

Comments and Discussion: The Bosworth and Gordon Papers

Alan Greenspan: I thought Bosworth's paper was excellent. I am grateful for the presentation and assembling of data that had not previously been properly correlated—for example, the reconciliation of Pay Board wage approvals with the Bureau of Labor Statistics data on union wage settlements. In evaluating the impact of the current controls, I see two important issues: One, to what extent does the evidence indicate that the controls are suppressing underlying pressures for wage and price increases? Two, if they are not, how do we explain an “immaculate conception” disinflation beginning on August 15, 1971?

Undoubtedly, a significant disinflation began with the freeze. Although individual estimates of the precise impact may vary, something significant happened at that point. Nonetheless, the data lead me to conclude that the impact of both the Price Commission and the Pay Board on actual prices and wages has been surprisingly small. First, the overall price situation indicates that, with the exception of a few building materials, few prices would be raised if the controls were removed. For example, recent evidence on the term limit pricing agreements indicates that, whereas the average approved price increase was about 2 percent, only a small proportion of these increases have been put into effect. Thus, it appears that because of weak market conditions, these firms were unable to raise their prices even with Price Commission approval.

The wage side is a little more difficult to evaluate. The institution of a general wage standard would be expected to narrow the distribution of wage increases by reducing the larger increases. Some evidence of this is apparent in Table 5 of Bosworth's paper for union wages. But in the manufacturing area 60 percent of the union wage increases were still above 7 percent. In the nonunion area, the very low average increase suggests that the general

wage ceiling was not the primary influence. Finally, a higher quit rate would be anticipated if the controls were holding wage increases significantly below the rate that market forces would have otherwise produced; yet this is not evident in the data for manufacturing. I conclude that the evidence at this stage does not strongly indicate a significant impact of the operations of the Pay Board and the Price Commission on the wage and price structure.

What, then, happened on August 15 that slowed inflation? Is it conceivable that the mere postulation of a 5.5 percent general wage standard generated a major change of pace? The experience of incomes policies of other countries argues for great skepticism about that possibility.

To explain this phenomenon basically requires more knowledge of what was going on in the 1968–71 period. In other words, the proper evaluation of a controls program requires a conceptual framework for the structure of wages and prices at the time the program was initiated. Until this question is answered satisfactorily, we will not fully understand the impact of the controls. Alternatively, we may get a better understanding of the controls if inflationary pressures resume and put the guidelines of the Pay Board and the Price Commission to serious test.

Robert Solow: I would like to focus on the differences between Bosworth's and Gordon's interpretations of the past year. Bosworth does not offer a precise estimate of the effects of the first year of the controls. Instead, he makes some informal comparisons of prefreeze and postfreeze rates of change of wages and prices, using alternative measures for each, and dividing the interval since August 1971 into various subperiods. This comparison is supplemented with a description of the rules and criteria imposed by the Pay Board, by the Price Commission, and by Congress.

Bosworth's conclusion appears to be that the Pay Board has been more effective than the Price Commission; and, if the controls have had any significant redistributive effect, they have reduced real wages. It is not clear how strongly he thinks that this last is true, but it is the impression that I got. Yet his table shows that wages and prices have both decelerated by about an equal amount. His feeling that the Price Commission is probably not responsible for this improvement seems to come from looking at the type of criteria they use, especially the permissive cost pass-through provisions. He concludes that the Price Commission's operations could not have had much restraining effect.

This conclusion stands in sharp contrast to the results that Robert J.

Gordon reports. He concludes that the price deflator for private nonfarm output is almost 2 percentage points lower than it would have been without controls, while the corresponding wage index is only about half of a percentage point lower. As a result the ratio of price to unit labor costs is 1½ percent lower than the model predicted would have been the case without controls; thus about \$14 billion of property income was shifted to wages.

Part of the difference between these two results can be traced to the fact that the evaluation of controls in the Bosworth paper focuses on the second quarter of 1972, whereas Gordon's paper stresses the average of three quarters of the controls program—from 1971:3 to 1972:2. Although Bosworth analyzes the whole period in detail, he discounts the wage data for earlier quarters because of special factors associated with the freeze and the post-freeze periods, such as retroactivity disputes and catchups. He seems to conclude that the second quarter is when the air cleared and when we can get a distinct view of where we really stand. I too find it rather hard to believe that the Pay Board and the wage standard were as insignificant as Gordon's figures suggest.

In spite of Bosworth's lack of enthusiasm about the Price Commission, he argues that, in the long run, the focus of controls should shift more heavily onto prices and away from wages. If the price line is held, he argues, employers would do a good part of the policing on wages, and would have added motivation to reduce costs as well, provided that the price control regulations do not automatically allow for the pass-through of labor cost increases.

That proviso looms large in terms of the politics of price controls. If a price regulation system can be guaranteed not to pass cost increases through into prices, I would tend to find Bosworth's argument convincing. But if wage controls are to be part of the system, so must some rules for cost pass-through. One branch of the government cannot keep on endorsing wage increases while another branch insists that they must be paid out of profits and cannot be added into prices. But, as Bosworth suggests, a general cost pass-through on the price side unites workers and employers on the wage side. It becomes very difficult for the government to resist wage increases when employers are willing to give in with confidence that they will be allowed to pass them through into prices.

In a recent article Arthur Okun appears to take a rather different view from Bosworth by favoring more informal and selective controls. Robert J. Gordon has yet another position, favoring manpower policy to reduce the

dispersion of unemployment. But I am inclined to believe that the comprehensive controls in place since August have made fiscal and monetary policy makers willing to push expansion somewhat more vigorously than they would have done otherwise. Similarly, for the long run, controls would permit us to operate the economy at slightly higher utilization rates than otherwise. So I am coming around to the view that not only is it necessary to keep some kind of controls apparatus in existence on a standby basis, but it had better be a rather formal apparatus.

One last remark of a more technical nature. I am unhappy with the variable coefficient model presented in Gordon's paper. I have no conviction that the coefficient is fixed, but I cannot believe the results of a whole statistical enterprise that really rests on one observation—namely, the period from 1968 to 1971. No matter how hard we mine the data—even strip mine the data—there is no way to get results that warrant any faith at all unless and until we get more episodes of persistent inflation or disinflation.

Otto Eckstein: In comparing alternative wage models for the post-Korean period, the essential issue turns on the substantive question of how they deal with two basic mysteries about the behavior of wages over the past decade. One mystery is why the wage experience from 1964 to 1966 was so good and the second is why it was so bad from 1969 to 1971. For the earlier period, alternative explanations revolve around the role of the guideposts or the influence of the composition of the labor force (as reflected in “disguised” unemployment of women and teenagers). For the second period, the alternative explanations are either a buildup of inflationary expectations or changes in the structure of the labor force (greater dispersion of unemployment rates among groups).

I turned to the expectational type of explanation only after realizing that the experience of early 1971 could not conceivably be explained by any labor market theory. After considering the expectational idea and subjecting it to lengthy tests, I was impressed with how well it stood up. The buildup of price expectations can be expressed in many ways that all fit nearly as well—linear and nonlinear, lags of one year or two. Brinner and I settled on a linear transition from a coefficient of about one-half to one with a two-year lag, because it worked best by a narrow margin. Gordon also really comes to the conclusion that he must invoke the buildup of inflationary expectations, rather than the changing structure of the labor market, to account for the persistence of high rates of inflation after the 1969–70

recession. As he concedes, his basic model goes way off the track in the first half of 1971. In my own evaluation of equations, I would give great weight to their ability to account for the most recent observations, and only the expectational buildup idea will turn in a good performance on that criterion.

It is true that the recent period gives only a single observation. But supporting evidence can be drawn from the record of the fifties and, indeed, of prior inflations throughout the twentieth century. In the long-run analysis, the role of price expectations comes through in virtually any straightforward approach and produces price coefficients in the wage equation that are close to one. Furthermore, the buildup of price expectations during the late sixties gets some confirming evidence from Turnovsky's study of price expectations. Turnovsky concluded that price inflation was substantially underpredicted until the recent bout, which essentially has been fully expected. This provides one explanation of the rising price expectation coefficients cited in our JEC study, and by Gordon here.

The inflation-severity variable actually helps to clarify the earlier puzzle about the favorable behavior of wages in 1964 and 1965. With that index included, the role of the guideposts is unambiguously a better explanation for that period than variables such as disguised unemployment that suggested that labor markets were looser than the overall unemployment rate indicated. But I would expect other labor market variables, besides the global unemployment rate, to have some impact, and I plan to come back to them in future studies. In that sense I don't really quarrel with either Perry or Gordon in pointing to the structure of the labor force as a factor that can affect the inflation tradeoff.

General Discussion

George Perry agreed with Eckstein that the continuation of rapid wage increases as late as 1971 could not be explained by structural changes in the labor market; however, he was more inclined to admit his inability to explain the behavior of early 1971 than to invoke elaborate expectational models as an explanation. Extensive experimentation with the data is likely to produce some version that fits, but also results that are implausible. For example, in some of Gordon's wage equations, the dispersion index completely supplants the overall unemployment rate, with the totally unreasonable implication that the doubling of unemployment rates for teenagers and

women would result in a tighter rather than a looser labor market. Similarly, the heavier weight on disguised unemployment in some of Gordon's equations implies, incredibly, that a woman or teenager does more to hold down wages by staying home than by actively hunting a job—indeed, more than an adult male actively seeking a job.

Gordon responded that these other labor market variables were closely related to overall unemployment. Even if they drowned out the overall unemployment rate in the regression, its influence is still reflected. The doubling of unemployment rates for teenagers and women with a constant rate for adult males, in Perry's example, could not occur unless accompanied by less labor market pressure and higher rates of official, disguised, and hours unemployment, since otherwise the adult male unemployment rate would have to decline as males fill the jobs for which teenagers and women are no longer suitable. The model simply says that for *given* rates of disguised and hours unemployment (and implicitly, because of its high correlation, official unemployment) the labor market is tighter when dispersion is high than when it is low. Responding to Eckstein's interpretation of 1964–65, Gordon reported that disguised unemployment remained significant in all equations, whether the guidepost dummy was included or not, and whether or not a threshold or variable response to inflationary expectations was introduced. Gordon also returned to the methodological issue of mining time series data that contained only a few degrees of freedom. He felt that it was important to reach at least tentative conclusions on issues confronting public policy and economic analysis. The statistical exploration of history is one of the best available techniques, for all its limitations. The answers it provides are not conclusive and their tentative character must be explicitly recognized.

Charles Holt questioned the appropriateness of the particular index of unemployment dispersion used by Perry and Gordon. He felt that a proper index would be constructed to reflect the theoretical grounds for putting a dispersion variable into the equations. Dispersion is important because Phillips curves are nonlinear. Changes in the degree of tightness in sectors of low unemployment have a greater impact on wages than equal changes in the degree of tightness in sectors of higher unemployment. He was not convinced that the dispersion variable of the models in Gordon's paper captured the effect correctly and wished that the results of using alternative measures of dispersion had been studied.

Also along methodological lines, William Fellner noted that both Bosworth and Gordon had carefully focused on fixed-weight price indexes for

measuring inflation in order to eliminate biases associated with erratic changes in the composition of output. Yet the investigators were obliged to compare these indexes with cost and productivity indexes that were not based on these fixed-weight measures and thus were influenced by shifts in output. Fellner did not feel that any set of measures offered a perfect solution to this problem, but he noted that fixed-weight and conventional implicit deflators had behaved in ways that differed substantially during critical subperiods in the analysis.

A substantial part of the discussion was focused on the puzzle that Greenspan had raised about the seeming “immaculate conception” of the recent disinflation. Robert Solow reminded the group that prior to the controls the inflation seemed to be operating like a perpetual motion machine, driving itself without drawing on any outside fuel. Since its ability to maintain itself in the face of widespread excess supply seemed so puzzling, the fact that controls slowed it down with relatively little downward pressure should not be surprising. Arthur Okun noted that George Perry’s optimism about the controls *ex ante*, as expressed in his report in *Brookings Papers on Economic Activity* (2:1971), was consistent with Greenspan’s observations about the ease of accomplishing a price-wage slowdown. If wages were initially being driven by habitual emulative patterns—a wage-wage spiral—the establishment of a standard could slow them down without creating disequilibrium. Similarly, if inflationary expectations had played a large role prior to controls, the freeze and Phase II could have worked by changing those expectations. On either of these interpretations, the success of the controls was aided by the absence of excess demand pressure in the initial situation. The restraints were moving with the tides of market forces rather than against them.

Arnold Packer reported that his own statistical investigations gave some credence to the wage-wage spiral hypothesis. He found that “wage distortion”—differences in industry wage rates that did not fit traditional patterns of wage structure—had been especially large in the late sixties and may have triggered demands for especially rapid wage increases. Interestingly, wage distortion began to decline significantly prior to the freeze, and that may have eased the task of controls. Frank Schiff judged that, if the wage-wage hypothesis is correct, a full-scale controls program might be needed to arrest a spiral that had developed considerable momentum, but may not be required to operate on a permanent basis. At the same time, there can be justifications for an incomes policy during the forthcoming economic up-

swing that are not connected with the “hangover” effects of past inflationary spirals. John Kareken felt, however, that those offering wage emulation as an explanation had not presented convincing reasons why wages should have been chasing wages with more intensity in the late sixties than in earlier periods.

Charles Schultze pointed to two areas where the “immaculate conception” puzzle did not arise, since the controls had had quite visible effects. First, the deceleration of medical care costs was very substantial and not consistent with the experience of prior periods of economic slowdown. Second, comparison of consumer and wholesale price indexes suggested that the controls program had arrested the long-term upward creep in retail margins. Greenspan suspected, on the other hand, that the apparent turnaround of retail margins was a statistical illusion if the wholesale list prices reported in the statistics had outpaced actual realized prices at wholesale.

Walter Salant felt that some of the skepticism about the ability of an incomes policy to succeed in the United States rested on incorrect inferences from foreign experience. In Salant’s view, the negative verdict on incomes policies abroad that has become conventional wisdom is based largely on the fact that they have broken down in most countries at some point, but often not until pressures of excess demand mounted in the controlling country or in a country with which it has close trading relations. A more accurate appraisal of such programs requires examining their success during the whole period of their operation.

A number of participants mentioned inflationary factors on prices and wages that had not been stressed in either the Bosworth or the Gordon paper. Lawrence Klein felt that the Vietnam war may have played an important role in creating and maintaining inflation. He argued that serious underestimates of the severity of the guns-butter tradeoff were made in 1965. The American economy was on a semi-war footing and became relatively inefficient during the late sixties, thereby giving an extra push to inflation. Just as economists now recognize that the Korean war had a special influence on inflation in its time, future researchers, he predicted, would adopt a Vietnam explanation for the recent inflation. According to Lawrence Krause, higher import prices following the devaluation of the dollar may have done more to push the wholesale price index during the controls period than Bosworth’s analysis implied. Greenspan wondered whether some of the observed change in wage behavior during the late sixties might reflect the much expanded coverage of unemployment insurance. He noted

that state unemployment programs covered 77 percent of all unemployed men (25 years of age and over) in 1970, compared with only 60 percent in 1965. When unemployment is made less costly, it becomes less of a depressive influence on wages. Perry suggested, however, that at least some of that shift was part of a normal cyclical pattern; insured unemployment is traditionally a larger fraction of total unemployment in periods of recession and slack than in periods of full employment.

Bosworth concluded the discussion by clarifying and elaborating some of his findings. He agreed with Solow's interpretation that he was focusing particularly on the second quarter of 1972 as a good indicator of the probable future trend of prices and wages under controls. In Bosworth's judgment, there had been an approximately equal deceleration of wages and prices at that point. He did not believe that this improvement could be neatly allocated between the Pay Board and the Price Commission. As Schultze had noted, price controls had had a direct effect on medical care and retail margins. Moreover, the price program may have shortened the response lag of prices to a deceleration of wages and may have acted as a deterrent to price increases in some cases. But Bosworth reiterated his doubts that the Price Commission's program for Tier I firms could have made a very large contribution and particularly that it could have had a redistributive impact adverse to profits. If the share of profits behaved abnormally in 1971-72, that should not be attributed to the controls, Bosworth argued.

Gordon emphasized that his estimate of the impact of controls on profits was not based on any comparison with actual profits. Rather, it depended on the fact that the ratio of price to unit labor costs has risen far less during the current period of controls than would have been anticipated on the basis of its behavior during previous periods of cyclical recovery.