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Comment: Is There a Meaningful Trade-off between Inflation and Unemployment?

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Most highly influential works in economics are either pure deduction or a close blend of deduction and more or less casual or systematic empiricism. But we have also a few pieces of almost pure induction, such as the Gibson paradox, that are part of the equipment of nearly every person with some economic training. The Phillips curve has acquired this status amazingly quickly; another recent success story of this type would be the Leontief paradox, but it is not even a close second. On the usual time scale of the dissemination of ideas in economics, the Phillips curve and the associated “Dilemma” problem achieved a prominent place in undergraduate textbooks almost instantly.

It is rather fascinating to pose this story as a problem in the sociology of knowledge and to ask what makes almost an entire scientific profession accept a piece of induction of this sort as being (a) “a fact,” and (b) a fact of such evident importance that it is recognized as belonging, so to speak, on the ground floor of any doctrine aspiring to widespread acceptance. We are, after all, not dealing with a finding that decisively revealed previously held, fundamental beliefs as “false.” On the contrary, what is curious about the Phillips curve is that it produced, as it were, a strong *déjà vu* reaction, the reaction: “Oh, yes! That puts the whole problem-complex in focus, all right.”

Phillips’ original article was of course an impressive piece of work. Still, it is curious to note that the *déjà vu* reaction was so strong that the Phillips curve immediately achieved a life of its own in professional discussion and teaching even while the numerous attempts to reproduce the experiment with American data, which quickly followed, failed entirely to isolate a comparably “neat” and reliable relationship. Neither the failure to find a similar simple and stable relationship for the United States nor the fact

* Except for some abbreviation, this Comment remains as written for the Conference. It does not, therefore, reflect the revisions made, or the new results incorporated, in Professor Brechling’s paper.

that the simplicity of the Phillips relation soon began to dissolve in the further work of British economists seems to have affected the widespread conviction that the Phillips curve “makes sense” and points out an important issue.

This conviction, of course, is also reflected in the subsequent, never ceasing flood of inductive experiments in search of a simple relation of similar nature. Given the allure of the Phillips curve, this is to some extent explainable, for there are any number of problems with the imperfect correspondence between the available data and even the roughest conceptual categories. The hope is always there that, with the right choice of variables or the right proxy for “structural change,” a dramatically more reliable relationship will pop out.

Phelps’s forty-nine-item bibliography is only a sample of this literature. For someone who is not squarely in this field, it is difficult to say how much has been accomplished by the proliferation of studies of this sort. The “hypotheses” tested seem all too often to be of the type: “It seems reasonable to suppose that, by using variable x as an additional or substitute independent variable, a better regression result should be obtained.” When the theoretical underpinnings are no more ambitious than that, there is almost no basis on which to compare results, and new studies seldom knock old ones out of consideration.

I must admit that I find Part III of Brechling’s paper no more helpful in this regard than most of the earlier studies that I have read. I have puzzled over his regressions of the rate of change of the GNP deflator on the non-wage markup and total wage-bill per unit output, but I do not really know what—or how much—to make of the results. I am not sure what hypotheses are hinted at in such statements as “There is some conventional economic theory . . . which suggest that all three components of p may vary over the business cycle,” and so on. Here, I think, we need more theoretical work on the optimal short-run pricing-, output-, and employment-policies of firms faced with fluctuations in demand. Such models should, in particular, face up to the “fixed factor” nature of part of the work force.

Since it asserts a straightforward “no” to the question of whether a trade-off between employment and inflation exists, we are especially interested in Friedman’s argument that the adaptation of price expectations, and therefore of market behavior, to experienced rates of inflation will shift the short-run Phillips curve upward if lower than “natural” unemployment levels are made to persist and that the long-run relationship is simply a vertical line. (This is also the result of Phelps’s model.) The test that Brechling applies to this hypothesis yields a weak contradiction; but this is reconciled by the statement that “the long-run effects of a marginal reduction in unemployment tend to be much stronger than the short-run effects.”

This being so, one need hardly feel dishonest in sticking with Friedman's a priori argument which is simple, straightforward, and has all the authority behind it of the well-established basic propositions of price theory.

One would feel more comfortable criticizing those who have undertaken the brute work of "curve-fitting" for such sins of commission were it not that those of us who lack either the competence or simply the taste for econometric efforts are responsible for sins of omission that are just as serious. It has taken almost ten years for the question of whether there is a theoretical rationale for the presumption that a stable Phillips relation exists to come onto the agenda. Systematic scrutiny of this issue seems long overdue indeed; and Phelps's paper represents a very impressive start.

Brechling's discussion proceeds within the framework of a loosely sketched political decision model. This is so much in fashion nowadays that one can well understand the impulse, in dealing with almost any conceivable problem, to throw in some criterion function with the usual convenient properties and then to complete the formalities by briefly checking the second-order conditions for a well-defined optimum. But in this case I must feel the exercise to be premature. Is it not a bit mischievous to put together such a seemingly neat decision model and then to derive propositions about "socially optimal policies" when the functional relationships constituting the "opportunity set" are so ill-understood that the very existence of the animal is in doubt?

On analytical grounds, what bothers me more is that, quite apart from the questions relating to the alternatives of choice, I can attach no sound sense to Brechling's "collective utility function." Brechling assumes that "a nation can arrive at a collective utility function which is like an individual utility function." I doubt that the percentage rate of national unemployment, the rate of inflation, and the balance of payments deficit are prominent arguments in the utility functions of very many households. And is the inflation unforeseen or foreseen, or unforeseen by some while foreseen at least by the policy makers? If foreseen, is it also fully adjusted for? If so, need we bother?

I look at the policy problem here more as one of managing social conflicts (between debtors and creditors, unemployed and well-to-do) than as one of maximizing aggregate output and consumption of goods and services over time. Hopefully, we can presume that the powers that be will at any point in time exhibit the "increasing marginal rate of substitution" between inflation and unemployment that Brechling assumes. I think the matter should be left at that. The dangers inherent in any given rate of inflation, for example, are apt to depend upon the actual rate experienced up to the time in question. The indifference map, therefore, is likely to shift about. Choice models in which tastes are not quite independent of opportunities are usually quite messy. If it ever in the far future comes to

choosing in one fell swoop entire time paths for unemployment and inflation, the problem will be even more intricate than Brechling's formal exercise makes it appear.

Brechling suggests that, were it not for the disutility of inflation, governments would aim only "at maximizing GNP" and "should raise the level of effective demand until the productive potential of the economy is completely exhausted." This, of course, adumbrates Harrod's subsequently considered view that the proper goal is the "Swedish" one of zero per cent unemployment.

Now, the reasonableness of a goal of zero per cent unemployment, or more generally any given target figure, depends very much on the way in which a particular country defines labor-force participation, and thus unemployment, for official statistical purposes, and also on the incentives present in a given system for persons not employed in the market sector to be registered as part of the labor force. For example, it is generally believed that if Swedish criteria were used here, the resulting U.S. unemployment rate would be significantly lower than the one actually reported.

At any one time, the pool of unemployed will contain (a) persons *actively* searching for a new job at a wage rate which they believe they should be able to earn *steadily*, and (b) persons "passively unemployed" who choose to have a reservation price for their services such that they expect only intermittent employment. This second group is "unemployed" in an analytically relevant sense only if they come to average less employment over time than they expected and wanted. By engineering an unanticipated inflation that makes bid prices for labor move ahead of the reservation wage of these people, they can be "tricked" for some time into accepting more work than they would have done otherwise. This will raise the output of goods and services for the time being, but the change in GNP is clearly not the appropriate measure of the "social gain" from such a policy.

The first category to my mind embodies the old notion of "frictional unemployment." The "frictionally unemployed" we conceive—in the way of Alchian and Allen (1967, chap. xxv)—as actively sampling employment opportunities and as accepting or rejecting those found on the basis of some expectation as to the remuneration and kind of job they should be able to obtain. As this sampling proceeds, their reservation price may be adjusted either upward or downward depending upon how the labor-market situation "looks." On the other side of the market, firms have to regulate the size and turnover of their work force by adjusting their bid prices for labor. (Phelps's paper provides a systematic analysis of this side of the market.)

If labor-market information were costless to produce and disseminate, of course, there would be no revolving pool of "frictionally unemployed." As it is, however, the pools of vacancies and job hunters, on the one hand,

fill a very real social function and, on the other, reflect the optimizing behavior of decision units that have to act under imperfect information.

The point is, of course, that the authorities can fool firms and job seekers by adopting an unforeseen monetary policy and thereby reducing the pool of "frictionally unemployed" below its steady-state level. This will lead to an increased output of "concrete" goods and services, but the "social income" in the proper sense declines despite the increased total output of goods and services, since the alternative use of the resources in producing the information needed for efficient allocation has a higher value.

Still, however, one must not miss the real point in Harrod's position. For, whereas the pool of unemployed does serve a useful social function, this useful function of x per cent unemployment falls to 100 per cent unemployed individuals to perform. The man who lost his job yesterday may be "willingly" unemployed in the sense that the option of shining shoes or selling apples is open to him today. But he is certainly "unwillingly" unemployed in that were he offered the option of changing places with one of his fellows who remains employed (and on the knowledge of whose present wage his own reservation price is based) he would take it. By the nature of the case, the "frictionally unemployed" cannot be paid their social marginal value as producers of information (which, we have argued, *exceeds* the marginal product they would turn out if "tricked" by inflation to accept a job right away). The difficulty here is relevant to the earlier comments on Brechling's collective utility function. Some sort of "aggregative" welfare criterion may be quite misleading when a latent social conflict is at the core of the problem.

In the context of the Phillips-curve literature, finally, Brechling's insistence that "as a rule, unemployment and inflation cannot and should not be treated independently of other objectives of economic policy" is surely a welcome reminder. Once more, however, one would wish that the emphasis had been put on the "opportunity set" part of the decision problem rather than on the utility function part. The opinion seems quite widespread among economists that steady-state unemployment can be reduced through a "creeping inflation" that need not turn into a gallop for a very long time as long as the government does not get altogether overambitious in pursuing low unemployment rates. The hypothesis involved in this position is that the endogenous price-expectation adjustment mechanism, on which the standard argument against the "meaningfulness" of Phillips curves rests, works so slowly in the "responsible" range of rates of inflation that it need not be taken seriously, given the time horizons relevant to political decision makers. On this issue, we must await the results of further empirical research; meanwhile, however, it had better be realized that this a priori argument is not the only one to put the notion of a stable trade-off seriously in question: At least for countries

whose foreign trade looms large in relation to GNP, the feedback effects of today's inflation on tomorrow's trade balance *and unemployment in the export sectors* should be taken into account. For such countries, Phillips-type regressions on the observations generated by past "stop-and-go" policies may well yield a quite "flat" curve. To the policy maker who believes that this regression line represents the steady-state trade-off that he actually faces, the urge to *go* would be nearly irrepressible—the costs in terms of inflation of bringing unemployment to a fraction of 1 per cent are apparently negligible. Yet, he is being misled in more ways than one: Not only does the regression line not tell him of the "stops" that will lie ahead if he succumbs to temptation, it probably gives much too rosy a picture of the trade-off open to him in the go-phase considered separately.

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Comment

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I take it that the term inflation is understood in the present context to mean simply a rise in the price level, with no distinction being made between anticipated and unanticipated inflation, a difference many economists now believe is crucial for the fruitful analysis of many issues in this area. Phelps does assert that expectations have an important role here, and I heartily agree with him. In fact, I find his paper a fine report of good progress toward improved understanding of labor-market dynamics. Regretfully, his work has not yet progressed enough to tell us very much about the substantive content of "trade-offs" between inflation and unemployment, except in the sense that his analysis points to many of the flaws in much of the Phillips-curve literature. I hope also that he will expand his analysis to include explicit concern for an open economy in which internationally traded goods have some bearing on domestic prices as well as on what is taken to be the appropriate general price index.

Phelps's paper contains much of my criticism of Brechling's paper. However, there are several points I wish to emphasize and several additional ones I wish to make, especially since the Brechling paper is typical