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Distinguished Award in Political Economy

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The Frank E. Seidman Distinguished Award

In Political Economy

The Frank E. Seidman Distinguished Award in Political Economy was established in Memphis State University in memory of Frank E. Seidman by Mr. and Mrs. P.K. Seidman. An honorarium of ten thousand dollars will be given to an economist who has distinguished himself or herself by contributing internationally, in the judgement of his or her peers, to the interdisciplinary advancement of economic thought as it applies to the implementation of public policy. The first recipient was Gunnar Myrdal in 1974.

The purpose of the Award is to recognize and encourage economists who are attempting to extend their methodology into the interdependent areas of the other social sciences. It is applicable to the advancement of societal welfare when proper cognizance is given to environmental and institutional influences upon the economic behavior of the individual and groups. The basis for evaluation will be broad enough to encompass both the synthesis of existing economic thought and the pathbreaking development of new concepts.

For the purposes of this Award, the recipient shall be considered an economist by imputation from the quality and importance of his or her professional work and interests. The distinguished contribution must be judged to have satisfied the specific criteria which are stated in terms to reflect the basic objectives of the Award.

Arthur A. Bayer
Director

About This Publication

The recipient of the Award gives a paper which exemplifies their work. The purpose of this volume is to publish the remarks made by Professor Myrdal. In his paper, Professor Myrdal discusses the nature of Political Economy.

Published with Professor Myrdal's paper are other papers which were selected to complement and illustrate his remarks. The papers were originally submitted to the *Journal of Economic Criticism* but were selected for this volume because of their merit and because they are indicative of the sort of debate that the *Journal* intends to foster among those who would aspire to be Political Economists.

Roger K. Chisholm
Editor

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WHAT IS POLITICAL ECONOMY?

by
Gunnar Myrdal

Being the first recipient of *The Frank E. Seidman Distinguished Award in Political Economy*, it might not be out of order that tonight I focus my remarks on the problem: What is Political Economy? It so happens that this very question, and a multitude of methodological problems raised by it, has concerned me throughout my academic life. Taking it up at this banquet as topic for the paper which, according to the By-Laws of the Award, is my responsibility to deliver, will, I hope, "exemplify the recipient's work that is being recognized by the award," again to quote the By-Laws.

I

We all know that the academic discipline now commonly referred to as "economics" a couple of generations ago equally commonly was called "political economy." The first Chair I held at the University of Stockholm from 1933, as successor to Gustav Cassel, was named "Political Economy and Financial Science."

What did economists in earlier time mean by inserting the adjective "political"? And why was it dropped? Was the change only meant to be a rationalizing abridgement without logical significance? Or can there be found a deeper significance motivating the change? And when you now name me a "political economist," do you mean to imply that I belong to a different and specific species of economists?

As an idiomatic alteration of term, from political economy to economics, when it gradually occurred, it was seldom, if ever, discussed as a problem or even noted as signifying an important change in emphasis or direction of our work. Looking backward it seems to me, however, to have been important as a sign of a change in the pursuit of our work. It pertained to a fundamental difference in our approach when studying the economy.

II

Already from the beginning in the eighteenth and early nineteenth centuries, economists unhesitatingly saw themselves as political economists. They firmly believed it was part of their duty to draw policy conclusions. And they held that they were entitled to this on rational grounds, i.e., as logical inferences from their knowledge about the facts.

It is true that, at least since Senior and on, economists often emphasized that their "theory" did not permit them to draw policy conclusions. They so presented a formula that has been with us into present time. Then as now the principle was continually broken in actual practice.

Keeping, however, to the economists in the classical and neo-classical line—that is up till almost around the First World War—this contradiction in their thinking was more apparent than real. What they referred to as not possible to serve as basis for policy conclusions was their "theory" in a restricted sense.

From Ricardo and on, that theory was a very simplified deductive analysis built upon a few abstract assumptions; Senior reckoned four. Even though the attempt was to make the assumptions realistic so far as possible, they were aware of the need to encompass much detailed data on how various conditions actually were in order to formulate valid policy conclusions.

Thus, when John Stuart Mill tackled a more comprehensive task, he had only to tack on to the title of his book a phrase to make it plain that he was dealing with a field wider than that of pure economic theory: "Principles of Political Economy, With Some of Their Applications to Social Philosophy."

About the possibility of so enlarging the field of study, in order to be able to render policy prescriptions, they had no doubt.

In the meaning clarified by John Stuart Mill in his earliest works, the study of the economy is a "moral science," as in his view are all other social sciences. Crucial for this view, which established economic science as political economy, was the conception that there were objective values, that could be known as facts, could be observed and analyzed, and so laid as basis both for the study of all other facts and for rational policy conclusions.

This value basis for economic study was provided by the moral philosophy of that time, initially by the natural law philosophy and later by the utilitarian philosophy (which, as I have shown, was only a variation and reformulation of the natural law philosophy).

The neo-classical authors then refined this moral philosophy and, in particular, its foundation in the hedonistic associational psychology. As a matter of fact, the marginal theory of value from the eighteen-seventies stands out as giving the final finishing to the moral philosophy of utilitarianism. Many of the prominent economists of that era, for instance Sidgwick, also figure prominently in the pantheon of philosophers in that line of thought.

The "welfare theory" developed by the first generation of neo-classical authors had thus its logical foundation upon the utilitarian moral philosophy, which, in turn, rested on the hedonistic associational psychology. This development happened at about the same time that both the utilitarian moral philosophy and, in particular, the basic hedonistic associational psychology became abandoned by the professional philosophers and psychologists. The very apparent isolation of the economic science from the other social science disciplines and from philosophy as they developed, dates from that time.

III

Modern establishment economists have retained the welfare theory but have done their best to conceal and forget its foundation in a particular and now obsolete moral philosophy and an equally obsolete psychology. They have then succeeded in pursuing what appears to be an amoral economic theory, and they are proud of stressing this as "professionalism." They are not political economists as our forebearers.

I thought I had finally disposed of the modern welfare theory forty years ago by demonstrating the superficiality and logical inconsistency of this theory. But it grows like a malignant tumor. Hundreds of books and articles are produced every year on "welfare economics." But if the approach is not entirely meaningless, it has a meaning only in terms of a forlorn hedonistic psychology, and a utilitarian moral philosophy. I have always wondered why the psychologists and philosophers have left the economists alone and undisturbed in their futile exercise.

The trend toward narrow "professionalism" in contemporary establishment economics in regard to training, reading and, indeed, awareness of everything outside the narrow field they have staked out for their work, protects them from being disturbed by much knowledge about modern psychology and philosophy. And the relative neglect we can find in the curricula for the study of the history of economic science helps them to have an exaggerated belief in the newness of their own contributions to welfare economics, and often even protects them from grasping that what they are attempting is normative economic theory in disguise, but kept into such an abstract compartment that it has no relations with practical issues.

Those great economists who originally developed the hedonistic and utilitarian welfare theory—among them, Jevons, Sidgwick and Edgeworth—could work with conviction and in clear terms, since they knew what they were doing. They were not apt to skip over the basic psychological and philosophical assumptions implied in welfare theory. The contemporary welfare theorists mostly miss the historical perspective they should gain by intensive study of their predecessors and, at the same time, the awareness they could get by such studies of where the basic difficulties are buried.

Few attempts have been made by economists to study, empirically and in terms of modern psychology, people's behavior as income earners, consumers, savers and investors. What attempts there have been made outside our fraternity to carry out realistic psychological research about economic behavior, free from the assumptions of the old and new welfare theory, have been completely disregarded in establishment economics. The deeper reason for this neglect is, of course, that the results of such research cannot possibly be integrated into the conceptual framework of welfare theory of the inherited and still dominant type.

I may note in passing that the recent flourishing of welfare economics is closely related to the growing predilection for hyper-abstract theoretical models. Among the many implicit and not sufficiently scrutinized assumptions of these models, and sometimes even in their explicit structure, the objectivified welfare conception almost always plays a role. An institutional approach cannot so easily escape the human valuations that are at the same time objects of research and premises in research.

IV

In one sense, a student who is a political economist and thus recongnizes that economics is a moral science, works in the great tradition that began in the eighteenth century. When, at the same time, he discards the moral philosophy of utilitarianism and its implied hedonistic psychology, that traditionally was laid as a basis for political economy—and is a hidden and repressed assumption even for welfare theory in modern economics—as invalid and metaphysical, he has to account for what other valuational basis he has for his work.

Valuations are always with us. Disinterested research there has never been and can never be. Prior to answers there must be questions. There can be no view except from a viewpoint. In the questions raised and the viewpoint chosen valuations are implied.

Our valuations determine our approaches to a problem, the definition of concepts, the choice of models and the selection of our observations—in fact, the whole pursuit of a study from the beginning to end. When we remain unaware of the valuations basic to our research, this implies that we succeed to reason with one premise missing, which opens the door for biases.

In this situation I have argued, and in my own research tried to observe, the necessity in any scientific undertaking of stating clearly and explicitly, instrumental value premises. They are needed already for establishing relevant facts and not only for drawing policy conclusions. These explicit value premises cannot be chosen arbitrarily. They have to be relevant and significant for the society studied, logically consistent and feasible.

The use of explicit value premises serves three purposes. It determines in a rational way the statement of problems, the approach and the definition of concepts used in a study. It further lays a tenable, logical basis for reaching by rational reasoning policy conclusions. And it helps to purge as far as possible, the scientific investigation of distorting biases. The development of a sociology and social psychology of social science and scientists, which I have urged, would for this problem of biases have the importance of sharpening our awareness of the need for logical clarity created by defining and keeping to explicit value premises.

By working with specific and explicit value premises, we are not simply "expressing our own biases," as is often suggested. For biases are ordinarily not conscious to the researcher and are thus not under his control. A loose declaration of personal biases does not substitute for the argued choice of value premises.

V

The tremendous difficulties of realistically basing our value premises on prevalent valuations in the society we are studying should not be concealed.

Opinion research has by far not reached the level of incisiveness and conclusiveness needed for the use of their results for this purpose. Among other things, it has usually not even seriously attempted to separate valuations from beliefs in the opinions–the beliefs being regularly opportunistically distorted in order to conceal valuation conflicts.

Rational policy conclusions should, of course, be founded on the valuations people would have, if their beliefs about reality were more correct–if, to begin with, they knew as much as we. Moreover, those valuations should regularly concern a future situation, when it has changed, as foreseen by the researcher, but not by ordinary people. This all implies that in selecting our value premises, we are mainly forced to resort to impressionistic observations and to speculation.

Ideally, a study should be based on several alternative sets of value premises. This last requirement can for practical reasons seldom be met, except very partially. We should not forget that the value premises determine the approach and thereby the entire pursuit of a study. To work with several approaches at the same time would ordinarily tax our research resources beyond capacity.

In order not to complicate the work excessively, we must ordinarily select a single set of value premises as instrumental in a study. But we must not deceive ourselves on this point: the selection of the instrumental norm has material significance. We have given the one approach, signified by the selected set of value premises, a stragegically favorable position in a study.

I want to honestly to account for these difficulties, and I have often stressed that I do not pretend to have reached a final and fully satisfactory solution of the methodological problem of how to ascertain the value premises needed both in factual research and particularly when drawing policy conclusions.

Valuations are always implied in research, as I said, and they should not be brushed under the carpet. Ordinarily they are kept hidden and are mostly not conscious to the reasearcher himself. They are not under his control and this opens the door for arbitrariness and the creeping into research of systematic biases.

I, therefore, do insist that if we place ourselves under the discipline to spell out, in as definite terms as we can, a set of instrumental value premises–however they have been reached and whichever they may be–and if we allow them to determine our approach, the definitions of our concepts and the formulations of our theories, this represents an advance towards the goal of honesty, clarity, and effectiveness of research, and, particularly, towards defending ourselves against biases.

We have then, at least, spelled out the role that valuations have actually played in a study. Anyone wishing to challenge the choice of value premises is relieved of the cumbersome task of discovering, through immanent criticism, the otherwise only implied valuations and the way in which they have determined the approach and the results.

VI

When in many fields of study I have tried to apply this insight, and worked under the discipline of stating my value premises explicitly and justifying thier selection, I feel myself, in one sense, to be working in the great tradition, that began in the eighteenth century. As it implies that economic policy conclusions can rationally be inferred from these value premises, and from the facts ascertained from the viewpoint of the same value premises, economic science has been restored to a moral science in the meaning of John Stuart Mill—though deprived of the reliance the old political economists had on the existence of an objective system of values.

Even in other respects I feel aligned to the old tradition. When the writers in the classical and neo-classical line observed that their abstract theory could not permit them to draw policy conclusions, this was also, as I pointed out, a recognition of the need for a much fuller knowledge of the society they were studying. Most of the writers in that line, from Adam Smith to Marshall, were what we would not recognize as institutional economists, taking into account all sorts of things outside the so-called economic factors. Only by doing so could they become political economists, drawing policy conclusions.

For choosing an institutional approach to our problems there are, indeed, the logical reasons that there are causal interrelations between all conditions in a society. Economic growth, for instance, is interdependent with what happens to equality. When in recent decades I have for the most part been engaged in the study of the development problems in underdeveloped countries, I have thus seen the need for large-scale reforms of their whole institutional structure in order to have development. A study restricted to the economic factors will not be conclusive, least of all if dominated by the reliance on a very simplified model.

VII

And here I have to stop, as my time is running out. I am embarrasingly conscious that by having had to condense my message to the half-hour allotted to me, and nevertheless trying to cover the tremendous field of the topic chosen, I may have presented my argument so utterly sketchy that its relevance may not have been easily understood.

Let me end by saying this, that however dark my views are about the present trends in the world today, I feel no pessimism in regard to the development of our economic science. I expect that within the next fifteen or twenty years the highly abstract model-building, often founded on not stated and not carefully scrutinized, and often utterly unrealistic assumptions, is going to lose out in prestige. A more institutional approach will be winning ground, simply because it is needed for dealing in an effective way with the practical and political problems that are now towering and threatening to overwhelm us. Much of present establishment economics is just going to be left on the wayside as irrelevant and uninteresting.

And for the same reason we will have to consider more frankly and systematically the methodological means we have to insert in a controlled and rational manner valuations in our study. This will be seen to be necessary in order to cope with the mounting practical policy problems of today.

In both ways, economics will again become political economy, as it was in the older generations, though it will have to stand on a very different philosophical and psychological basis.

The change will probably not come in a dramatic way as a reasoned conversion, except by a few scientists who like me are leading and pressing for the change, but as a gradual adjustment of establishment economics in response to the altered requests in our society, as its problems grow and intensify. Historically, when looking backward, the impact on the work in our discipline of these new and altered requests will stand out as having been more important than merely conceding to the logic of me and other colleagues, who have argued its rationality. On us challengers falls the continuous duty to aid this foreseen adjustment process by pressing logical clarity of the arguments for change of our methods, and by in our own work on various problems courageously utilizing the new approach.

WAS THE GREAT SOCIALIST DEBATE REALLY OVER SOCIALISM: AN APPLICATION OF BUCHANAN'S CHOICE-INFLUENCING COST

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Two recent articles by Drewnowski (4) and Roberts (15) deny that the "great socialist debate" was really over socialism. This paper reviews these denials, examines their implications, analyzes the debate as a confrontation of different notions of economic cost identified by Buchanan (2), and concludes that it was a legitimate discussion of one form of socialism.

I

The great socialist debate was a series of contributions by Pareto (10), Pierson (11), Barone (1), Mises (9), Lange (7), Taylor (16), Hayek (5,6), Dickinson (3), Robbins (12), Lerner (8), and others between 1900 and 1945 who argued whether a socialist state could reproduce the efficiency of a market economy without all the elements of markets.[1] Drewnowski rejects this as a legitimate socialist debate because it began (with Pareto, Pierson, and Barone) when there was no real world socialist economy to evaluate. "They solved a theoretical problem and that was all they could do."[2] In 1920 Mises continued to argue about theoretical socialism even though some limited empirical evidence from the Soviet Union was available. When the debate picked up again in the 1930's, Lange, Hayek, Lerner, Robbins, and others continued to ignore the Soviet Union and concentrated on theoretical socialism. Drewnowski rejects this as legitimate because "a theory of socialism should start from an analysis of the existing socialist systems."[3]

Roberts denies that it was a debate over socialism because "socialism intended to *replace* a system of market relations with a system of planning" and the Lange-Lerner model is a system of market simulation not market replacement.[4] He justifies his position by explaining "why" socialism intended to replace markets with planning, but his explanation is curious because he leaps from explaining socialist intent to explaining Marxist intent as if the two are synonymous.

Roberts argues (13,14), correctly it seems, that market exchange relationships (commodity production in Marx) are the Marxian cause of alienation. To get rid of alienation, a socialist economy that accepts Marx's explanation must get rid of markets and then to avoid anarchy it introduces planning. But what about a socialist economy that does not accept the Marxian concern with alienation? Can it not legitimately seek to remove something other than alienation? If a socialist economy is not concerned with removing alienation then it need not be concerned with removing all markets. The Lange-Lerner model is such a non-Marxist socialist alternative. Instead of alienation it is concerned with monopoly, restrictionism, imperialism, concentration of economic control by a few, and social and economic instability.[5] Is Lange to be denied the title socialist because he is not a Marxist? This is precisely the implication of Roberts' argument: the great socialist debate was not over socialism because its major theoretical mode, Lange-Lerner socialism, retains markets and this is contrary to Marxism. A corollary of this is that the participants in the debate were guilty of an intellectual error of major proportions.

II

Drewnowski's major implication is that the economic theory of socialism ought to be derived from real world socialist economies and not from theoretical debates. His concern is with the emphasis given the Lange-Lerner model to the exlusion of the Soviet and other real world experiments, but his recommendation is to go to the other extreme and look only at the real world cases while forgetting the Lange-Lerner model. If we agree with him that one extreme is undesirable, then surely the other is to be avoided as well.

Roberts' implication is more disturbing because it is wrong. Left to stand unchallenged, it is evidence of a kind of historical revisionism which suggests that Lange, Lerner, Hayek, and Robbins, and many others were confused and did not realize that their differences were not substantive. The great socialist debate was neither great nor socialist.

His argument says that the Lange-Lerner model cannot be socialism because it retains some markets and simulates others. The Central Planning Board functions as an automaton to ratify whatever excess supply and demand pressures it detects, and therefore does no "planning" at all in the sense of non-market allocation. Roberts is correct that the Lange-Lerner model does not eliminate market relations and therefore, in the Marxian analysis, does not eliminate alienation. But just because it does not remove Marxian alienation does not disqualify it as socialism. Socialism can be socialism without being Marxism, but not in Roberts' analysis. Since this is a rather severe charge, let us look at his own words. Under the subheading "The Original Intentions of Socialist Planning," he writes,

> Socialism intended to replace a system of market relations with a system of planning (toward which Mises directed his criticism). The reason for this socialist intention can be outlined as follows.[6]

One would expect that he is now going to tell us "why" socialism intends to eliminate markets. But he does not; instead, he tells us why Marxism intends to eliminate markets. The next sentences read,

> In Marx, alienation is a phenomenon defined to be inherent in developed market economy. It is not a result of private property, division of labor, surplus value, or exploitation but of market-exchange relationships, a system defined by Marx as "commodity production."[7]

But Marxism is not all there is to socialism and the socialist intention is not necessarily the Marxist intention. Roberts has reminded us of nothing more than that Lange and Lerner were not Marxists, but he takes this accepted fact and concludes that they were not socialists either.

III

Drewnowski's concern that there ought to be more to the economic theory of socialism than the Lange-Lerner type model is well-founded, but his recommendation that the theory only include models of the real world is as extreme as the situation he denounces.

Roberts' concern that the Lange-Lerner model is not socialism is based on his "organizational criteria" for the theoretical classification of economic systems.[8] If one system is "organized" through markets, then a "different" system cannot include markets and still be different. Under this organizational criteria, socialism can only be distinguished from markets if it does not include markets. Thus, the Lange-Lerner market simulation model cannot be socialism. But there is at least one other set of criteria to use for the theoretical classification of economic systems: differences in normative value judgments upon which the rules of the systems are based.

The value judgment underlying a market economy is that individual preferences count positively in the allocation of resources. Socialism rejects this and replaces individual preferences with planners' preferences. Different socialist models substitute planners' for individual preferences to different degrees. On this set of criteria, Drewnowski's concern for real world socialist economies is integrated with the Lange-Lerner model and with Roberts' Marxist variant.

The great socialist debate during the 1930's centered on whether a socialist model could be devised to replace individual preferences in capital markets with planners' preferences and still maintain allocative efficiency. Lange, Lerner, and Taylor said yes, Mises, Hayek, and Robbins said no. Roberts claims that this is no real socialist debate because only one market is eliminated, others are retained, and the planners' preferences in the capital market are being dictated to by individual preferences through excess supply and demand signals. In effect, he argues that planners' preferences do not actually have any effect on the allocative efficiency of the economy since they are just responding to market signals as if individual preferences were still in charge. But this is what Lange, Lerner, and Taylor were arguing, that you could get rid of private ownership of the means of production, set up a central planning board, and through trial and error approximations guarantee the allocative efficiency of capital markets without the unwanted distributional effects of private ownership. Roberts is wrong that this is not socialism and Lange, Lerner, and Taylor are wrong that it generates allocative efficiency. It is socialism and it is not efficient because it does inhibit individual preferences. To see how, we turn to the core of the great socialist debate indentified by Buchanan (2), the issue of whether costs are objectively observable and independent of the act of choice or subjectively invisible in the mind of a decision maker before the act of choice.

IV

Buchanan suggests that the theory of cost is in a sorry state because of the dual existence of a neoclassical predictive theory of cost and a London School of Economics-Austrian choice theory of cost. The neoclassical predictive theory is the familiar textbook concept of alternative products reflected in the market prices of resources. This is objective cost that can be measured and observed. It is a result of the act of choice and therefore independent of the decision process in making a choice. In this context, allocative efficiency is also a result of the act of choice and is independent of the process of choice. Allocative efficiency norms such as marginal cost pricing cannot be objectively identified and posted before the act of choice since they are the results that flow from choice.

The choice theory of cost is subjectivist, it exists in the mind of a decision maker before a choice is made and can only be known by the decision maker. No one else can observe the "choice-influencing costs" that decisions are based on.[9] This identifies the relationship between cost and choice as intimate and individual.

Lange-Lerner socialism assumes that the neoclassical objective theory of cost can serve as a choice theory of cost. The Central Planning Board posts prices, managers are directed to marginal-cost price as if they were in competitive markets, and managerial choices are made on the basis of these objective cost and price data. Cost is divorced from the act of choice in this socialist framework. But Mises, Hayek, and Robbins were early developers of the subjectivist choice-influencing cost concept and their participation in the great socialist debate included the denial that socialist managers could make the right decisions for allocative efficiency when their preferences are constrained. In Buchanan's terminology, the Central Planning Board and the absence of capital markets prevent each individual from calculating his own perception of the choice-influencing costs that will lead to allocative efficiency. "Efficiency in allocation will emerge only if the effective decision-makers are converted into economic eunuchs" who make decisions based on planners' preferences and not their own.[10]

It is rather surprising that Roberts does not acknowledge the different notions of economic cost that lie at the heart of the debate over Lange-Lerner socialism since he does footnote Buchanan and two of Buchanan's references.[11] He even notes that the "marginal rule" for socialist managers is the result of competitive markets and not an objective guideline that can be followed to reproduce allocative efficiency, but he does not seem to see that this is so because of the differences between costs that result from choice and costs that determine the direction of choice. He is so tied to his organizational criteria for distinguishing between economic systems that he does not see that even if the Central Planning Board does automatically ratify excess supply and demand information this does not permit individual firm managers to correctly calculate their subjective choice-influencing costs.[12] This is a substantive difference between the two sides in the great socialist debate. The Lange-Lerner socialists assume that the costs that influence managerial choice can be objectively determined outside the mind of the managers before the choices are made, the opponents deny it.

V

The conclusion is that the great socialist debate was over socialism after all, a socialism that proposed to replace individual choice costs with objectively determined dollar costs. Roberts is inaccurate to deny this as socialism, both for his bending of socialist intent to mean Marxist intent and for his failure to recognize the implication of the two different notions of cost identified by Buchanan. The opponents of the Lange-Lerner model were defending the fundamental value judgment of market economies, the desirability of guiding the allocation of resources through individual preferences expressed in the choice-influencing cost calculations of individual minds.

FOOTNOTES

[1]Hayek (5,pp. 291-293) lists thirty other participants up through 1935.

[2]Drewnowski (4,p. 341) does not mean to imply that the opponents of liberal socialism were correct. In the next sentence he declares that everybody now agrees that Mises was wrong.

[3]Drewnowski (4,p. 342) defines socialism in a footnote "in the sense in which Pareto, Mises, and Lange used it. It means a system in which all the means of production are owned by the state." This is important because Roberts (15,p. 563) rejects such a definition and substitutes the replacement of all markets with planning. But Roberts (15, pp. 562-563) also uses Drewnowski's call for an economic theory of socialism based on real world economies as supporting evidence that "the Lange-type model has nothing to do with socialist planning," but it will not work. Drewnowski's point is not Roberts' point, they are discussing different definitions of socialism, and Roberts cannot logically use Drewnowski to support him.

[4]Roberts (15, p. 563,565,570.)

[5]Lange (7,0. p. 126.)

[6]Roberts (15, p. 563.)

[7]Roberts (15, p. 563.)

[8]Roberts (15, p. 563.)

[9] Buchanan (2, p. 43.)

[10] Buchanan (2, pp. 96-97.)

[11] Roberts (15, p. 569).

[12] The only interpretation to give to Roberts' position is that he misreads Buchanan (2) and the whole development of the dual theories of economic cost. These two theories are historically verifiable traditions, and Buchanan has cogently offered a sampling of the most important evidence. But Roberts fails to see that these two theories were at issue in the great socialist debate.

REFERENCES

Enrico Barone, "The Ministry of Production in the Collectivist State," in *Collectivist Economic Planning*, ed. F.A. Hayek. London: Routledge & Kegan Paul Ltd., 1935.

James M. Buchanan. *Cost and Choice, An Inquiry in Economic Theory*. Chicago; Markham, 1969.

H.D. Dickinson. *Economics of Socialism*. New York: Oxford, 1939.

Jan Drewnowski, "The Economic Theory of Socialism: A Suggestion for Reconstruction." *Journal of Political Economy*, 69 (August 1961), 3410354.

F.A. Hayek (ed.) *Collectivist Economic Planning*. London: Routledge & Kegan Paul Ltd., 1935.

_____________. "Socialist Calculation: The Competitive 'Solution'." *Economica*, n.s. 7 (May 1940), 125-149.

Oskar Lange, "On the Economic Theory of Socialism," in *On the Economic Theory of Socialism*, ed. B.E. Lippincott. Minneapolis: Univ. of Minn. Press, 1956 (first published in *Rev. Econ. Studies*, 4 (October 1936) (February 1937).

A.P. Lerner, "Economic Theory and the Socialist Economy," *Review of Economic Studies*, 2 (1934-35), 51-61.

L. von Mises, "Economic Calculation in the Socialist Commonwealth," *in Collectivist Economic Planning*, ed. F.A. Hayek. London: Routledge & Kegan Paul Ltd., 1935 (first published as "Die Wirtschaftsrechnung im sozialistischen Gemeinwesen," *Archiv Socialwissenschaften*, 47 (1920))

Vilfredo Pareto. *Manual of Political Economy*. New York: Kelley, 1971. (first published in Paris, 1909)

N.G. Pierson, "The Problem of Value in the Socialist Community," *De Economist*, 41 (1902), 423-456.

Lionel Robbins, "Certain Aspects of the Theory of Cost," *Economic Journal*, 44 (March 1934), 1-18.

Paul Craig Roberts and M.A. Stephenson, "Alienation and Central Planning in Marx," *Slavic Review*, 27 (September 1968), 470-474.

_________________ *Alienation and the Soviet Economy*. Albuquerque: Univ. New Mexico Press, 1971.

Paul Craig Roberts, "Oskar Lange's Theory of Socialist Planning," *Journal of Political Economy*, 79 (May/June 1971). 562-577.

Fred M. Taylor, "The Guidance of Production in a Socialist State," *American Economic Review*, 19 (March 1929).

ALIENATION, FREEDOM, AND ECONOMIC ORGANIZATION: A COMMENT ON A VIEW FROM CHICAGO

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Writing in a recent issue of the *Journal of Political Economy* Edwin Dolan discusses the nature of alienation and its relationship to freedom.[1] In this discussion he explicitly rejects the classic Marxian model of alienation in favor of an eclectic socio-psychological one.[2] We will argue that (1) this rejection is not justified on the grounds offered by Dolan and that (2) the alternative model which he subsequently adopts implies solutions which most of us (Marxians and libertarians alike) would find unacceptable.

Sources of Alienation

After identifying the major components of the alienation syndrome as (1) powerlessness, (2) meaninglessness, and (3) a lack of intrinsic reward in work, Dolan attempts to discover the origins of this syndrome. He begins by arguing that the traditional Marxian model in which alienation and capitalism are causally linked can be of little use, "... after fifty years of experience with socialism in the Soviet Union, such a simple identification of alienation with the capitalist form of ownership seems a bit naive."[3] What is it about the Soviet experience which accounts for the naiveté of the Marxian approach and should encourage us to reject it? The answer is simple: the continued existence of alienation in Soviet society. As if to foreclose further argument, Dolan presents a quote from the writings of a Soviet sociologist, "... the process of liquidation of private property does not signify the complete and automatic liquidation of the phenomena of alienated labor."[4]

The above reasoning leads Dolan to conclude that the origins of alienation must be"... sought in more basic characteristics of industrial society which prevail independently of the legal form of property relations."[5] What are these characteristics which are common to industrial societites regardless of property relations and are responsible for alienation? Drawing on the works of a variety of writers (including Smith, Say, Mannheim and indeed Marx), Dolan identifies three: (1) the division of labor, (2) the wage system, and (3) consummerism. He then completes this segment of his argument with a discussion of how each of these basic characteristics of industrialism are thought to produce the alienation syndrome.

The question we wish to raise at this point is not whether Dolan's analysis of the sources of alienation is preferable to Marx's, but whether Dolan's easy dismissal of Marx can be justified by his simple reference to the Soviet experience. Our answer is that it cannot. The existence of alienation in the Soviet Union, far from negating the Marxian schema, is entirely consistent with it. Dolan fails to recognize this consistency because he implicitly assumes that the establishment of socialism necessarily implies the elimination of alienating forms of property relations. This assumption is incorrect.

In his writings Marx is careful to distinguish at least two stages of socialism or communism. The first stage, he notes,"... is a communist society, not as it has *developed* on its own foundation, but, on the contrary, just as it *emerges* from the capitalist society; and which is thus in every respect, economically, morally and intellectually, still stamped with the birth-marks of the old society from whose womb it emerges."[6] Not only are the alienating property relations of capitalism retained during this stage, but, according to Marx, "... the two sides of the [property] relation are raised to a *supposed* universality; *labour* as a condition in which everyone is placed, and [State] *capital* as the acknowledged universality and power of the community."[7]

What Marx means here is that during the stage of 'crude communism' property ownership is concentrated in the hands of the State and the proletarian (i.e. propertyless) class is universalized. The implications of this arrangement should be clear. Denied the means of production the worker continues to sell his labor to the owners of capital, in this case the State. As with earlier forms of wage labor, the worker is excluded from decisions concerning the product of his labor and the work process itself. This exclusion (i.e. working for a wage) is, according to Shlomo Avineri, "... the universal principle of this crude communism which preserves the most distinct elements of alienation."[8] Marx anticipated this but argued that "... these defects are inevitable in the first phase of communist society as it is when it has just emerged after prolonged birth-pangs from capitalist society."[9]

Even the most casual examination of the organization of Soviet society will reveal that it has yet to go beyond the stage of crude communism. Until Dolan can prove otherwise (i.e. until he can demonstrate that the State, like the individual capitalist of an earlier era, has been expropriated, that the ownership and control of the means of production have been finally passed to the workers, and the stage of 'higher communism' has been established), the existence of alienation in the Soviet Union will remain an insufficient basis for the rejection of Marx.[10]

Alienation and Freedom

After claiming to have established that "... alienation is a phenomenon resulting from certain aspects of economic organization which are less a function of the type of property relations prevailing in society than of the level of industrial and technological development which it has attained,"[11] Dolan goes on to argue that the existence of alienation represents a serious threat to the maintenance of libertarian freedoms. First it results in an important loss of freedom for those born into alienating (i.e. proletarian) environments and for those engaged in alienating (i.e. wage laboring) occupations. This loss, however, will probably not "..arouse the concern of the unalienated liberatarian,"[12] who views such matters as the private responsibility of the individuals involved. In order to arouse such concern, Dolan calls attention to the negative external effects alienation (i.e. the existence of a large mass of alienated and unfree people) may have on the freedom of the unalienated. Alienated individuals, we are told, may (1) be unwilling and incapable of defending their country, (2) attempt to impose their standards on others, (3) oversimplify complex social problems, and (4) develop a fear of the 'free individual' and prevent the latter from doing his 'own thing'.[13] In short, alienated and unfree individuals have a tendency to develop 'authoritarian personalities' which render them susceptable to 'authoritarian movements' which offer solutions to their predicament.[14]

If the unalienated and free are to protect themselves from the alienated and unfree ('... our alienated and unfree fellow citizens'), a solution to the problem of alienation must be found. Accordingly, Dolan pleads with members of the former group to take alienation seriously and to "... seek their *own* solutions to the problem" before one is imposed upon them by "... the authoritarian altruists of the left."[15] That Dolan avoids offering a solution of his own after such an intensive study of the problem should not be surprising. For if one accepts his analysis of the sources of alienation there can be only one solution; and that would entail (among other things) the abolition of labor markets (wage labor), a headlong retreat from economic specialization (division of labor), and the rejection of modern technology. In short, the only solution consistent with his model of alienation is the abandonment of industrial society in favor of one organized (we can only guess) along rural utopian or guild socialist lines.

Few of us (including, we suspect, Dolan) would be willing to accept such a solution. Indeed, even so radical an individual as Marx could only be appalled by the suggestion that industrial society be abandoned. For Marx, industrialism is a necessary condition for the realization of human freedom, not a threat to it. It is industrialism, and the great productive capacity which it implies, which allows man to escape the oppressive 'realm of necessity' (a world in which he must devote all of his energies to satisfying his purely biological or *animal* needs) and enter the 'realm of freedom' (a world in which he is free to devote most of his energies to the fulfillment of his *human* or non-animal needs). Industrially-based models of alienation, such as Dolan's, would have us move in the opposite direction. The Marxian model, which admits the potential compatibility of industrialism and freedom from alienation, is infinitely more hopeful in this respect.

FOOTNOTES

[1]Edwin G. Dolan, "Alienation, Freedom, and Economic Organization," *Journal of Political Economy* 79 (September/October 1971), pp. 1084-94.

[2]For a perceptive discussion of the differences between these two approaches see Paul Craig Roberts and Mathew A. Stephenson, "A Note on Marxian Alienation," *Oxford Economic Papers* (November, 1970), pp. 438-42.

[3]Dolan, p. 1086.

[4]*Ibid.*

[5]*Ibid.*

[6]Karl Marx, *Selected Writings in Sociology and Social Philosophy*. Translated by T.B. Bottomore. (New York: McGraw-Hill Book Company, 1964), p. 256.

[7]Karl Marx, *Early Writings*. Translated by T.B. Bottomore. (New York: McGraw-Hill Book Company, 1964), p. 154.

[8]Sholomo Avineri, *The Social and Political Thought of Karl Marx*. (Cambridge: Cambridge University Press, 1970), p. 224.

[9]Marx, *Selected Writings*, p. 258.

[10]This error is not uncommon among those who criticize Marxian theory on the basis of what has or has not happened in the Soviet Union. If criticism along these lines is not to be wholly misleading, care must be taken not to confuse the 'crude communism' of the Soviet Union with the 'higher communism' of some future date.

[11]Dolan, p. 1089.

[12]Dolan, p. 1092.

[13] Dolan, p. 1093.

[14] Dolan is implying that the maintenance of libertarian freedoms is actually in the interest of the alienated and unfree. Alienation, however, so batters their consciousness that they are incapable of perceiving their 'true' interests and are unable to find rational ways of satisfying these interests. Although Marx has a different view of what the 'true' interests of this class are, his explanation of why these interests are not recognized and seized by this group is identical to Dolan's. See Bertell Ollman, *Alienation: Marx's Conception of Man in Capitalist Society.* (Cambridge: Cambridge University Press, 1971), pp. 238-39.

[15] Dolan, p. 1093.

PUBLIC SECTOR WAGE DETERMINATION: A COMMENT

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Two articles on wage determination in the public sector appeared in the April, 1974 issue of the *Industrial and Labor Relations Review.*[1] Both articles look at wage determination for a large number of public employees throughout the nation. This represents a departure from much of the prior research which focuses on a single occupation. The Freund article examines the influence of market forces and the power of unions on the wages of large city employees. Fogel and Lewin were concerned with public-private pay differentials and their causes. They drew upon various sources for their data, including municipal and private wage data in 11 cities.

From Freund's regressions it appears that market forces are major determinants of wage increases in large city governments and, that there is only a weak relationship between measures of union power and wage increases. Fogel and Lewin found evidence that wages for public sector blue collar and lower level white collar workers were higher than for their counterparts in the private sector, and salaries for public managers and professionals were lower than for those in the private sector. The public-private differences are partially explained by the discretion public employers must exercise in implementing the prevailing wage rule and the nature of the political forces that affect wage decisions.

The findings of the two studies taken together imply market forces determine wages of public sector employees to a greater extent than unionization; but that public-private differentials are explained by non-market factors. A logical conclusion would be that manipulation of such non-market factors by public employee organizations could further the economic status of the bulk of public employees relative to, and perhaps at the expense of, employees in the private sector; but discretion in applying the comparable wage rule and political activity would have little impact on determining wages in the public sector. Is such a situation possible or is there something amiss in the research which allows for such a conclusion to be reached? It appears the answer lies in the methodology of Freund's research. Three points in particular are open to debate. First, should percentage changes in non-government wages, as Freund assumes, be considered solely as a market force determinant? The market for private sector workers is influenced by unions, employer monopsony power, government intervention and other factors. When Freund treats the non-government wages as market determined he must assume that these factors either have no impact on wages or work in a way to neutralize one another. This is a heroic assumption when the purpose of such intervention, in many of these cases, is to protect wages from competitive market forces. Second, does the percentage change in local government expenditures reflect only the demand for local government services, and should it only be treated as a market-force determinant? Public employees have influenced budget formulation through lobbying by their associations and unions. As a result they have had an impact on the demand for their own labor. Finally, how is union strength measured? Is it solely attributable to degree of organization, strike activity, arbitration awards, and legislation which permits the union to negotiate, campaign and engage in political activity, and strike; or is it something more? These points will now be treated in detail.

Prevailing Wages

Both Freund, and Fogel and Lewin treat the question of cities paying comparable wages to those paid in the private sector. Freund finds that cities which made an effort to make wages comparable were more responsive than others to market conditions. Fogel and Lewin find the "discretion" exercised by employers in applying prevailing standards responsible, in part, for the differentials which exist between public and private sectors. The findings of the two articles are complimentary in that the one looks at the effect of prevailing wages on earnings of public workers in different cities, and the other looks at the impact of discretionary application of the standard and finds it helps to explain the differences between the two sectors.

The question arises, however, whether nongovernment wages should be treated by Freund as a market force determinant or whether they should, at least partially, be credited to unions? The question is important because this is one of the three market factors found by Freund to be related to increased wages for city employees.

Fogel and Lewin point out the importance of the prevailing wage rule in the United States. Specifically mentioned is the requirement to pay federal employees rates comparable with private enterprise under the Federal Salary Reform Act of 1962, and the charter of the city of Los Angeles which makes a similar provision. In addition to their examples, it should be mentioned that in other cities, as a matter of law, policy, or custom the prevailing wage rate has come into being. In some cities construction-craft city employees are paid a percentage of the wage negotiated by the union with the employers' association, the rationale for the difference being that city employees are guaranteed year-round work and better fringe benefits. But where did the legislation, charter requirements, policies, and customs come from? To some extent the prevailing wage principle was furthered by public employee unions, associations, traditional private sector unions with some public employee members, and other private sector unions sympathetic to the plight of government employees. It is questionable that if wage decisions were left to the public employer or the public alone, whether the public employee would be compensated on a comparable basis.

The findings of Freund and, in particular, the inclusion of nongovernment wages as a market force factor are further questioned by the work of Fogel and Lewin. They present evidence which shows that comparable pay is determined in such a way that unions in the private sector have a disproportionate effect in biasing the wage level in the public sector upwards. If public unions can be given some responsibility for making their wages comparable to the private sector, and private unions are responsible for biasing the comparability upward, can the nongovernment wage variable be considered solely a market force?

Percentage Change in Local Government Expenditures

The percentage change in local government expenditures, treated by Freund as a market force, was also found to relate to wage increases for city employees. However, a question could be raised as to the propriety of including it as a market force. Freund treats this variable as representing the change in the cities' demand for Labor. But what is the impact of collective bargaining on local government expenditures? Why does compulsory arbitration in Pennsylvania for police and firefighters have to be completed prior to the formulation of city budgets? It would be difficult to defend the proposition that government expenditures should be treated solely as a market force when the union may partially determine wages and the city determine employment.

Union Strength

Freund states that "regardless of the extent of organization, unions obviously will not affect wages unless they exercise their strength." Degree of organization, strike incidence and compulsory arbitration are reflections of union strength in this analysis. He is also aware that such activities as election support of favorable candidates and lobbying have been held to affect wages and cites Wellington and Winter, "that the unique aspect of collective bargaining in the public sector is the power unions derive by being a political interest group in the struggle for local tax dollars." Freund argues that when activities such as lobbying and campaigning are limited by law, unions will be less effective. This is speculative, because in numerous jurisdictions where either the law is silent or legislation, court rulings, and administrative decisions have prohibited union activity, public employee organizations have nonetheless lobbied, bargained, engaged in strikes and applied other pressure tactics. Thus, the finding that there is no evidence that local public sector unions affect their members' wages by political activities aimed at garnering a "disproportionate" share of local government budgets should be qualified by the reservation that the measures of union activity or inactivity used may be fallacious.

Freund's selective use of union strength measures can be questioned as to whether they are individually indicative of strength, and more importantly whether they leave out numerous important factors which influence union power.

Union strength is more than the strike, degree of organization, and the legal framework in which public employees' organizations operate. It could even be argued that a strong organization does not have to strike to attain its objective. Other factors which influence union strength could be the militance of the membership, financial resources, leadership capability, type of work being performed, ties with private sector unions, and political influence.

Conclusion

Essentially Freund is telling us that wages were significantly related to market factors and weakly, at most, to unions. But there is reason to question whether his market forces measuring rod isn't really totaling up union bargaining power. There is also some question about the impact of legal prohibitions limiting public employee organizations.

Empiricizing wage theory has a long and not very encouraging history. Work in the private sector by Douglas, Lester, Ozanne, Rees, Ross and a host of others has demonstrated a heroic futility from which economists have learned what not to do. Students of wage determination in the public sector could learn a great deal from the experiences of their predecessors in the private sector.

FOOTNOTES

[1] James L. Freund, "Market and Union Influences on Municipal Employee Wages," and Walter Fogel and Walter Levin, "Wage Determination in the Public Sector," *Industrial and Labor Relations Review*, Vol. 27, No. 3 (April 1974) pp. 391-404 and 410-431.

The following article by Gordon Tullock will almost certainly draw one or more replies. It is included in the hope that such a debate will be joined and that out of the debate will emerge a clearer understanding of the role of mathematics in Political Economy.

Editor

ON MATHEMATICS AS DECORATION

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The usual arguments for the use of mathematics in any field are that it is more precise and briefer. In my opinion, the use of mathematics is frequently almost entirely decorative. Two "shorter papers" in the *American Economic Review* (December, 1973) can be used as illustration. Both make points which are new and of some substantial importance, but in both cases the actual line of reasoning needed to reach their conclusions can be put in simple English in one paragraph without any loss of rigor.

The first of these is Eugene P. Seskin's "Residential Choice and Air Pollution: A General Equilibrium Model." The general level of mathematical difficulty of this article can be seen from the fact that there is a mathematical appendix on a mathematical article, and that Herbert A. Simon is the co-author of the mathematical appendix. The matter is put in simple English below.

> Recently there have been some suggestions that pollution could be dealt with by dividing the country into areas, in one of which pollution would be freely generated and the other of which it would be strictly controlled. Individuals could then choose where they wished to live or place their enterprises, and would have free choice of the amount of pollution that they choose to receive. Although this solution might conceivably be an improvement on present institutional arrangements, it will not achieve optimality. The individual choosing to move into a pollution area for the purpose of acquiring a higher income while generating pollution would take into account the costs on himself of the pollution in the area, but not the costs on other people. As a result, the amount of pollution generated in such areas would be superoptimal. The problem is on all fours with Frank Knight's famous road problem or, indeed, almost all of the discussion of congestion on roads.

My second example is Amartya Sen's "On Ignorance and Equal Distribution." Putting the correct part of this article in a simple paragraph is easy.

> Granted that income has declining marginal returns,[1] an individual would always choose a certain sum of money over a gamble having the same present value. This phenomenon, which has been discussed in the literature since Jevons, has seldom been applied to the problem of income distribution per se, although the discussions of risk preference may be referring to the same thing. It is clear that, if we take a Rawlsian approach and assume that individuals make decisions on income distribution before they know what their own role will be, they in essence are making a choice among lotteries. All individuals would thus choose an absolutely equal income distribution above all others, provided, of course, that the individuals do not think that the income distribution has any effect on the total production and that the individuals do not regard general utility as something to be maximized rather than income. [2]

In the case of the Sen article, there is a good deal of rather confusing discussion about the relationship between the above proof and Friedman's criticism of Lerner. It would appear that Sen believes that a policy that maximizes utility expectation for each individual in the society will maximize it for the whole. This is true if we do not follow the policy which Friedman hypothesized. Nevertheless, that is beside the point and the discussion of the Friedman point is quite separate in the article from the mathematics.

In the Sen article there is another mathematical proof which is that if you divide the given sum of money equally among people, the minimum amount that any one of those persons can receive is higher than if you divide the money unequally. The point is so obvious that I do not see how he could waste almost one-quarter of a page on it, but he clearly regards it as a minor part of his note. In general, it should be pointed out that the Sen article is much shorter than the Seskin article and hence the expansion of the article by use of decorative mathematics is much less significant there.

Unfortunately, although it would be totally untrue to say that all mathematical articles are like these two, articles of this sort do occur a good deal in the present literature. They not only waste journal space by putting things at too great a length and increase printing costs by requiring difficult notation, but they also make the journals a good deal harder to read. They may be prettier than literary articles, but they certainly impede the communication process.

FOOTNOTES

[1] Sen says the utility function is concave which is somewhat more general, but I think the only reason it should be concave would be declining marginal returns.

[2] The individual who favored general utility maximization might be interested in making it possible for individuals to trade off leisure against income.

REFERENCES

Sen, Amartya. "On Ignorance and Equal Distribution." *American Economic Review*, LXIII (December, 1973), 1022-1024.

Seskin, Eugene P. "Residential Choice and Air Pollution: A General Equilibrium Model." *American Economic Review*, LXIII (December, 1973), 960-967.

GIBSON ON INFLATION EXPECTATIONS: COMMENT

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In a recent paper William Gibson describes the effect of inflation expectations on interest rates by estimating the basic equation:

$$(1) \qquad i = a_o + a_1 P^*$$

where i is the nominal rate of interest and P* is the expected rate of price change. Sample survey expectational data is used. Gibson finds that the estimates of a_1, while depending on the term of i, are generally close to unity, as one might expect, and he concludes that market rates are strongly affected by price expectations.

The use of survey expectational data, while bypassing some of the difficulties associated with indirect estimation, interjects a new one: the direction-of-causality question. Gibson's findings may be interpreted as supporting the Fisher effect, but an equally plausible interpretation is that survey respondents base their price forecasts on interest rate levels. After all, a large number of those surveyed have special economic training, and within such groups the Fisher effect is widely *believed* to be operative. The fact that the survey data have low predictive value also supports the latter interpretation. For the period 1954-69, the simple squared coefficient of correlation between predicted and actual price level changes in 0.41, and for the period 1954-64, the same figure is only 0.194.[1] These coefficients are consistent with evidence showing that interest rates are poor predictors of future price changes.[2]

A second point applies to the estimators of (1). "Equation (1)," explains Gibson, "does not represent a full and complete theory of interest rate determination. Rather it seeks to quantify the effects of expectations on nominal interest rates." The specification in (1) would be more convincing if satisfactory Durbin-Watson statistics could be cited. Inexplicably, the D-W's are not even reported. There is reason to believe that they may be quite low. Regression coefficients will have t-statistics that are inflated if autocorrelation is present, and in the equations reported, they are huge. The t's are readily obtainable, and my calculations show them to range from 8.21 to 14.30 for the 10 estimates of a_1 that are presented in Table 1. Low D-W statistics do not necessarily imply misspecification but they suggest it. On a priori grounds, too, misspecification is suggested. Fluctuations in output and liquidity are not always mild and in fact can be quite pronounced. Given the substantial theoretical evidence (Nunn, Gupta, Feldstein and Eckstein, Sargent) linking the real rate to these two variables, a constant real rate seems hard to justify. If either variable is excluded from (1), one would expect to get biased and inconsistent estimates of a_1. Specification error is a fact of life of course, but this seems less a reason to ignore it than to be alert to its hazards, particularly–and this is repeatedly the case here– where the values of the coefficients are presumed to warrant fine empirical judgments.

FOOTNOTES

[1] See Turnovsky, p. 1752, who used the same expectations data in a separate study.

[2] Irving Fisher himself concluded that this is the case, p. 425.

REFERENCES

Feldstein, M., and O. Eckstein, "The Fundamental Determinants of the Interest Rate," *Review of Economics and Statistics*, Nov. 1970, 363-374.

Fisher, I., *The Theory of Interest* (New York: Macmillan, 1930), reprinted by Augustus M. Kelley, 1967.

Gibson, W.E., "Interest Rates and Inflation Expectations: New Evidence," *American Economic Review*, Dec. 1972, 854-865.

Gupta, S., "The Portfolio Balance Theory of the Expected Rate of Change of Prices," *Review of Economic Studies*, April 1970, 187-203.

Nunn, G., "Aggregate Price Expectations: An Alternative Approach," (unpublished doctoral dissertation, University of Virginia, 1973).

Sargent, T., "Commodity Price Expectations and the Interest Rate," Quarterly *Journal of Economics*, Feb. 1969, 127-140.

Turnovsky, S., "Empirical Evidence on the Formation of Price Expectations," *Journal of the American Statistical Association*, Dec. 1970, 1441-1454.

In a short article, Gordon Tullock, "Universities Should Discriminate Against Assistant Professors," Journal of Political Economy, *Vol. 81, no. 5, (Sept/Oct., 1973) p. 1256-7 it was argued that the present system used by universities does not give the right signals to younger faculty. The article may have been only partially seriously written but it did generate several replies, one of which was used by the* JPE. *(See R.P. Parks, "Assistant Professors Should be Discriminated Against; Or The Less Productive I Am, The More I Should Be Paid,"* Journal of Political Economy, *Vol. 83, February, 1975, pp. 225-226.) Two further comments are published here for their instructive value to assistant professors. At some time in the future it might be well to publish the letters from the* JPE *relative to these articles. Faculty of all ranks could learn a great deal about appropriate incentives from a careful reading of the letters as well as the articles.*

Editor

UNIVERSITIES' PRESIDENTS SHOULD DISCRIMINATE AGAINST FULL PROFESSORS

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In a recent article, Gordon Tullock concludes that a university can maximize the number of published papers from its faculty by discriminating against the neophyte in favour of those with established publication records. We examine two of Tullock's assumptions, and argue instead that his conlcusion should be turned on its head. If, on the one hand, we look at his assumption concerning rationality it can be shown that he misses the thrust of his own argument: it is full professors who should be discriminated against. On the other hand, by reconsidering the presupposition that universities do any discriminating at all we can again show the conclusion: university presidents ought to discriminate against senior professors. After sketching Tullock's argument we will consider these cases in turn.

As Tullock sees it, the present university custom is to motivate research by tying individual faculty income to published output. The payoff differs according to age, stage in career, and previous productivity. Specifically, Tullock asserts, the individual's payoff per article (as measured by lifetime earnings) decreases substantially as the number of publications grows. Given this, junior faculty who do not invest much of their own resources in producing articles are totally irrational.

Because of the quantity discount, senior faculty have less motivation to produce. Supposing that the pay schedule is not modified to equalize financial reward for publication, the university should, Tullock argues, at least discriminate against junior faculty by distributing secretarial and research assistance to productive senior faculty. Recognizing that the income tax laws make it rational for senior faculty to take part of their income as handsome offices, secretaries, etc., such discrimination would simultaneously increase the payoff per article and reduce the individual's cost of production, thus increasing research productivity.

Interestingly, Tullock admits that such discrimination is only a third choice as far as increasing productivity of senior personnel is concerned. A more efficient means of so doing would be to alter the pay schedule to that the return on articles is about the same in all ranges. This, he suggests, is not likely because universities do not seem to have much preference for (indeed may have a positive preference against) efficiency. A still more efficient means of maximizing research productivity would be to *decrease* wages for those senior faculty who fail to maintain a certain level of research output. This move, Tullock says, is discouraged by the "sacred custom" of tenure, and he is left with the conclusion that only a modest increase in the efficiency of universities is plausible, and that increase is to be obtained by discriminating against junior faculty in favour of senior with respect to various kinds of research assistance.

Let us examine one foundation of the above argument. Tullock asserts that the university can largely ignore young scholars because (p. 1256) "A man of 28, preparing to work on his third article, can anticipate a massively greater increase in his lifetime earnings as a result of publication of that article than can a man of 50, contemplating his fiftieth article." Evidently Tullock is assuming that each individual seeks to maximize his lifetime earnings *without regard to the distribution of that income*, for if he is not making this assumption, his claim is almost surely false. An individual of 50 having about 50 articles to his credit would normally be receiving about twice the annual compensation of a 28 or 30 year old neophyte just beginning to publish.

If each produced the same number of articles during a given year, both could normally anticipate equal percentage raises. Hence, the 50-year-old's raise would roughly double the 30-year-old's gross increase. However, the 50-year-old can anticipate only 10 to 20 years of receiving his increment while the 30-year-old would receive his increment for a period of 30 to 40 years. For his article, then, the 30-year-old would at least equal, and at most double, the 50-year-old's lifetime payoff for his. Thus, if we ignore distribution of income, Tullock seems correct in suggesting that the junior person could receive massively greater lifetime earnings for his publication effort.

But what happens if we do *not* ignore distribution? If we consider the present worth of receiving \$2 for each of 10 to 20 years versus the present worth of receiving \$1 for each of 30 to 40 years the results are just the opposite. At a rate of 5%, the present worth of \$2 for each of 10 to 20 years varies from \$15.44 to \$24.92 whereas the present worth worth of \$1 for each of 30 to 40 years varies from \$15.37 to \$17.16–much less for the junior man. At 10% the differences are more pronounced. The present worth of \$2 for each of 10 to 20 years ranges from \$12.24 to \$17.02 whereas the present worth of \$1 for each of 30 to 40 years is only \$9.43 to \$9.78. Thus, either Tullock assumes that distribution of income is irrelevant, or his claim that the junior man is favoured by current salary adjustment procedures is just false.

Clearly, no rational man ignores the distribution of his income. Money in hand is valued more highly than numerically equivalent sums anticipated in the future. Otherwise credit institutions would soon be bankrupt. The sophisticated rational man seeks to maximize present value of earnings, given his utilities. The less sophisticated seeks only to maximize current income. But in either of these cases, the senior faculty member, by virtue of *continuing* to publish, stands to gain massively more than the junior for beginning to publish.

Seen in this light, it is clear that the university should discriminate against senior personnel rather than junior. A senior man has more to gain by publishing than a junior man. Furthermore, the income tax laws serve to subsidize the senior man. If he chooses to increase his productivity by hiring private secretarial help and by remodeling his home to accommodate his scholarly activity, he can both afford to do so and benefit from the deductions.

Indeed, the senior man has so much to gain by publishing that *he* is totally irrational not to publish, and so the university can justifiably ignore him. The university could then proceed more adequately to equalize the reward for publication for junior faculty by reducing their cost of production through provision of research and secretarial assistance. It is more rational for the junior man to take part of his salary in terms of such service, since the income tax laws would not provide as sizeable a subsidy for him as for the senior man.

While a switch toward discrimination against full professors would be more efficient than the present system, we must agree with Tullock that the most efficient approach would involve salary cuts to those senior faculty who do not produce up to standards. Tullock claims that the "sacred custom" of tenure prevents such a rational approach. Nothing, however, about tenure requires numerical increases in salaries and, indeed, so long as inflation eats away at the real value of one's wages, salaries can be effectively cut merely by holding the line. Moreover, by holding the line on senior faculty salaries, more junior personnel could receive higher payoffs for their publications. Hopefully, these payoffs could be brought up to the present level of those for senior personnel. The net result would be increased motivation for research at all levels, and thus an increase in the total publication output of the university.

Depending, as it does, on the claim that assistant professors are at a great publication payoff advantage over senior professors, Tullock's argument comes off only under the implausible assumption that the temporal distribution of payoff is irrelevant. Under more plausible assumptions, the assistant professor is already at a disadvantage. In fact, assistant professors already suffer from discriminating actions even greater than Tullock advocates. In addition to receiving higher payoffs for publications, plush offices, handsome secretaries, and graduate student research assistance, senior faculty are frequently relieved of teaching responsibilities as well. This frees time for research and publication. Under the current system, a neophyte must spend much of his time developing courses. If he does an outstanding job at this task, he may physically be unable to carry the additional burden of extensive original research. Given the publish-or-perish system, such an individual will be forced to neglect his teaching duties to one degree or another in order to publish enough to keep his job.

To those in the university who make decisions as to its direction, the economic value of instructional activity may not be as directly evident as the economic value of good research. However, the economic value to a state or nation in terms of development of its human resources and ultimately in terms of its economic prosperity is tremendous. Surely ineffective teaching reduces this value.

We fully support screening assistant professors so that only the best and hardest working survive. However, we are not at all convinced that the publish-or-perish system serves as an adequate screening process. Indeed, since it ignores the teaching function of the faculty and rewards only research, the net cost of the system may be greater than the payoff it engenders. We would suggest that by reducing the cost of production to the neophyte enough so that he could reasonably be expected to do creative research *without* neglecting preparation for his instructional activities, and then by judging him on *both*, the gain to society would be maximized. This reduction in cost could be accomplished by transferring discrimination from assistant professors to senior professors.

We have argued that Tullock's claims about rewards for publication are incorrect. Even if he were correct on this matter, however, he could not legitimately arrive at the conclusion he claims to arrive at. To see this we now turn to the second of Tullock's assumptions: "the university" makes decisions based solely on financial considerations. Surely Tullock would agree that even if there is such an entity as "the university" it is hardly endowed with the capabilities to make decisions. Individuals make decisions, not institutions. Assume now that responsibility for running the university lies with its president. Secondly, assume his goal is to expend available resources in the most efficacious fashion to make his a "first class" university—a far more convincing objective than the mere production of research publication. Given this goal, would the president decide to implement Tullock's suggestion? He would be irrational if he did. Instead he would decide to discriminate against full professors. (Associate professors should not feel slighted that they are being left out of the discussion, just lucky.)

Recognizing that an important measure of university quality is research publication, the conditions for maximizing such publication demand careful attention. The first requirement, of course, is to sustain a faculty capable of producing the research. The second requirement is the funding necessary to carry it out.

A primary factor determining a university's reputation and its drawing power for new faculty and funds is the reputation of its present faculty. For those with reputations established over many years, it would be highly irrational to stop publishing because of such a minor inconvenience as no secretary. As we have already argued, senior members have all the motivation that one could ask to continue publishing whether they have secretaries or not.

On the other hand, if the university is to maximize research potential, it has a substantial stake in providing junior faculty with all the assistance it can. If young faculty are encouraged to publish and are provided with help toward that end, the university will maintain its vitality and quality and attract increasing funding. This amounts to recognizing that the university has a great deal to gain by assisting junior people and a great deal to lose by concentrating on aiding only senior people. If publications and reputation are any source of funds then the university has much to gain by helping qualified young scholars establish a reputation as quickly as they can. For the sooner they become qualified researchers in their own right, the larger the percentage of actively producing faculty and the greater the potential for attracting income to the university.

This point leads to another equally obvious one. If we have young people on a faculty who can do the kinds of research which will insure the development of their reputations and the university's then the potential should be fostered in all ways possible. In particular senior men should be encouraged to handle more of the teaching load than they normally do so as to free the junior men for research. In this way we maximize the potential for publication and, hence, income to further development.

But now we can turn to the most important point of all. For it has been a working assumption here that publication is a good measure of reputation, both for faculty and university, and this assumption is surely suspect. Rewarding only publication, for whatever reason, contributes to publication pollution. There is a sense in which the requirement to publish is counter productive to the overall goal of increasing knowledge. Rather than encourage thorough checking of results, tracing all implications of research, and eliminating duplication, the rush to add yet another entry to one's *curriculum vitae* generates so much trivia that it is nearly impossible to ferret out truly landmark work. What social or epistemological good is a breakthrough publication if it is so buried in "pollution" that no one reads it?

Thus, in considering factors of rationality, financial efficacy and value to the sociey, Tullock's conclusion is in doubt. Indeed it seems he has it backwards. The university president ought to discriminate against full professors in favour of junior personnel. Knowing that he is both rational and an advocate of efficiency, we suggest Professor Tullock lead the fight for discrimination against senior faculty in favour of junior at his own university

UNIVERSITIES SHOULD DISCRIMINATE TOWARD ASSISTANT PROFESSORS

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Recently Gordon Tullock suggested (perhaps with tongue in cheek and his beautiful secretary guarding the door of his handsome office) that universities should discriminate against assistant professors. This conclusion seems to be based on rather simple assumptions about universities' objective functions and about the relative costs and returns from publications to assistant versus full professors. By a slightly different assumption about the objective function, he could have made his case even stronger. For example, by assuming that universities wish to maximize *prestige* which (let's say) depends on both the *stock and flow* of publications, he could have made a case for universities hiring more full professors to replace assistant professors; thus, adding job discrimination to "facility" discrimination.[1]

However, a more reasonable description of a university objective might be "the creation and dissemination of knowledge"; this criterion could be separated into such components as research, publication *and teaching*. Differences in relative abilities and preferences for teaching *vis a vis* publication and in relative bargaining power usually lead to the assignment of higher course loads and a higher proportion of undergraduate courses (and otherwise undesirable tasks) to assistant professors. Also, universities may actually reward excellent teaching with tenure, promotion, and/or salary.[2] Thus assistant professors may have less actual time available for creative activity than full professors and the incentive to use that creative activity for research rather than teaching may be relatively low. The latter will especially be true if assistant professors find it more difficult to get their research published,[3] to get grants to support complementary factors (e.g. research assistants, secretaries, ghost writers, etc.), and to realize the prospective income gains without many unpleasant changes in environment[4] since all of these tend to lower *expected* net benefits from research activity. If professors are actually more creative (per unit time) at a younger age and if it is "socially desirable" that creative energy be channeled into research, then it would be appropriate for universities to discriminate toward assistant professors in the allocation of facilities.

This conclusion is strengthened when one considers that such facilities enable the production of human capital that is employable at any university. Thus, from the standpoint of any single university, it may be more efficient to allocate such facilities to support the teaching program or the research of those professors with the longest expected career at *that* university.[5] Older, well known professors who are often the willing object of "faculty raids" would hardly qualify for such investment.[6] Besides, universities might reasonably expect such highly paid professors to be "self-financing," that is to bring in enough grants to pay for additional research facilities and university overhead.

FOOTNOTES

[1]Of course, if many universities simultaneously pursue this policy there may only be a redistribution of full professors between universities, a redistribution of income from assistant to full professors, and a low net gain in prestige for all universities if the elasticity of supply of full professors is low. Fortunately for assistant professors, only a few "top" universities apply this policy with a vengence.

[2]Given the current tendency toward small budget increases and "across the board" salary adjustments, higher rates of research and publication may not result in significantly higher salary increases *at a given institution.*

[3]As might be indicated by some findings of Alice Vandermeulen ("Manuscripts in the Maelstrom: A Theory of the Editorial Process," *Public Choice* 13 (Fall, 1972) pp. 109-10) and R.P. Strauss ("A Younger Economists's Views on the Market" *American Economic Review* 61 (May, 1971) pp. 330-2).

[4]Universities tend to utilize their monopsony position to pay lower than market salaries either because of true cost minimizing motives or misguided attempts at internal egalitarianism. To gain the market salary, the young economist might have to prove himself ready to leave what otherwise he might consider a very pleasant environment. Not many young professors with distinguished academic records can capture their true value by simply moving to an uptown university. Of course, to some, regular moves may seem desirable *per se.*

[5]Part of the difference between market and actual salaries may reflect these expenditures by the university. For related points see G.S. Becker, "Investment in human capital: a theoretical analysis," *Journal of Political Economy* 70 (Oct. 62) pp. 9-49.

[6]Of course, this ignores that the administrator may prefer to bet on a proven "winner".