COMMENT

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JOHN DUNLOP has presented what are certainly some of the most provocative remarks to appear in a scholarly journal in the labor field in many years. We find much to agree with in his remarks; however, we also find many areas where we feel he condemns research because of his overly optimistic expectations about its ability to contribute to the policy process, and other areas where he appears to be unaware that research in labor economics has already contributed fairly directly to policy decisions.

Who is right—Keynes or Dunlop? Does academic research lead to policy decisions, or do academics distill *their* frenzy from the ravings of some very active politicians? In many cases Dunlop is clearly correct. The surge in interest in the economics of labor market discrimination, for example, was clearly a result rather than a cause of the civil rights movement. Also, interest in the economics of trade unions peaked in the late 1940s and early 1950s, but it was a relatively quiet branch of labor economics prior to and even during the late 1930s when, of course, fundamental questions concerning unionism were being resolved. In other cases, however, the causation runs the other way. The current push for deregulation has its intellectual foundations in the huge accumulation of prior work on the effects of government regulation on the operation of markets. Similarly, the public's apparent decreased tolerance for high unemployment (as compared to that during the years before 1940) probably stems from the recognition, based on the work of Keynes and his successors, that deep depressions are avoidable. Thus, the truth lies somewhere between the positions of Keynes and Dunlop on this issue. Ideas affect policy and vice versa, and variable lags operate in each part of this feedback process.

The *de rigueur* ending of the Ph.D. thesis or young labor economist's article—"Conclusions and Policy Implications"—represents as Dunlop implies, either extreme naïveté, extreme egotism, or both. One cannot expect ideas to leap from the pages of *The American Economic Review*, this journal, or even the *Brookings Papers on Economic Activity* into bills being signed on national television by the President. Policy making is an inchoate process, and economists—even labor economists—have an effect on this process in their professional capacity. We will argue that this has been true even in two of the cases in which Dunlop does not find any effect.

For many purposes it is convenient to divide the labor field into three parts: (A) the analysis of individual collective bargaining situations, or industrial relations; (B) the theory of labor market intervention, or applications of microeconomics to labor market analysis; and (C) the theory of aggregate labor market behavior, or applica-

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tions of macroeconomics. In broad terms we interpret Dunlop as saying that A should be the prime concern of labor economists and that the research under C (the Phillips Curve maze) is not relevant for influencing outcomes under A. Furthermore, by implication, B is also irrelevant for A. We feel that Dunlop's attack on C is unfair and that he completely ignores B—the area in which most of the good work in labor economics over the past fifteen years has been done.

The analysis of the relationship between inflation and unemployment has admittedly flowed into many backwaters that have had a singular lack of influence on policy. This has been due either to the narrow technical nature or the sheer silliness of the policy implications drawn in these analyses.[1] The mainstream of research on this subject, however, beginning with Phillips' seminal paper, has washed the entire policy debate along in its flow. Before 1960 discussion of macroeconomic policy did not center around the tradeoffs; since then, as we saw in the 1976 Presidential campaign, the major focus has been the choice between stimulating the economy to lower unemployment and letting things slide to avoid touching off more rapid inflation. The obverse of this debate was clear in the popular discussion of President Nixon's economic policies in 1969. More recently, the notion of a vertical Phillips Curve has entered the popular debate, providing at the present time an intellectual basis for proponents of more rapid economic growth and opponents of seemingly-free lunch programs such as Humphrey-Hawkins. No doubt, now that these ideas have been explained by the editors of *Business Week* and the *Wall Street Journal*, their effect on the policy debate will be enhanced still further.

Dunlop does not view this research as a "useful contribution to policy making" primarily because it cannot be used in setting up systems of wage controls. But the purpose of the Phillips Curve literature was to investigate the nature of the inflationary constraint on macroeconomic policy, *not* to devise the perfect control system.[2] Thus, Dunlop's criticism of the Phillips Curve literature is both unfair and misdirected.

Dunlop ignores a host of areas in which labor economists have done very solid work that has in turn had a direct influence on policy. One such example is the continuing debate over the desirability of a high legal minimum wage. For decades economists have been pointing out that although a high minimum wage will make marginal (i.e., teenage and female) workers better off per unit of time, it will diminish the number of jobs for such workers. Thus, a high minimum wage is likely to exacerbate the unemployment problem for those who suffer the highest unemployment rates. As a result of the publication of some recent technical papers (using a wide range of modern techniques), the economic effects of high minimum wages have come to be recognized by opinion leaders (the *New York Times* wrote recently of the "minimally useful minimum wage") and policy makers (the Carter Administration held the line at a minimum wage of $2.65 when $3.00 was anticipated).

The amorphous field called human capital theory has so many facets that it is easy to claim a lack of relevance for policy despite the strenuous efforts of so many researchers. Economic policy has (quite rightly) been unaffected by the recent interest in the economics of marriage and other aspects of behavior previously the province of sociologists and demographers. Similarly, the substantial work designed to pin down definitively the fourth derivative of the age-earnings profile has not had a discernible effect on legislation, program administration, or the general policy debate. Nonetheless, the development of the view of training as an investment—of forgone current earnings and later returns of

[1] For example, Perry's results on the "effects" of profit rates on wage inflation led some economists to advocate a profits tax as a means of controlling cost-push inflation. Fortunately, given that the basis of this policy proposal was the weak aggregative evidence provided by Perry and others, it had little effect on actual policy formulation, and profit rates have, of course, long since been dropped from aggregate wage equations.

[2] Indeed, the clear implication of most modern wage determination models is that the optimal wage control policy in a U.S.-style economy is no policy at all.

differentially higher earnings—has affected the way noneconomists think about labor market policy. In the narrowest sense this effect has been manifested in the (often misguided) concern with measurement of rates of returns. More broadly, it has shifted the focus of the discussion of manpower policy away from training programs solely as a redistributive device and toward viewing them as investments that increase the amount of resources in the economy. Here again, the chief effect has been the generalized change in the way policy is considered, not any immediate policy change that flowed from some specific piece of research.

Human capital theory has been increasingly utilized in a number of specific situations, however. Evaluations of the impact of programs for affirmative action and occupational safety and health, among many others, make use of micro wage equations that were developed on the basis of the human capital literature. Moreover, there is an enormous potential for human capital models to be used in individual collective bargaining situations. It could be argued, for example, that public utilities should not be allowed to pass excessive labor cost increases on to consumers and that this question should be analyzed in the context of human capital models. Moreover, human capital analysis is used widely in litigation involving wrongful death or injury, and it is likely that in the future even some of the more obscure aspects of the theory (like hedonic prices and the economics of marriage) will have legal applications.

Despite these areas of success there is room for improvement, and there are directions research can take that will make it both more useful and intellectually more satisfying. The most important of these directions is toward the need to integrate a knowledge of institutions with the work done by analytical labor economists. Too often we have been content to derive our hypotheses and estimate our regressions in at least a partial vacuum of knowledge about the institutions with which we deal. Similarly, institutional economists have too often concentrated on the detailed description of the institutions and the presentation of case studies and paid little attention to how these institutions affect the workings of the labor market and the economic agents within that market. Although we do not wish to proclaim a plague on both houses, each could benefit by accepting the good points of the other's approach.

It is unfair, however, to expect that those who have the requisite quantitative skills to be successful modern economists will also have time to learn about all the institutions associated with each topic they research. In some areas—unemployment insurance is a good example—the institutions are so complex and detailed that the effort required to gain anything approaching complete knowledge of the institution is sufficient to preclude the analysis of the economics of the institution by most economists. Therefore, the careful analytical labor economist must develop the judgment to decide what institutional knowledge is worth acquiring, just as he must decide what abstractions to make in modeling the phenomena that concern him.

Better data are an important need of analytical labor economics, both to increase its relevance to policy and to make it more satisfying intellectually. Better data do not mean more data, though. The government now produces huge collections of statistics that have little use for policy and even less use in enabling us to understand how the world works. The payoff to data collection has not been high for academic economists, but it has not been zero (One need only cite the OEO-ISR and Parnes longitudinal data sets as examples). What is important is that economists who engage in data collection must operate *as economists* and have explicit notions of how the data are to be used. Otherwise, more useless data will be collected and the information needed to answer questions of policy interest will not be produced.

Other areas that Dunlop views as fruitful for economists—knowledge of organizations, their decision-making processes, and their international interdependence, in particular—require so much detailed work outside of economics as to deter the economist from doing labor economics. These are examples of cases in which the disciplines should not be allowed to cross-sterilize. The complexities of the issue require a division of labor that likely precludes the economist

from a central role, unless, of course, one views economics as an imperialistic discipline that should expand to all areas of policy inquiry.[3]

While labor economics has had more impact than Dunlop admits, there are impediments to its achieving a greater impact. First, in academic labor economics, the simple idea, appropriately dressed up with Hamiltonians and several classes of labor, is more likely to be published in a leading journal than the same idea stated simply and fleshed out with some empirical verification. This barrier is artificial, since we economists ourselves have created it. But there are also natural barriers. Too often economists who accept policy positions in the federal government give no sign that they are economists or that the ideas they have studied and expanded upon throughout their careers have any relevance to their policy-making function. Partly this results from the crush of affairs, partly from a natural desire to be "one of the [policy-making] boys." Also, great pressure against applying the simple analysis of labor economics to policy problems often stems from bureaucratic and constituent fears that a program will be shown to be ineffective or even harmful.

How can these barriers be broken down? We agree with Dunlop that increasing the number of middlemen is not the solution; too often these persons are those who could not succeed either as academics or as policy makers. Dunlop's suggestion that labor economists broaden their focus has merit if it means we consider some of the more relevant, previously ignored *economic* and institutional aspects of the problems. By being better economists we can have a greater impact on policy; by branching out into the focus and methods of other disciplines we may well become second-rate sociologists.

The average congressman does not understand differential equations; but to produce good analytical economics that can be useful, the labor economist must employ this and other arcane aspects of mathematics and statistics. He must do more, though, for the congressman to understand him: in addition to publishing in academic journals, he must be willing to spend the time rewriting his ideas in nontechnical language. Occasionally, too, he must broaden his focus beyond the narrow object of his academic research and consider and comment on the economics of the entire policy or program with which his own narrow academic research deals. Failing to do this leaves the labor economist open to Dunlop's charges of uselessness, and still worse, leaves the policy debate open to those who ignore its economic aspects.

One way labor economists can be stimulated to broaden their focus while retaining their ability to do analytical labor economics is for them to spend a year or two in government during the second five years of their career. To do so any earlier is likely to result in their abandoning analytical labor economics before they have developed sufficient skills to enable them to produce useful analytical work, while delay beyond this means the experience comes too late to alter the person's view of research and policy.

As much as we would like our ideas to be heeded, we should not expect economics to be the major determinant of all labor market policy. There are, after all, relatively few economists, and we are but one voice of many seeking to influence policy.[4] But with a litle more effort to sell our ideas our influence can be increased slightly, and we can do so without sacrificing what has already proven a very useful approach to problems of labor market policy.

[3]Perhaps the most valuable point an economist can contribute in the creation of organizational structures is that a system must possess incentives for the desired policies to be carried out. The CETA system, for example, was set up with a weak set of incentives, and its resultant failure was easily predictable.

[4]Stigler has recently pointed out that if economists were so extremely valuable to society, surely the market would have led to a large expansion beyond our current numbers. That this has not occurred, and that we are expanding only slightly more rapidly than all other occupations is a good indicator of our value to society—increasingly useful, but not quite so prized as we might hope.