the real existence of Walrasian general equilibrium, and since he boxed himself into the position that only inflationary bank credit could finance innovations, some important consequences necessarily followed. Since general equilibrium is by definition a world of perfect knowledge and certainty, and since that world of endlessly unchanging rounds of activity has no room for entrepreneurship, it followed automatically that the only entrepreneurial function could be disruption of equilibrium. Entrepreneurs could not make any adjustments, since in the fixed and certain world of general equilibrium, there is nothing to adjust.

Second, it followed that entrepreneurial profits could only redound to the innovators, and that interest is the return on inflationary bank loans. Economic development, and the inflationary boom, a boom sparked by bank credit to innovations, had begun. But if the economy begins in Walrasian equilibrium, it had to return there, otherwise equilibrium is only relevant to one originating point of the economic process. Equilibrium cannot be a real entity unless a strong tendency exists to return to that state, once dislodged. So to maintain his Walrasianism in dealing with economic change, Schumpeter had to come up with the business cycle; the depression would have to be the mechanism by which the economy returned to the general equilibrium state. Schumpeter found the mechanism of that return in the alleged moment in which the new products or new equipment are finally produced and poured onto the market; the advent of the new products, Schumpeter theorized, outcompeted the older firms and drove them into bankruptcy. The losses imposed on the older firms constituted the depression phase of the cycle.

It was an ingenious schema, but with many grave flaws. Apart from the fact that there is no evidence that booms are confined to innovations or recessions to older processes (which forced Schumpeter to confuse matters still more with a multi-cycle schema two decades later), one wonders why in a Walrasian world of perfect certainty—or, indeed, in the real world of reasonably astute entrepreneurs—the older firms had to wait for the shock of the influx of new products. Why couldn't they foresee the moment much earlier and take precautionary measures?

But the major problem is fundamental and methodological. Schumpeter's business cycle theory and his theory of growth are, for all their suggestiveness, not positive theories of the real world at all; they are simply ways by which slavish adherence to Walrasian categories boxed Schumpeter in and forced him into his conclusions. In a sense, this was theory by default.[1]

The Schumpeter case highlights the true nature of Austrian economics and Austrian methodology. Austrian economics has generally been dismissed as extreme *a priorism*, cut off from the empirical data of the real world. The true situation is exactly the opposite. Austrian theory ruthlessly confines itself to an analysis of real life in the real world. It avoids abstract and unreal "models" and theoretical boxes. It shuns false assumptions and premises. It rests its deductive theoretical structures squarely on empirically grounded general axioms. Methodologically, it is far closer to classical economics than is the current Walrasian orthodoxy.

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[1]For a development of this theme, see "Breaking Out of the Walrasian Box: The Cases of Schumpeter and Hansen," *Review of Austrian Economics* 1 (1987): 97–108; included in this volume as chapter 14.

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Toward a Reconstruction of Utility and Welfare Economics

Individual valuation is the keystone of economic theory. For, fundamentally, economics does not deal with things or material objects. Economics analyzes the logical attributes and consequences of the existence of individual valuations. "Things" enter into the picture, of course, since there can be no valuation without things to be valued. But the essence and the driving force of human action, and therefore of the human market economy, are the valuations of individuals. Action is the result of choice among alternatives, and choice reflects values, that is, individual preferences among these alternatives.

Individual valuations are the direct subject matter of the theories of utility and of welfare. Utility theory analyzes the laws of the values and choices of an individual; welfare theory discusses the relationship between the values of many individuals, and the consequent possibilities of a scientific conclusion on the "social" desirability of various alternatives.

Both theories have lately been foundering in stormy seas. Utility theory is galloping off in many different directions at once; welfare theory, after reaching the heights of popularity among economic theorists, threatens to sink, sterile and abandoned, into oblivion.

The thesis of this paper is that both related branches of economic theory can be salvaged and reconstructed, using as a guiding principle of both fields the concept of "demonstrated preference."

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Demonstrated Preference

A Statement of the Concept

Human action is the use of means to arrive at preferred ends. Such action contrasts to the observed behavior of stones and planets, for it implies purpose on the part of the actor. Action implies choice among alternatives. Man has means, or resources, which he uses to arrive at various ends; these resources may be time, money, labor energy, land, capital goods, and so on. He uses these resources to attain his most preferred ends. From his action, we can deduce that he has acted so as to satisfy his most highly valued desires or preferences.

The concept of *demonstrated preference* is simply this: that actual choice reveals, or demonstrates, a man's preferences; that is, that his preferences are deducible from what he has chosen in action. Thus, if a man chooses to spend an hour at a concert rather than a movie, we deduce that the former was preferred, or ranked higher on his value scale. Similarly, if a man spends five dollars on a shirt we deduce that he preferred purchasing the shirt to any other uses he could have found for the money. This concept of preference, rooted in real choices, forms the keystone of the logical structure of economic analysis, and particularly of utility and welfare analysis.

While a similar concept played a role in the writings of the early utility economists, it had never received a name, and it therefore remained largely undeveloped and unrecognized as a distinct concept. It was generally discarded in the 1930s, before it had even achieved recognition. This view of preference as derived from choice was present in varying degree in the writings of the early Austrian economists, as well as in the works of Jevons, Fisher, and Fetter. Fetter was the only one who clearly employed the concept in his analysis. The clearest and most thorough formulation of the concept has been the works of Professor Mises.[1]

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[1]See Alan R. Sweezy, "The Interpretation of Subjective Value Theory in the Writings of the Austrian Economists," *Review of Economic Studies* (June 1934): 176–85, for an historical survey. Sweezy devotes a good part of the article to a criticism of Mises as the leading exponent of the demonstrated preference approach. For Mises's views, see *Human Action* (New

Positivism and the Charge of Tautology

Before developing some of the applications of the demonstrated preference principle to utility and welfare theory, we must consider the methodological objections that have been levelled against it. Professor Alan Sweezy, for example, seizes on a sentence of Irving Fisher's which very succinctly expressed the concept of demonstrated preference: "Each individual acts as he desires." Sweezy is typical of the majority of present-day economists in not being able to understand how such a statement can be made with absolute validity. To Sweezy, insofar as it is not an empirically testable proposition in psychology, such a sentence must simply reduce to the meaningless tautology: "each individual acts as he acts."

This criticism is rooted in a fundamental epistemological error that pervades modern thought: the inability of modern methodologists to understand how economic science can yield substantive truths by means of logical deduction (that is, the method of "praxeology"). For they have adopted the epistemology of positivism (now dubbed "logical empiricism" or "scientific empiricism" by its practitioners), which uncritically applies the procedures appropriate in physics to the sciences of human action.[2]

In physics, simple facts can be isolated in the laboratory. These isolated facts are known directly, but the laws to explain these facts are not. The laws may only be hypothecated. Their validity can only be determined by logically deducing consequents from them which can be verified by appeal to the laboratory facts. Even if the laws explain the facts, however, and their inferences are consistent with them, the laws of physics can never be *absolutely* established. For some other law may prove more elegant or capable of explaining a wider range of facts. In physics, therefore, postulated explanations have to be hypothecated in such a way that they or their consequents can be empirically tested. Even then, the laws are only tentatively rather than absolutely valid.

Haven, Conn.: Yale University Press, 1949), pp. 94–96, 102–03; *Theory of Money and Credit* (1912, 3rd ed; New Haven, Conn.: Yale University Press, 1951), pp. 46ff. Also see Frank A. Fetter, *Economic Principles* (New York: The Century Co., 1915), pp. 14–21.

[2]See the methodological treatises of Kaufman, Hutchison, Souter, Stonier, Myrdal, Morgenstern, *and so on*.

In human action, however, the situation is reversed. There is here no laboratory where "facts" can be isolated and broken down into their simple elements. Instead, there are only historical "facts" which are complex phenomena, resultants of many causal factors. These phenomena must be explained, but they cannot be isolated or used to verify or falsify any law. On the other hand, economics, or praxeology, has full and complete knowledge of its original and basic axioms. These are the axioms *implicit in the very existence of human action*, and they are absolutely valid so long as human beings exist. But if the axioms of praxeology are absolutely valid for human existence, then so are the consequents which can logically be deduced from them. Hence, economics, in contrast to physics, can derive absolutely valid substantive truths about the real world by deductive logic. The axioms of physics are only hypothecated and hence subject to revision; the axioms of economics are already known and hence absolutely true.[3] The irritation and bewilderment of positivists over the "dogmatic" pronouncements of praxeology stem, therefore, from their universal application of methods proper only to the physical sciences.[4]

The suggestion has been made that praxeology is not really scientific, because its logical procedures are verbal ("literary") rather than mathematical and symbolic.[5] But mathematical logic is uniquely appropriate to physics, where the various logical steps along the way are not in themselves meaningful; for the axioms and therefore the

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[3]On the methodology of praxeology and physics, see Mises, *Human Action*, and F.A. Hayek, *The Counter Revolution of Science* (Glencoe, Ill.: The Free Press, 1952), pt 1.

[4]It is even dubious that positivists accurately interpret the proper methodology of physics itself. On the widespread positivist misuse of the Heisenberg Uncertainty Principle in physics as well as in other disciplines, cf. Albert H. Hobbs, *Social Problems and Scientism* (Harrisburg, Penn.: The Stackpole Co., 1953), pp. 220–32.

[5]For a typical suggestion, cf. George J. Schuller, "Rejoinder," *American Economic Review* (March 1951): 188. For realization that mathematical logic is essentially subsidiary to basic verbal logic, cf. the remarks of André Lalande and René Poirier, on "Logique" and "Logistique," in André Lalande, ed., *Vocabulaire téchnique et critique de la philosophie*, 6th ed. (Paris: Presses Universitaires de France, 1951), pp. 574, 579.

deductions of physics are in themselves meaningless, and only take on meaning "operationally," insofar as they can explain and predict given facts. In praxeology, on the contrary, the axioms themselves are known as true and are therefore meaningful. As a result, each step-by-step deduction is meaningful and true. Meanings are far better expressed verbally than in meaningless formal symbols. Moreover, simply to translate economic analysis from words into symbols, and then to retranslate them so as to explain the conclusions, makes little sense, and violates the great scientific principle of Occam's Razor that there should be no unnecessary multiplication of entities.

The crucial concept of the positivists, and the one that forms the basis for their attack on demonstrated preference, is that of "operational meaning." Indeed, their favorite critical epithet is that such and such a formulation or law is "operationally meaningless."[6] The test of "operationally meaningful" is derived strictly from the procedures of physics as outlined above. An explanatory law must be framed so that it can be tested and found empirically false. Any law which claims to be absolutely true and not empirically capable of being falsified is therefore "dogmatic" and operationally meaningless—hence, the positivist's view that if a statement or law is not capable of being falsified empirically, it must simply be a tautologous definition. And consequently, Sweezy's attempted reduction of Fisher's sentence to a meaningless identity.[7]

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[6]Paul Samuelson has added the weight of his authority to Sweezy's criticism of Mises and demonstrated preference, and has couched his endorsement in terms of "operational meaning." Samuelson explicitly rejects the idea of a *true* utility theory in favor of one that is merely hypothetical. See Paul A. Samuelson, "The Empirical Implications of Utility Analysis," *Econometrica* (1938): 344ff.; and Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass.: Harvard University Press, 1947), pp. 91–92.

The concept of operational meaning was originated by the physicist Percy W. Bridgman explicitly to explain the methodology of physics. Cf. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927). Many founders of modern positivism, such as Mach and Boltzmann, were also physicists.

[7]The heros of positivism, Rudolf Carnap and Ludwig Wittgenstein, disparaged deductive inference as merely drawing out "tautologies" from the axioms. Yet all reasoning is deductive, and this process is peculiarly vital to

Sweezy objects that Fisher's "each man acts as he desires" is circular reasoning, because action implies desire, and yet desires are not arrived at independently, but are only discoverable through the action itself. Yet this is not circular. For desires exist by virtue of the concept of human action and of the existence of action. It is precisely the characteristic of human action that it is motivated by desires and ends, in contrast to the unmotivated bodies studied by physics. Hence, we can say validly that action is motivated by desires and yet confine ourselves to deducing the *specific* desires from the real actions.

Professor Samuelson and "Revealed Preference"

"Revealed preference"—preference revealed through choice—would have been an apt term for our concept. It has, however, been preempted by Samuelson for a seemingly similar but actually quite different concept of his own. The critical difference is this: Samuelson assumes the existence of an underlying preference scale that forms the basis of a man's actions and that remains constant in the course of his actions over time. Samuelson then uses complex mathematical procedures in an attempt to "map" the individuals preference scale on the basis of his numerous actions.

The prime error here is the assumption that the preference scale remains constant over time. There is no reason whatever for making any such assumption. All we can say is that an action, at a specific point of time, reveals part of a man's preference scale *at that time*. There is no warrant for assuming that it remains constant from one point of time to another.[8]

The "revealed preference" theorists do not recognize that they are assuming constancy; they believe that their assumption is simply that of *consistent* behavior, which they identify with "rationality." They will admit that people are not always "rational," but uphold

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arriving at truth. For a critique of Carnap and Wittgenstein, and a demonstration that inference is not merely identity to "tautology," cf. Lalande, "Tautoglie," in *Vocabulaire*, pp. 1103–04.

[8]Samuelson's analysis suffers from other errors as well, such as the use of invalid "index number" procedures. On the theoretical fallacies of index numbers, cf. Mises, *Theory of Money and Credit*, pp. 187–94.

their theory as being a good first approximation or even as having normative value. However, as Mises has pointed out, *constancy* and *consistency* are two entirely different things. Consistency means that a person maintains a transitive order of rank on his preference scale (if A is preferred to B and B is preferred to C, then A is preferred to C). But the revealed preference procedure does not rest on this assumption so much as on an assumption of *constancy*—that an individual maintains the same value scale over time. While a violation of the former might be called irrational, there is certainly nothing irrational about someone's value scales changing through time. Hence, no valid theory can be built on a constancy assumption.[9]

One of the most absurd procedures based on a constancy assumption has been the attempt to arrive at a consumer's preference scale not through observed real action, but through quizzing him by questionnaires. *In vacuo*, a few consumers are questioned at length on which abstract bundle of commodities they would prefer to another abstract bundle, and so on. Not only does this suffer from the constancy error, no assurance can be attached to the mere questioning of people when they are not confronted with the choices in actual practice. Not only will a person's valuation differ when talking about them from when he is actually choosing, but there is also no guarantee that he is telling the truth.[10]

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[9]See Mises, *Human Action*, pp. 102–03. Mises demonstrates that Wicksteed and Robbins committed a similar error.

[10]It is to Samuelson's credit that he rejects the questionnaire approach. Professors Kennedy and Keckskemeti, for different reasons, defend the questionnaire method. Kennedy simply says, rather illogically, that *in vacuo* procedures are being used anyway, when the theorist states that *more* of a good is preferred to *less*. But this is not *in vacuo*; it is a conclusion based on the praxeological knowledge that since a *good* is any object of action, more must be preferred to less while it remains a good. Kennedy is wrong, therefore, when he asserts that this is a circular argument, for the fact that action exists is not "circular."

Keckskemeti actually asserts that the questionnaire method is preferable to observing behavior in discovering preferences. The basis of his arguments is a spurious dichotomy between utility and ethical valuations. Ethical valuations may be considered either as identical with, or a subset of, utility judgments, but they can not be separated.

The bankruptcy of the revealed-preference approach has never been better portrayed than by a prominent follower, Professor Kennedy. Says Kennedy: "In what respectable science would the assumption of consistency (that is, constancy) be accepted for one moment?"[11] But he asserts it must be retained anyway, else utility theory could not serve any useful purpose. The abandonment of truth for the sake of a spurious usefulness is a hallmark of the positivist-pragmatist tradition. Except for certain auxiliary constructions, it should be clear that the false cannot be useful in constructing a true theory. This is particularly the case in economics, which is explicitly built on *true* axioms.[12]

Psychologizing and Behaviorism: Twin Pitfalls

The revealed-preference doctrine is one example of what we may call the fallacy of "psychologizing," the treatment of preference scales as if they existed as separate entities apart from real action. Psychologizing is a common error in utility analysis. It is based on the assumption that utility analysis is a kind of "psychology," and that, therefore, economics must enter into psychological analysis in laying the foundations of its theoretical structure.

Praxeology, the basis of economic theory, differs from psychology, however. Psychology analyzes the *how* and the *why* of people forming values. It treats the concrete content of ends and values. Economics, on the other hand, rests simply on the assumption of the *existence* of ends, and then deduces its valid theory from such a

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Cf. Charles Kennedy, "The Common Sense of Indifference Curves," *Oxford Economic Papers* (January 1950): 123–31; Kenneth J. Arrow, "Review of Paul Keckskemeti's *Meaning, Communication, and Value*," *Econometrica* (January 1955): 103.

[11]Kennedy, "The Common Sense of Indifference Curves." Kennedy's article furnishes the best brief explanation of the revealed-preference approach.

[12]This error again stems from physics, where such assumptions as absence of friction are useful as first approximations—to *known* facts from *unknown* explanatory laws! For a refreshing skepticism on the value of false axioms, cf. Martin Bronfenbrenner, "Contemporary Economics Resurveyed," *Journal of Political Economy* (April 1953).

general assumption.[13] It therefore has nothing to do with the content of ends or with the internal operations of the mind of the acting man.[14]

If psychologizing is to be avoided, so is the opposite error of *behaviorism*. The behaviorist wishes to expunge "subjectivism," that is, motivated action, completely from economics, since he believes that any trace of subjectivism is unscientific. His ideal is the method of physics in treating observed movements of unmotivated, inorganic matter. In adopting this method, he throws away the subjective knowledge of action upon which economic science is founded; indeed, he is making any scientific investigation of human beings impossible. The behaviorist approach in economics began with Cassel, and its most prominent modern practitioner is Professor Little. Little rejects the demonstrated preference theory because it assumes the existence of preference. He glories in the fact that, in his analysis, the maximizing individual "at last disappears" which means, of course, that economics disappears as well.[15]

The errors of psychologizing and of behaviorism have in common a desire by their practitioners to endow their concepts and procedures with "operational meaning," either in the areas of observed behavior or in mental operations. Vilfredo Pareto, perhaps the founder of an explicitly positivist approach in economics, championed both errors. Discarding a demonstrated preference approach as "tautologous," Pareto, on the one hand, sought to eliminate subjective preferences from economics and, on the other, to investigate and

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[13]The axiom of the existence of ends may be considered a proposition in philosophical psychology. In that sense, praxeology is grounded in psychology, but its development then completely diverges from psychology proper. On the question of purpose, praxeology takes its stand squarely with the Leibnizian tradition of philosophical psychology as opposed to the Lockean tradition upheld by positivists, behavorists, and associationists. For an illuminating discussion of this issue, cf. Gordon W. Allport, *Becoming* (New Haven, Conn.: Yale University Press, 1955), pp. 6–17.

[14]Thus, the law of diminishing marginal utility does not at all rest on some postulated psychological law of satiety of wants, but on the praxeological truth that the first units of a good will be allocated to the most valuable uses, the next units to the next-most valuable uses, and so on.

[15]I.M.D. Little, "A Reformulation of the Theory of Consumers' Behavior," *Oxford Economic Papers* (January 1949): 90–99.

measure preference scales apart from real action. Pareto was, in more ways than one, the spiritual ancestor of most current utility theorists.[16,17]

A Note on Professor Armstrong's Criticism

Professor Armstrong has delivered a criticism of the revealed-preference approach which he would undoubtedly apply to demonstrated preference as well. He asserts that when more than one commodity is being ranked, individual preference scales cannot be unitary, and we cannot postulate the ranking of the commodities on one scale.[18] On the contrary, it is precisely the characteristic of a deduced preference scale that it is unitary. Only if a man ranks two alternatives as more and less valuable on one scale can he choose between them. Any of his means will be allocated to his more preferred use. Real choice therefore always demonstrates relevant preferences ranked on a unitary scale.

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[16]Vilfredo Pareto, "On the Economic Phenomenon," *International Economic Papers* 3 (1953): 188–94. For an excellent rebuttal, cf. Benedetto Croce, "On the Economic Principle, Parts I and II," ibid., pp. 175–76, 201. The famous Croce-Pareto debate is an illuminating example of early debate between praxeologic and positivist views in economics.

[17]Vivian C. Walsh is an interesting current example of the combinations of both types of error. On the one hand, he is an extreme behaviorist, who refuses to recognize that any preferences are relevant to, or can be demonstrated by, action. On the other hand, he also takes the extreme psychologizing view that psychological states *per se* can be directly observed. For this, he falls back on "common sense." But this position fails because Walsh's psychological "observations" are *ideal types* and not analytic categories. Thus, Walsh says that: "saying that someone is a smoker is different from saying that he is smoking now," upholding the former type of statement for economics. But such statements are historical ideal types, relevant to history and psychology, but not to economic analysis. Cf. Vivian C. Walsh, "On Descriptions of Consumers' Behavior," *Economica* (August 1954): 244–52. On ideal types and relation to praxeology, cf. Mises, *Human Action*, pp. 59–64.

[18]Wallace E. Armstrong, "A Note on the Theory of Consumer's Behavior," *Oxford Economic Papers* (January 1950): 199ff. On this point, cf. Little's rebuttal, in I.M.D. Little, "The Theory of Consumer's Behavior—A Comment," ibid., pp. 132–35.

Utility Theory

Utility theory, over the last generation, has been split into two warring camps: (1) those who cling to the old concept of cardinal, measurable utility, and (2) those who have thrown over the cardinal concept, but have dispensed with the utility concept as well and have substituted an analysis based on indifference curves.

In its pristine form, the cardinalist approach has been abandoned by all but a rearguard. On demonstrated preference grounds, cardinality must be eliminated. Psychological magnitudes cannot be measured since there is no objectively extensive unit—a necessary requisite of measurement. Further, actual choice obviously cannot demonstrate any form of *measurable* utility; it can only demonstrate one alternative being preferred to another.[19]

Ordinal Marginal Utility and Total Utility

The ordinalist rebels, led by Hicks and Allen in the early 1930s, felt it necessary to overthrow the very concept of marginal utility along with measurability. In doing so, they threw out the Utility baby together with the Cardinal bathwater. They reasoned that marginal utility itself implies measurability. Why? Their notion rested on the implicit neoclassical assumption that the marginal in marginal utility is equivalent to the marginal of the differential calculus. Since, in mathematics, a total "something" is the integral of "marginal somethings," economists early on assumed that "total utility" was the mathematical integral of a series of "marginal utilities."[20] Perhaps, too, they realized that this assumption was essential to a mathematical representation of utility. As a result, they assumed, for example, that the "marginal utility" of a good with a supply of six units is equal to the "total utility" of six units minus the "total utility" of five units. If utilities can be subjected to the arithmetical operation of subtraction, and can be differentiated and integrated, then obviously the

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[19]Mises's priority in establishing this conclusion is acknowledged by Professor Robbins; cf. Lionel Robbins, "Robertson on Utility and Scope," *Economica* (May 1953): 99–111; Mises, *Theory of Money and Credit*, pp. 38–47 and passim. Mises's role in forging an ordinal marginal utility theory has suffered almost total neglect.

[20]The error began perhaps with Jevons. Cf. W. Stanley Jevons, *Theory of Political Economy* (London: Macmillan, 1888), pp. 49ff.

concept of marginal utility must imply cardinally measurable utilities.[21]

The mathematical representation of the calculus rests on the assumption of *continuity*, that is, infinitely small steps. In human action, however, there can be no infinitely small steps. Human action and the facts on which it is based must be in observable and discrete steps and not infinitely small ones. Representation of utility in the manner of the calculus is therefore illegitimate.[22]

There is, however, no reason why marginal utility must be conceived in calculus terms. In human action, "marginal" refers not to an infinitely small unit, but to the *relevant* unit. Any unit relevant to a particular action is marginal. For example, if we are dealing in a specific situation with single eggs, then each egg is the unit; if we are dealing in terms of six-egg cartons, then each six-egg carton is the unit. In either case, we can speak of a marginal utility. In the former case, we deal with the "marginal utility of an egg" with various supplies of eggs; in the latter, with the "marginal utility of cartons" whatever the supply of cartons of eggs. Both utilities are marginal. In no sense is one utility a "total" of the other.

To clarify the relationship between marginal utility and what has been misnamed "total utility" but actually refers to a marginal utility of a larger-sized unit, let us hypothetically construct a typical value scale for eggs:

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[21]That this reasoning lay at the base of the ordinalists' rejection of marginal utility may be seen in John R. Hicks, *Value and Capital*, 2nd ed. (Oxford: Oxford University Press, 1946), p. 19. That many ordinalists regret the loss of marginal utility may be seen in the statement by Arrow that: "The older discussion of diminishing marginal utility as aiming for the satisfaction of more intense wants first makes more sense" than the current "indifference-curve" analysis, but that, unfortunately it is "bound up with the untenable notion of measurable utility." Quoted in D.H. Robertson, "Utility and All What?" *Economic Journal* (December 1954): 667.

[22]Hicks concedes the falsity of the continuity assumption but blindly pins his faith on the hope that all will be well when individual actions are aggregated. Hicks, *Value and Capital*, p. 11.

Ranks in Value

5 eggs
4 eggs
3 eggs
2 eggs
1 egg
2nd egg
3rd egg
4th egg
5th egg

This is a man's ordinal value, or preference, scale for eggs. The higher the ranking, the higher the value. At the center is one egg, the first egg in his possession. By the Law of Diminishing Marginal Utility (ordinal), the second, third, fourth eggs, and so on, rank below the first egg on his value scale, and in that order. Now, since eggs are goods and therefore objects of desire, it follows that a man will value two eggs more than he will one, three more than he will two, and so on. Instead of calling this "total utility," we will say that *the marginal utility of a unit of a good is always higher than the marginal utility of a unit of smaller size*. A bundle of 5 eggs will be ranked higher than a bundle of 4 eggs, and so on. It should be clear that the only arithmetic or mathematical relationship between these marginal utilities is a simple ordinal one. On the one hand, given a certain sized unit, the marginal utility of that unit declines as the supply of units increases. This is the familiar Law of Diminishing Marginal Utility. On the other hand, the marginal utility of a larger-sized unit is greater than the marginal utility of a smaller-sized unit. This is the law just underlined. And there is no mathematical relationship between, say, the marginal utility of 4 eggs and the marginal utility of the 4th egg except that the former is greater than the latter.

We must conclude then that *there is no such thing as total utility*; all utilities are marginal. In those cases where the supply of a good totals only one unit, then the "total utility" of that whole supply is simply the marginal utility of a unit the size of which equals the

whole supply. The key concept is the *variable size* of the marginal unit, depending on the situation.[23]

A typical error on the concept of marginal utility is a recent statement by Professor Kennedy that "the word 'marginal' presupposes increments of utility" and hence measurability. But the word "marginal" presupposes *not* increments of utility, *but the utility of increments of goods*, and this need have nothing to do with measurability.[24]

Professor Robbins's Problem

Professor Lionel Robbins, in the course of a recent defense of ordinalism, raised a problem which he left unanswered. Accepted doctrine, he declared, states that if *difference* between utility rankings can be judged by the individual, as well as the rankings themselves, then the utility scale can in some way be *measured*. Yet, Robbins says, he can judge differences. For example, among three paintings, he can say that he prefers a Rembrandt to a Holbein far less than he prefers a Holbein to a Munnings. How, then, can ordinalism be saved?[25] Is

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[23]The analysis of total utility was first put forward by Mises, in *Theory of Money and Credit*, pp. 38–47. It was continued by Harro F. Bernardelli, especially in his "The End of the Marginal Utility Theory?" *Economica* (May 1938): 206. Bernardelli's treatment, however, is marred by laborious attempts to find some form of legitimate mathematical representation. On the failure of the mathematical economists to understand this treatment of marginal and total, see the criticism of Bernardelli by Paul A. Samuelson, "The End of Marginal Utility: A Note on Dr. Bernardelli's Article," *Economica* (February 1939): 86–87; Kelvin Lancaster, "A Refutation of Mr. Bernardelli," *Economica* (August 1953): 259–62. For rebuttals see Bernardelli, "A Reply to Mr. Samuelson's Note," *Economica* (February 1939): 88–89; and "Comment on Mr. Lancaster's Refutation," *Economica* (August 1954): 240–42.

[24]See Charles Kennedy, "Concerning Utility," *Economica* (February 1954): 13. Kennedy's article, incidentally, is an attempt to rehabilitate a type of cardinalism by making distinctions between "quantity" and "magnitude," and using the Bertrand Russell concept of "relational addition." Surely, this sort of approach falls with one slash of Occam's Razor—the great scientific principle that entities not be multiplied unnecessarily. For a criticism, cf. D.H. Robertson, "Utility and All What?" pp. 668–69.

[25]Robbins, "Robertson on Utility and Scope," p. 104.

he not conceding measurability? Yet Robbins's dilemma had already been answered twenty years earlier in a famous article by Oskar Lange.[26] Lange pointed out that in terms of what we would call demonstrated preference, only pure rankings are revealed by acts of choice. "Differences" in rank are not so revealed, and are therefore mere psychologizing, which, however interesting, are irrelevant to economics. To this, we need only add that differences of rank can be revealed through real choice, whenever the goods *can* be obtained by money. We need only realize that *money* units (which are characteristically highly divisible) can be lumped in the same value-scale as commodities. For example, suppose someone is willing to pay $10,000 for a Rembrandt, $8,000 for a Holbein and only $20 for a Munnings. Then, his value-scale will have the following descending order: Rembrandt, $10,000; Holbein, $9,000, $8,000, $7,000, $6,000; . . . Munnings, $20. We may observe these ranks and no question of the measurability of utilities need arise.

That money and units of various goods can be ranked on one value scale is the consequence of Mises's money-regression theorem, which makes possible the application of marginal utility analysis to money.[27] It is characteristic of Professor Samuelson's approach that he scoffs at the whole problem of circularity which money-regression had solved. He falls back on Léon Walras, who developed the idea of "general equilibrium in which all magnitudes are simultaneously

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[26]Oskar Lange, "The Determinateness of the Utility Function," *Review of Economic Studies* (June 1934): 224ff. Unfortunately, Lange balked at the implications of his own analysis and adopted an assumption of cardinality, solely because of his anxious desire to reach certain cherished "welfare" conclusions.

[27]See Mises, *Theory of Money and Credit*, pp. 97–123. Mises replied to critics in *Human Action*, pp. 405ff. The only further criticism has been that of Gilbert, who asserts that the theorem does not explain how a paper money can be introduced after the monetary system has broken down. Presumably he refers to such cases as the German *Rentenmark*. The answer, of course, is that such paper was not introduced *de novo*; gold and foreign exchange existed previously, and the *Rentenmark* could exchange in terms of these previously existing moneys. Cf. J.C. Gilbert, "The Demand for Money: The Development of an Economic Concept," *Journal of Political Economy* (April 1953): 149.

determined by efficacious interdependent relations," which he contrasts to the "fears of literary writers" about circular reasoning.[28] This is one example of the pernicious influence of the mathematical method in economics. The idea of mutual determination is appropriate in physics, which tries to explain the unmotivated motions of physical matter. But in praxeology, the *cause* is known: individual purpose. In economics, therefore, the proper method is to proceed from the causing action to its consequent effects.

The Fallacy of Indifference

The Hicksian Revolutionaries replaced the cardinal utility concept with the concept of indifference classes, and for the last twenty years, the economic journals have been rife with a maze of two- and three-dimensional indifference curves, tangencies, "budget lines," and so on. The consequence of an adoption of the demonstrated preference approach is that the entire indifference-class concept, along with the complicated superstructure erected upon it, must fall to the ground.

Indifference can never be demonstrated by action. Quite the contrary. Every action necessarily signifies a *choice*, and every choice signifies a definite preference. Action specifically implies the *contrary* of indifference. The indifference concept is a particularly unfortunate example of the psychologizing error. Indifference classes are assumed to exist somewhere underlying and apart from action. This assumption is particularly exhibited in those discussions that try to "map" indifference curves empirically by the use of elaborate questionnaires.

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[28]Samuelson, *Foundations of Economic Analysis*, pp. 117–18. For similar attacks on earlier Austrian economists, cf. Frank H. Knight, "Introduction" in Carl Menger, *Principles of Economics* (Glencoe, Ill.: The Free Press, 1950), p. 23; George J. Stigler, *Production and Distribution Theories* (New York: Macmillan, 1946), p. 181. Stigler criticizes Böhm-Bawerk for spurning "mutual determination" for "the older concept of cause and effect" and explains this by saying that Böhm-Bawerk was untrained in mathematics. For Menger's attack on the mutual determination concept, cf. Terence W. Hutchison, *A Review of Economic Doctrines, 1870–1929* (Oxford: Clarendon Press, 1953), p. 147.

If a person is really indifferent between two alternatives, then he cannot and will not choose between them.[29] Indifference is therefore never relevant for action and cannot be demonstrated in action. If a man, for example, is indifferent between the use of 5.1 ounces and 5.2 ounces of butter because of the minuteness of the unit, then there will be no occasion for him to act on these alternatives. He will use butter in larger-sized units, where varying amounts are *not* indifferent to him.

The concept of "indifference" may be important for psychology, but not for economics. In psychology, we are interested in finding out intensities of value, possible indifference, and so on. In economics, however, we are only interested in values revealed through choices. It is immaterial to economics whether a man chooses alternative A to alternative B because he strongly prefers A or because he tossed a coin. The *fact of ranking* is what matters for economics, *not* the reasons for the individuals arriving at that rank.

In recent years, the indifference concept has been subjected to severe criticism. Professor Armstrong pointed out that under Hicks's curious formulation of "indifference," it is possible for an individual to be "indifferent" between two alternatives and yet choose one over the other.[30] Little has some good criticisms of the indifference concept, but his analysis is vitiated by his eagerness to use faulty theorems in order to arrive at welfare conclusions, and by his radically behaviorist methodology.[31] A very interesting attack on the indifference concept from the point of view of psychology has been levelled by Professor Macfie.[32]

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[29]The "indifference theorists" also err in assuming infinitely small steps, essential for their geometric representation but erroneous for an analysis of human action.

[30]Wallace E. Armstrong, "The Determinateness of Utility Function," *Economic Journal* (1939): 453–67. Armstrong's point that indifference is not a transitive relation (as Hicks assumed), only applies to different-sized units of *one* commodity. Also cf. Armstrong, "A Note on the Theory of Consumers' Behavior."

[31]Little, "Reformulation" and "Theory." It is another defect of Samuelson's revealed preference approach that he attempts to "reveal" indifference-curves as well.

[32]Alec L. Macfie, "Choice in Psychology and as Economic Assumption," *Economic Journal* (June 1953): 352–67.

The indifference theorists have two basic defenses of the role of indifference in real action. One is to cite the famous fable of *Buridan's Ass*. This is the "perfectly rational" ass who demonstrates indifference by standing, hungry, equidistant from two equally attractive bales of hay.[33] Since the two bales are equally attractive in every way, the ass can choose neither one and starves therefore. This example is supposed to indicate how indifference can be revealed in action. It is, of course, difficult to conceive of an ass, or a person, who could be *less* rational. Actually, he is not confronted with *two* choices but with *three*, the third being to starve where he is. Even on the indifference theorists' own grounds, this third choice will be ranked lower than the other two on the individuals value-scale. He will *not* choose starvation.

If both bundles of hay are equally attractive, then the ass or man, who must choose one or the other, will allow pure chance, such as the flip of a coin, to decide on either one. But then indifference is still not revealed by this choice, for the flip of a coin has enabled him to establish a preference![34]

The other attempt to demonstrate indifference classes rests on the consistency—constancy fallacy, which we have analyzed above. Thus, Kennedy and Walsh claim that a man can reveal indifference if, when asked to repeat his choices between A and B *over time*, he chooses each alternative 50 percent of the time.[35]

If the concept of the individual indifference curve is completely fallacious, it is quite obvious that Baumol's concept of the "community indifference curve," which he purports to build up from individual curves, deserves the shortest possible shrift.[36]

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[33]Thus, cf. Joseph A. Schumpeter, *History of Economic Analysis* (New York: Oxford University Press, 1954), pp. 94*n* and 1064.

[34]Also see Croce's warning about using animal illustrations in analyses of human action. Croce, "Economic Principle I," p. 175.

[35]Kennedy, "The Common Sense of Indifference Curves" and "On Descriptions of Consumer's Behavior."

[36]William J. Baumol, *Welfare Economics and the Theory of the State* (1952; Cambridge, Mass.: Harvard University Press, 1965), pp. 47ff.

The Neo-Cardinalists: The von Neumann-Morgenstern Approach

In recent years, the world of economics has been taken by storm by a neo-cardinalist, quasi-measurement theory of utility. This approach, which has the psychological advantage of being garbed in a mathematical form more advanced than economics had yet known, was founded by von Neumann and Morgenstern in their celebrated work.[37] Their theory had the further advantage of being grounded on the most recent and fashionable (though incorrect) developments in the philosophy of measurement and the philosophy of probability. The von Neumann-Morgenstern thesis was adopted by the leading mathematical economists and has gone almost unchallenged to this day. The chief consolation of the ordinalists has been the assurance by the neo-cardinalists that their doctrine applies only to utility under conditions of uncertainty, and therefore does not shake the ordinalist doctrine too drastically.[38] But this consolation is really quite limited, considering that some uncertainty enters into every action.

The von Neumann-Morgenstern theory is briefly as follows: an individual can compare not only certain events, but also combinations of events with definite numerical probabilities for each event. Then, according to the authors, if an individual prefers alternative A to B, and B to C, he is able to decide whether he prefers B or a 50:50 probability combination of C and A. If he prefers B, then his preference of B over C is deduced as being greater than his preference of A over B. In a similar fashion, various combinations of probabilities

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[37]John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior*, 2nd ed. (Princeton, N.J.: Princeton University Press, 1947), pp. 8, 15–32, 617–32.

[38]Thus see the excellent expository article by Armen A. Alchian, "The Meaning of Utility Measurement," *American Economic Review* (May 1953): 384–97. The leading adherents of the Neumann-Morgenstern approach are Marschak, Friedman, Savage, and Samuelson.

Claims of the theory, even at its best, to measure utility in any way have been nicely exploded by Ellsberg, who also demolishes Marschak's attempt to make the theory normative. Ellsberg's critique suffers considerably, however, from being based on the "operational meaning" concept. D. Ellsberg, "Classic and Current Notions of Measurable Utility," *Economic Journal* (September 1954): 528–56.

are selected. A quasi-measurable numerical utility is assigned to his utility scale in accordance with the indifference of utilities of B as compared with various probability combinations of A or C. The result is a numerical scale given when arbitrary numbers are assigned to the utilities of two of the events.

The errors of this theory are numerous and grave:

(1) None of the axioms can be validated on demonstrated preference grounds, since admittedly all of the axioms can be violated by the individual actors.

(2) The theory leans heavily on a constancy assumption so that utilities can be revealed by action over time.

(3) The theory relies heavily on the invalid concept of *indifference* of utilities in establishing the numerical scale.

(4) The theory rests fundamentally on the fallacious application of a theory of numerical probability to an area where it cannot apply. Richard von Mises has shown conclusively that numerical probability can be assigned only to situations where there is a class of entities, such that nothing is known about the members except they are members of this class, and where successive trials reveal an asymptotic tendency toward a stable proportion, or frequency of occurrence, of a certain event in that class. There can be no numerical probability applied to specific individual events.[39]

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[39]Richard von Mises, *Probability, Statistics, and Truth* (New York: Macmillan, 1957). Also Ludwig von Mises, *Human Action*, pp. 106–17. The currently fashionable probability theories of Rudolf Carnap and Hans Reichenbach have failed to shake the validity of Richard von Mises's approach. Mises refutes them in the third German edition of his work, unfortunately unavailable in English. See Richard von Mises, *Wahrscheinlichkeit, Statistik, und Wahrheit,* 3rd ed. (Vienna: J. Springer, 1951). The only plausible critique of Richard von Mises has been that of W. Kneale, who pointed out that the numerical assignment of probability depends on an *infinite* sequence, whereas in no human action can there be an infinite sequence. This, however, *weakens* the application of numerical probability even to cases such as lotteries, rather than enabling it to expand into other areas. See also Little, "A Reformulation of the Theory of Consumers' Behavior."

Yet, in human action, precisely the opposite is true. Here, there are no classes of homogeneous members. Each event is a unique event and is different from other unique events. These unique events are not repeatable. Therefore, there is no sense in applying numerical probability theory to such events.[40] It is no coincidence that, invariably, the application of the neo-cardinalists has always been to lotteries and gambling. It is precisely and *only* in lotteries that probability theory can be applied. The theorists beg the entire question of its applicability to general human action by confining their discussion to lottery cases. For the purchaser of a lottery ticket knows only that the individual lottery ticket is a member of a certain-sized class of tickets. The entrepreneur, in making his decisions, is on the contrary confronted with unique cases about which he has some knowledge and which have only limited parallelism to other cases.

(5) The neo-cardinalists admit that their theory is not even applicable to gambling if the individual has either a like or a dislike for gambling itself. Since the fact that a man gambles demonstrates that he likes to gamble, it is clear that the von Neumann-Morgenstern utility doctrine fails even in this tailor-made case.[41]

(6) A curious new conception of measurement. The new philosophy of measurement discards concepts of "cardinal" and "ordinal" in favor of such labored constructions as measurable up to a multiplicative constant (cardinal); "measurable up to a

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[40]Frank Knight's basic distinction between the narrow cases of actuarial "risk" and the more widespread, non-actuarial "uncertainty." Frank H. Knight, *Risk, Uncertainty, and Profit*, 2nd ed. (London: London School of Economics, 1940). G.L.S. Shackle has also leveled excellent criticism at the probability approach to economics, especially that of Marschak. His own "surprise" theory, however, is open to similar objections; C.F. Carter, "Expectations in Economics," *Economic Journal* (March 1950): 92–105; and G.L.S. Shackle, *Expectations in Economics* (Cambridge, Mass.: Cambridge University Press, 1949), pp. 109–23.

[41]It is curious how economists have been tempted to discuss gambling by first assuming that the participant doesn't like to gamble. It is on this assumption that Alfred Marshall based his famous "proof" that gambling (because of each individual's diminishing utility of money) is "irrational."

> monotonic transform" (ordinal); "measurable up to a linear transform" (the new quasi-measurement, of which the von Neumann-Morgenstern proposed utility index is an example). This terminology, apart from its undue complexity (under the influence of mathematics), implies that everything, including ordinality, is somehow measurable. The man who proposes a new definition for an important word must prove his case; the new definition of measurement has hardly done so. Measurement, on any sensible definition, implies the possibility of a unique assignment of numbers which can be meaningfully subjected to all the operations of arithmetic. To accomplish this, it is necessary to define a fixed unit. In order to define such a unit, the property to be measured must be extensive in space, so that the unit can be objectively agreed upon by all. Therefore, subjective states, being *intensive* rather than objectively extensive, cannot be measured and subjected to arithmetical operations. And utility refers to intensive states. Measurement becomes even more implausible when we realize that utility is a praxeologic, rather than a directly psychologic, concept.

A favorite rebuttal is that subjective states *have* been measured; thus, the old, unscientific subjective feeling of heat has given way to the objective science of thermometry.[42] But this rebuttal is erroneous; thermometry does *not* measure the intensive subjective feelings themselves. It assumes an approximate correlation between the intensive property and an objective extensive event—such as the physical expansion of gas or mercury. And thermometry can certainly lay no claim to precise measurement of subjective states: we all know that some people, for various reasons, feel warmer or colder at different times even if the external temperature remains the same.[43] Certainly no correlation whatever can be found for demonstrated preference scales in relation to physical lengths. For preferences have no *direct* physical basis, as do feelings of heat.

No arithmetical operations whatever can be performed on ordinal numbers; therefore, to use the term measurable in any way for

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[42]Thus, cf. von Neumann and Morgenstern, *Theory of Games and Economic Behavior*, pp. 16–17.

[43]Morris R. Cohen, *A Preface to Logic* (New York: H. Holt, 1944), p. 151.

ordinal numbers is hopelessly to confuse the meaning of the term. Perhaps the best remedy for possible confusion is to avoid using *any* numbers for ordinal rank; the rank concept can just as well be expressed in letters (A, B, C . . .), using a convention that A, for example, expresses higher rank.

As to the new type of quasi-measurability, no one has yet proved it capable of existence. The burden of proof rests on the proponents. If an object is extensive, then it is at least theoretically capable of being measured, for an objective fixed unit can, in principle, be defined. If it is intensive, then no such fixed unit can apply, and any assignment of number would have to be ordinal. There is no room for an intermediate case. The favorite example of quasi-measurability that is always offered is, again, temperature. In thermometry, centigrade and Fahrenheit scales are supposed to be convertible into each other *not* at a multiplicative constant (cardinality) but by multiplying and then adding a constant (a "linear transform"). More careful analysis, however, reveals that both scales are simply derivations from one scale based on an absolute zero point. All we need to demonstrate the cardinality of temperature is to transform both centigrade and Fahrenheit scales into scales where "absolute zero" *is* zero, and then each will be convertible into the other by a multiplicative constant. Furthermore, the actual measurement in temperature is a measurement of length (say, of the mercury column) so that temperature is really a derived measure based on the cardinally measurable magnitude of length.[44]

Jacob Marschak, one of the leading members of the von Neumann-Morgenstern School, has conceded that the temperature case

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[44]On measurement, see Norman Campbell, *What is Science?* (New York: Dover, 1952), pp. 109–34; and Campbell, *An Account of the Principles of Measurement and Calculation* (London: Longmans, Green, 1928). Although the above view of measurement is not currently fashionable, it is backed by the weighty authority of Mr. Campbell. A description of the controversy between Campbell and S. Stevens on the issue of measurement of intensive magnitudes was included in the unpublished draft of Carl G. Hempel's *Concept Formation*, but was unfortunately omitted from Hempel's published *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago, 1952). Campbell's critique can be found in A. Ferguson, et al. *Interim Report* (British Association for the Advancement of Science Final Report, 1940), pp. 331–49.

is inappropriate for the establishment of quasi-measurability, because it is derived from the fundamental, cardinal, measurement of distance. Yet, astonishingly, he offers *altitude* in its place. But if "temperature readings are nothing but distance," what else is altitude, which is solely and purely distance and length?[45]

Welfare Economics: A Critique

Economics and Ethics

It is now generally accepted among economists, at least *pro forma*, that economics *per se* cannot establish ethical judgments. It is not sufficiently recognized that to accept this need not imply acceptance of the Max Weber position that ethics can never be scientifically or rationally established. Whether we accept the Max Weber position, or we adhere to the older view of Plato and Aristotle that a rational ethics is possible, it should be clear that *economics* by itself cannot establish an ethical position. If an ethical science is possible, it must be built up out of data supplied by truths established by all of the other sciences.

Medicine can establish the fact that a certain drug can cure a certain disease, while leaving to other disciplines the problem whether the disease *should* be cured. Similarly, economics can establish that Policy A leads to the advancement of life, prosperity, and peace, while Policy B leads to death, poverty, and war. Both medicine and economics can establish these consequences scientifically, and without introducing ethical judgments into the analysis. It might be protested that doctors would not inquire into possible cures for a disease if they did not want a cure, or economists would not investigate causes of prosperity if they did not want the result. There are two answers to this point: (1) that this is undoubtedly true in almost all cases, but not *necessarily* so—some doctors or economists may care only about the discovery of truth, and (2) this only establishes the psychologic motivation of the scientists; it does not establish that the discipline itself arrives at values. On the contrary, it bolsters the thesis that ethics is arrived at apart from the specific sciences of medicine or economics.

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[45]Jacob Marschak, "Rational Behavior, Uncertain Prospects, and Measureability," *Econometrica* (April 1950): 131.

Thus, whether we hold the view that ethics is a matter of non-rational emotions or taste, or whether we believe in a rational ethic, we must agree that economic science *per se* cannot establish ethical statements. As political policy judgment is a branch of ethics, the same conclusion applies to politics. If prosperity vs. poverty, for example, are political alternatives, economic science cannot decide between them; it simply presents the truth about the consequences of each alternative political decision. As citizens, we take these truths into account when we make our politico-ethical decisions.

The Problem of the New Welfare Economics: The Unanimity Rule

The problem of "welfare economics" has always been to find some way to circumvent this restriction on economics, and to make ethical, and particularly *political*, statements directly. Since economics discusses individuals' aiming to maximize their utility or happiness or welfare, the problem may be translated into the following terms: When can economics say that "society is better off" as a result of a certain change? Or alternatively, when can we say that "social utility" has been increased or "maximized"?

Neoclassical economists, led by Professor Pigou, found a simple answer. Economics can establish that a man's marginal utility of money diminishes as his money-income increases. Therefore, they concluded, the marginal utility of a dollar is less to a rich man than to a poor man. *Other things being equal*, social utility is maximized by a progressive income tax which takes from the rich and gives to the poor. This was the favorite demonstration of the "old welfare economics," grounded on Benthamite utilitarian ethics, and brought to fruition by Edgeworth and Pigou.

Economists continued blithely along this path until they were brought up short by Professor Robbins. Robbins showed that this demonstration rested on interpersonal comparisons of utility, and since utility is not a cardinal magnitude, such comparisons involve ethical judgments.[46] What Robbins actually accomplished was to

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[46]Lionel Robbins, "Interpersonal Comparisons of Utility," *Economic Journal* (December 1938): 635–41; and Robbins, *An Essay on the Nature and Significance of Economic Science*, 2nd ed. (London: Macmillan, 1935), pp. 138–41.

reintroduce Pareto's Unanimity Rule into economics and establish it as the iron gate where welfare economics must test its credentials.[47] This Rule runs as follows: We can only say that "social welfare" (or better, "social utility") has *increased* due to a change, if no individual is worse off because of the change (and at least one is better off). If one individual is worse off, the fact that interpersonal utilities cannot be added or subtracted prevents economics from saying anything about social utility. Any statement about social utility would, in the absence of unanimity, imply an ethical interpersonal comparison between the gainers and the losers from a change. If X number of individuals gain, and Y number lose, from a change, any weighing to sum up in a "social" conclusion would necessarily imply an ethical judgment on the relative importance of the two groups.[48]

The Pareto-Robbins Unanimity Rule conquered economics and liquidated the old Pigovian welfare economics almost completely. Since then, an enormous literature known as the "new welfare economics" has flourished, devoting itself to a series of attempts to square the circle: to assert certain political judgments as scientific economics, while still retaining the unanimity rule.

Professor Robbins's Escape Route

Robbins's own formulation of the Unanimity Rule far undervalues the scope of its restrictive power over the assertions of economists. Robbins stated that only *one* ethical assertion would be necessary for economists to make interpersonal comparisons: namely, that every man has an "equal capacity for satisfaction" in similar circumstances. To be sure, Robbins grants that this ethical assumption cannot be established by economics; but he implies that since all good democrats are bound to make this egalitarian assumption, we can all pretty well act *as if* interpersonal comparisons of utility can be made and go on to make ethical judgments.

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[47]Vilfredo Pareto, *Manuel d'Économie Politique*, 2nd ed. (Paris: Marcel Giard, 1927), p. 617.

[48]Kemp tries to alter the Unanimity Rule to read that social utility is only increased if everyone is better off, non being worse off or indifferent. But, as we have seen, indifference cannot be demonstrated in action, and therefore this alteration is invalid. Murray C. Kemp, "Welfare Economics: A Stocktaking," *Economic Record* (November 1954): 245.

In the first place, it is difficult, upon analysis, to make sense of the phrase "equal capacity for satisfaction." Robbins, as we have seen, admits that we cannot scientifically compare utilities or satisfactions between individuals. But since there is no unit of satisfaction by which we can make comparisons, there is no meaning to any assumption that different men's satisfactions will be "equal" to any circumstances. "Equal" in what way, and in what units? We are not at liberty to make any ethical assumption we please, because even an ethical assumption must be framed meaningfully, and its terms must be definable in a meaningful manner. Since there is no meaning to the term "equality" without some sort of definable unit, and since there is no unit of satisfaction or utility, it follows that there can be no ethical assumption of "equal capacity for satisfaction," and that this cannot provide a shortcut to permit the economists to make conclusions about public policy.

The Robbins position, moreover, embodies a highly oversimplified view of ethics and its relation to politico-economic affairs. The problem of interpersonal comparisons of utility *is only one* of the very many ethical problems which must at least be discussed before any policy conclusions can rationally be framed. Suppose, for example, that two social changes take place, each of which causes 99 percent of the people to gain in utility and 1 percent to lose. Surely no assumption about the interpersonal comparison of utility can suffice to establish an ethical judgment, divorced from the *content* of the change itself. If, for example, one change was the enslavement of the 1 percent by the 99 percent, and the other was the removal of a governmental subsidy to the 1 percent, there is apt to be a great deal of difference in our ethical pronouncements on the two cases, even if the assumed "social utility" in the two cases is approximately the same.

The Compensation Principle

A particularly notable attempt to make policy conclusions within the framework of the Unanimity Rule was the Kaldor-Hicks "compensation principle," which stated that "social utility" may scientifically be said to increase, if the winners *may* be able to compensate the losers and still remain winners.[49] There are many fatal errors in

[49]On the compensation principle, see Nicholas Kaldor, "Welfare Propositions in Economics," *Economic Journal* (September 1939): 549; John R.

this approach. In the first place, since the compensation principle is supposed to help economists form policy judgments, it is evident that we must be able to compare, at least in principle, *actual* social states. We are therefore always concerned with *actual*, and not *potential*, winners and losers from any change. Whether or not the winners *may* compensate the losers is therefore irrelevant; the important question is whether the compensation *does*, in fact take place. Only if the compensation is actually carried out so that not a single person remains a loser, can we still assert a gain in social utility. But *can* this compensation ever be carried out? In order to do so, everybody's utility scale would have to be investigated by the compensators. But from the very nature of utility scales this is an impossibility. Who knows what has happened to anyone's utility scale? The compensation principle is necessarily divorced from demonstrated preference, and once this occurs, it is impossible to find out what has happened to anyone's utility. The reason for the divorce is that the act of compensation is, necessarily, a unilateral gift *to* a person rather than an act *of* that person, and therefore it is impossible to estimate how much his utility has increased as compared to its decrease in some other situation. Only if a person is actually confronted with a *choice* between two alternatives can we say that he prefers one to the other.

Certainly, the compensators could not rely on questionnaires in a situation where everyone need only *say* that he has lost utility in order to receive compensation. And suppose someone proclaims that his sensibilities are so hurt by a certain change that no monetary reward could ever compensate him? The existence of one such person would annul any compensation attempt. But these problems necessarily occur when we leave the realm of demonstrated preference.

Hicks, "The Foundations of Welfare Economics," *Economic Journal* (December 1939): 706. For a criticism, see William J. Baumol, "Community Indifference," *Review of Economic Studies* (1946–1947): 44–48; Baumol, *Welfare Economics and the Theory of the State*, pp. 12ff.; Kemp, "Welfare Economics: A Stocktaking," pp. 246–50. For a summary of the discussion, see D.H. Robertson, *Utility and All That* (London: Allen and Unwin, 1952): pp. 29–35. The weakness in Robbins's accession to the Unanimity Rule is demonstrated by his endorsement of the compensating principle. Robbins, "Robertson on Utility and Scope."

The Social Welfare Function

Under the impact of criticisms far less thoroughgoing than the above, the compensation principle has been abandoned by most economists. There have been recent attempts to substitute another device—the "Social Welfare Function." But after a flurry of activity, this concept, originated by Professors Bergson and Samuelson, quickly struck rocky waters, and virtually sank under the impact of various criticisms. It came to be regarded as an empty and therefore meaningless concept. Even its founders have given up the struggle and concede that economists must import ethical judgments from outside economics in order to make policy conclusions.[50] Professor Rothenberg has made a desperate attempt to salvage the social welfare function by radically changing its nature, that is, by identifying it with an existing "social decision-making process." To uphold this shift, Rothenberg must make the false assumption that "society" exists apart from individuals and makes "its" own valuation. Furthermore, as Bergson has pointed out, this procedure abolishes welfare economics, since the function of the economist would be to observe empirically the social decision-making process at work and to pronounce its decisions as gains in "social utility."

The Economist as Adviser

Failing the establishment of policy conclusions through the compensation principle or the social welfare function, there is another very popular route to enable the economist to participate in policy formation while still remaining an ethically neutral scientist. This view holds that someone else may set the ends, while the economist is justified in telling that person (and in being hired by that person) the correct means for attaining these desired ends. Since the economist takes *someone else's* hierarchy of ends as given and only points out the means to attain them, he is alleged to remain ethically neutral

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[50]See Abram Bergson, "On the Concept of Social Welfare," *Quarterly Journal of Economics* (May 1954): 249; Paul A. Samuelson, "Welfare Economics; Comment," in *A Survey of Contemporary Economics*, B.F. Haley, ed. (Homewood, Ill.: R.D. Irwin, 1952), vol. 2, p. 37. Also Jerome Rothenberg, "Conditions for a Social Welfare Function," *Journal of Political Economy* (October 1953): 397; Sidney Schoeffler, "Note on Modern Welfare Economics," *American Economic Review* (December 1952): 881; I.M.D. Little, "Social Choice and Individual Values," *Journal of Political Economy* (October 1952): 422–32.

and strictly scientific. This viewpoint, however, is a misleading and fallacious one. Let us take an example suggested by a passage in Professor Philbrook's seminal article; a monetary economist advising the Federal Reserve System.[51] Can this economist simply take the ends set by the heads of this System and advise on the most efficient means to attain them? *Not unless the economist affirms these ends as being positively good*, that is, not unless he makes an ethical judgment. For suppose that the economist is convinced that the entire Federal Reserve System is pernicious. In that case, his best course may well be to advise that policy which would make the System highly *inefficient* in the pursuit of its ends. The economist employed by the System cannot, therefore, give any advice whatever without abandoning ethical neutrality. If he advises the System on the best way to achieve its ends, it must be logically inferred that he supports these ends. His advice involves no less an ethical judgment on his part if he chooses to "tacitly accept the decisions of the community as expressed through the political machinery."[52]

The End of Welfare Economics?

After twenty years of florid growth, welfare economics is once more confined to an even tighter Unanimity Rule. Its attempts to say anything about political affairs within the confines of this rule have been in vain.

The death of the New Welfare Economics has begun to be reluctantly recognized by all of its supporters, and each has taken turns in pronouncing its demise.[53] If the strictures advanced in this paper are conceded, the burial rites will be accelerated, and the corpse

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[51]Clarence Philbrook, "'Realism' in Policy Espousal," *American Economic Review* (December 1953): 846–59. The entire article is of fundamental importance in the study of economics and its relation to public policy.

[52]E.J. Mishan, "The Principle of Compensation Reconsidered," *Journal of Political Economy* (August 1952): 312. See especially the excellent note of I.M.D. Little, "The Scientist and the State," *Review of Economic Studies* (1949–50): 75–76.

[53]Thus, see the rather mournful discussion in the American Economic Association's second volume of the *Survey of Contemporary Economics*; Kenneth E. Boulding, "Welfare Economics," pp. 1–34; Melvin W. Reder, "Comment," pp. 34–36; and Samuelson, *The Empirical Implications of Utility Analysis*. Also see the articles by Schoeffler, Bergson, and Kemp cited above.

decently interred. Many New Welfare Economists understandably continue to grope for some way of salvaging something out of the wreckage. Thus, Reder suggests that economics make specific, piecemeal policy recommendations anyway. But surely this is only a despairing refusal to take the fundamental problems into account. Rothenberg tries to inaugurate a constancy assumption based on psychologizing about underlying basic personalities.[54] Aside from the fact that "basic" changes can take place at any time, economics deals with *marginal* changes, and a change is no less a change for being marginal. In fact, whether changes are marginal or basic is a problem for psychology, not praxeology. Bergson tries the mystical route of denying demonstrated preference, and claiming it to be possible that peoples values "really differed" from what they chose in action. He does this by adopting the "consistency"-constancy fallacy.

Does the Unanimity Rule then spell the end of *all* possible welfare economics, as well as the "old" and the "new" versions? Superficially, it would seem so. For if all changes must injure nobody, that is, if no people must feel worse off as a result of a change, what changes could pass muster as socially useful within the Unanimity Rule? As Reder laments: "Consideration of the welfare implications of envy, for example, make it impossible even to say that welfare will be increased by everyone having more of every commodity."[55]

Welfare Economics: A Reconstruction

Demonstrated Preference and the Free Market

It is the contention of this paper that the wake for all welfare economics is premature, and that welfare economics can be reconstructed with the aid of the concept of demonstrated preference. This reconstruction, however, will have no resemblance to either of the "old" or "new" edifices that preceded it. In fact, if Reder's thesis is correct, our proposed resurrection of the patient may be considered by many as more unfortunate than his demise.[56]

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[54]Jerome Rothenberg, "Welfare Comparisons and changes in Tastes," *American Economic Review* (December 1953): 888–90.

[55]Reder, "Comment," p. 35.

[56]"To a considerable extent, welfare (and related) theorizing of the 1930s and 1940s was an attempt to show the variety and importance of the circumstances under which *laissez-faire* was inappropriate." Ibid.

Demonstrated preference, as we remember, eliminates hypothetical imaginings about individual value scales. Welfare economics has until now always considered values as hypothetical valuations of hypothetical "social states." But demonstrated preference only treats values as revealed through chosen action.

Let us now consider exchanges on the free market. Such an exchange is voluntarily undertaken by both parties. Therefore, the very fact that an exchange takes place demonstrates that both parties benefit (or more strictly, *expect* to benefit) from the exchange. The fact that both parties chose the exchange demonstrates that they both benefit. The free market is the name for the array of all the voluntary exchanges that take place in the world. Since every exchange demonstrates a unanimity of benefit for both parties concerned, we must conclude that *the free market benefits all its participants*. In other words, welfare economics can make the statement that the free market increases social utility, while still keeping to the framework of the Unanimity Rule.[57]

But what about Reder's bogey: the envious man who hates the benefits of others? To the extent that he himself has participated in the market, to that extent he reveals that he likes and benefits from the market. And we are not interested in his opinions about the exchanges made by *others*, since his preferences are not demonstrated through action and are therefore irrelevant. How do we *know* that this hypothetical envious one loses in utility because of the exchanges of others? Consulting his verbal opinions does not suffice, for his proclaimed envy might be a joke or a literary game or a deliberate lie.

We are led inexorably, then, to the conclusion that the processes of the free market always lead to a gain in social utility. And we can say this with absolute validity as economists, without engaging in ethical judgments.

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[57]Haavelmo criticizes the thesis that the free market maximizes social utility on the grounds that this "assumes" that the individuals "somehow get together" to make an optimal decision. But the free market is precisely the method by which the "get together" takes place! See Trygve Haavelmo, "The Notion of Involuntary Economic Decision," *Econometrica* (January 1950): 8.

The Free Market and the "Problem of Distribution"

Economics, in general, and welfare economics, in particular, have been plagued with the problem of distribution. It has been maintained, for example, that assertions of increased social utility on the free market are all very well, but only within the confines of assuming a given distribution of income.[58] Since changes in the distribution of income seemingly injure one person and benefit another, no statements, it is alleged, can be made about social utility with respect to changes in distribution. And income distribution is always changing.

On the free market, however, there *is* no such thing as a separate "distribution." A man's monetary assets have been acquired precisely because his or his ancestors' services have been purchased by others on the free market. There is no distributional process apart from the production and exchange processes of the market; hence the very concept of "distribution" becomes meaningless on the free market. Since "distribution" is simply the result of the free exchange process, and since this process benefits all participants in the market and increases social utility, it follows directly that the distributional results of the free market also increase social utility.

The strictures of the critics do apply, however, to cases of State action. When the State takes from Peter and gives to Paul it is effecting a separate *distribution* process. Here, there does exist a process *separate* from production and exchange, and hence the concept becomes meaningful. Moreover, such State action obviously *and demonstrably* benefits one group and injures another, thus violating the Unanimity Rule.

The Role of the State

Until quite recently, welfare economics has never analyzed the role of the State. Indeed, economics in general has never devoted much attention to this fundamental problem. Specific problems, such as public finance, or price controls, have been investigated, but the State itself has been a shadowy figure in the economic literature. Usually, it has vaguely been considered as representing "society" or "the public in some way." "Society," however, is not a real entity; it is

[58]It would be more correct to say given distribution of money *assets*.

only a convenient short-hand term for an array of all existing individuals.[59] The largely unexplored area of the State and State actions, however, can be analyzed with the powerful tools of Demonstrated Preference and the Unanimity Rule.

The State is distinguished from all other institutions in society in two ways: (1) it and it alone can interfere by the use of violence with actual or potential market exchanges of other people; and (2) it and it alone obtains its revenues by a compulsory levy, backed by violence. No other individual or group can legally act in these ways.[60] Now what happens when the State, or a criminal, uses violence to interfere with exchanges on the market? Suppose that the government prohibits A and B from making an exchange they are willing to make. It is clear that the utilities of both A and B have been lowered, for they are prevented by threat of violence from making an exchange that they otherwise would have made. On the other hand, there has been a gain in utility (or at least an anticipated gain) for the government officials imposing this restriction, otherwise they would not have done so. As economists, we can therefore say nothing about social utility in this case, since some individuals have demonstrably gained and some demonstrably lost in utility from the governmental action.

The same conclusion follows in those cases where the government forces C and D to make an exchange which they otherwise would not have made. Once again, the utilities of the government officials gain. And *at least one* of the two participants (C or D) lose in utility, because at least one would not have wanted to make the exchange in the absence of governmental coercion. Again, economics can say nothing about social utility in this case.[61]

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[59]On this fallacy of methodological collectivism, and the broader fallacy of conceptual realism, see the excellent discussion in Hayek, *Counter Revolution of Science*, pp. 53ff.

[60]*Criminals* also act in these ways, but they cannot do so legally. For the purpose of praxeologic rather than legal analysis, the same conclusions apply to both groups.

[61]We cannot discuss here the praxeological analysis of general economics which shows that, in the long run, for many acts of coercive interference, the coercer himself loses in utility.

We conclude therefore that *no government interference with exchanges can ever increase social utility*. But we can say more than that. It is the essence of government that it alone obtains its revenue by the compulsory levy of taxation. All of its subsequent acts and expenditures, whatever their nature, rest on this taxing power. We have just seen that whenever government forces anyone to make an exchange which he would not have made, this person loses in utility as a result of the coercion. But taxation is just such a coerced exchange. If everyone would have paid just as much to the government under a system of voluntary payment, then there would be no need for the compulsion of taxes. Given the fact that coercion is used for taxes, therefore, and since all government actions rest on its taxing power, we deduce that: *no act of government whatever can increase social utility*.

Economics, therefore, without engaging in any ethical judgment whatever, and following the scientific principles of the Unanimity Rule and Demonstrated Preference, concludes: (1) that the free market always increases social utility; and (2) that no act of government can ever increase social utility. These two propositions are the pillars of the reconstructed welfare economics.

Exchanges between persons can take place either voluntarily or under the coercion of violence. There is no third way. If, therefore, free market exchanges always increase social utility, while no coerced exchange or interference can increase social utility, we may conclude that the maintenance of *a free and voluntary market "maximizes" social utility* (provided we do not interpret "maximize" in a cardinal sense).

Generally, even the most rigorously *Wertfrei* economists have been willing to allow themselves one ethical judgment: they feel free to recommend any change or process that increases social utility under the Unanimity Rule. Any economist who pursues this method would have to (a) uphold the free market as always beneficial, and (b) refrain from advocating any governmental action. In other words, he would have to become an advocate of "*ultra*" *laissez-faire*.

Laissez-faire Reconsidered

It has been quite common to scoff at the French "optimist" *laissez-faire* school of the nineteenth century. Usually, their welfare economic analysis has been dismissed as naive prejudice. Actually, however, their writings reveal that their *laissez-faire* conclusions were

post-judices—were judgments *based* on their analysis, rather than preconceptions of their analysis.[62] It was the discovery of the general social benefit from free exchange that led to the rhapsodies over the free exchange process in the works of such men as Frédéric Bastiat, Edmond About, Gustave de Molinari, and the American, Arthur Latham Perry. Their analyses of State action were far more rudimentary (except in the case of Molinari), but their analyses generally needed only the ethical presumption in favor of social utility to lead them to a pure *laissez-faire* position.[63] Their treatment of exchange may be seen in this passage from the completely neglected Edmond About:

> Now what is admirable in exchange is that it benefits the two contracting parties. . . . Each of the two, by giving what he has for that which he has not, makes a good bargain. . . . This occurs at every free and straightforward exchange. . . . In fact, whether you sell, whether you buy, you perform an act of preference. No one constrains you to give over any of your things for the things of another.[64]

The analysis of free exchange underlying the *laissez-faire* position has suffered general neglect in economics. When it is considered, it is usually dismissed as "simple." Thus, Hutchison calls the idea of exchange as mutual benefit "simple"; Samuelson calls it

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[62]Lionel Robbins's *The Theory of Economic Policy in English Classical Political Economy* (London: Macmillan, 1952) is devoted to the thesis that the English classical economists were really "scientific" because they did not uphold *laissez-faire*, while the French optimists were dogmatic and "metaphysical" because they did. To uphold this, Robbins abandons his praxeological approach of twenty years before, and adopts positivism: "The final test whether a statement is metaphysical (sic) or scientific is . . . whether it argues dogmatically a priori or by way of appeal to experience." Naturally, Robbins cites examples from the physical sciences to bolster this fallacious dichotomy. Ibid., pp. 23–24.

[63]Bastiat's writings are well known, but his "welfare" analysis was generally inferior to that of About or Molinari. For a brilliant analysis of State action, see Gustave de Molinari, *The Society of Tomorrow* (New York: G.P. Putnam and Sons, 1904), pp. 65–96.

[64]Edmond About, *Handbook of Social Economy* (London: Straham, 1872), p. 104. Also, ibid., pp. 101–12; and Arthur Latham Perry, *Political Economy*, 21st ed. (New York: Charles Scribners' Sons, 1892), p. 180.

"unsophisticated." Simple is perhaps it, but simplicity *per se* is hardly a liability in science. The important consideration is whether the doctrine is correct; if it is correct, then Occam's Razor tells us that the simpler it is, the better.[65]

The rejection of the simple seems to have its root in the positivist methodology. In physics (the model of positivism), the task of science is to go beyond common-sense observation, building a complex structure of explanation of the common-sense facts. Praxeology, however, begins with the common-sense truths as its *axioms*. The laws of physics need complicated empirical testing; the axioms of praxeology are known as obvious to all upon reflection. As a result, positivists are uncomfortable in the presence of universal truth. Instead of rejoicing in the ability to ground knowledge on universally accepted truth, the positivist rejects it as simple, vague, or "naïve."[66]

Samuelson's only attempt to refute the l*aissez-fare* position was to refer briefly to the allegedly classic refutation by Wicksell.[67] Wicksell, however, also dismissed the approach of the "French harmony economists" without argument, and went on to criticize at length the far weaker formulation of Léon Walras. Walras tried to prove "maximum utility" from free trade in the sense of an interpersonally cardinal utility and thus left himself wide open to refutation.

Furthermore, it should be stressed that the theorem of maximum social utility applies not to any type of "perfect" or "pure" competition, or even to "competition" as against "monopoly." It applies simply to any voluntary exchange. It might be objected that a voluntary cartel's action in raising prices makes many consumers worse off, and therefore that assertion of the benefits of voluntary exchange would have to exclude cartels. It is not possible, however, for an observer scientifically to compare the social utilities of results on the free market

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[65]Terence W. Hutchison, *A Review of Economic Doctrines, 1870–1929*, p. 282; Samuelson, *Foundations of Economic Analysis*, p. 204.

[66]For an example of this attitude, see the critique of Hayek's *Counter Revolution of Science* by May Brodbeck, in "On the Philosophy of the Social Sciences," *Philosophy of Science* (April 1954). Brodbeck complains that the praxeologic axioms are not "surprising"; if she pursued the analysis, however, she might find the conclusions surprising enough.

[67]Knut Wicksell, *Lectures on Political Economy* (London: Routledge and Kegan Paul, 1934), vol. 1, pp. 72ff.

from one period of time to the next. As we have seen above, we cannot determine a man's value-scales over a period of time. How much more impossible for all individuals! Since we cannot discover people's utilities over time, we must conclude that whatever the institutional conditions of exchange, however large or small the number of participants on the market, the free market at any time will maximize social utility. For all the exchanges are exchanges effected voluntarily by all parties. Then, suppose some producers voluntarily form a cartel in an industry. This cartel makes its exchanges in Period 2. Social utility is again maximized, for again no one's exchanges are being altered by coercion. If, in Period 2, the government should intervene to prohibit the cartel, it could not increase social utility since the prohibition demonstrably injures the producers.[68]

The State as a Voluntary Institution: A Critique

In the development of economic thought, far more attention has been paid to analysis of free exchange than to State action. Generally, as we have indicated, the State has simply been assumed to be a voluntary institution. The most common assumption is that the State is voluntary because all government must rest on majority consent. If we adhere to the Unanimity Rule, however, it is obvious that a majority is not unanimity, and that therefore economics cannot consider the State as voluntary on this ground. The same comment applies to the majority voting procedures of democracy. The man who votes for the losing candidate, and even more the man who abstains from voting, can hardly be said voluntarily to approve of the action of the government.[69]

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[68]It is also possible to argue, on *general* economic, rather than welfare-economic, grounds, that a voluntary cartel action, *if profitable*, will benefit consumers. In that case, consumers as well as producers would be injured by governmental outlawry of the cartel. As we have indicated above, *welfare* economics demonstrates that no governmental action can increase social utility. *General* economics demonstrates that, in many instances of government actions, even those who immediately benefit lose in the long run.

[69]Schumpeter is properly scornful when he says: "The theory which construes taxes on the analogy of club dues or of purchase of services of, say, a doctor only proves how far removed this part of the social sciences is from scientific habits of mind." Joseph A. Schumpeter, *Capitalism, Socialism, and*

In the last few years, a few economists have begun to realize that the nature of the State needs careful analysis. In particular, they have realized that welfare economics must prove the State to be in some sense voluntary before it can advocate any State action whatever. The most ambitious attempt to designate the State as a "voluntary" institution is the work of Professor Baumol.[70] Baumol's "external economy" thesis may be put succinctly as follows: certain wants are by their nature "collective" rather than "individual." In these cases, every individual will rank the following alternatives on his value scale: In (A) he would most prefer that *everyone but himself* be coerced to pay for the satisfaction of the group want (for example, military protection, public parks, dams, and so on). But since this is not practicable, he must choose between alternatives B and C. In (B) *no one* is forced to pay for the service, in which case the service will probably not be provided since each man will tend to shirk his share; in (C) *everyone*, including the particular individual himself, is forced to pay for the service. Baumol concludes that people will pick C; hence the State's activities in providing these services are "really voluntary." Everyone cheerfully chooses that he be coerced.

This subtle argument can be considered on many levels. In the first place, it is absurd to hold that "voluntary coercion" can be a demonstrated preference. If the decision were truly voluntary, no tax coercion would be necessary—people would voluntarily and publicly agree to pay their share of contributions to the common project. Since they are all supposed to prefer getting the project to not paying for it and not getting it, they are then really *willing* to pay the tax-price to obtain the project. Therefore, the tax coercion apparatus is not necessary, and all people would bravely, if a bit reluctantly, pay what they are "supposed" to without any coercive tax system.

Second, Baumol's thesis undoubtedly is true for the *majority*, since the majority, passively or eagerly, must support a government if it is to survive any length of time. But even if the majority are willing to coerce themselves in order to coerce others (and perhaps tip

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Democracy (New York: Harper and Brothers, 1942), p. 198. For a realistic analysis see Molinari, *The Society of Tomorrow*, pp. 87–95.

[70]See William J. Baumol, "Economic Theory and the Political Scientist," *World Politics* (January 1954): 275–77; and Baumol, *Welfare Economics and the Theory of the State*.

the balance of coercion *against* the others), this proves nothing for welfare economics, which must rest its conclusions on *unanimity*, not majority, rule. Will Baumol contend that *everyone* has this value ordering? Isn't there *one* person in the society who prefers freedom for all to coercion over all? If one such person exists, Baumol can no longer call the State a voluntary institution. On what grounds, *a priori* or empirical, can anyone contend that no such individual exists?[71]

But Baumol's thesis deserves more detailed consideration. For even though he cannot establish the existence of voluntary coercion, if it is really true that certain services simply cannot be obtained on the free market, then this would reveal a serious weakness in the free-market "mechanism." Do cases exist where only coercion can yield desired services? At first glance, Baumol's "external economy" grounds for an affirmative answer seem plausible. Such services as military protection, dams, highways, and so on, are important. People desire that they be supplied. Yet wouldn't each person tend to slacken his payment, hoping that the others would pay? But to employ this as a rationale for State provision of such services is a question-begging example of circular reasoning. For this peculiar condition holds only and precisely because the State, not the market, provides these services! The fact that the State provides a service means that, unlike the market, its *provision of the service is completely separated from its collection of payment*. Since the service is generally provided free and more or less indiscriminately to the citizens, it naturally follows that every individual—assured of the service—will try to shirk his taxes. For, unlike the market, his individual tax payment brings him nothing directly. And this condition cannot be a justification for State action; for it is only the *consequence* of the existence of the State action itself.

But perhaps the State must satisfy some wants because these wants are "collective" rather than "individual"? This is Baumol's second line of attack. In the first place, Molinari has shown that the existence of collective wants does not necessarily imply State action. But, furthermore, the very concept of "collective" wants is a dubious

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[71]Galbraith, in effect, does make such an assumption, but obviously without adequate basis. See John K. Galbraith, *Economics and the Art of Controversy* (New Brunswick, N.J.: Rutgers University Press, 1955), pp. 77–78.

one. For this concept must imply the existence of some existent collective entity who does the wanting! Baumol struggles against conceding this, but he struggles in vain. The necessity for assuming such an entity is made clear in Haavelmo's discussion of "collective action," cited favorably by Baumol. Thus, Haavelmo grants that deciding on collective action "requires a way of thinking and a power to act which are outside the functional sphere of any individual group as such."[72]

Baumol attempts to deny the necessity for assuming a collective entity by stating that some services can be financed only jointly, and will serve many people jointly. Therefore, he argues that individuals on the market cannot provide these services. This is a curious position indeed. For all large-scale businesses are "jointly" financed with huge aggregations of capital, and they also serve many consumers, often jointly. No one maintains that private enterprise cannot supply steel or automobiles or insurance because they are "jointly" financed. As for joint consumption, in one sense no consumption can be joint, for only individuals exist and can satisfy their wants, and therefore everyone must consume separately. In another sense, almost all consumption is "joint." Baumol, for example, asserts that parks are an example of "collective wants" jointly consumed, since many individuals must consume them. Therefore, the government must supply this service. But going to a theater is even more joint, for all must go at the same time. Must all theaters therefore be nationalized and run by the government? Furthermore, in a broad view, all modern consumption depends on mass production methods for a wide market. There are no grounds by which Baumol can separate certain services and dub them "examples of interdependence" or "external economies." What individuals could buy steel or automobiles or frozen foods, or almost anything else, if enough other individuals did not exist to demand them and make their mass-production methods worthwhile? Baumollian interdependencies are all around us, and there is no rational way to isolate a few services and call them "collective."

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[72]Haavelmo, "The Notion of Involuntary Economic Decision." Yves Simon, cited favorably by Rothenberg, is even more explicit, postulating a "public reason" and a "public will" as contrasted to individual reasonings and wills. See Yves Simon, *Philosophy of Democratic Government* (Chicago: University of Chicago, 1951); Rothenberg, "Conditions," pp. 402–03.

A common argument related to, though more plausible than, Baumol's thesis is that certain services are so vital to the very existence of the market that they must be supplied collectively outside the market. These services (protection, transportation, and so on) are so basic, it is alleged, that they permeate market affairs and are a prior necessary condition for its existence. But this argument proves far too much. It was the fallacy of the classical economists that they considered goods in terms of large *classes*, rather than in terms of *marginal units*. All actions on the market are marginal, and this is precisely the reason that valuation and imputation of value-productivity to factors can be effected. If we start dealing with whole classes rather than marginal units, we can discover all sorts of activities which are necessary prerequisites of, and vital to, all market activity; land, room, food, clothing, shelter, power, and so on—and even paper! Must all of these be supplied by the State and the State only?

Stripped of its many fallacies, the whole "collective wants" thesis boils down to this: certain people on the market will receive benefits from the action of others without paying for them.[73] This is the long and short of the criticism of the market, and this is the only relevant "external economy" problem.[74] A and B decide to pay for the building of a dam for their uses; C benefits though he did not pay. A and B educate themselves at their expense and C benefits by being able to deal with educated people, and so on. This is the problem of the Free Rider. Yet it is difficult to understand what the hullabaloo is all about. Am I to be specially taxed because I enjoy the sight of my neighbor's garden without paying for it? A's and B's purchase of a good reveals that *they* are willing to pay for it; if it indirectly benefits C as well, no one is the loser. If C feels that he would be deprived of the benefit if only A and B paid, then he is free to contribute too. In any case, all the individuals consult their own preferences in the matter.

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[73]See the critique of a similar position of Spencer's by "S.R.," "Spencer As His Own Critic," *Liberty* (June 1904).

[74]The famous "external diseconomy" problems (noise, smoke nuisance, fishing, and so on) are really in an entirely different category, as Mises has shown. These "problems" are due to insufficient defense of private property against invasion. Rather than a defect of the free market, therefore, they are the results of invasions, of property, invasions which are ruled out of the free market by definition. See Mises, *Human Action*, pp. 650–56.

In fact, we are *all* free riders on the investment, and the technological development, of our ancestors. Must we wear sackcloth and ashes, or submit ourselves to State dictation, because of this happy fact?

Baumol and others who agree with him are highly inconsistent. On the one hand, action cannot be left up to voluntary individual choice because the wicked free rider might shirk and obtain benefits without payment. On the other hand, individuals are often denounced because people will not *do enough* to benefit free riders. Thus, Baumol criticizes investors for not violating their own time-preferences and investing more generously. Surely, the sensible course is neither to penalize the free rider nor to grant him special privilege. This would also be the only solution consistent with the unanimity rule and demonstrated preference.[75]

Insofar as the "collective want" thesis is not the problem of the Free Rider, it is simply an ethical attack on individual valuations, and a desire by the economist (stepping into the role of an ethicist) to substitute his valuations for those of other individuals in deciding the *latter's* actions. This becomes clear in the assertion by Suranyi-Unger: "he (an individual) may be led by a niggardly or thoughtless or frivolous evaluation of utility and disutility and by a corresponding low degree or complete absence of group responsibility."[76]

Tibor Scitovsky, while engaging in an analysis similar to Baumol's, also advances another objection to the free market based on what he calls "pecuniary external economies."[77] Briefly, this conception suffers

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[75]In a good, though limited, criticism of Baumol, Reder points out that Baumol completely neglects voluntary social organizations formed by individuals, for he assumes the State to be the only social organization. This error may stem partly from Baumol's peculiar definition of "individualistic" as meaning a situation where no one considers the effects of his actions on anyone else. See Melvin W. Reder, "Review of Baumol's *Welfare Economics and the Theory of the State*," *Journal of Political Economy* (December 1953): 539.

[76]Theo Suranyi-Unger, "Individual and Collective Wants," *Journal of Political Economy* (February 1948): 1–22. Suranyi-Unger also employs such meaningless concepts as the "aggregate utility" of the "collectivized want satisfaction."

[77]Tibor Scitovsky, "Two Concepts of External Economies," *Journal of Political Economy* (April 1954): 144–51.

from the common error confusing the general (and unattainable!) equilibrium of the evenly rotating economy with an ethical ideal and therefore belaboring such ever-present phenomena as the existence of profits as departures from such an ideal.

Finally, we must mention the very recent attempts of Professor Buchanan to designate the State as a voluntary institution.[78] Buchanan's thesis is based on the curious dialectic that majority rule in a democracy is really unanimity because majorities can and do always shift! The resulting pulling and hauling of the political process, because obviously not irreversible, are therefore supposed to yield a social unanimity. The doctrine that endless political conflict and stalemate really amount to a mysterious social unanimity must be set down as a lapse into a type of Hegelian mysticism.[79]

Conclusion

In his brilliant survey of contemporary economics, Professor Bronfenbrenner described the present state of economic science in the gloomiest possible terms.[80] "Wilderness" and "hash" were typical epithets, and Bronfenbrenner ended his article in despair by quoting the famous poem *Ozymandias*. Applied to currently fashionable theory, his attitude is justified. The 1930s was a period of eager activity and seemingly pathbreaking advances in economic thought. Yet one by one, reaction and attenuation have set in, and in the mid-1950s the high hopes of twenty years ago are either dying or fighting desperate rearguard action. None of the formerly new approaches any longer inspires fresh theoretical contributions. Bronfenbrenner

[78]See James M. Buchanan, "Social Choice, Democracy, and Free Markets," *Journal of Political Economy* (April 1954): 114–23; and Buchanan, "Individual Choice in Voting and the Market," *Journal of Political Economy* (August 1954): 334–43. In many other respects, Buchanan's articles are quite good.

[79]How flimsy this "unanimity" is, even for Buchanan, is illustrated by the following very sensible passage: "a dollar vote is never overruled; the individual is never placed in the position of being a member of dissenting minority"—as he is in the voting process (Buchanan, "Individual Choice in Voting and the Market," p. 339). Buchanan's approach leads him so far as to make a positive virtue out of inconsistency and indecision in political choices.

[80]Bronfenbrenner, "Contemporary Economics Resurveyed."

specifically mentions in this connection the imperfect competition and the Keynesian theories, and justly so. He could also have mentioned utility and welfare theory. For the mid-1930s saw the development of the Hicks-Allen indifference curve analysis and the New Welfare Economics. Both of these theoretical revolutions have been enormously popular in the upper reaches of economic theory; and both are now crumbling.

The contention of this paper is that while the formerly revolutionary and later orthodox theories of utility and welfare deserve an even speedier burial than they have been receiving, they need not be followed by a theoretical vacuum. The tool of Demonstrated Preference, in which economics deals only with preference as demonstrated by real action, combined with a strict Unanimity Rule for assertions of social utility, can serve to effect a thoroughgoing reconstruction of utility and welfare economics. Utility theory can finally be established as a theory of ordinal marginal utility. And welfare economics can become a vital *corpus* again, even though its new personality might not attract its previous creators. It must not be thought that we have, in our discussion of welfare economics, been attempting to set any ethical or political program. On the contrary, the proposed welfare economics has been put forward without inserting ethical judgments. Economics by itself and standing alone cannot establish an ethical system, and we must grant this regardless of what philosophy of ethics we hold. The fact that the free market maximizes social utility, or that State action cannot be considered voluntary, or that the *laissez-faire* economists were better welfare analysts than they are given credit for, in itself implies no plea for *laissez-faire* or for any other social system. What welfare economics does is to present these conclusions to the framer of ethical judgments as part of the data for his ethical system. To the person who scorns social utility or admires coercion, our analysis might furnish powerful arguments for a policy of thoroughgoing Statism.

Section Three

Property and the Public Sector

18

The Politics of Political Economists

In the course of his interesting discussion of "The Politics of Political Economists," Professor Stigler challenges the alleged view of Professor Mises that "economic statistics, or more generally quantitative—economics, generates a radical political viewpoint."[1] Stigler asserts that the empirical student acquires a "real feeling" for the functioning of an economic system, and "has had the complexities of the economy burned into his soul." Without going into the question of Mises's precise viewpoint on this issue, I think it important to note that Stigler has overlooked several fundamental considerations.

In the first place, statistics are desperately needed for any sort of government planning of the economic system. In a free market economy, the individual business firm has little or no need of statistics. It need only know its prices and costs. Costs are largely discovered internally within the firm and are not the general data of the economy which we usually refer to as "statistics." The "automatic" market, then, requires virtually no gathering of statistics; government intervention, on the other hand, whether piecemeal or fully socialist, could do literally nothing without extensive ingathering of masses of statistics. Statistics are the bureaucrat's only form of economic knowledge, replacing the intuitive, "qualitative" knowledge of the entrepreneur, guided only by the quantitative profit-and-loss test.[2]

Originally appeared as "The Politics of Political Economists: Comment" in *Quarterly Journal of Economics* 74, no. 4 (November 1960): 659–65.

[1]George Stigler, "The Politics of Political Economists," *Quarterly Journal of Economics* 73 (November 1959): 529.

[2]On the type of knowledge required of the entrepreneur in the market economy, see F.A. Hayek, *Individualism and the Economic Order* (Chicago: University of Chicago Press, 1948), chaps. 4 and 2.

Accordingly, the drive for government intervention, and the drive for more statistics, have gone hand in hand.[3]

The enormous expansion of governmental activity in the gathering and disseminating of statistics in the last twenty-five years, is surely more than coincidentally related to the similar expansion of the role of government in regulating and manipulating the economy. One of the leading authorities on the growth of government expenditures has put it this way:

> Advance in economic science and statistics improved our knowledge of interstate and intrastate differences in needs and capacities and may have helped stimulate the system of state and federal grants-in-aid. It strengthened belief in the possibilities of dealing with social problems by collective action. It made for increase in the statistical and other fact-finding activities of government.[4]

We need not detail here the extensive use that has been made of national income and gross national product statistics, as well as other statistical measures, in the attempts of the federal government at combating business cycles or unemployment.

Nor is this just a contemporary story. An authoritative work on British government puts the case thus:

> the minor role of government during the nineteenth century reflects more than the absence of violent economic disruption; it also reflects the infancy of the economic and social sciences. Compared with recent decades, the volume of systematic information about social conditions was very small, which meant that the existence of problems was hard to establish persuasively. . . . If the volume of unemployment is unknown, the gravity of the problem is in doubt. . . .

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[3]In this connection, we may note Professor Hutchison's distinction between Carl Menger's stress on the beneficent, unplanned, "unreflected" phenomena of society (which, of course, include the free market), and the growth of "social self-consciousness" and government planning. To Hutchison, a prominent component of "social self-consciousness" is social and economic statistics. T.W. Hutchison, *A Review of Economic Doctrines, 1870–1929* (Oxford: The Clarendon Press, 1953), pp. 150–51, 427.

[4]Solomon Fabricant, *The Trend of Government Activity in the United States since 1900* (New York: National Bureau of Economic Research, 1952), p. 143.

> The accumulation of factual information about social conditions and the development of economics and the social sciences increased the pressure for government intervention. . . . Surveys like Charles Booth's *Life and Labour of the People in London* revealed conditions which shocked public opinion in the late eighties and nineties. As statistics improved and students of social conditions multiplied, the continued existence of such conditions was kept before the public. Increasing knowledge of them aroused influential circles and furnished working class movements with factual weapons.[5]

Surely the role of the Fabian Society's industrious empirical studies in furthering the cause of socialism in Great Britain is too well known to need stressing here.

On the continent and in America in the late nineteenth century, it is well known that the rebels against *laissez-faire* and the classical political economy stressed their replacement with induction from economic history and statistics. That was the goal of the German Historical School and its *Verein für Sozialpolitik*, and of the young German-trained exponents of the "new political economy" of government intervention in the 1870s and 1880s.[6] One of their leaders, Richard T. Ely, who called the new approach the "look and see"

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[5]Moses Abramovitz and Vera F. Eliasberg, *The Growth of Public Employment in Great Britain* (Princeton, N.J.: National Bureau of Economic Research, 1957), pp. 22–23, 30.

[6]Thus, the new school

> found the deductive method of reasoning inadequate for its purposes. It championed the inductive method. . . . It rejected all a priori principles and looked to history and statistics to provide the facts of economic life. With the information thus obtained, the young economists approached economic problems in a pragmatic spirit, judging each case on its individual merits. In this way, they sought to prevent economic science from degenerating into a few abstract formulas, divorced from the realities of the age. (Sidney Fine, *Laissez-Faire and the General-Welfare State* [Ann Arbor: University of Michigan Press, 1956], p. 204)

Also see the principles of the new school as presented in Joseph Dorfman, "The Role of the German Historical School in American Economic Thought," *American Economic Review, Papers and Proceedings* XLV (May 1955): 21.

method, made it clear that the aim of fact-gathering was to "mold the forces at work in society and to improve existing conditions"; they believed that as economists they had a responsibility for "shaping the character of the national economy."[7] And let us not overlook the eminent interventionist sociologist Lester Frank Ward, whose proposed "scientific," "positive," planned economy, would consist of a "social engineering" based on statistical information fed from all parts of the country into a central bureau of statistics.[8]

Nor was it only abstract speculators who expressed such views. Statisticians themselves participated in this movement. As early as 1863, Samuel B. Ruggles, American delegate to the International Statistical Congress in Berlin, declared that "statistics are the very eyes of the statesman, enabling him to survey and scan with clear and comprehensive vision the whole structure and economy of the body politic." One of the founders of the *Verein für Sozialpolitik* was the famous statistician Ernst Engel, head of the Royal Statistical Bureau of Prussia.[9] And Carroll D. Wright, one of the early Commissioners of Labor in the United States and a man greatly influenced by Engel, urged the collection of statistics of unemployment because he wanted to find a remedy (presumably via government action). Wright hailed the new German School as including men of all lands "who seek by legitimate means, and without revolution, the

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[7]Fine, *Laissez-Faire and the General-Welfare State,* p. 207. We might add that the French laissez-faire economist Maurice Block attacked the German Historical School and their followers as "empirics" seeking to replace principle by sentiment and holding that "the state . . . should conduct everything, direct everything, decide everything." Dorfman, "The Role of the German Historical School in American Economic Thought," p. 20. And recently Professor Hildebrand has commented, on the inductive emphasis of the German School, that "perhaps there is, then, some connection between this kind of teaching and the popularity of crude ideas of physical planning in more recent times." George H. Hildebrand, "International Flow of Economic Ideas—Discussion," *American Economic Review, Papers and Proceedings* XLV (May 1955): 37. Also see F.A. Hayek, "History and Politics," in F.A. Hayek, ed., *Capitalism and the Historians* (University of Chicago Press, 1954), p. 23.

[8]Fine, *Laissez-Faire and the General-Welfare State*, p. 258.

[9]See Dorfman, "The Role of the German Historical School in American Economic Thought," p. 18.

amelioration of unfortunate industrial and social relations." Henry Carter Adams, a student of Engel's, who established the Statistical Bureau of the Interstate Commerce Commission, believed that "ever-increasing statistical activity by the government was essential not only for the sake of controlling naturally monopolistic industries, but also for the efficient functioning of competition wherever possible."[10] And certainly one of the great spurs toward constructing index numbers of wholesale and other prices was the desire to have government stabilize the price level.[11]

Unquestionably one of the prime founders of modern statistical inquiry in economics was Wesley C. Mitchell. There is no doubt that Mitchell aspired to lay the basis for "scientific" government planning. Thus:

> [quoting from Mitchell] . . . clearly the type of social invention most needed today is one that offers definite techniques through which the social system can be controlled and operated to the optimum advantage of its members." To this end he [Mitchell] constantly sought to extend, improve, and refine the gathering and compilation of data. . . . Mitchell believed that business-cycle analysis might indicate the means to the achievement of orderly social control of business activity.[12]

And:

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[10]Joseph Dorfman, *The Economic Mind in American Civilization* (New York: The Viking Press, 1949), vol. 3, pp. 172, 123. Dorfman notes that the accounting system of the Bureau devised by Adams "served as a model for the regulation of public utilities here and throughout the world." Dorfman, "The Role of the German Historical School in American Economic Thought," p. 23. We might also add that the first professor of statistics in the United States, Roland P. Falkner, was a devoted student of Engel's and a translator of the works of Engel's assistant, August Meitzen.

[11]"One of the greatest obstacles then standing in the way of stabilization was the prevalent idea that index numbers were unreliable. Until this difficulty could be met, stabilization could scarcely be expected to become a reality. In order to do my bit toward solving this problem, I wrote *The Making of Index Numbers* . . ." Irving Fisher, *Stabilised Money* (London: George Allen and Unwin, 1935), p. 383.

[12]Dorfman, *The Economic Mind in American Civilization*, vol. 4, pp. 76, 361.

> he [Mitchell] envisaged the great contribution that government could make to the understanding of economic and social problems if the statistical data gathered independently by various Federal agencies were systematized and planned so that the interrelationships among them could be studied. The idea of developing social statistics, *not merely as a record but as a basis for planning*, emerged early in his own work.[13]

The federal government's own account of the growth of its statistical agencies differs little from the above examples. The Bureau of the Budget, during President Eisenhower's not rabidly socialistic administration, explained the continued growth of federal statistics as follows:

> National growth and prosperity demanded an enlightened conduct of public affairs with the aid of factual information. The ultimate responsibility of the Federal Government for underwriting the health of the national economy has always been implicit in the American system.[14]

Then, speaking of the New Deal era after 1933, the Bureau added:

> A realization grew in the Congress and in high administration circles that sound and positive proposals to combat the depression required analysis based upon reliable information. As a result . . . statistical expansion was resumed at an accelerated pace.[15]

Suffice it then to say that a leading cause of the proliferation of governmental statistics is the need for statistical data in government economic planning. But the relationship works also in reverse: the growth of statistics, often developed originally for its own sake, ends by multiplying the avenues of government intervention and planning. In short, statistics do not have to be developed originally for politico-economic ends; their own autonomous development, directly or indirectly, opens up new fields for interventionists to

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[13]Lucy Sprague Mitchell, *Two Lives* (New York: Simon and Schuster, 1953), p. 363; my italics.

[14]Statement by the Bureau of the Budget, in *Economic Statistics*, Hearings Before the Subcommittee on Economic Statistics of the Joint Committee on the Economic Report, 83d Congress, 2d Session, July 12, 1954 (Washington, D.C.: United States Printing Office, 1954), pp. 10–12.

[15]Ibid.