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AFTER THE PHILLIPS CURVE: PERSISTENCE OF HIGH INFLATION AND HIGH UNEMPLOYMENT

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PROCEEDINGS OF A CONFERENCE HELD IN JUNE 1978

FEDERAL RESERVE BANK OF BOSTON

AFTER THE PHILLIPS CURVE: PERSISTENCE OF HIGH INFLATION AND HIGH UNEMPLOYMENT

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Opening Remarks

Frank E. Morris

It is probably fair to say that economic policy is now being made in at least a partial vacuum of economic theory. Unlike earlier periods, no one body of theory seems to have a very broad acceptance. If Keynesianism is not bankrupt, as Messrs. Lucas and Sargent suggest, it is at least in disarray. Certainly, the confidence that I felt as a member of the Kennedy Treasury in our ability to use the Keynesian system to generate outcomes for the economy which were highly predictable has been shaken, and I believe a great many other people have also lost that confidence. I look back with nostalgia on those years in the early sixties when we used, with remarkable success, small econometric models to make fairly exact estimates of what we needed to produce a given result in the economy. Now we have much more elaborate econometric models that are coming up with estimates in which we have much less confidence.

Since the early sixties another school of theory, the Monetarist school, has flowered and most of us have learned a very great deal from it; however, at the same time, few of us are willing to accept the entire Monetarist body of theory. I have a feeling that the high water mark of Monetarism is already behind us. So with Keynesianism in disarray and Monetarism ebbing (if that is the case), we see a new school of theory evolving around the label of rational expectations. It is not clear just what this new school will generate that will be operational for policy-makers. Certainly, much of what it has generated as far as monetary policy is concerned is pretty much what we have already learned from the Monetarists – that the market is not a blank sheet of paper on which we can write with some confidence whatever we want. I think we have all learned that market feedback is something we must consider in formulating policy and we have seen such remarkable feedbacks recently in a sharp rise in the stock market after short-term interest rates rose by 3/4 of 1 percent.

My only problem with the rational expectations school and the Lucas-Sargent paper is that they promise us a complete system ready for policy-makers in ten years. Obviously, ten years is a rather long time to wait; particularly for me, since ten years from now I will be on the verge of retirement. I am afraid that it will be a long time before we again have the complete confidence which we had in the early 1960s – that we knew exactly what we were doing. I await the return of such confidence. I think we are all looking for a new synthesis in economic theory. Historically we all know that such a new synthesis does not

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arise out of the brain of one man in a moment of great inspiration. We know from the history of Keynes' general theory that it reflected the work of a great many people during the decade preceding his writing. Many people put together building blocks and pieces that contributed to the formation of the new synthesis. We are not expecting this meeting will generate the new synthesis that we are all seeking but perhaps it will generate one building block or two upon which a new synthesis will be based. Or perhaps a building block is already in place and will be revealed to us so that we can spread the gospel. That is the background upon which we can begin this investigation.

I. Documenting the Problem

Diagnosing the Problem of Inflation and Unemployment in the Western World

Geoffrey H. Moore

The economic recovery following the recession of 1974-75 has left virtually every industrial country with higher unemployment rates than could be considered normal, as well as with higher inflation rates than could be considered desirable. In some countries, such as France, Italy, the United Kingdom and Canada, the unemployment rate in 1977 was as high as or higher than in 1973, the last year of general prosperity; but the inflation rate was higher also. In the United States, the unemployment rate also was higher in 1977 than in 1973, and the rate of inflation was only slightly lower. Only in West Germany and Japan, where unemployment was substantially higher in 1977 than in 1973, was the inflation rate substantially below what it had been in 1973, although even those countries with inflation at 4 to 5 percent had not achieved what they regarded as a satisfactory position with respect to inflation. Table 1 presents the unemployment and inflation rates for each of these countries for 1973, 1975, and 1977.

Although it is easy to point to this anomaly, it is not easy to explain it, to say nothing of curing it. It is useful, however, to recall that it is not entirely new. Indeed, some 27 years ago Arthur Burns gave expression to the phenomenon in a single phrase that summed up a wealth of experience: "Inflation does not wait for full employment."[1] He was describing the lessons distilled from Wesley Mitchell's studies of business cycles, prior to World War II, and warning that economists might have to relearn this particular one. The advice was warranted then, and it is still relevant. Inflation has not waited for full employment, and those who thought there was no need to worry about inflation as long as there was considerable unemployment have had to learn the lesson the hard way.

What I propose to do in this paper is to show how the situation can be described in a way that is more understandable, if not more palatable. Better understanding may lead to a more rational choice of policies that will effect a cure. What I shall do is examine the behavior of inflation during periods of slow

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[1] Introduction to Wesley C. Mitchell's *What Happens During Business Cycles,* National Bureau of Economic Research, 1951, p. xxi.

TABLE 1

Unemployment Rates and Inflation Rates
in Seven Countries, 1973–77

	Unemployment Rate (%)			Inflation Rate, CPI (%)		
	1973	1975	1977	1973	1975	1977
United States	5	8	7	9	7	7
Canada	6	7	8	9	10	10
United Kingdom	3	5	7	10	25	12
West Germany	1	4	4	8	5	4
France	3	4	5	8	10	9
Italy	3	3	3	13	11	14
Japan	1	2	2	17	8	5
Average, 6 countries excluding United States	3	4	5	11	12	9

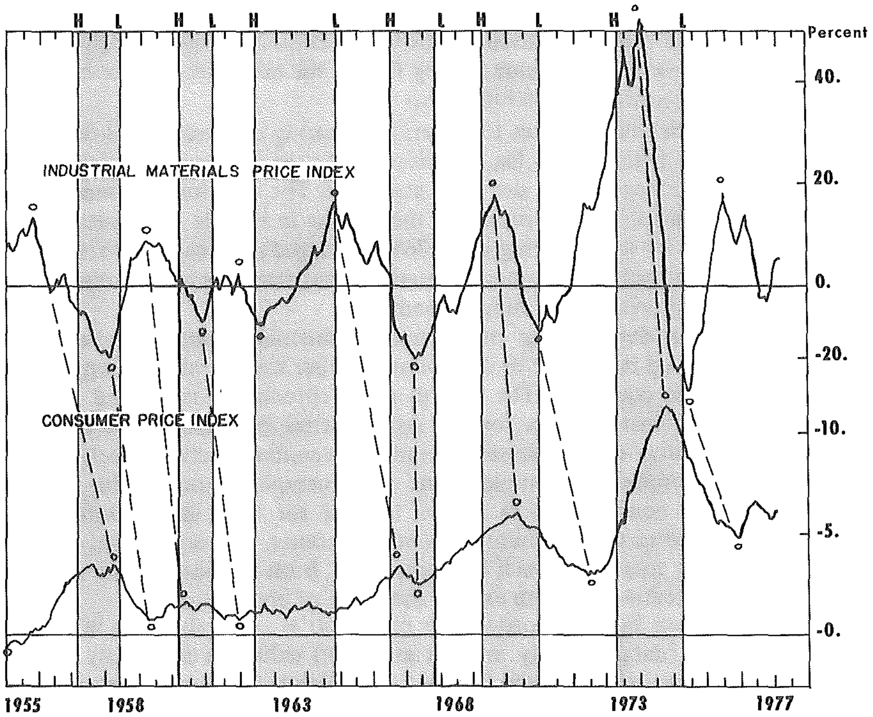
Source: Unemployment rates are from the U.S. Bureau of Labor Statistics and are adjusted to U.S. labor force concepts. See Joyanna Moy and Constance Sorrentino, "An Analysis of Unemployment in Nine Industrial Countries," *Monthly Labor Review,* April 1977, Table 2, p. 15, and release dated April 1978. Inflation rates are percent changes in the consumer price index from December of preceding year to December of current year, based on indexes published in *Business Conditions Digest,* U.S. Department of Commerce.

growth or recession on the one hand, and during periods of rapid growth on the other. What we shall find is that, both in the United States and abroad, reductions in the rate of inflation have always been associated with periods of slow growth, and have not occurred at other times. We shall also find that it is important to consider the lags in this relationship, which in the United States at least have been increasing. These lags account in part for the anomaly of high inflation and high unemployment.

In order to distinguish periods of slow growth from periods of rapid growth we shall use the concept of a growth cycle. A growth cycle is, in effect, a business cycle after adjustment for long-run trend. That is, a growth cycle distinguishes periods of rapid growth from periods of slow growth by reference to a long-run trend. Trend-adjusted data rise as long as the short-run rate of growth exceeds the long-run rate. They decline as long as the short-run rate is less than the long-run rate. The peaks and troughs in trend-adjusted data, therefore, delineate periods of rapid and slow growth.

For the United States, a chronology of growth cycles based on trend-adjusted data in various measures of the physical volume of aggregate economic activity has been developed by the National Bureau of Economic Research, in work initiated by Ilse Mintz. The latest version of this chronology is used in Chart 1 as a backdrop against which to examine the movements in the rate of change in two price indexes. The index of industrial materials prices – which includes commodities such as scrap steel, print cloth and rubber – shows

Chart 1
RATES OF CHANGE IN TWO PRICE INDEXES DURING GROWTH CYCLES, UNITED STATES, 1955-77
(measured over 12-month span, smoothed)



Shaded areas represent slowdowns in economic growth, as determined from trend-adjusted measures of aggregate output, income, sales and employment.

an especially close relationship to the growth cycle. Downswings in the rate of change in these prices are associated with every period of slow growth or recession (the shaded areas on the chart), upswings with every period of rapid growth (the white areas). Indeed, the downswings often have begun before the onset of the slow growth periods, e.g., in 1956 and 1959. This price index is one of the leading indicators, and its rate of change leads not only the growth cycle but also the rate of change in the consumer price index, the bottom line in the chart.[2] The latter, which of course includes the prices of services as well as commodities, and at retail rather than wholesale, responds to the growth cycle as well, but often with a lag of a year or more. The lags are so long, especially in recent years, that sometimes the rate of inflation (in the CPI) has risen almost throughout the period of slow growth or recession, giving rise to the erroneous impression that slow growth had no influence on inflation.

Watching both price indexes together, and bearing in mind their differences in sensitivity and tendency to lag, enables one to see that growth cycles have very pervasive influences upon the price structure. The reaction one sees in the consumer price index (as, for example, the decline in its rate of increase from the autumn of 1974 to the spring of 1976) is a lagged response to or reflection of similar developments in commodity markets that react far more promptly to changes in demand pressures or supply conditions.

Corresponding data for six other major industrial countries, taken as a group, are employed in Chart 2 to determine whether similar relationships are to be found in these countries. The growth cycle chronology is derived from a composite trend-adjusted index for the six countries combined. This index is based upon measures of the physical volume of economic activity such as real GNP, industrial production, employment and unemployment, so the growth cycle chronology conceptually is similar to that for the United States. The cyclical experience of the six countries is not, of course, entirely similar, and we plan in later work to analyze each one separately, both to check on the validity of our summary treatment and to extend the range of observation.

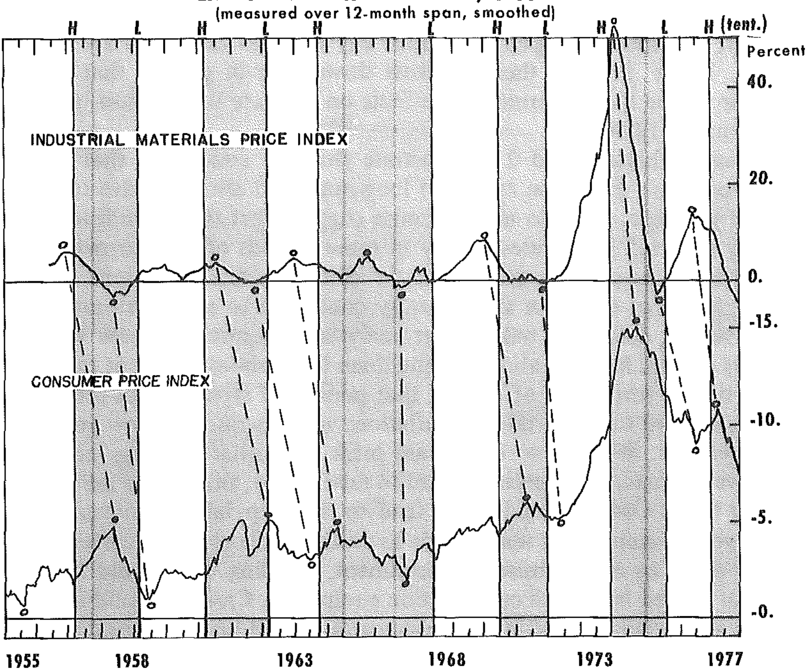
Rates of change in a composite index of industrial materials prices in five of the six countries (data for Italy are not available) exhibit a sensitivity to the growth cycle similar to that in the United States. Every slowdown in growth has been accompanied by a reduction in the rate of increase in these prices, and often by an absolute decline (i.e., where the line of the chart goes below the zero level). Every period of rapid growth delineated by the trend-adjusted coincident

[2] Materials price indexes have qualified as leading indicators in four successive NBER studies of this subject – in 1938, 1950, 1960 and 1966. These analyses were made in terms of the index itself, not its rate of change, and pertained to its behavior during business cycles, not growth cycles. In the past ten years or so the index has shown a tendency to lag at business cycle peaks and troughs (see text below), and this was one factor prompting the decision, in the BEA's study in 1975, to use the rate of change in the index, rather than the index itself, as the indicator. At the same time the BEA substituted a more comprehensive index of crude materials prices (excluding foods, feeds and fibers) for the index of more limited coverage that was previously used. In Chart 1 and elsewhere in this paper we use the rate of change in the more restricted index. Both indexes move in rather similar fashion, and the choice as to which is the superior indicator is marginal.

Chart 2

RATES OF CHANGE IN TWO PRICE INDEXES DURING GROWTH CYCLES, SIX COUNTRIES EXCLUDING UNITED STATES, 1955-77

(measured over 12-month span, smoothed)



Shaded areas represent slowdowns in economic growth, as determined from the trend-adjusted coincident index for six countries. The six countries are Canada, United Kingdom, West Germany, France, Italy, and Japan. The indexes are weighted by each country's GNP in 1970, in U. S. dollars. The industrial materials index excludes Italy (data not available).

index has been accompanied by an acceleration in materials prices. The consumer price index for the six countries exhibits a delayed response, akin to that in the United States. Taking the delay factor into account, it is possible to trace a relationship both to the materials prices and to the growth cycle (see the dashed lines on the chart, connecting high and low points in the rates of change in the two price indexes).

By comparing Charts 1 and 2 one can observe the close interconnection between the prices of crude materials in the United States and in the six other industrial countries. Most of these materials are traded on world markets, and changes in demand or supply conditions anywhere in the world are registered promptly. Partly through these markets slowdowns in growth that are international in scope have international effects on the rate of inflation, notably in 1957-58 and in 1974-75.

Although Charts 1 and 2 demonstrate that the conditions that make for rates of economic growth in excess of long-run trend are conducive to an acceleration of inflation, they do not of course suggest what those conditions are, or show why inflation accelerates greatly in some periods of rapid growth while in other periods it accelerates only modestly. Similarly, the conditions that make for slow growth or recession are evidently conducive to a reduced rate of inflation or even to deflation, but further analysis is required to show what those conditions are and how variations among them bring about different results.

It is hardly surprising, of course, that periods of rapid growth produce conditions conducive to rising rates of inflation, while periods of slow growth have opposite effects. When new orders are brisk and order backlogs accumulate, sellers have opportunities and incentives to raise prices, and buyers are less averse to paying them. Costs of production tend to creep up, labor turnover increases, control over efficiency and waste tends to decline. New commitments for investment are made in an optimistic environment, building up demand for limited supplies of skilled labor and construction equipment. Credit to build inventories is more readily available and in greater demand, even if higher interest rates must be paid for it, raising costs. Labor unions see better opportunities to get favorable contract settlements, and their members are more willing to strike to get them. All these conditions apply to more and more firms and industries, and produce upward pressure on more and more prices. Indeed, it is not always recognized that a rising rate of inflation in the general price level reflects the fact that more prices go up at more frequent intervals, not just that they rise in bigger jumps.

During periods of slow growth or actual decline in aggregate economic activity the opposite conditions prevail. More firms and industries cutback their output, reduce or eliminate overtime, tighten up to shave costs of production, give bigger discounts off list prices, reduce inventories and repay bank debt, postpone new investment projects and stretch out existing ones. Quit rates decline, reducing the cost of labor turnover, and labor demands for pay raises become more conservative. Interest rates drop. As price increases become less widespread and less frequent, and as more price cutting takes place, the rate of inflation subsides.

Many of the processes sketched above are represented among the leading and lagging indicators. In an earlier study I showed that the leading indicators could be viewed as sensitive measures of demand pressures, and that in the United States their movements during growth cycles were rather effective not only in accounting for the varying leads and lags in the rate of inflation from one growth cycle to another, but also in accounting for the varying amount of change in the rate of inflation in different growth cycles.[3] This analysis can now be brought up to date for the United States and extended to the other six countries as well.

The record of leads and lags (Tables 2 and 3) shows that, both in the United States and in the other six countries taken as a group, the turns in the trend-adjusted leading index and in the rate of change in industrial materials prices lead the growth cycle turns (coincident index) by about four to six months on the average. Furthermore, although the length of these leads varies considerably from one cycle to another, long or short leads in the leading index correspond with long or short leads in the rate of change in materials prices (see the correlation coefficients in the note to the tables). That is, the turning points in the two series are associated with one another. The tables also show that the rates of change in the consumer price index lag behind the growth cycle turns by nine or ten months, on the average, and hence follow the turns in the leading index and in materials prices by a year or more. Again, the variation in the length of lag behind growth-cycle turns is partly accounted for by similar variations in the timing of the leading index or, alternatively, the industrial materials price index. This suggests that, despite the long lag, the turns in the rate of change in the consumer price index are associated with those in the leading index and in industrial materials prices.

It is of some interest to determine whether there has been a long-run shift in the length of the lags in prices vis-à-vis the growth cycle. A test of the U.S. data suggests that the lags in the rate of change in the consumer price index have been getting longer, both with respect to the growth cycle and with respect to the leading index and the materials price index. The leads in the latter two indexes may also have been getting shorter, but this is more conjectural. Regressions in which the dependent variable is the length of lead or lag in months, and the independent variable is the year in which the turn occurred (e.g., 48, 49, etc.) are as follows:

Correlation between Leads and Lags and Time

	No. of Observations	Regression Coefficients and t-Statistics			Regression Estimate* for	
		a	*b*	*r*	1948	1978
Leading index % of trend	18	−16.8 (− 1.4)	+.18 (.89)	+.22	−8	−3
Materials price, rate of change	14	−34.9 (− 1.6)	+.49 (1.43)	+.38	−11	+3
Consumer price, rate of change	15	−25.8 (−1.8)	+.56 (2.52)	+.57	+1	+18

*Lead (−) or lag (+) in months.

[3] "Price Behavior during Growth Recessions," *Perspectives on Inflation*, Canadian Studies 36, The Conference Board in Canada, Symposium held January 1974.

The coefficient for time (column b) is positive in all three cases, although it is statistically significant only in the case of the consumer price index. During the 30-year period 1948-78 the regression suggests a substantial shift, with the estimated lag for the CPI increasing by nearly a year and a half. The regressions for the leading index and the materials price index suggest a shift in the same direction, but smaller. In short, the rate of inflation (CPI) lags behind the growth cycle more than it used to, and to a lesser extent, also lags farther behind the wholesale prices of materials and the sensitive leading indicators.[4] One possible reason is the increasing relative importance of services in the CPI and their more sluggish price behavior.[5] Another is the similar tendency exhibited by unit labor costs.[6]

If the leading index is a measure of demand pressure, one would expect that large increases in it would be associated with large increases in the rate of inflation. Tables 4 and 5 show that this is indeed the case. The size of the upswings and downswings in the leading index are positively correlated with those in the rate of change in materials prices and in consumer prices. The swings in materials prices and consumer prices are correlated also. This is true both in the U.S. data and in the figures for the six other countries.

One of the concomitants of slow growth in output is slow growth in employment. In deriving the growth-cycle chronologies used above, several measures of employment, after adjustment for long-run trend, have been used, along with series on output, income and trade. Table 6 gives a conspectus of the change in the unemployment rate and in the employment ratio between the growth-cycle peak and trough dates. Both these measures are, to a degree, adjusted for trend. The unemployment rate (U/L) is the number of unemployed adjusted for the growth in the civilian labor force. The employment ratio (E/P) is the number employed adjusted for the growth in the working-age population. However, these trend adjustments are only approximate. The unemployment rate has exhibited an upward trend in the last decade or so, and so has the employment ratio. In Table 6 we use them without further adjustment.

The table shows that the unemployment rate has risen about 2 percentage points, on the average, during growth-cycle contractions, while the employment ratio has fallen about 1 percentage point. In three of the contractions (1951-52,

[4] The data in Table 3 for the six other countries do not show a similar trend. The regression coefficients on time are positive for the six-country leading index and for the materials price index but negative for the consumer price index; none of the coefficients, however, is statistically significant.

[5] Phillip Cagan, however, found a trend towards more sluggish response in the wholesale prices of commodities alone, although he concentrated attention upon the amplitude of price change rather than the length of lag. See his "Changes in the Recession Behavior of Wholesale Prices in the 1920's and post-World War II," *Explorations in Economic Research*, Vol. 2, No. 1, Winter 1975, pp. 54-104.

[6] See my "Lessons of the 1973-1976 Recession and Recovery," in *Contemporary Economic Problems*, edited by William Fellner, American Enterprise Institute, 1978.

TABLE 2

Leads and Lags during Growth Cycles: Leading Index and Two Price Indexes, United States

Growth Cycle[a]		Date of Turn and Lead (–) or Lag (+) in Months: Leading Index, Deviation from Trend[b]		Rate of Change in Industrial Materials Price Index[c]		Rate of Change in Consumer Price Index[c]	
Peak	Trough	Peak	Trough	Peak	Trough	Peak	Trough
July 48		Jan. 48 (–6)					
	Oct. 49		June 49 (–4)				Jan. 50(+3)
Mar. 51		Aug. 50 (–7)		Jan. 51 (–2)		Feb. 51 (–1)	
	July 52		Nov. 51 (–8)		June 52 (–1)		May 53 (+10)
Mar. 53		Mar. 53 (0)		–		Oct. 53 (+7)	
	Aug. 54		Jan. 54 (–7)		–		Jan. 55 (+5)
Feb. 57		Sept. 55 (–17)		Dec. 55 (–14)		Mar. 58 (+13)	
	Apr. 58		Jan. 58 (–3)		Apr. 58 (0)		May 59 (+13)
Feb. 60		Apr. 59 (–10)		May 59 (–9)		May 60 (+3)	
	Feb. 61		Dec. 60 (–2)		Dec. 60 (–2)		Jan. 62 (+11)
May 62		Feb. 62 (–3)		Jan. 62 (–4)		–	
	Oct. 64		June 62 (–28)		Sept. 62 (–25)		–
June 66		Mar. 66 (–3)		Nov. 64 (–19)		Oct. 66 (+4)	
	Oct. 67		Jan. 67 (–9)		Apr. 67 (–6)		May 67 (–5)
Mar. 69		Jan. 69 (–2)		Sept. 69 (+6)		May 70 (+14)	
	Nov. 70		Nov. 70 (0)		Jan. 71 (+2)		Aug. 72 (+21)
Mar. 73		Feb. 73 (–1)		Feb. 24 (+11)		Nov. 74 (+20)	
	Mar. 75		Feb. 75 (–1)		June 75 (+3)		Dec. 76 (+21)
Average Lead or Lag at Growth Cycle							
Peaks		–5		–4		+9	
Troughs			–7		–4		+10
All turns		–6		–4		+9	

[a]Based on the concensus of turning points in trend-adjusted data for 19 measures of aggregate output, income, sales and employment. See Victor Zarnowitz and Geoffrey H. Moore, "The Recession and Recovery of 1973-1976," *Explorations in Economics Research,* Fall 1977, NBER, p. 508.

[b]Commerce Department's index (BCD series 910), trend-adjusted by NBER.

[c]Change over 12 months, smoothed (not centered). Centering the rates would increase the leads by six months and reduce the lags by six months.

Note: The correlation coefficients (r) between the leads of the three series are:

	At Peaks	At Troughs	At All Turns
Leading index and industrial materials price index	+.54	+.98	+.76
Leading index and CPI	+.14 (+.64)	+.82	+.42 (+.72)
Industrial materials price and CPI	+.56 (+.74)	+.98	+.56 (+.67)

[Coefficients excluding the 1957 peak are shown in parentheses].

TABLE 3

Leads and Lags during Growth Cycles: Leading Index and Two Price Indexes, Six Countries excluding United States

Growth Cycle[a]		Leading Index, Deviation from Trend		Rate of Change in Industrial Materials Price Index[c]		Rate of Change in Consumer Price Index[c]	
Peak	Trough	Peak	Trough	Peak	Trough	Peak	Trough
Feb. 57		Feb. 57 (0)		Dec. 56 (–2)		June 58 (+16)	
	Jan. 59		June 58 (–7)		June 58 (–7)		July 59 (+6)
Mar. 61		May 61 (+2)		Aug. 61 (+5)		Apr. 63 (+25)	
	Feb. 63		Oct. 62 (–4)		Oct. 62 (–4)		Aug. 64 (+18)
Sept. 64		Feb. 64 (–7)		Jan. 64 (–8)		June 65 (+9)	
	May 68		June 67 (–11)		July 67 (–10)		Aug. 67 (–9)
June 70		Nov. 69 (–7)		Dec. 69 (–6)		Sept. 71 (+15)	
	Dec. 71		Feb. 72 (+2)		Dec. 71 (0)		June 72 (+6)
Nov. 73		Feb. 74 (+3)		Mar. 74 (+4)		Oct. 74 (+11)	
	Aug. 75		July 75 (–1)		June 75 (–2)		Aug. 76 (+12)
Jan. 77[b]		July 76 (–6)		July 76 (–6)		May 77 (+4)	
Average Lead or Lag at Growth Cycle							
Peaks		–2		–2		+13	
Troughs			–4		–5		+7
All turns		–3		–3		+10	

[a]Based on six-country coincident index, deviations from trend.

[b]Tentative

[c]Change over 12 months, smoothed (not centered). Centering the rates would increase the leads by six months and reduce the lags by six months.

Note: The correlation coefficients (r) between the leads of the three series are:

	At Peaks	At Troughs	At All Turns
Leading index and materials price index	+.96	+.997	+.95
Leading index and CPI	+.54	+.61	+.59
Materials price index and CPI	+.63	+.65	+.64

TABLE 4

Amplitude of Change in Leading Index and in the Rate of Inflation during Growth Cycles, United States, 1951–75

Date of Growth Cycle		Change in Leading Index, Trend-adj.[a]		Change in Rate of Change (% points): Indus. Materials Price Index		Change in Rate of Change (% points): Consumer Price Index	
High	Low	Low to High	High to Low	Low to High	High to Low	Low to High	High to Low
	Oct. 49		–14				
Mar. 51		18		108.3		11.1	
	July 52		–11		–106.9		–8.2
Mar. 53		7		12.5[b]		0.6	
	Aug. 54		–14		15.6[b]		–1.8
Feb. 57		18		12.2[b]		4.1	
	Apr. 58		–17		–27.7		–2.8
Feb. 60		15		23.0		0.9	
	Feb. 61		–13		–15.9		–0.8
May 62		7		9.3		0.4[b]	
	Oct. 64		–4		–9.9		–0.1[b]
June 66		8		24.2		2.3[b]	
	Oct. 67		–9		–30.8		–1.0
Mar. 69		10		32.7		3.6	
	Nov. 70		–13		–27.4		–3.0
Mar. 73		15		62.0		8.3	
	Mar. 75		–27		–73.8		–6.5

Coefficient of correlation (r)	Rises	Falls	Rises and Falls [c]
Leading index and industrial materials price index	+.56	+.31	+.40
Leading index and CPI	+.69	+.52	+.55
Industrial materials price index and CPI	+.93	+.90	+.89

Note: For the dates of highs and lows used to measure changes in the leading index and in the rate of change in prices, see Table 2.

[a] In index points, i.e., in percent of trend.

[b] Change to growth cycle high or low, since there is no corresponding turn in the price series (see Table 2).

[c] The correlation is computed without regard to the sign of the rise or fall.

TABLE 5

Amplitude of Change in Leading Index and in the Rate of Inflation during Growth Cycles, Six Countries excluding United States, 1957–77

Date of Growth Cycle		Change in Leading Index, Trend-adj.[a]		Change in Rate of Change (% points) Indus. Materials Price Index		Consumer Price Index	
High	Low	Low to High	High to Low	Low to High	High to Low	Low to High	High to Low
Feb. 57							
	Jan. 59		–6.9		–8.8		–3.9
Mar. 61		8.2		6.5		4.2	
	Feb. 63		–5.4		–4.3		–2.2
Sept. 64		4.8		5.0		1.7	
	May 68		–5.0		–6.1		–2.6
June 70		6.4		10.2		4.0	
	Dec. 71		–9.6		–9.1		–0.9
Nov. 73		11.8		53.5		9.9	
	Aug. 75		–11.6		–55.4		–6.0
Jan. 77		6.9		16.4		1.8	

Coefficients of Correlation (r)	Rises	Falls	Rises and Falls [b]
Leading index and industrial materials price index	+.89	+.81	+.85
Leading index and CPI	+.94	+.45	+.69
Industrial materials price index and CPI	+.89	+.83	+.82

Note: For the dates of highs and lows used to measure changes in the leading index and in the rate of change in prices see Table 3.

[a]In index points, i.e., in percent of trend.

[b]The correlation is computed without regard to sign of the rise or fall.

TABLE 6

Unemployment Rate and Employment Ratio during Growth Cycles, United States

Growth Cycle Peak			Growth Cycle Trough			Change during Growth Cycle Contractions		Change during Growth Cycle Expansions	
Date	Unemp. Rate	Empl. Ratio	Date	Unemp. Rate	Empl. Ratio	Unempl. Rate	Empl. Ratio	Unempl. Rate	Empl. Ratio
July 48	3.6	56.4							
			Oct. 49	7.9	54.1	4.3	−2.3		
Mar. 51	3.4	56.3						−4.5	2.2
			July 52	3.2	55.2	−0.2	−1.1		
Mar. 53	2.6	56.2						−0.6	1.0
			Aug. 54	6.0	53.6	3.4	−2.6		
Feb. 57	3.9	56.1						−2.1	2.5
			Apr. 58	7.4	54.0	3.5	−2.1		
Feb. 60	4.8	55.0						−2.6	1.0
			Feb. 61	6.9	54.3	2.1	−0.7		
May 62	5.5	54.3						−1.4	0.0
			Oct. 64	5.1	54.4	−0.4	0.1		
June 66	3.8	55.5						−1.3	1.1
			Oct. 67	4.0	56.0	0.2	0.5		
Mar. 69	3.4	56.4						−0.6	0.4
			Nov. 70	5.9	55.7	2.5	−0.7		
Mar. 73	4.9	56.9						−1.0	1.2
			Mar. 75	8.5	55.2	3.6	−1.7		
Mean	4.0	55.9		6.1	54.7	2.1	−1.2	−1.8	1.2
St. Dev.	0.9	0.8		1.8	0.8	1.8	1.1	1.3	0.8

1962–64, 1966–67) the increase in unemployment and decline in employment was small. These were periods of slow growth but not recession. In the other six growth-cycle contractions the rise in unemployment and decline in employment were much more substantial. These periods encompassed recessions. During the intervening periods of rapid growth the decline in the unemployment rate and rise in the employment ratio has been about the same as the opposite changes during contractions, about 2 and 1 percentage points, respectively, reflecting the roughly horizontal trend in these series. The current recovery, incidentally, has been exceptionally vigorous, with a decline of 2.4 percent in the unemployment rate from March 1975 to the latest figure, May 1978, and a rise of 3.4 percent in the employment ratio. The latter is by far the largest increase for any expansion since 1948. With 58.6 percent of the working-age population employed in May, this measure of labor utilization has set a new high record.

One further observation should be made on the basis of Table 6. The declines in the percentage employed during growth-cycle contractions have been getting smaller relative to the increases in the unemployment rate. During the first three contractions the decline in the percentage employed was four-fifths of the rise in the unemployment rate, on the average. During the next three contractions the decline in the percentage employed was only about half the rise in the unemployment rate. During the last three contractions the decline in the percentage employed was less than a third as large as the rise in the unemployment rate.[7] The rise in unemployment during recessions has become less and less a consequence of a decline in employment. Or, to put it differently, the unemployment problem in recessions has become less and less a consequence of a decline in demand, more and more a consequence of an increase in supply.

Table 6 tells us what happens to employment and unemployment during the periods marked off by the growth cycle chronology. It does not say anything about systematic leads or lags. Table 7 provides this information. It shows that on the average during 1948–75 the unemployment rate and the employment ratio were virtually coincident with the turns in the growth cycle. This is not unexpected, of course, but it is in marked contrast both with the leads in the leading index and in the rate of change in materials prices, and with the lags in the consumer price index.

In two respects, however, the leads and lags of employment and unemployment exhibit a relationship to those in the leading index and in the price data. First, they are positively correlated, as the following list shows:

[7]These comparisons suffer from the fact that the percentages are not computed on the same base. Nevertheless, the conclusion is similar if the unemployment rate is computed on the base of the working-age population instead of the labor force. In the first three growth contractions the decline in the percentage employed was larger than the rise in the percentage unemployed. In the next three the decline in the percentage employed was about the same as the rise in the percentage unemployed. In the last three the decline in the percentage employed was less than half the rise in the percentage unemployed.

TABLE 7

Leads and Lags of Unemployment Rate and Employment Ratio during Growth Cycles, United States

Growth Cycle		Unemployment Rate: Trough			Unemployment Rate: Peak			Employment Ratio: Peak			Employment Ratio: Trough		
Peak	Trough	Date	%	Mos. Lead or Lag	Date	%	Mos. Lead or Lag	Date	%	Mos. Lead or Lag	Date	%	Mos. Lead or Lag
July 48		Dec. 47	3.1	−7				July 48	56.4	0			
	Oct. 49				Oct. 49	7.9	0				Oct. 49	54.1	0
Mar. 51		May. 51[a]	3.0	+2				Aug. 50	56.1	−7			
	July 52				Nov. 51[a]	3.5	−8				Aug. 52	55.0	+1
Mar. 53		June 53	2.5	+3				Feb. 53	56.3	−1			
	Aug. 54				Sept. 54	6.1	+1				July 54	53.4	−1
Feb. 57		Mar. 57	3.7	+1				Jan. 56	56.3	−13			
	Apr. 58				July 58	7.5	+3				July 58	53.9	+3
Feb. 60		June 59	5.0	−8				June 60	55.3	+4			
	Feb. 61				May 61	7.1	+3				Sept. 61	53.9	+7
May 62		Oct. 62[a]	5.4	+5				Sept. 62[a]	54.5	−8			
	Oct. 64				May 63[a]	5.9	−17				Feb. 63[a]	53.9	−20
June 66		Nov. 66[a]	3.6	+5				Nov. 66[a]	56.0	+5			
	Oct. 67				Oct. 67[a]	4.0	0				Mar. 67[a]	55.4	−7
Mar. 69		May 69	3.4	+2				Jan. 70	56.7	+9			
	Nov. 70				Aug. 71	6.1	+9				June 71	55.2	+7
Mar. 73		Oct. 73	4.7	+7				Mar. 74	57.4	+12			
	Mar. 75				May 75	9.0	+2				Apr. 75	55.1	+1
Mean			3.8	+1.1		6.3	−0.9		56.1	+0.1		54.4	−1.0
Standard Deviation			9.9	5.2		1.8	8.0		0.8	8.3		0.7	8.3

[a]These turns pertain to minor movements associated with growth-cycle contractions but not comparable in size with the other cyclical movements in the series.

Correlation between Leads and Lags at Growth Cycle Turns

Dependent Variable	Independent Variable	No. of Observations	r	Regression Coef. and t-Statistics a	b
Unemployment rate	Leading index	18	+.78	4.6 (3.5)	0.7 (5.0)
Employment ratio	Leading index	18	+.80	5.2 (3.3)	0.9 (5.3)
Consumer price, rate of change	Unemployment rate	15	+.43	8.2 (4.0)	0.8 (1.7)
Consumer price, rate of change	Employment ratio	15	+.50	8.5 (4.5)	0.6 (2.1)

The constant terms (a) tell us that the unemployment rate and employment ratio reach their turns some four or five months after the leading index, as a rule, and some seven or eight months *before* the rate of inflation (CPI).[8]

The second point is that there is some tendency for the unemployment rate and the employment ratio to lag at recent growth-cycle turns. In this respect the trend resembles that shown by the leading index and the rates of price change. Regressions similar to those given earlier are:

Correlation between Leads and Lags and Time

	Observations	Regression Coefficients and t- Statistics a	b	r	Regression Estimate* for 1948	1978
Unemployment rate	18	−16.6 (−1.6)	.28 (1.62)	+.37	−3.2	+5.2
Employment ratio	18	−18.9 (−1.4)	.30 (1.38)	+.33	−4.5	+4.5

*Lead (−) or lag (+) in months.

The correlation is not statistically significant, and the estimated shift during 1948–78 is not as large as in the case of the rate of change in consumer prices. Nevertheless, it is conceivable that this shift in behavior of these measures of labor market tightness account in part for the changing behavior of the inflation rate. What it is, in turn, that accounts for the shift in timing of the labor utilization measures, if it is a real shift, is another matter. Among the possibilities is the shift in composition of employment towards the service industries, a shift that is more marked in terms of employment than it is in terms of output.[9]

[8] The lags in the rate of inflation depend in part on the interval over which the rate is measured and how the figures are dated. Here we use 12-month change, smoothed, dated in the terminal month. If this rate were centered it would be dated six months earlier, but it could not be observed at that time since the rate would depend upon changes in the index that have not yet occurred. Rates of change over shorter intervals would have shorter lags, but more erratic fluctuations.

[9] See Victor Zarnowitz and Geoffrey H. Moore, "The Recession and Recovery of 1973–1976," *Explorations in Economic Research*, Fall 1977, pp. 493–494.

We have not yet completed a strictly comparable analysis of employment ratios and unemployment rates for countries other than the United States, but earlier results suggest that slowdowns in growth cycles abroad have been accompanied by roughly coincident movements in employment and unemployment (Table 8). Short lags predominate over leads, however, most notably in Japan.

I conclude that not only in the United States but also in other industrial countries declines in the rate of inflation have almost invariably been associated with slowdowns in real economic growth and a diminution in labor utilization rates, *and have not occurred at other times.* This result, it seems to me, is of great importance. For short periods, of the kind encompassed by the growth-cycle concept, it may not be possible – in the sense that it has almost never been done – to achieve rapid growth, an increase in labor utilization rates and reduction in the inflation rate. This does not mean, however, that a reduction in the inflation rate cannot be (i.e., has not been) achieved when labor utilization rates are "high", or that they must be reduced to a "low" level in order to achieve a reduction in the inflation rate. The level of these utilization rates is of less consequence than the direction in which they are moving. When a slowdown starts, labor utilization rates are typically high, and they may remain relatively high throughout the slowdown (as in 1951-52 and 1966-67), but a reduction in the inflation rate takes place nonetheless. But one must always bear in mind, and allow for, the lag.

TABLE 8

Leads and Lags of Employment and Unemployment during Growth Cycles, Four Countries

	Mean Lead (–) or Lag (+) at Growth Cycle		Standard Deviation of Leads and Lags at Growth Cycle	
	Peaks (months)	Troughs	Peaks (months)	Troughs
Canada, 1954–70				
Nonfarm employment, no.	+3.2	+0.8	5.1	2.3
Unemployment rate, %	+2.4	–0.2	8.2	3.4
United Kingdom, 1951–72				
Employees in employment, no.	+1.4	+2.2	3.0	6.7
Wholly unemployed, no.	+2.8	0.0	6.3	0.7
West Germany, 1952–73				
Employment, mfg. & mining, no.	+1.5	+3.5	2.1	4.0
Unemployment rate, %	–2.3	+0.4	4.5	3.2
Japan, 1955–72				
Regular workers employment, no.	+3.2	+5.2	3.2	6.6
Unemployment rate, %	+3.5	+5.0	3.5	2.5

Source: Geoffrey H. Moore and Philip A. Klein, "Monitoring Business Cycles at Home and Abroad," NBER, manuscript.

Interpreted in this manner, with the aid of both the sensitive and the slower-moving indicators bearing upon prices, costs of production and demand, I believe that the growth-cycle concept and the system of international economic indicators being developed at the NBER, OECD, and cooperating agencies in many countries will prove to be an illuminating instrument to use in observing and appraising trends in the employment-inflation matrix in the Western World.

An Empirical Assessment of "New Theories" of Inflation and Unemployment

Stephen K. McNees

Introduction

It is generally considered impolite for a host to criticize his guests and tell them what they should not discuss. Nevertheless, that is exactly what I propose to do this morning.

I want to start by saying what I think this conference is *not* about. It is not about either the Keynesian (or aggregate demand) explanation of unemployment or the monetarist explanation of inflation. There are mounds of both theoretical and empirical work on each of these propositions. We have already formulated strong prior opinions on each so that it would be too much to hope this conference could resolve our views on these time-honored propositions.

The role of this conference *is*, instead, to advance non-Keynesian views of the determinants of unemployment and nonmonetary views of the inflation process. The role of this paper is to summarize some of the empirical evidence on these "new theories" of inflation and unemployment. Let me warn you now, the preliminary verdict is not good. (I must confess, however, this judgment also springs mainly from my prior opinions – otherwise, we would have no excuse for holding this conference.)

The Keynesian and monetarist propositions can be combined and restated to imply that the rate of inflation is *directly* related and the rate of unemployment is *inversely* related to the strength of aggregate demand (which may wholly or partly reflect the rate of monetary growth). In other words, these two time-honored propositions are consistent with a simple short-run Phillips curve, depicting an inverse relationship between inflation and unemployment rates.

The simple inverse or "Phillips curve" relationship provides a fairly accurate description of the inflation and unemployment rate data for the United States in the 1950s and 1960s, as illustrated by much of the economics literature of that period and by the open circles in Figure 1. In contrast, a positive relationship indicated by the filled circles has often been observed so far in the 1970s, particularly in 1970, 1972, 1974, and 1976. Many of these deviations from the "normal" negative relationship could be accounted for by appealing to "external" or "special" factors, such as extreme "wage distortion" in 1970, wage and price controls in 1972, and the oil price shock in 1974. In short, the inflationary

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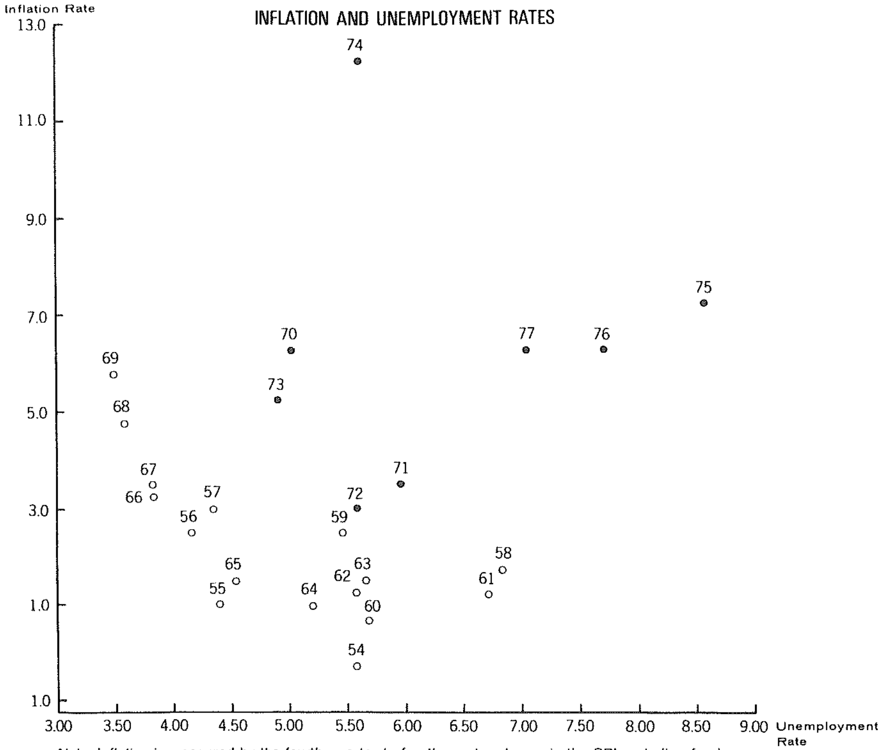
Figure 1

INFLATION AND UNEMPLOYMENT RATES



Note: Inflation is measured by the fourth-quarter to fourth-quarter change in the CPI excluding food.

experience of the 1970s has led empirically oriented nonmonetarists, primarily the builders of large-scale structural econometric models, to attempt to refine the role of supply factors, government policies, and international economic considerations in price determination. (See, for example, Klein [1978].)

Even before the turbulence of the 1970s, the Phillips curve was not without its critics. In the late 1960s, Friedman (1968) and Phelps (1967) criticized the inverse or "trade-off" notion and proposed instead the concept of a "natural" unemployment rate (NUR). The NUR idea gained currency as it was capable of accounting for the experience in 1970 and 1972 without resorting to "special factors." However, in the early 1970s, the original specification of the NUR theory also came under attack. On a theoretical level, Lucas (1972) criticized the "adaptive expectations" mechanisms which characterized early attempts to implement the NUR theory empirically. The early adaptive expectations version of the NUR theory could not simply reconcile the paths of inflation and unemployment in 1973-75 and the failure of inflation to decelerate during the subsequent recovery period.[1] Just as the failure of the Phillips curve has sent nonmonetarists back to their drawing boards to explain price behavior in the 1970s, acceptance of the NUR concept shifts the focus of non-Keynesians' attention to the formulation and measurement of expectations and to explanations of the cyclical behavior of the unemployment rate. One of the earliest and clearest empirical implementations is Sargent's (1973) combination of the NUR hypothesis with the assumption of rational expectations (NUR-RE).

The original objective of this paper was to take a preliminary look at the empirical success of these "new theories." The first part of the paper considers the NUR-RE model and the second part deals with various nonmonetarists' attempts to refine or replace the Phillips curve. As the research progressed a secondary objective developed – to explain the difficulties in determining the "empirical success" of these (or any) theories. Some of the evidence is taken from ex ante (before the fact) forecasting situations, in which no information about the future can be known with certainty. Some of the evidence is from ex post (after the fact) model simulations in which the actual historical values of the exogenous variables are used to solve the model. Some of the ex post simulations (and, of course, all ex ante forecasts) are post-sample – i.e., they pertain to a period subsequent to the period to which the model was fit. Some of the ex post simulations are in-sample, i.e., they show how well the model tracks the period from which the model coefficients were estimated. No single type of evidence will be regarded as conclusive by everyone. This paper, therefore, presents a variety of different types of evidence and employs several

[1] The simplest version of the NUR hypothesis also had some problems in the early 1960s when a stable inflation rate was associated with unemployment rates higher than anyone then (and almost anyone now) would have measured the "natural rate." This problem could be remedied by raising one's estimate of the NUR in the early 1960s but this solution would only come at the cost of rendering earlier periods like 1954 and 1959 inexplicable. The point is that without some modicum of agreement about how to measure the NUR, how to describe and measure the formulation of expectations, and some attention to the appropriate (stable?) lag structure, the NUR theory is without empirical content.

different imperfect (i.e., not definitive) tests of these theories. The reader must determine whether it is the theories that succeed or fail or whether it is the tests themselves that fail. The hope is that the paper will contribute to a greater appreciation of both the importance and the difficulty of attempting to evaluate theories on an empirical basis.

Tests of the Rational Expectations Version of the Natural Unemployment Rate Hypothesis

Sargent (1973) tested the natural unemployment rate hypothesis under the assumption of rational expectations (NUR-RE). His test exploits this theory's strong implication "that the 'innovation,' or new random part of the unemployment rate, cannot be predicted from past values of any variables, and that it cannot be affected by movements in past values of government policy variables." (p. 451). Sargent's model is a simple third-order autoregression for the unemployment rate, following the implication of the theory "that there is no better way to predict subsequent rates of unemployment than fitting and extrapolating a mixed autoregressive, moving-average process in the unemployment rate itself." Adding lagged values of wages and prices did not improve the fit of the basic Sargent model so that the NUR-RE model could not be rejected on the basis of that test. However, when a larger set of information, including the money supply and government deficits, was included, there was a statistically significant improvement in the fit, requiring "rejection of the version of the natural rate hypothesis that assumes rational expectations formed on the basis of this expanded set of information." (p. 453). While this rejection can hardly be taken as support for Sargent's hypothesis, there are good reasons to reject the test itself rather than the model. First, Sargent cites several econometric reasons for interpreting the rejections with caution. In addition, he correctly notes that his tests

> have not been shown to be of comfort to advocates of any particular alternatives to the natural rate hypothesis. That is, it has not been shown that an autoregression for unemployment yields ex ante predictions of unemployment inferior to those of a particular structural macroeconometric model that embodies a particular aggregate supply theory other than the natural rate hypothesis. A particular alternative aggregate supply hypothesis might well be able to predict unemployment better than an autoregressive moving-average process, but there is no way of knowing for sure until a horse race is held.

Sargent cites Nelson (1972) on the performance of the FRB-MIT-PENN model as evidence for his assertion that he was "aware of no evidence that shows that any particular existing structural model embodying a specific alternative to the natural rate hypothesis can outperform it in predicting the course of the unemployment rate." (p. 464). He urges "that the natural unemployment rate hypothesis [with rational expectations], . . . be tested against specific competing hypotheses by setting up statistical prediction 'horse races.' " (p.451).

To the best of my knowledge, no one has accepted Sargent's challenge. Below, I present one test like Sargent's along with several types of "horse races" between the Sargent model and various alternative models and predictive procedures.[2] Although no single statistical test is sufficient to declare "a winner," it is hoped that the battery of tests will provide some indication of the empirical success of the competing hypotheses.

The first test is much like Sargent's – an examination of whether the addition of the other economic variables significantly improves the *within sample fit* of the Sargent model. It would be of little interest to find, after an exhaustive search of economic time series, *some* variable that is correlated with the residuals of the Sargent model and thus could improve its in-sample fit.[3] I have chosen, therefore, to test only the explanatory variable that would probably first occur to a practical forecaster conversant with "Okun's law" – the GNP gap (see Okun [1962]). The result of adding the gap, lagged one-period, to the Sargent model is given below:

$$UR_t = \underset{(.28)}{1.70} + \underset{(.14)}{1.14}\, UR_{t-1} - \underset{(.16)}{0.72}\, UR_{t-2} + \underset{(.09)}{0.19}\, UR_{t-3} + \underset{(.03)}{0.15}\, GAP_{t-1}$$

$\bar{R}^2 = 0.9569$; S.E. = .292; D.W. = 1.92

Period of fit: 1952:2–1977:4. GAP is based on the Council of Economic Advisers' definition of potential GNP.

The t-statistic on the lagged value of the GNP gap is 5.15, highly significant statistically. Consequently, this application of Sargent's test, like his own second application, requires rejection of Sargent's version of the natural rate hypothesis with rational expectations.

For the reasons Sargent has noted (p. 453), the result of this in-sample test, while certainly not favorable, cannot be regarded as conclusive grounds for rejecting the model. He rightly encourages post-sample "horse races" between his model and alternative competitors.

Table 1 presents three "horse races" between the Sargent equation and alternative predictive techniques. All of the predictions are outside of the sample – the Sargent equation was reestimated each quarter up to the start of the prediction period (using the latest version of the actual data) and extrapolated forward dynamically.

[2] The Sargent model is defined as the third-order autoregression he used in the 1973 tests. Sargent's period of fit was 1952:1 through 1970:4; when the equation is reestimated through 1977:4 the fit improves somewhat, the standard error holds constant, and the coefficients, on the basis of a Chow test, are not significantly different. There is presumably, therefore, no reason to believe this specification is not still representative of the natural rate *cum* rational expectations "new theory" of the unemployment rate.

[3] This is undoubtedly the major reason why Sargent so heavily discounts the results of his second test (pp. 452–53) which is based on the addition of three lagged values of eight economic variables the selection of which was unmotivated and therefore apparently unabashedly ad hoc.

TABLE 1

Post-Sample Test of the Sargent Equation
Root Mean Square Error
(cumulative changes, percentage points)

A. vs. Ex Post Dynamic Simulation of an Econometric Model

Simulation period: 1969:2–1977:4

	Forecast Horizon (quarters)					
	1	2	3	4	5	6
Sargent	.3	.7	1.0	1.3	1.5	1.7
Fair, EM	.4	.6	.8	.8	.8	.8

B. vs. Subjectively Adjusted, Ex Ante Forecasts

Forecast period: 1970:3–1977:2

	Forecast Horizon (quarters)					
	1	2	3	4	5	6
Sargent	.3	.8	1.1	1.4	1.6	1.8
ASA	.2	.4	.7	.9	1.0	–
Chase	.3	.6	.8	1.0	1.2	1.4
DRI	.3	.5	.7	.9	1.1	1.3
Wharton	.3	.6	.8	1.0	1.1	1.1

C. vs. Mechanically Generated Ex Ante Forecasts

Forecast period: 1970:3–1975:2

	Forecast Horizon (quarters)			
	1	2	3	4
Sargent	.3	.8	1.1	1.3
Fair, FM	.3	.7	1.0	1.1

SOURCES: The Fair econometric model (EM) data are from Fair (1978) Table 4. The subjectively adjusted ex ante forecast data are from McNees (1977). The Fair forecasting model (FM) data are from McNees (1975). For each test, the Sargent equation (1973) was reestimated with the latest actual data from 1952:1 through the quarter before the extrapolation period. The Fair econometric model was also reestimated repeatedly through two quarters before the simulation period.

The first test (Panel A) is a comparison of the Sargent equation and ex post, post-sample dynamic simulations of the Fair econometric model (1974). Ex post simulations, in which the actual past and future values of the exogenous variables are used to generate the predictions, are traditionally used to test a model's validity. A structural econometric model is based on the proposition that there is important information in the (future) values of the exogenous variables. The Sargent model contains no exogenous variables. A defender of the Sargent approach could argue that this comparison is biased in favor of the econometric model whose ex post errors reflect information on the actual, future values of the exogenous variables in the model.

Panel B presents a comparison with the ex ante (or before the fact) forecasts of three of the major econometrically based forecasting services as well as the median forecast from the American Statistical Association/National Bureau of Economic Research survey. The forecasts were formulated before the fact and clearly, therefore, do not benefit from any certain information about the future. Although these forecasts were based on an econometric model, they are not strictly "scientific" (in the sense of being mechanically replicable) because the model forecasts are subjectively adjusted by the model proprietor. These forecasts can benefit (or suffer!) from the forecasters' subjective opinions about the future.

The last test (Panel C in the table) is a comparison of the Sargent equation and the ex ante forecasts which were mechanically generated with the Fair forecasting model (1970). In order to solve a model some estimate of the future values of the exogenous variables must be made. The future values of many of the exogenous variables were taken from external sources available at the time the forecast was made. The values of the other variables appear to have been chosen on the basis of fairly simple, mechanical rules involving a minimal amount of judgment. Once the exogenous variables were chosen, no subjective adjustments were made to the "pure model" results to account for events such as wage and price controls or increases in the price of imported oil. This test, which excludes both subjective adjustments and exogenous variable certainty, does not appear to contain any bias in favor of the structural model.

The results of these three tests are similar and, hence, easily summarized: The Sargent equation's one-quarter-ahead post-sample predictions are about the same as those based on alternative techniques. However, the Sargent equation does exhibit a distinctly stronger tendency toward error accumulation when extrapolated dynamically over a longer horizon.

Interpretation of this result is not as straightforward – the glass can be viewed as half empty or half full. A defender of the Sargent approach would stress the similarity of the one-period result and would note that the multi-period results for the alternative approaches incorporate external information – subjective (in Panel B) or objective (in Panel A) – which, it could be argued, biases the multi-period test against the Sargent equation. The results in Panel C, where there are no apparent biases, are probably too similar to draw a statistically rigorous verdict.

A critic of the Sargent equation could argue that the comparisons in panels B and C are biased in favor of the Sargent equation because it was estimated with and judged against the latest revision of the data whereas in a realistic ex ante forecasting situation even recent history is uncertain. As for the incorporation of external information, this is an inherent difference between econometric and time series modeling. Placing the econometric models in an ex ante forecasting situation puts each approach on an equal footing with respect to using only "historical" rather than "future" information for the forecast. Time series models, by their very nature, are restricted to using a limited amount of information in arriving at their forecasts.[4]

Summary and Assessment of the Evidence on the NUR-RE Model

In the strictest sense, a time series model, such as Sargent's NUR-RE model, and a structural, econometric model are not comparable. The former contains no exogenous variables while the latter inherently must. This standard is too strict for most who strive to have some informed opinion on the relative importance of Sargent's NUR-RE model and its alternatives. On the basis of one of Sargent's tests and the similar one conducted above, Sargent's model can be rejected on rigorous statistical grounds. While this evidence ought not to be ignored, Sargent's verdict that these tests must be interpreted with caution is sound. The results of in-sample tests cannot be regarded as conclusive. For this reason, three post-sample tests were conducted. The post-sample results show a disparity between the single-period and the multi-period results.[5] One-period-ahead, the NUR-RE model performs about as well as the alternative approaches. On the basis of this evidence *alone,* the results are inconclusive – whatever differences that would emerge by presenting the data to more decimal places could surely not be regarded as significant in a statistical sense. In the multi-period results, the NUR-RE model exhibits a greater tendency toward error accumulation as the horizon extends further into the future. This may be due to the linear specification of the NUR-RE model. It may also be due to the enhanced value of the information in the exogenous variables over longer horizons. This result appeared in three different tests, each containing a different type of information: a) ex post simulations (using actual values of the exogenous variables), b) subjectively adjusted ex ante forecasts, and c) ex ante forecasts with no subjective adjustments and mechanical selection of the values of the exogenous variables. In light of the small number of post-sample observations and the small differences in the summary error statistics, the multi-period results may be insufficient for making a statistically rigorous rejection of the NUR-RE model. Nevertheless, if *any* importance is assigned to either the in-sample results or the multi-period results, the case for Sargent's NUR-RE model stands unproven.

[4] This matter is discussed more fully below. For an alternative method of accounting for exogenous variable uncertainty, see Ray Fair, "Estimating the Expected Predictive Accuracy of Econometric Models," Cowles Foundation Discussion Paper No. 480, January 1978, where he develops and estimates standard errors for econometric and autoregressive models. His results for the unemployment rate are similar to those reported in Panel A of Table 1.

[5] A more complete discussion of the problems of interpretation in comparing single-period and multi-period results of time series models and structural models appears in McNees (1978).

Tests of Nonmonetarist Wage and Price Models

The term "nonmonetarist wage and price models" clearly covers a variety of different approaches which probably should not be lumped into one amorphous phrase. The term may have been more applicable to the circa-1970 vintage of wage and price models but modelers have reacted differently to the dramatic events of the 1970s. Most have chosen to refine the Phillips curve approach by incorporating additional equations representing supply phenomena, while some have taken new (e.g., "stage-of-processing") approaches. It is one of the goals of this conference, but beyond the scope of this paper, to describe and catalogue these efforts.

The best test of a model is its post-sample performance. The opening section presents recently published post-sample assessments by two model builders. Post-sample assessments of other wage and price models are not readily available and it is perilous for an outsider to attempt to reestimate others' models because special data and estimation techniques are often used. On the other hand, it is fairly easy to perform simulation experiments with current versions of structural models and these are also presented below. Although these in-sample simulations are not sufficient to establish the validity of the models, fitting the historical data relatively well is the logical first check of a model's performance.

Post-sample Results

Robert J. Gordon (1977) analyzed the post-sample performance of his wage-price model originally fit through mid-1971.[6] When the price equation is refit over the same period using the latest revised data, several of the coefficients change substantially and the fit deteriorates somewhat (the standard error increased 18 percent). (See Table 2, A1 and A2.) More importantly, the refitted equation does not work well outside the sample period. The post-sample root mean square error (RMSE) was 2.4 percent (at a simple annual rate), nearly two and one-half times larger than the in-sample standard error. This increase is large enough to support the conclusion that the model fitted to the sample period was a poor representation of the post-sample events which were to follow.[7] In addition, the post-sample RMSE for Gordon's price measure (the deflator for nonfood business product net of energy) is 50 percent larger than the RMSE of ex ante forecasts of the more volatile implicit GNP price deflator (IPD) over the same period. In addition, the post-sample errors accumulate

[6] It is important to note that although the equation is similar to the one Gordon originally proposed in 1971, the equation "was altered somewhat in 1975 and thus incorporates knowledge of events to that point." (p. 264).

[7] Under the null hypothesis that the estimated coefficients and the standard error of estimate computed from the sample period accurately represent the post-sample structure of the mechanism generating the variable of interest, the ratio of the mean squared error and the square of the standard error of estimate is distributed as an F statistic. If the null hypothesis were true in this case, the F value is highly unlikely to exceed a critical value of about two. In this and the following instances the ratio exceeds two, indicating an inappropriate model and/or a particularly misleading sample-period draw.

TABLE 2

Wage-Price Models:
Post-Sample Performance
(Simple annual growth rates)*

A.	Gordon price equation:	
	1) Standard error, original data	.8
	2) Standard error, revised data	1.0
	3) RMSE post-sample (1971–76)	2.4
	4) RMSE Ex Ante IPD forecast error (1971–76)	1.6
B.	Gordon wage equation:	
	1) Standard error, original data	.5
	2) Standard error, revised data	.6
	3) RMSE, post-sample	
	Price deflator	
	a) private nonfood business product net of energy	1.5
	b) private nonfarm business	2.1
	4) RMSE Ex Ante forecast error	1.7

C. Fair price (IPD) equation, RMSE of cumulative percent changes:

	1969:II–1977:IV Forecast Horizon (quarters)							
	1	2	3	4	5	6	7	8
1) Fair Model	2.00	1.86	1.91	1.97	1.99	1.97	1.96	1.92
2) Naive Model	1.88	1.96	2.12	2.36	2.61	2.82	3.06	3.26
3) Ex Ante Forecast	1.58	1.78	2.05	2.26	N.A.	N.A.	N.A.	N.A.

D. Fair wage equation, RMSE of cumulative percent changes.

	1969:II–1977:IV Forecast Horizon (quarters)							
	1	2	3	4	5	6	7	8
1) Fair Model	3.12	2.50	2.28	2.31	2.31	2.33	2.32	2.32
2) Naive Model	2.68	1.88	1.61	1.47	1.37	1.34	1.37	1.42
3) Ex Ante Forecast	2.42	1.74	1.55	1.52	1.60	1.66	1.68	1.63

*To facilitate comparisons, all data were converted to simple annual rates.

SOURCES: The model data are from Gordon (1977) and Fair (1978). Gordon's price variable is the deflator for nonfood business product net of energy. The ex ante price forecasts are of the implicit GNP price deflator (IPD) and are the median from the ASA/NBER survey. Gordon's wage variable is his own measure of the wage rate. The ex ante wage forecasts are Wharton EFA's forecasts of its own compensation measure and are not, therefore, strictly comparable to either Gordon or Fair. The Fair model (naive model) was reestimated repeatedly through two quarters prior to (one quarter of) the simulation period.

leading the equation to "overpredict inflation during 1971–76 very substantially." (p. 258). The error accumulation problem can be remedied by using a different proxy for excess demand and by constraining the sum of the coefficients on labor cost to equal 1.0 but even this altered version of "the best equation" exhibits a post-sample RMSE of 2.7 percent, nearly three times larger than its in-sample standard error and an even larger multiple of the RMSEs of the ex ante forecasts of that period. When the price equation is refit through 1976, however, the in-sample standard error falls to a little more than 1.0 percent at an annual rate.

A parallel story can be told for Gordon's wage equation. When the original 1971 wage equation was reestimated with revised data, the fit deteriorated only slightly but the coefficients were unstable. The data revisions rendered one of the proxies for labor market tightness, unemployment-dispersion, insignificant and vindicated the natural rate hypothesis in that wage changes fully incorporate changes in [product] price inflation. "As in the case of the structural price equation, the post-sample extrapolation errors of the wage equation are vastly larger than the in-sample standard error." (p. 268) More precisely, the post-sample RMSE increased to about 1.5 percent at a simple annual rate, nearly three times the standard error, using Gordon's preferred price measure (excluding food and energy) and to more than 2.0 percent, four times the standard error, using a broader alternative index. These compare with the RMSEs of ex ante forecasts which are about 1.7 percent over a two-quarter horizon.

Constraining the equation to conform to the adaptive expectations version of the NUR hypothesis cut down the post-sample error accumulation but even this constrained equation had a RMSE nearly two and a half times (using the deflator for private nonfood business product net of energy as the price measure) or more than three times (using the deflator for private nonfarm business) the in-sample standard error. When the equation was refit through 1976, the standard error declined to about .6 percent at an annual rate.

This evidence shows that Gordon's original price and wage model did not perform well outside the fit period. Even with the "best" respecification, the post-sample errors are larger than those made by ex ante forecasts at the time. When the equations are refit through 1976, their standard errors fall back to near those of the original specification. However, in light of the poor post-sample performance of the original specifications, there is no assurance that the later specifications will perform successfully outside the period of fit.

Ray Fair (1978) analyzed the post-sample properties of his econometric model. Some of his results for the wage and price variables in his model are presented in the bottom part of Table 2.[8] Panel C of the table presents the

[8] Fair also conducts stochastic simulations to compute the estimated standard errors of his model and a naive autoregressive model. Fair's ingenious method permits decomposition of the standard error into four alternative sources – stochastic error terms, coefficient estimates, exogenous-variable forecasts, and the degree of misspecification of the model. (See Table 1, pp. 27–28). The RMSEs of ex post simulations presented above do not, of course, reflect errors due to incorrect selection of values for exogenous variables. Nevertheless, Fair's estimated standard errors and the RMSEs are very similar, quantitatively *and* qualitatively, for the deflator and the wage rate.

post-sample RMSEs of the model's price (IPD) predictions along with the corresponding measures for a "naive" autoregressive model. The naive model consists of regressing each variable on a constant, a linear time trend, and its first eight lagged values. For purposes of comparison the RMSEs of the median ex ante forecast of the ASA/NBER survey over the same time period are also shown. The performance of the Fair model price predictions is mixed. Over very short horizons, the Fair model simulations are somewhat inferior to the ex ante forecasts and about the same as the naive model. Over horizons of a year or more, the Fair model simulations are more accurate than the naive model and appear to be more accurate than the ex ante forecasts (although precisely comparable ex ante data were not available). The ex post model simulations clearly benefit from using actual exogenous variable values, the importance of which probably increases with the forecast horizon. Although these are post-sample simulations (so that the coefficients are not estimated with actual data from the simulation period), it should be noted that the model was first specified in 1974, well into the test period, and that some modifications were made as late as 1977. A conclusive assessment of the Fair model's price performance must await the accumulation of more post-specification experience.

Ex post simulations of the wage rate, shown in Panel D, were disappointing. The naive model outperforms the structural model in forecasting the wage rate over all horizons. The same result holds for a comparison with set of ex ante forecasts of compensation, shown in row D3, although these results are not strictly comparable because they pertain to a different variable and a slightly different forecast period. Nevertheless, these results are not encouraging for the structural model's ability to explain wage behavior. Taken at face value, the model contributes no additional explanatory power to that of a naive, purely statistical model.

Thus, there is considerable evidence that *no* 1970–71 vintage wage-price model is capable of explaining the wage-price behavior of the 1970s at all adequately. As a consequence, those earlier specifications are now obsolete – probably *all* serious wage-price models have been respecified during the last few years. Rather than continue to search the entire 1970–71 vintage of wage-price models, it would seem more fruitful to focus attention on the performance of the current stock of wage-price models.

In-sample Results

What have we learned about wage-price determination from the 1970s experience? Were the large errors in the 1970s unavoidable (in the sense of being due to noneconomic events) or could different specifications have tracked the wage-price behavior in the 1970s? In order to address this question, I collected one- through eight-quarter-ahead dynamic simulations of the current versions of some of the most prominent macroeconometric models – DRI, Fair, FMP, Michigan, and Wharton. An assessment of these in-sample results must be conducted with extreme care because *cross-model comparisons are unwarranted and virtually certain to be misleading.*[9]

Even though cross-model comparisons are not warranted, simulation performance cannot be evaluated in a vacuum. Because the absolute standard of perfection is unrealistic, prediction performance must be evaluated relative to some standard of comparison. Two standards of comparison are considered here: (1) each model simulation is contrasted with a companion "naive" or purely statistical time series model (which contains no exogenous variables); (2) each model simulation is also compared with ex ante forecasts.

(1) *Time Series Model as a Standard of Comparison*

A time series model was fit for the implicit GNP price deflator (IPD) over the period 1954-1977. The estimated equation is:[10]

$$\Delta IPD_t = \underset{(6.6)}{.6555}\, \Delta IPD_{t-1} + \underset{(2.8)}{.2827}\, \Delta IPD_{t-2} + \underset{(1.5)}{.0767}$$

$$\hat{\sigma}_u = .327$$

[9] To appreciate this fact one must recall that an econometric model is conditioned upon a set of exogenous variables (the values of the predicted variables are determined by the set of predetermined variables). Models inherently differ with respect to the size and composition of the set of exogenous variables on which they are based. To attempt to standardize for use of exogenous variables would violate the intended structure of the model. (Requiring all models to adopt a common set of assumptions with regard to the *values* of the exogenous variables does not violate the integrity of the model. This is, in fact, what is done in ex post comparisons where all models use the actual values of exogenous variables. Standardizing on a common set of exogenous variable values is a useful exercise which, unfortunately, is seldom followed in an ex ante context.) A simple example is the treatment of the period of wage and price controls (both imposition and relaxation). Some of the models *include* dummy variables in their estimated equations to account for controls. Other models *exclude* any special treatment of the controls period. To standardize the treatment of the controls episode among the models, i.e., to force all the models to conform to a certain procedure, would, at a minimum, require reestimation of several models and quite possibly would call for respecification of their basic structure. This standardized set of models would not be representative of those models in current use.

An additional obstacle to cross-model comparisons is that, due primarily to data problems, different models are capable of being simulated over different historical intervals. Specifically, some models cannot be simulated for the early postwar period due to the lack of data for some key series. Experience has shown that the forecast interval is a major source of variation in prediction errors. To standardize for simulation intervals would require discarding some of the available data, which are all too limited in any case.

[10] A chi square test (X^2_{21} = 12.08) indicates that the hypothesis that the residuals are white noise could not be rejected at a high (94 percent) level of significance. It is of some interest to note that the fit is better than that which Nelson (1972) obtained with a different time series model. Nelson's specification was not satisfactory over the longer sample period. suggesting that some time series models may share with some econometric models the property of structural instability.

Table 3 presents the in-sample ex post dynamic simulation RMSEs of five econometric models relative to those of this time series model. With a single exception, the current vintage of econometric models are able to track history more accurately than the time series model. The margin of superiority is inversely related to the forecast horizon – the one-period RMSEs are generally fairly close, the two-period errors substantially smaller with the margin of superiority increasing steadily through the eight-quarter horizon (where all of the models are at least as good as their companion time series model).

These results need to be interpreted with the following qualifications: (a) the model simulations were taken "as is" with no attempt to standardize for selection of exogenous variables, or simulation intervals; (b) the time series model, by design, does not benefit from information contained in the actual values of the exogenous variables which, by design, are presumed of great importance in the structural models.[11] The evidence is consistent with the hypothesis that this information takes on greater importance as the forecast horizon is extended and/or the alternative hypothesis that the nonlinearity of the econometric models is a greater advantage for longer horizons.

These results are consistent with several previous studies: (a) Nelson (1972) found that the one-period-ahead RMSE of IPD predictions of the FMP model was smaller than that of a time series model both in-sample (.195 vs. .230) and post-sample (.261 vs. .346); (b) Hirsch, Grimm, and Narasimham (1976, p. 245) found substantially smaller RMSEs for various versions of the BEA model than a time series model for one-period forecasts (.45 to .59 vs. .75 index points) and the margin of superiority increased as the forecast horizon was extended.

Even though some earlier studies reached different conclusions, the recent evidence indicates that most of the current stock of econometric models fit the historical data for IPD better than a time series model and better than earlier vintages of structural models. While one can hope that this superior performance is an indication that current models have captured the "true" wage-price structure more completely, one must await their post-sample performance to be sure.

(2) *Ex Ante Performance as a Standard of Comparison*

Much of the dissatisfaction with the current state of empirical macroeconomics undoubtedly stems from the errors of ex ante forecasts that were issued for the period 1973-1974. Many believe these were "poor" forecasts in the sense that it is now known (or perhaps known even then) that important factors were overlooked or misassessed. Nevertheless, it is obvious that there is a certain irreducible, minimum feasible error inherent in any attempt to prophesy the future. Because this "innovation" undoubtedly varies over time, it would be inappropriate to attach much significance to a large *absolute* error. The ex post simulations provide an interesting standard of comparison for evaluating these errors. They address the hypothetical question: Knowing what we know now

[11] Howrey, Klein, and McCarthy (1976) have argued "that sample-period mean squared error comparisons of the autoregressive and structural models are not powerful tests," particularly when confined to one-period horizons.

TABLE 3

Ex Post Dynamic Simulations of Prices:
Structural and Time Series Models
RMSE (cumulative percent change at simple annual rate)

Model	Simulation Period	Forecast Horizon (quarters)							
		1	2	3	4	5	6	7	8
DRI	1965–77	1.2	.9	.6	.6	.5	.5	.5	.4
ARIMA		1.2	1.5	1.5	1.7	1.7	1.8	2.1	2.4
FAIR	1960–77	1.3	.8	.7	.8	.8	.8	.7	.6
ARIMA		1.3	1.3	1.3	1.4	1.6	1.6	1.8	2.0
FMP	1961–77	1.0	1.0	.9	1.0	1.1	1.1	1.1	1.2
ARIMA		1.4	1.4	1.3	1.5	1.6	1.6	1.8	2.1
MICHIGAN	1956–77	1.0	.6	.5	.5	.6	.5	.5	.5
ARIMA		1.3	1.3	1.2	1.4	1.5	1.5	1.7	1.9
WHARTON	1963–77	3.4	2.6	2.4	2.5	2.3	2.2	2.1	2.2
ARIMA		1.3	1.5	1.4	1.6	1.7	1.8	2.0	2.2

NOTES: The times series model's RMSEs are in-sample dynamic extrapolations of the (2, 1, 0) model described in the text. The in-sample dynamic model simulations were generously supplied by Otto Eckstein and Frank Cooper of Data Resources, Inc. (DRI), Ray Fair of Yale University (Fair), Jared J. Enzler, Board of Governors of the Federal Reserve System, using the Federal Reserve–MIT–University of Pennsylvania model (FMP), Saul H. Hymans of the University of Michigan (Michigan), and Lawrence R. Klein and Richard M. Young, Wharton Econometric Forecasting Associates, Inc. (Wharton). Each simulation started from the fourth quarter of the preceding year so that for DRI, for example, there are 13 one-quarter through four-quarter-ahead observations and 12 five-quarter through eight-quarter-ahead observations.

(including the actual values of the exogenous variables), how accurate might our predictions have been?

Table 4 presents a comparison of ex post and ex ante forecast errors over the 1970s. These data suggest that the current version of four of the five econometric models can, with hindsight knowledge of the actual values of the exogenous variables, simulate the course of inflation in the 1970s considerably more accurately than the ex ante forecasts of those times. However, these ex post simulations benefit both from using actual values of the exogenous variables and from being in-sample, i.e., using actual values to estimate their coefficients. Some indication of the benefits of using actual data to estimate the model can be gained from contrasting the in-sample and post-sample performance of the time series model. (For the post-sample extrapolations, the time series model was repeatedly reestimated up to the forecast period.) Row 1 in the table shows that the in-sample performance of the time series model is about the same as the ex ante forecasts and that the post-sample performance is considerably worse.

TABLE 4

Ex Post Simulations and Ex Ante Forecasts
of Inflation in the 1970s

RMSEs (cumulative percent changes at simple annual rates)

Model/Forecaster	Forecast Horizon (quarters) 1	2	3	4
1) ARIMA, in-sample	1.4	1.7	1.8	2.1
ARIMA, post-sample	1.7	2.3	2.5	2.9
2) DRI, ex post	1.2	.9	.6	.5
DRI, ex ante	1.6	2.0	2.0	2.4
3) Fair, ex post	1.7	1.0	.7	.7
ASA, ex ante	1.5	1.6	1.9	2.3
4) FMP, ex post	1.0	1.2	1.3	1.5
ASA, ex ante	1.5	1.6	1.9	2.3
5) Michigan, ex post	.8	.5	.4	.3
ASA, ex ante	1.5	1.6	1.9	2.3
6) Wharton, ex post	4.8	3.6	3.4	3.6
Wharton, ex ante	1.6	1.5	1.6	1.9

NOTES: The ex ante data are from McNees (1977). The ex post simulations are those described in Table 3. Each simulation was started in the fourth quarter, so that each RMSE is based upon only seven observations.

The ex post simulations benefit similarly and thus give an overly optimistic impression of the maximum accuracy which could have been expected.

Summary and Assessment of the Evidence on Wage and Price Models

Current specifications of macroeconometric models can simulate the in-sample movements of wages and prices well relative to a time series model and well also relative to the forecasts that were released ex ante. However, these ex post, in-sample results may provide an optimistic impression of the models' post-sample performance or their future performance in ex ante situations where the future values of the exogenous variables are unknown. As was noted above, previous wage-price models which also fit the historical data at the time did not perform nearly as well outside the same period *or* relative to autoregressive models *or* relative to the ex ante forecasts. Have we learned something from the 1970s' experience, or do we just think we have learned something? Will the current vintage of econometric models suffer the same post-sample fate as their predecessors?

These are empirical questions that cannot be answered with any degree of certainty with data presently available. The fundamental empirical problem is the size and frequency of institutional changes or "external shocks" which occurred in the 1970s. Among the more obvious are the following: (1) The imposition and relaxation of several different phases of wage and price controls. (2) The switch from fixed to flexible exchange rates and the subsequent experience of learning to live with exchange rate induced variations in prices of traded commodities. (3) The sudden quadrupling of the price of imported oils. (4) The changes in demographics, public policy, and social attitudes and their alleged impact on the "natural" rate of unemployment. (5) The growing importance of governmentally mandated supply-restricting or cost-raising measures. (6) The introduction of a new framework for conducting monetary policy.

All of these "special factors" were intertwined, both temporally and causally, and superimposed upon the most extreme business cycle of the postwar period. Econometric model-builders responded to these events by respecifying (sometimes repeatedly) their empirical statement of the wage-price process. Even if a more complete list of the determinants of wage-price behavior has now been identified, it is not clear that our experience with these new institutions has been sufficiently long to quantify the independent influence of each of these factors.

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II. "New" Explanations of the Persistence of Inflation and Unemployment

After Keynesian Macroeconomics

Robert E. Lucas and Thomas J. Sargent

1. Introduction

For the applied economist, the confident and apparently successful application of Keynesian principles to economic policy which occurred in the United States in the 1960s was an event of incomparable significance and satisfaction. These principles led to a set of simple, quantitative relationships between fiscal policy and economic activity generally, the basic logic of which could be (and was) explained to the general public, and which could be applied to yield improvements in economic performance benefiting *everyone.* It seemed an economics as free of ideological difficulties as, say, applied chemistry or physics, promising a straightforward expansion in economic possibilities. One might argue about how this windfall should be distributed, but it seemed a simple lapse of logic to oppose the windfall itself. Understandably and correctly, this promise was met at first with skepticism by noneconomists; the smoothly growing prosperity of the Kennedy-Johnson years did much to diminish these doubts.

We dwell on these halcyon days of Keynesian economics because, without conscious effort, they are difficult to recall today. In the present decade, the U.S. economy has undergone its first major depression since the 1930s, to the accompaniment of inflation rates in excess of 10 percent per annum. These events have been transmitted (by consent of the governments involved) to other advanced countries and in many cases have been amplified. These events did not arise from a reactionary reversion to outmoded, "classical" principles of tight money and balance budgets. On the contrary, they were accompanied by massive governmental budget deficits and high rates of monetary expansion: policies which, although bearing an admitted risk of inflation, promised according to modern Keynesian doctrine rapid real growth and low rates of unemployment.

That these predictions were wildly incorrect, and that the doctrine on which they were based is fundamentally flawed, are now simple matters of fact, involving no novelties in economic theory. The task which faces contemporary students of the business cycle is that of sorting through the wreckage, determining which features of that remarkable intellectual event called the

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Keynesian Revolution can be salvaged and put to good use, and which others must be discarded. Though it is far from clear what the outcome of this process will be, it is already evident that it will necessarily involve the reopening of basic issues in monetary economics which have been viewed since the thirties as "closed," and the reevaluation of every aspect of the institutional framework within which monetary and fiscal policy is formulated in the advanced countries.

This paper is in the nature of an early progress report on this process of reevaluation and reconstruction. We begin by reviewing the econometric framework by means of which Keynesian theory evolved from disconnected, qualitative "talk" about economic activity into a system of equations which could be compared to data in a systematic way, and provide an operational guide in the necessarily quantitative task of formulating monetary and fiscal policy. Next, we identify those aspects of this framework which were central to its failure in the seventies. In so doing, our intent will be to establish that the difficulties are *fatal:* that modern macroeconomic models are of *no* value in guiding policy, and that this condition will not be remedied by modifications along any line which is currently being pursued.

This diagnosis, if successful, will suggest certain principles which a useful theory of business cycles must possess. In the latter part of this paper we shall review some recent research which is consistent with these principles.

2. Macroeconometric Models

The Keynesian Revolution was, in the form in which it succeeded in the United States, a revolution in *method.* This was not Keynes's [13] intent, nor is it the view of all of his most eminent followers. Yet if one does not view the revolution in this way, it is impossible to account for some of its most important features: the evolution of macroeconomics into a quantitative, *scientific* discipline, the development of explicit statistical descriptions of economic behavior, the increasing reliance of government officials on technical economic expertise, and the introduction of the use of mathematical control theory to manage an economy. It is the fact that Keynesian theory lent itself so readily to the formulation of explicit econometric models which accounts for the dominant scientific position it attained by the 1960s.

As a consequence of this, there is no hope of understanding either the success of the Keynesian Revolution or its eventual failure at the purely verbal level at which Keynes himself wrote. It will be necessary to know something of the way macroeconometric models are constructed and the features they must have in order to "work" as aids in forecasting and policy evaluation. To discuss these issues, we introduce some notation.

An econometric model is a system of equations involving a number of endogenous variables (variables that are determined by the model), exogenous variables (variables which affect the system but are not affected by it), and stochastic or random shocks. The idea is to use historical data to estimate the model, and then to utilize the estimated version to obtain estimates of the consequences of alternative policies. For practical reasons, it is usual to use a standard linear model, taking the structural form[1]

[1] Linearity is a matter of convenience, not of principle. See Section 6.3, below.

$$(1)\quad A_0 y_t + A_1 y_{t-1} + \ldots + A_m y_{t-m} = B_0 x_t + B_1 x_{t-1} + \ldots + B_n x_{t-n} + \epsilon_t$$

$$(2)\quad R_0 \epsilon_t + R_1 \epsilon_{t-1} + \ldots + R_r \epsilon_{t-r} = u_t,\ R_0 \equiv I.$$

Here y_t is an (Lx1) vector of endogenous variables, x_t is a (Kx1) vector of exogenous variables, and ϵ_t and u_t are each (Lx1) vectors of random disturbances. The matrices A_j are each (LxL); the B_j's are (LxK), and the R_j's are each (LxL). The (Lx1) disturbance process u_t is assumed to be a serially uncorrelated process with $Eu_t = 0$ and with contemporaneous covariance matrix $Eu_t u_t' = \Sigma$ and $Eu_t u_s' = 0$ for all $t \neq s$. The defining characteristic of the exogenous variables x_t is that they are uncorrelated with the ϵ's at all lags so that $Eu_t x_s'$ is an (LxK) matrix of zeroes for all t and s.

Equations (1) are L equations in the L current values y_t of the endogenous variables. Each of these structural equations is a behavioral relationship, identity, or market clearing condition, and each in principle can involve a number of endogenous variables. The structural equations are usually not "regression equations"[2] because the ϵ_t's are in general, by the logic of the model, supposed to be correlated with more than one component of the vector y_t and very possibly one or more components of the vectors $y_{t-1}, \ldots y_{t-m}$.

The structural model (1) and (2) can be solved for y_t in terms of past y's and x's and past shocks. This "reduced form" system is

$$(3)\quad y_t = -P_1 y_{t-1} - \ldots - P_{r+m} y_{t-r-m} + Q_0 x_t + \ldots + Q_{r+n} x_{t-n-r} + A_0^{-1} u_t$$

where[3]

$$P_s = A_0^{-1} \sum_{j=-\infty}^{\infty} R_j A_{s-j}$$

$$Q_s = A_0^{-1} \sum_{j=-\infty}^{\infty} R_j B_{s-j}.$$

The reduced form equations are "regression equations," that is, the disturbance vector $A_0^{-1} u_t$ is orthogonal to $y_{t-1}, \ldots, y_{t-r-m}, x_t, \ldots, x_{t-n-r}$. This follows from the assumptions that the x's are exogenous and that the u's are serially uncorrelated. Therefore, under general conditions the reduced form can be estimated consistently by the method of least squares. The population parameters of the reduced form (3) together with the parameters of a vector autoregression for x_t,

$$(4)\quad x_t = C_1 x_{t-1} + \ldots + C_p x_{t-p} + a_t$$

[2] A "regression equation" is an equation to which the application of ordinary least squares will yield consistent estimates.

[3] In these expressions for P_s and Q_s, take matrices not previously defined (for example, any with negative subscripts) to be zero.

where $Ea_t = O$ and $Ea_t . x_{t-j} = O$ for $j \geqslant 1$ completely describe all of the first and second moments of the (y_t, x_t) process. Given long enough time series, good estimates of the reduced form parameters – the P_j's and Q_j's – can be obtained by the method of least squares. Reliable estimates of those parameters is all that examination of the data by themselves can deliver.

It is not in general possible to work backwards from estimates of the P's and Q's alone to derive unique estimates of the structural parameters, the A_j's, B_j's, and R_j's. In general, infinite numbers of A, B, and R's are compatible with a single set of P's and Q's. This is the "identification problem" of econometrics. In order to derive a set of estimated structural parameters, it is necessary to know a great deal about them in advance. If enough prior information is imposed, it is possible to extract estimates of the (A_j, B_j, R_j)'s implied by the data in combination with the prior information.

For purposes of *ex ante* forecasting, or the unconditional prediction of the vector $y_{t+1}, y_{t+2}, \ldots$ given observation of y_s and x_s, $s \leqslant t$, the estimated reduced form (3), together with (4), is sufficient. This is simply an exercise in a sophisticated kind of extrapolation, requiring no understanding of the structural parameters or, that is to say, of the *economics* of the model.

For purposes of *conditional* forecasting, or the prediction of the future behavior of some components of y_t and x_t *conditional* on particular values of other components, selected by policy, one needs to know the structural parameters. This is so because a change in policy *necessarily* alters some of the structural parameters (for example, those describing the past behavior of the policy variables themselves) and therefore affects the reduced form parameters in highly complex fashion (see the equations defining P_s and Q_s, below (3)). Without knowledge as to which structural parameters remain invariant as policy changes, and which change (and how), an econometric model is of *no* value in assessing alternative policies. It should be clear that this is true *regardless* of how well (3) and (4) fit historical data, or how well they perform in unconditional forecasting.

Our discussion to this point has been at a high level of generality, and the formal considerations we have reviewed are not in any way specific to *Keynesian* models. The problem of identifying a structural model from a collection of economic time series is one that must be solved by anyone who claims the ability to give quantitative economic advice. The simplest Keynesian models are attempted solutions to this problem, as are the large-scale versions currently in use. So, too, are the monetarist models which imply the desirability of fixed monetary growth rules. So, for that matter, is the armchair advice given by economists who claim to be outside the econometric tradition, though in this case the implicit, underlying structure is not exposed to professional criticism. *Any* procedure which leads from the study of observed economic behavior to the quantitative assessment of alternative economic policies involves the steps, executed poorly or well, explicitly or implicitly, which we have outlined above.

3. Keynesian Macroeconometrics

In Keynesian macroeconometric models structural parameters are identified by the imposition of several types of *a priori* restrictions on the A_j's, B_j's, and

R_j's. These restrictions usually fall into one of the following categories:[4]

(a) *A priori* setting of many of the elements of the A_j's and B_j's to zero.
(b) Restrictions on the orders of serial correlation and the extent of the cross serial correlation of the disturbance vector ϵ_t, restrictions which amount to *a priori* setting many elements of the R_j's to zero.
(c) *A priori* categorization of variables into "exogenous" and "endogenous." A relative abundance of exogenous variables aids identification.

Existing large Keynesian macroeconometric models are open to serious challenge for the way they have introduced each category of restriction.

Keynes's *General Theory* was rich in suggestions for restrictions of type (a). It proposed a theory of national income determination built up from several simple relationships, each involving a few variables only. One of these, for example, was the "fundamental law" relating consumption expenditures to income. This suggested one "row" in equations (1) involving current consumption, current income, and *no other* variables, thereby imposing many zero-restrictions on the A_i and B_j. Similarly, the liquidity preference relation expressed the demand for money as a function of income and an interest rate *only*. By translating the building blocks of the Keynesian theoretical system into explicit equations, models of the form (1) and (2) were constructed with many theoretical restrictions of type (a).

Restrictions on the coefficients R_i governing the behavior of the "error terms" in (1) are harder to motivate theoretically, the "errors" being by definition movements in the variables which the *economic* theory cannot account for. The early econometricians took "standard" assumptions from statistical textbooks, restrictions which had proved useful in the agricultural experimenting which provided the main impetus to the development of modern statistics. Again, these restrictions, well-motivated or not, involve setting many elements in the R_i's equal to zero, aiding identification of the model's structure.

The classification of variables into "exogenous" and "endogenous" was also done on the basis of prior considerations. In general, variables were classed as "endogenous" which were, as a matter of institutional fact, determined largely by the actions of private agents (like consumption or private investment expenditures). Exogenous variables were those under governmental control (like tax rates, or the supply of money). This division was intended to reflect the ordinary meaning of the word "endogenous" to mean "determined by the [economic] system" and "exogenous" to mean "affecting the [economic] system but not affected by it."

By the mid-1950s, econometric models had been constructed which fit time series data well, in the sense that their reduced forms (3) tracked past data closely and proved useful in short-term forecasting. Moreover, by means of

[4] These three categories certainly do not exhaust the set of possible identifying restrictions, but in Keynesian macroeconometric models most identifying restrictions fall into one of these three categories. Other possible sorts of identifying restrictions include, for example, *a priori* knowledge about components of Σ, and cross-equation restrictions across elements of the A_j, B_j, and C_j's. Neither of these latter kinds of restrictions is extensively used in Keynesian macroeconometrics.

restrictions of the three types reviewed above, it was possible to identify their structural parameters A_i, B_j, R_k. Using this estimated structure, it was possible to simulate the models to obtain estimates of the consequences of different government economic policies, such as tax rates, expenditures or monetary policy.

This Keynesian solution to the problem of identifying a structural model has become increasingly suspect as a result of developments of both a theoretical and statistical nature. Many of these developments are due to efforts to researchers sympathetic to the Keynesian tradition, and many were well-advanced well before the spectacular failure of the Keynesian models in the 1970s.[5]

Since its inception, macroeconomics has been criticized for its lack of "foundations in microeconomic and general equilibrium theory." As astute commentators like Leontief [14] (disapprovingly) and Tobin [37] (approvingly) recognized early on, the creation of a distinct branch of theory with its own distinct postulates was Keynes's conscious aim. Yet a main theme of theoretical work since the *General Theory* has been the attempt to use microeconomic theory based on the classical postulate that agents act in their own interests to suggest a list of variables that belong on the right side of a given behavioral schedule, say, a demand schedule for a factor of production or a consumption schedule.[6] But from the point of view of identification of a given structural equation by means of restrictions of type (a), one needs reliable prior information that certain variables should be *excluded* from the right-hand side. Modern probabilistic microeconomic theory almost never implies either the exclusion restrictions that were suggested by Keynes or those that are imposed by macroeconometric models.

[5] Criticisms of the Keynesian solutions of the identification problem along much the following lines have been made in Lucas [17], Sims [33], and Sargent and Sims [31].

[6] [This note was added in revision, in part in response to Benjamin Friedman's comments.] Much of this work was done by economists operating well within the Keynesian tradition, often within the context of some Keynesian macroeconometric model. Sometimes a theory with optimizing agents was resorted to in order to resolve empirical paradoxes by finding variables that had been omitted from some of the earlier Keynesian econometric formulations. The works of Modigliani and Friedman on consumption are good examples of this line of work, a line whose econometric implications have been extended in important work by Robert Merton. The works of Tobin and Baumol on portfolio balance and of Jorgenson on investment are also in the tradition of applying optimizing microeconomic theories for generating macroeconomic behavior relations. In the last thirty years, Keynesian econometric models have to a large extent developed along the line of trying to model agents' behavior as stemming from more and more sophisticated optimum problems. Our point here is certainly *not* to assert that Keynesian economists have completely foregone any use of optimizing microeconomic theory as a guide. Rather, it is that, especially when explicitly stochastic and dynamic problems have been studied, it has become increasingly apparent that microeconomic theory has very damaging implications for the restrictions conventionally used to identify Keynesian macroeconometric models. Furthermore, as Tobin [37] emphasized long ago, there is a point beyond which Keynesian models must suspend the hypothesis either of cleared markets or of optimizing agents if they are to possess the operating characteristics and policy implications that are the hallmarks of Keynesian economics.

To take one example that has extremely dire implications for the identification of existing macro models, expectations about the future prices, tax rates, and income levels play a critical role in many demand and supply schedules in those models. For example, in the best models, investment demand typically is supposed to respond to businessmen's expectations of future tax credits, tax rates, and factor costs. The supply of labor typically is supposed to depend on the rate of inflation that workers expect in the future. Such structural equations are usually identified by the assumption that, for example, the expectation about the factor price or rate of inflation attributed to agents is a function *only* of a few lagged values of the variable itself which the agent is supposed to be forecasting. However, the macro models themselves contain complicated dynamic interactions among endogenous variables, including factor prices and the rate of inflation, and generally imply that a wise agent would use current and many lagged values of many and usually most endogenous and exogenous variables in the model in order to form expectations about any one variable. Thus, virtually any version of the hypothesis that agents behave in their own interests will contradict the identification restrictions imposed on expectations formation. Further, the restrictions on expectations that have been used to achieve identification are entirely arbitrary and have not been derived from any deeper assumption reflecting first principles about economic behavior. No general first principle has ever been set down which would imply that, say, the expected rate of inflation should be modeled as a linear function of lagged rates of inflation alone with weights that add up to unity, yet this hypothesis is used as an identifying restriction in almost all existing models. The casual treatment of expectations is not a peripheral problem in these models, for the role of expectations is pervasive in the models and exerts a massive influence on their dynamic properties (a point Keynes himself insisted on). The failure of existing models to derive restrictions on expectations from any first principles grounded in economic theory is a symptom of a somewhat deeper and more general failure to derive behavioral relationships from any consistently posed dynamic optimization problems.

As for the second category, restrictions of type (b), existing Keynesian macro models make severe *a priori* restrictions on the R_j's. Typically, the R_j's are supposed to be diagonal so that cross equation lagged serial correlation is ignored and also the order of the ϵ_t process is assumed to be short so that only low-order serial correlation is allowed. There are at present no theoretical grounds for introducing these restrictions, and for good reasons there is little prospect that economic theory will soon provide any such grounds. In principle, identification can be achieved without imposing any such restrictions. Foregoing the use of category (b) restrictions would increase the category (a) and (c) restrictions needed. In any event, existing macro models do heavily restrict the R's.

Turning to the third category, all existing large models adopt an *a priori* classification of variables into the categories of strictly endogenous variables, the y_t's, and strictly exogenous variables, the x_t's. Increasingly, it is being recognized that the classification of a variable as "exogenous" on the basis of the observation that it *could* be set without reference to the current and past values of other variables has nothing to do with the econometrically relevant question of how this variable has *in fact* been related to others over a given historical period. Moreover, in light of recent developments in time series econometrics, we know that this arbitrary classification procedure is not necessary. Christopher Sims [34] has shown that in a time series context the hypothesis of econometric exogeneity can be tested. That is, Sims showed that the hypothesis that x_t is strictly econometrically exogenous in (1) necessarily implies certain restrictions that can be tested given time series on the y's and x's. Tests along the lines of Sims's ought to be used as a matter of course in checking out categorizations into exogenous and endogenous sets of variables. To date they have not been. Prominent builders of large econometric models have even denied the usefulness of such tests.[7]

4. Failure of Keynesian Macroeconometrics

Our discussion in the preceding section raised a number of theoretical reasons for believing that the parameters identified as structural by the methods which are in current use in macroeconomics are not structural in fact. That is, there is no reason, in our opinion, to believe that these models have isolated structures which will remain invariant across the class of interventions that figure in contemporary discussions of economic policy. Yet the question of whether a particular model is structural is an empirical, not a theoretical, one. If the macroeconometric models had compiled a record of parameter stability, particularly in the face of breaks in the stochastic behavior of the exogenous variables and disturbances, one would be skeptical as to the importance of prior theoretical objections of the sort we have raised.

In fact, however, the track record of the major econometric models is, on any dimension other than very short-term unconditional forecasting, very poor. Formal statistical tests for parameter instability, conducted by subdividing past series into periods and checking for parameter stability across time, invariably reveal major shifts (for one example, see [23]). Moreover, this difficulty is implicitly acknowledged by model-builders themselves, who routinely employ an elaborate system of add-factors in forecasting, in an attempt to offset the continuing "drift" of the model away from the actual series.

Though not, of course, designed as such by anyone, macroeconometric models were subjected in the 1970s to a decisive test. A key element in all Keynesian models is a "tradeoff" between inflation and real output: the higher is the inflation rate, the higher is output (or equivalently, the lower is the rate of unemployment). For example, the models of the late 1960s predicted a sustained unemployment rate in the United States of 4 percent as consistent with a 4 percent annual rate of inflation. Many economists at that time urged a deliberate policy of inflation on the basis of this prediction. Certainly the erratic "fits and starts" character of actual U.S. policy in the 1970s cannot be

[7] For example, see the comment by Albert Ando [35, especially pp. 209-210], and the remarks of L. R. Klein [24].

attributed to recommendations based on Keynesian models, but the inflationary bias *on average* of monetary and fiscal policy in this period should, according to all of these models, have produced the lowest average unemployment rates for any decade since the 1940s. In fact, as we know, they produced the highest unemployment since the 1930s. This was econometric failure on a grand scale.

This failure has not led to widespread conversions of Keynesian economists to other faiths, nor should it have been expected to. In economics, as in other sciences, a theoretical framework is always broader and more flexible than any particular set of equations, and there is always the hope that, if a particular specific model fails, one can find a more successful one based on "roughly" the same ideas. It has, however, already had some important consequences, with serious implications both for economic policy-making and for the practice of economic science.

For policy, the central fact is that Keynesian policy recommendations have no sounder basis, in a scientific sense, than recommendations of non-Keynesian economists or, for that matter, noneconomists. To note one consequence of the wide recognition of this, the current wave of protectionist sentiment directed at "saving jobs" would have been answered, ten years ago, with the Keynesian counter-argument that fiscal policy can achieve the same end, but more efficiently. Today, of course, no one would take this response seriously, so it is not offered. Indeed, economists who ten years ago championed Keynesian fiscal policy as an *alternative* to inefficient direct controls increasingly favor the latter as "supplements" to Keynesian policy. The idea seems to be that if people refuse to obey the equations we have fit to their past behavior, we can pass laws to *make* them do so.

Scientifically, the Keynesian failure of the 1970s has resulted in a new openness. Fewer and fewer economists are involved in monitoring and refining the major econometric models; more and more are developing alternative theories of the business cycle, based on different theoretical principles. In addition, increased attention and respect are accorded to the theoretical casualties of the Keynesian Revolution, to the ideas of Keynes's contemporaries and of earlier economists whose thinking has been regarded for years as outmoded.

At the present time, it is impossible to foresee where these developments will lead. Some, of course, continue to believe that the problems of existing Keynesian models can be resolved within the existing framework, that these models can be adequately refined by changing a few structural equations, by adding or subtracting a few variables here and there, or perhaps by disaggregating various blocks of equations. We have couched our preceding criticisms in such general terms precisely to emphasize their generic character and hence the futility of pursuing minor variations within this general framework.

A second response to the failure of Keynesian analytical methods is to renounce analytical methods entirely, returning to "judgmental" methods. The first of these responses identifies the quantitative, scientific goals of the Keynesian Revolution with the details of the particular models so far developed. The second renounces both these models and the objectives they were designed to attain. There is, we believe, an intermediate course, to which we now turn.

5. Equilibrium Business Cycle Theory

Economists prior to the 1930s did not recognize a need for a special branch of economics, with its own special postulates, designed to explain the business cycle. Keynes founded that subdiscipline, called *macroeconomics,* because he thought that it was impossible to explain the characteristics of business cycles within the discipline imposed by classical economic theory, a discipline imposed by its insistence on adherence to the two postulates (a) that markets be assumed to clear, and (b) that agents be assumed to act in their own self-interest. The outstanding fact that seemed impossible to reconcile with these two postulates was the length and severity of business depressions and the large scale unemployment which they entailed. A related observation is that measures of aggregate demand and prices are positively correlated with measures of real output and employment, in apparent contradiction to the classical result that changes in a purely nominal magnitude like the general price level were pure "unit changes" which should not alter real behavior. After freeing himself of the straight-jacket (or discipline) imposed by the classical postulates, Keynes described a model in which rules of thumb, such as the consumption function and liquidity preference schedule, took the place of decision functions that a classical economist would insist be derived from the theory of choice. And rather than require that wages and prices be determined by the postulate that markets clear—which for the labor market seemed patently contradicted by the severity of business depressions—Keynes took as an unexamined postulate that money wages are "sticky," meaning that they are set at a level or by a process that could be taken as uninfluenced by the macroeconomic forces he proposed to analyze.

When Keynes wrote, the terms "equilibrium" and "classical" carried certain positive and normative connotations which seemed to rule out either modifier being applied to business cycle theory. The term "equilibrium" was thought to refer to a system "at rest," and both "equilibrium" and "classical" were used interchangeably, by some, with "ideal." Thus an economy in classical equilibrium would be both unchanging and unimprovable by policy interventions. Using terms in this way, it is no wonder that few economists regarded equilibrium theory as a promising starting point for the understanding of business cycles, and for the design of policies to mitigate or eliminate them.

In recent years, the meaning of the term "equilibrium" has undergone such dramatic development that a theorist of the 1930s would not recognize it. It is now routine to describe an economy following a multivariate stochastic process as being "in equilibrium," by which is meant nothing more than that at each point in time, postulates (a) and (b) above are satisfied. This development, which stemmed mainly from work by K. J. Arrow [2] and G. Debreu [6], implies that simply to look at any economic time series and conclude that it is a "disequilibrium phenomenon" is a meaningless observation. Indeed, a more likely conjecture, on the basis of recent work by Hugo Sonnenschein [36], is that the general hypothesis that a collection of time series describes an economy in competitive equilibrium is *without content.*[8]

The research line being pursued by a number of us involves the attempt to discover a particular, econometrically testable equilibrium theory of the business cycle, one that can serve as the foundation for quantitative analysis of macroeconomic policy. There is no denying that this approach is "counter-revolutionary," for it presupposes that Keynes and his followers were wrong to give up on the possibility that an equilibrium theory could account for the business cycle. As of now, no successful equilibrium macroeconometric model at the level of detail of, say, the FMP model, has been constructed. But small theoretical equilibrium models have been constructed that show potential for explaining some key features of the business cycle long thought to be inexplicable within the confines of classical postulates. The equilibrium models also provide reasons for understanding why estimated Keynesian models fail to hold up outside of the sample over which they have been estimated. We now turn to describing some of the key facts about business cycles and the way the new classical models confront them.

For a long time most of the economics profession has, with some reason, followed Keynes in rejecting classical macroeconomic models because they seemed incapable of explaining some important characteristics of time series measuring important economic aggregates. Perhaps the most important failure of the classical model seemed to be its inability to explain the positive correlation in the time series between prices and/or wages, on the one hand, and measures of aggregate output or employment, on the other hand. A second and related failure was its inability to explain the positive correlations between measures of aggregate demand, like the money stock, and aggregate output or employment. Static analysis of classical macroeconomic models typically implied that the levels of output and employment were determined independently of both the absolute level of prices and of aggregate demand. The pervasive presence of the above mentioned positive correlations in the time series seems consistent with causal connections flowing from aggregate demand and inflation to output and employment, contrary to the classical "neutrality" propositions. Keynesian macroeconometric models do imply such causal connections.

[8] For an example that illustrates the emptiness at a general level of the statement that "employers are always operating along dynamic stochastic demands for factors," see the remarks on econometric identification in Sargent [29]. In applied problems that involve modeling agents' optimum decision rules, one is impressed at how generalizing the specification of agents' objective functions in plausible ways quickly leads to econometric under-identification. A somewhat different class of examples is seen in the difficulties in using time series observations to refute the view that "agents only respond to unexpected changes in the money supply." A distinguishing feature of the equilibrium macroeconometric models described below is that predictable changes in the money supply do not affect real GNP or total employment. In Keynesian models, predictable changes in the money supply do cause real GNP and employment to move. At a general level, it is impossible to discriminate between these two views by observing time series drawn from an economy described by a stationary vector random process (Sargent [28]).

We now have rigorous theoretical models which illustrate how these correlations can emerge while retaining the classical postulates that markets clear and agents optimize.[9] The key step in obtaining such models has been to relax the ancillary postulate used in much classical economic analysis that agents have perfect information. The new classical models continue to assume that markets always clear and that agents optimize. The postulate that agents optimize means that their supply and demand decisions must be functions of real variables, including perceived relative prices. Each agent is assumed to have limited information and to receive information about some prices more often than other prices. On the basis of their limited information–the lists that they have of current and past absolute prices of various goods–agents are assumed to make the best possible estimate of all of the *relative* prices that influence their supply and demand decisions. Because they do not have all of the information that would enable them to compute perfectly the relative prices they care about, agents make errors in estimating the pertinent relative prices, errors that are unavoidable given their limited information. In particular, under certain conditions, agents will tend temporarily to mistake a general increase in all absolute prices as an increase in the *relative* price of the good that they are selling, leading them to increase their supply of that good over what they had previously planned. Since everyone is, on average, making the same mistake, aggregate output will rise above what it would have been. This increase of output will rise above what it would have been. This increase of output above what it would have been will occur whenever this period's average economy-wide price level is above what agents had expected this period's average economy-wide price level to be on the basis of previous information. Symmetrically, average output will be decreased whenever the aggregate price turns out to be lower than agents had expected. The hypothesis of "rational expectations" is being imposed here because agents are supposed to make the best possible use of the limited information they have and are assumed to know the pertinent objective probability distributions. This hypothesis is imposed by way of adhering to the tenets of equilibrium theory.

In the preceding theory, disturbances to aggregate demand lead to a positive correlation between unexpected changes in the aggregate price level and revisions in aggregate output from its previously planned level. Further, it is an easy step to show that the theory implies correlations between revisions to aggregate output and unexpected changes in any variables that help determine aggregate demand. In most macroeconomic models, the money supply is one determinant of aggregate demand. The preceding theory easily can account for positive correlations between revisions to aggregate output and unexpected increases in the money supply.

While such a theory predicts positive correlations between the inflation rate or money supply, on the one hand, and the level of output on the other, it also asserts that those correlations do not depict "tradeoffs" that can be exploited by a policy authority. That is, the theory predicts that there is no way that the monetary authority can follow a systematic activist policy and achieve a rate of

[9] See Edmund S. Phelps *et al.* [25] and Lucas [15], [16].

output that is on average higher over the business cycle than what would occur if it simply adopted a no-feedback, X-percent rule of the kind Friedman [8] and Simons [32] recommended. For the theory predicts that aggregate output is a function of current and past unexpected changes in the money supply. Output will be high only when the money supply is and has been higher than it had been expected to be, i.e., higher than average. There is simply no way that on average over the whole business cycle the money supply can be higher than average. Thus, while the preceding theory is capable of explaining some of the correlations long thought to invalidate classical macroeconomic theory, the theory is classical both in its adherence to the classical theoretical postulates and in the "nonactivist" flavor of its implications for monetary policy.

Small-scale econometric models in the sense of Section 2 of this paper have been constructed which capture some of the main features of the equilibrium models described above.[10] In particular, these models incorporate the hypothesis that expectations are rational, or that all available information is utilized by agents. To a degree, these models achieve econometric identification by invoking restrictions in each of the three categories (a), (b), and (c). However, a distinguishing feature of these "classical" models is that they also heavily rely on an important fourth category of identifying restrictions. This category (d) consists of a set of restrictions that are derived from probabilistic economic theory, but play no role in the Keynesian framework. These restrictions in general do not take the form of zero restrictions of the type (a). Instead, the restrictions from theory typically take the form of *cross-equation* restrictions among the A_j, B_j, C_j parameters. The source of these restrictions is the implication from economic theory that current decisions depend on agents' forecasts of future variables, combined with the implication that these forecasts are formed optimally, given the behavior of past variables. These restrictions do not have as simple a mathematical expression as simply setting a number of parameters equal to zero, but their economic motivation is easy to understand. Ways of utilizing these restrictions in econometric estimation and testing are being rapidly developed.

Another key characteristic of recent work on equilibrium macroeconometric models is that the reliance on entirely *a priori* categorizations (c) of variables as strictly exogenous and endogenous has been markedly reduced, although not entirely eliminated. This development stems jointly from the fact

[10] For example, Sargent [27]. Dissatisfaction with the Keynesian methods of achieving identification has also led to other lines of macroeconometric work. One line is the "index models" described by Sargent and Sims [31] and Geweke [10]. These models amount to a statistically precise way of implementing Wesley Mitchell's notion that there is a small number of common influences that explain the covariation of a large number of economic aggregates over the business cycle. This "low dimensionality" hypothesis is a potential device for restricting the number of parameters to be estimated in vector time series models. This line of work is *not* entirely a-theoretical (but see the comments of Ando and Klein in Sims [35]), though it is distinctly unKeynesian. As it happens, certain equilibrium models of the business cycle do seem to lead to low dimensional index models with an interesting pattern of variables' loadings on indexes. In general, modern Keynesian models do not so easily assume a low-index form. See the discussion in Sargent and Sims [31].

that the models assign important roles to agents' optimal forecasts of future variables, and from Christopher Sims's demonstration that there is a close connection between the concept of strict econometric exogeneity and the forms of the optimal predictors for a vector of time series. Building a model with rational expectations necessarily forces one to consider which set of other variables helps forecast a given variable, say income or the inflation rate. If variable y helps predict variable x, then Sims's theorems imply that x cannot be regarded as exogenous with respect to y. The result of this connection between predictability and exogeneity has been that in equilibrium macroeconometric models the distinction between endogenous and exogenous variables has not been drawn on an entirely *a priori* basis. Furthermore, special cases of the theoretical models, which often involve side restrictions on the R_j's not themselves drawn from economic theory, have strong *testable* predictions as to exogeneity relations among variables.

A key characteristic of equilibrium macroeconometric models is that as a result of the restrictions across the A_j, B_j, and C_j's, the models predict that in general the parameters in *many* of the equations will change if there is a policy intervention that takes the form of a change in one equation that describes how some policy variable is being set. Since they ignore these cross-equation restrictions, Keynesian models in general assume that all other equations remain unchanged when an equation describing a policy variable is changed. Our view is that this is one important reason that Keynesian models have broken down when there have occurred important changes in the equations governing policy variables or exogenous variables. Our hope is that the methods we have described will give us the capability to predict the consequences for all of the equations of changes in the rules governing policy variables. Having that capability is necessary before we can claim to have a scientific basis for making quantitative statements about macroeconomic policy.

At the present time, these new theoretical and econometric developments have not been fully integrated, although it is clear they are very close, both conceptually and operationally. Our preference would be to regard the best currently existing equilibrium models as prototypes of better, future models which will, we hope, prove of practical use in the formulation of policy. But we should not understate the econometric success already attained by equilibrium models. Early versions of these models have been estimated and subjected to some stringent econometric tests by McCallum [20], Barro [3], [4], and Sargent [27], with the result that they do seem capable of explaining some broad features of the business cycle. New and more sophisticated models involving more complicated cross-equation restrictions are in the works (Sargent [29]). Work to date has already shown that equilibrium models are capable of attaining within-sample fits about as good as those obtained by Keynesian models, thereby making concrete the point that the good fits of the Keynesian models provide no good reason for trusting policy recommendations derived from them.

6. Criticism of Equilibrium Theory

The central idea of the equilibrium explanations of business cycles as sketched above is that economic fluctuations arise as agents react to *unanticipated* changes in variables which impinge on their decisions. It is clear that *any* explanation of this general type must carry with it severe limitations on the ability of governmental policy to *offset* these initiating changes. First, governments must somehow have the ability to foresee shocks which are invisible to private agents but at the same time lack the ability to reveal this advance information (hence defusing the shocks). Though it is not difficult to write down theoretical models in which these two conditions are assumed to hold, it is difficult to imagine actual situations in which such models would apply. Second, the governmental countercyclical policy must *itself* be unanticipatable by private agents (certainly a frequently realized condition historically) while at the same time be systematically related to the state of the economy. Effectiveness then rests on the inability of private agents to recognize systematic patterns in monetary and fiscal policy.

To a large extent, criticism of equilibrium models is simply a reaction to these implications for policy. So wide is (or was) the consensus that *the* task of macroeconomics is the discovery of the particular monetary and fiscal policies which can eliminate fluctuations by reacting to private sector instability that the assertion that this task either should not, or cannot be performed is regarded as frivolous independently of whatever reasoning and evidence may support it. Certainly one must have some sympathy with this reaction: an unfounded faith in the curability of a particular ill has served often enough as a stimulus to the finding of genuine cures. Yet to confuse a possibly functional *faith* in the existence of efficacious, re-active monetary and fiscal policies with scientific evidence that such policies are known is clearly dangerous, and to use such faith as a criterion for judging the extent to which particular theories "fit the facts" is worse still.

There are, of course, legitimate issues involving the ability of equilibrium theories to fit the facts of the business cycle. Indeed, this is the reason for our insistence on the preliminary and tentative character of the particular models we now have. Yet these tentative models share certain features which can be regarded as *essential,* so it is not unreasonable to speculate as to the likelihood that *any* model of this type can be successful, or to ask: what will equilibrium business cycle theorists have in ten years if we get lucky?

Four general reasons for pessimism which have been prominently advanced are (a) the fact that equilibrium models postulate cleared markets, (b) the assertion that these models cannot account for "persistence" (serial correlation) of cyclical movements, (c) the fact that econometrically implemented models are linear (in logarithms), and (d) the fact that learning behavior has not been incorporated. We discuss each in turn in distinct subsections.

6.1 Cleared Markets

One essential feature of equilibrium models is that all markets clear, or that all observed prices and quantities be explicable as outcomes of decisions taken by individual firms and households. In practice, this has meant a conventional, competitive supply-equals-demand assumption, though other kinds of equilibrium can easily be imagined (if not so easily analyzed). If, therefore, one takes as a basic "fact" that labor markets do not clear one arrives immediately at a contradiction between theory and fact. The facts we actually have, however, are simply the available time series on employment and wage rates, plus the responses to our unemployment surveys. Cleared markets is simply a principle, not verifiable by direct observation, which may or may not be useful in constructing successful hypotheses about the behavior of these series. Alternative principles, such as the postulate of the existence of a third-party auctioneer inducing wage "rigidity" and noncleared markets, are similarly "unrealistic," in the not especially important sense of not offering a good description of observed labor market institutions.

A refinement of the unexplained postulate of an uncleared labor market has been suggested by the indisputable fact that there exist long-term labor contracts with horizons of two or three years. Yet the length *per se* over which contracts run does not bear on the issue, for we know from Arrow and Debreu that if infinitely long-term contracts are determined so that prices and wages are contingent on the same information that is available under the assumption of period-by-period market clearing, then precisely the same price-quantity process will result with the long-term contract as would occur under period-by-period market clearing. Thus equilibrium theorizing provides a way, probably the only way we have, to construct a *model* of a long-term contract. The fact that long-term contracts exist, then, has *no* implications about the applicability of equilibrium theorizing. Rather, the real issue here is whether actual contracts can be adequately accounted for within an equilibrium model, that is, a model in which agents are proceeding in their own best interests. Stanley Fischer [7], Edmund Phelps and John Taylor [26], and Robert Hall [12] have shown that some of the "nonactivist" conclusions of the equilibrium models are modified if one substitutes for period-by-period market clearing the imposition of long-term contracts drawn contingent on restricted information sets that are exogenously imposed and that are assumed to be independent of monetary and fiscal regimes. Economic theory leads us to predict that costs of collecting and processing information will make it optimal for contracts to be made contingent on a small subset of the information that could possibly be collected at any date. But theory also suggests that the particular set of information upon which contracts will be made contingent is not immutable but depends on the structure of costs and benefits to collecting various kinds of information. This structure of costs and benefits will change with every change in the exogenous stochastic processes facing agents. This theoretical presumption is supported by an examination of the way labor contracts differ across high-inflation and low-inflation countries and the way they have evolved in the United States over the last 25 years.

So the issue here is really the same fundamental one involved in the dispute between Keynes and the classical economists: Is it adequate to regard certain superficial characteristics of existing wage contracts as given when analyzing the consequences of alternative monetary and fiscal regimes? Classical economic theory denies that those characteristics can be taken as given. To understand the implications of long-term contracts for monetary policy, one needs a model of the way those contracts are likely to respond to alternative monetary policy regimes. An extension of existing equilibrium models in this direction might well lead to interesting variations, but it seems to us unlikely that major modifications of the implications of these models for monetary and fiscal policy will follow from this.

6.2 *Persistence*

A second line of criticism stems from the correct observation that if agents' expectations are rational and if their information sets include lagged values of the variable being forecast, then agents' forecast errors must be a serially uncorrelated random process. That is, on average there must be no detectable relationships between this period's forecast error and any previous period's forecast error. This feature has led several critics to conclude that equilibrium models are incapable of accounting for more than an insignificant part of the highly serially correlated movements we observe in real output, employment, unemployment and other series. Tobin has put the argument succinctly in [38]:

> One currently popular explanation of variations in employment is temporary confusion of relative and absolute prices. Employers and workers are fooled into too many jobs by unexpected inflation, but only until they learn it affects other prices, not just the prices of what they sell. The reverse happens temporarily when inflation falls short of expectation. This model can scarcely explain more than transient disequilibrium in labor markets.
>
> So how can the faithful explain the slow cycles of unemployment we actually observe? Only by arguing that the natural rate itself fluctuates, that variations in unemployment rates are substantially changes in voluntary, frictional, or structural unemployment rather than in involuntary joblessness due to generally deficient demand.

The critics typically conclude that the theory only attributes a very minor role to aggregate demand fluctuations and necessarily depends on disturbances to aggregate supply to account for most of the fluctuations in real output over the business cycle. As Modigliani [21] characterized the implications of the theory: "In other words, what happened to the United States in the 1930s was a severe attack of contagious laziness."

This criticism is fallacious because it fails to distinguish properly between "sources of impulses" and "propagation mechanisms," a distinction stressed by Ragnar Frisch in a classic 1933 paper [9] that provided many of the technical foundations for Keynesian macroeconometric models. Even though the new classical theory implies that the forecast errors which are the aggregate demand "impulses" are serially uncorrelated, it is certainly logically possible that "propagation mechanisms" are at work that convert these impulses into serially correlated movements in real variables like output and employment. Indeed, two concrete propagation mechanisms have already been shown in detailed theoretical work to be capable of performing precisely that function. One mechanism stems from the presence of costs to firms of adjusting their stocks of capital and labor rapidly. The presence of these costs is known to make it optimal for firms to spread out over time their response to the relative price signals that they receive. In the present context, such a mechanism causes a firm to convert the serially uncorrelated forecast errors in predicting relative prices into serially correlated movements in factor demands and in output.

A second propagation mechanism is already present in the most classical of economic growth models. It is known that households' optimal accumulation plans for claims on physical capital and other assets will convert serially uncorrelated impulses into serially correlated demands for the accumulation of real assets. This happens because agents typically will want to divide any unexpected changes in the prices or income facing agents. This dependence assets. Thus, the demand for assets next period depends on initial stocks and on unexpected changes in the prices or income-facing agents. This dependence makes serially uncorrelated surprises lead to serially correlated movements in demands for physical assets. Lucas [16] showed how this propagation mechanism readily accepts errors in forecasting aggregate demand as an "impulse" source.

A third likely propagation mechanism is identified by recent work in search theory.[11] Search theory provides an explanation for why workers who for some reason find themselves without jobs will find it rational not necessarily to take the first job offer that comes along but instead to remain unemployed for some period until a better offer materializes. Similarly, the theory provides reasons that a firm may find it optimal to wait until a more suitable job applicant appears so that vacancies will persist for some time. Unlike the first two propagation mechanisms mentioned, consistent theoretical models that permit that mechanism to accept errors in forecasting aggregate demand as an impulse have not yet been worked out for mainly technical reasons, but it seems likely that this mechanism will eventually play an important role in a successful model of the time series behavior of the unemployment rate.

In models where agents have imperfect information, either of the first two and most probably the third mechanism is capable of making serially correlated movements in real variables stem from the introduction of a serially uncorrelated sequence of forecasting errors. Thus, theoretical and econometric models have been constructed in which in principle the serially uncorrelated process of forecasting errors is capable of accounting for any proportion between zero and one of the steady-state variance of real output or employment. The argument

[11] For example [19], [22] and [18].

that such models must necessarily attribute most of the variance in real output and employment to variations in aggregate supply is simply wrong logically.

6.3 *Linearity*

Most of the econometric work implementing equilibrium models has involved fitting statistical models that are linear in the variables (but often highly nonlinear in the parameters). This feature is subject to criticism on the basis of the indisputable principle that there generally exist nonlinear models that provide better approximations than linear models. More specifically, models that are linear in the variables provide no method of detecting and analyzing systematic effects of higher than first-order moments of the shocks and the exogenous variables on the first moments of the endogenous variables. Such systematic effects are generally present where the endogenous variables are set by risk-averse agents.

There is no *theoretical* reason that most applied work has used linear models, only compelling technical reasons given today's computer technology. The predominant technical requirement of econometric work which imposes rational expectations is the ability to write down analytical expressions giving agents' decision rules as functions of the parameters of their objective functions and as functions of the parameters governing the exogenous random processes that they face. Dynamic stochastic maximum problems with quadratic objectives, which give rise to linear decision rules, *do* meet this essential requirement, which is their virtue. Only a few other functional forms for agents' objective functions in dynamic stochastic optimum problems have this same necessary analytical tractability. Computer technology in the foreseeable future seems to require working with such a class of functions, and the class of linear decision rules has just seemed most convenient for most purposes. No issue of *principle* is involved in selecting one out of the very restricted class of functions available to us. *Theoretically,* we know how to calculate via expensive recursive methods the nonlinear decision rules that would stem from a very wide class of objective functions; no new econometric principles would be involved in estimating their parameters, only a much higher computer bill. Further, as Frisch and Slutsky emphasized, linear stochastic difference equations seem a very flexible device for studying business cycles. It is an open question whether for explaining the central features of the business cycle there will be a big reward to fitting nonlinear models.

6.4 *Stationary Models and the Neglect of Learning*

Benjamin Friedman and others have criticized rational expectations models apparently on the grounds that much theoretical and almost all empirical work has assumed that agents have been operating for a long time in a stochastically stationary environment. As a consequence, typically agents are assumed to have discovered the probability laws of the variables that they want to forecast. As Modigliani made the argument in [21]:

> At the logical level, Benjamin Friedman has called attention to the omission from [equilibrium macroeconomic models] of an explicit learning mechanism, and has suggested that, as a result, it can only be interpreted as a description not of short-run but of long-run equilibrium in which no agent would wish to recontract. But then the implications of [equilibrium macroeconomic models] are clearly far from startling, and their policy relevance is almost nil (p. 6)

But it has been only a matter of analytical convenience and not of necessity that equilibrium models have used the assumption of stochastically stationary "shocks" and the assumption that agents have already learned the probability distributions that they face. Both of these assumptions can be abandoned, albeit at a cost in terms of the simplicity of the model.[12] In fact, within the framework of quadratic objective functions, in which the "separation principle" applies, one can apply the "Kalman filtering formula" to derive optimum linear decision with time dependent coefficients. In this framework, the "Kalman filter" permits a neat application of Bayesian learning to updating optimal forecasting rules from period to period as new information becomes available. The Kalman filter also permits the derivation of optimum decision rules for an interesting class of nonstationary exogenous processes assumed to face agents. Equilibrium theorizing in this context thus readily leads to a *model* of how process nonstationarity and Bayesian learning applied by agents to the exogenous variables leads to time-dependent coefficients in agents' decision rules.

While models incorporating Bayesian learning and stochastic nonstationarity are both technically feasible and consistent with the equilibrium modeling strategy, almost no successful applied work along these lines has come to light. One reason is probably that nonstationary time series models are cumbersome and come in so many varieties. Another is that the hypothesis of Bayesian learning is vacuous until one either arbitrarily imputes a prior distribution to agents or develops a method of estimating parameters of the prior from time series data. Determining a prior distribution from the data would involve estimating a number of initial conditions and would proliferate nuisance parameters in a very unpleasant way. It is an empirical matter whether these techniques will pay off in terms of explaining macroeconomic time series; it is not a matter distinguishing equilibrium from Keynesian macroeconometric models. In fact, no existing Keynesian macroeconometric model incorporates either an economic model of learning or an economic model in any way restricting the pattern of coefficient nonstationarities across equations.

The macroeconometric models criticized by Friedman and Modigliani, which assume agents have "caught on" to the stationary random processes they face, give rise to systems of linear stochastic difference equations of the form (1), (2), and (4). As has been known for a long time, such stochastic difference equations generate series that "look like" economic time series. Further, if viewed as *structural* (i.e., invariant with respect to policy

[12] For example, see Crawford [5] and Grossman [11].

interventions) the models have some of the implications for countercyclical policy that we have described above. Whether or not these policy implications are correct depends on whether or not the models are structural and not at all on whether the models can successfully be caricatured by terms such as "long run" or "short run."

It is worth reemphasizing that we do not wish our responses to these criticisms to be mistaken for a claim that existing equilibrium models can satisfactorily account for all the main features of the observed business cycle. Rather, we have argued that no sound reasons have yet been advanced which even suggest that these models are, as a class, *incapable* of providing a satisfactory business cycle theory.

7. Summary and Conclusions

Let us attempt to set out in compact form the main arguments advanced in this paper. We will then comment briefly on the main implications of these arguments for the way we can usefully think about economic policy.

First, and most important, existing Keynesian macroeconometric models are incapable of providing reliable guidance in formulating monetary, fiscal and other types of policy. This conclusion is based in part on the spectacular recent failures of these models, and in part on their lack of a sound theoretical or econometric basis. Second, on the latter ground, there is no hope that minor or even major modification of these models will lead to significant improvement in their reliability.

Third, *equilibrium* models can be formulated which are free of these difficulties and which offer a different set of principles which can be used to identify structural econometric models. The key elements of these models are that agents are *rational,* reacting to policy changes in a manner which is in their best interests privately, and that the impulses which trigger business fluctuations are mainly unanticipated shocks.

Fourth, equilibrium models already developed account for the main qualitative features of the business cycle. These models are being subjected to continued criticism, especially by those engaged in developing them, but arguments to the effect that equilibrium theories are, in principle, incapable of accounting for a substantial part of observed fluctuations appear due mainly to simple misunderstandings.

The policy implications of equilibrium theories are sometimes caricatured, by friendly as well as unfriendly commentators, as the assertion that "economic policy does not matter" or "has no effect."[13] This implication would certainly startle neoclassical economists who have successfully applied equilibrium theory

[13] A main source of this belief is probably Sargent and Wallace [30], in which it was shown that in the context of a fairly standard macroeconomic model, but with agents' expectations assumed rational, the choice of a reactive monetary rule is of *no* consequence for the behavior of real variables. The point of this example was to show that within *precisely* that model used to rationalize reactive monetary policies, such policies could be shown to be of no value. It hardly follows that *all* policy is ineffective in *all* contexts.

to the study of innumerable problems involving important effects of fiscal policies on resource allocation and income distribution. Our intent is not to reject these accomplishments, but rather to try to *imitate* them, or to extend the equilibrium methods which have been applied to many economic problems to cover a phenomenon which has so far resisted their application: the business cycle.

Should this intellectual arbitrage prove successful, it will suggest important changes in the way we think about policy. Most fundamentally, it directs attention to the necessity of thinking of policy as the choice of stable "rules of the game," well understood by economic agents. Only in such a setting will economic theory help us to predict the actions agents will choose to take. Second, this approach suggests that policies which affect behavior mainly because their consequences cannot be correctly diagnosed, such as monetary instability and deficit financing, have the capacity only to disrupt. The deliberate provision of misinformation cannot be used in a systematic way to improve the economic environment.

The *objectives* of equilibrium business cycle theory are taken, without modification, from the goal which motivated the construction of the Keynesian macroeconometric models: to provide a scientifically based means of assessing, quantitatively, the likely effects of alternative economic policies. Without the econometric successes achieved by the Keynesian models, this goal would be simply inconceivable. Unless the now evident limits of these models are also frankly acknowledged, and radically different new directions taken, the real accomplishments of the Keynesian Revolution will be lost as surely as those we now know to be illusory.

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Discussion

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Professors Lucas and Sargent have done an admirable job of providing a paper that stimulates our thinking along several different lines, all central to the inflation-and-unemployment theme of this conference. Consequently there is much to which I could respond in my assigned role as discussant. For example, I could easily spend my allotted time applauding their path-breaking work on expectations and their progress to date in integrating this work into modern macroeconomics. Or I could concentrate entirely on the relationship of their work to that of the other economists whom their paper so harshly criticizes. Or I could focus on their interpretation of historical facts, or on their exegesis – both stated and implied – of the literature of macroeconomics. Their paper is indeed thought-provoking in a variety of directions. Given the limited available time, I will reluctantly leave their fine accomplishments on the expectations front to speak for themselves and will instead focus my discussion on what I interpret to be the principal message of their paper.

Professors Lucas and Sargent argue vigorously that a methodological divide separates their work from the existing corpus of modern macroeconomics. Specifically, they state that "the Keynesian Revolution was . . . a revolution in method . . ." and that " . . . if one does not view the revolution in this way, it is impossible to account for some of its most important features." They further state that equilibrium business cycle theory, for which their paper so eloquently argues, is essentially characterized by the adoption of a different methodological approach to macroeconomic research. According to Professors Lucas and Sargent, the central distinction between Keynesian macroeconomics on the one hand, and the work which they and their associates and followers pursue on the other, lies in the rejection by the one and the acceptance by the other of the "classical" postulates of market clearing and especially of optimizing behavior on the part of economic agents including businessmen, consumers, and so on. According to their description, the methodological essence – and therefore the fundamental feature – of the Keynesian revolution was the abandonment of the attempt to derive behavioral models from the assumption that people act as well as they can in their own self-interest, and in its place the systematic resort to " . . . a model in which rules of thumb . . . took the place of decision functions that a classical economist would insist be derived from the theory of choice." As examples of such ad hoc, arbitrary rules of thumb standing at the core of Keynesian macroeconomics, they cite the familiar consumption, investment and

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money demand functions – and, of course, Keynes's own assumption of a money wage rate determined outside the model. By contrast, the feature of the new equilibrium business cycle research that Professors Lucas and Sargent emphasize is that it eschews such resort to nonoptimizing behavior in favor of derived behavioral propositions, parameter restrictions only to the extent that the time series data validate them, and cross-equation restrictions derived especially from processes of dynamic optimization.

Let me say straight out that I cannot recognize this methodological distinction drawn by Professors Lucas and Sargent, except perhaps as some gross caricature out of date by more than a generation. Hence I believe that the central message of their paper does not stand up to careful appraisal. To explain why will require some brief comments on both the tradition which they derogate and that which they advance. I find that somewhat unfortunate, because I have no taste for shouldering responsibility for any broad-based defense of what could be regarded as "status quo" economics. (I am here reminded of Secretary of State Acheson's remarks in the matter of Mr. Edmund Clubb.) Instead, my purpose is merely to discuss critically the principal point argued by Professors Lucas and Sargent in their paper, the sharp methodological distinction posited between Keynesian macroeconomics and equilibrium business cycle theory.

Which of the two shall we address first? I prefer to begin with a quotation which may be familiar to some people here:

> The economic theory which underlies the construction of our model is classical in its methodology. We view the economic system as composed of two groups. One group consists of households and the other of business firms. It is assumed that the individuals in each group follow specific types of behavior patterns . . . For example, we assume that entrepreneurs behave so as to maximize profits, subject to the constraint that they operate according to the technological possibilities expressed by their production functions . . . we should not be misled by those economists who insist that entrepreneurs do not know the meaning of partial derivatives and hence do not behave so as to maximize profits or psychic income of some type . . . We assume further that households behave so as to maximize their satisfactions or utilities, subject to budgetary constraints; and in this way we obtain the equations of consumer demand.

No doubt, one supposes after reading Professors Lucas and Sargent, these must be the words of either a pre-Keynesian classical theorist or a modern proponent of equilibrium business cycle theory. Correct? No. The publication date was in fact 1950. In that case, no doubt the author must have been an anti-Keynesian dissident whom the mainstream of the Keynesian macroeconometric literature either rejected or simply passed by without notice. Correct? No, again. The author was in fact Lawrence Klein, and the source was his *Economic Fluctuations in the United States* – the single book that, more than any other, set the path for a generation of quantitative research on Keynesian macroeconometrics.

Since the identification of the quotation's source has now revealed what its substance did not – that is, that I have begun my discussion with Keynesian economics rather than equilibrium business cycle theory – let us next examine somewhat closer the ad hoc "rules of thumb" which Professors Lucas and Sargent cite among the basic building blocks of the modern Keynesian macroeconometric model. Does the large literature of the life-cycle model of consumer behavior, in which the crux of the decision is resource allocation over a lifetime, jibe with the description of the consumption function as an arbitrary rule of thumb not derived "... from any consistently posed dynamic optimization problems"? Does the proliferating literature of investment behavior call to mind something that "... took the place of decision functions that a classical economist would insist be derived from the theory of choice"? And what about portfolio behavior in general and the demand for money in particular – in fact perhaps the most obvious place to note the application of explicitly derived optimizing behavior including the use of cross-equation restrictions? Finally, as for Keynes's own use of the exogenous money wage assumption, I will not go into the many attempts (mostly unsuccessful) to explain wage-setting behavior either analytically or econometrically. Professors Lucas and Sargent have cogently argued that exogeneity is a statistical property subject to rigorous testing along the lines set out by C.W.J. Granger and Christopher Sims, and they advocate such tests as an essential first step in empirical model construction. It is therefore interesting to note in this context that the battery of Granger-Sims tests presented with Professor Sargent's well-known "Classical Macroeconometric Model for the United States" by and large suggested that the money wage rate was indeed exogenous with respect to the variables in the model (which, incidentally, the money stock was not) while itself having a causal influence on the unemployment rate and the interest rate.

I could proceed in this vein for some time, enumerating examples of the use, by economists *within* the existing macroeconomic tradition, of behavioral relationships explicitly grounded in optimizing behavior. I will not do so for two reasons. First, with limited time available it will be more interesting to focus directly on equilibrium business cycle research. And, second, I have already stated my unwillingness to assume the role of all-purpose defender of any status quo body of economics as it currently exists. Then, too, Ray Fair (from whose recent book I could have chosen a quotation just as apt as the one from Klein a quarter-century ago) will presumably provide examples of explicit optimizing behavior in his own work when he presents his paper tomorrow. I can summarize my discussion so far simply by saying that one-half of the methodological contrast asserted by Professors Lucas and Sargent – in particular, the absence of optimizing behavior in Keynesian macroeconomics – does not withstand close inspection. Equilibrium business cycle theory has no monopoly on optimizing behavior.

What about the other half of this supposed contrast in basic method? Is it true that equilibrium business cycle theory eschews arbitrary restrictions? Here, to argue that it does not, I will cite only two examples, one theoretical and one empirical. But I think these two examples go quite to the heart of the matter.

My theoretical example is the derivation of the aggregate supply function, originally posited by Professor Lucas, that provides the key to a form of the "natural rate" hypothesis consistent with a negative short-run correlation between unemployment and inflation. Professors Lucas and Sargent concisely summarize the argument in their paper:

> On the basis of their limited information – the lists that they have of current and past absolute prices of various goods – agents are assumed to make the best possible estimate of all of the *relative* prices that influence their supply and demand decisions. Because they do not have all of the information that would enable them to compute perfectly the relative prices they care about, agents make errors in estimating the pertinent relative prices . . . In particular, under certain conditions, agents will tend temporarily to mistake a general increase in all absolute prices as an increase in the relative price of the good that they are selling, leading them to increase their supply of that good over what they had previously planned . . . This increase of output above what it would have been will occur whenever this period's average economy-wide price level is above what agents had expected this period's average economy-wide price level to be on the basis of previous information. Symmetrically, output will be decreased whenever the aggregate price turns out to be lower than agents had expected.

The story sounds plausible enough. If a cobbler sees shoe prices rising and does not yet realize that leather prices (and all others) are rising in step, he will mistakenly perceive a relative price shift giving an advantage to producing more shoes. As a good optimizer he will accordingly increase production because of an imperfectly perceived rise in all prices.

But what if, instead, the cobbler first learns that the price of leather is rising and does not yet realize that the market will bear a higher price for his shoes? In this case he will mistakenly perceive a relative price shift giving a *dis*advantage to producing shoes. As a good optimizer he will now *de*crease production because of an imperfectly perceived rise in all prices.

The point of this illustration is that the crucial aggregate supply function on which equilibrium business cycle theory relies is valid if, and only if, agents learn the prices of goods they are *selling* before learning the prices of goods they are *buying*. If instead a producer typically learns the price he has to pay for his *inputs* before learning the price at which he can market his *output*, this aggregate supply function implies results exactly opposite to those which it is assumed to produce in equilibrium business cycle theory as described by Professors Lucas and Sargent.

I do not have evidence adequate to decide, for an economy with complicated market arrangements like those in the United States, what is on average the correct chronological order of price learning. The input-then-output ordering however, seems to me at least as plausible as the output-then-input ordering that Professors Lucas and Sargent require. In the absence of an outright assumption grounded only on the premise that it must be thus in order to fit the data – an assumption that would, if made by someone else, probably be called an ad hoc arbitrary restriction – how do they know that the output-then-input ordering is the right description of the imperfect information flow in the modern economy?

My second example is empirical. Professors Lucas and Sargent caution against any tendency to "... understate the degree of econometric success already attained ..." by equilibrium business cycle models, stating that "these models have been subjected to testing under standards more stringent than customarily applied to macroeconometric models ..." Of the three studies which they then cite (one is Professor Sargent's model, to which I have already referred), one is Robert Barro's well-known demonstration that unemployment in the United States is correlated only with the *un*anticipated component of money growth and not with the anticipated component. Since the Federal Reserve publishes no series entitled "unanticipated money growth," one naturally asks how this test proceeds. Before answering this question, however, it is instructive to recall some remarks of Professors Lucas and Sargent about Keynesian models:

> Such structural equations are usually identified by the assumption that, for example, the expectation about the factor prices or rate of inflation attributed to agents is a function *only* of a few lagged values of the variable itself which the agent is assumed to be forecasting ... the restrictions on expectations that have been used to achieve identification are entirely arbitrary and have not been derived from any deeper assumption reflecting first principles about economic behavior. No general first principle has ever been set down which would imply that, say, the expected rate of inflation should be modeled as a linear function of lagged rates of inflation alone with weights that add up to unity ...

How, then, did this test, supposedly under more stringent than customary standards, proceed? In fact, the "anticipated money growth" series was simply a two-period lag on past money growth, plus an allowance for Federal expenditures and the unemployment rate. Not surprisingly, this rather crude "anticipated money growth" series accounted for only a part of the variance of actual money growth during the sample period, leaving much of the actual variance – as well as the covariance with unemployment – for the residually determined "unanticipated money growth" series.

Did this procedure – that would, if used by someone else, probably be called an ad hoc arbitrary restriction – make a difference for the outcome of the test? Yes, it did. David Small has shown that allowing agents' anticipations of money growth to rely on a less restrictive view of how Federal expenditures influence money growth, especially during wars, produces an "anticipated money growth" series that accounts for much more of the variance of actual money growth – and with it the covariance with unemployment.

As promised, I will now stop this line of argument after but those two important examples. I can summarize this part of my discussion by saying that the second half of the methodological contrast asserted by Professors Lucas and Sargent – in particular, the lack of arbitrary restrictions in equilibrium business cycle models – does not stand up either. Keynesian macroeconomics has no monopoly on ad hoc restrictions.

Finally, what can we say about equilibrium business cycle theory on its own merits, apart from the question of a methodological divide or lack thereof between it and Keynesian macroeconomics? I have argued before that such theories have an essentially long-run character – that is, that they use a form of what I call "asymptotic reasoning" to deal with questions that many people pose, and some economists attempt to answer, within a shorter time frame. In response to a question about whether or not to implement a particular monetary policy to combat today's problems, for example, the familiar refrain notes that if we always alter money growth in response to economic conditions, optimizing agents will discover that fact and act accordingly. Indeed, Professors Lucas and Sargent explicitly state in their paper that equilibrium business cycle theory ". . . directs attention to the necessity of thinking of policy as the choice of stable 'rules of the game,' well understood by economic agents." I think that that is my point too. *Over the long run,* there is no coherent way of describing a policy that consists of a set of unrelated single actions. But in many circumstances people do want to be able to discuss whether, for example, a $20 billion tax cut in 1978 is helpful or harmful – not whether it would be wise or foolish to enact a rule calling for a tax cut of similar proportion at the corresponding point of all future business expansions. Already in 1978 businessmen, workers, and consumers (economic agents, if we must call them that) are forming expectations and taking actions accordingly. To argue that repeated tax cuts would over time come to alter their expectations is to apply asymptotic reasoning to a different kind of problem. (It is true, of course, that one should always keep in mind the future consequences of his current actions; but the points at issue here are, I believe, more fundamental than the mere assertion that the political process applies too high an interest rate in discounting the future.)

In the work to which Professors Lucas and Sargent refer in their paper, I argued on the basis of information requirements that the conclusions about the impotence of monetary policy, from what they now call equilibrium business cycle theory, were really *long-run* conclusions and hence not very surprising, since most economists accept them and most macroeconometric models embody them as descriptions of long-run equilibrium. After reading their new paper, I see yet further reasons why one should regard these models as having a fundamentally long-run orientation. The primary example from the paper is the question of institutional wage- and price-setting arrangements. When they first evolved, these models simply assumed the existence of flexible wages and prices. The next cut added some realism by noting the undeniable existence of long-term wage contracts. More recently researchers in this vein have acknowledged widespread "stickiness," both explicit in formal contracts and also implicit in less formal understandings, of wages as well as prices. In their paper, however, Professors Lucas and Sargent reply by noting that even these institutional arrangements have to be determined somehow and that they should be considered not exogenous but endogenous to the model. While I sympathize entirely with this approach, I again ask what is the time frame of a model that fully endogenizes the determination of such institutional arrangements. To cite only one example from an area familiar to most of the nonacademic participants at this conference, well-developed financial markets are often noted as a field of business in which innovation is, by comparison with the rest of the economy, relatively inexpensive and therefore rapid. Nevertheless, despite more than a decade of rapid and variable price inflation in the United States, our financial markets have yet to produce an instrument with which the investor willing to pay for it can buy protection of his purchasing power.

I especially applaud, although with some feeling of irony, the explicit recognition by Professors Lucas and Sargent of the role of the constraints subject to which equilibrium business cycle theory assumes that people optimize. I note some irony here because, heretofore, those of us who have emphasized the implications of transactions costs and have constructed arguments crucially depending on slow adjustments have often met with the automatic (though unwarranted) criticism of denying optimizing behavior. The presumption, of course, was that behavioral relations more explicitly derived from simpler models were necessarily better than behavioral relations less explicitly derived from more complicated models; and realistic models of dynamic adjustment in the presence of transactions costs can be very complicated indeed. Perhaps, now that Professors Lucas and Sargent have turned to costs of adjustment as the route to explaining the "persistence" of unemployment using equilibrium business cycle theory, there may be opportunities for more constructive interchange here.

As equilibrium business cycle theory comes to rely more heavily on such adjustment costs, however, I hope that it will be possible for it to assume a testable – that is, a potentially falsifiable – form, rather than degenerate into a mere semantic distinction. In practice, it is often extremely difficult to distinguish a theory which asserts that markets always clear but that adjustment costs temporarily (and how long is that?) make people's demands and supplies different from what they will be later on, from an alternative theory which asserts that because of adjustment costs markets temporarily do not clear. In my own work on price and yield determination in financial asset markets, for example, I have always used the former verbiage, and I think that that is what Professors Lucas and Sargent have in mind too; many other people, however, choose to interpret this work as equivalent to positing nonclearing markets. No one knows, of course, whether this new emphasis on adjustment costs will produce better business cycle models, or whether the best route lies instead in some other approach, but I for one can certainly wish them all good luck in the effort.

In conclusion, therefore, I think that there is much to applaud in the work that Professors Lucas and Sargent are doing, and that it is not so far removed from what others of us do as they suggest. Indeed, if their paper had simply said that the inadequate treatment of expectations constitutes a major weakness in modern macroeconomics, and that they had already made significant progress on this point and were continuing to pursue it, my own discussion would have been altogether different from what I have said. In fact, however, the main argument of their paper is that their work marks a fundamental methodological departure from the corpus of Keynesian macroeconomics, and here I have been forced to disagree sharply. Equilibrium business cycle theory has no monopoly on optimizing behavior, and Keynesian macroeconomics has none on ad hoc arbitrary restrictions.

The same problem arises in interpreting the recent empirical evidence. A reader of Professors Lucas and Sargent who had not independently been exposed to the data would probably be surprised to learn that in the United States, which has pursued one kind of macroeconomic policy, the unemployment rate has fallen from 9 percent at the recession's trough three years ago to 6 percent today (which many economists argue is almost full employment), while throughout Europe, where fiscal policies especially have been starkly different, unemployment has not fallen at all. (Furthermore, such a reader would probably be surprised, too, to learn that in the United States the primary macroeconomic problem is now accelerating inflation, while in Europe inflation rates have decelerated markedly and continue to do so.) I will not pursue these casual observations, especially since Stephen McNees' paper has already presented the relevant evidence in substantial detail. Whether that evidence strikes our hypothetical reader as showing that, in the words of Professors Lucas and Sargent, macroeconometric models' predictions have been "wildly incorrect," and whether he would recognize in it "the spectacular failure of the Keynesian models in the 1970s" and the associated "econometric failure on a grand scale," I leave to others to decide. Nevertheless, here as well as with respect to the premises on method that comprise the central focus of their paper, a lower rhetorical profile would better advance the cause of scientific interchange.

Response to Friedman†

Robert E. Lucas and Thomas J. Sargent

Our understanding of the purpose of the Conference was to discuss certain outstanding issues in macroeconomics in the hope of increasing general understanding of the potential role of economic theory in improving public policy. Since both of us are on record as rather severe critics of Keynesian macroeconometric models, we assumed that we were included in the program to express this dissenting view as forcefully and as accurately as possible. This we attempted to do, using both plain English and the technical language of econometrics and economic theory as best we could.

Benjamin Friedman's comments provide clear testimony to the complete failure of our efforts to engage in substantive discussion of the reliability of current macroeconomic models. Most of his comments are devoted to a defense of the proposition that: "Equilibrium business cycle theory has no monopoly on optimizing behavior, and Keynesian macroeconomics has none on ad hoc arbitrary restrictions." Friedman makes no effort to explain either how this proposition is related to anything in our paper (it is not) or what possible bearing it might have on the questions of economic policy which we thought were under discussion.

Professor Friedman also expressed skepticism on some details of our recent research, as well as on some valuable related work by Robert Barro. Though we do not agree with all these comments, they are, in tone and in substance, no more critical of that research than we have been ourselves, both elsewhere and in our paper. For example, we view the technical considerations raised in Sargent (28) as providing more compelling reasons for exercising caution in interpreting Sargent's and Barro's empirical results than do Friedman's remarks. Further, the reader can judge whether or not Friedman has strengthened the extensive caveats made in Sargent (27).[1] Although we feel Friedman's detailed substantive comments are all answerable, we will not respond to them further here.

In his concluding paragraph, Friedman objects to our "rhetorical profile," an objection which several others also expressed at the Conference. To illustrate his point, he cites our reference to "wildly incorrect" predictions of Keynesian

[1] It should be pointed out that the econometric work in Sargent (27), Sargent and Sims (31), and Sims (33) does not reveal that the "money wage rate was indeed exogenous with respect to the variables in the model." Reference numbers refer to those in the Lucas-Sargent paper.

†This reply was written after the conclusion of the Conference and is not intended as a transcript or summary of any remarks made there.

macroeconometric models, to "the spectacular failure of the Keynesian models in the 1970s," or their "econometric failure on a grand scale." These phrases were intended to refer to a specific and well-documented historical event. In 1970, the leading econometric models predicted that an inflation of 4 percent on a sustained basis would be associated with unemployment rates less than 4 percent. This prediction was not one which was teased from the models by unsympathetic critics; on the contrary, it was placed by the authors of these models and by many other economists at the center of a policy recommendation to the effect that such an expansionary policy be deliberately pursued. We recognize that comparison between the experience of the 1970s and the tradeoffs for this period which were forecast at the beginning of the decade may induce some discomfort, but if one is to discuss this well-documented discrepancy, what language is appropriate? Should these forecasts be termed *"accurate,"* or "an econometric *success?"* Or shall these questions be left, as Friedman suggests, "to others to decide"?

The "rhetorical profile" adopted in our paper was not chosen independently of the arguments developed using more precise and technical language in the text, and more fully developed by each of us in earlier writings. It was, on the contrary, an attempt to summarize the main implications of this work in as clear and graphic a way as we could find. If this research is flawed in some essential way, it is difficult to see how softening our rhetoric will help matters. If the implications we have drawn are close to the mark, how can "the cause of scientific interchange" be best served by summarizing them in a way which averages what we believe to be true with what others find pleasant or familiar?

Rebuttal

Benjamin M. Friedman

In their post-conference response, as at the conference itself, Professors Lucas and Sargent have again declined to answer substantive questions raised about their "equilibrium business cycle theory." For example, does this theory require that producers observe the prices of outputs that they sell before observing the prices of inputs that they buy? If this assumption is indeed necessary, is there a justification behind it or is it purely ad hoc?

The intentionally understated proposition that Professors Lucas and Sargent quoted in their response is indeed the essence of my discussion. It is relevant because, instead of simply saying that they are working to improve the treatment of expectations in macroeconomic models, they chose in their paper to present their work as a sharp contrast to "Keynesian economics" as a whole – with the reliance on optimizing behavior versus ad hoc rules of thumb at the heart of the supposed contrast. As their post-conference footnote 6 shows, the overall contrast is weak at best, as well as difficult to support concretely. Do not the MPS and other current "Keynesian models" include these optimizing features? Is the intended contrast against today's models or against those of a generation ago? Why not say precisely which models are under criticism and then look carefully at their actual record of performance? That, and certainly not any resort of wishy-washy verbal compromise, is the alternative I suggest in place of the unfocused rhetorical attack on "Keynesian models" that Professors Lucas and Sargent presented here.

Disturbances to the International Economy

Lawrence R. Klein

1. Identification of the Disturbances

In the second full year of operation of the international trading model built under the auspices of Project LINK we encountered the first of a series of world scale shocks, NEP (President Nixon's New Economic Policy) with the closing of the gold window, surcharging of automobile imports, and a host of domestic economic restrictions. This phase, known in Japan as Nixon shocks, led to the Smithsonian agreement on exchange rates and later dollar devaluation in 1973. This was only the beginning of a tumultuous period with many other shocks of a comparable magnitude.

The specific episodes or scenarios that I shall consider in this paper are the following:

(i) Nixon shocks and the Smithsonian agreement
(ii) Soviet grain purchases, rising food prices, rising raw material prices
(iii) Oil embargo and quadrupling of OPEC prices
(iv) Protectionism
(v) Capital transfers
(vi) Wage offensive

These are actual events or hypothetical scenarios that have been simulated through the LINK system. It is worthwhile considering some cases that have not occurred but need looking into because of the threats they impose on world stability. The added shocks are

(vii) Debt default
(viii) Speculative waves in currencies and commodities
(ix) Famine as a result of large-scale crop failure.

We do not know what the next wave of shocks will be or when it will occur. Some episodes in (i)-(vi) could be repeated or some new and quite unexpected ones could occur. Some plausible cases that have been hinted at as a result of actual developments or that have been openly discussed are being considered here under (vii)-(ix).

(i) The NEP was introduced August 15, 1971. The original edicts were temporary. The surcharge on imported cars was soon lifted and the closing of the gold window was only a prelude to a more significant move, namely, the realignment of exchange rates under the terms of the Smithsonian agreement. The stated expectation of the U.S. Secretary of Treasury, John Connally, was

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that a prompt turn-around in the U.S. trade balance, by some $8 billion, would occur. The United States was in the middle of a strong cyclical expansion phase, while many partner countries were experiencing slowdowns. This became an ideal test situation for applications of the LINK system.

(ii) Russia experienced a significant crop failure in 1972 and began systematically and quietly buying grain in the world market for delivery mainly in 1973. The circumstances of the purchase and lax surveillance on our part made the purchase a bargain at U.S. taxpayers' expense. It also depleted our grain reserves in short order. This led to sharp increases in world and domestic food prices in 1973. The situation was made worse by the ending of phase II under NEP and the weakening of controls under the disastrous phase III. In addition there was a failure in the anchovy catch off Peru. Fish meal served as a close substitute for grain in poultry and other animal feeding.

Accompanying these shocks in early 1973 were two dollar devaluations in February and March. Later, a speculative wave took over many primary commodity markets. In this situation many monetary or fiscal authorities recommended a conscious tightening of policies in order to slow the rate of expansion.

(iii) The biggest single shock was surely the oil embargo of 1973. Given that the authorities were trying to slow down their respective countries, the oil importers as a whole were vulnerable to a large-scale synchronized shock. In place of a "soft landing," there was a significant decline. When prices were increased in 1974 and held at that level, a number of serious trade imbalances were nurtured in the OECD world.

The embargo period itself was disruptive in cutting off supplies of a necessary production ingredient. The result was a sharp fall in output, since producers did not commit their reserve stock to use during the period, not knowing how long the embargo would last or not knowing that in fact significant leakages in the embargo would occur. Although oil was not itself a large component of GNP, it was a strategic one and its shortfall in the market held back many producing processes. It also led to a reduction in many components of final demand – – expenditures on gasoline and oil, household operating expenditures, purchases of automobiles, and purchases of homes.

The subsequent period of high oil prices, without the embargo, continued to be one of recession; the increase in oil prices acted like an excise tax on the economy. The general result of simulating an increased excise tax through a macro model of an industrial economy is to induce a lowering of activity and an increase of prices. When this happened, as in 1974–75, in several industrial countries simultaneously, there were international reverberation effects and the final result was worse than each individual country may have experienced had it been subjected, alone, to the price increase.

(iv) The recession of 1974–75 influenced many countries to introduce protectionist measures in order to counteract business cycle impacts. In two noteworthy cases, Germany and Japan, there were export-oriented increases to maintain domestic activity and lead the respective economies into revivals. As a consequence, both these countries realized enormous trade surpluses. When combined with the OPEC surpluses, a large burden of adjustment faced deficit countries. Deficits there would have to be, because of the world trade identity

$$\text{World exports} = \text{World imports},$$

but they were not evenly distributed throughout the world. Those countries with large deficits looked to protectionism as a way to improve their trade accounts.

Trigger prices against Japanese and European steel imported into the United States are protectionist measures that have recently been introduced. Voluntary quotas imposed on exporters of shoes, textiles, and TV sets are another version of protectionism. Enforcement of anti-dumping laws are yet another. A more straight-forward form would be an increase in tariffs.

The move toward liberalization of trade on a multilateral basis has been set back in recent years and is likely to be set back further given the attitude of powerful industrialists who have been hurt by import competition, and by equally powerful trade unionists whose jobs have been displaced by imported goods. In some countries, an exceptional claim for protecting *infant* industries has been replaced by a claim for protection of *mature* industries. The end result reduces world trade and production because of widespread adoption of "beggar-thy-neighbor" protectionist policies.

(v) In North-South confrontations or dialogues there has long been a request for capital transfers from the former to the latter. The request is based on the argument that the poorer peoples of the world in the southern hemisphere, to a large extent, needed, on pure welfare or humanitarian grounds, capital in order to grow and enjoy some material economic benefits. Another argument is that the northern countries would benefit themselves by creating better markets for their products.

Some progress has been made in implementing capital transfers, but mainly on emergency conditions and not for general growth on a large enough scale to change the world pattern. OPEC nations have made some capital grants to other developing nations that do not have energy resources and find it difficult to pay the high world price for oil. IMF facilities and particularly the proceeds of gold auctions make limited funds available for capital transfer.

This is a shock or episode that has not yet taken place on a large scale, yet can be simulated through the LINK system.

(vi) Wage offensives took place in the United Kingdom, Scandinavia and other countries where domestic prices responded to the new high oil prices after 1973. Inflation rates of 25 percent in Britain induced large wage demands of the same order of magnitude. If wage costs go up at this high rate, prices are sure to be marked up by a similar amount in the next round and we shall have a co-ordinated wage-push effect through the world. As in other synchronized cyclical movements the effect tends to amplify, thus increasing the inflation rate. This process can also be simulated through the LINK system. It was fairly common in 1974-75 and receded only in 1976. Wage pressures lessened a bit as the world recession wore on. It is not back to the high level of 1974-75 but it is on the rise once again. As some countries reflate, in contrast to very slow growth in the past year or two, as in the German case, trade union restiveness and assertiveness could put significant pressures on wages. If inflation rates turn up again, the rise could very well be the result of higher wage demands.

(vii) Every time a particular country gets into trouble in its debt servicing, the possibility of debt default looms. To a large extent, debt service has become a critical issue for developing countries – Peru, Zaire, Zambia are primary examples. But large amounts of outstanding debt are on the books of Mexico, Brazil, Taiwan, S. Korea and India, all of whom are much better situated for covering service costs than are the troubled countries. For this reason, the danger of a widespread wave of international bankruptcy is far-fetched, but it is a shock scenario that is worth considering.

It is not only in the area of developing countries where debt default is a live issue but several developed countries are likely to have trouble in meeting obligations. The leading cases are Spain, Portugal, and Turkey. Some centrally planned economies have been troubled by debt burdens, but it seems unlikely that they, as a group or individually, would willingly fail to honor international obligations. They have voluntarily restrained their indebtness once it became apparent that they were overextended.

(viii) In 1973 there was substantial speculation in markets for basic materials, both agricultural and industrial. Grain market prices rose by 100 percent or more and there was much speculative activity although the primary disturbance came from the large scale Soviet purchases. Later speculative waves came in 1974 (sugar) and 1976 (coffee). As for industrial commodities, speculation in copper and other nonferrous metals was significant.

These high prices had adverse effects on the import value and external balance of several consuming countries. The United Kingdom is a case in point. This kind of disturbance led to the restrictive fiscal/monetary policies that made countries highly vulnerable to the oil shock.

Currency speculation had also been evident and caused significant international disturbances. The runs on sterling and lira in early 1976 induced domestic inflation, followed by a whole train of events that impeded the United Kingdom and Italy. In the case of sterling, there was some degree of suspicion that shifts of sterling balances by OPEC countries were responsible for much of the decline in the exchange values of sterling.

(ix) Some of the most volatile prices that have risen on a scale comparable with oil prices in 1974-75 have been food prices. They doubled, while the cartel raised oil prices by a factor of 3 or 4. The principal difference from the oil case was that supply could be quickly increased and high food prices were promptly brought down as stocks were rebuilt. Thus an agricultural harvest disturbance is likely to be shorter in duration than are others, where supply is less responsive.

Nevertheless, a large crop shortfall on a world scale could bring about significant price increases for some foods perhaps by as much as 100 percent or more. If this were to occur, suddenly, the world economy could well be faced with a new crisis with dimensions as large as or larger than those experienced earlier in this decade.

2. Outline and Use of the LINK System[1]

When modeling and studying a national economy by simulation methods, it is generally assumed that export volume and import prices are exogenous variables. Export volume depends mainly on world trade or world economic activity, or import requirements of partner countries. Either export volume itself or world (foreign) activity variables, once removed, on which exports depend, are treated as exogenous. This is not strictly correct since price competitiveness, which depends on endogenous domestic behavior, also influences exports. But as a first approximation, we shall accept the usual assumption that exports are exogenous. Similarly, import prices are determined by cost and pricing decisions of partner countries. They are, therefore, treated as exogenous, too. To the extent that a major country influences its partners' pricing decisions, for competitive reasons, import prices are not wholly exogenous, but again, as a first approximation, they are treated as exogenous variables.

Import volume and export prices are both endogenous variables. The former depend on domestic activity variables and relative prices – at home and abroad. The latter depend on domestic cost and supply conditions. To the extent that a country tries to remain competitive with its partners and prices exports accordingly, or is a price taker in a world market for basic commodity exports, it may not be appropriate to classify export prices as endogenous. But the principal practice is to put import and export prices in the endogenous category.

The primary purpose of the LINK model is to endogenize export volumes and import prices. For the world trade economy, as a whole, both exports and imports, export prices and import prices are endogenous. On a world basis, there are no exogenous elements in this nexus. It may also be said that the purpose of LINK is to analyze the international transmission mechanism or to form international linkages among national econometric models.

The LINK system does this in a consistent way by imposing two accounting identities:

$$\sum_{i=1}^{n} X_i = \sum_{i=1}^{n} M_i \qquad \text{world export volume = world import volume}$$

$$\sum_{i=1}^{n} (PX)_i X_i = \sum_{i=1}^{n} (PM)_i M_i \qquad \text{world export value = world import value}$$

These identities are imposed in terms of a common numeraire unit (the U.S. dollar) at FOB valuation. The identities hold for commodity classes,

[1] See R.J. Ball, *International Linkage of National Economic Models,* J. Waelbroeck, *The Models of Project LINK,* (Amsterdam: North Holland, 1973, 1976). A third volume edited by John Sawyer is now in press.

SITC	0,1	food, beverages, tobacco
SITC	2,4	other raw materials
SITC	3	mineral fuels
SITC	5-9	manufactures and semimanufactures

The number of countries or areas (n) is presently 24.

There are

13 OECD Countries	(Australia, Austria, Belgium, Canada, Finland, France, West Germany, Italy, Japan, Netherlands, Sweden, United Kingdom, United States)
7 CMEA Countries	(Bulgaria, Czechoslovakia, East Germany, Hungary, Poland, Romania, U.S.S.R.)
4 Developing Regions	(Africa, Asia, Latin America, Middle East) An OPEC/non OPEC split is also provided

A residual category (ROW) is not explicitly modeled, but assumed to have a constant share of world trade.[2]

A detailed way of insuring fulfillment of the accounting identities would be to model bilateral trade equations between countries for separate commodity groupings. An example would be

SITC 0,1

$$\ell\eta X_{ij} = \underset{(-6.8)}{-11.327} + \underset{(12.2)}{1.890}\,\ell\eta M_j - \underset{(-2.0)}{1.271}\,\frac{PX_{ij}}{PC_{ij}}$$

$$R^2 = 0.973 \quad S.E. = 0.079 \quad D.W. = 0.989$$

i = Netherlands	PX_{ij} = price index of Netherlands shipments (SITC 01) to Germany
j = West Germany	PC_{ij} = price index of competing countries' shipments (SITC 01) to Germany

This is just an example of many equations that have been estimated by P. Ranuzzi of the EEC, bilaterally, for commodity groups. It covers SITC (0,1) imports by Germany from the Netherlands. There is some evidence of serial correlation of residuals, which could be reduced on further research into the

[2] This assumption is being weakened, and small models are being built for 13 separate countries (Mainland China, Denmark, Greece, Iceland, Ireland, New Zealand, Norway, Portugal, South Africa, Spain, Switzerland, Turkey, Yugoslavia). For some, major models may be used.

time shape of reaction, but most of the bilateral equations do have a more random pattern for residuals.

There are many bilateral combinations to be determined for this number of countries or areas. To simplify the work, we estimate total (not bilateral) import equations for each model, by SITC categories. Exports are (endogenously) computed from

$$X = AM$$

where A is a world trade share matrix with element

$$A_{ij} = X_{ij}/M_j$$

$$X_{ij} = \text{exports from i to j}$$

$$M_j = \text{imports of j}$$

X is a vector of exports (across countries / areas) and M is a vector of imports (across countries / areas). As long as the column sums of A are unity, we satisfy the world trade identity. If this identity is in volume terms (constant prices), the corresponding identity in value terms (current prices) is

$$(PX)' \; AM = (PM)' \; M$$

from which we deduce

$$(PM) = A' \; (PX)$$

In the LINK system, we do not assume that the elements of A are constant. They are functions of relative prices and move through time as endogenous variables of the complete system.

The system is solved by assuming export volumes and import prices for each country / area model. The individual model solutions for M and (PX) are substituted into the above matrix equations for estimation of X and (PM). The models are then solved again for M and (PX); new values for X and (PM) are computed, and the iterative process stops when the total value of world trade does not change from iteration to iteration (approximately).

This is a highly condensed description of the LINK system. How can it be used for studying world disturbances? Each of the specific disturbances (i) – (ix) described in the previous section can be examined as a scenario or structured simulation. A base simulation is first established as a dynamic projection of the system from fixed initial conditions and exogenous inputs on a "best judgment" time path that does not include the particular disturbance. Then an alternative simulation is developed with the disturbance included, everything else unchanged from the baseline path. The difference, at each successive time point, between the scenario and baseline path provides an estimate of the effect of the disturbance.

Preliminary to the working out of scenarios, we first estimate multipliers of the system that show sensitivity to changes in exogenous variables or to exogenous shifts of entire relationships. It is instructive to examine the standard fiscal multiplier from a single country viewpoint and a world system viewpoint. To make matters simple let us use the two-country world model

$$y = e\,y + m^*y^* - m\,y + g$$

$$y^* = e^*y^* + m\,y - m^*y^* + g^*$$

There are two countries with output levels y and y* respectively. Output in each country is the sum of

induced spending, ey or e*y*, on consumption and capital goods

exports, m*y* or m y
less imports, m y or m*y*
exogenous government spending, g or g*

The relationships are assumed, for purposes of exposition only, to be linear and proportional. In this two-country world, the world trade identity is automatically satisfied because one country's imports (m*y*) is another country's exports. In the second country, exports (m y) are the first country's imports. It is assumed that an exchange conversion makes the units comparable in the two countries.

Taking the single-country view, in isolation, we can derive the reduced form equation for the first country as

$$y = \frac{g + m^*y^*}{1 - e + m}$$

The conventional multiplier, for a given level of exports (m*y*) is

$$\frac{dy}{dg} = \frac{1}{1 - e + m}$$

The simplest multiplier formula $\left(\frac{1}{1 - e}\right)$ is modified by the inclusion of the import leakage factor in the denominator, thus tending to reduce the multiplier's value. Indeed, countries with very high marginal propensities to import – prototypes being the United Kingdom and the Netherlands – are known to have low multiplier values, possible less than unity.

In the two-country case, the reduced form is

$$y = \frac{(1 - e^* + m^*)\,g + m^*g^*}{(1 - e + m)\,(1 - e^* + m^*) - m\,m^*},$$

and the multiplier is

$$\frac{dy}{dg} = \frac{1}{1 - e + m - \dfrac{m\ m^*}{1 - e^* + m^*}}$$

By including the term

$$- \frac{m\ m^*}{1 - e^* + m^*}$$

in the denominator, we have increased the size of the multiplier. Thus the world model is more sensitive to a disturbance when intra-country trade effects are taken into account in the model. If the second country also stimulates by moving g^*, there is another effect to be added, namely

$$\frac{m^*}{(1 - e + m)\ (1 - e^* + m^*) - m\ m^*}$$

provided g^* moves pari passu with g. This result shows not only that international repercussion effects exist as well as direct country effects, but also that synchronized effects intensify movements in both countries simultaneously. It shows, moreover, that one country is sensitive to policy changes in another. In this example, y depends on (partial) movements in g^*. These are indirect effects.

In LINK simulations, synchronized effects and indirect policy effects have been examined across countries. Simultaneous fiscal changes; inventory drawdowns in a crisis, such as the oil embargo; simultaneous wage-push increases; simultaneous limitations on imports (protectionism) have all been studied.

Multipliers have been calculated for the LINK system without synchronization; i.e., by changes, one at a time, to fiscal variables in a given model. Both direct and indirect effects on other countries are studied in these multiplier scenarios. Although these are not simultaneously introduced, for multiplier calculations, the effects are simultaneously spread over several OECD economies at once.

Oil price increases are not synchronized except to the extent that all the countries in OPEC, plus outsiders that are large producers, will have imposed on other economies an equivalent of a world excise tax. The synchronization of this case is in the movements of the oil-importing economies.

Apart from some strictly controlled prices, like the cartel-determined oil price, domestic costs or world competition largely govern the determination of export prices. These prices are then converted into import prices (exogenous to a single country), by means of a transformation using the world trade matrix. If the world inflation rate increases, it will result from higher export prices. This is the counterpart of the strategic importance of imports in determining the volume of world trade. By using the row elements of a trade-share matrix to convert imports into each component of exports, we are doing essentially the same thing as transforming changes in export prices into changes in import prices. In this latter case, columns of the trade matrix are weights in the transformation process.

An important aspect of the present stage of the LINK system has not yet been explained, namely the role of exchange rates. They are, at the present time, exogenous in the LINK system. They are not constant because frequently they are exogenously changed in the middle of a LINK simulation. Only now have we been able to turn our attention to analysis of exchange rates and endogenize them for projection and simulation analysis. Since the Smithsonian Agreement we have gained enough experience to examine the body of data available from 1971 to date in order to make first attempts at estimating equations that try to explain exchange rates – as functions of country interest rate differentials, growth rate differentials, inflation rate differentials, changes in reserves, and levels of wealth.

As imports and export prices are endogenously generated by the solution of each model, they are expressed in local currency units, which are different for nearly every country.[3] In order to use these series in the LINK algorithm with dollar-denominated trade flows, we must convert imports and export prices from dollar denominations into exports and import prices, also in dollar quotations. As imports and export prices leave individual models, expressed in local currency units, we multiply them all by a series of exogenous exchange rates into dollar-denominated totals. The operational formulas are:

$$M\ (L)\ Ex\ (\$/L) = M\ (\$)$$

$$(PX)\ (L)\ Ex\ (\$/L) = PX\ (\$)$$

The right-hand side variables are all expressed in dollar terms. The trade matrix is based on dollar valuations; so it should be multiplied into either M($) or PX($), imports and export prices in dollar units. These multiplications generate

$$X\ (\$)$$

$$PM\ (\$)$$

i.e., dollar valuations of exports and import prices. Before these variables can be reinserted into individual models, for the next iterative step in the system solution process, they must be converted back into local currency units appropriate to each model. This step takes the form

$$X\ (\$)\ /\ Ex\ (\$/L) = X(L)$$

$$(PM)\ (\$)\ /\ Ex\ (\$/L) = (PM)\ (L)$$

[3] Developing countries are treated by area grouping. Area models are based on dollar-denominated variables, aggregated over countries.

Exchange rates, used in this way, have significant impacts on the entire solution. So exchange rates play important roles; they are simply in need of endogenization.

By and large, when persistent deficits or surpluses appear in country accounts for simulation exercises, we find that the former lead to currency depreciation while the latter lead to currency appreciation. After exchange rates are changed, either exogenously or endogenously, on the basis of a solution, we have feed-back information for altering the solution.

In SITC groups 0,1 and 2,4, the relevant prices are determined in world markets, balancing supply and demand. For the most part, primary producing countries are "price takers." In order to obtain good estimates of export prices for such countries, it is necessary to couple the LINK system with systems of simultaneous equations to explain commodity markets, either major agricultural crops and other products, or markets for industrial materials. The principal feedback on the LINK system is through determination of price for producing countries, and, consequently, export earnings in these commodity lines. Some twenty-odd commodity models have been estimated by F.G. Adams and others for combination with the LINK models.[4] They have estimated equations of the form

$$(PX01) = f(P_1, P_2, \ldots P_n)$$

$$(PX24) = g(Q_1, Q_2, \ldots Q_m)$$

PX01 = export price index of group SITC 0,1

PX24 = export price index of group SITC 2,4

P_i = world price of i-th food commodity

Q_i = world price of i-th industrial commodity

These equations have been estimated for each primary producing country or area.

The commodity models are solved, for primary price determination, on the basis of input values for demand or other factors from the LINK system. The prices estimated from the commodity models are then inserted into f and g, above, to estimate new values of (PX01) and (PX24). The LINK system is re-solved with these new estimates of export prices, and the commodity models are solved in another iteration. This procedure continues until convergence is attained. This extended model and program is known as COMLINK.

[4] F.G. Adams, "Primary Commodity Markets in a World Model System," *Stabilizing World Commodity Markets*, ed. by F.G. Adams and S.A. Klein, (Lexington: Lexington Books, 1978), 83–104.

3. Some Empirical Results.

A number of LINK studies have examined, in the past, many of the issues raised in the first section, using the procedures and systems in the second section.

Increases in basic commodity prices (hypothetically) during 1975-76, interpreted as an increase in export prices of developing countries by an extra 10 percent over a baseline case, produced the following deviations from the baseline values of GNP, GNP deflator, consumer price deflator, and trade balance.

It is evident from studying the left panel of Table 1 that higher export prices in primary producing countries in the developing world would generally increase inflation rates in the industrial (using) countires. Of the two measures of inflation presented here – GDP deflator and consumer price deflator – the latter is probably more suitable, because price increases in imports can often lead to *lower* GNP prices. This is because imports enter negatively in the GNP identity. A clearer picture of domestic inflation is given by the consumer price deflator. Mainly domestic goods are being priced in this index measure. A few countries stand to make trade gains, but these are a minority, and most of the significant changes are losses, on trade account. Only the LINK OECD countries are included in Table 1. Although these are the largest countries and the ones that dominate the world economy, not all important countries are included. The results are clearest and most reliable for the major countries that are specifically modeled; those are the ones listed in Table 1.

The payment of higher prices to primary producing countries is not all negative, however. The developing countries earn some extra purchasing power since many primary products are price-inelastic. With the extra purchasing power in the hands of some developing countries, they are able to increase their imports from the industrial countries. This accounts for some of the "perverse" signs – rising GDP in the face of higher primary input prices.

The right-hand panel is possibly more interesting. It induces more pronounced changes since it is a scenario that is far from what actually happened. What if there had been no oil embargo and no forceful setting of world oil prices by OPEC? The increases in GDP rates and the fall in inflation rates are considerably bigger than those in the left panel, when prices are changed by a mere factor of 10 percent. In the case of the other simulation, oil price is, hypothetically, held constant at its 1973 value way into 1976.

Large oil-importing countries have significant declines registered in their prices as a result of having held the line on oil prices. It shows how important energy is in the pricing decision. The inflation rate is substantially down in every country except Australia and Austria. At the same time that price would have been held down in this "what if" scenario, real output rose, with the exceptions of Australia, Austria, Canada, and Finland. Canada is, of course, an energy exporter, but on a small scale. Austria is more in a swapping posture, importing and exporting energy, but Australia has real GNP gains, against the tide of most partner countries.

TABLE 1

Effects of Commodity Price Increase and Constant Oil Price
(Percentage Deviation from baseline except
Trade Balance, Value of Deviation, billions of U.S. dollars)

		Higher Export Prices Developing Countries				Constant Oil Price (1973 value)			
		GNP	GNP De-flator	Con-sumer Price De-flator	Trade Bal-ance	GNP	GNP De-flator	Con-sumer Price De-flator	Trade Bal-ance
Australia	1974					−4.2	2.7	0.7	−0.35
	75	1.2	−0.7	−0.2	0.16	−3.9	4.0	1.6	0.35
	76	1.9	−1.1	−0.5	0.03	−5.5	3.4	1.5	0.74
Austria	1974					−0.9	−0.4	−1.4	0.12
	75	0.2	0.1	0.3	−0.03	0.6	−0.2	−1.3	0.24
	76	0.1	0.2	0.3	−0.04	2.2	0.7	−0.8	0.21
Belgium	1974					0.0	−3.1		−0.61
	75		0.5		0.02	2.0	−4.0		−0.78
	76	−0.1	0.6		0.01	2.9	−4.1		−1.00
Canada	1974					−3.6	−1.2	−1.3	−3.44
	75	0.9	0.5	0.6	0.85	−2.5	−3.0	−2.3	−4.14
	76	0.6	1.0	1.0	0.97	−1.8	−4.4	−3.5	−4.19
Finland	1974					−1.5	−1.5	−4.5	0.53
	75	0.5	0.3	0.8	−0.05	−1.4	−2.0	−5.4	0.90
	76	0.7	0.6	1.2	−0.09	−0.6	−3.4	−5.8	1.17
France	1974					1.3	−6.9	−6.4	0.61
	75	−0.4	1.2	1.3	−0.03	4.4	−7.0	−6.7	0.26
	76	−0.6	1.3	1.4	0.02	4.8	−7.5	−7.0	1.60
Germany	1974					0.4	−0.7	−0.7	−0.92
	75	−0.1	0.3	0.3	0.43	0.1	−0.3	−0.3	0.90
	76	0.1	0.6	0.7	1.00	0.3	0.5	0.5	3.20
Italy	1974					0.2	−3.9	−8.8	2.71
	75	0.5	−0.1	0.7	−0.43	3.9	−11.8	−16.6	1.35
	76	0.2	0.4	1.3	−1.17	5.3	−8.3	−12.7	−1.52
Japan	1974					0.9	−0.3	−2.5	6.93
	75	0.0	0.0	0.6	−1.25	5.1	−5.2	−5.4	7.97
	76	−0.7	1.2	1.3	−1.29	10.1	−8.8	−7.3	8.87
Netherlands	1974					0.3	−7.0		0.68
	75	−0.1	1.4		−0.11	2.0	−9.8		0.36
	76	−0.2	2.1		0.04	3.9	−11.1		0.19
Sweden	1974					−0.5		−2.0	−0.50
	75	0.1		0.4	0.11	0.5		−2.2	−0.06
	76	0.1		0.4	0.06	1.8		−2.5	0.09
U.K.	1974					0.3	−3.8	−6.7	7.67
	75	−0.2	1.3	1.7	−1.58	1.5	−8.3	−11.3	9.85
	76	−0.3	2.1	2.6	−1.84	2.6	−10.6	−13.9	12.30
U.S.	1974					−1.0	0.3	−0.5	6.95
	75	0.3	−0.1	0.2	−1.54		−0.3	−1.1	15.01
	76	0.3	−0.1	0.3	−2.52	1.4	−1.1	−2.0	19.72

On balance, the trade accounts would have moved toward surplus. The right-hand side column is dotted with negative entries. Some of these are due to the fact that 1973 oil prices would allow most countries to grow. Those that do, sometimes import so much that trade becomes unsettled again.

Oil is basically a traded commodity, albeit, a highly strategic one. What would have been the disturbance to the world commodity if Saudi Arabia had not been persuaded by the U.S. authorities to use its power to freeze oil prices in 1978?

The sensitivity of the world economy to further price shocks is examined by simulating the LINK system, 1978–79, for different oil price rises – 0, $2, and $4 per barrel.[5] To carry out this calculation, the export prices for group 3 SITC was increased for the oil-exporting countries. The variable appears now as an index, and its level in 1978 was assumed to stand for $14.00 per barrel of crude oil. It was then either held constant or increased by 2/14 or 4/14 for the appropriate case being studied. The increases were implemented for the Middle East, those parts of Latin America, South and East Asia, and Africa corresponding to the inclusion of OPEC countries (Venezuela, Ecuador, Indonesia, and Nigeria), and for Canada. At the time of this calculation it was thought that the increase would come to about $1.00 per barrel, and that figure was used in the standard projections. As it turned out, the case of zero increase, which was one variant on the low side, could best have served as a baseline case. In the present circumstance, we use that as a base case to study the effect of price increases, but it probably will not be the best control position to assume now for 1979.

The clearest story is told by the global totals in Table 2. Oil priced at $2 per barrel higher in 1978 and again in 1979 is the first alternative. The increments are $4 in each year in the second alternative simulation. Each price increase lowers the estimated value of real world output and real world trade. At the same time, inflation rates go up, whether measured by the unit value of exports, the GNP deflator, or the consumer price index. The positive and negative offsets are less than perfect, but the influence of an increase in an import price is more clearly and strikingly shown in the estimates of consumer prices. Estimated inflation goes up by a full percentage point between the no-change and $2 alternative case. This is clearly a potential contribution to global inflation rates. The increase from $2 to $4 per barrel contributes less to overall inflation than does the increase from no change to $2 per barrel. It appears that the large German and Japanese external surpluses are severely reduced as the price of oil rises by an amount from $0.00 to $4.00 per barrel. The changes affect most, but not all, countries in similar ways. The results for a number of countries (LINK countries) are shown in Table 2.

The U.S. trade balance is considerably worsened, as is the real growth rate. The other locomotive countries, Germany and Japan, would be similarly affected, but large trade surpluses would not be wiped out. The U.K. deficit would improve in 1979 but deteriorate in 1978. Other oil-producing or ex-

[5] Dr. Vincent Su of the LINK research staff prepared these simulations of alternative oil prices, 1978–79.

TABLE 2

Effects of Increasing Oil Prices, 1978–1979

(Percentage Point Deviation from No-Change Case, Except Trade Balance, Value of Deviation, billions of U.S. dollars)

		$2/Barrel Increase				$4/Barrel Increase			
		GDP	GDP Deflator	Consumer Price Deflator	Trade Balance	GDP	GDP Deflator	Consumer Price Deflator	Trade Balance
Australia	78	–0.2	0.0	0.0	–0.25	–0.3	–0.1	0.1	–0.54
	79	–0.2	0.0	0.1	–0.64	–0.4	–0.1	0.1	–1.31
Austria	78	–0.6	–0.1	0.2	–0.29	–0.7	–0.2	0.3	–0.53
	79	–1.2	–0.2	0.2	–0.63	–2.1	–0.4	0.3	–1.04
Belgium	78	–0.9	0.4	0.7	–0.21	–2.1	1.0	1.5	–0.46
	79	–1.6	0.6	1.1	–0.67	–2.6	0.9	1.7	–1.23
Canada	78	0.0	0.5	0.3	0.15	–0.1	1.1	0.6	0.24
	79	–0.1	0.9	0.7	0.10	–0.4	1.8	1.4	0.14
Finland	78	–0.1	0.2	0.3	–0.13	–0.2	0.3	0.6	–0.25
	79	–0.3	0.3	0.5	–0.32	–0.7	0.5	0.8	–0.55
France	78	–0.6	1.6	1.9	–1.87	–1.3	2.9	3.5	–3.71
	79	–0.9	1.9	2.3	–4.31	–1.5	2.8	3.4	–7.59
Germany	78	–0.8	–0.3	– – –	–1.61	–1.8	–0.8	– – –	–3.75
	79	–1.1	–0.7	– – –	–4.18	–1.9	–1.2	– – –	–7.54
Italy	78	–0.6	–0.1	0.4	–1.18	–1.4	–0.3	0.8	–2.39
	79	–0.8	–0.1	0.6	–2.28	–1.2	0.0	1.0	–3.72
Japan	78	–2.3	1.0	6.6	–6.04	–4.9	2.0	7.6	–12.44
	79	–4.0	1.1	6.0	–13.00	–7.1	1.7	6.3	–22.27
Netherlands	78	–0.9	–1.0	0.5	–1.71	–2.4	–2.7	0.9	–2.46
	79	–0.4	–0.2	0.5	–2.86	0.0	–1.6	0.3	–4.80
Sweden	78	0.0	– – –	0.6	–0.66	0.0	– – –	1.2	–1.36
	79	–0.2	– – –	0.7	–1.60	–0.4	– – –	1.0	–2.86
U.K.	78	–0.4	0.4	0.6	–0.24	–0.8	0.8	1.2	–0.45
	79	–0.5	1.2	1.2	0.09	–0.8	2.1	2.0	0.38
U.S.	78	–0.4	0.0	0.2	–6.66	–0.7	0.1	0.4	–14.10
	79	–0.5	0.0	0.2	–15.49	–0.9	0.1	0.4	–30.68

		$2/Barrel Increase	$4/Barrel Increase
TWXV	78	$10 b.	$26 b.
	79	$18 b.	$41 b.
PWX	78	4.36%	7.42%
	79	2.08%	4.92%
TWXR	78	$–15.0 b.	$–25.0 b.
	79	$–25.0 b.	$–43.0 b.
GDP (13)	78	$–10.0 b.	$–29.0 b.
	79	$–32.0 b.	$–73.0 b.
PGDP (13)	78	0.25%	0.49%
	79	0.32%	0.53%
PC (13)	78	1.10%	1.49%
	79	1.08%	1.35%

TWXV = Nominal value of world trade, billions of US$
PWX = Unit Value of world exports, 1970: 1.0, US$ denomination
TWXR = Real value of world trade, billions of US$ 1970
GDP (13) = Percentage change real GDP, 13 LINK countries, billions of 1970 US$
PGDP (13) = Percentage change GDP deflator, 13 LINK countries, 1970: 1.0
PC (13) = Percentage change consumer deflator, 13 LINK countries, 1970: 1.0

porting countries such as Canada and Netherlands (refined products) would benefit one way or another, the former on trade account and the latter in terms of GNP growth. But on the whole, it is good for the world economy that the line has been held on oil prices for 1978.

Simulations with the LINK system, reported in Tables 1 and 2, provide estimates of the world effect of changes in petroleum and other basic material prices. There are few, if any, systematic world-linked estimates available for verification or validation purposes, but there is a careful study of unlinked estimates of the effects on the U.S. economy alone by a staff team of the Federal Reserve Board.[6] They conclude that consumer price rises between 1971 and 1974 were strongly influenced by dollar depreciation and extraordinarily large increases in export/import prices (mainly food and fuel). About 15 percent of the consumer price rise was accounted for by decline in the dollar's exchange value and 25 percent by the price disturbance. In the simulation of Table 1, with oil prices held constant at their 1973 levels, we estimated that the overall effect on the world inflation rate was about 20 percent of the total price increase in 1974. As an order of magnitude estimate, considering that only one commodity's price rise is being held constant, that only the 1974 effect is being compared, and that the effect is world-wide, the Federal Reserve judgment and the LINK judgment are consistent with each other.

The Federal Reserve team also emphasizes that it is necessary to take into account which prices were affected and why they have risen in order to assess the effect on the domestic inflation rate. If the inflationary impulses come from external sources, stagflation, i.e., rising prices with rising unemployment, can be produced. Demand impulses, internally generated, can produce the standard trade-off relation of falling unemployment and rising prices.[7] The external shock acts like an excise tax, reducing demand, increasing unemployment, and generating inflation. This is a familiar macroeconometric result.

Among the remaining shock scenarios that have been investigated on previous occasions, let us examine capital transfers (v).[8] This case has been worked

[6] R. Berner, P. Clark, J. Enzler, and B. Lowrey, "International Sources of Domestic Inflation", *Studies in Price Stability and Economic Growth,* Joint Economic Committee, U.S. Congress (Washington, D.C.: U.S. Government Printing Office, August 5, 1975), pp. 1–41.

[7] Similar conclusions were reached with Wharton Model simulations by L.R. Klein, "The Longevity of Economic Theory," *Quantitative Wirtschaftsforschung,* ed. by Horst Albach, et al., (Tubingen: J.C.B. Mohr, 1977), 411–19. The Federal Reserve team used the Federal Reserve model.

[8] Protectionism is taken up in L.R. Klein and V. Su, "Protectionism: An Analysis from Project LINK," *Journal of Policy Modeling,* I(1978) 1–30, and wage offensive is in L.R. Klein and K. Johnson, "Stability in the International Economy: The LINK Experience," *International Aspects of Stabilization Policies,* ed. by A. Ando et al., (Boston: Federal Reserve Bank of Boston, 1975). Protectionism generally reduces world trade and growth, with more inflation. Some countries gain but losses outweigh gains. In the case of simultaneous wage pushes in many countries, together, there is noticeable amplification of the final result on price inflation but somewhat less regular than in the case of a quantity shock as occurred in the oil embargo.

out by Carl Weinberg of the LINK staff. He assumed that $20 billion per year, 1976-78, is transferred to the developing countries of Africa, Latin America and South/East Asia. No capital transfer was (assumed to be) made to the Middle East countries. The objective was to examine the effects on growth in the recipient nations but also to estimate the feedback effect on the developed industrial countries to see how prosperity in the developing world induces imports that originate with exports of the developed world. This scenario was worked out on the assumption that the transfer did not arise as a cost item for the developed industrial country. It could presumably have been a transfer within the developing world – as if from OPEC reserves – or from the assets of world organizations such as the IMF. The other case, in which there is a genuine donor's cost, needs to be worked out. It is in process but has not been completed.

In the developing country models there is a variable representing financial inflows. The increment to these flows is distributed to the three developing regions according to their shares of capital inflows historically. It was done for a single year and for three years running. The latter case is analyzed here.

The developing nations gain most clearly and by largest amounts. Among developed nations, the Netherlands stands out. Most countries are grouped from 0.3 to 0.8 percent, as percentage deviations from the baseline case. The developed world gains from the prosperity of the developing countries, but the larger gains are with the latter.

The next world shock could come through a harvest failure.[9] This case is represented by a large price increase for agricultural exports by the big grain-exporting countries – United States, Canada, Australia, Argentina, France. We have assumed for this scenario that prices double in the first year (1978) but slacken as new acreage is brought under cultivation in a supply response.[10] The doubling in 1978 is followed by an increase of 75 percent (over the baseline PX01) in 1979 and by 25 percent in 1980.

The grain-producing countries will have higher export prices for SITC 0,1. Grain-importing countries are assumed to have demand elasticity with respect to price at the low figure of 0.25. Import values of food and imports, generally, rise greatly in the consuming countries. Inflation goes up faster, however, than nominal values; consequently, real magnitudes fall. This holds for both real trade volume and real gross domestic product. Also, the lags in import relationships, as well as at other places of the macro economy, make the time pattern of reaction a bit slow. Larger effects are noted for the second year, 1979, than 1978. The effects are larger in the second year, in spite of the fact that we assumed a supply response adequate to hold PX01 to 75 percent (second year, 1979) and to 25 percent (third year, 1980) increments over the baseline.

[9] "Scenario of a Worldwide Grain Shortage," with Vincent Lee and Mino Polite, LINK memorandum, July 1978.

[10] France and Australia have somewhat lower export price rises since grain exports account for only 30 and 47 percent of total agricultural exports, respectively.

TABLE 3

EFFECTS OF CAPITAL TRANSFERS OF $20 BILLION ON GDP

Percentage deviation from baseline

	1976	1977	1978
AUSTRALIA	0.5	0.7	0.4
AUSTRIA	0.6	1.1	0.5
BELGIUM	0.6	0.6	0.3
CANADA	0.4	0.4	0.3
FINLAND	0.8	0.8	0.7
FRANCE	0.6	0.5	0.3
GERMANY	0.5	0.4	0.4
ITALY	0.7	0.6	0.5
JAPAN	1.0	1.1	0.9
NETHERLANDS	0.8	1.7	1.8
SWEDEN	0.5	0.8	0.3
U.K.	0.7	0.8	0.5
U.S.	0.3	0.4	0.2
AFRICA	3.1	3.4	2.8
SOUTHEAST ASIA	0.7	0.9	0.8
LATIN AMERICA	2.8	2.8	2.3
TWXV	2.6	2.6	2.0
PWX	−0.4	−0.2	0.3
TWXR	3.0	2.8	1.8
GDP (13)	0.5	0.6	0.4
GDP (DEVE)	1.7	1.8	1.5

On a global scale, PX0-9, the export unit value index for all merchandise trade goes up by at most 2.4 percent in the first year, while PX0,1, the export unit value for food, beverages and tobacco goes up by 24.9 percent maximum – also reached in the first year.

The decline in GDP, for 13 major LINK countries in the OECD group, is held to less than 1.0 percent. In the third year, there is some slight relief in the trade surplus for Germany and Japan. In Germany the relief shows up as early as 1978, for this simulation exercise. The United States, as the world's largest grain exporter, gets enough export stimulus to make its GNP slightly larger than in the baseline solution. The U.S. trade deficit is, on balance, a gainer in this scenario. The main anomaly in Table 4 is the United Kingdom. Prices both overall and in the consumer sector are lower in the case of the harvest failure. The movement of GDP and the trade balance are as expected, but the price movement is not.

Inflation goes up slightly in the harvest failure scenario. The overall index of inflation, measured by GDP prices, is about 0.2 above the baseline values in the first two years. In the individual country tabulations, we often find that consumer price inflation is more sensitive to the external price than is the overall deflator. This is perhaps one of the most dangerous and inadequately appreciated aspects of the external shock to the price system.

In the case of the oil embargo, followed by raising of oil prices, there were larger and more dramatic effects on the economy of the whole world, as well as for many national parts. Supply response to fill a gap between supply and demand was weaker in the petroleum case. Also, petroleum has a more extensive interindustry (intermediate processing) use. This makes for bottlenecks and production substitutions. Hence, the oil crisis was able to send the world economy into recession, but this particular agricultural scenario merely slows down growth by fractional points. There is, of course, a great deal of difference between one year's doubling, in the case of grain price, and many years' quadrupling of price in the petroleum case. Although the assumptions may have been large in scope, the final result appears to be fairly mild. It follows a predictable path, and the main value of the LINK exercise is to put empirical magnitudes in proper perspective.

TABLE 4

SIMULATED EFFECTS OF WORLD HARVEST FAILURE

(Percentage Deviation from Baseline Simulation
Trade Balance Deviation billions of U.S. dollars)

		GDP	GDP Deflator	Consumer Price Deflator	Trade Balance
AUSTRALIA	1978	−1.40	0.30	0.70	0.64
	79	−1.10	0.80	1.30	1.25
	80	0.30	0.90	1.20	2.47
AUSTRIA	1978	−3.80	−0.70	−0.30	−0.30
	79	−1.30	−0.70	−0.40	0.00
	80	2.80	0.00	−0.30	1.10
BELGIUM	1978	−1.00	0.60	0.90	−0.20
	79	−0.50	0.50	0.70	−0.10
	80	−1.50	0.10	0.30	−0.50
CANADA	1978	−1.00	1.40	1.00	−1.04
	79	−0.50	1.90	1.80	−0.12
	80	−2.10	1.80	1.50	−0.43
FINLAND	1978	0.00	0.40	0.40	−0.60
	79	0.30	4.70	0.30	0.00
	80	4.80	−1.60	−0.60	0.60
FRANCE	1978	−1.90	1.40	−0.04	1.69
	79	−0.80	1.00	0.00	3.57
	80	−1.80	0.40	−0.20	−1.06
GERMANY	1978	−0.60	−0.30		−1.32
	79	−0.60	−0.50		−1.13
	80	−1.10	−0.90		−2.14
ITALY	1978	5.10	−0.20	1.40	−1.30
	79	3.90	0.60	2.00	−0.06
	80	−7.30	1.20	1.40	4.12
JAPAN	1978	0.50	0.90	1.00	4.55
	79	0.20	1.30	1.30	4.55
	80	−1.30	0.60	0.70	−5.74
NETHERLANDS	1978	0.20	−0.70	0.40	−1.94
	79	0.20	−0.50	0.20	0.21
	80	−4.40	1.20	0.00	−2.05
SWEDEN	1978	−0.20		0.20	0.60
	79	0.20		0.20	−0.10
	80	−0.40		−0.08	1.67
U.K.	1978	0.00	−1.40	−0.10	−0.52
	79	0.40	−1.10	−0.20	0.12
	80	−1.00	−0.60	−0.10	−3.03
U.S.	1978	0.20	0.00	0.07	6.37
	79	0.30	0.10	0.00	7.06
	80	0.05	0.00	0.06	0.88

	Total Trade SITC 0–9	Real Trade SITC 0–9	Unit Value SITC 0–9	Unit Value SITC 0, 1	LINK GDP	LINK PGDP
1978	2.00	−0.80	2.90	24.90	−0.15	0.22
79	2.00	−0.10	2.10	20.70	−0.35	0.22
80	−1.10	−2.20	1.20	9.90	−0.67	−0.17

Discussion

John F. Helliwell

Prof. Klein's paper is an excellent exposition of the results from an important research project. The paper makes two very valuable contributions to the subject of this conference. On the one hand it assesses the price and output impacts of various international disturbances, and on the other hand it puts the history and models of a number of economies on a comparable basis, and thus greatly expands the information base available for our use.

The paper presents a lot of material in an admirably succinct way. At the beginning of the paper, Prof. Klein identifies nine actual or potential disturbances to the world economy. He then outlines the procedures used by Project LINK in combining econometric models of nations, regions, and commodities; and presents example results for the effects of higher export prices for developing countries (1974-76), lower oil prices (1974-76), higher oil prices (1978-79), capital transfers to the developing countries, and world harvest failure.

Before starting my detailed commentary, I would like to make a general comment on Project LINK. I have no doubt that Project LINK provides the most useful, best organized, and best documented explanations and forecasts of past and future evolution of the world economy.

The idea of linking national sources of expertise as well as national econometric models, and of doing so on a continuing basis with coordinated annual forecasts is remarkably daunting, especially to anyone who has had substantial experience in model building and use. I doubt that anyone else but Prof. Klein could have provided the necessary combination of scholarly prestige, technical skills, organizing ability, and diplomacy to make such a project work at all, let alone to continue developments and improvements over a period now approaching a decade in length.

In preparing my comments, I have been able to exploit the excellent documentation of Project LINK to focus the Project LINK models and forecasting experience on the issues facing this conference. Having read all of the papers prepared for the conference, I am inclined to pose three questions that seem to be common among them:

1. Do any models that are based on pre-1974 experience serve to satisfactorily explain the size and duration of the post-1974 stagflation?

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2. If not, are there any specific changes in model structure that would enable the experience of the middle and late 1970s to be better explained?
3. Finally, if one class of model can be demonstrated to have superior logical and explanatory power, what does this class of model suggest by way of policy improvements at the national or international level?

Prof. Klein's application of the LINK models does not address these specific questions, although the general tenor of his presentation presumes the basic validity of the underlying models and emphasizes the importance of higher oil prices in contributing to the high inflation and slow growth of the mid-1970s. I shall try to address myself more closely to the economic structure of the LINK system, in the context of the first two of the questions I have presumed to underlie the papers and discussion at this conference.

The excellent documentation of the LINK system allows an independent researcher, even one situated in a cabin on the far-off shores of Lake Huron, to assess how well the component models have dealt with the mid-1970s, and to examine model structure to look for clues that might explain the pattern of results. The primary sources, in addition to Prof. Klein's current paper, are the LINK forecasts for 1975 and 1976 by Klein et al [1976] and the individual models for 13 industrial countries contained in Waelbroeck [1976][1]. The forecasts, which were prepared at the end of 1974, embody the full extent of the 1973-74 increases in oil prices. To some extent the forecasts are not pure tests of model structure, as they involve forecasts of exogenous variables for 1975 and 1976, plus some exogenous adjustments designed to capture additional depressive effects anticipated in the aftermath of the oil crisis. It would now be possible, and it would certainly be worthwhile, to go back and recreate the same forecasts on an ex post basis, using actual values of exogenous and policy variables, and eliminating any other adjustments to model structure, in an attempt to see whether the actual post-1974 history is adequately depicted by the model structure. For the time being, the comparison of the ex ante forecasts with actual results will provide a valuable first test of whether the domestic and international transmission mechanisms of Project LINK capture the essence of the mid-1970s stagflation.

In the context of this conference, the question to be asked of the LINK models is whether their implied possibilities for growth and inflation are belied by actual experience in the mid-1970s. If there is systematic error, then the subsequent task is to see whether there are specific model improvements that might have helped to explain events rather better. Alternatively, the forecast record from the Project LINK models can be used as a standard against which to

[1] These forecasts were prepared at the end of 1974, and the model descriptions relate to roughly the same structures that were used to generate the forecasts. Also helpful are the papers by Johnson and Klein [1974] and Hickman [1974] presented to Federal Reserve Bank of Boston's 12th Conference in June 1974. Table 1 in Prof. Klein's current paper is drawn from Tables 5 and 6 in Klein et al [1976].

test the forecasting ability of other models based on different data or conceptions of how national economies operate separately and together.

Table 1 shows the forecast and actual percentage changes in real GNP (or GDP in several countries), consumer prices, and wages for 1974, 1975, and 1976 for each of the 13 industrial countries that were then represented by country models within the LINK system.[2] What is apparent from the table is that real GNP in general dropped more or rose less from 1973 to 1976 than was forecast by the models at the end of 1974. If we cumulate the three-year 1973-76 growth paths of forecast and actual growth of real GNP, the 1976 forecast level exceeds the actual level for 10 of the 13 countries.[3] For six of these ten countries the cumulative error is greater than 4 percent. One hypothesis (which is easily testable by re-running the models with actual values for policy variables) to explain this is that the oil-induced balance-of-trade deficits in many countries led them to adopt deflationary policies intended to restore their own trade balances but doing so, if at all, at the cost of lower real growth for the world as a whole.

However, this hypothesis does not square with the results for consumer inflation and for wage rates, which reveal that more inflation took place than could be consistent with the structure of the models and either the actual or the forecast values for real GNP growth. Only for Japan and the Netherlands were the actual (cumulated) 1973-76 inflation rates less than the forecast rates, although for the United States, Sweden, and Germany the cumulated error was about 2 percent or less. For the other eight countries the cumulated three-year error was over 4 percent in all cases, and averaged 8.6 percent for the eight countries.

Turning to the wage forecasts, only for Japan was the actual 1973-76 wage growth less than that forecast at the end of 1974, by an amount cumulating to 3.6 percent by 1976. For Sweden and the United States the 1974 forecasts for the 1976 wage level are almost exactly right, and for Austria, Germany, and the United Kingdom, the cumulative forecast error is about 3 percent or less.[4] For the remaining seven countries the cumulative forecast error (i.e., the excess of the actual 1976 wage rate over the forecast 1976 wage rate) averages 14.9 percent.

Figure 1 shows the pattern of forecast errors for real GNP, and Figure 2 shows the pattern for changes in wages. All of the changes, whether forecast or actual, are measured as the cumulative three-year percent change from the base year 1973 to 1976. For GNP, all of the observations are near or below the 45° line, showing the most of the LINK models overforecast real GNP growth. For

[2] The forecast changes are from Klein et al [1976, p. 9], while the actual changes are from International Financial Statistics. Especially for wage rates, the Project LINK series may not correspond exactly to that reported in IFS.

[3] The exceptions are Belgium, Italy, and the United States. For all three of these countries, as well as for Finland and Sweden, the cumulative error is less than 2 percent.

[4] For the U.K. model the wage rate is exogenous, so the U.K. result contains no information about model structure.

TABLE 1

Annual Percentage Changes, Forecast and Actual 1974–76

		Real GNP		Consumer Prices		Wage Rates	
		LINK FORECAST	ACTUAL	LINK FORECAST	ACTUAL	LINK FORECAST	ACTUAL
Australia	1974	5.4	2.5	10.3	15.1	13.6	22.3
	1975	2.6	1.7	9.6	15.1	12.1	18.5
	1976	2.7	3.5	9.5	13.5	11.4	14.5
Austria	1974	5.4	4.1	8.0	9.5	14.0	16.7
	1975	4.3	−2.0	5.4	8.5	11.7	13.4
	1976	2.6	5.2	4.7	7.3	9.9	9.0
Belgium	1974	3.5	4.9	9.9	12.7	10.9	20.9
	1975	1.6	−2.0	9.0	12.7	12.1	20.2
	1976	2.2	5.5	6.0	9.2	10.7	11.1
Canada	1974	6.2	3.7	14.5	10.9	17.5	13.5
	1975	4.9	1.1	6.1	10.7	9.2	15.7
	1976	6.1	4.9	3.5	7.5	6.8	13.8
Finland	1974	2.9	4.2	14.3	16.6	12.5	21.4
	1975	1.6	0.9	10.7	17.8	14.5	17.6
	1976	1.2	0.4	8.8	14.4	15.0	19.0
France	1974	4.9	2.3	16.7	13.7	17.1	19.2
	1975	3.9	0.1	7.0	11.7	11.0	20.3
	1976	4.4	5.2	6.6	9.2	11.1	16.5
Germany	1974	1.7	0.4	6.7	7.0	8.3	10.2
	1975	2.9	−2.5	2.9	5.9	4.8	7.9
	1976	3.0	5.6	5.6	4.5	8.2	6.4
Italy	1974	3.1	3.9	19.2	19.1	20.9	20.1
	1975	−1.6	−3.5	19.9	17.0	30.8	28.0
	1976	3.0	5.6	11.3	16.8	4.1	20.8
Japan	1974	−2.3	−1.2	25.2	24.3	27.6	24.8
	1975	5.9	2.4	13.4	11.9	19.1	16.9
	1976	7.9	6.0	8.3	9.3	12.1	12.6
Netherlands	1974	4.4	4.2	13.3	9.5	6.7	17.3
	1975	2.7	−2.3	9.1	10.3	8.2	13.6
	1976	4.1	5.2	8.0	8.8	7.1	9.0
Sweden	1974	4.0	4.0	11.0	9.9	15.2	10.8
	1975	2.4	0.9	10.5	9.8	14.1	14.9
	1976	2.0	1.7	6.6	10.3	13.9	17.5
U.K.	1974	−1.5	−0.6	16.9	16.0	17.2	17.9
	1975	2.8	−1.4	17.9	24.2	24.2	26.6
	1976	3.5	2.5	11.3	16.6	16.2	16.0
U.S.	1974	−0.8	−1.4	11.0	10.9	8.5	8.1
	1975	0.4	−1.3	7.9	9.2	8.2	9.1
	1976	2.9	6.0	5.5	5.8	7.4	7.7

Figure 1

1973-1976 CHANGE IN REAL GNP

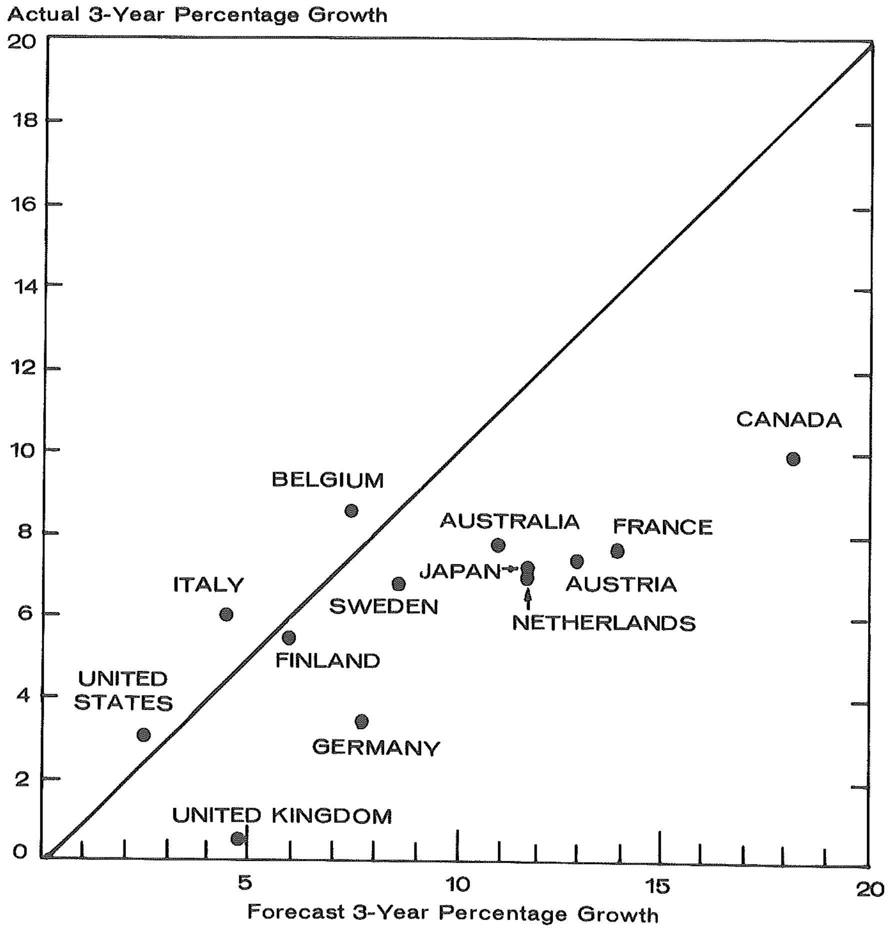
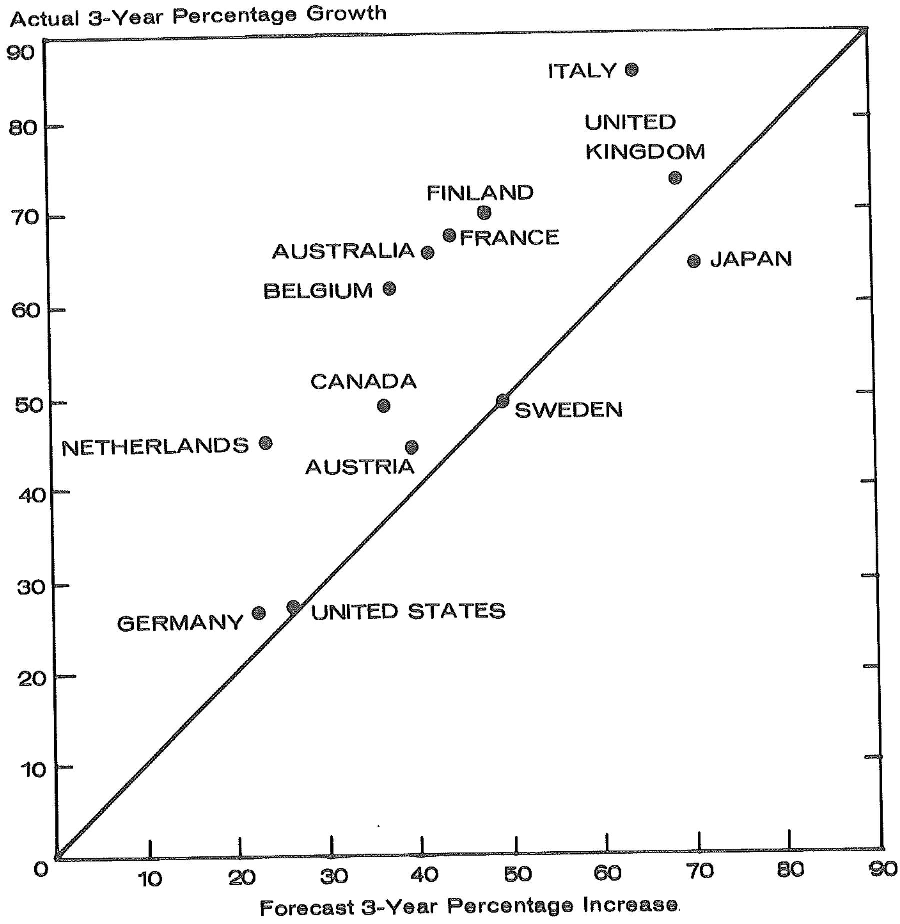


Figure 2

1973-1976 CHANGE IN WAGES



wage increases, all of the observations are near or above the 45° line, indicating that wage increases tended to be underforecast.

Figure 3 brings the wage and GNP forecasts and actuals together in an all purpose graph. Three-year wage changes are measured on the vertical axis, with 0 at the origin. Three-year growth of real GNP or GDP is measured on the horizontal axis, with 20 percent at the origin and going down as one moves to the right. Conventionally defined virtue is attained as one approaches the origin along either axis. The small circles represent the Project LINK 1974–76 forecasts for each of the 13 countries, while the asterisks represent the actual outcomes. The light lines with arrows connect the forecast and actual values for each country. Good wage forecasts are represented by arrows that are short in the vertical direction; good GNP forecasts by arrows that are short in the horizontal direction.

If there is any meaning to be attached to a cross-sectional definition of a Phillips-type relationship linking output growth and wage growth, then it can be defined in two ways: The circles define the cross-sectional frontier according to the Project LINK models with their assumed pattern of policies and external events, while the asterisks represent the observations based on what actually happened.

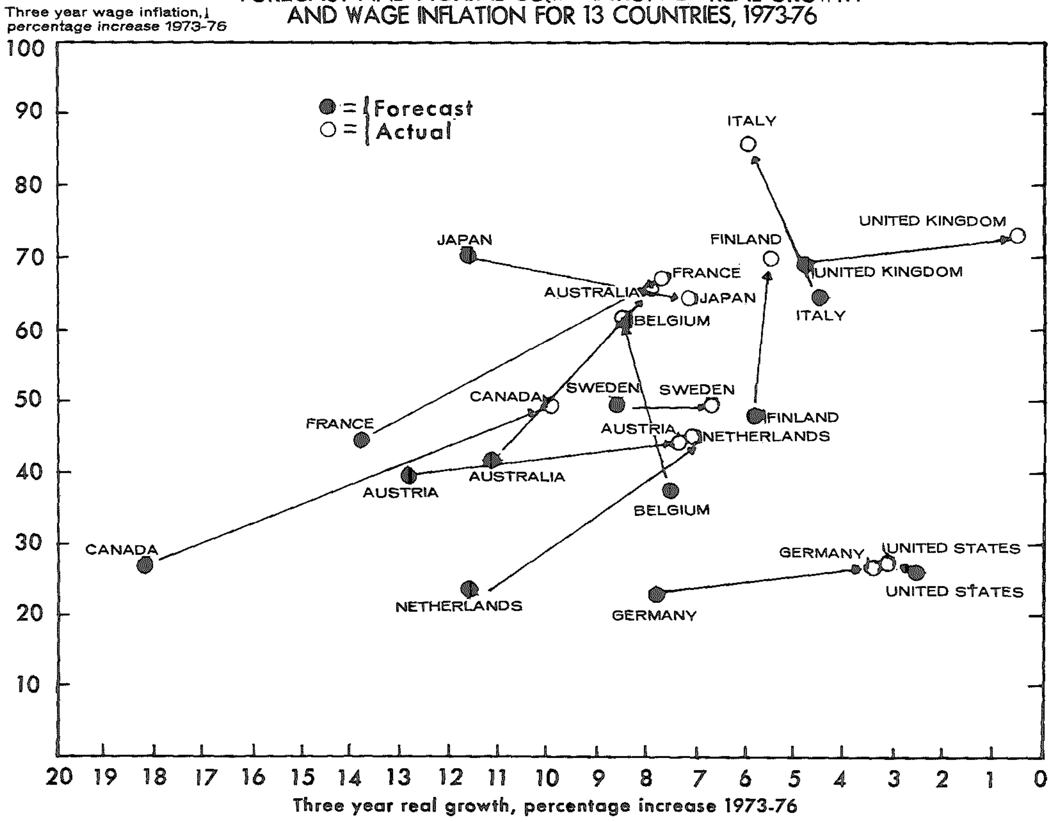
Neither the circles nor the asterisks represent a clearly defined frontier, although it is apparent that any curve that could be fitted would be further from the origin if fitted to the actual observations than if fitted to the model forecasts. Another way of putting this is that 8 of the 13 arrows point North-East, indicating that there was less GNP growth and more wage inflation than was forecast. Of the other arrows, that for the United States is so short as to represent almost perfect forecasting from 1974 to 1976: those for Sweden and Japan point South-East, with less growth of GNP and of wages; and those for Italy and Belgium involve more growth of GNP and of wages. None of the arrows point South-West towards the origin.

Hence we must conclude that most of the Project LINK national models, whose fitting periods generally ended between 1969 and 1971, had structures that were too optimistic about the possibilities for the 1974–76 period. If the forecasts had been made in 1973, then the over-optimism might have been due to the failure to consider the effects of the oil price increase, and not to the structures of the models themselves. As Prof. Klein's Table 1 shows, the Project LINK models would have shown markedly more growth in real GNP and less growth in wages and prices without the"excise tax" effects of the oil price increases of 1973 and 1974. However, the forecasts I have been examining were made after the oil price increases, and take them fully into account.

My next task is to examine briefly the structural characteristics of the models to see if there are important respects in which they might have understated the stagflationary effects of the oil price increases. If so, then it is possible that the oil price increases, when combined with the government and private sector behaviour as depicted in the models, could give a reasonably accurate

Figure 3

FORECAST AND ACTUAL COMBINATION OF REAL GROWTH AND WAGE INFLATION FOR 13 COUNTRIES, 1973-76



picture of the evolution of the major industrial economies through the middle and late 1970s.[5]

Before I proceed with that task, however, it is worth noting that the Project LINK forecasts did manage to capture the 1974-76 industrial recession and recovery, at least in their broad terms. Although the LINK models did in general overestimate growth and underestimate inflation between 1974 and 1976, their forecasts were far better than could have been obtained, for example, by simple extrapolation of previous trends. This is true whether one is interested in explaining world trends or intercountry differences. Looking first at the average experience of the industrial countries, the average 70-73 GNP growth was 16.2 percent (over the three years), the average LINK forecast for the 1973-76 was 9.3 percent and the average actual was 6.2 percent. For consumer prices, the LINK forecasts were even better, averaging 34.4 percent, compared to the 73-76 actual of 36.5 percent and the 70-73 actual of 21.4 percent. For wages, the average LINK forecast of 45.0 percent for 73-76 was less than one-third of the way from the 70-73 actual of 40.8 percent to the 73-76 actual of 56.1 percent.

Looking at intercountry variation, cross-sectional regressions of the actual three-year growth (of real GNP, wages, and prices) from 1973-76 were run alternatively on the LINK forecasts and the 1970-73 actual growth rates. In all cases the LINK forecasts explained more of the actual cross-sectional differences that did previous experience. The LINK forecasts were relatively strongest for real GNP and consumer prices. The LINK forecasts explained 56 percent of actual intercountry variance in actual 1973-76 growth rates of real GNP. By contrast, the 1970-73 actual figures explained only 22 percent of the 1973-76 intercountry variation. For consumer prices, the LINK forecasts explained 78 percent and the 1970-73 actual 51 percent of the intercountry variance for 1976 over 1973. For wages, the LINK forecasts explained 62 percent of the actual variance, while the earlier experience explained 49 percent. In addition, there was less average bias in the LINK forecasts as predictors, as their slope coefficient was in all cases closer to 1.0 than was the slope coefficient for the regressions based on previous experience.

I turn now to my second question, which asks whether there are specific changes in model structure that might have made the Project LINK models better able to handle the 1974-76 period. Following Prof. Klein's emphasis on international disturbances, I shall concentrate on the ability of the models to depict the consequences of the oil price increases.

The first and most obvious thing to note is that capital flows and exchange rates are not determined within the Project LINK system; and monetary policies, to the extent that they are modeled at all, are in general defined with interest rates or the money supply treated as exogenous. As a consequence, the

[5] Even here, there is the possibility that the 1974 forecasts already involved such heavy adjustments to the wage-price mechanisms of the national models that the correctness of the aggregate forecasts for wages, prices, and output would not provide any test of the aptness of the underlying model specifications. To check this possibility would require more information than is available to me.

models are incapable of showing the distribution or monetary consequences of the OPEC-related capital flows. With no modeling of capital flows, it is not possible to realistically model exchange rates; indeed, the asset approach to flexible exchange rates emphasizes how important it is to model capital flows and exchange rate expectations in a consistent manner. Although I would be surprised if the multilateral determination of capital flows and exchange rates were not fairly high on the Project LINK research agenda, the monetary repercussions of oil price increases and the resulting trade imbalances are for now handled in an ad hoc manner, with exchange rates set exogenously and then reassessed in the light of the trade imbalance implied by the Project LINK solutions for trade prices and trade flows. In general, one would have to conclude that the domestic and international determinants and consequences of monetary policy, including private sector expectations about future policies, are not adequately handled within the Project LINK national models.

Within the present structure of linkages, what are the various features of an oil price increase that might have important macroeconomic consequences for the oil-consuming countries?

1) A rise in the oil price implies worse terms of trade and lower real incomes and real money balances for the consuming countries. To model this correctly, it is necessary that the absorption price rather than the output price be used to deflate income and wealth in spending equations. In most of the Project LINK models this requirement is partially met by deflating disposable income by some measure of consumer prices.

   The treatment of real balances is less satisfactory; five of the models had no monetary sector at all, and six used the output price to define real balances. Only the U.S. and U.K. models used the consumer price to determine real balances and the real value of bank lending.

2) Even if the absorption price is correctly used to define real incomes, it is also necessary that the price of traded goods should influence the absorption price in some appropriate manner. Of the 13 models presented in Waelbroeck [1976], four do not contain any direct channel for import prices to influence the absorption price. These models, and several others in the Project LINK system, have absorption prices responding to current unit labor costs (rather than permitting current and normal unit labor costs to have separately estimated effects), and thus show temporary price increases in response to demand reduction.

3) If oil price increases lead to trade deficits that are financed by capital account inflows, then the accumulation of foreign indebtedness requires a continuing increase in foreign interest payments with a corresponding drop in GNP. Unless these increasing interest payments are appropriately modeled, the rate of growth of GNP for borrowing countries is likely to be seriously overstated. Of all the 13 models, only that for Japan depicts foreign interest payments in such a way as to show them rising with the size of foreign indebtedness. In all of the other models, foreign debt service

payments are either exogenous or modeled without proper feedback from the stock of debt to the flow of interest payments.

4) If net foreign interest and dividends are properly modeled, then it is also important to make endogenous the distinction between GNP and GDP. Gross national product is the income of residents, while gross domestic product is income produced by labor and capital employed within the national boundaries. Where net foreign indebtedness is changing fast, as in the aftermath of the oil price increases, then GNP and GDP can move rather differently, because net debt service payments to foreigners must be added back to GNP in the derivation of GDP. If this were not done, then the derived demands for domestic factors of production, which should be based on expected growth of GDP rather than GNP, would be falsely reduced. Only the Canadian model has the appropriate endogenous distinction between GNP and GDP, but it does not serve the intended purpose because the net interest payments are not properly based on the stock of foreign indebtedness.

5) Finally, there is the important question of how domestic wage rates respond to changes in the terms of trade. In the Scandinavian model of inflation (for a survey, see,e.g., Jorgen Gelting [1974]), the wage rate is set by productivity in the traded goods sector. In the present context of a terms of trade shift, and with emphasis on a rise in the price of an import with few domestic substitutes, the wage rate would remain relatively fixed in the face of the oil price increase. Most of the Project LINK models do not focus on the output price but on the consumer price as a key determinant of money wages. This procedure, relative to the alternative of using the output price or some similar measure of the marginal revenue product of labor, means that the domestic economy will incur more wage inflation and more unemployment. The reason is fairly obvious. If a deterioration in the terms of trade leads to an offsetting increase in the money wage, then the real wage will be above its equilibrium level and the levels of employment and output will be correspondingly reduced.

   It is not easy to decide whether the output or the consumer price ought to be the key determinant of money wages; in principle, especially at the industry level, both ought to have some importance. If, at the aggregate level, a choice has to be made, then the consumer price seems a better bet, if only because it produces more stagflation in response to an oil price increase, and the existing model structures have tended to underestimate the resulting degree of stagflation.

   The recent variability of the relative prices of traded goods, and hence in national terms of trade, gives rise to another hypothesis about the effects of international price disturbances on domestic wages and prices. It is at least possible that sequential favorable and unfavorable changes in the terms of trade have an upward ratcheting effect on domestic wages and prices. When the terms of trade improve, then domestic wages would rise to claim a share of the higher national income, as they are supposed to have done in

Australia during the mineral boom of the early 1970s. But when the terms of trade deteriorate, then emphasis may shift to achieving real wage protection, with consumer price increases used to define minimum increases in money wages. Such a ratcheting mechanism has no place in any aggregate model of wage determination, and its theoretical rationale is as weak as that of most ratchet models, but it may nevertheless have something to contribute to explanations of the continuing high rates of inflation and slow growth.

That is enough by way of discussion of the structural features of the Project LINK models. Although I have made a number of suggestions for improving the ability of the models to capture the repercussions of oil price increases, the suggestions I make are not such as to threaten Prof. Klein's main conclusion that the oil price increases have been an important source of the mid-1970s stagflation in the industrial economies. There are two reasons for my confidence that his basic conclusion is correct. First, my comments are based on the versions of the models used for the results reported by Prof. Klein in his Table 1. Several of the models have since been updated; and in several cases have built in more appropriate treatment of oil price increases. The results in Prof. Klein's Table 2, which are broadly consistent with those in his Table 1, are based on the updated versions of the models. Thus the changes made so far to the LINK system have been such as to strengthen the basis for Prof. Klein's conclusion. Second, almost all of the suggestions I have made are such as to increase the estimated stagflationary effects of an oil price increase, and hence would be likely, if implemented, to strengthen rather than weaken his main conclusion that oil price increases have been a key source of the post-1974 stagflation in the international economy.

Thus, my brief review of the LINK models and results tends to support the view that major increases in import prices, however they may be caused, tend to have stagflationary consequences for the importing countries. This does not, however, provide any direct evidence about, for example, the relative importance of monetary and nonmonetary causes of the world inflation of the 1970s, or about the origins of the increases in the prices of oil and other major commodities. The models of Project LINK could be used to provide one interpretation of these other issues, and the high standards of LINK documentation will permit the LINK view to be methodically tested and compared with explanations from models with different structure and emphasis. I must conclude by repeating my note of congratulation to Prof. Klein and his collaborators for their continuing focus on issues of great importance, and for their continual high standards of care and clarity in the documentation of models and forecasts.

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Anti-Inflationary Policies in a Democratic Free Market Society

Barry Bosworth

There is no way to say that the battle against inflation in this country is going well, because it's not. In the last few months, mainly because of food prices, the rate of inflation has accelerated dramatically. However, we don't expect that to continue for the rest of the year. Taking a longer perspective on the inflation problem in the United States for the past few years, remember that in 1975, 9 million people were unemployed; the next year 7 million people were still unemployed. By the most conservative estimates we have now reached the "fantastically low" level of 6 million people unemployed in this country.

In the 1960s it would have been unthinkable to predict a three-year period when unemployment averaged around 7 million people each year without any slowdown in the rate of inflation. I remember working on the staff of the Council of Economic Advisers for the Johnson Administration when the unemployment rate was 3 1/2 percent. We thought we could stop inflation if we just let the unemployment rate rise to 4 1/2 percent. We were wrong. In looking at forecasts to see what kind of an unemployment rate it would take to stop inflation, most economists found that our estimates were consistently too low. A look at the behavior of consumer and industrial prices and wages now makes it perfectly obvious that we are making no progress: the rate of price inflation except for minor fluctuations in food prices, has been a steady 6 to 7 percent year in and year out. Until the beginning of this year prices showed no real signs of acceleration, but it was clear there were no signs of deceleration either. On the wage side, despite the high level of unemployment, increases were running at a very constant 7 percent a year for money wages; if fringe benefits and other compensation increases were included, increases amounted to about 8 percent a year.

Our rate of productivity growth has now declined from 3 percent in the 1950s and 1960s to under 2 percent a year; thus we have had unit labor cost increases of 6 to 7 percent a year, almost exactly in line with the rate of price inflation. Now I think that the inflation has reached an equilibrium. The best possible forecast for the next three or four years is an inflation rate of 6 to 7 percent a year. Certainly it is not likely to be any lower. To be realistic, we must acknowledge that none of our forecasts ever anticipates OPEC oil prices, crop failures, commodity shortages, or any of the things that always happen to us but are never anticipated. The risks that inflation will be considerable higher than 6

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to 7 percent three and four years from now are a lot greater than the probabilities that it will be lower.

We must also take into account the fact that we do not like 6 percent unemployment, and we want to make further progress on this front. While we may not want to argue that the rate of inflation will dramatically accelerate if unemployment is reduced, certainly the pressure is in that direction. That too would add to inflation. So if we don't do anything about the problem of inflation, we have to admit that in the next few years we will have at least 6 to 7 percent inflation, and it could be worse.

If we do not regard that as an acceptable outcome, then we must look at what the alternatives are and what policies we can possibly use. The problem that we have always had is that everyone, not just economists, has his own idea of what causes inflation. Perhaps the best procedure to illustrate these ideas is to discuss our limited choices and to look at their consequences.

People frequently propose balancing the budget. Another proposal, which amounts to about the same thing, is to slow the growth of the money supply. Since this is mainly a financial group, I would imagine that such policies are very popular with you. I think we can say frankly that yes, if we balance the budget over the next few years and if we slow down the growth of the money supply, inflation could be brought to a halt. At the same time we must be absolutely clear about the consequences of such actions. Those consequences would be something in the order of 10 to 12 million people unemployed. I know of no econometric study or arguments by economists that say that the cost of price stability to aggregate demand restraint is a lower level of unemployment than that. Most of the studies that I have seen indicate that lowering the rate of inflation by 1 percentage point will take one million people out of work for at least two years. Even if we follow such a policy and achieve price stability, we cannot continue to run an economy day in and day out with 10 and 12 million people unemployed. So we would have to go back to some lower level of unemployment, and as soon as we did that, the expansion period would cause more inflationary problems. When we got back down to the unemployment level that we have today, then the same inflationary problem that we now have would recur.

In other words, I think we have to conclude that within the current institutional structures that we have in this country unemployment, or the fear of increasing unemployment, is just not a viable threat or a viable means of trying to stop inflation. By any definition, the social cost of such policies has become intolerable. For better or for worse the fear of unemployment is not very effective anymore in restraining price increases. I think there are various reasons for this, but the basic cause is that our economy just really is not that competitive anymore. Most Americans do not want a competitive economy. We once had high degrees of competition, with a system that was very effective in allocating resources. But at the same time it was a very cruel economic system. The books of Upton Sinclair and others probably exaggerate the situation in labor markets at the turn of the century, yet they are representative of the type of problems that most of the institutional changes since have tried to address. We changed the structure of our economy and attempted to make it more humane by dealing with its worst features and the cruelties of competition. But these changes have made the problem of inflation worse; we have given groups in society more and more discretion over their ability to set wages and prices. If we look at the way that labor markets operate today, there is almost no resemblance to the type of labor market that is described in Paul Samuelson's textbook on economics. It is not a competitive labor market, and the factors that determine wages seem to have very little to do with the fear of unemployment.

For example, recently we spent a considerable period of time looking at union wage negotiations and the patterns of long individual union settlements. We have a series on union wage increases and union contract negotiations that extends back into the 1950s. As far as I can detect, the magnitude of union wage negotiations and settlements shows no sensitivity to the unemployment rate. In the 1970 and the 1975 recessions union wage settlements actually were halved, but the magnitudes of the settlements increased during that period. Even adjusting for underlying factors, such as inflation and others that affected those wage increases, unemployment clearly had very little impact on large union wage negotiations. They followed and still follow a life and a pattern of their own. Basically, each union wants what the other union got last time around, so there is a pattern of ongoing settlements. If you look at the whole sequence of the teamsters', the auto workers', and the railroad workers' negotiations, you find the pattern of their wage increases will not vary from one to another by more than about 3 percentage points over the entire three-year contract. They all got almost exactly the same contract. It varies only slightly in terms of how much the wage increase is and how much the fringe increases are.

Many of these labor contracts are so structured today that they build in inflation. In looking at the automobile contract, for example, even before the negotiations open, the companies will owe the union a 27 percent wage increase over three years. This is due to financing the existing benefits of pensions, health care, and vacation days with no expansion, paying a cost-of-living adjustment and the annual productivity improvement factor that the contracts call for in their second and third years. So *many* of these union contracts just build in inflation. Trying to deal with this sort of situation by creating higher levels of unemployment is simply not an effective solution.

At the other extreme, another possible solution that is very attractive to some people is wage and price controls. Although we have no legislative authority to put on controls, we could probably get it in about 24 hours if we really wanted it. However, the administration is determined not to go to controls for another reason, and that is its real belief that controls are not effective. Controls are only a short-term device, and after a period of time people will find ways to avoid or get around them. We have learned that from past attempts. If there is one thing we should know by now, it is that inflation in this country is not a short-term problem. It is deeply imbedded in the nature of our institutions and it cannot be solved by a short-term period of controls.

Therefore, we continue to struggle to find some way to change the institutions to get lower rates of inflation. I think in the short run we are left with trying to do something in a voluntary program and trying to identify the real alignments of our institutions. How do we change the nature of labor-management negotiations to give people adequate incentives to hold down their wage and price increases without trying to use an incentive that has proved pretty ineffective, namely, increasing the fear of unemployment? I think the administration has finally begun a policy that will be effective in dealing with the problem, at least for the short run. That policy has four basic parts.

First, we must recognize that the government has become a major source of inflationary pressures, not by trying to create too many jobs or by running budget deficits, but by all the other things the government does. All the legislative actions which are responsive to special interest groups, the farm bill that came up a few months ago, and the sugar bill that is currently before Congress are examples. At times the list appears endless. It includes both the regulatory areas and some actions taken because we are afraid of the pressures of international competition. If you look at the last few years, those actions have had a very large and a very significant impact on the magnitude of inflation. For example, we estimate that in 1978 alone three government actions – the minimum wage increase, the Social Security tax increase, and the unemployment insurance tax increase – contributed 3/4 of a percentage point to the 1978 inflation rate. In the field of economic regulation, the most conservative estimates of the activities of EPA, OSHA, and other regulatory agencies suggest that they also account for about 3/4 of a percentage point in the inflation rate. Just that narrow range of government action represents about 1 1/2 percentage points of the overall inflation rate at the present time. So we are trying to figure out mechanisms to limit these actions. We can't promise that the government will never commit an inflationary act again, but we can pledge to commit fewer inflationary acts. We are trying to make major changes in the regulations.

In the past, government agencies regulated without analyzing the benefits or the costs. To clean up the environment, a standard was just set for pollution; the benefits over the costs of this standard were never measured. The administration now requires every regulatory agency to put forth an analysis showing the costs, the benefits, and any alternatives that the agency might have considered when proposing a regulation. To make sure that the agencies really take into account benefit-cost analysis in its final regulation, an interagency review group has been established which is supposed to examine the analysis to see if there is some less costly way to achieve the goal.

Earlier, I would have been quite hopeful that it was unnecessary to cut back on our regulatory goals, and I still think most of them can be achieved. The biggest waste comes from the inefficient way the regulations are carried out. Recently we had our first test of whether the administration would be able to deal effectively with that sort of situation. It was a regulation affecting the textile industry having to do with cotton dust emissions which cause a disease called byssinosis. Well, I have seen some bloody battles in my life, but an attempt to change a minor regulation in several minor respects became about the biggest internal political battle this administration has had in the last two years. We are now at the point where the President's attempt to say anything to the

Secretary of Labor about how a regulation ought to be promulgated leads to court suits because, although you may not realize it, a President's communications with a Secretary of Labor are "ex parte." They must be put on the public record; the public must have an opportunity to comment on them and to refute his statements of how the regulations ought to be enacted. Under the law apparently the President is not entitled to examine how OSHA regulations should be put into effect. That is left solely to the Department of Labor.

I think we do have to find some mechanism to budget Federal regulations and try to scale them down. The most important thing is to find ways to regulate more efficiently.

I am also trying to get businessmen to recognize that part of the inflation problem is their fault. They have to exercise more restraint on their cost increases, and undertake more of an obligation to hold down their price increases. To do that, we have asked each and every business firm to try to hold down its price increases below the last two-year average. On the whole, business firms have been quite responsive. In part, they are no doubt worried about the public implications of criticisms for not cooperating with the government and many of them are genuinely concerned about inflation.

Unfortunately, however, when we tried to point out a similar solution to labor, we made absolutely no progress. Probably our greatest difficulty at present is trying to come up with the means of restraining wage increases, particularly those for the large labor unions.

During past recessions, union wage increases tended to run ahead of and be slightly larger than nonunion wage increases. But they were never before at the magnitude of the last recession or of the magnitude they continue to be at present. Now, the very large unions like steel, autos, airlines, teamsters, railroad unions, and others are getting annual wage increases about 3 percent higher than what everybody else in the rest of the economy is getting. If we are going to slow the rate of inflation, we have to bring the wage increases of those very largest unions back in line with the rest of the economy. In the meantime, unless we achieve success in this area, as unemployment declines and labor markets strengthen, the nonunion sector and the smaller unions will naturally push to get wage increases that match those of the large unions. The differential tends to be closed, not by a slowing of the large wage increases, but by an acceleration of the magnitude of wage increases among the smaller unions.

In other words, the greatest difficulty in slowing the rate of inflation, particularly with respect to wages, is the ability of large unions (which are unresponsive to unemployment and continue year in and year out no matter how economic conditions change) to get wage increases far in excess of the rest of the economy. When good times return, and unemployment fears decline, other people push to get wage increases of equal magnitude. We are beginning right now to get considerable acceleration for the smaller unions and 10 percent annual wage increases are becoming more common. Clearly, without some way to limit the gains of the large unions, I think we will inevitably face a situation where the rate of wage inflation will accelerate over the next year or two and the inflation rate will gradually begin to worsen. One way to measure whether we are successful in that regard will be the outcome of the teamster and automobile negotiations next year. Because if the teamsters get 10 percent wage increases each year, then all of the big unions will demand matching wage increases. If we embark on another three-year cycle of 10 percent wage increases among the larger unions, the problem of trying to slow down the rate of inflation on the rest of the economy will be largely hopeless. The magnitude of the wage differentials and the accompanying inequities will have become so large that it will be almost impossible to restrain them.

Finally, we recognize that in this inflation there are some special problem areas – the health care sector and the housing industry are two examples – where the rates of price increases have been dramatic. Most Americans are terribly upset about the rate of health care cost increases and would like to do something about it. I would like to point out that we proposed a program to hold down the magnitude of hospital cost increases to the Congress 18 months ago, but neither the Congress nor the administration has made any progress whatsoever in getting that bill decided. It just takes an interminable amount of time for the government to make any change, even when most people seem to agree that there is a basic problem and something ought to be done about those health care costs.

The political processes of trying to get people to agree move so slowly that there is little hope of making the sort of dramatic changes in individual sectors that would have a sharp impact on the rate of inflation. Many people have said that this voluntary program we are trying won't work, and because it won't work, they are not going to do anything. Everybody sits back and waits for somebody else to take action. As a result the program has not had many positive results.

Assessing it sector by sector, the administration's concern about the inflationary implications of its actions shows its willingness to undertake a lot more restraint with respect to the government's regulatory decisions. The President did threaten to veto the farm bill and he has taken stronger action on similar issues than in the past. I also think we have had some encouraging cooperation from business, particularly the basic industries. For example, we have pledges from the steel industry to try to hold down their price increases to a reasonable level. We have a pledge from the automobile industry to cut the size of its price increases this year below those of the last two years. The aluminum industry has made a commitment to hold its price increases considerably below those of the last two years. There are also several other basic industries where individual firms aren't in a position to make a formal public commitment to price restraint, but their increases will be less than the average of the last two years. It appears that their prices are not going to rise as rapidly this year as they did in the past.

So in these basic industries I think we have had considerable support, but right now our problems lie mainly in the labor area, in trying to devise a meaningful formula that will be acceptable to labor. We need some means by which labor is willing to reduce the magnitude of wage increases in large union contracts and that puzzle will be the focus of our policies over the next few months. We will see whether it will be successful in those early contract negotiations of next year. That will be a turning point at least for *this* type of a voluntary anti-inflation program. If it is going to succeed, some signs of success will have to be visible by the first part of next year. Otherwise I think that if those negotiations start over again at their present high levels, then it is unlikely that this type of voluntary restraint approach to inflation can be a successful mechanism.

Institutional Factors in Domestic Inflation

Michael L. Wachter and Susan M. Wachter

During the 1970s the inflation and unemployment rates have tended to increase together. Although to some these developments have signalled the end of the Phillips curve view of inflation, we take the opposite position. By the Phillips curve approach, we mean a downward sloping short-run tradeoff between inflation and unemployment and a vertical long-run relationship which intersects the unemployment axis at U*, the nonaccelerating-inflation rate or sustainable rate of unemployment.[1] The coincidental upward movement in unemployment and prices, which gives the appearance of an upward sloping short-run Phillips curve is due to three developments:

First, due in part to demographic changes in the population, U* has been increasing since the late 1950s and this increase has continued through the 1970s. The rise in U*, however, has not been accepted by policy-makers who continue to press for lower unemployment rates using monetary and fiscal policy. The resulting overly tight product and labor markets have led to traditional demand-pull inflation. This is especially true during the periods 1965-1969 and 1972-1974.[2]

Second, as a result of a series of adverse exogenous shocks, such as the food and fuel shortages of the early 1970s, the economy has at times been forced off its short-run Phillips curve. To prevent the price increases in these sectors from being completely built into expectations, the government (especially the Ford Administration) opted not to ratify the inflation entirely. The result was short-

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[1] The exact shape of the long-run tradeoff is not important for our purposes. This is especially the case because the nature of long-run equilibrium is unclear in an economic model where institutional change is an important factor. For example, the long-run Phillips curve may be upward sloping rather than vertical. This type of relationship appears in, among others, Ross and M. Wachter [1973] and M. Friedman [1977].

[2] See M. Wachter [1976] for the construction of the U* series utilized in this paper. The overstimulative characteristic of monetary and fiscal policy is relevant to the product market and the GNP gap as well. See Perloff-M. Wachter [1979] for the development of a lower potential output series than that currently being utilized by the Council of Economic Advisers.

run bulges in the inflation rate accompanied by increases in the unemployment rate.

Third, due to long lags in the response of inflation to tight product and labor markets, and the longer duration of recent cycles, the synchronization between movements in the inflation and unemployment rates has been broken. In the late 1940s and early 1950s the business cycle was relatively short, alternating frequently between periods of market tightness and slack. Inertia or expectational errors on the upswing were cancelled out by the effects of the recession. Without a backlog of built-in inflationary momentum, changes in product and labor market tightness were quickly reflected in inflation rate changes. The economy fluctuated along a narrow band of downward sloping short-run Phillips curves.

With the long period of slack after 1958 and the subsequent period of tightness after 1965, the long lags began to build up momentum. In this context, fixed wage and price contracts resulted in serially correlated "errors" on the part of unions and firms. The momentum of, for example, the 1965-1969 expansion carried over into the mild downturn of 1970-1971. The cyclical effect of an unemployment rate slightly greater than U^* was overridden by the surfacing of inflationary pressures from the past cycle. Hence, inflation and unemployment increased together. The reverse example is provided by the recovery in late 1975 and 1976 when unemployment and inflation simultaneously declined. In this later case the major decline in the inflation rate took place after the unemployment rate had peaked.

In this paper we shall concentrate on this third factor. The rigidity in the wage and price mechanism is a response to the tendency of firms and labor unions to engage in contracting that fixes prices and wages, a process that we shall refer to as obligational market contracting.[3] The empirical manifestation of this phenomenon is long lags in the estimated wage and price equations. The main direct impact of the exchange arrangements of institutions on domestic inflation is thus to generate long, but variable lags in the inflation process.

Changes in the inflation itself, however, have important feedback effects on the institutions. More specifically, it is changes in the variance rather than the mean of the inflation rate which are likely to generate changes in the exchange relationships. But for the United States, where institutional arrangements have been structured on a near zero mean, low variance inflation rate, the increase in the inflation rate over the past decade has generated the increase in the variance. Since mean and variance effects are difficult to separate for the recent U.S. data, we shall simply refer to inflation effects on institutions.

Although largely ignored in the literature, institutional responses are a central component of the inflation process. That is, inflation can cause alterations in the method of obligational contracting, changes that imply long-run costs to the economy. In addition, the changes in the contracting mode, such as the adoption of escalator clauses, are geared to speeding-up the response of the

[3] The term is introduced and described in detail in M. Wachter and Williamson [1979]. It is summarized below.

micro actors to macro developments. That is, the lags grow shorter as the inflation rate increases.

Of prime concern are the policy implications of long lags in the inflation process. It is often argued that for the economy to climb down from its current and persistent 6 to 7 percent inflation rate would require an extended and perhaps a deep recession. Given long lags, the inflation spiral would indeed appear to unwind very slowly. In addition, the presumed nonlinear shape of the Phillips curve implies that downturns must be steeper than that original expansions which caused the upswing of inflation.

Indeed, to some researchers, this is an optimistic assessment of the problem. If the Phillips curve is flat (i.e., the coefficient on the excess demand variable is small and insignificant), then excess demand plays a minor role and the inflationary spiral cannot be stopped by running a recession. In this scenario, the onset of inflation is largely due to exogenous shocks from the international sector or from cost-push union settlements. The result is a wage-price spiral which then feeds on itself and is largely independent of aggregate demand management.[4] One way to break the inflationary spiral would be through deflationary exogenous shocks. Even this, however, seems unlikely. In part, because of political reactions, the exogenous shocks are not random with zero mean. For example, although poor harvests cause prices to rise, abundant harvests cause price supports to rise to mitigate any favorable deflationary effect. The upshot of this analysis often involves the call for wage and price controls as the mechanism for breaking the inflationary spiral.

In this paper we shall contest the view that the high inflation rate problem can only be resolved by a prolonged and/or deep recession or by wage and price controls. The institutional factors in the inflation process do give rise to long response lags. To interpret these lags as implying an ongoing wage-price spiral that is not affected by monetary and fiscal policy, however, is inappropriate. The inflation spiral necessitates validation by the monetary authorities. In this sense, the long lags may be the connecting link between past monetary growth rates and current inflation rather than between current and past inflation. Viewed in this framework, both the theory and evidence suggest two potential alternatives to the notion that a deep and/or prolonged recession would be required to slow the inflation process:

The first is that the institutional factors should adjust to higher rates of inflation by shortening the response lag. The growth of escalator clauses in union contracts and indexed price contracts for long delivery items provides an example of changes which shorten the lags. Empirical wage equations provide some econometric evidence for this proposition, although the results are not unambiguous. The coefficients on the wage equation now exhibit a higher first year response on the part of the inflation rate than they did prior to the 1970s.

The second is that the inertia or expectational elements that are the basis of the lagged response may be lowered, albeit very slowly, while the economy is

[4] See, for example, Okun [1975], Nordhaus [1976] and Perry [1978]. A similar point is made by Fair in this conference volume. One of the earliest and strongest statements of this position is Weintraub [1958].

close to U*. That is, a slowdown in the nominal level of monetary and fiscal stimulus, with a time horizon adapted to the length of the recontracting period in obligational markets, may be able to reduce inflation without leading to a severe recession. This program makes use of the widely accepted notion that over the longer run, at U*, inflation is approximately homogeneous of degree one in money growth.[5]

The economic engineering required to maintain the economy close to U* is difficult, particularly since the level of U* is a source of disagreement. The political problems of accepting a slow reduction in the inflation rate are undoubtedly great, but clearly less than the "prolonged and deep" recession which some researchers argue is implied by the data on the wage-price spiral. At worst, our argument is for "accepting" the current inflation rate and maintaining the economy close to U*. With some luck, however, the shifting coefficients and a reduction in the frequency of overstimulative monetary and fiscal policies may allow an unwinding of the inflation rate at much less real cost to the economy than anticipated by most inflation models.

Elsewhere, we have also stressed the use of structural labor market policies to reduce U*.[6] These policies should be a central component of an inflation policy and could supplement the anticipated favorable (for U*) demographic developments that should appear around 1980. A low growth economy, with a small, but positive U > U* has important income distribution effects. Improvements on the supply side of the labor market, aimed at younger and disadvantaged workers, can both neutralize the distributional effects of slow growth and lower U*, the nonaccelerating inflation rate of unemployment.

As of 1978, this proposed solution does not seem likely to be tried. Policy-makers are still reluctant to accept a U* as high as the approximately 5.5 percent figure indicated by the evidence.[7] On the other hand, little policy effort is directed at lowering U* through structural policies.[8] Instead, the economy seems headed in the short run towards tighter product and labor markets and thus a renewed and significant upswing in the inflation rate.

[5] It can also be based on the findings of the rational expectations model, but where the rationality properties are determining only over the length of the recontracting period in the obligational markets.

[6] M. Wachter [1976] and Wachter and Wachter [1978].

[7] Nor are policy-makers ready to accept the relatively low GNP gap which is associated with a 5.5 percent sustainable unemployment rate. See Perloff and M. Wachter [1979]. The notion that U* is at least as high as 5.5 percent is accepted by a diverse group of economists such as Cagan [1977], Hall [1974], and Modigliani and Papademous [1975].

[8] The main effort of policy is in direct job creation or public service employment. This type of program is part of the general fiscal stimulus package and has little chance to lower U* by improving the structure of the labor market. Experiments with new types of incomes policies or attempts to resuscitate old wage and price controls are also unlikely to have a positive effect on the long-run inflation and unemployment problems.

I. The Institutional Setting

Discussions of the institutional factors in inflation have historically been concerned with oligopolies and labor unions. Although these institutions, and especially the latter, are important components of an analysis of inflation, a broader conceptual framework is necessary. In particular, many industries which are not unionized or heavily concentrated display similar pricing and wage behavior. We refer to these industries as being in the obligational market sector.[9]

The impact of institutional factors on inflation should be divided into two separate issues; the rate of inflation itself and the mechanism by which inflation is transmitted through the economy. Our position is that institutional arrangements have a very important impact on the inflation mechanism, but little or no direct effect on the rate of inflation.[10] In this sense, the private market institutions do not impart an inflationary bias to the economy.[11]

At some stage, all ongoing inflationary processes must be accommodated by the money supply. Short-run fluctuations in inflation rates and real output can proceed with adjustments in the velocity of money, but in discussing the U.S. experience of rising inflation rates between 1965 and 1978, a rising rate of growth of the money supply is a necessary component.

A. *Obligational Market Contracting*

Since the basis of obligational market contracting is described in detail by M. Wachter and Williamson [1979], we only summarize its salient implications for our topic. Obligational contracting is based on the prevalence of ongoing exchange relationships between buyer and seller (including employer and employee) and the tendency for such relationships to involve idiosyncratic features. These contracts are to be found in final product markets, intermediate product markets, and labor markets.

An example of obligational contracting is found in the internal labor market of the firm. In this case specific training renders jobs within a firm different from similarly named jobs at other firms. The heterogeneity of tasks means that the incumbent worker (or supplier in product markets) has an advantage over outsiders in performance. This gives rise to a gap in the workers' current and opportunity wages and the firm's current and opportunity unit labor costs. The result is that the incumbent worker is not in an external labor market and

[9] In other treatments, derived from Okun's work [1976], the term customer markets is utilized. Although the underlying framework is somewhat different for these two concepts, the industries included are largely the same. Our treatment follows the usage adopted in M. Wachter and Williamson [1979]. The importance of transactions costs in the theory of inflation and unemployment is stressed by Gordon [1976] in his review article.

[10] Changes in the institutional arrangements are, however, likely to have an indirect effect on the inflation rate. As shall be argued below, changes in inflation uncertainty cause alterations in contracting modes. This imposes a cost to the economy which reduces the supply potential and is likely to increase U* as well.

[11] Okun [1976] argues the reverse case.

individual wages in the internal labor market are buffered, in the short run, from changes in labor market conditions. Since both parties have a pecuniary interest in maintaining the relationship, care is exercised to avoid a break. This often takes the form of an implicit (or explicit, especially where labor unions are involved) governance structure which suppresses opportunities for either side to maximize individual short-run gains at the expense of mutual long-run advantage. Any change in external market conditions which raises the question of a change in internal wages can lead to a problem as to whether a new internal wage should or does alter the sharing of the benefits of the ongoing relationship. To avoid these problems, wages adjust to changing economic circumstances only with a lag and mainly to reflect long-term rather than transient labor supply conditions.

That wages adjust with a lag does not mean that they are unchanging in the short run. Wages can move continuously, or more likely in short discrete jumps, but the *rules* which govern these wage changes are invariant to short-run market conditions and can change only when the (implicit or explicit) contract is subject to renegotiation.

Some flexibility to macro shocks is built into obligational market contracting through indexing. But the extent of this indexing is severely limited; real shocks are omitted entirely and nominal shocks are only partially indexed. The reasons for this involve the costs of writing and enforcing complex contingent claims contracts.

At the heart of this problem is "bounded rationality" which may be defined as the cognitive limits of human agents in relation to the complexity of the problems that they confront. Due to these cognitive limitations, economic agents may intend to be rational, but they can achieve only a limited rationality.[12] The result is that it is not possible to identify all future contingencies and to specify, ex ante, the appropriate response. In addition, even if a complete contract or index could be written, execution difficulties exist. In particular, there is the need to declare what state of the world has actually occurred at each delivery date. The result is the development of incomplete contracting with a governance mechanism to interpret what future events have evolved and what adaptations in the wage or price contract should be made.[13]

In the short run, or more precisely, over the length of the contract period in the obligational markets, the rules which determine wage and price increases are fixed. The incomplete indexing described above is part of the fixed rule structure. Within the framework, any unresolved disputes are handled through a governance structure whose primary purpose is to maintain the relationship and

[12] The notion of bounded rationality is developed in detail in Williamson [1975]. In the macro literature on expectations, B. Friedman [1978] has stressed the limitations of the rationality concept.

[13] The development of the implicit contracting literature circumvents, in part, the problem of writing a contingent claims contract. This literature, however, cannot deal effectively with the enforcement problems that arise in executing the implicit contract. Transactions costs and not differentiated risk aversion are at the heart of the problem.

thus the long-run gains of the parties rather than to achieve equilibrium with short-run market conditions.

B. *Implications for the Inflation Equation*

The obligational markets framework is consistent with the rational expectations model, but only over the long run. The bounded rationality of the economic agents weakens the sharp distinction drawn in the rational expectations literature between preannounced policy changes and policy surprises. In part, the cognitive skills of the micro parties, ignoring the skills of the policy-makers themselves, make it difficult to translate short-run macro announcements into the proper course of action. This is not a minor point: the assumption that preannounced policies will lead to market-clearing behavior in the near term is likely to prove erroneous. This does not rest on the notion that the micro parties form expectations irrationally; rather it means that there are cognitive limitations for translating preannouncement policy changes into appropriate micro responses.

In the short run, even if economic agents formed expectations rationally, they would be constrained from making short-run adjustments by the workings of the contract. The difficulties in dealing with preannounced policy changes in the short run are related to the reasons why incomplete contracting emerges in the first place; both are reflections of the need to economize on transactions costs becuase of the inherent cognitive limitations of individuals. Only for a time horizon that is long enough for full recontracting, can the price or wage contact be fully adjusted to a rational expectations view.[14]

What constitutes the long run is, of course, an empirical issue. The popularity of the three-year contract in the labor market provides some evidence that the effect of events three years in the past may not be fully incorporated into the system. Indeed, important lagged effects are likely to extend well beyond three years. As is well known, even when management and unions renegotiate a contract at the end of the specified period, the contract is, to an important degree, not fully renegotiated. Here again, this results from bounded rationality and the problem of monitoring whether either party is seeking short-run gains at the expense of the mutual long-run benefits. One example is the fact that escalator clauses tend to be introduced and strengthened slowly over several contracts. Frequently, when either party wants to alter an important implicit or explicit contract clause, the desired change is announced at one contract renewal and then pressed for adoption at a later contract renewal date. It is for this reason that the economy can anticipate the continued growth of escalator provisions, even if the inflation rate were to remain unchanged or even fall somewhat.

The growth of escalator clauses indicates an important feature of obligational market contracting: namely, that the inflation rate has a feedback effect on the precise methods of contracting. That is, the institutions or exchange

[14]The implications of the rational expectations view are developed by Lucas [1972], Sargent [1973], [1976] and Sargent and Wallace [1975]. For a debate on the implications of rational expectations in a contracting world, see Barro [1977] and Fischer [1977].

arrangements of the economy themselves are an endogenous element. For institutions structured on a near zero mean and low variance inflation rate, the emergence of ongoing inflation of 7 percent implies an increase in the variance as well as the mean rate of inflation. This generates the need for changes in the contracting mode. These changes are costly and the costs are not recouped even if the inflation settles down to a steady state. In short, there is a permanent loss in potential output as the economy moves to a higher (unanticipated) inflation rate. As important, however, is that the very uneven speed of institutional responses to inflation implies that achieving a new steady state inflation rate is an extraordinarily lengthy procedure which leaves the economy in a prolonged state of disequilibrium.[15]

The response of obligational markets to an (unanticipated) increase in the inflation rate is likely to be lumpy or discontinuous. The adoption of escalator clauses of varying degrees of complexity and completeness does not seem to proceed continuously with the variance of the inflation rate. In addition, even where wage rates (and prices) become fully indexed, there is no implication that the complete employment relationship has become indexed. Compensation as distinct from wage rates provides an important example. Fringe benefits and pension plans in particular, respond even slower to the increase in (unanticipated) inflation. The reason again involves bounded rationality and the difficulty of redesigning complex pension plans that involve incomplete vesting, annuities fixed in money terms in earlier contracts, and unfunded actuarial obligations.

Although there is considerable set-up time in establishing the escalators, after they are in place they will remain in place over a considerable upward or downward inflation range. Once the contracting mode is changed to allow for faster responses, monetary and fiscal stimulus translates more rapidly into nominal rather than real changes in economic activity. Although the economy is now less susceptible to monetary disturbances, it is more susceptible to real disturbances.[16] Given this exposure to increased fluctuations in real variables, the next step in the response of obligational markets may be to shorten the length of the contracting period. A move to shorter contracting periods is likely to be an exceptionally costly development.

II. Empirical Results

A. *Background*

The evidence of inflationary changes is usually obtained from inspection of wage and price equations.[17] The wage equation may be written in the form

[15] The Federal Government itself is one of the larger laggards in the adoption process. Although Federal pay scales and Social Security are, in effect, nearly fully indexed over a three-year time horizon, Federal tax rates and a whole series of Federal regulations are still based on the assumption of near zero inflation.

[16] This point is developed by Gray [1976].

[17] For an overview of this type of approach, see Tobin [1972].

(1) $\dot{w} = f_1 (UGAP, [\dot{w}_{t-i}], UNION)$

where $\dot{w}$ is the percentage rate of change in wages, $UGAP = U_N/U$ where U_N is a normalized unemployment rate which takes account of changing demographic and structural features of the labor market, $[\dot{w}_{t-i}]$ is a vector of lagged dependent variables and UNION is a "cost-push" variable(s) to take account of changes in the degree of unionization and/or militancy of the present membership. The U_N variable is an empirical approximation of U*. See, for example, Perloff and M. Wachter [1979].

Equation (1) is similar to those found in the literature with a few notable exceptions. Some other researchers include an array of unemployment, wage, and price terms rather than the two shown above. Although it is possible to improve the $\bar{R}^2$ of equations by this technique, there is no evidence to suggest that any one such equation consistently forecasts better than any other equation in this genre. An advantage of (1) is that it is simpler to decipher the impact of policy targets or instrument variables.[18]

A union pressure variable which is generally omitted from most equations is included here. Our purpose in including that variable, however, is only to highlight the fact that we are explicitly excluding it from the empirical results. As shall be discussed below, there is virtually no evidence that unions, either through growth in new unions or changes in power or militancy, have been a significant source of exogenous wage-push pressure in the post-1954 U.S. experience. This does not mean that unions, or even labor militancy in general, could never be a factor in inflation. Indeed, it is likely that some European countries over the postwar period, for example the United Kingdom and Italy, have experienced inflation pressure from the unionized sectors.[19]

Empirical price equations, available in the literature, tend to be of the form

(2) $\dot{p} = g(CAP\,U, [\dot{p}_{t-i}], [\dot{c}], [\dot{w}-\dot{\rho}], S)$

where $\dot{p}$ is the percentage change in prices, CAP U is the capacity utilization rate or some other demand variable, $[\dot{c}]$ is a vector of other price changes, e.g., materials, $\dot{\rho}$ is the percentage change of productivity so that $[\dot{w}-\dot{\rho}]$ is a vector of lagged unit labor costs, and S is a cost-push variable(s) to take account of supply shocks in the product market.[20]

The problem with (2) is that it is largely an accounting identity explaining prices as a function of cost increases. To the extent that it is not an identity,

[18] R.J. Gordon [1972] takes the alternative approach in specifying relatively complicated variables in the wage equation. For example, he includes a measure of the discouraged worker effect and differentiates between product and consumer price effects on wages. For the use of lagged wages in place of prices, see Hall [1974].

[19] For an analysis of labor unions in the European inflation experience see, for example, Laidler [1976] and Schelde-Andersen [1978].

[20] For discussions of aggregate price equations see Nordhaus [1972].

(2) is better viewed as a profits rather than a price equation. In many equations, the demand variable is insignificant and S is omitted because it cannot be quantified. Given a quick "pass-through" of costs into prices, all that prevents achieving an $\bar{R}^2 = 1$ is measurement errors in the cost and price variables and the cyclical behavior of profit margins. Since the cost variables are simply other prices, namely those of inputs, explaining prices as a function of costs does not deal with the inflation question. The cost equations would now become the inflation equations, unless they also are a function of $\dot{p}$ and other costs. Equation (2), however, is a reasonable way of taking account of cyclical changes in profit margins; that is, the difference between prices and costs.

In discussing changes in domestic inflation and, in particular, the institutional factors in domestic inflation, it is useful to concentrate on the wage rather than the price equation. Most important, prices are much more sensitive to international inflation than are wages. International factors affect domestic wages as well as prices, but their impact on prices is direct whereas their impact on wages is filtered through either domestic prices (if p were included in (1) in place or with $\dot{w}$) or labor market conditions (UGAP). The same argument holds for exogenous shocks in agriculture. In addition, the recent rash of government regulations concerning factors such as pollution control and product and occupational safety are likely to have a greater impact on prices than on wages. Unfortunately, these regulations are difficult to quantify. Some aspects of the new government regulation, e.g., occupational safety, are likely to alter wages directly as well as prices. We are currently attempting to study these effects on wages by decomposing recent shifts in productivity. Attempts to include a productivity variable directly into the wage equation have not yielded significant results.

B. *Wage Equation*

The results from our wage equation are shown in Table 1. In the standard equation (i) both UGAP and lagged wages are significant and the sum of the coefficients on $[\dot{w}_{t-i}]$ is close to unity. Whether the sum of the weights are somewhat above or below unity depends upon the exact specification of the independent variables and the length of the lag on $[\dot{w}_{t-i}]$. The $\bar{R}^2$ of the equation is 0.723. Adding a controls variable for the Nixon Administration's Phases I to IV results in equation (ii). Substituting lagged prices instead of wages results in equations (iii) and (iv). The qualitative nature of the results are the same among the four equations.[21]

The typical refrain in the literature is that equations such as (1) imply a wage-wage spiral in (i) and (ii) and a wage-price spiral in (iii) and (iv). The re-

[21] Given the limited variation in the data, one cannot determine whether the United States faces a wage-wage or wage-price spiral. There is some evidence that the food-fuel price explosion did not feed through directly to wages. On the other hand, it is possible that such a pass-through would have occurred if the Federal Reserve and Ford Administration had not opted for a recession in 1974.

latively small coefficient on UGAP, combined with the long lags on $\dot{w}_{t-i}$, implies a relatively small short-run payoff from running recessions to lowering the inflation rate. For example, using equation (ii), an increase in U of 2 percentage points to 7.5, given $U_N = 5.5$, implies an initial inflation reduction of 0.20 after one quarter. Thereafter the gains come even slower; that is, if U is maintained at 7.5 percent, the decline in inflation after one full year is 0.2830 and after two years is approximately 0.45.

The long and slow response of the wage or price inflation rate to a prolonged period of excess supply is found in virtually all wage and price equations similar to (1) and (2). The exact form of the demand variables and lagged wages and/or prices has little effect on the policy message.

For example, the Phillips curve is usually assumed to be nonlinear with an increasing elasticity for tighter labor markets. The empirical evidence, however, as shown in equation (i) of Table 1, cannot distinguish between the convex or linear Phillips curve.[22] Equations with UGAP or $UGAP^{-1}$ have nearly identical $\overline{R}^2$. But, given the range of UGAP over the postwar period, the difference in the inflation response to aggregate demand between the UGAP and $UGAP^{-1}$ equations is small. In any case, the UGAP coefficient only captures the short-run response of inflation. Shifts in the Phillips curve, embodied in Table 1 by the lagged wage or price terms, measure longer-run effects. For any significant change in the inflation rate, the speed and magnitude of shifts in the Phillips relationship are more important than short-run movements along the curve.

A second experiment is to measure the inertia term with lagged money supply changes in $[\dot{m}_{t-i}]$ instead of lagged wages and prices. Here again, the results are largely the same. The $\overline{R}^2$ is unchanged and the long lags are intact. Indeed, the mean length of the lag on money growth is larger than for either lagged wages or prices. This is shown in equations (vi) and (vii). The $[\dot{m}_{t-i}]$ equations, however, are open to a different interpretation than either the lagged wage or price equations.[23] This is discussed below.

That the American economy works with lagged responses surprises no one. Many large labor union contracts are for three years and this alone implies a certain rigidity to the system. Add to this the pervasiveness of obligational contracting in both product and labor markets and a long response pattern is guaranteed. But do long lags in setting relative wages and prices by the micro actors

[22] The potential importance of the nonlinear Phillips curve response to the overall inflation policy issues is stressed by Cagan [1977].

[23] For a skeptical view of the direct role of money growth in inflation equations, see Modigliani and Papademos [1975].

TABLE 1

Wage Equations, Quarterly 1954:1 – 1978:1[a]

Equation	Constant	UGAP[b]	NIXCON[c]	Lagged or Inertia Effect[d] $\sum_{i=1}^{24} X_{t-i}$	$\bar{R}^2$/DW
(i)	−0.7004 (3.65)	2.5764 (4.61)		1.1264 (12.41) $[\dot{w}_{t-i}]$	0.723/2.01
(ii)	−0.7068 (3.94)	2.8534 (5.40)	−0.0886 (3.40)	1.07766 (12.57) $[\dot{w}_{t-i}]$	0.758/2.14
(iii)	−0.2674 (2.03)	3.3619 (7.40)		1.0037 (13.01) $[\dot{p}_{t-i}]$	0.778/2.09
(iv)	−0.2838 (2.16)	3.4888 (7.55)	−0.03388 (1.34)	0.9876 (12.71) $[\dot{p}_{t-i}]$	0.780/2.01
(v)	0.6411 (4.50)	−0.1404[e] (5.46)	−0.0915 (3.92)	1.0372 (12.39) $[\dot{w}_{t-i}]$	0.759/2.20
(vi)	−0.2091 (1.48)	0.8368 (1.29)		0.9590 (13.33) $[\dot{m}_{t-i}]$	0.691/1.49
(vii)	−0.1909 (1.54)	0.9838 (1.72)	−0.1173 (5.24)	0.9454 (14.92) $[\dot{m}_{t-i}]$	0.761/1.90

[a]The dependent variable is the quarterly change in the average hourly earnings index. The variable excludes compensation.

[b]UGAP is the normalized unemployment rate divided by the actual unemployment rate, multiplied by 0.25 as a scaling factor; that is, $(U_N/U) \times 0.25$. The variable is described in Wachter [1976] and has been updated for this paper.

[c]The NIXCON variable represents the controls period of 1971:3 – 1975:2. It has a mean value of zero indicating that the controls had no permanent effect on the wage level.

[d]The lagged or inertia effect variables are constructed using a third degree polynomial, 24 quarters in length and constrained to zero at the end point. The lags begin with period t−1.

[e]The UGAP variable in equation 7 is in inverse form; that is $(U/U_N) \times 4.0$.

require that it takes several years to reduce wage inflation by 1 percentage point?[24]

There is no theoretical answer to how long are the long lags. In terms of the empirical evidence, however, there are some important reservations as to the validity of the exercise which yielded a one-half percent reduction in inflation after a two-year wait. This technique for calculating a steady-state tradeoff is incorrect. Two reasons for this conclusion are discussed in the remaining part of this section.

Instability of the Coefficients of the Wage Equation

The first issue involves the pervasive instability of the coefficients in the Phillips curve-type equation. This instability is not an accidental or fortuitous event, nor is it unique to the Phillips curve. The Phillips curves, like many macro equations, are simplified dynamic relationships that are meant to represent a complex economy. Changes in the nature of policy rules, in institutional arrangements, and in the distribution of excess demand across the disaggregated units are all likely to cause shifts in the wage equation. Since many of these underlying variables, such as the changes in institutional arrangements, and many policy variables are nonquantifiable, there is a necessity for continual updating of the parameters of the relationship.[25]

In some cases, a priori evidence suggests that the coefficients may be shifting in a systematic fashion. We argue that this is the case in the wage equation and the result is that the battle against inflation need not take as much time as suggested by the equations of Table 1. As discussed in M. Wachter [1976], the coefficients on either UGAP and/or the inertia term--whether $\dot{w}$, $\dot{p}$, or $\dot{m}$--have

[24] One explanation of the long lags involves measurement errors in both dependent and independent variables. The UGAP variable, although an improvement on U alone, is still a very imperfect proxy for excess demand. Since measurement errors bias coefficients towards zero, and since the coefficient on UGAP is the key parameter in the short-run tradeoff, the immediate inflation response may be greatly understated. An additional issue involves the wage and/or price variable; both essentially reflect a list price or average price concept. In this respect Stigler and Kindahl [1970] provided strong evidence that price inflexibility was overstated because of undercutting list prices during recessions. For labor, the tendency to layoff the lowest-wage workers imparts an additional source of rigidity to the wage data which reflect average earnings rather than wage rates. Both the direction of the measurement bias and its quantitative importance are unknown.

[25] Often equations can be "patched-up" by introducing new variables to explain past shifts. Our experience, however, is that this will not improve out-of-sample predictions; each shift seems to require a new variable. The alternative is to reestimate the equations with some frequency. With shifting parameters the "simple" equations often predict as well as the "patched-up" equations with new variables. We doubt that this is a question of "Keynesian" vs. rational expectations-type equations. Either school should find it difficult to isolate stable structural forms given the limitations of the data and the complexity of the true relationships.

been increasing over the period of rising inflation.[26] Since $\dot{w}$ and $\dot{p}$ can be interpreted as distributed-lag generators for UGAP in the wage equation, increasing values on these terms imply an increasing impact of aggregate demand. In equations where UGAP and $\dot{m}$ appear, there is less difficulty in identifying the aggregate demand effects on the rate of wage inflation.

In a sense the UGAP term and the coefficients on $[\dot{w}_{t-i}]$ for low i can be interpreted as the short-run effect. As shown in Tables 2 and 3 the coefficients on UGAP and the sum of the coefficient weights over the first year on $[\dot{w}_{t-i}]$ or $[\dot{m}_{t-i}]$ imply a stronger short-run reaction in 1978 than existed in the middle 1960s.[27] The short-run aggregate demand multiplier is approximated here by $\alpha/[1 - \sum_{i=1}^{4} \beta_i]$ where α is the coefficient on UGAP and the denominator is the sum of the first four weights on the inertia effect.

The changing coefficients of the wage equation reflect the changing economic environment of the sample period. For the initial sample period, 1954–1960, of steady inflation and short, shallow business cycles, the $\dot{w}$ term can be largely explained by the constant term of the equation. The coefficient on U/U_N is very small (in absolute value) and is actually negative on $[\dot{w}_{t-i}]$. The initial lags on $[\dot{w}_{t-i}]$, however, are positive, yielding a tiny but positive multiplier $(\alpha/[1 - \sum_{i=1}^{4} \beta_i])$ of 0.0857. Expanding the sample period through 1964:4 results in a more traditional Phillips curve, with a wage inflation response multiplier of 0.0980.

An additional major change in the wage equation occurs when the period of high inflation and low unemployment is included. Adding the years 1965 through 1969 results in an increase in the coefficient on $UGAP^{-1}$ from −0.0665 to −0.1302 and on $\sum_{i=1}^{4} \beta_i$ from 0.3214 to 0.5270. The inflation response multiplier nearly triples from 0.0980 to 0.2753. The second period of rising inflation ends in 1974:4. The wage equation estimated for 1954:1 through 1974:4 yields an increase in the long-run coefficient on $\dot{w}$ to 1.0320. The short-run inflation

[26] Given the sluggish adjustment of obligational markets, an increase in the inflation rate implies an increase in the variance of the inflation rate around the fixed rate built into the obligational contracts. The policy significance of shifting coefficients in the wage equation are emphasized by Lucas [1972] and Fellner [1976]. For evidence, across countries, that economies with greater variation in inflation rates have shorter lags see S. Wachter [1976]. Shifting parameters in employment equations, as a function of policy, are emphasized by Baily [1978].

[27] This confirms the findings in M. Wachter [1976] where a different econometric approach to this question was utilized. That is, the equation was estimated for the entire sample period with a fixed coefficient on $[\dot{p}_{t-i}]$ and a variable weight on UGAP. With that constraint, the coefficient on UGAP increased over the period.

multiplier rises moderately to 0.3135. During this second period of rising inflation, although the short-run response of inflation to unemployment increases slightly, the major change is in the long run, with the emergence of a vertical Phillips curve.

The differences between the 1954-1960 and 1954-1974 equations are dramatic. In the shorter period, the offsetting nature of the expansions and recessions resulted in a nearly constant rate of wage inflation, with seemingly little regard for short-run demand conditions. By 1974:4, after a decade of rising inflation and generally tight labor and product markets, the sluggish response of obligational markets in setting wages generated significant and quantitatively large values for the long lag terms in the $[\dot{w}_{t-i}]$ distributed lag.[28] At the same time, the obligational markets were forced to react faster and in a more systematic way to the fluctuations in demand. The adoption of escalators in three-year contracts helped to provide larger weights on $[\dot{w}_{t-i}]$ for low i.

Adding the last three years, a period of loose labor markets, results in a largely unchanged Phillips curve. The coefficients on $UGAP^{-1}$ and $[\dot{w}_{t-i}]$ are insignificantly different for 1954:1 – 1978:1 compared with 1954:1 – 1974:4. It can be argued that the 1974 to 1978 period was marked with two offsetting factors, while the continued adoption of escalator clauses and other devices acted to speed the responsiveness of the system, the unanticipated food-fuel-controls shocks helped to maintain the appearance of long lags.

The wage equations with the money supply growth as the inertia factor, also exhibit a substantial increase in the short-run responsiveness of wage inflation. The lags on $[\dot{m}_{t-i}]$, as mentioned above, are weighted towards the high values of i, even relative to the $[\dot{w}_{t-i}]$ equations. The trend towards a greater wage response to aggregate demand, in this case directly represented by money supply growth, is not only pronounced, but also continues through the most up-to-date sample period.

The pattern of changing coefficients in Tables 2 and 3 indicates the potential for a dramatic speed-up in the estimated inflation responsiveness of the system. First, a new period of excess aggregate demand would almost certainly cause a shift in the coefficient weights toward the front of the lag structure. The currently estimated long lags would be very costly for obligational market firms to maintain at higher inflation rates. Second, even without an additional increase in the mean and variance of the inflation rate, there should still be a forward shifting of the lag weights in the wage equation. New data, in a stable inflation environment, would allow the escalator clauses that were adopted in the previous period to be reflected in the coefficients. That is, the incremental data to the wage equation would no longer be affected by the slow response to the

[28] If the 1954-1960 period had been marked by the longer business cycles of the latter period, but with the same mean inflation rate, the long lags on $[\dot{w}_{t-i}]$ would probably have been significant. In a heuristic sense, the long lags on $[\dot{w}_{t-i}]$ were present even in the earlier period, but could not be measured econometrically because of the particular time path of the economy.

real food-fuel-income policy shocks of the 1970s. The predominance of surprise effects, as occurred in 1974–1978, "artificially" raised the mean length of the lag.[29]

The Impact of Money Supply Growth: The Systematic and Residual Components

The second problem concerns the variables included in the wage and/or price equations. In terms of equations (1) and (2), we are dealing with a mixed type of equation that is not meant to be a structural equation, yet it does not contain any exogenous or policy variables. To derive an empirical Phillips curve from a general macro model, it is possible to settle on a myriad of different forms along the continuum of purely structural to purely reduced form equations.

We have introduced the money supply inertia equations, shown in (v) and (vi) of Table 1, as one alternative to the wage-wage or wage-price spiral view of the world. That is,

(3) $\dot{w}_t = f_2\,(\text{UGAP}, [\dot{m}_{t-i}], \text{NIXCON}).$

Our intent is not to argue that the former is the true causal mechanism, but that the wage-wage or wage-price spiral presents a one-sided picture which may severely understate the effect of aggregate demand on wage inflation rates.

As mentioned above, there is little to choose from among these equations in terms of $\bar{R}^2$; the wage-wage, wage-price, and wage-money supply connections fit about the same. In fact, in comparing Tables 2 and 3 it appears that the wage-money supply variant does slightly better than the wage-wage spiral. The comparison of $\bar{R}^2$, however, is of limited interest. First, we obviously are not attempting to obtain the wage equation with the highest $\bar{R}^2$. Since the Phillips curve is a hybrid structural-reduced form equation, a test based on highest $\bar{R}^2$ would involve including numerous other variables. The resulting best reduced form equation would vary considerably depending upon the time period and the computer resources of the researcher. It is likely that the resulting equation would contain all of these variables in a complicated package.[30] For our purposes it is useful to unscramble the reduced form equation so as to isolate the lagged m equations from the lagged $\dot{w}$ or $\dot{p}$ equations.

The lagged $\dot{m}$ in equation (3) replaces the wage-wage or wage-price spiral with a wage-money supply spiral. The lagged $\dot{m}$ equations do confirm some aspects of the alternative equations. The lags on $\dot{m}$ are very long, indeed the lag to 50 percent completion is over three years when the polynomial lag structure

[29] Of course, whether or not a forward shift actually takes place would depend upon a host of other factors including the expectations of future policy actions and the government pronouncements of future macro targets.

[30] For a discussion of macro models based on reduced form specification see Sims [1977]. His results support the importance of a direct linkage between $[\dot{m}_{t-i}]$ and $\dot{w}_t$.

TABLE 2

Wage Equations for Different Sample Intervals
Lagged Wage Change as the Inertia Variable

Sample Interval	Constant	U/U_N[a]	$[\dot{w}_{t-i}]$[b]	NIXCON[b]	$\bar{R}^2$/DW	$\sum_{i=1}^{4}$[c]	$\alpha / (1 - \sum_{i=1}^{4})$[d]
1954:1 – 1960:4	1.6121 (2.27)	–0.0775 (1.82)	–0.2313 (0.52)		0.414/2.03	0.0961	0.0857
1954:1 – 1964:4	0.5890 (1.86)	–0.0665 (1.64)	0.6180 (2.99)		0.353/2.01	0.3214	0.0980
1954:1 – 1969:4	0.8837 (4.07)	–0.1302 (4.60)	0.7287 (4.35)		0.627/1.90	0.5270	0.2753
1954:1 – 1974:4	0.6127 (3.19)	–0.1332 (4.41)	1.0320 (9.87)	–0.0959 (4.03)	0.735/2.20	0.5751	0.3135
1954:1 – 1978:1	0.6411 (4.50)	–0.1404 (5.46)	1.0372 (12.39)	–0.0915 (3.92)	0.759/2.20	0.5401	0.3053

[a]The $UGAP^{-1}$ variable is scaled by multiplying by 4. This makes it approximately equal to the unemployment rate in mean value over the sample period.

[b]Same as Table 1.

[c]$\sum_{i=1}^{4}$ is the sum of the weights on the lagged wage term, $\dot{w}_{t-i}$, for the first four quarters.

[d]The approximation of the short-run response of wage inflation to aggregate demand is calculated as the coefficient on $UGAP^{-1}$ divided by $[1 - \sum_{i=1}^{4}]$.

TABLE 3

Wage Equations for Different Sample Periods
Lagged Money Supply Changes as the Inertia Variable

Sample Interval	Constant	U/U_N[a]	$[\dot{m}_{t-i}]$[a]	NIXCON[a]	$\bar{R}^2$/DW	$\sum_{i=1}^{8}$[b]
1954:1 – 1960:4	1.5414 (3.62)	–0.1179 (2.12)	–0.1333 (0.43)		0.469/2.06	–0.4374
1954:1 – 1964:4	1.4040 (3.73)	–0.0954 (1.93)	–0.1209 (0.44)		0.446/2.04	–0.4697
1954:1 – 1969:4	0.8601 (2.59)	–0.0754 (1.64)	0.7904 (3.93)		0.589/1.47	0.1065
1954:1 – 1974:4	0.5752 (2.74)	–0.0395 (1.15)	0.9855 (11.16)	–0.1224 (5.24)	0.738/1.86	0.1495
1954:1 – 1978:1	0.5950 (3.47)	–0.0428 (1.36)	0.9525 (15.11)	–0.1175 (5.26)	0.761/1.89	0.1978

[a]Terms are defined in Table 1.

[b] $\sum_{i=1}^{8}$ is the sum of the weights on $\dot{m}_{t-i}$ or i = 1 through 8.

is ended after six years. This is also coupled with only a low and marginally significant coefficient on UGAP.

The differences between equations (3) and (1), however, are important. In the wage-wage model there is at least the implicit notion that the aggregate demand authorities can do little to influence the inflation rate. The equations give the appearance of an inflation process that simply builds upon itself with an occasional, but small, spike from changes in the unemployment rate. The wage-money supply equations highlight the active influence of aggregate demand forces. The autoregressive $\dot{w}_{t-i}$ terms are capturing the relationship between lagged wages and lagged money. In this framework, active control of $\dot{m}$ dominates future fluctuations in the inflation rate.

Given the fact that the inflation spiral can be significantly broken by aggregate demand policies, the next question is whether such a process requires either a long or a deep recession. Since UGAP is related to $\dot{m}$, it is not possible to arbitrarily choose numbers for these two variables without inspecting the cross equation restrictions which delineate the potential UGAP and $\dot{m}$ tradeoffs. Can $\dot{m}$ be reduced by the Federal Reserve in such a fashion as to have largely nominal rather than real effects?

The evidence on this point is mixed. Most macro models have a built-in, reduced-form relationship between U and $\dot{m}$, suggesting that changes in m are related to wide swings in U and much smaller fluctuations in p. More recent work which distinguishes between systematic and unsystematic changes in m imply a different result. For example, Barro [1977] using annual data, has suggested that only "surprises" in the $\dot{m}$ series cause changes in U, but can persist for three years.

We have conducted a limited series of tests on the notion that systematic monetary growth has little impact on unemployment. We stress that these tests are meant to be conjectural in nature and to suggest that there are alternatives to the pessimistic wage-price spiral view of the world where aggregate demand has little role in the inflation process. Using quarterly data for the period 1948-1978, we have differentiated between the systematic money supply growth, designated DMA, and the residual factor, designated DMR. A few series were constructed on the general form

$$\text{(4)} \qquad \dot{m} = f([\dot{m}_{t-i}], [\text{Trend}]),$$

where $[\dot{m}_{t-i}]$ is a vector of lagged dependent variables and [Trend] is a vector of trends raised to various powers.[31]

The specific equation utilized below is of the form

[31] This differs somewhat from Barro's [1977] specification of the equation including the fact that we utilized quarterly rather than annual data. It is clear that the specification of (4) requires considerably more work than could be attempted here. We have developed this line of inquiry to be suggestive.

$$\text{(5)}\quad \dot{m} = \underset{(0.70)}{0.7612} + \underset{(7.93)}{0.6992}\,\dot{m}_{t-i} - \underset{(1.59)}{0.1823}\,\dot{m}_{t-2} + \underset{(0.71)}{0.0798}\,\dot{m}_{t-3} - \underset{(0.86)}{0.0793}\,\dot{m}_{t-4}$$

$$- \underset{(0.69)}{0.9381}\,T + \underset{(1.05)}{0.4446}\,T^2 \qquad \bar{R}^2 = 0.556 \qquad D.W. = 1.99$$

The predicted values of (5), designated DMA, are the systematic money supply increase. The residuals from (5), designated DMR, are the deviations in money supply growth.

Our equation is intentionally a simpler form than Barro's money equation. This reflects unresolved issues in the dichotomy of $\dot{m}$ into surprise and anticipated components using the methodology proposed by Barro. We have chosen to create the DMR and DMA series without utilizing cyclical variables in (5). Defined as a function of trends and autoregressive terms, the DMR and DMA variables are less sensitive to changes in the specification of the equation. The result is that positive values for DMR reflect only above trend and autoregressive growth rates of the money supply. By construction, therefore, they can only differentiate between stable (DMA) and high or low deviations (DMR) from the money supply growth rates.

The residuals of (5), DMR, by definition, capture the periods when the Federal Reserve is altering the money supply trajectory from its trend and autoregressive path. Whether these "above or below average" increases are expected or not is unclear. It is clear, however, that the positive (negative) residuals represent the short-run peaks (troughs) in monetary stimulus. In this sense, it should not be surprising, even to traditional Keynesian macro forecasters, that these periods of unusual positive or negative monetary activity feed more directly into unemployment.

Introducing DMA and DMR from equation (5) into an unemployment equation for the period 1954:1 – 1978:1 results in the following:

$$\text{(6)}\quad UGAP = \underset{(2.90)}{0.02843} + \underset{(16.05)}{1.3483}\,UGAP_{t-1} - \underset{(5.45)}{0.4578}\,UGAP_{t-2} + \sum_{i=1}^{20} \beta_i\,DMA_{t-i}$$

$$+ \sum_{i=1}^{20} \gamma_i\,DMR_{t-1} \qquad \bar{R}^2 = 0.956 \qquad D.W. = 2.06$$

The weights for β_i and γ_i are shown in Table 4. No attempt was made to experiment with the length and specification of the lag structures.

Equation (6) and the weights in Table 4 suggest that the stable component of the money supply growth (DMA) has no significant effect on explaining unemployment. The sum of the lag weights on DMA has the wrong sign and is quantitatively close to zero. On the other hand, the residual or deviation element of money supply growth (DMR) is significant and positive in the initial few

TABLE 4

Weights for UGAP Equation

t-i	*Coefficient*	*T-Statistic*
UMA (−1)	.277E-02	.72
UMA (−2)	.586E-03	.16
UMA (−3)	−.104E-02	−.30
UMA (−4)	−.217E-02	−.66
UMA (−5)	−.287E-02	−.95
UMA (−6)	−.317E-02	−1.23
UMA (−7)	−.315E-02	−1.52
UMA (−8)	−.285E-02	−1.87
UMA (−9)	−.234E-02	−2.17
UMA (−10)	−.167E-02	−1.63
UMA (−11)	−.900E-03	−.63
UMA (−12)	−.823E-04	−.04
UMA (−13)	.725E-03	.28
UMA (−14)	.146E-02	.48
UMA (−15)	.208E-02	.62
UMA (−16)	.251E-02	.72
UMA (−17)	.271E-02	.80
UMA (−18)	.262E-02	.86
UMA (−19)	.217E-02	.90
UMA (−20)	.132E-02	.95
UMR (−1)	.550E-02	2.07
UMR (−2)	.591E-02	2.04
UMR (−3)	.604E-02	1.68
UMR (−4)	.592E-02	1.43
UMR (−5)	.558E-02	1.30
UMR (−6)	.507E-02	1.22
UMR (−7)	.442E-02	1.18
UMR (−8)	.366E-02	1.16
UMR (−9)	.283E-02	1.17
UMR (−10)	.196E-02	1.11
UMR (−11)	.108E-02	.71
UMR (−12)	.244E-03	.13
UMR (−13)	−.526E-03	−.20
UMR (−14)	−.119E-02	−.35
UMR (−15)	−.172E-02	−.44
UMR (−16)	−.207E-02	−.48
UMR (−17)	−.222E-02	−.51
UMR (−18)	−.212E-02	−.53
UMR (−19)	−.174E-02	−.54
UMR (−20)	−.104E-02	−.55
	SUM OF WEIGHTS	T-STATISTIC
UMA (−1)	−.129E-02	−.22
UMR (−1)	.356E-01	1.89

periods. The long-run coefficient on DMR has the right sign and is significant at the 95 percent confidence interval.

The sum of the lagged UGAP terms in (6) is close to unity. This suggests that a positive monetary residual leads not to a once-and-for-all decrease in unemployment, but rather to an ongoing decline in the unemployment rate. This is offset, however, by the fact that the weights on DMR for high t-i are negative. The result is that the quantitative size of the coefficients on DMR is quite small. In any case, in the long run, a "continuing" positive deviation in monetary growth would not lead to a continuing reduction in unemployment since the private sector would presumably adjust to the money supply growth rule. The surprise would cease to be a surprise or, in other words, the average adjusts to the new higher growth rate. But, as was true in the Barro model, the lags in adjusting to surprises are very long and there is considerable room for monetary policy to alter the unemployment rate.

Different specifications of equations (5) and hence the DMA and DMR terms of (6) lead to somewhat varying results. At times, the results for equation (6) were insignificant for both DMA and DMR. It is worth noting, however, that we found no situations where DMA was significant and positive and where DMR was insignificant and negative. That is, depending upon the specification of (4) and the resulting definition of DMA and DMR, the results supported an insignificant DMA with a tendency towards the incorrect sign and a DMR which was most often positive and significant for the initial weights of the lag structure.

If the residuals (DMR) of the money supply equation largely affect UGAP, then it should be expected that the predicted values (DMA) should largely impact on wage inflation. This, in fact, is the case. For the period 1954:1 – 1978:1 we find

$$(7)\quad \dot{w} = \underset{(0.44)}{-0.0964} + \underset{(2.07)}{1.5140}\,\text{UGAP} - \underset{(4.45)}{0.1072}\,\text{NIXCON} - \underset{(0.35)}{0.1361}\,[\text{DMR}_{t-i}]$$

$$+ \underset{(11.19)}{1.1741}\,[\text{DMA}_{t-i}] \qquad \bar{R}^2 = 0.741 \qquad \text{D.W.} = 1.75$$

where DMR and DMA are estimated with third degree polynomial lags of 20 quarters of duration, constrained to zero at the end point. The coefficients on DMR and DMA are the long-run coefficients.

The implications of equations (4) through (7) for inflation control are suggestive. Given a long enough time horizon in adjusting monetary growth rates, a slower monetary growth rate can directly yield a lower inflation rate without necessarily requiring an extended period of high unemployment.

Lowering inflation without a recession would not be a simple task and it certainly would take perseverance. To simplify the task somewhat, set aside for the moment the difficulties in controlling monetary aggregates in offsetting exogenous shocks in the private sector that could cause cyclical changes. Given the long lags in the wage and money supply equations (equations of Table 1 and (6) and (7)), it would take several years before the inflation rate would begin to slow. Since the wage inflation equation has a lag structure on $\dot{m}$ that is higher near the end than at the beginning, the inflation rate might even grow for several years before beginning to subside. But the possibility remains that inflation could be reduced over time without a prolonged recession.

III. Inflationary Bias

In our discussion of obligational markets in Section I, we argued that institutional arrangements have an important impact on the mechanism through which inflation is transmitted through the economy, but not a significant direct role in causing inflation. In this section we elaborate on this point.

That the private institutions in our economy are viewed as an important source of inflation is indicated daily by official public statements from various levels high in the Federal bureaucracy. In the academic literature, this position is argued on the basis of an "inflationary bias" in the private wage and price setting mechanisms.

The strawman position on inflationary bias is that oligopolists and labor unions persistently drive up prices and wages irrespective of aggregate demand. This research, which surfaces in some of the public policy literature, ignores the necessary role of validation by the monetary and fiscal authorities. For example, in the U.S. experience with ongoing inflation over the past 12 years, monetary expansion (with the likely encouragement from fiscal policy) must be part of the inflationary process.

The legitimate debate over the source of inflation and the role of institutions in the inflation mechanism begins by accepting the notion of a wage-price-money supply spiral. A wage-wage or wage-price spiral obviously cannot go very far for very long on its own. Within the monetary validation context, the inflationary bias argument is pursued along two basic lines: The first is based on the presence of certain key sectors, where wage and price decisions are made largely independent of aggregate demand. These decisions then "spillover" into the rest of the economy. The private wage and price decision makers have a higher tolerance for unemployment and unutilized capacity than the monetary and fiscal authorities so that the private decisions are essentially validated by monetary expansion. The second line is based on the notion that wages and prices have greater upward than downward flexibility. In this situation any important change in relative wages or prices necessitates general inflation to bring the relative wage and price structure back into equilibrium. Although originally stated in terms of levels, the argument can be recast in terms of inflation rates by adding an expectational mechanism with a larger and faster upward than downward response pattern.

A. *The Wage (Price) Leadership Case*

The issue of wage leadership was debated in the industrial relations literature in the 1950s.[32] In this model a few key unions, largely ignoring market

[32] For a comprehensive discussion of these issues and an updating of the evidence see Burton and Addison [1977] and Mitchell [1977].

conditions, would negotiate settlements that would then become the "pattern" for other unionized industries. As a consequence of the "threat effect," this pattern would be adopted by the nonunion sectors. In a similar fashion, certain key industries in the input-output array could have an excessive impact on overall prices. The wage (and/or price) leadership model is particularly receptive to arguments for wage (and/or price) controls. A central argument against controls is administrative feasibility. In this case, by close enforcement of a few large offenders, the wage leaders, the inflation process can be controlled at low administrative cost and with little interference to the market in the great bulk of industries.[33]

The wage leadership model has empirical problems. Whereas the model seems to argue for a fixed group of leaders, the labor relations research has not been able to isolate such a group. Rather the leadership role, to the extent that it can be identified, shifts over time. This raises the question of whether the so-called key industries are simply those that, at a given period of time, are enjoying the most favorable excess demand positions. More generally, there is the question of whether the observed spillovers represent similarly shared excess demand conditions rather than an institutional mechanism of wage leadership. The econometric research to date, controlling for aggregate demand forces across industries, finds little evidence of institutional spillover.[34]

The Wage Contour Approach

An alternative to wage leadership is the wage contour approach.[35] In this case, the unionized sector itself is segmented into a series of contours. Wages are set in each contour or segment with substantial independence from the other contours. There is some degree of spillover among contours and between the union and nonunion sectors, but this receives less emphasis. An important variant of this model allows for the possibility of wage contours outside of the union sectors. These contours are formed along product market lines and are national or regional in scope depending, in part, upon the scope of the product market.

A major difference between the wage leadership and contours models is that the loci of decision-making is narrow in the first and relatively broad in the second. The greater the number of segments or contours, the more decentralized the wage decision-making process. In addition, market forces are viewed as having a larger role in the wage-setting process within contours.

Our view is that the wage-contour model is similar in spirit to the obligational market framework that we presented above. These wage (and price) contours define the institutional arrangements that exist as a result of established

[33] Presumably, the jaw-boning approach to wage controls is based on this view of the economy.

[34] One of the best econometric papers to focus specifically on spillovers is by Mehra [1977]. Burton and Addison [1977] and Mitchell [1977] provide a broad summary of the evidence on spillovers.

[35] See Dunlop [1957] for an original formulation of the problem.

relationships between buyers and sellers and among competitors. As we have stressed elsewhere, this version of labor (and products) markets is antithetical to a general wage and price controls policy. Although there is a range of indeterminancy in wages and prices in these markets, the observed wages and prices reflect the allocation of transaction cost savings. Macro tampering with relative wage and price structures can cause serious efficiency and allocation loss. That is, the existence of a micro discrepancy between own wages (prices) and opportunity or market wages (prices) does not indicate slack in the system that can be costlessly erased by macro controllers. Furthermore, in the relatively atomistic obligational market-wage contour framework, it is more difficult to argue that a few key private decisions establish a pattern for exogenous inflationary pressure.

The "appearance" of cost-push inflationary pressures arises because of the fact that obligational markets, of which unions and oligopolies are an important subset, tend to have above average wage and price increases during recessions. This however, is the lagged response of obligational markets to excess demand forces and not an independent source of inflationary pressure.

As shown elsewhere, the relative wage structure within manufacturing varies in a systematic way over the cycle.[36] A new development since this earlier work has been the widespread adoption of escalator clauses. This should lead to a difference in the response of relative wages to inflation prior to and after 1970. The result is confirmed in equations (1) – (3) of Table 5 where $\dot{p}_{1947-1969}$ is the inflation variable for the 1947-1969 period (zero elsewhere) and $_{1970-1977}$ is the inflation variable for the 1970s. Whereas higher inflation as well as tighter labor markets would reduce the wage spiral prior to 1970, escalator clauses now enable the high wage unionized sector to keep up with inflation. Deviations of U from U_N now cause the most significant and systematic changes in the real wage structure. In short, indexing has helped to buffer nominal but not real fluctuations in the economy.

Although the complex lagged behavioral response of obligational markets is not a cause of inflation, it may give rise to policy errors which can result in additional inflation. The tendency of the monetary and fiscal authorities to misinterpret lagged wage and price increases as cost-push can lead to inappropriate responses. A prime historical example was the widespread view that unions and oligopolies were engaging in cost-push inflation during the 1970-71 recession; a position which led to adoption of wage and price controls to restrain the "bad actors" while the monetary and fiscal authorities pursued an expansionary policy. On the other hand, the lags in the obligational markets encourage the monetary and fiscal authorities to overshoot supply constraints during expansions. For example, the sluggish response of union wages and oligopoly prices during the late 1960s misled the monetary and fiscal authorities to understate the inflationary pressures that were building in the economy.

[36] See, for example, M. Wachter [1976]. Before the widespread adoption of escalator clauses, the coefficients on $\dot{p}$ were significant over the entire sample period, for example, 1947 through 1973. The few degrees of freedom after 1973 suggest some skepticism in interpreting the division of the inflation variable into pre- and post-1970 components.

TABLE 5

Coefficient of Variation Equations, Annual 1947 – 1977[a]

Equation	Constant	$\dot{p}_{1947-1969}$[b]	$p_{1970-1977}$[c]	UGAP[d]	KD[e]	NIXCON	$\bar{R}^2$/DW
1	0.2226 (33.21)	−0.4370 (3.67)	0.1555 (2.44)	−0.1898 (5.87)			0.921/0.995
2	0.2188 (41.76)	−0.2249 (2.07)	0.2113 (4.06)	−0.1861 (7.37)	−0.0140 (3.80)		0.952/1.644
3	0.2223 (43.95)	−0.1872 (1.86)	0.2713 (4.95)	−0.2057 (8.31)	−0.0125 (3.64)	0.0028 (2.21)	0.960/2.128

[a]The dependent variable is the coefficient of variation, which is constructed as an unweighted average of the 21 manufacturing industries in the SIC. (The original series are adjusted for overtime. Since no overtime earnings series is available for printing, the industry is omitted.)

[b,c]The variable [$\dot{p}$] is the rate of change of the CIP. The [$\dot{p}$] variable is divided with the first term, superscript b, equal to $\dot{p}$ through 1969 and zero elsewhere. The second term, superscript c, is equal to $\dot{p}$ 1970 through 1977 and is zero prior to 1970. For both b and c, third degree polynomial, five-year length of lag, and constraints zero at $t = -4$ were imposed.

[d]Third degree polynomial, five-year length of lag, and constraint zero at $t = -4$ were imposed.

[e]Korean War Dummy. It has a value of one for 1950–1953 and zero elsewhere.

B. *Asymmetry Between Upward and Downward Relative Wage Movements*

The asymmetry argument has been stressed by a number of economists.[37] A useful and broad statement of the problem is given by Duesenberry [1975]:

> In fact, however, there appears to be an asymmetry in the response of prices and wages to market imbalances. Price makers who have market power tend to increase prices more readily in response to cost increases and strong demand than to decrease them in response to cost reductions or weak demand. In competitive sectors, producers often successfully appeal to government for protection when competitive forces tend to drive down prices. There is clearly great resistance to absolute reductions in wages even in the face of high unemployment. Beyond that there are strong wage linkages so that upward demand pressure on wages in one labor market can pull up wages in related markets with no labor shortage or even a surplus. These tendencies are, of course, fortified by trade unions – strong defenders of wage linkages – and by the market power of employers which permits them to recoup increased costs by raising prices. The result of this inflationary bias is that the average price level tends to rise even when there is substantial underutilization of resources.

There is evidence of this asymmetric pattern in the relative wage data. One major indicator is that whereas a few industries have succeeded in achieving an improvement in their relative wage position, none have suffered a significant decline. Specific examples over the past two decades are Federal Government wages, state and local wages (to a lesser extent), contract construction, and transportation. Most recently coal and steel wages have pulled ahead. All of these industries have experienced improvements in relative position in excess of cyclical factors.[38]

An examination of the characteristics of the industries involved in these relative wage increases is informative. First, many have had large increases in employment during their period of increasing wages. Government employment and contract construction are examples. In both of these cases, however, their recent employment growth has become depressed, partly in response to their wage changes. This, in turn, has led to a reversal in relative wage trends. Second, a number of these industries are subject to government regulation and/or controls which act to buffer or support high levels of employment. Government employment is the most obvious example. The role of government regulation in construction (Davis-Bacon) and in transportation, as well, as the special labor

[37] One of the earliest treatments is by Schultze [1959]. More recently the topic has been explored in depth by Tobin [1972].

[38] The product market data are less clear. Whereas there have been some chronic inflationary sectors, for example, health care, a number of industries have experienced dramatic price declines. The high technology area is the most often cited example of relative and even absolute price declines of considerable magnitude.

legislation in this latter area, appears to be important.[39] Third, short-run factors in collective bargaining, such as the experimental no-strike clause in steel, and the increased product demand and intra-union political problems in coal, are factors in creating relative wage gains in these area. Barring government interference with international trade, steel wages will probably fall somewhat back into line at the next negotiating round. Mining wages, on the other hand, may continue to grow if demand for eastern coal continues to increase. Mining is a clear example of an "inferior" occupation that requires increasing relative wages to attract additional workers.

The purpose of this brief review of the relative wage problem areas is to suggest a potential remedy for the problem. In general, to the extent that asymmetric upward movements in wages (or prices) are the source of the inflationary bias, an appropriate government anti-inflation response would be to adopt a sector-specific approach to the problem areas. That is, rather than reacting with generalized wage and price controls (whether in the form of freezes, jaw-boning or tax-based controls), the sector-specific issues should be addressed. This type of approach is not novel and has long been advocated by labor specialists such as Dunlop [1966], [1977].

A sectoral approach has a number of advantages. First, it saves on transaction costs (bounded rationality problems) which render all general control schemes largely unworkable. Second, it has the equity advantage of dealing only with those sectors where relative wages are showing very large increases. Third, it can be easily turned on and off since the bureaucratic apparatus is small and there should be no post-controls wage explosion. This is especially true since, as indicated above, the upward relative wage movements have self-limiting properties. The purpose of the sector approach is to reinforce the market forces which tend to hold the relative wage structure together. Finally, and most important, if done properly, it can actually deal with the true underlying inflationary biases.[40] It recognizes that overall controls are no solution to an overexpansionary monetary and fiscal policy mix.

IV Conclusion

We have argued that the emergence, during the 1970s, of rising inflation coincidental with rising unemployment is due to three factors: a persistent failure on the part of the Federal Government to recognize the supply constraints of the economy (i.e., the rise in U* and the associated decline in the growth rate of potential output); exogenous inflation shocks stemming from OPEC oil increases, a series of poor harvests, and an ill-conceived wage and price

[39] For a systematic treatment of structural problems in the construction industry, see Mills [1972]. He places less stress on Davis-Bacon as a problem area.

[40] See M. Wachter [1976]. The overall controls program suffers from attempting to hold down general wages and prices while the government is pursuing an overly expansionary policy.

controls program; and finally, long lags in the obligational markets in the response of inflation to aggregate demand pressures. This paper focuses on the latter issue.

In the early post-World War II period, the business cycle was relatively short. Expectational errors and/or inertia effects had no chance to accumulate, so that increases in excess aggregate supply and unemployment were quickly reflected in decreases in inflation. After the long period of slack between 1958 and 1964, followed by a decade of near uninterrupted tightness (or very mild recession), serially correlated expectational errors and inertia effects became built into the obligational markets. Consequently, the mild period of slack between 1970–1971 did not reduce the inflation rate: catch-up inertia effects dominated the upward course of the inflation rate. Hence, unemployment and inflation increased together. Only after the prolonged and deep recession of 1974 to 1977 did the long lags in the obligational markets allow the downward push of unemployment on inflation to outweigh the now reduced lagged effects of past inflation. By this point in the cycle, however, the recovery had already begun. Markets were still slack, but a declining unemployment rate coincided with a declining inflation rate.

For institutional contracting modes, based on a near zero inflation rate, the emergence of ongoing inflation of 6 to 7 percent implies an increase in the variance as well as the mean rate of inflation. This requires costly changes in the contracting arrangements, costs that are not recouped even if the inflation settles down to a new steady state. The potential output of the economy suffers at least a once-and-for-all reduction. The highly uneven speed of institutional responses to inflation is a lengthy procedure which leaves the economy in a prolonged state of disequilibrium. Indeed, the "benefits" of a high pressure, low unemployment economy can be generated, in part, because of the ponderous adjustment process of the obligational markets.

But once the economy has managed to build in a 6 percent or higher rate of inflation, what solutions can be offered? We suggest the following points: First, with the widespread adoption of escalator clauses and other such devices, obligational markets may now respond more rapidly to excess demand than they did a decade ago. We believe that the empirical wage equations overstate the length of the lags.

Second, if a long enough time horizon is adopted, fitted to the workings of obligational contracting, policy-makers using relatively stable rules may be able to translate a program of reduced money supply growth rates more directly into inflation rather than real output changes. That is, there is at least the potential of slowing inflation while maintaining the economy close to its U* or potential output constraint.

The evidence to support the notion of a slowly declining inflation rate, while maintaining the economy close to U*, is conjectural. In addition, the issue raises theoretical and empirical problems. Will a democratic government be able to adopt a planning horizon which is long enough to allow for recontracting? How will real shocks be accommodated? Holding to a targeted slow reduction in money supply growth rates, in an environment with real shocks, can lead to substantial fluctuations in real output in the short run. Can fluctuations in U* be measured and used to retarget general monetary and fiscal policy?

Given these problems, the "fall-back" position is to accept the current inflation rate and attempt to at least equalize the odds between upward and downward changes in the inflation rate. A clear policy commitment to the twin goals of using monetary and fiscal policy to avoid overheating the economy coupled with avoiding a further acceleration in the inflation rate is needed. With equally weighted policy goals between maintaining U* and nonaccelerating inflation, exogenous shocks may actually become random with zero inflation mean. That is, not every potential downward price supply shock would be met by special legislation to maintain prices.

Third, the asymmetric tendency of relative wages (and prices) to move ahead in some sectors, without corresponding declines elsewhere, should be handled by sector-specific policies and not general controls. Often these relative wage (and price) adjustments are due to changes in the collective bargaining structure, government regulations (unconnected with inflation policy) and/or longer-run changes in relative demand and cost conditions. In these cases, sector-specific, anti-inflation policies can provide a mediation or arbitration role with respect to the institutional arrangements that prevents or buffers the relative wage or price changes from occurring or from feeding into inflationary expectations.

Fourth, policy should be devoted to reducing U* and raising the growth rate of potential output. These can only be accomplished through structural, supply-side policies. The issue of structural policies aimed at U* and potential output, however, are beyond the scope of this paper.

The changes in the economy brought about by a decade of increasing inflation have a pessimistic side. The micro actors in the obligational markets are still relatively sluggish, but the speed of their response to the next period of tight product and labor markets will be different than it was in the inflation of the 1960s. The increasing responsiveness of inflation to excess demand pressures and the associated more "direct" feed-through of money growth rates into inflation suggests the potential for rapidly rising inflation in the coming decade without the benefit of relatively low levels of cyclical unemployment.

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Discussion

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Before I comment on the specifics of the Wachters' paper, let me say a few words about rational expectations. It is generally useful to know what a model that assumes rational maximizing behavior has to say about an economic issue. But that does not mean that such models are always the best ones for the purposes of prediction or policy formulation. Of course one can often rescue rationality by specifying imperfect information or the costs of decision-making, but that is not the point. Good models are those that are based upon empirically supported stable behavioral rules, whether or not such rules can be rationalized. Such rules are hard to find, but then so are good models.

To assume that all expectations are totally rational is a very strong assumption. Critics of its use are usually told that their criticisms are analogous to those made in the past of the assumption of utility maximization. Now I am not sure that even this assumption has been validated all that overwhelmingly. The evidence that individuals rationally allocate consumption so as to maximize their lifetime utility is quite weak. Prior to the existence of the Social Security System many persons saved almost nothing for retirement and deeply regretted their decision in later life. One can define such behavior as rational on the grounds that whatever people do is what they want to do, but that makes the theory a tautology.

But suppose we accept, as I basically do, that utility and profit maximization are useful assumptions to make. This does not say people have rational expectations. Remember that argument by analogy is not proof. The testing of individual subjects has shown that people frequently fail to follow efficient strategies in stochastic environments. It is very hard to make rational decisions under uncertainty, if being rational includes using stochastic information efficiently. Not only that, but advocates of rational expectations take the breath-taking additional step of assuming that people have an intuitive grasp of the whole economic system. They go well beyond any assertion that people know what they like.

Let me now turn to the Wachters' paper. In answer to the questions posed by this conference: what do we do after the Phillips curve? The Wachters give the following answers. The Phillips curve should not be abandoned, they argue. It is basically stable, but people haven't been looking at it correctly and it has been knocked around a little by food and fuel. On the policy implications, they suggest we should hang in there with a high unemployment rate, and try to

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convince the private sector of the seriousness of our intentions in order to bring down the expected rate of inflation as fast as possible.

On the stability of the Phillips curve, they suggest that the rate of unemployment consistent with no acceleration of inflation is much higher than had been thought – closer to 6 than to 5 percent at the present time. If this is correct, it means that during periods we thought were deflationary the labor market was actually in equilibrium or even in a state of net excess labor demand. I am not sure if they are correct about this; the evidence is murky. But I fear that there is indeed a lot of structural unemployment around and, if so, it points up for me an urgent need for measures to help the disadvantaged workers, the hard-to-employ and teenagers in order to lower the long-run sustainable unemployment rate. I would also advocate reform of some of our income security programs, although not the abandonment of these programs.

The next main point they argue concerning the Phillips curve is that the lag in the response of wages to demand conditions is very long indeed. This means, for example, that the period of low unemployment in the late 60s was still pushing wages in the early 70s. The overwhelming inertia or momentum in the inflationary process is indeed impressive. Inflation accelerated very slowly during the mid-60s and has been dampened very slowly indeed by the mid-70s recession.

The insensitivity of the rate of wage inflation to short-run variations in aggregate demand is, of course, a basic tenet of post-Keynesian macroeconomics, a tenet strongly reinforced by the experience of the 70s. Explaining this insensitivity, or the inertia in wage inflation, has been a major issue for decades. The Wachters look for an explanation in economic institutions. They emphasize long-term fixed wage contracts and argue that such contracts are not arbitrary or irrational, but are optimal in the context of "obligational markets" and "idiosyncratic exchange." I am certainly sympathetic to the stress on wage contracts. I have argued in my own work that wage contracts are an important element in the inertia of wage inflation. I also think that the importance of institutions is being understated in our rush towards a super-rational economic theory. To say that institutions do adapt to a changing economic environment does not imply that the institutions are irrelevant to the response of the economy. Thus the work of the Wachters, together with that of their colleague Oliver Williamson, is very valuable and I urge them to press on with it.

Somewhat more negatively, however, I wonder if they have, as yet, been able to focus on the specific features of institutions that are the most important for wage behavior and to show that these features are also consistent with other observations. Specifically, they say that personnel arrangements represent "a governance structure whose primary purpose is to maintain the relationship and thus the long-term gains of the parties, rather than to achieve equilibrium with short-run market conditions." That sounds fine on the face of it as an explanation of wage stickiness, but let's remember what happened during 73–75. The year-to-year rates of growth of average hourly earnings in manufacturing were:

73 over 72	6.0%
74 over 73	8.6%
75 over 74	9.9%

At the same time the rates of change of employment of production workers in manufacturing were:

73 over 72	5.8%
74 over 73	–1.0%
75 over 74	–10.6%

Thus, between 1973 and 1975 the rate of wage inflation accelerated by over 50 percent while employment dropped by 11.5 percent, a reduction of 1.7 million production workers. It is not obvious that dumping 1.7 million workers onto the streets while stepping up wage increases is so consistent with "a governance structure whose primary purpose is to maintain the relationship." One might wonder about more wage and hours flexibility in order to maintain the employment relationship and preserve the firm-specific human capital.

My own view (expressed in the 1976 No. 2 *Brookings Papers*) is that the observed response represents a structure whose purpose is to preserve the position and living standards of a subset of the workers. There is a buffer zone of younger workers and recent hires who are laid off. These employees, plus entrants and reentrants to the labor force, are the workers that bear most of the costs of employment variations. The "permanent" employees are sometimes put on temporary furlough, where they are protected reasonably well by unemployment insurance, and are recalled within a short period of time. Firms acquiesce in this kind of arrangement partly under duress and partly because it may have long-run profit advantages – for reasons discussed by the Wachters.

The Wachters then go on to present empirical evidence that the insensitivity of wages to demand in the short run reflects adjustment lags not a fundamental wage rigidity. The long-run elasticity of inflation to unemployment is high, they say, in fact the infinite elasticity of the long-run vertical Phillips curve. The length of the distributed lags they use is indeed astonishing. But I might have expected more in the way of statistical tests of alternative lag specifications. Does adding the fourth, fifth and sixth year of lagged wages or prices really improve the fit of the equation? I suspect that the quagmire of collinearity makes it hard to be sure.

On one small point: they mention productivity effects. Any wage equation with price inflation feeding back into wage inflation does contain a productivity effect. Let $\dot{w} = a + \frac{b}{U} + \dot{p}$ and $\dot{p} = \dot{w} - \rho$, where $\dot{w}$ and $\dot{p}$ are the rates of wage and price inflation, U is the unemployment rate and ρ the rate of productivity growth. Then if U^* is the unemployment rate consistent with nonaccelerating inflation, the arithmetic makes U^* a decreasing function of ρ. Of course, these equations are pretty simplistic, but the same idea will hold in more realistic formulations.

Let me give a few reflections on the Phillips curve in theory and practice. I am not at all convinced that we are telling the correct or complete stories about the process of wage inflation. Search theory, contract theory and obligational markets can all give us insights into certain aspects of the process. But a convincing dynamic analysis that will track the data remains elusive. I think we probably have to disaggregate, to distinguish workers in unions or who work for large corporations from self-employed workers and employees of small companies. The high-wage/union workers appear to have wage scales that are almost totally unresponsive to short-run demand conditions, even when those wage scales are reset or renegotiated. The rest of the work force has wages that are somewhat responsive to demand, with a downside rigidity linked to unemployment insurance and the minimum wage, at least for some jobs.

Of course in practice there is a spectrum of workers between these two extremes and there is a relative wage structure across the spectrum that changes over the cycle, but which always acts as a link among markets. Some of Mike Wachter's early work on the cyclical behavior of relative wages has been very informative in this area.

The next main part of the paper is an application of the Barro approach of dividing monetary changes into anticipated and unanticipated components. In the context of the Wachters' paper, the idea is that some part of the lags described by the empirical work is attributable to the slow adaptation of expectations about inflation. If this is so, it may be possible to reduce the rate of wage inflation without incurring all of the heavy price, in terms of unemployment, that their equations suggest. There may be something to this approach, but I do not think that the Barro methodology is valid.

Let me first comment on what Barro did and then on the Wachters' version. I will use the Barro analysis of real output movements from his forthcoming *JPE* paper. Equation 1 is intended to model the way in which expectations about money growth are formed.

Equation 1: The determinants of the rate of change of M_1

$$DM = \underset{(0.27)}{0.082} + \underset{(0.14)}{0.41}\ DM_{-1} + \underset{(0.12)}{0.21}\ DM_{-2} + \underset{(0.16)}{0.072}\ FEDV + \underset{(0.009)}{0.026}\ UN_{-1}$$

$DMR = DM - \widehat{DM}$ 1941-76 Annual Data

Standard errors in parentheses.

Variables:

DM = change in the log of M_1,

FEDV = real Federal expenditure less a distributed lag of past real Federal expenditure,

UN = the log of the unemployment rate divided by one minus the unemployment rate,

$\widehat{DM}$ = the fitted values from equation 1. Called the anticipated part of money growth.

DMR = the residuals from equation 1. Called the unanticipated part of money growth.

According to Barro it is only the unanticipated part of DM, i.e., DMR, that influences real output. This is tested by equations 2 and 3.

Equation 2: Real output determined by DMR and MIL

$$\log y = \underset{(0.14)}{2.95} + \underset{(0.21)}{1.04}\,DMR + \underset{(0.22)}{1.21}\,DMR_{-1} + \underset{(0.21)}{0.44}\,DMR_{-2} + \underset{(0.16)}{0.26}\,DMR_{-3}$$

$$+ \underset{(0.09)}{0.55}\,MIL + \underset{(0.0004)}{0.0354}\,t$$

R^2 = relative to trend 0.82, 1946–76.

Equation 3: Real output determined by DM and MIL

$$\log y = \underset{(0.08)}{3.13} + \underset{(0.26)}{0.95}\,DM + \underset{(0.26)}{0.53}\,DM_{-1} - \underset{(0.23)}{0.20}\,DM_{-2} - \underset{(0.16)}{0.27}\,DM_{-3}$$

$$+ \underset{(0.16)}{0.31}\,MIL + \underset{(0.0007)}{0.0335}\,t$$

R^2 = relative trend to 0.70, 1946–76.

y = real GNP.

MIL = ratio of military personnel to male population 15–44. Set equal to zero 1970–76.

t = time.

These show that DMR gives a better fit to output fluctuations and has a stronger significance test performance than DM. The real question, however, is: does it make sense to describe DMR as the unanticipated part of money growth? If not, what is it? and why does it fit pretty well to GNP movements? Consider the last part of this first. Substitute equation 1 into equation 2. This gives the following table of coefficients:

Implied Coefficients from Equation 2

	DM	UN	FEDV
t	1.04	--	–0.075
t-1	0.784	–0.027	–0.087
t-2	–0.275	–0.032	–0.032
t-3	–0.175	–0.011	–0.019
t-4	–0.199	–0.007	–
t-5	–0.055	–	–
Sums of Coefficients	1.12	–0.077	–0.21

First notice that the coefficients from equation 2 imply a pattern of coefficients on DM that are very much like the coefficients on DM in equation 3. But the fit of equation 2 is helped out by having two additional variables. We know that GNP and unemployment are linked through Okun's Law. We also know that unemployment is correlated with its own lagged values. So the UN variables will help the fit. FEDV is a more complicated variable. It marks *shifts* in total real Federal expenditures. It has been very high during war years and this might have led us to expect a positive correlation between FEDV and y. But with MIL in the equation (and also DM itself, which is high in war periods) there is an effective control for this factor. The other force driving FEDV is the impact of the automatic stabilizers. When output falls, transfer payments rise, and so does FEDV. Thus, given MIL and DM, the presence of FEDV with a negative coefficient will help the fit of equation 2. Just to check this out I ran equation 2 without the constraints on the coefficients of DM, UN and FEDV that are implied by equation 1. The resulting sums of coefficients were:

Sums of Coefficients on DM, UN and FEVD when included separately in equation 2, rather than constrained in the form implied by DMR:

DM	1.49	(0.58)
UN	–0.068	(0.030)
FEDV	–0.281	(0.19)

These are quite close to those given above.

None of this refutes Barro's interpretation of his findings. But it does suggest an alternative explanation for the statistical results. Are there other reasons for thinking that Barro's interpretation is doubtful? There are several.

(i) The Federal Reserve did not follow a stable rule for determining M_1 over the period 1941-76. In fact it did not use M_1 targets at all until quite recently. It used interest rate targets.

(ii) Persons acting before 1976 could not have known the parameters of equation 1. These are based on information available only after 1976. In fact one might have expected Federal Reserve behavior in the 30s and 40s to be the main guide to expected money growth in the 50s.

(iii) Equation 1 assumes people know, say, the 1975 value of FEDV when forming their anticipated value of money growth for 1975. Figures on M_1 are available weekly while FEDV is uncertain right through the year.

(iv) If DMR really is the unanticipated part of money growth, why is it affecting GNP after three whole years? Between (iii) and (iv), the process of information diffusion that is assumed is very odd indeed.

This was a long digression on Barro, but I have been increasingly concerned by the widespread acceptance of the idea that only unanticipated money movements influence real output, and the Wachters seem to be going along with this view. To give them credit, they do point out that most macroeconomists would agree that changes in money growth relative to its recent trend influence real output. But the reasoning behind this needs stress. Compare two cases. In case one, inflation has been running at around, say, 7 percent a year and money growth at around 11 percent a year. This is roughly consistent with real GNP growth at 4 percent a year in line with potential GNP. In case two, inflation has been at, say, 1 percent a year and money growth at 5 percent a year. If in both cases the rate of money growth is now set at, say, 8 percent a year, then it will lead to real contraction in case one and real expansion in case two. But it is the inertia in the inflation rate in both cases that is at work, not whether or not the change was anticipated.

I shall now comment on the Wachters' version of Barro's procedure. From Equation 5 we have:

$$DM = \sum_{j=1}^{4} \lambda_j DM_{-j} + \text{time variables.}$$

This gives the determination of anticipated and unanticipated money growth analogous to Barro's equation 1 above. The fitted values (they call these values DMA rather than the $\widehat{DM}$ of Barro's terminology) and the residuals (DMR) from this equation are used to explain UGAP, the Wachters' cylical variable. Their equation 6 is of the form:

$$UGAP = \sum_{i=1}^{20} \beta_i DMA_{t-i} + \sum_{i=1}^{20} \gamma_i DMR_{t-i} + \text{lagged UGAP.}$$

They find that DMR, the so-called unanticipated part of money growth, performs more strongly as a determinant of changes in UGAP (i.e., changes in real output and employment) than does DMA – although actually neither variable does that well.

In equation 7 the Wachters included DMA and DMR in their Phillips curve regression instead of wage or price feedbacks. They find the opposite of the results on UGAP, i.e., that DMA, the anticipated part of money growth, performs much more strongly than DMR as a determinant of wage inflation.

What explains these results? The Wachters' procedure lacks the additional identifying variables that Barro used and thus consists basically of juggling distributed lag coefficients. Since DM = DMA + DMR, their UGAP equation is simply:

$$\begin{aligned} UGAP &= \sum_{i=1}^{20} \gamma_i DM_{-i} + \sum_{i=1}^{20} (\beta_i - \gamma_i) \left[\sum_{j=1}^{4} \lambda_j DM_{-j}\right] + \dots \\ &= \gamma_1 DM_{-1} + [\gamma_2 + \lambda_1(\beta_1 - \gamma_1)] DM_{-2} \\ &\quad + [\gamma_3 - \lambda_1(\beta_2 - \gamma_2) + \lambda_2(\beta_1 - \gamma_1)] DM_{-3} + \dots . \end{aligned}$$

The implied coefficients on DM (I only computed the first three) were 0.0055 DM_{-1} + 0.0040 DM_{-2} + 0.0028 DM_{-3} + ... Thus the regression simply shows a rather conventional declining distributed lag on DM. The Wachters' procedure attributes all of the coefficient on DM_{-1} to DMR and most of the coefficient on DM_{-2} to DMR also.

The finding that recent values of DM are the ones with the largest impact on real output changes is a familiar one. So too is the finding that recent values of DM have very little impact on wage inflation, but that a long-run distributed lag of money growth is highly correlated with inflation.

Thus the real story being told by these regressions does not, in my view, have anything to do with anticipated versus unanticipated money changes. The real story relates to the one given earlier. The current rate of inflation is largely insensitive to short-run demand conditions. Real output responds to aggregate demand changes and money is certainly an important influence on aggregate demand (there may also be some reverse influence of output on M_1). In an economy that remains reasonably close to full employment, the arithmetic of money demand ensures a long-run relation between inflation and money growth. Either the inflation rate adjusts over the long run, or the monetary authorities accommodate the inflation rate in order to maintain full employment (or both).

Where does one come out on the policy question after all this? The Wachters suggest that "policy makers using preannounced rules may be able to translate money supply growth rates directly into inflation rather than real output changes. That is, there is at least the potential of slowing inflation while maintaining the economy close to its U* or potential output constraint." My preceding discussion suggests that I do not regard any current empirical work as convincing evidence for the usefulness of splitting money growth changes into anticipated and unanticipated components. Nevertheless, there is nothing wrong with trying to convince the private sector of the facts of life in order to give anti-inflation policy the best possible chance. The Open Market Committee of the Federal Reserve Board has shown over the past few years a determination to hold down money growth rates. We should certainly try to educate everyone to realize that this must inevitably lead either to a reduction in the rate of wage increase or recession (or both). If the impact of this educational effort were simply to discourage needed capital accumulation rather than to encourage wage restraint, then I would rather see direct intervention in wage and price setting than another period like 1975.

Finally, let me say that there were many things in this paper that were insightful and that I agreed with. I have concentrated my discussion, in the traditional way, on the points of disagreement.

Inflation and Unemployment in a Macroeconometric Model

Ray C. Fair

1. Introduction

The main question of this conference is why there has recently been both high inflation and high unemployment in the U.S. economy. The purpose of this paper is to consider this question within the context of a macroeconometric model. Much of the literature on inflation and unemployment since Phillips wrote his classic paper [10] has centered around the question of whether the relationship between inflation and unemployment is stable over time. The fact that this relationship does not appear to be stable (i.e., appears to "shift" over time) has caused much puzzlement. From the perspective of a macroeconometric model builder, however, this lack of stability is not necessarily surprising. Inflation and unemployment are two endogenous variables out of many in a model, and there is in general no reason to expect that the combined influences on any two endogenous variables in a model are such as to lead to a stable relationship between them. This holds true not only for the relationship between inflation and unemployment, but also for the relationship between such variables as unemployment and output ("Okun's law") and inflation and output.

A model builder must approach the task of explaining inflation and unemployment with considerable caution. A major problem in this area is the difficulty of testing alternative hypotheses. It is relatively easy with aggregate time series data to fit the data well within the sample period, but a good within-sample fit is by no means a guarantee that the particular equation or model is a good representation of the actual process generating the data. It is also difficult to make comparisons of predictive accuracy across models because of differences in the number and types of variables that are taken to be exogenous in models. These difficulties and the fact that inflation has not been particularly well explained in the past obviously (and justifiably) make people skeptical of any new attempt at an explanation.

This paper is primarily a review of that part of my recent work ([2], [3], [4], [5], [6]) that relates to the inflation-unemployment question, and so the value added of this paper to someone who is already familiar with this work is small. Sections II-IV contain a review of the determination of inflation and unemployment in my theoretical and empirical macro models, and Section V contains a discussion of some of the important properties of the empirical model

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regarding the relationship between these two variables. Some estimates of the accuracy of the empirical model regarding the explanation of the two variables are presented in Section VI. The main conclusions of my work with respect to the inflation-unemployment question are presented in Section VII. It is difficult to explain the structure and properties of a large-scale model in a short paper, but I hope that I have been at least partly successful in this paper in presenting my response to the main question of this conference.

II. The Theoretical Model[1]

My approach to the construction of an econometric model has been to develop a theoretical model first and then to use this model to guide the specification of the econometric model. The two main features of the theoretical model that are relevant to the present discussion are (1) the decisions of the individual agents in the model are derived from the solutions of multiperiod optimization problems and (2) explicit consideration is given to possible disequilibrium effects in the system. The following is a brief discussion of these two features.

With respect to the first feature, firms and banks in the theoretical model maximize the present discounted value of expected future profits, and households maximize the present discounted value of expected future utility. At the beginning of each period each agent solves its maximization problem, knowing all past values, receiving in some cases information from others regarding certain current-period values, and forming expectations of future values. Expectations are generally assumed to be formed in simple ways in the model, although in a few cases the agents estimate some of the important parameters in the system before making their expectations. No agent knows the complete model, and so expectations can turn out to be wrong even though there are no random shocks in the model. The main decision variables of a bank are its loan rate and the maximum amount of money that it will lend in the period; the main decision variables of a firm are its price, production, investment, wage rate, and the maximum amount of labor that it will employ in the period; and the main decision variables of a household are the number of goods to purchase and the number of hours to work. The determinants of an agent's decisions in a given period are the variables that affect the solution of its optimal control problem.

With respect to the disequilibrium feature, an important distinction is made in the model between the *unconstrained* and *constrained* decisions of firms and households. A firm or household in a period may be constrained in how much money it can borrow at the current loan rate, and a household may also be constrained in how many hours it can work at the current wage rate. An unconstrained decision of a firm is defined to be a decision that results from the solution of its optimal control problem when the loan constraint is not imposed, and a constrained decision is defined to be a decision that results when the loan constraint is imposed. Similarly, an unconstrained decision of a household is

[1] The discussion in this section is a review of some of the material in [2]. See also Section 1.1 in [3].

defined to be a decision that results from the solution of its optimal control problem when neither the loan constraint nor the hours constraint is imposed, and a constrained decision is defined to be a decision that results when one or both constraints are imposed.[2] The actual quantities traded in a period in the model are the quantities determined from the constrained optimization problems.

There are different "regimes" in the model corresponding to the different cases of binding and nonbinding constraints. Periods of "disequilibrium" are periods in which one or more of the constraints are binding. Binding constraints in the loan market are due to mistakes on the part of banks in setting loan rates, and binding constraints in the labor market are due to mistakes on the part of firms in setting prices and wages. These mistakes are the result of expectation errors. There is a continual adjustment to past mistakes in the model in that each period the individual agents reoptimize on the basis of information from the previous period.

The main determinants of a household's decision variables, other than the loan and hours constraints when they are binding, are the initial value of its assets or liabilities and the current and expected future values of the price of goods, the wage rate, interest rates, tax rates, and nonlabor income. Except for the constraints, a household's decision problem is a straightforward problem in choosing the optimal time paths of consumption and leisure, and the variables that affect this decision are well known from microeconomics.

The decision problem of a firm is more complicated and less tied to the previous literature. The five main decision variables of a firm mentioned above are simultaneously determined in the model, and this approach has generally not been followed in the past. It is usually the case that the price, production, investment, wage, and employment decisions of a firm are analyzed separately rather than within the context of a complete behavioral model. Space limitations prevent a detailed discussion of a firm's decision problem here, but two features of this problem should be mentioned. The first is that the concepts of "excess labor" and "excess capital" play an important role in the model. The underlying technology of a firm is of a putty-clay type, and given this technology it is possible to compute for any period the amounts of labor and capital that are required to produce the output. The differences between the actual amounts of labor and capital on hand and the required amounts are defined to be the "excess" amounts on hand. Because of adjustment costs, it is sometimes optimal for a firm to plan to hold either excess capital or excess labor or both during certain periods. The fact that firms may hold as an optimizing strategy excess labor and/or excess capital during certain periods provides a reconciliation of the commonly observed phenomena of cyclical swings in "productivity" with optimizing behavior.

The second feature that should be mentioned is that market share considerations play an important role in determining a firm's price and wage behavior. A

[2] There are obviously other constraints facing firms and households, such as budget constraints, but for purposes of the present discussion nothing is lost by using "constrained" to refer only to the loan and hours constraints.

firm has a certain amount of monopoly power in the short run in the sense that raising its price above prices charged by other firms will not result in an immediate loss of all its customers and lowering its price below prices charged by other firms will not result in an immediate gain of everyone else's customers. There is, however, a tendency for high-price firms to lose customers over time and for low-price firms to gain customers. A firm also expects that the future prices of other firms are in part a function of its own past prices. Similar considerations apply to a firm's wage decision and its ability to gain or lose workers. Because of this market share nature of the model, some of the most important factors affecting a firm's decisions are its expectations of other firms' price and wage decisions.

The main determinants of a firm's decision variables are the amounts of excess labor and capital on hand, the stock of inventories on hand, the current and expected future values of the loan rate and other determinants of the cost of capital, and variables affecting the firm's expectations of other firms' price and wage decisions. There are also two constraints that may be binding on a firm. One is the loan constraint, which has been mentioned above. The other, which will be called the labor constraint, results from the fact that a firm may (by mistake) set its wage rate too low to attract the amount of labor that it planned to employ in the period. In this case the firm may be forced to produce less output in the period than it originally planned.

One important property of this theoretical model of firm behavior that will be useful to keep in mind in the following discussion of the empirical model is that an increase in the loan rate or other determinants of the cost of capital causes, among other things, a firm to raise its price. This "cost-of-capital" effect on price, which comes out of the optimizing process of the firm, is not generally a part of other models, Nordhaus [8, p. 40], for example, notes that none of the studies of price behavior that he has reviewed introduced capital costs into the analysis.

III. The Transition from the Theoretical to the Empirical Model[3]

The application of the theoretical model to macro time series data is subject to the usual caveats. There is first the aggregation problem. I have, for example, used the analysis of the behavior of the individual firms and households in the theoretical model to guide the specification of the behavioral equations that pertain to the entire firm and household sectors in the empirical model. Because of this jump from individual to aggregate behavior, there is obviously a wide gap between the theoretical and empirical models, and these two models are not in a strict sense the same model. I have really nothing further to say about this except to stress that my choice of the general structure of the empirical model and of the explanatory variables to use in the estimated equations has been heavily influenced by the general structure of the theoretical model and by the determinants of the decision variables of the individual agents in the model.

The application of the theoretical model to the data also poses another problem, namely that two important types of variables in the model, expecta-

[3] The discussion in this section is a review of some of the material in [3].

tions and unconstrained decisions, are unobserved. With respect to expectations, the standard procedure in accounting for expectational effects in econometric work is to use current and lagged values as "proxies" for expected future values, and this is the procedure that I have followed. Lagged values of endogenous variables have been used freely to try to account for expectational effects. It is well known, of course, that it is difficult to separate expectational effects from lagged response effects when lagged endogenous variables are used as explanatory variables, and I have made no attempt to do this in the empirical work. The lagged endogenous variables in the estimated equations below should thus be interpreted as picking up some unknown mix of expectational and lagged response effects. It also should be noted that the use of current and lagged values as proxies for expected future values does not necessarily imply that people are naive in their formation of expectations. It is true that expectations are not rational in the model since no constraints have been imposed requiring that people's expectations be equal to the model's predictions. The present procedure is, however, consistent with the use of considerable current and past information in forming expectations; it is just not consistent with complete knowledge of the model.

With respect to the unconstrained decision values, these values are the actual (observed) values in the theoretical model if none of the constraints are binding on the behavioral unit in question. Otherwise, however, only the constrained decision values are assumed to be observed. In the empirical application of this model some way must be found for distinguishing between the case in which the observed values are unconstrained and the case in which the observed values are constrained. This is a difficult problem, and much of the empirical work for the model has been concerned with this issue. The following is a brief discussion of the treatment of disequilibrium effects in the empirical model.

For present purposes it will be useful to ignore the possibility of a binding loan constraint on firms and households and use "constrained" to refer only to the hours and labor constraints. Also, since the empirical model is now under consideration, the following variables should be interpreted as pertaining to the entire household and firm sectors. Let:

LUN_t^S = household sector's unconstrained supply of labor,

L_t = household sector's constrained supply of labor (observed),

XUN_t^D = household sector's unconstrained demand for goods,

X_t = household sector's constrained demand for goods (observed),

LUN_t^D = firm sector's constrained demand for labor,

PUN_t = firm sector's price if it is unconstrained,

P_t = first sector's actual price (observed).

Consider the household sector first. From the theoretical model the determinants of LUN_t^S and XUN_t^D are known and have been mentioned above. Write the equations determining these two variables as

(1) $LUN_t^S = f_1(\ldots),$

(2) $XUN_t^D = f_2(\ldots).$

The observed supply of labor and demand for goods are L_t and X_t, respectively, and if the hours constraint is not binding, $L_t = LUN_t^S$ and $X_t = XUN_t^D$. Otherwise, the observed quantities are less than the unconstrained quantities, and the approach that I have taken is to postulate equations explaining the ratios of the observed and unconstrained quantities. In the present notation these equations are:

(3) $$\frac{L_t}{LUN_t^S} = Z_t^{\gamma_1}, \gamma_1 > 0,$$

(4) $$\frac{X_t}{XUN_t^D} = Z_t^{\gamma_2}, \gamma_2 > 0,$$

where Z_t is some variable that takes on a value of one when the hours constraint is not binding and of less than one otherwise. For the empirical work Z_t was taken to be a nonlinear function of a measure of labor market tightness in the model, J_t^*. J_t^* is a detrended ratio of total worker hours paid for to the total population 16 and over. Although J_t^* was used as the measure of labor market tightness, the results were not sensitive to this particular choice: similar results were obtained using one minus the unemployment rate as the measure of labor market tightness. The nonlinear function that was chosen has the property that Z_t is close to one when the labor market is very tight and becomes progressively less than one as the labor market becomes progressively looser.

Equations (1) and (3) can be combined to eliminate the unobserved variable LUN_t^S. If, as is assumed for the empirical work, equation (1) is in log form and contains only observed right-hand side variables, then combining (1) and (3) yields an equation with log L_t on the left-hand side and only observed variables, including γ_1 log Z_t, on the right-hand side. This equation can then be estimated. The coefficient γ_1, which is unknown, can be estimated along with the other unknown coefficients in equation (1). Similar considerations apply to equations (2) and (4).

Consider now the firm sector. From the theoretical model the determinants of PUN_t are known. Write the equation determining this variable as

(5) $PUN_t = f_5 (\ldots)$.

If the labor constraint is not binding $(LUN_t^S > LUN_t^D)$, then $P_t = PUN_t$. If, on the other hand, the labor constraint is binding, then the firm sector is assumed to adjust to this by raising its price. In particular, it is assumed that

$$(6) \qquad \frac{P_t}{PUN_t} = (Z_t')^{\gamma_3}, \gamma_3 < 0,$$

where Z_t' is some variable that takes a value of one when the labor constraint is not binding and of less than one otherwise. For the empirical work Z_t' was also taken to be a nonlinear function of a measure of labor market tightness, in this case one minus the unemployment rate. The nonlinear function that was chosen has the property that Z_t' is close to one when the labor market is very loose and becomes progressively less than one as the labor market becomes progressively tighter. Equations (5) and (6) can also be combined to eliminate the unobserved variable PUN_t, thus ending up with an equation that can be estimated. This equation contains log P_t on the left-hand side and, among other terms, γ_3 log Z_t' on the right-hand side.

The possible labor constraint on the firm sector is thus handled in the empirical model through the price equation. If this constraint is binding, the firm sector is assumed to raise its price. A higher price leads in the model to a lower level of sales, which in turn leads to a lower level of production, which then results in less labor demand. There is, in other words, an indirect link in the model between a higher price level and a lower demand for labor. In the case in which the labor constraint is binding on the firm sector, the price is assumed to be raised enough so that the new demand for labor is equal to the supply from the household sector.

In the theoretical model the labor and hours constraints are never binding at the same time. Either the households are constrained, in which case the observed quantity of labor is equal to the demand from the firms, or the firms are constrained, in which case the observed quantity of labor is equal to the supply from the households. In practice, of course, this dichotomy is not literally true. At any one time some households and some firms are likely to be constrained, and it is a matter of degree as to which type of constraint is quantitatively more important.[4] The above approach for the empirical model does allow for this kind of flexibility. The nonlinear functions that relate labor market tightness to Z_t and Z_t' do not have the property that Z_t is equal to one when Z_t' is less than one and vice versa. Z_t is equal to one only for very tight labor markets, and Z_t' is equal to one only for very loose labor markets. In between these two extremes Z_t and Z_t' are both less than one, although Z_t is, of course, much closer to one in relatively tight labor markets than is Z_t', and vice versa in relatively loose labor markets.

[4] This heterogeneity of labor markets, which I argue in the following discussion has at least been partly accounted for in the empirical model, has been emphasized by Tobin [11], among others.

IV. Some Equations of the Empirical Model[5]

The following is a discussion of the equations of the empirical model that relate most directly to the determination of inflation and unemployment. The procedure that I followed in the empirical work for the household sector was first to regress each of its main decision variables (four consumption variables and three labor supply variables) on the same set of variables. This set consisted of the hours constraint variable (Z_t above and ZJ_t in the following notation), a similar loan constraint variable, and variables that were expected from the theoretical analysis to affect a household's unconstrained optimization problem. The highly insignificant variables were then dropped from each equation, and each equation was reestimated on a smaller set. The variables in the original set were highly collinear, and there were generally a number of insignificant variables in the first estimate of each equation.

The three labor supply equations in the model explain the labor force of males 24-54 (TLF_{1t}), the labor force of all others 16 and over (TLF_{2t}), and the number of people holding two jobs ($MOON_t$). These equations are:[6]

5. $$\log\frac{TLF_{1t}}{POP_{1t}} = \underset{(1.43)}{-0.0834} + \underset{(5.76)}{0.540}\log\frac{TLF_{1t-1}}{POP_{1t-1}} + \underset{(1.88)}{0.0170}\log\frac{WT_t}{PH_t}$$

$$\underset{(1.42)}{-0.00804}\log\frac{YNLH_{t-1}}{PH_{t-1}POP_{t-1}} + \underset{(3.92)}{0.0813}\log(1.0 - d^M_{3t-1} - d_{6t-1}),$$

$$R^2 = 0.969, \; SE = 0.00199, \; DW = 2.06,$$

6. $$\log\frac{TLF_{2t}}{POP_{2t}} = \underset{(3.41)}{-0.356} + \underset{(16.38)}{0.842}\log\frac{TLF_{2t-1}}{POP_{2t-1}} - \underset{(3.15)}{0.0403}\log\frac{AA_{t-1}}{POP_{t-1}}$$

$$+\underset{(3.43)}{0.0647}\log\frac{WT_{t-1}}{PH_{t-1}} + \underset{(0.07)}{0.000553}\log RMORT_t + \underset{(3.92)}{0.139}\log ZJ_t,$$

$$R^2 = 0.988, \; SE = 0.00508, \; DW = 1.86,$$

[5] The empirical model has been changed slightly and updated since [3] was published, and the updated version has been used for the results cited in this paper. The main change that has been made to the original model is the addition of an equation explaining the behavior of the Federal Reserve. This addition is discussed in [4]. The updated version of the model consists of 97 equations, 29 of which are stochastic, and has 188 unknown coefficients to estimate. The complete list of the equations of this version is contained in [7], which is available from the author upon request.

[6] The sample period for all the estimated equations presented in this section was 1954 I - 1977 IV, a total of 96 observations. All the equations were estimated by two-stage least squares, with in the case of equation 12 below, account also taken of the first order serial correlation of the error term t-statistics in absolute value are in parentheses. The variables that were used as regressors in the first-stage regressions for each equation are listed in Table 2-5 in [7].

7. $$\log \frac{MOON_t}{POP_t} = \underset{(3.63)}{-1.23} + \underset{(7.51)}{0.695} \log \frac{MOON_{t-1}}{POP_{t-1}} + \underset{(1.73)}{0.211} \log \frac{WT_t}{PH_t}$$

$$+ \underset{(0.96)}{0.305} \log (1.0 - d^M_{3t-1} - d_{6t-1}) + \underset{(2.36)}{1.46} \log ZJ_{t-1} ,$$

$$R^2 = 0.810 , SE = 0.0653 , DW = 2.04 .$$

The variables are defined in Table 1. The equation numbers are as in [3], Table 2-2. Equation 5 states that the labor force participation of males 25-54 is a positive function of the real wage, a negative function of nonlabor income, and a negative function of the marginal personal income tax rate and the social security tax rate. Equation 6 states that the labor force participation of all others 16 and over is a positive function of the real wage, a negative function of net wealth of the household sector, a positive (although negligible) function of the mortgage rate, and a positive function of the hours constraint variable ZJ_t. Equation 7 states that the percent of the population holding two jobs in a positive function of the real wage, a negative function of the two tax rates, and a positive function of the hours constraint variable.

Although not every variable in the basic set of explanatory variables was significant in every equation, the above results do seem to indicate that the variables that one expects from microeconomics to affect labor supply are in fact important in explaining the aggregate data. It should also be noted that the significance of the hours constraint variable in an equation like 6 means that the observed labor force participation rate is less when the hours constraint is binding than when it is not. This effect can be interpreted as being similar to what are sometimes referred to in the literature as "discouraged worker" effects. The main difference here is that the hours constraint affects both the consumption and labor supply decisions; there are thus both "discouraged consumption" and "discouraged worker" effects in the model.

The link from the theoretical model to the empirical model is somewhat looser for the firm sector than it is for the household sector. Although a firm's decisions are determined simultaneously in the theoretical model, for empirical purposes the decisions were assumed to be made sequentially. This sequence is from the price decision, to the production decision, to the investment and employment decisions, to the wage rate decision. A firm is first considered as having chosen its optimal price path. This path implies a certain expected sales path, from which the optimal production path is chosen. Given the optimal production path, the optimal paths of investment and employment are chosen. Finally, given the optimal employment path, the optimal wage rate path is chosen. The optimal wage rate path is assumed to be that path that the firm expects is necessary to attract the amount of labor implied by its optimal employment path.

The equations of the firm sector that are relevant for present purposes are equations explaining its demand for workers ($JOBF_t$), its price level (PF_t), and

TABLE 1

Selected Variables in the Empirical Model in Alphabetic Order

AA_t =	total net wealth of the household sector.
$^{\dagger}d_{1t}$ =	profit tax rate.
d_{3t}^{M} =	marginal personal income tax rate.
$^{\dagger}d_{6t}$ =	employee Social Security tax rate.
$^{\dagger}DTAXCR_t$ =	investment tax credit variable.
$EMPL_t$ =	total number of people employed.
J_t =	ratio of total worker hours paid for to the total population 16 and over.
J_t^* =	J_t detrended.
$JOBF_t$ =	number of jobs in the firm sector.
$^{\dagger}JOBGC_t$ =	number of civilian jobs in the government sector.
$^{\dagger}JOBGM_t$ =	number of military jobs in the government sector.
$M_tH_t^M$ =	number of worker hours required to produce Y_t.
$MOON_t$ =	difference between the total number of jobs in the economy and the total number of people employed.
PF_t =	implicit price deflator for nonfarm output of the firm sector.
PH_t =	implicit price deflator for domestic sales inclusive of indirect business taxes.
$^{\dagger}PIM_t$ =	implicit price deflator for imports.
$^{\dagger}POP_t$ =	noninstitutional population 16 and over.
$^{\dagger}POP_{1t}$ =	noninstitutional population of men 25–54.
$^{\dagger}POP_{2t}$ =	noninstitutional population of all persons 16 and over except men 25–54.
PX_t =	implicit price deflator for total output of the firm sector.
$RAAA_t$ =	Aaa corporate bond rate.
$RMORT_t$ =	mortgage rate.
$^{\dagger}t$ =	linear time trend, $t = 1$ in 1952 I.
TLF_{1t} =	total labor force of men 25–54.
TLF_{2t} =	total labor force of all persons 16 and over except men 25–54.
U_t =	number of people unemployed.
UR_t =	civilian unemployment rate.
WFF_t =	average hourly earnings, excluding overtime, of workers in the firm sector.
WT_t =	average hourly earnings, excluding overtime, of all workers in the economy.
Y_t =	output of the firm sector.
$YNLH_t$ =	nonlabor income of the household sector.
ZJ_t =	hours constraint variable for the household sector.
ZJ_t' =	labor constraint variable for the firm sector.

†exogenous variable

its wage rate (WFF_t).[7] These equations are:

12. $$\log JOBF_t - \log JOBF_{t-1} = -\underset{(3.25)}{0.623} - \underset{(3.23)}{0.0990}(\log JOBF_{t-1} - \log M_{t-1}H^M_{t-1})$$
$$+ \underset{(3.52)}{0.000156}t + \underset{(4.84)}{0.269}(\log Y_t - \log Y_{t-1})$$
$$+ \underset{(4.21)}{0.190}(\log Y_{t-1} - \log Y_{t-2}) + \underset{(0.72)}{0.0285}(\log Y_{t-2} - \log Y_{t-3})$$
$$+ \text{ 2 strike dummies,}$$
$$\hat{\rho} = \underset{(2.62)}{0.304}, \; R^2 = 0.747, \; SE = 0.00385, \; DW = 2.06,$$

9. $$\log PF_t = \underset{(7.63)}{-0.183} + \underset{(35.11)}{0.785} \log PF_{t-1} + \underset{(13.25)}{0.0702} \log PIM_t + \underset{(6.28)}{0.0833} \log WFF_t$$
$$+ \underset{(2.02)}{0.0107} \log RAAA_t - \underset{(1.66)}{0.00225} DTAXCR_t + \underset{(2.03)}{0.0684} \log (1.0 + d_{1t-1})$$
$$- \underset{(3.52)}{0.00335} \log ZJ'_t, \; R^2 = 0.9998, \; SE = 0.002986, \; DW = 2.03,$$

15. $$\log WFF_t = \underset{(4.96)}{0.195} + \underset{(17.28)}{0.766} \log WFF_{t-1} + \underset{(5.76)}{0.00191}t + \underset{(\text{constrained})}{0.508} \log PX_t$$
$$- \underset{(3.26)}{0.344} \log PX_{t-1} - \underset{(1.14)}{0.00214} \log ZJ'_t,$$
$$R^2 = 0.999, \; SE = 0.006179, \; DW = 1.92.$$

Equation 12 explains the number of jobs in the firm sector. The first term after the constant term on the right-hand side is a measure of the amount of excess labor on hand. The inclusion of the constant term and time trend in the equation is due to the particular form of the excess labor variable, and so the first three right hand side terms can be thought of as the excess labor term. The equation states that the change in the number of jobs (in log form) is a function of the amount of excess labor on hand and of three change-in-output terms. The two lagged change-in-output terms can be interpreted either as representing the effects of past output behavior on current employment decisions that are not captured in the measure of excess labor or as being proxies for expected future output changes. Equation 12 is meant to approximate the employment decisions

[7]For the model as presented in [3] and [7], WFF_t is in units of millions of dollars per hour per job, but for the results cited in this paper WFF_t is in units of dollars per hour per job. Also, as discussed in [7], WFF_t rather than WF_t is now used as the variable explained by the wage equation. WF_t has been dropped from the model.

of firms that result from the solutions of their multiperiod optimization problems in the theoretical model. The sequential assumption mentioned above is reflected in this equation in that Y_t is used as an explanatory variable. If the decisions on $JOBF_t$ and Y_t were truly made simultaneously, it would not be appropriate to use one of the variables to explain the other. Equation 12 is also similar to the equation that I used in [1] to explain the demand for employment by three digit industries.[8]

Before discussing equations 9 and 15, it will be useful to note how the unemployment rate is determined in the model. Given the endogenous variables TLF_{1t}, TLF_{2t}, $MOON_t$, and $JOBF_t$ and the exogenous government variables $JOBGC_t$ and $JOBGM_t$, the following three definitions determine the unemployment rate:

81. $EMPL_t = JOBF_t + JOBGC_t + JOBGM_t - MOON_t$, [total number of people employed]

82. $U_t = TLF_{1t} + TLF_{2t} - EMPL_t$, [total number of people unemployed]

83. $UR_t = \dfrac{U_t}{TLF_{1t} + TLF_{2t} - JOBGM_t}$. [civilian unemployment rate]

Equation 81 states that the total number of people employed equals the total number of jobs in the economy less the number of people holding two jobs. Equation 82 states that the number of people unemployed is equal to the number in the labor force less the number employed, and equation 83 states that the civilian unemployment rate is equal to unemployment divided by the civilian labor force.[9] The definition of the labor constraint variable ZJ'_t should

[8] For the work with the three digit industry data in [1] actual future values of output were used with some success as proxies for expected future values. For the work with the aggregate data, however, this was not the case, and so only lagged values are used in equation 12 as proxies for expected future values.

[9] One link between the discussion of disequilibrium effects in Section III and the equations presented in this section should be noted. Although three labor supply equations have been estimated (equations 5, 6, and 7), no equation explaining the supply of *jobs* has been estimated. The difference between the supply of labor as reflected in TLF_{1t}, TLF_{2t}, and $MOON_t$ and the demand for labor as reflected in $JOBF_t$, $JOBGC_t$, and $JOBGM_t$ is the unemployment variable U_t. U_t is thus indirectly affected by both the hours and labor constraint variables. The hours constraint variable directly affects two of the three labor supply variables, and the labor constraint variable indirectly affects $JOBF_t$ through its effect on the price variable PF_t.

It should be stressed that this approach is not the only way that one might try to account for disequilibrium effects. One alternative approach would be i) specify an equation explaining the supply of jobs to the firm sector (say, $JOBF_t^S$), ii) postulate that the observed number of jobs is equal to the minimum of the supply and demand ($JOBF_t = min[JOBF_t^S, JOBF_t^D]$), and iii) use some of the recent econometric techniques that have been developed for estimating markets in disequilibrium to estimate the equations. Whether an approach like this would provide a better explanation of the data than has so far been achieved with the present approach is clearly an open question.

also be noted. As mentioned above, ZJ'_t is a nonlinear function of the unemployment rate:

$$78. \quad ZJ'_t = 4.454062 + \frac{1}{1 - UR_t - 1.199514}.$$

The two coefficients in equation 78 are chosen so that ZJ'_t equals one when the unemployment rate is 9.0 percent and zero when the unemployment rate is 2.5 percent.

Equation 9 explains the price of nonfarm output of the firm sector. The explanatory variables include the lagged dependent variable, the price of imports *(PIM)*, the wage rate *(WFF)*, three cost-of-capital variables (the bond rate, *RAAA*, the investment tax credit variable, *DTAXCR*, and the profit tax rate, d_1), and the labor constraint variable *(ZJ')*. In a manner similar to that for equation 12, equation 9 is meant to approximate the price decisions of firms that result from the solutions of their multiperiod optimization problems in the theoretical model. The inclusion of the *PIM, WFF,* and lagged dependent variable terms in the equation is in part designed to pick up expectational effects. As noted above, a firm's expectations of other firms' prices play an important role in the theoretical model in determining the price that the firm sets for the period, and after some experimentation, the three variables just mentioned were chosen to represent expectational effects in the empirical model. The reason for the inclusion of the cost-of-capital variables has been mentioned above. The cost of capital does appear from the present results to have an effect on the price level.

The only variable in equation 9 that can be considered to be like a demand pressure variable is the labor constraint variable. Other demand variables were tried, but none proved to be significant. In particular, the following four variables, which have an influence in the theoretical model on the price that a firm sets, were tried and found not to be significant: the ratio of the stock of inventories to the level of sales, the level of sales itself, the amount of excess labor on hand, and the amount of excess capital on hand (all lagged one period). Since ZJ'_t is close to one for high unemployment rates (and thus log ZJ'_t close to zero), there is essentially no effect of the current unemployment rate on the price level in equation 9 in periods of high unemployment rates. In periods of low unemployment rates, on the other hand, the effect is large, and it in fact approaches infinity as the unemployment rate approaches 2.5 percent.

Although ZJ'_t as defined in equation 78 is used in the price equation to pick up demand effects on the price level, it is important to note that many other variables work equally well in this regard. Alternative measures of labor market tightness are highly correlated, and it is my conclusion from trying different measures in the price and wage equations that it is not possible using aggregate time series data to choose any one measure as being best. To give an example of this, I estimated equation 9 14 times using 14 different measures of labor market tightness, and the following is a summary of these results:[10]

	SE	*t-statistic for the coefficient estimate of the measure*	*Measure*	
1.	0.002986	−3.52	$\log ZJ'_t$,	where ZJ'_t is as defined in equation 78: ZJ'_t = 1.0 when UR_t = 0.090 and ZJ'_t = 0.0 when UR_t = 0.025.
2.	0.002987	−3.52	$\log ZJ'_t$,	where the coefficients in equation 78 are changed so that ZJ'_t = 1.0 when UR_t = 0.090 and ZJ'_t = 0.0 when UR_t = 0.020.
3.	0.002988	−3.52	$\log ZJ'_t$,	where the coefficients in equation 78 are changed so that ZJ'_t = 1.0 when UR = 0.090 and ZJ'_t = 0.0 when UR_t = 0.015.
4.	0.002988	−3.38	$\log ZJ'_t$,	where UR_t is the unemployment rate for married men and where the coefficients in equation 78 are changed so that ZJ'_t = 1.0 when UR_t = 0.090 and ZJ'_t = 0.0 when UR_t = 0.010.
5.	0.002994	−3.28	$\log ZJ'_t$,	where ZJ'_t is as in 2 except that UR_t is Perry's weighted unemployment rate.
6.	0.002983	−3.45	$\log ZJ'_t$,	where ZJ'_t is as in 3 except that UR_t is Perry's weighted unemployment rate.
7.	0.002978	−3.51	$\log ZJ_t$,	where ZJ'_t is as in 4 except that UR_t is Perry's weighted unemployment rate.
8.	0.002998	3.43	$\log (1-UR_t)$,	where UR_t is as defined in equation 83.
9.	0.002990	−3.52	$\log UR_t$,	where UR_t is as defined in equation 83.
10.	0.002970	3.45	$\log (1-UR_t)$,	where UR_t is the unemployment rate for married men.
11.	0.002974	−3.51	$\log UR_t$,	where UR_t is the unemployment rate for married men.
12.	0.002973	3.44	$\log (1-UR_t)$,	where UR_t is Perry's weighted unemployment rate.
13.	0.002973	−3.54	$\log UR_t$,	where UR_t is Perry's weighted unemployment rate.
14.	0.002985	3.54	$\log J_t^*$,	where J_t^* is a detrended ratio of total worker hours paid for in the economy to the total population 16 and over.

[10] The unemployment rate for married men that was used for some of these results is only available from 1955 on, and so for 1954 this series was spliced to the standard unemployment rate series. For a discussion of Perry's weighted unemployment rate series, see [9]. I am indebted to George Perry for supplying me with the latest data on this series.

It is clear from these results that essentially the same fit of the price equation has been achieved for each measure. Also, when different pairs of the above variables were tried in the equation, no one variable was ever individually significant. These results indicate, in other words, that one cannot distinguish among the total civilian unemployment rate, the unemployment rate for married men, Perry's weighted unemployment rate, and a detrended employment-population ratio as the variable to be used in the price equation. Nor can one distinguish among alternative nonlinear functions of these variables. This situation is unfortunate because, among other things, one's policy conclusions are likely to differ depending on which measure is used. It does seem from these results, however, that any policy conclusions that are sensitive to the particular measure used are not supported by the aggregate data.

It should finally be noted with respect to the price equation that some experimentation was done, primarily through the use of dummy variables, to see if the effects of price controls should be taken into account. These effects at best seemed small, and so the decision was made not to incorporate them into the model. To give an example, when dummy variables for 1971 IV, the quarter affected by the first price freeze, and 1972 I, the quarter following the lifting of the freeze, were added to equation 9, their coefficient estimates were 0.00102 and 0.00906, respectively, with t-statistics of 0.34 and 3.07. The first coefficient estimate is of the wrong expected sign, but it is clearly not significantly different from zero. The second coefficient estimate is positive, as expected, and significant. Foes of price controls may like these results as indicating that price controls, if anything, exacerbate inflation in the long run, but a better conclusion is probably that there is little evidence in the aggregate data of any lasting effects of price controls on inflation.

Equation 15 explains the wage rate paid by the firm sector. The explanatory variables include the lagged dependent variable, the current and lagged value of the price deflator of the total output of the firm sector (PX), a time trend, and the labor constraint variable. This equation is meant to approximate the wage decisions of firms in the theoretical model. It can also be considered, at least in a loose sense, as reflecting the outcome of bargaining between the firm and household sectors over the real wage. In the theoretical model bargaining takes the form of the firm sector adjusting over time to changes in the labor supply curve facing it, the labor supply curve being determined each period by the household sector. If the equation is interpreted in this way, an important question is which price variable is relevant for the bargaining process. The choice here of PX, which excludes import prices and indirect business taxes, reflects the assumption that the household sector is aware that some price increases benefit the foreign and government sectors rather than the firm sector and considers only the prices that benefit the firm sector in its bargaining process with the firm sector.

The above conclusion about the inability to distinguish among alternative measures of labor market tightness for the price equation also holds for the wage equation. The measure that is used for the wage equation ($\log ZJ_t'$) is the same as the one used for the price equation, but the other 13 measures discussed above gave similar results. These results are:

	SE	t-statistic for the coefficient estimate of the measure		SE	t-statistic for the coefficient estimate of the measure
1.	0.006179	−1.14	8.	0.006173	1.18
2.	0.006176	−1.17	9.	0.006173	−1.21
3.	0.006175	−1.19	10.	0.006210	0.99
4.	0.006202	−1.12	11.	0.006200	−1.12
5.	0.006201	−0.99	12.	0.006193	1.13
6.	0.006192	−1.09	13.	0.006188	−1.17
7.	0.006189	−1.13	14.	0.006234	2.20

It is clear from these results that essentially the same fit has been achieved for each measure.

One other point about the price and wage equations should be noted, which is that a restriction has been imposed on the coefficients of the wage equation. This restriction is as follows. First, since PX_t and PF_t are approximately the same variable, for sake of the following analysis the latter can be substituted for the former in the wage equation. Therefore, write these two equations as follows:

9. $$\log PF_t = \beta_1 \log PF_{t-1} + \beta_2 \log WFF_t + \ldots$$

15. $$\log WFF_t = \gamma_1 \log WFF_{t-1} + \gamma_2 \log PF_t + \gamma_3 \log PF_{t-1} + \ldots$$

From these two equations the reduced form equation for the real wage (forgetting about the other endogenous variables in equations 9 and 15) is:

(i) $$\log WFF_t - \log PF_t = \frac{1}{1-\beta_2\gamma_2}(\gamma_1 - \beta_2\gamma_1)\log WFF_{t-1} + \frac{1}{1-\beta_2\gamma_2}(\beta_1\gamma_2 + \gamma_3 - \beta_2\gamma_3 - \beta_1)\log PF_{t-1} + \ldots$$

Now, in order for the real wage not to be a function of the absolute size of the money wage and price level in the long run, it must be the case that the coefficient of log WFF_{t-1} in (i) be equal to the negative of the coefficient of log PF_{t-1}. This requires that:

$$\gamma_1 - \beta_2\gamma_1 + \beta_1\gamma_2 + \gamma_3 - \beta_2\gamma_3 - \beta_1 = 0 ,$$

or

$$\text{(ii)} \quad (\gamma_1 + \gamma_3)(1 - \beta_2) - \beta_1(1 - \gamma_2) = 0 .$$

Since it does not seem sensible for the real wage to be a function of the price level in the long run, the constraint in (ii) was imposed in the estimation work. This was done by (1) estimating the PF_t equation in the usual way by two stage least squares (TSLS), (2) using the resulting estimates of β_1 and β_2 to impose a linear restriction on the γ coefficients in the WFF_t equation, and (3) estimating the WFF_t equation by TSLS under this restriction. Given the β estimates, the linear restriction is merely:

$$\text{(iii)} \quad \gamma_2 = 1 - \frac{(1 - \hat{\beta}_2)}{\hat{\beta}_1} (\gamma_1 + \gamma_3) .$$

This restriction can be easily imposed within the context of the TSLS procedure.[11,12]

V. Some Properties of the Empirical Model

It should be fairly clear from the equations presented in the previous section that many factors affect inflation and unemployment in the model, and there is no particular reason to expect that the relationship between these two variables is stable. The price level is affected by the price level lagged one period, the price of imports, the wage rate, three cost-of-capital variables, and the labor constraint variable (when it is binding). The unemployment rate is residually determined as one minus the ratio of employment to the labor force. The labor force is affected by the wage rate, the price level, the marginal personal income tax rate and the social security tax rate, the net wealth of the household sector, nonlabor income, and the hours constraint variable (when it is binding). Employ-

[11] William Parke, a student at Yale, has recently developed a computationally feasible algorithm for obtaining full information maximum likelihood estimates (FIML) of large-scale models. In future work I plan to use this algorithm to obtain FIML estimates of my model, and when this is done, it will be possible to impose the restriction in (ii) directly on the coefficients (i.e., without resorting to the above two-step procedure). For the present results, the hypothesis that the restriction (iii) is valid in the wage equation was (using the conventional F test) rejected at the 5 percent confidence level, but accepted at the 1 percent level.

[12] Note that the decision sequence of the firm sector outlined on page 172 is not quite right for the price equation because the current wage rate is on the right-hand side of it. To be consistent with the sequence, the lagged wage rate should appear on the right-hand side of the price equation rather than the current wage rate, and in fact quite similar results were obtained using WFF_{t-1} in place of WFF_t in equation 9. The use of WFF_t rather than WFF_{t-1} in equation 9 should be interpreted as being dictated by the use of quarterly data (as opposed to data for a shorter interval) rather than as being derived from any theoretical proposition.

ment demand is affected by the amount of excess labor on hand and current and past levels of output. Finally, the number of people holding two jobs, which is needed to link employment in terms of jobs to employment in terms of people, is affected by the wage rate, the price level, the two tax rates, and the hours constraint variable (when it is binding). Given the large number of diverse factors that influence the price level, the labor force, and employment, it would clearly be surprising if the net result of all these factors were a stable relationship between inflation and unemployment.[13]

It is also interesting to note, although this is off the main topic of this paper, that there is no reason to expect the relationship between real output and the unemployment rate to be stable in the model. In other words, there is no reason to expect a stable Okun's law. The relationship between output and employment is affected, among other things, by the amount of excess labor on hand, and the large number of factors that affect the labor force have already been mentioned. Even though no stable Okun's law is expected in the model, the model does provide an explanation of the short-run leakages between changes in output and changes in the unemployment rate. When, say, output increases by 1 percent, the number of jobs increases by less than 1 percent in the current period (equation 12). Also, an increase in the number of jobs results in a less binding hours constraint, which in turn results in an increase in the labor force (equation 6) and, with a lag of one period, in the number of moonlighters (equation 7). Both an increase in the labor force and in the number of moonlighters causes the unemployment rate to fall less than it otherwise would in response to the increase in jobs.

An important characteristic of the model with respect to the relationship between inflation and unemployment or output is that when loss functions that target a given level of output and a given rate of inflation each period are minimized, the optima tend to correspond more closely to the output targets being achieved than they do to the inflation targets being achieved.[14] This is true even when the output target is weighted much less than the inflation target in the loss function. The model has the property that output can be increased by government policies to a high-activity level without having too much effect on the rate of inflation, whereas the rate of inflation cannot be decreased much without having a serious effect on output. As noted above, the only type of demand pressure variable in the price and wage equations is log ZJ_t', and the

[13] To drive home this point once more, note that government tax policy affects the relationship between inflation and unemployment through, among other things, its effect on the labor force. If, say, net taxes are increased by increasing the marginal personal income tax rate (d_3^M), this causes, other things being equal, a decrease in the labor force, whereas if net taxes are increased by decreasing transfer payments (which are included in the nonlabor income variable *YNLH*), this causes, other things being equal, an increase in the labor force (equation 5).

[14] The optimal control results cited in this paragraph are presented in [3], Chapter 10, and in [5].

estimated effects of this variable on the price level and wage rate only become large as the unemployment rate approaches 2.5 percent. In other words, the estimated demand effects on prices and wages are generally not large, and so high-activity output levels can be achieved for relatively modest increases in inflation. This property of the model is also true when the other measures of labor market tightness discussed in the previous section are used, although it obviously makes some difference to the optimal control results which nonlinear function of the unemployment rate is used. This basic result of small estimated demand effects on prices clearly has important policy implications if it is in fact an accurate characterization of the real world.

Another important property of the model is that the price of imports has a fairly large effect on the domestic price level. As can be seen from equation 9, an increase in *PIM* of, say, 1 percent has an impact effect on *PF* of 0.0702 percent and a long-run effect (ignoring all variables in the equation except the lagged dependent variable) of 0.327 percent. Prior to 1969 *PIM* grew very little, and so it contributed little to the domestic inflation rate. (For the 1952 I - 1968 IV period the annual average rate of growth of *PIM* was 0.05 percent.) For the 1969 I-1972 IV period, *PIM* grew at an average annual rate of 6.17 percent, and so it contributed somewhat more. The largest contribution of *PIM* to the domestic inflation rate, however, was during the 1973 I - 1974 IV period, when it grew at an average annual rate of 34.37 percent.

In order to see the contribution of *PIM* to the domestic inflation rate during the 1973-1975 period in the model, the following experiment was performed. A perfect tracking solution was first obtained by adding the estimated residuals to the stochastic equations. The model was then simulated for the 1973 I - 1975 IV period (using these same estimated residuals) under the assumption of a 6 percent annual rate of growth of *PIM*. The results of this simulation for selected variables are presented in Table 2. The results show that had *PIM* only grown at 6 percent, there would have been no double digit inflation in the United States. The GNP deflator, for example, would have risen at an annual rate of about 3 percent in 1974 rather than at the actual rate of about 11 percent. Also, real output growth would have been larger, and the unemployment rate would have been about 1.5 percentage points lower by the end of 1975. This experiment is useful in that it demonstrates, in addition to the large influence of *PIM* in the model, that there can at times be a positive relationship between inflation and unemployment.

The final property that will be discussed here is the effect of the Fed in the model. In the version of the model used for the *PIM* results in Table 2, the behavior of the Fed is endogenous. The Fed is assumed to choose each period an optimal value of the bill rate and then to achieve this value through changes in its policy variables. The equation explaining Fed behavior, which is presented and discussed in [4], has the bill rate on the left-hand side and variables that seemed likely to affect the Fed's optimal value of the bill rate on the right-hand side. The right-hand side variables include the lagged bill rate, the lagged rate of inflation, the current degree of labor market tightness (as measured by J_t^*), the current and lagged growth rate of real GNP, and the lagged growth rate of the

TABLE 2

The Estimated State of the Economy for 1973 I – 1975 IV

a) if import prices had grown at a 6 percent annual rate.
b) if the bill rate had been 5 percent.

	1973				*1974*				*1975*			
	I	*II*	*III*	*IV*	*I*	*II*	*III*	*IV*	*I*	*II*	*III*	*IV*
%GNPD												
Actual	5.7	7.1	7.5	9.7	8.5	11.3	11.6	12.6	10.9	5.7	7.3	6.3
a)	5.2	4.9	4.7	5.2	1.5	2.3	2.5	4.4	3.9	1.5	4.4	2.4
b)	5.6	7.0	7.2	9.9	8.7	11.9	11.8	12.6	10.9	5.9	7.5	5.2
UR												
Actual	4.9	4.9	4.8	4.7	5.0	5.1	5.6	6.5	8.2	8.8	8.5	8.3
a)	4.9	4.9	4.8	4.7	5.0	5.0	5.3	6.1	7.4	7.7	7.1	6.8
b)	4.9	4.8	4.5	4.1	4.1	3.9	4.0	4.7	6.2	6.9	6.6	6.4
%GNPR												
Actual	9.6	0.4	1.8	2.0	–4.0	–1.8	–2.5	–5.5	–9.6	6.4	11.4	3.0
a)	9.7	0.5	2.0	2.6	–3.0	–0.2	–0.3	–2.6	–5.2	11.8	15.1	4.2
b)	9.9	1.3	3.6	4.5	–2.8	–0.0	–0.5	–3.4	–7.4	7.9	12.4	3.3
%WFF												
Actual	11.5	6.5	9.8	9.3	3.3	8.9	8.5	13.0	11.6	7.2	7.5	5.8
a)	11.2	5.4	8.2	6.8	–0.5	3.7	3.0	7.5	6.6	3.6	4.6	3.7
b)	11.4	6.5	9.8	9.7	3.7	9.4	9.0	13.3	11.6	7.1	7.3	5.7
RBILL												
Actual	5.6	6.6	8.4	7.5	7.6	8.3	8.3	7.3	5.9	5.4	6.3	5.7
a)	5.6	6.6	8.2	7.1	7.1	7.4	7.1	6.1	4.9	5.1	6.7	6.4
b)	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0
%PIM												
Actual	13.8	33.3	21.9	41.9	63.7	62.8	33.8	13.6	8.5	–5.2	0.1	–4.7
a)	6.0	6.0	6.0	6.0	6.0	6.0	6.0	6.0	6.0	6.0	6.0	6.0
b)	13.8	33.3	21.9	41.9	63.7	62.8	33.8	13.6	8.5	–5.2	0.1	–4.7

Notes: *%GNPD* = percentage change in the GNP deflator (annual rate).
UR = civilian unemployment rate.
%GNPR = percentage change in real GNP (annual rate).
%WFF = percentage change in the wage rate (annual rate).
RBILL = three-month Treasury bill rate.
%PIM = percentage change in the price of imports (annual rate). *PIM* is an exogenous variable.

money supply. The behavior that is reflected in this equation is behavior in which the Fed "leans against the wind." As the economy expands or as inflation increases, the Fed is estimated to cause the bill rate to rise. From the *PIM* results in Table 2 it can be seen that the model predicts that the bill rate would have been smaller at the beginning of the 1973–1975 period and larger at the end had *PIM* grown at 6 percent rather than at its actual rate. In other words, the net effect on Fed behavior of the lower inflation and higher real growth that resulted from the lower *PIM* growth was, according to the model, for the Fed to target lower bill rates at the beginning of the period and higher bill rates at the end.

In order to examine the effects of the relatively high interest rate policy of the Fed during the 1973–1975 period, the model was simulated for this period (using the same estimated residuals as above) under the assumption that the Fed instead kept the bill rate at 5 percent throughout the period. (In other words, the equation explaining Fed behavior was dropped from the model, and the bill rate was taken to be exogenous.) The actual values of *PIM* were used for this simulation. The results for selected variables are also presented in Table 2. They show that the unemployment rate by the end of the period would have been 1.9 percentage points lower than it actually was had the Fed kept the bill rate at 5 percent. Inflation, on the other hand, would have been little changed. This is a good illustration of the above mentioned property of the model that demand variables have little effect on inflation in periods in which the unemployment rate is relatively high. It is also the case with respect to the effects of Fed behavior on inflation that higher interest rates lead, other things being equal, to higher rates of inflation because of the cost-of-capital effects on the price level. The bond rate *(RAAA)* has a positive effect on *PF* in equation 9, and the Fed has an effect on the bond rate through its effect on short-term rates. The rates of inflation in the 5 percent bill rate case in Table 2 are thus somewhat lower than they otherwise would be because of the cost-of-capital effects on inflation.

VI. An Estimate of the Accuracy of the Model

The standard procedure that is followed in examining the predictive accuracy of econometric models is to compute root mean squared errors (RMSEs) of their *ex post* forecasts. Although this is a common practice, there are a number of problems associated with it. First, it is well known that the true variances of forecast errors are not constant across time, and so RMSEs are not estimates of true variances. RMSEs are in some loose sense estimates of the averages of the variances across time, but no rigorous statistical interpretation can be placed on them. Second, as noted in the Introduction, models differ in the number and types of variables that are taken to be exogenous, and so it is difficult to compare RMSEs, which are generally based on the use of actual exogenous variable values, across models. Finally, if RMSEs are based on within-sample forecasts, as is often the case, there is the obvious danger that the accuracy of the model has been overestimated because of data mining.

In a recent study [6] I have proposed a method for estimating the uncertainty of a forecast from an econometric model. This method accounts for the four main sources of uncertainty of a forecast: uncertainty due to (1) the error terms, (2) the coefficient estimates, (3) the exogenous-variable forecasts, and (4) the possible misspecification of the model. It also accounts for the fact that the variances of forecast errors are not constant across time. Because the method accounts for all four sources of uncertainty, it is possible to use it to make comparisons of predictive accuracy across models.

I have applied this method to a recent forecast from my model, and the results of this exercise for five selected variables are presented in Table 3. For comparison purposes I have also applied the method to a forecast from an eight-order autoregressive model, and these results are also presented in Table 3. The autoregressive model is one in which each variable is regressed on a constant, a time trend, and its first eight lagged values.

Space limitations prevent a detailed discussion of the method here. Estimating the uncertainty from the error terms and coefficient estimates is a straightforward exercise in stochastic simulation, given estimates of the relevant variance-covariance matrices. The uncertainty from the exogenous-variable forecasts can also be estimated by means of stochastic simulation, although this requires that one first estimate the uncertainty of the exogenous-variable forecasts themselves. The procedure that was followed for the present results was to regress each exogenous variable in the model on a constant, a time trend, and its first eight lagged values, and then to take the estimated standard error from this regression as the estimate of the uncertainty attached to forecasting the change in this variable for each quarter. Estimating the uncertainty from the possible misspecification of the model is the most difficult and costly part of the method, and it also rests on one strong assumption. This part of the method requires successive reestimation and stochastic simulation of the model. It is based on a comparison of estimated variances computed by means of stochastic simulation with estimated variances computed from outside-sample forecast errors. The strong assumption is that the model is misspecified in such a way that for each variable and length of forecast, the expected value of the difference between the two estimates of the variance is constant across time. Given this assumption, it is possible to estimate the total variance of the forecast error for each variable and length of forecast. The square roots of these estimated variances are printed in the *d* rows in Table 3. These results are based on 35 sets of estimates of each model.[15]

Comparing the results in the *d* rows in Table 3, it can be seen that my model is more accurate than the autoregressive model for the GNP deflator, the unemployment rate, real GNP, and the bill rate. It is less accurate for the wage rate. With respect to the GNP deflator, the estimated standard error of the eight-quarter-ahead forecast is 3.48 percent for my model and 6.20 percent for the autoregressive model. With respect to the wage rate, the estimated standard

[15] All sample periods for my model began in 1954 I, and all sample periods for the autoregressive model began in 1954 II. For the first set of estimates of each model the sample period ended in 1968 IV; for the second set the sample period ended in 1969 I; and so on through 1977 II. For the results in Table 3 except the d-row results, and for all the results in the previous sections, the sample period ended in 1977 IV.

TABLE 3

Estimated Standard Errors of Forecasts for Five Variables

a = uncertainty due to error terms.
b = uncertainty due to error terms and coefficient estimates.
c = uncertainty due to error terms, coefficient estimates, and exogenous-variable forecasts.
d = uncertainty due to error terms, coefficient estimates, exogenous-variable forecasts, and possible misspecification of the model.

Forecast Period = 1978II–1981IV.

Model I = model in [7].
Model II = autoregressive model. For the autoregressive model there are no exogenous variables, so $c = b$ for this model.

For the unemployment rate and the bill rate, the errors are in the natural units of the variables. For the other variables, the errors are expressed as percentages of the forecast means (in percentage points).

	1978			1979				1980				1981			
	II	III	IV	I	II	III	IV	I	II	III	IV	I	II	III	IV
Model I. GNP Deflator															
a	0.28	0.35	0.42	0.47	0.51	0.55	0.59	0.61	0.64	0.65	0.65	0.65	0.66	0.67	0.68
b	0.31	0.47	0.58	0.71	0.83	0.93	1.02	1.10	1.19	1.28	1.37	1.44	1.50	1.57	1.63
c	0.44	0.67	0.84	1.04	1.21	1.36	1.49	1.62	1.75	1.88	1.98	2.09	2.23	2.35	2.43
d	0.53	0.93	1.37	1.87	2.33	2.74	3.15	3.48							
Model II. GNP Deflator															
a	0.20	0.36	0.53	0.71	0.90	1.08	1.24	1.37	1.49	1.58	1.65	1.71	1.76	1.80	1.83
b, c	0.24	0.45	0.70	1.00	1.36	1.73	2.10	2.48	2.84	3.18	3.52	3.85	4.17	4.48	4.80
d	0.45	0.94	1.53	2.25	3.12	4.05	5.10	6.20							

TABLE 3, continued

	1978 II	1978 III	1978 IV	1979 I	1979 II	1979 III	1979 IV	1980 I	1980 II	1980 III	1980 IV	1981 I	1981 II	1981 III	1981 IV
Model I. Unemployment Rate (units of percentage points)															
a	0.27	0.45	0.57	0.64	0.71	0.77	0.80	0.82	0.82	0.85	0.90	0.92	0.93	0.96	0.97
b	0.36	0.58	0.76	0.92	1.03	1.12	1.16	1.23	1.28	1.34	1.38	1.42	1.50	1.56	1.62
c	0.36	0.60	0.80	0.95	1.08	1.17	1.24	1.31	1.35	1.41	1.47	1.50	1.55	1.59	1.64
d	0.35	0.60	0.77	0.82	0.85	0.83	0.77	0.71							
Model II. Unemployment Rate (units of percentage points)															
a	0.28	0.55	0.77	0.94	1.02	1.08	1.12	1.14	1.15	1.16	1.16	1.16	1.15	1.15	1.16
b, c	0.29	0.58	0.84	1.04	1.17	1.27	1.34	1.40	1.44	1.48	1.52	1.55	1.59	1.63	1.66
d	0.36	0.74	1.12	1.48	1.73	1.91	2.07	2.19							
Model I. Real GNP															
a	0.65	0.88	1.03	1.15	1.25	1.30	1.35	1.34	1.36	1.40	1.43	1.44	1.47	1.46	1.43
b	0.67	0.95	1.19	1.38	1.49	1.59	1.66	1.69	1.77	1.81	1.82	1.84	1.88	1.88	1.94
c	0.74	1.09	1.37	1.63	1.76	1.94	2.04	2.08	2.15	2.18	2.22	2.30	2.34	2.36	2.43
d	0.80	1.23	1.54	1.96	2.27	2.51	2.48	2.27							
Model II. Real GNP															
a	0.61	1.02	1.34	1.64	1.84	1.94	2.01	2.03	2.04	2.03	2.04	2.04	2.03	2.03	2.03
b, c	0.67	1.13	1.53	1.90	2.20	2.38	2.50	2.59	2.64	2.68	2.73	2.77	2.81	2.84	2.87
d	1.09	1.93	2.72	3.45	4.01	4.32	4.58	4.74							
Model I. Wage Rate															
a	0.60	0.77	0.88	0.89	0.96	1.01	1.03	1.05	1.07	1.10	1.08	1.07	1.08	1.04	1.05
b	0.70	0.93	1.12	1.34	1.52	1.65	1.76	1.82	1.94	2.04	2.15	2.27	2.35	2.45	2.51
c	0.67	0.95	1.16	1.35	1.53	1.66	1.80	1.94	2.08	2.20	2.32	2.40	2.52	2.61	2.69
d	0.65	1.06	1.45	2.01	2.53	3.07	3.59	4.16							

TABLE 3, continued

	1978			1979				1980				1981			
	II	III	IV	I	II	III	IV	I	II	III	IV	I	II	III	IV
Model II. Wage Rate															
a	0.30	0.40	0.48	0.53	0.59	0.61	0.67	0.72	0.76	0.81	0.85	0.88	0.91	0.94	0.98
b, c	0.36	0.48	0.59	0.75	0.86	0.97	1.15	1.29	1.46	1.64	1.81	1.99	2.19	2.39	2.59
d	0.63	0.84	1.04	1.26	1.41	1.56	1.81	2.04							
Model I. Bill Rate (units of percentage points)															
a	0.45	0.67	0.78	0.84	0.91	0.93	0.97	0.98	0.97	0.98	0.98	0.97	0.97	1.01	1.03
b	0.48	0.71	0.86	1.01	1.08	1.14	1.21	1.25	1.28	1.32	1.32	1.35	1.37	1.37	1.42
c	0.49	0.72	0.92	1.06	1.16	1.25	1.31	1.37	1.44	1.51	1.53	1.54	1.56	1.58	1.61
d	0.61	0.96	1.08	1.17	1.31	1.47	1.56	1.76							
Model II. Bill Rate (units of percentage points)															
a	0.46	0.72	0.80	0.85	0.90	0.93	0.94	0.94	0.94	0.94	0.96	0.99	1.02	1.03	1.04
b, c	0.47	0.77	0.93	1.05	1.14	1.20	1.22	1.19	1.16	1.14	1.11	1.14	1.15	1.16	1.18
d	0.69	1.12	1.33	1.51	1.63	1.71	1.77	1.83							

Note: d row estimates are available only for the first eight quarters.

error of the eight-quarter-ahead forecast is 4.16 percent for my model and 2.04 percent for the autoregressive model. For the unemployment rate the estimated standard error of the eight-quarter-ahead forecast is 0.71 percentage points for my model and 2.19 percentage points for the autoregressive model.

The estimates in Table 3 do not show how the models performed in any particular period, and this is sometimes useful information. The 1973-1975 period is one of the most difficult to forecast, and so it is of some interest to see how the models performed during this period. There is, of course, a serious problem with examining the performance of a model for any given period, which is that some assumption must first be made about the exogenous-variable values. For present purposes, I have used actual values of the exogenous variables to examine the performance of my model during the period, and this should be kept in mind in the following discussion. The model is not as accurate as the following results reveal in that the uncertainty from the exogenous variables has been ignored. The results are presented in Table 4 for five selected variables. Results for the autoregressive model, which has no exogenous variables except the time trend, are also presented in Table 4.[16]

Consider the results for my model first. With respect to the GNP deflator, the model forecast the double digit inflation quite well, although the rate of inflation is somewhat overestimated for the outside-sample results. The price of imports is, of course, the key exogenous variable that is affecting the predictions of inflation during this period. The rate of wage inflation is considerably overestimated for the outside-sample results. The coefficient estimates of the wage equation changed considerably from the sample periods that ended in 1973 IV or before to the sample periods that ended in 1974 III or after, and this is in fact the primary cause of the large d-row estimates for the wage rate in Table 3. For the more recent sample periods the coefficient estimate of the lagged dependent variable in the equation is larger. This difference reflects itself in Table 4 in larger outside-sample than within-sample predictions of the wage rate. With respect to the unemployment rate, the outside-sample predictions are more accurate than the within-sample predictions because (speaking loosely) of the larger inflation-rate predictions, and this reflects itself in Table 4 in more accurate predictions of real GNP and the unemployment rate. With respect to the bill rate, the outside-sample predictions are much lower than the within-sample predictions by the end of the period. The Fed was estimated to respond less to the inflation rate for the sample period that ended in 1972 IV than it was for the sample period that ended in 1977 IV, and this is the main reason for the different bill rate predictions in Table 4.

[16] The predicted values in Table 4 are computed from deterministic simulations (i.e., by setting the error terms equal to zero and solving once) rather than from stochastic simulations. As can be seen from Table 3 in [6], the predicted values computed from deterministic simulations are quite close to the mean values from the stochastic simulations for the two models. This result has also been obtained by a number of others for different models. There thus seems to be little harm in the present case in using deterministic simulations for the results in Table 4.

TABLE 4

Predicted Values for 1973 I – 1975 IV
(Dynamic Simulation for Each Model Beginning in 1973 I)

Model I = model in [7]
Model II = autoregressive model.
OS = outside sample. (Sample period for the coefficient estimates ended in 1972 IV.)
WS = within sample. (Sample period for the coefficient estimates ended in 1977 IV.)

	1973				1974				1975			
	I	II	III	IV	I	II	III	IV	I	II	III	IV
%GNPD												
Actual	5.7	7.1	7.5	9.7	8.5	11.3	11.6	12.6	10.9	5.7	7.3	6.3
Model I:												
OS	7.4	9.7	9.5	9.7	11.0	11.4	13.3	12.4	11.1	10.5	9.5	8.2
WS	6.3	8.5	8.2	8.6	9.7	10.4	11.6	10.8	9.8	9.5	8.6	5.9
Model II												
OS	3.5	3.2	3.7	3.1	2.9	3.1	2.8	2.6	2.6	2.4	2.2	2.2
WS	4.6	5.0	5.6	5.9	6.3	6.5	6.7	6.8	6.8	6.8	6.7	6.7
UR												
Actual	4.9	4.9	4.8	4.7	5.0	5.1	5.6	6.5	8.2	8.8	8.5	8.3
Model I:												
OS	4.9	4.7	4.9	5.2	5.8	6.4	6.9	7.5	8.0	8.4	8.3	8.2
WS	4.9	4.6	4.6	4.7	4.9	5.1	5.3	5.6	5.9	6.2	6.3	6.2
Model II:												
OS	5.2	5.1	5.0	5.1	5.2	5.2	5.2	5.2	5.2	5.2	5.1	5.1
WS	5.2	5.2	5.3	5.5	5.6	5.7	5.8	5.9	5.9	6.0	6.0	6.1
%GNPR												
Actual	9.6	0.4	1.8	2.0	−4.0	−1.8	−2.5	−5.5	−9.6	6.4	11.4	3.0
Model I:												
OS	7.0	3.4	2.5	0.7	−2.0	−0.2	−2.4	−2.7	−2.9	−2.6	5.0	3.4
WS	6.9	3.6	3.0	1.7	0.0	2.0	−0.1	−0.1	−0.3	−0.3	6.1	3.8
Model II:												
OS	4.5	2.9	2.2	2.5	2.2	2.6	3.3	3.1	3.0	2.8	2.7	2.5
WS	4.7	2.2	1.6	1.6	0.7	1.1	2.0	2.0	1.9	2.2	2.2	2.0
%WFF												
Actual	11.5	6.5	9.8	9.3	3.3	8.9	8.5	13.0	11.6	7.2	7.5	5.8
Model I:												
OS	10.3	11.9	12.1	11.4	12.8	12.2	15.1	14.4	13.0	12.2	11.8	10.4
WS	6.8	8.3	8.7	8.6	9.6	9.4	11.4	10.8	10.0	9.5	9.2	8.3
Model II:												
OS	5.5	6.9	5.8	6.3	5.9	6.5	6.3	6.3	6.2	6.2	6.2	6.3
WS	7.2	6.5	8.4	6.9	6.5	8.1	7.4	7.5	8.1	7.6	7.6	8.1
RBILL												
Actual	5.6	6.6	8.4	7.5	7.6	8.3	8.3	7.3	5.9	5.4	6.3	5.7
Model I:												
OS	5.5	5.7	5.9	5.8	5.6	5.2	4.8	4.2	3.6	3.0	2.8	2.7
WS	5.7	6.0	6.4	6.6	6.8	7.0	7.2	7.2	7.1	6.8	6.9	7.0
Model II:												
OS	5.5	5.9	6.2	6.5	6.8	6.8	6.8	6.7	6.6	6.5	6.4	6.3
WS	5.2	5.8	6.3	6.7	6.9	7.0	7.0	6.9	6.7	6.5	6.3	6.2

Notes: See notes to Table 2.

The predictions from the autoregressive model are about as expected in Table 4. They show less variability across time than do the predictions from my model. The autoregressive model considerably underpredicts the rate of change of the GNP deflator throughout the period. It also tends to underpredict the rate of change of the wage rate, although on average the model is more accurate with respect to the wage rate than it is with respect to the GNP deflator. The error in predicting the unemployment rate by the end of the period is 3.2 percentage points for the outside-sample results and 2.2 percentage points for the within sample results.

VII. Summary and Conclusion

I have reviewed in this paper that part of my recent theoretical and empirical work that relates to the explanation of inflation and unemployment. The discussion in Section II is meant to provide a general idea of the theoretical framework upon which the empirical work is based. The determinants of labor supply are those factors that affect the solutions of the multiperiod optimization problems of households, including expectations of future values and possible loan and hours constraints. The determinants of prices, wages, and labor demand are those factors that affect the solutions of the multiperiod optimization problems of firms, including expectations of future values and possible loan and labor constraints. Disequilibrium can arise in the system because of expectation errors. Because of the many factors that affect the decisions of households and firms, it has been argued in this paper that there is no particular reason to expect the relationship between inflation and unemployment to be stable over time.

The main conclusions from the empirical work are the following:

1. The aggregate data do not appear to be able to distinguish among alternative measures of labor market tightness as the measure to include in price and wage equations. Essentially the same fits of the price and wage equations were obtained using 1) the standard unemployment rate, 2) the unemployment rate for married men, 3) Perry's weighted unemployment rate, 4) a detrended employment-population ratio, and 5) various nonlinear functions of these variables. Therefore, any policy conclusions that are sensitive to a particular measure used in a price or wage equation do not appear to be supported by the aggregate data.
2. Irrespective of which measure is used, the effect of labor-market conditions on prices and wages is fairly small except when the labor market is very tight. Because of this, optimal control experiments with the model tend to result in more closely met output than inflation targets.
3. The estimated effect of import prices on domestic prices is fairly large, and the large increase in import prices in the 1973-1975 period is, according to the model, the cause of the double digit domestic inflation rates during this period.

4. The cost of capital is estimated to have an effect on the price level. This means that Fed behavior that results in higher interest rates is, other things being equal, inflationary.

The estimates of the model's accuracy that are presented in Table 3 should help one in deciding how much confidence to place on future forecasts from the model. I am, of course, somewhat embarrased that my model is less accurate than the autoregressive model for the wage rate forecasts, and all that I can say is that I hope to do better in the future. In general, however, I would say that the results in Table 3 show that my model is considerably more accurate than the autoregressive model, although I leave it to the reader to judge whether the absolute sizes of the errors for my model are small or large. The results in Table 3 can also be used as a basis of comparison for other models. Were other model builders to carry out the calculations that are necessary for results like those in the table, this would be a useful way of comparing the accuracy of alternative models. I hope in the future that this can be done and that there is a gradual weeding out of alternative explanations of inflation and unemployment until only the one best explanation remains. Then a conference like this can be devoted to complete fun and frolic on the island without any need to spend the morning listening to yet another paper on inflation and unemployment.

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Discussion

Franco Modigliani

I have been following all the speakers and discussants as though I were following a match. Whoever was speaking was right, and the next one was right too. So, now I must reconcile all these ideas.

Of course, I don't really mean everyone was right. I still have a lot of very deep reservations about the so-called equilibrium theories – particularly the emphasis on a supposed crisis of Keynesian economics and of econometric models which would have become altogether useless.

I trust that in the final version of their paper Lucas and Sargent will choose to stress that their analysis of rational expectations is not to be seen as a radical break with a hopelessly mistaken past but merely as a useful, or at least logically stimulating, contribution to an area which has long been recognized as deficient and open to the criticism of "ad hockery" – namely that of modeling expectations. It is true that there is one extreme version of rational expectation – which I have earlier labeled Macro Rational Expectations – which would largely do away with all macroeconometric models, at least for purposes of demand management. But this is an extreme formulation which rests not so much on superrational expectations as on a host of other assumptions such as competitive markets, fluid prices, including no long-term contracts, and the like.

This portion of the equilibrium theorist contribution is, in my view, of notable theoretical or logical interest and deserves high recognition but its empirical relevance is close to zero. Personally I am convinced that an empirically relevant modeling of expectations must rely on what I call the theory of nonirrational expectations. Of course, it includes rational expectations at one extreme and mechanical formulas at the other because the essence of nonirrational expectations is that they are not obviously silly, taking into account the knowledge of the time and the cost and bother of refined forecasting. One aspect of being silly is that somebody could exploit you if you held those expectations. One reason why I don't think that rational expectations play a very important role in the labor market is precisely that I do not see any easy way by which anybody can arbitrage. I can see it in the stock market, in the bond market, in the foreign exchange markets, and to some extent in the very well-organized, very highly competitive markets. As you move from these toward markets which, for a variety of reasons, are less and less close to the perfectly competitive paradigm and involve more and more features of oligopoly

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and bilateral monopoly and so forth, the more difficult it becomes both to see the relevance of very refined expectations and to show some obvious loss involved by the people who fail to hold those expectations. In particular, to assume that, empirically, people in the labor market know what price level is consistent with full employment, given announced monetary policy, are able to decide what individual wages go with those prices and then to enforce those wages promptly, just seems to be so clearly inconsistent with what we know about the process of bargaining in the labor market that nothing but errors can arise from postulating the empirical relevance of that kind of model, even as a very first approximation.

All this is relevant to the specific problem of commenting on Fair's paper because it explains a dilemma that paper poses for me. I feel his paper does show that there really wasn't any crisis of either Keynesian economics or macroeconometric models. Of course for Keynesian economics there can't be any crisis because we are talking about the implications of a pretty universal phenomenon – lack of perfect instantaneous price flexibility. One can argue how important certain lags are but on the principles there can be no "crisis." And Fair certainly has shown that there is no crisis of models, given the quality of his results. It is quite clear that the model he presents, fitted through '72 for instance, does do a remarkably good job of explaining what happened in the three very disturbed years following that period and does a great deal better than any mechanical extrapolation could have done.

To a large extent the sweeping indictment of the Lucas and Sargent paper confuses two kinds of crises. One is the crisis of whether these models have captured the world itself. The second crisis which I believe is the real problem is that the world we capture is extremely hard to tame, to cure from inflationary shocks, the new disease of '73-74 and thereafter. So the crisis is right there in the structure of the world, not in our ability to capture that structure. I think the Fair model provides strong evidence in this direction.

However, when I look in detail at the way his equations, particularly the price-wage equation, are structured, I find a number of fairly serious objections, some perhaps more logical than empirical – so that to some extent I am a little surprised though pleased that it did as well as it did. Maybe it shows – though I hate to admit it – that somebody with a different model can do as well as I, or even better!

Let me, therefore, start out with a brief criticism of those portions of the model which he reports in his paper – the labor supply equations and the price/wage sector. He has explained how conceptually he relies on the notion of constrained maximization, where economic agents are constrained either in the quantity they sell or the quantity they buy, and how he tries to take into account these constraints. I think it is an elegant formulation although it appears to result in practice in specifications similar to those of other models. For instance, for his labor supply equation it results in the participation rate being reduced when there is a slack as measured by his J variable. This specification is clearly similar to that used by other models, sometimes under the name of the "discouraged worker" effect. Let me mention also here a number of detailed objections to the labor supply equations which are of some importance because they reappear later. For instance in the participation equation for the prime group two variables appear, one of which is wealth per capita. The idea of introducing wealth is an interesting one but it seems to me it should be the wealth-income ratio rather than wealth per capita, which is fundamentally a trend.

Similarly, he has the mortgage rate and here as elsewhere all rates are nominal. It seems questionable whether the *nominal* mortgage rate should appear. Furthermore, its quantitative importance is doubtful because it only affects new buyers in the housing market, not people who already have mortgages. For this same reason I think its importance in the cost of living index is greatly exaggerated. In any event this variable is so insignificant in this equation that one can forget it.

I have a suggestion for his "moonlighting" equation. This subject has concerned me a great deal recently because of my interest in Italy, an economy that seems to rest on moonlighting. It has been estimated that something like 25 percent of the jobs in Italy are second jobs or somehow jobs outside of the regular market where workers and employers don't pay taxes, don't pay social security, etc. In Italy moonlighting is spectacular and in many other parts of Europe it is important but I think it is a universal phenomenon and my suspicion is that one important variable here is the number of hours worked per week. As unions and other forces have reduced the number of regular weekly hours, people who want to work more than the regular hours, end up working several shifts. So I hope Fair can test the effect of the number of average weekly hours including its cyclical aspects. I suspect that if the hours you work in your main job are curtailed for whatever reason, then you are more likely to take a second job.

Let me come to the price/wage sector which is by far the most important for our purposes. Here I was disturbed by the layout from the very beginning because it runs pretty much against my fundamental view of the wage price mechanism, a view which I am not prepared to abandon lightly. I am referring to the notion that the wage equation, the wage bargaining sector, is the one in which *money* wages are set, whereas *real* wages are set in the business sector where prices are set. In other words, we'll bargain about the wage. But once I know the wage I set the price and that gives the real wage. That is fundamental in our system. Wages are not continuously set but prices are, so that fundamentally the real wage is something which is set in the business sector. This should be described by a price equation that sets prices for given wages (and other variables affecting cost and possibly demand). The wage equation might involve a number of variables but should not involve *current* prices, at least for a short enough time period.

In the light of these considerations equation 9 in no way fits my model of price determination for given costs for two fundamental reasons. One is that, if it sets prices for given wages, then it must take into account unit labor costs, therefore productivity, at least through a time trend. As far as I know, other price equations typically include a measure of productivity or a time trend. The

MIT–PENN–SSRC happens to use both. This equation has neither. That's the first reason why it disturbs me. The second is the fact that the coefficient of wage is extremely small. Of course, to determine the long-run properties one must allow for the lagged dependent variable. But if one does that, one still finds that the wage coefficient is well below one-half. That just doesn't square for an equation which is an aggregate value-added equation in the economy and so doesn't include material costs.

The foreign import price coefficient is very big but I am willing to agree with that on the ground that foreign prices do enter here not just as costs but also as foreign competition which may affect markups. This experience is confirmed in very open countries like Italy where the indirect effect from competing import prices is much larger than the direct cost effect. The other disturbing feature is that the sum of the coefficient is less than one, although in this equation the departure is not so striking. In other words the equation is not homogeneous of the degree one, which means that in the long run, if wages and foreign prices rise, prices will not rise at the same rate.

But this problem gets much more serious in the wage equation. If the wage equation sets money wages, then no time trend is needed and productivity is not important. Some formulations which I think are quite acceptable suggest a role for productivity, e.g., the hypothesis that money wages adjust gradually to the difference between real wages and productivity. So one might include productivity, although I found in our own work that for the United States one could do as well without it. But in Fair's formulation the time trend appears only in the wage equation. Since the wage equation has current wages and current prices, there is a terrible identification problem. As far as I know, the wage equation really is the price equation, in which prices depend on current wages and time trend and the demand pressure variable. If you look at the MIT–PENN–SSRC equation, that is fundamentally the way the price equation looks.

My next qualm is that wage equations have usually been designed to explain not the level of wages but the change of wages, which is seen as a response to demand pressures and the like. Fair's equation instead "explains" the level. Of course, if on the right-hand side you had the level lagged or a series of lag terms adding up to one, then one would always be able to interpret it as a wage-change equation. But again this is not the case here – the lagged variables do not add up to one.

To be sure because the wage and price equations form a simultaneous system, to understand fully the behavior of wages and prices implied by his model one must solve the equations simultaneously and look at the reduced forms. When I did this, and assuming that I had the better of a somewhat messy algebra, I find that in the last analysis demand pressure determines the *change* and not the *level* of prices or wages, as it should. But I also found two less agreeable surprises. The first is that Fair's model implies a nonvertical long-run Phillips curve – a stable long-run tradeoff between inflation and unemployment or whatever is used for his J variable. I personally am not terribly upset by a nonvertical Phillips curve, and indeed am inclined to the view that this is a good approximation at least for excess unemployment. However, his is nonvertical throughout and the departure from verticality is appreciable. The second surprise is that the rate of change of wages depends on the time trend. That doesn't make much sense, because it implies that inflation will grow in time no matter what the demand pressure might be. In summary, the wage equation seems to me particularly unsatisfactory, and so I was not too unhappy that the one equation to which I objected most is the one that repeatedly does least well.

Turning to a couple of other matters relating to the wage price model, Fair questions why the Phillips curve should be stable, given all the variables that appear in it. But the issue of stability depends on its meaning. In the course of a private exchange with Sargent and Lucas we concluded that what they really meant by a "stable" Phillips curve is the existence of a long-run tradeoff between employment (or unemployment) and inflation. The instability to which Fair seems to refer on the other hand is that of the short-run relation between inflation and unemployment. Obviously as soon as you have lagged prices, the Phillips curve understood in this sense is not going to be stable. But as we have just seen, Fair's Phillips curve is indeed "stable" in the Lucas and Sargent sense (though its behavior might be affected by a few minor exogenous variables).

Next a few words about the effect of interest rates on prices in Fair's model – a feature which he regards as important and novel. My first comment here is that this effect, properly understood, is quite classical and follows from well-behaved production functions and competitive behavior. Specifically, a rise in the *real* interest rate must increase prices relative to wages – i.e., reduce the real wage (technology constant). But clearly what must be relevant is the real rate. Yet Fair uses the nominal rate. Thus his model has the most questionable implication that a higher rate of inflation by resulting in higher *nominal* rates reduces real wages – an implication which incidentally is clearly rejected for the United States by an analysis of the relation between inflation and income shares. Finally, to acknowledge that a rise in interest rates will raise prices relative to wages does not justify saying that it will raise prices; that depends on many things and, in particular, on monetary policy.

One last comment relates to Fair's remarks that the effect of demand pressure on inflation is so highly nonlinear. I think he should be very careful here because his J variable is by construction a complicated highly nonlinear function of U. But as he shows himself, he would get just about the same result if he used log U, which is not so nonlinear and I would suggest that, if he tried just plain U, as I have, he might be surprised to find that it does not make much difference either. So we really do not know at this time whether these effects are highly nonlinear. I have desperately looked for clear evidence of nonlinear effects in which I firmly believe but with little success so far. Yet the curvature of the Phillips curve is a very important characteristic for the choice of anti-stagflation demand policies.[1]

Now turning to the forecasting results, I was really quite pleased with what I saw. It seems to me that the first set of results from the simulation in Table 2 is

[1] Cf. F. Modigliani and L. Papademos, "Optimal Demand Policies Against Stagflation," *Weltwirtschaftliches Archiv,* forthcoming in the December issue, 1978.

quite interesting and quite credible. I do believe firmly that what happened in the period '73–'75 was a result of two combined effects – a carry-over of excess demand including the over-expansion of '72 and perhaps early '73; and the great oil problem of '74 which, per se, plays a very large role. Therefore I certainly am inclined to agree with the result that with no oil problem the picture would have been a great deal different even though I find his simulations a little too optimistic in the sense that prices come down rather fast, perhaps faster than I would have expected. The general picture does seem too square, however.

These results, incidentally, also square well with a very elementary reduced-form equation of the wage price sector which Papademos and I have fitted and reported in the Brookings Paper and which explains inflation in terms of unemployment, food prices, and import prices. That equation, which we fitted through 1971, explains quite well what happened in 1974 and 1975, largely in terms of the role of the exogenous prices.

Among the contributions of Fair's paper one of the most valuable in my view is that of the decomposition of the errors of models carried out in Table 3. In the past people had gone as far as looking at the effect of errors of a single equation, and then to their joint effect, by stochastic simulations relying on the joint distribution of such errors. But Fair goes on to an exercise which everybody agreed needed doing, but has actually seldom been done – namely, to examine the effect of errors of the coefficients. And finally he goes on to an important and novel step, that of trying to estimate the effect of possible misspecification of the equation. Essentially he asks how does the equation perform out of the sample as compared with its in-sample errors, because if the equation is misspecified – in particular in order to fit the historical data (data mining) – then as soon as you go out of sample you should do poorly. So his decomposition is a very interesting and valuable one and the results of his Table 3. shed interesting light on the results of various other tables. What is particularly interesting is that simple autoregressive schemes can do quite well in forecasting one period ahead but over longer horizons the forecasts quickly deteriorate because their coefficients seem to be extremely unstable – which is precisely what one would expect of reduced forms. The difference in this respect between structural and autoregressive "reduced" forms is quite striking, except in the case of wages.

Let me finally come to Table 4. It is the most relevant one to judge whether models fitted to earlier periods failed to account for what happened during the great inflation of '74 and the great contraction of '75. I think the results in this table are really quite encouraging. When extrapolated out of sample using the true exogenous variables to 72:4 this model does remarkably well in every respect. The only really surprising feature of Table 4 is that, on the whole, the outside-sample forecast does better than the within-sample forecast. I am at a loss for an explanation, unless it is plain chance.

I would like to raise here an issue that is relevant to the tests of Table 4 and also to McNees' tests. The point has been made that, in comparing an out-of-sample forecast with an out-of-sample extrapolation of a time series model, the procedure of relying on the actual value of the exogenous variables, as is normally done, may load the test in favor of the econometric model. To even things up the exogenous variable used in the model's extrapolation should also be forecast by some mechanical formula. In my view, however, it would be appropriate and highly desirable to have an intermediate step which examines the accuracy of the model conditional on the actual value of the policy variables – with the remaining noncontrolled variables still projected by whatever means. This test is appropriate since the models hypothesize that the policy variables do affect the economy in the manner specified by the model. Indeed, this particular test of accuracy is clearly the one that is relevant to establish the reliability of the model for policy purposes. Furthermore, anyone who does not believe that policy variables affect the economy cannot object that this procedure biases the test. So I hope that some such tests will be worked out in the near future.

Let me conclude by stressing that, though I have chosen to emphasize points of difference, I find Fair's paper a very interesting one and that the areas of agreement far overshadow those of differences. This assessment is confirmed by the choice of points which Fair chooses to emphasize in his itemized conclusions, which turn out to be those on which disagreement, if any, is at a minimum. In conclusion 1) I might differ a bit in interpretation: the fact that the data cannot clearly discriminate between alternative measures of tightness need not be of great consequence since presumably these measures are highly intercorrelated. What it does warn against is fine tuning relying on presumed nonlinear effects of demand pressure – as I have emphasized earlier.

On the other hand I fully agree with his emphasis on the importance of exogenous prices, and with the view that the effect of demand pressure on the course of inflation, though systematic, is quantitatively distressingly small. This conclusion incidentally is consistent with my reduced form equation referred to earlier which tracks rather well through 1977. It suggests that, because the systematic effect is so weak, it is easily overshadowed by random shocks – especially when reinforced by systematically poor government policies. It also implies, unfortunately, that a policy of relying on slack to wind down inflation is bound to involve horrendous social costs.

III. Summary and Evaluation

Summary and Evaluation

Robert M. Solow

The group at this conference is fairly uniform. The speakers are all academic economists, especially if you count Geof Moore and Steve McNees as honorary academic economists. A nonprofessional would find this whole meeting very mysterious. The discussion is very abstract; it is full of insiders' language; people break into hysterical laughter for incomprehensible reasons. There are also some people here who are more directly concerned with practical matters. There are even more such people out in the streets of Edgartown, and those are people who could not care less about rational expectations or even about irrational expectations or identifying restrictions, whatever those words mean.

Practical people have been led to believe, first, that economists knew all the answers, and now they seem to believe that economists know absolutely nothing or perhaps even know negative amounts about the determinants of inflation. I guess many practical people would like to know what the truth of the matter is, and whether economics offers any guidance out of what they perceive to be a mess. I would like to assure the practical people in this room and also the ones out in the streets of Edgartown that although the battles that are fought in conferences like this appear to be fought with antique pop guns, the bullets are real and they may soon be fired at you by the Federal Reserve.

I am supposed to give my impression of where this conference leaves us, and Bill Poole will, of course, say exactly the opposite in a few minutes. Naturally I begin with my opinions, and I have to confess that I haven't had any blinding revelations in the last two mornings; but I have learned some useful things.

What really brings us here is Steve McNees' picture of the 1960s and the 1970s. In opening the conference, Frank Morris mentioned his disappointment or disillusionment – which many others share – that the analytical success of the 1960s didn't survive that decade. I think we all knew, even back in the 1960s, that as Geof put it, "inflation doesn't wait for full employment." These days inflation doesn't even seem to care if full employment is going along on the trip. McNees documented the radical break between the 1960s and 1970s. The question is: what are the possible responses that economists and economics can make to those events?

One possible response is that of Professors Lucas and Sargent. They describe what happened in the 1970s in a very strong way with a polemical vocabulary reminiscent of Spiro Agnew. Let me quote some phrases that I culled from their

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paper: "wildly incorrect," "fundamentally flawed," "wreckage," "failure," "fatal," "of no value," "dire implications," "failure on a grand scale," "spectacular recent failure," "no hope." Now if they were doing that just to attract attention, for effect, so that people don't say "yes, dear, yes, dear," then I would really be on their side. Every orthodoxy, including my own, needs to have a kick in the pants frequently, to prevent it from getting self-indulgent, and applying very lax standards to itself. But I think that Professors Lucas and Sargent really seem to be serious in what they say, and in turn they have a proposal for constructive research that I find hard to talk about sympathetically. They call it equilibrium business cycle theory, and they say very firmly that it is based on two terribly important postulates – optimizing behavior and perpetual market clearing. When you read closely, they seem to regard the postulate of optimizing behavior as self-evident and the postulate of market-clearing behavior as essentially meaningless. I think they are too optimistic, since the one that they think is self-evident I regard as meaningless and the one that they think is meaningless, I regard as false. The assumption that everyone optimizes implies only weak and uninteresting consistency conditions on their behavior. Anything useful has to come from knowing what they optimize, and what constraints they perceive. Lucas and Sargent's casual assumptions have no special claim to attention. Even apart from all that, I share Franco Modigliani's view that the alarmism, the very strong language that I read to you, simply doesn't square with what in fact actually happened. If you give grades to all the standard models, some will get a B and some a B minus on occasion, especially for wage equations, but I don't see anything in that record that suggests suicide.

I also think that the Lucas-Sargent judgment is at variance with Geoffrey Moore's findings. I would not regard Geof's findings as very optimistic for received macro theory, either. What he reported is that the rate of inflation appears to accelerate when the unemployment rate falls in the upswings of growth cycles and to decelerate when the unemployment rate is rising. Franco pointed out yesterday, quite correctly, that this almost says that the rate of inflation is high when the unemployment rate is low, and low, when the unemployment rate is high. But it is not quite the same thing. If I can draw a diagram on a nonexistent blackboard for you, you can have a curve which moves through time up to the left, one measurement rising while the other is falling, and then comes back not right down the same curve, but at a slightly higher level, and then goes up again at a still slightly higher level and then comes back down again, once more at a higher level. It will always be true that x is rising while y is falling and y is rising while x is falling, but if you combine all those points you get a scatter that has essentially zero correlation as a whole. Clearly that sort of thing can and does happen. Geof Moore's paper is certainly a problem for Lucas and Sargent but it is not an unmitigated blessing for the traditional macroeconomic view. I say traditional macroeconomic view, not Keynesian view, because if Lucas and Sargent are right, then the St. Louis Fed is as dead as DRI, and you might as well realize that.

A second possibility is not to go so far as Lucas and Sargent in crying catastrophe, but to suppose that the underlying socio-economic structure has changed. Of course it is always possible, and I believe that this is what Lucas and Sargent would do, to define the structure of the economy as what doesn't change. I think that tactic is futile because it asks more of economics than economics can ever possibly deliver. So I would suggest that another possible response to the events of the 1970s is to suppose that the doctrines of the 1960s were right for the 1960s, and that the situation has changed in the 1970s, so naturally the earlier theories tend to break down. But that would be much too easy, too relaxing. There is a very valuable and important point which is in very large part due to Lucas and Sargent, and one must give them credit for it, that what often looks casually like a change in structure is really the economic system reacting to its own past. It is possible that what happened between the 1960s and the 1970s is a kind of loss of virginity with respect to inflationary expectations. That doesn't mean that it cannot ever be regained. It may be that, if we could only get back to stable conditions for a while, the 1960s might come around again. Needless to say, I am not very confident about that. I also suspect that Lucas and Sargent have a good point about the game between the government and the private sector. You don't have to buy all or most of their whole apparatus to see that monetary policy has become a very peculiar animal when big money supply numbers are regarded by the financial press as deflationary news. And that is indeed a symptom of the game between the private economy and the stabilization effort of the government. In any event, I do want to mention this possible response to McNees' remarks precisely because it is not a popular thing to say; it seems to go against the science of economics. Physicists don't expect the velocity of light to change from one decade to the next, so why should underlying economic structures change from one decade to the next? But the economic world is not exactly like the physical world, and it is not a wholly unreasonable story to tell, that the theories and doctrines of the 1960s were right for the 1960s, only, as in the old television program, they were bound to self-destruct after some interval of time.

There is still another, even less cataclysmic, line of thought that one could take about recent events. Up until very recently, for historical reasons, macroeconomics had devoted almost all of its efforts to refining its understanding of the components of aggregate demand. Consumption functions were a dime a dozen, or ten a minute anyhow, investment demand equations were all over the place, and money demand equations were being estimated daily or hourly. Macroeconomics had utterly neglected to elaborate the supply side of the models. Not surprisingly, then, the sequence of supply shocks in the 1970s from the side of food, oil, nonfuel minerals, and the depreciation of the dollar caught the macroeconomics community by surprise. From this standpoint you would say that those bad points for the 1970s on Steve's graph are simply the track left by a series of supply shocks in a two-dimensional diagram that is ill-equipped to handle them. We know now that it is possible to rebuild the supply side of macro-models so that they do tell a consistent story and can explain the 1970s. That is what Lawrie Klein meant yesterday when he said that if he takes the current Wharton model and imposes a fiscal policy impulse, a demand side shock, he gets a track in the inflation-employment plane that slopes down to the right, but that if he imposes a food or oil price increase, he gets a path that slopes up to the right. Ray Fair's model that we talked about this morning quite clearly does the same thing. So fast does the economics profession move now that there are already text books that do the supply side quite adequately and have no difficulty in explaining, at least to the intermediate student, how the bad points in Steve's graph can in fact be explained without any revolutionary change in the structure of the model. The fact that you can reconstruct macromodels by paying a little more attention to the supply side and get a reasonable account of the 1970s is certainly better news for macroeconomics than if that could not be done. But I do want to caution you that it is not very good news, because you can almost always patch up a model after the fact. The question really is whether it will hold up into the next decade when the next unexpected event comes along. I think it might, though I would not be inclined to oversell. Helliwell gave the Link models a grade of B. Mom and Dad won't jump for joy, but that is hardly a "spectacular failure."

I rather liked the paper by the Wachters though I also disagreed with a lot of specific things in it. What I mean is that I think they were trying to do the right thing: trying to model the world as we know it with the kinds of institutions that it happens to have. I do think that the paper suffers from a modern disease, which a lot of papers suffer from, a tendency to build too much on a very thin econometric basis. In a complicated nonexperimental statistical situation, there are almost always several hypotheses which fit the data approximately equally well. We have hardly any way of distinguishing confidently among them. Even those horse races that everyone talks about don't really do the discrimination job very well, again because of that ever-present possibility of patching up a model after the fact. The time-honored device in laboratory science for solving this problem is the controlled experiment – the critical experiment. We can't perform experiments; so it is only prudent to be very leery of claims based on one or two t-ratios or on small reductions in standard errors of estimate. The significance tests we use have very little power against the next best competing alternative and I fear we tend often to forget that. I am especially uncomfortable, with long polynomial lags. (I might as well confess this although I'll be probably read out of the Econometric Society.) They usually seem implausibly long to me – not always, but very often, whenever I have some feeling about what the lag ought to be. They seem implausibly long and they also seem too sensitive to minor assumptions to be very reliable. I would not care to be burned at the stake for the Wachter paper's conclusion that lags are shorter at high rates of inflation. I don't find that implausible at all; I just don't think that I would do a Joan of Arc on behalf of that kind of conclusion.

While I am confessing, I also worry a lot about U*, the natural rate of unemployment. Here I guess I share a lot of Ray Fair's concerns. I even have trouble with the vertical long-run Phillips curve. I see its attractions very clearly, and I saw them at the very beginning. In fact, there is a peculiar inner conflict here. Deep down I really wish I could believe that Lucas and Sargent are right, because the one thing I know how to do well is equilibrium economics. The trouble is I feel so embarrassed at saying things that I know are not true. The long-run vertical Phillips curve seems so inevitable. On the other hand, nobody believes the deflationary half of the proposition. I don't know anybody who would even lie out in the sun, let alone be burned at the stake, for the belief that if the unemployment rate is U* plus epsilon and we wait long enough, there would be accelerating deflation. That part no one believes.

I even find it a bit hard to believe in the accelerating-inflation half of the story unless there is a good size zone of 5½ or 6 percent unemployment in which the acceleration is very small and very irregular. Then I can believe it, but then I do not know what implication follows.

What is the value of U*? What unemployment rate should policy aim at? Should I believe 5 1/2 percent for now as Wachter tells me I should, or should I believe the 6 percent that Henry Wallich tells me I should, or should I believe the people who tell me that whatever the unemployment rate is today is the natural rate of unemployment for today? I can't believe an answer is to be found in search theory. I regard search notions as simply empirically discredited. People don't do it. Job search is simply not a major occupation of the unemployed.

I concentrate on this point because you have to have a very good reason for believing that the natural unemployment rate is 5 1/2 percent if you want to go out and face all those people who are unemployed. It is no joke. For statisticians it is just numbers, just something that comes out when you set something equal to zero and divide one number by another. But those fellows out there are not working. You ought to be sure of what you are talking about, and that the right figure is 5 1/2 percent and not 3 1/2 or 4 1/2 percent before you pretend that it has some relevance to practical life.

Ray Fair is absolutely right: you can't get a decent estimate of a natural rate of unemployment out of aggregative data, nor is that what Mike Wachter does. Most studies that attempt an estimate of U* rely on a demographic decomposition of the labor force and of unemployment. I have never been comfortable with that. I can hardly think of any production function that specifies labor input in terms of so many women or so many men, so many 18- to 24-year olds and so many 25- to 29-year olds or anything of that sort. Presumably all this demography is proxying for skills or experience but surely it would be better, if that is what it is after, to demonstrate it directly and to talk about skilled and unskilled, and experienced and inexperienced workers rather than 18- to 24-year old males, 35- to 39-year olds females and so on. There might be something to be said for the demographic origin of U* in terms of the youth culture, mobility, sampling jobs, frequent voluntary job changes and all that. Although there again I would feel a lot better if someone could demonstrate that those voluntary turnover rates were invariant to the kinds of jobs that people were turning to and from.

One last point on U*: I want to emphasize that there is, for very good historical reasons, no evidence of the reversibility of the relationship between demography and U*. We will know soon (eight or ten years) because Mike's data say that U* will start falling pretty soon as the age structure of the population changes. I am going to be very curious to find out if that is so. At the moment we have only seen a movement in one direction, not that anyone can help it, that is just the way the demography has bounced.

It is plain as the nose on my face that the labor market and many markets for produced goods do not clear in any meaningful sense. Professors Lucas and Sargent say after all there is no evidence that labor markets do not clear, just the unemployment survey. That seems to me to be evidence. Suppose an unemployed worker says to you "Yes, I would be glad to take a job like the one I have already proved I can do because I had it six months ago or three or four months ago. And I will be glad to work at exactly the same wage that is being paid to those exactly like myself who used to be working at that job and happen to be lucky enough still to be working at it." Then I'm inclined to label that a case of excess supply of labor and I'm not inclined to make up an elaborate story of search or misinformation or anything of the sort. By the way I find the misinformation story another gross implausibility. I would like to see direct evidence that the unemployed are more misinformed than the employed, as I presume would have to be the case if everybody is on his or her supply curve of employment. Similarly, if the Chrysler Motor Corporation tells me that it would be happy to make and sell 1000 more automobiles this week at the going price if only it could find buyers for them, I am inclined to believe they are telling me that price exceeds marginal cost, or even that marginal revenue exceeds marginal cost, and regard that as a case of excess supply of automobiles. Now you could ask, why do not prices and wages erode and crumble under those circumstances? Why doesn't the unemployed worker who told me "Yes, I would like to work, at the going wage, at the old job that my brother-in-law or my brother-in-law's brother-in-law is still holding", why doesn't that person offer to work at that job for less? Indeed why doesn't the employer try to encourage wage reduction? That doesn't happen either. Why does the Chrysler Corporation not cut the price? Those are questions that I think an adult person might spend a lifetime studying. They are important and serious questions, but the notion that the excess supply is not there strikes me as utterly implausible.

The story that Mike Wachter tells rests a little too much on what he calls "cognitive limitation" or bounded rationality. The fact is true. Even we in this room have cognitive limitations. Ordinary mortals are allowed. But I would not emphasize it so much. Much more important is the rest of the story, especially the bilateral monopoly situation, as I would describe it, which is protected by the value to both parties in the labor market of the continued relationship between them. That bilateral monopoly is not protected by ordinary market imperfections, by the sort of thing that the Sherman Act or the Clayton Act might outlaw, but it is protected by the value to both parties of continuing what they're doing. That relationship opens room for bargaining and simultaneously for a joint need to avoid conflict. Especially because neither party is monolithic. There are different interests on the employer's side and also on the employee's side even within the same trade union, as we know.

Another thing I would have emphasized more in the story that the Wachters tell is the asymmetry between upward and downward flexibility of wages. They mention it at the end but don't elaborate on it, and I think it is very important. Given that protected bilateral monopoly, given the imperfection of the labor market and its willingness to tolerate nonclearing, partly for noneconomic reasons, there is an asymmetry between upward and downward flexibility in wages and in many other prices as well. Then it almost automatically follows that there is a kind of inflationary bias in the system because the only way the system can generate the relative price changes that it has to bring about is by having the general price level float upward. If, in addition, it should be true as I half think it is, that there are more shocks at high levels of output that at low levels of output, at least more upward shocks, there is already going to be a tendency for prices and wages to rise more rapidly in good years than in bad years. And if there are any long institutionally determined lags in the system, then it is going to be very hard to reverse those movements. I think you can tell a good story that way – especially if it is in fact true, as I casually think, that there is more flexibility on the upside than on the downside for wages and prices. If that story is true, then it suggests two very important roles for public policy. The first is simply to avoid major shocks and to move quickly to temper them when they happen, as they inevitably will, because the more major shocks there are, the faster and longer the price level will tend to rise. The second role for public policy is to remember that shocks can originate on the supply side as well as on the demand side, so supply management of one kind or another could be as important in the future as demand management has been in the past.

Summary and Evaluation

William Poole

I have been puzzling about the title of this conference – "After the Phillips Curve – Persistence of High Inflation and High Unemployment" – and its relationship to the papers we've heard. I've concluded that we ought to think about substituting a few words in that title.

One thought I had was to change "After" to "Because of." There certainly is an element of truth in that revised title. Belief in a stable tradeoff between inflation and unemployment has had much to do with the persistence of excessively expansionary policies since 1965. But the most appropriate title is, "The Phillips Curve is Dead – Long Live the Phillips Curve."

A major theme of the conference papers is the reconstruction of the Phillips curve. As the first element in this reconstruction, everyone decided some time ago that we have to add expectational variables to the Phillips curve. Expectations did not play a prominent role in early discussions of the Phillips curve, but now we know that empirically the Phillips relation must contain distributed lags on past prices or past wages, expectations of future prices, or some similar device. Empirically, you just have to get those past or future price change variables in there somehow. This is the reason that Geof Moore's paper is written in terms of linking the unemployment rate to *changes* in inflation – the second derivative of the price level – rather than, as Phillips had it, the unemployment rate to the inflation rate, or the first derivative in the price level.

The basic idea behind the Phillips curve is that of a stable supply response traced out by demand shifts. One of the reconstructions that we spent some time on at this conference involves taking account of the fact that the supply side itself is being disturbed and so we have a problem of sorting out the supply disturbances from the demand disturbances.

There are several themes along that line. One, of course, is Moore's emphasis on employment and not just on unemployment. We have the labor force participation and the demographic effects involved as well. But the emphasis on supply shocks in the way that Larry Klein brought them in – and there was also a simulation discussed by Ray Fair – leaves me quite uneasy. I was taught that prices are endogenous variables, and that we ought not to consider market experiment or to run model simulations based on moving prices exogenously to attempt to trace out supply effects. In a model that has a food supply function, I could understand a simulation experiment in which the supply function was moved to

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the left to represent a harvest failure or similar supply disturbance. But it is not satisfactory to play simulation games where we simply change one component of the total price structure exogenously and trace out the effects.

I was especially interested in a comment that Klein made about his experiment with OPEC price increases. He said these experiments were done on the basis of information as of last fall. Then he pointed out that in fact prices had not gone up and OPEC prices, it is thought, have in fact been shaded a bit. Why is that happening? Well, it is happening in good part because the cartel is finding price shading in its own interest for a variety of reasons and perhaps because the cartel is unable to put through price increases of $2 a barrel, or $4 a barrel, or anything else. This experience provides a good example of the invalidity of simulation experiments that move prices around exogenously. In a model properly specified to study this problem, the OPEC supply function would be shifted back and the effect on the average market price would depend on the model's equations for non-OPEC supply responses and on demand responses.

The other attempt to putting the Phillips curve story back together is the new theory of the unemployment-inflation correlation over the business cycle, or what we might as well call the "Lucas curve." I will discuss the Lucas curve later rather than now because it deserves a separate section in my outline.

Another important theme in this conference, which perhaps is not obvious until you look carefully, is that there is a growing interest in careful quantitative assessment of what models mean and what they do. John Helliwell emphasized this point in his discussion of Klein's paper; the model documentation is better and the record-keeping is better. Indeed, the situation is very much improved over the situation we were in five or eight years ago when it was difficult to know what large models meant, to know how to evaluate them, and to know how accurate they were. We have come a long way in this area, because model builders have been paying attention to these very important issues of documentation that physicists and chemists and so forth are brought up on but about which economists are frequently very sloppy. In the same vein, the work by Ray Fair in attempting to formalize the model accuracy question is extremely important for the scientific assessment of what we are doing. Steve McNees' work on forecasting accuracy is also in the same tradition and, if you will, is in fact helping to define the tradition.

If we look at the quantitative assessment of model forecasts, which is extremely important for the policy makers, where do we come out? Consider, for convenience, a forecast horizon of four quarters because that seems to be far enough out to be useful, and yet not beyond the capability of the models. Of course, 8- or 12-quarter forecasts would be very useful but since we do not have much information on the accuracy of such forecasts we can use four-quarter forecasts. As I read the McNees paper I would say that as long as we are talking about policies that are in the ball-park of the experience over which the models were fit, we might reasonably expect a standard error on the unemployment rate of 1 percentage point and a standard error on the inflation rate of about 2 percentage points. This degree of accuracy is a little but not a whole lot better than ARIMA forecasts over the same horizon.

Let me turn now to a general assessment of these Phillips curve fix-ups. Have we learned the right lessons from the experience of the seventies? I think it is fair to say that we can't tell yet. The models have been adjusted; they have gone through several generations. We know that there has been a big difference between the within-sample and post-sample performance of earlier versions of current models. But do we have it right now? I think we have good reason to be skeptical that we have it right now given the record. We certainly don't have any *evidence* that models are now right because we are still talking about incorporating the events of the last few years, and don't have any post-sample observations on which the models can be tested. If we look at this issue from the point of view of skeptical empirical social scientists, then we simply have to say that we don't have the evidence that we have it right now. If you believe that the models are now correct, or will provide forecasting accuracy that it is notably improved from the record of the seventies, then that belief can only be justified on the basis of faith and not evidence. The evidence is not here yet.

I am worried about the problem of the number of observations used to estimate these models. This issue shows up particularly clearly in Moore's paper in that he is looking at the relationships over a data base of only nine growth cycle peaks and nine growth cycle troughs in the postwar period. And the evidence Moore cites for lengthening lags comes from only two observations – the last two.

When we teach our statistics courses, we all warn our students that conclusions based on nine observations are unlikely to be very reliable. Certainly two observations are even less help. The problem of small sample size is not avoided in the large econometric models. Although most models use quarterly data, the bulk of the variance in the data comes from the business cycle fluctuations. The estimated properties of the equations of econometric models primarily reflect the same limited number of cyclical episodes as studied by Moore. In fact, model equations may reflect even fewer than nine business cycles because many models are estimated over shorter time periods than the whole postwar period.

So, on a statistical basis, we really don't have reason for confidence in our empirical work. This is one of the reasons why examination of foreign experience is extremely important. That is really the only way we have to expand our data base other than waiting. Expanding our data base is a very promising thing to do; we ought to be able to extract much more information from the behavior of other economies than we have done so far.

I'll now turn to Sargent-Lucas – the focal point of the controversy at this conference. What about their challenge? One thing that struck me as I was listening to Bob Solow, and some others, is that we have had so many comments to the effect that this work is overstated, exaggerated, and so forth that I suspect that there is a great sense of unease, even among those who are very opposed or very skeptical of the Sargent-Lucas work. All these comments about their work remind me of my freshman course in philosophy. The professor was going through one proof after another of the existence of God showing how all these proofs were flawed. One of the students said, "Well, after going through all this, doesn't the fact that there are so many proofs available show something?" Do all the comments critical of Sargent-Lucas show something? I think so. The criticisms of Sargent–Lucas reflect model-defenders' efforts to maintain the intellectual case for large macromodels when everyone realizes that the models cannot possibly provide correct predictions of the effects of certain policies.

Let me put the message that I take from the Sargent–Lucas work in this way. First of all, there is no model builder in this room who would expect his model to hold up if we were to consider an experiment, let's say, of 100 percent rate of money growth in the next 12 months. No model builder expects his model to stand up in that kind of an experiment. Clearly the institutional structure in the model, the lag structure, and so forth, simply would fall apart. Now, what about 50 percent money growth? Or 25 percent maney growth? As we go down to ranges that are closer to those that we are familiar with, and we have more confidence that we are within the ballpark of the historical range of observation, then we are more confident that the models can tell us something. But that is not the end of the story. In fact, there was a comment – I guess it was by Martin Baily about the Wachters' paper – to the effect that the changes in institutional structure that we have seen such as the growth of escalator clauses seemed rather minor. Indeed, we can mention a long list of apparently minor changes – things like the shorter contract periods we had during the controls period, the more frequent salary adjustments in nonunion situations and so forth. These things all seem relatively small. They don't involve major changes in the institutional structure, and it's hard to see how they make much difference.

But that is not the point, it seems to me. The point is – if you think about a limiting process – whether the changes in institutional structure in response to policy changes are large *compared* to the changes in forecasts of economic variables in response to policy adjustments within a fixed institutional structure. After all, none of us expect very big effects from policy experiments that involve a change in the annual rate of growth of money of 1 percent for six months. If we talk about 2 percentage points for six months, or 3 or 4, as we raise the policy dose, we expect larger policy effects. But, of course, we also expect larger changes in institutional structure.

So, it seems to me useful to think about a limiting process here. Starting from a large policy dose, it is not obvious that in reducing the policy dose the stability of the institutions after a point becomes great enough that we can ignore induced institutional change in forecasting the effects that we are likely to get from policy adjustments.

I was struck by a quote on this matter from the Wachter paper. "For any significant change in the inflation rate, the speed and magnitude of Phillips' relationship, (I think they mean *changes* in the Phillips' relationship) are more important than the short-run movements along the curve." That is a useful way of looking at this issue. Are we, when we change our policy instruments, changing the structure more rapidly than we are producing effects within the given structure, as suggested by the word "more" in the quote?

If we think about the Sargent–Lucas argument in this way, it seems to me that the changing structure point is much more significant than direct examination of institutional changes by themselves would suggest. When we compare the early 1960s with the 1970s, we are talking about an inflation rate that has risen from about 1 1/2 percent to a trend rate of 6 – 7 percent currently. That is not an enormous change. Indeed, by historical standards in the United States and certainly by standards in other countries, it is not much change at all. Yet this modest acceleration in inflation has caused tremendous problems. I don't think we should be surprised if the institutional changes seem modest, because after all the institutional changes required to adjust to a change in the rate of inflation from 1 1/2 to 6 or 7 percent don't have to be very large. We just don't need a wholesale institutional revolution to cope with a small acceleration in the rate of inflation.

Well, where do I come out on all this? On the basis of forecasting evidence, we need to accept standard errors of perhaps 1 percentage point on the unemployment rate and 2 percentage points on the inflation rate with a four-quarter horizon. Perhaps the models will do better, but we don't have any evidence as yet that that will be the case. We all agree that the Lucas and Sargent criticisms are right in principle – that the world will not stay put under very large fluctuations in policy instruments. We have many disagreements as to how important that point is for policy adjustment in the ballpark of past experience.

We know a lot about the failures of existing models, failures perhaps not as serious as described by Lucas and Sargent who in their paper have used too many fighting words and not enough scientifically neutral words, but failures none the less. Clearly, the model builders have not been totally pleased with the performance of their models over the last five or ten years and are working hard to correct what they admit to be at least some modest failures.

If we are honest about what we do, I think that we have to say that we have amazingly little solidly verified information on which to base an activist stabilization policy. This view is not going to satisfy policy activists because they will rightly point out that even if we have large standard errors added to our forecasting equations – accepting for the sake of this discussion an optimal control framework – the right thing to do is nevertheless to vary the policy instruments in small continuous adjustments in response to changes in point estimates of goal variables. Continuous policy adjustment is still the right thing to do from the point of view of optimal control no matter how large the standard errors are.

But I am convinced that the optimal control framework for policy is fundamentally wrong from a political standpoint. Earlier in the conference, we talked about adjustment costs producing sluggish reaction in investment functions and so forth, and surely the same thing is true in spades in the political process. It does not make sense to emphasize adjustment costs in modeling private behavior and then to ignore the very same consideration in discussing policy. It is not cheap for the President to get small policy adjustments, or any policy adjustments through the Congress. The process is long and it involves threats to the President's credibility if it turns out that before the political process is complete the point estimates swing around and you want the policy instrument to jiggle a bit in the other direction instead. So, from the point of view of the political process, policy adjustments based on control theory models are out of the question.

I am also concerned that continuous small policy adjustments based on the models we have run a serious problem of discrediting economists. It is difficult to sell the proposition that policy instruments were adjusted to take account of a 1/2 percentage point change in the forecast of unemployment and then we came out with unemployment changing by 1 or 2 percentage points just as a result of the normal standard error we know we have around the forecast. That does not make an economist look good (particularly if unemployment goes in the wrong direction, of course) because it sounds like double-talk justifying a policy failure. Even forgetting about the actual failures, the perceived failures of macro fine-tuning injure the credibility of economists for other matters where we have a lot more to say, such as on matters like the efficiency of the regulatory process. There are a lot of things we have to say on micro-efficiency and government organization and surely we are going to injure our credibility on those issues if we push too hard in the macro area where the evidence suggests that we really do not know all that much.

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