

Spinelli and Fratianni on Inflation: A Comment

In the December 1980 issue of this Review, two articles appeared on inflation in Italy (Spinelli and Fratianni): in both articles, the authors express certain critical remarks on some of my views on this subject. A brief comment on such remarks is called for.

In several writings (e.g. Sylos Labini 1974), I have considered three equations for interpreting the short run (yearly) changes of wages in different historical stages of industrial capitalism:

$$\Delta W = a_1 + b_1 U^{-1} \quad (1)$$

$$\Delta W = a_2 + b_2 U^{-1} + c_2 \Delta V \quad (2)$$

$$\Delta W = a_3 + b_3 U^{-1} + c_3 \Delta V + d_3 TUP \quad (3)$$

where W is the wage rate in manufacturing industry, U the rate of unemployment, V the cost of living, TUP the trade union pressure, quantified, for instance, by the days lost for strikes; Δ indicates a rate of change. Equation (1) is largely applicable to the period up to the first world war; equation (2) holds good for the forty or fifty years after 1920; equation (3) applies to the last fifteen years or so. The change depends on the growth of trade unions, which were relatively weak in the last century, stronger in this century and even stronger in recent times, particularly after the "wage explosion". In the last century, owing to the weakness of the unions, wages in certain years were increasing less than prices in prosperous periods (wages were "sticky") and decreasing more than prices in recessions: only in the "long run" — as a rule, the duration of a business cycle — did real wages recoup temporary losses or increase. When the unions had become strong enough, they were normally able to avoid cuts in real wages even in the short run (year by year) and often succeeded in obtaining increases; this they achieved by means of strikes and, in certain countries, also by means of escalators clauses. During recent years the trade unions in certain countries have become so strong, due in part to the support of the government, as to be able to launch, intermittently, lengthy strikes to obtain not only wage rises considerably bigger than price rises, but also other pecuniary and non-pecuniary advantages. This is why in the wage equation a third variable is useful, though it must be clear that such a variable is not and cannot be

significant every year: in the first world war a battle of Verdun was not fought every month or even every year.

This introduction was necessary to dissipate certain misunderstandings that I have found in both the articles under examination.

In discussing the cost-push hypothesis explaining inflation, Spinelli concentrates his criticisms on the "discretionary component" in the process of wage determination, which, as far as I am concerned, is represented by the third variable of equation (3), i.e. the number of days lost by strikes, which I have suggested as a possible (but not as the only conceivable) index of trade union "pushfulness".

Now, the cost push does not necessarily come from that discretionary component in wage determination: it may come from various elements of the cost of living; in the sector of industrial prices an increase in raw material and oil prices is a cost push, even if the prices of these commodities rise owing to an expansion of demand. As for the trade union pushfulness, I have always thought that this variable can be really significant only in those "hot" years characterized by lengthy strikes, like 1963 and 1970. In fact, when Del Monte discussed with me the possibility of using, in his analysis, a dummy for those two years instead of the "strike variable", I had no objection (Del Monte 1973). In my judgement, the relevance of the trade unions appears principally and continuously in the very structure of wage equation number 2, which includes the cost of living; for the classical economists and for Marx, money wages were influenced by the cost of living, not in the "short" but in the "long" run (several years), for reasons related to the efficiency of the workers. As a matter of fact, as Phillips has shown, in the past century the changes in consumer prices affected wages in the short run only discontinuously.

Spinelli is particularly interested in demonstrating that there is no long-run trade-off between wage inflation and unemployment. This view goes back to Friedman, according to which the Phillips curve has a certain validity only in the short run; in the long run, wages do not depend on unemployment, which, on the other hand, tends towards a "natural rate", determined by (mysterious) real factors. Thus, all the attempts of the government to reduce unemployment below that rate by means of an expansionary policy have the effect of reducing it only temporarily: the lasting effect is an increase in prices.

I have always pointed out that my three wage equations apply only to the short run; for the long run, the variations of wages tend to approximate those of the value of average productivity (Sylos Labini 1974, pp. 94-5). In other words, fundamentally I agree that unemployment affects short-run fluctuations of wages but not their trend. However, in the long run unemployment and wages are not unconnected. Whatever the cause of wage increases, when such increases exceed those of productivity systematically and for a non-negligible number of years, they can in certain conditions erode the profit margins in industry. Such an erosion can slow down the expansion of investment and

therefore the increase of employment (Sylos Labini 1974, pp. 151-57). Then, in the long run unemployment can increase as a consequence of sustained increases of wages. Such a relation, however, is different from the Phillips relation ($\uparrow U \rightarrow \downarrow \Delta W$), since it works in the opposite direction ($\uparrow \Delta W \rightarrow \uparrow U$).

All these observations go far beyond the question of the "strike variable". I have recalled them merely to make clear how secondary this question is in an analysis of the relations between wage inflation and unemployment.

Michele Fratianni, referring to the views of Spinelli, criticizes an allegedly purely sociological explanation of inflation, according to which inflation would depend on a wage push, due, not to market conditions, but to "a complex array of non-economic factors". Since Fratianni quotes approvingly the criticism that Spinelli addresses to me and other economists concerning the "autonomous" wage push, I must point out that I consider this push, not in isolation, but together with endogenous pushes coming from the labour market and from the variations in consumer prices; I must also point out that, later on (p. 525), Fratianni himself accepts the idea that in certain years (such as 1970) an autonomous wage push can in fact take place (he calls it, in this second passage, "exogenous"). Then Fratianni emphasizes that a bi-directional causality between prices and wages can be found — a concept quite clearly embodied in the analysis worked out in my 1967 essay from which he quotes (Sylos Labini 1974).

In criticizing a certain version of the cost-push explanation of inflation that he attributes to me, Fratianni writes: 1) such a version "begins with the assertion that sustained increases in the general price level result from the market power of large monopolistic firms which have complete control over price determination"; 2) "prices are determined jointly by both demand and supply schedules and not by supply forces alone"; 3) since "the ability of a large monopolistic firm to set a price can explain at best changes in relative prices but not a continued increase in the general price level", then, "to explain the inflation of the recent past, one would have to seriously entertain the empirical proposition that accelerations of inflation are accompanied by a larger (and larger) degree of monopolization" — a proposition (p. 50) that Fratianni judges to be "self-evidently disconfirmed by facts".

Fratianni addresses these criticisms not only to me, but also to other economists, particularly Galbraith. I am sorry to say that, as far as I am concerned, these criticisms radically misrepresent my views. I have always emphasized that large firms operating in concentrated industries, as well as relatively small firms operating in highly differentiated industries, are far from having complete control over prices: their market power meets with important limitations in the home market as well as from abroad — so much so that they are able to shift on to prices only those cost increases that are general, though not necessarily equal to all firms, and, at least in the short run, not even fully (see e.g. Sylos Labini 1979). Those limitations are such as to determine, under certain circumstances,

a systematic deterioration of profit margins and of the share of net profits in manufacturing, and thus reduce the self-financing capacity of the firms and compel them to increase the burden of external debt, as I have repeatedly pointed out. These observations apply, not only to the first, but also to the third of the above assertions (acceleration of inflation and increasing degree of monopoly).

As for the second assertion, I suggest that the cost-push theorists are not so remiss as to forget that goods, once produced, have to be sold to people who demand them. The point is that, if we assume that normally, industrial firms have unused capacity and that direct costs per unit are constant, at least in the relevant range of output, it follows that changes in demand determine changes in output, not in prices. These two are precisely the assumptions that I explicitly make and work out; nowadays, the empirical evidence in favour of both seems to be considerable. After distinguishing several categories of prices, one would find that, in fact, demand has a direct role in price variations of certain types of goods, e.g. agricultural products and mineral raw materials; in particular circumstances, even the prices of industrial products can be directly affected. I have worked out on several occasions all the above propositions — without much success, judging from the articles by Spinelli and Fratianni.

But the most serious objection that can be raised to both authors' arguments is that they choose as the object of their analysis the "general price level". It seems impossible to deny that the behaviour of various important categories of prices differs considerably both in the short and in the long run. To be sure, it is not *only* a question of market forms — a proposition that I have never maintained; it is also a question of the technical conditions of production and of the organization of the markets. Different behaviours imply different logics; if this is so, it is wrong to concentrate the analysis on the "general price level". The empirical results of such an analysis will not be particularly misleading only when the behaviour of the various categories of prices does not differ substantially, but it will be misleading when the differences are substantial. (In any case, the logical validity of the approach is much more important than any empirical result.) Fratianni illustrates the tests of two explanations of inflation: the "dominant impulse hypothesis" (DIH) and the "Phillips curve and mark-up pricing" (PCMP). In DIH, the author considers the demand for output — which is conditioned by money —, the supply of output and the internal and the foreign price levels (actual and expected); in the PCMP model he refers to the work by Modigliani and Tarantelli (1972) and considers two equations

$$\Delta \dot{W} = h + fE + g\Delta \bar{P} \quad (I)$$

$$\Delta P = c_1 + c_2\Delta(W-PR) + c_3\Delta PM \quad (II)$$

where W is the wage rate in industry, E is an index of excess demand in the labour market (which can be quantified either by the inverse of unemployment

or by a more sophisticated index with a similar meaning), P is the internal price level, PR is the average productivity in industry and PM the foreign price level; Δ indicates a rate of change and the bar over a variable indicates an expected value.

The empirical results are good in both cases, though — so it seems — statistically better for DIH.

First of all, I would like to observe that the PCMP model can be found almost literally, with reference to manufacturing industry, in my 1967 essay that Fratianni quotes in his bibliography, so that I found myself in the peculiar situation of being criticized in Sections II and III and implicitly praised in Section V.

From the standpoint of the statistical "forecasting power" of the two models, DIH seems superior, but, from a theoretical point of view, this is not so. In fact, looking carefully at the different coefficients in the three estimates, in each of them, at least one of the theoretically important variables is not significant. What really matters, however, is the concept which is taken as the object of the analysis, that is, the general price level. If it is true that this is, in principle, a misleading concept, then strictly speaking both tests are to be rejected, and it would be necessary to estimate different equations for different categories of prices and unify them only at the end of the analysis, for statistical and illustrative purposes. In particular, the second equation of the PCMP model can be criticized on two grounds. First, one can safely assume that the mark-up pricing applies to the industrial products; it does not apply, for instance, to agricultural prices. Further differences and complications arise in considering services, rents and the relations between wholesale and retail prices. In short, that model can be applied to wholesale industrial prices, not to the cost of living. Second, "import prices" cannot be taken as a homogenous variable; at least two categories of import prices must be distinguished, the former — raw material prices — relevant as an element of cost in manufacturing, the latter — prices of finished products — relevant as a limit to the increases of domestic industrial prices. In other words, the former is a moving floor, the latter is a moving ceiling: the two must not be put together. When considering properly the PCMP model, that is, applying it only to the manufacturing industry, then the forecasting errors noticed by Fratianni in the period 1953-1977 (in 1958, 1970, 1976 and 1977) will practically disappear. The further step is to include that model in a larger model, embracing different categories of prices as well as several other economic variables. In such a framework, the role of money, too, will appear in its proper light: that role is not negligible but neither is it so decisive in determining inflation (as well as other economic changes) as most monetarists claim.

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