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5 What Determines the Sacrifice Ratio?

Laurence Ball

Disinflations are a major cause of recessions in modern economies—perhaps the dominant cause. In the United States, for example, recessions occurred in the early 1970s, mid-1970s, and early 1980s. Each of these downturns coincided with falling inflation caused by tight monetary policy (Romer and Romer 1989).

Is there an iron law that disinflation produces large output losses? Or can favorable circumstances and wise policies reduce or even eliminate these costs? Economists have suggested a wide range of answers to these questions. One traditional view is that disinflation is less expensive if it occurs slowly, so that wages and prices have time to adjust to tighter policy. An opposing view (Sargent 1983) is that *quick* disinflation can be inexpensive, because expectations adjust sharply. Some economists argue that disinflation is less costly if tight monetary policy is accompanied by incomes policies or other efforts to coordinate wage and price adjustment. Finally, a number of authors suggest features of the economic environment that affect the output-inflation trade-off, such as the initial level of inflation (Ball, Mankiw, and Romer 1988), the openness of the economy (Romer 1991), and the nature of labor contracts (Gordon 1982).

Despite this debate, there has been little systematic empirical work on these issues. The speed of disinflation, the nature of incomes policies, and so on

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differ considerably across countries and disinflation episodes, but we do not know whether these differences produce important differences in output behavior. Many studies examine individual disinflation experiences, but few compare sizable numbers of episodes. Those that do consider multiple episodes focus on establishing that the output losses are generally large (e.g., Gordon 1982; Romer and Romer 1989). This paper measures the variation in the costs of disinflation across a sample of episodes, and asks whether this variation can be explained.[1]

I examine disinflations from 1960 to the present in moderate-inflation countries of the Organization for Economic Cooperation and Development (OECD). The sample contains all episodes in which trend inflation (defined as a moving average of actual inflation) falls substantially (usually more than two percentage points). Using quarterly data, I identify twenty-eight episodes in nine countries; with annual data, I identify sixty-five episodes in nineteen countries. I then develop a simple method for estimating the "sacrifice ratio" for each episode: the ratio of the total output loss to the change in trend inflation. This method is based on a new approach to measuring full-employment output during disinflation. Finally, I examine the relation between the sacrifice ratio and the variables that influence it in various theories.

There are two main results. First, the sacrifice ratio is decreasing in the speed of disinflation (the ratio of the change in trend inflation to the length of the episode). That is, as suggested by Sargent, gradualism makes disinflation more expensive. Second, the ratio is lower in countries with more flexible labor contracts. The most important feature of contracts is their duration.

I also examine the effects of initial inflation, incomes policies, and the openness of the economy. For these variables, the results range from negative to inconclusive.

5.1 Constructing Sacrifice Ratios

This section develops a method for identifying disinflation episodes and calculating the associated sacrifice ratios. This approach might prove useful for future studies of disinflation, as well as for the empirical work below.

5.1.1 Motivation

Many authors have estimated sacrifice ratios, but their techniques are not appropriate for the current study. The most common approach is to derive the ratio from an estimated Phillips curve—from the relation between output and inflation in a long time-series (Okun 1978; Gordon and King 1982). A limitation of this approach is that it constrains the output-inflation trade-off to be the same during disinflations as during increases in trend inflation or temporary fluctuations in demand. This restriction is false if the sacrifice ratio

1. A recent paper by Schelde-Andersen (1992) also attempts to explain variation in the costs of disinflation. As discussed in the conclusion, the findings are broadly similar to mine.

is influenced by factors specific to disinflations, such as incomes policies or credibility-induced shifts in expectations. Most important, the Phillips-curve approach constrains the sacrifice ratio to be the same for all disinflations within a time series. This paper estimates separate ratios for each episode to see whether the ratio varies systematically, both within the experience of a country and across countries.

A number of authors compute sacrifice ratios for particular episodes based on ad hoc estimates of the change in inflation and output losses. Mankiw (1991), for example, considers the Volcker disinflation. He notes that inflation, as measured by the gross domestic product (GDP) deflator, fell by 6.7 percent between 1981 and 1985. He assumes that the natural rate of unemployment is 6 percent, which implies that unemployment exceeded the natural rate by a total of 9.5 points from 1982 through 1985. Multiplying by an Okun's Law coefficient of two, Mankiw obtains a total output loss of 19 points. The sacrifice ratio for the Volcker episode is 19/6.7 = 2.8.

My estimates of sacrifice ratios are in the spirit of previous episode-specific estimates, but are more systematic. Previous estimates rely on judgment about the dating of episodes and the natural levels of unemployment or output. Applying such judgment on a case-by-case basis is cumbersome and raises the possibility that different episodes are treated inconsistently. I seek an algorithm for calculating sacrifice ratios that generally comes close to conventional estimates but can be applied mechanically to many episodes.

5.1.2 Selecting Episodes

The first step in my procedure is to identify disinflations—episodes in which trend inflation falls substantially. Trend inflation is defined as a centered, nine-quarter moving average of actual inflation: trend inflation in quarter t is the average of inflation from $t - 4$ through $t + 4$. This definition captures the intuition that trend inflation is a smoothed version of actual inflation. I doubt that other reasonable definitions would produce substantially different results.

To identify disinflations in a given country, I first identify "peaks" and "troughs" in trend inflation. A peak is a quarter in which trend inflation is higher than in the previous four quarters and the following four quarters; a trough is defined by an analogous comparison to four quarters on each side. A disinflation episode is any period that starts at an inflation peak and ends at a trough with an annual rate at least two points lower than the peak. These definitions assure that an episode is not ended by a brief increase in inflation in the midst of a longer-term decrease. Figure 5.1 illustrates the procedure by identifying disinflations in the United States, Germany, the United Kingdom, and Japan.

This procedure is meant to separate significant policy-induced shifts in inflation from smaller fluctuations arising from shocks. It appears quite successful. I have checked the historical record for each of the twenty-eight disinflations in my quarterly data set (mainly by reading the OECD *Economic Outlook* and OECD studies of individual countries). In every case, there is a significant

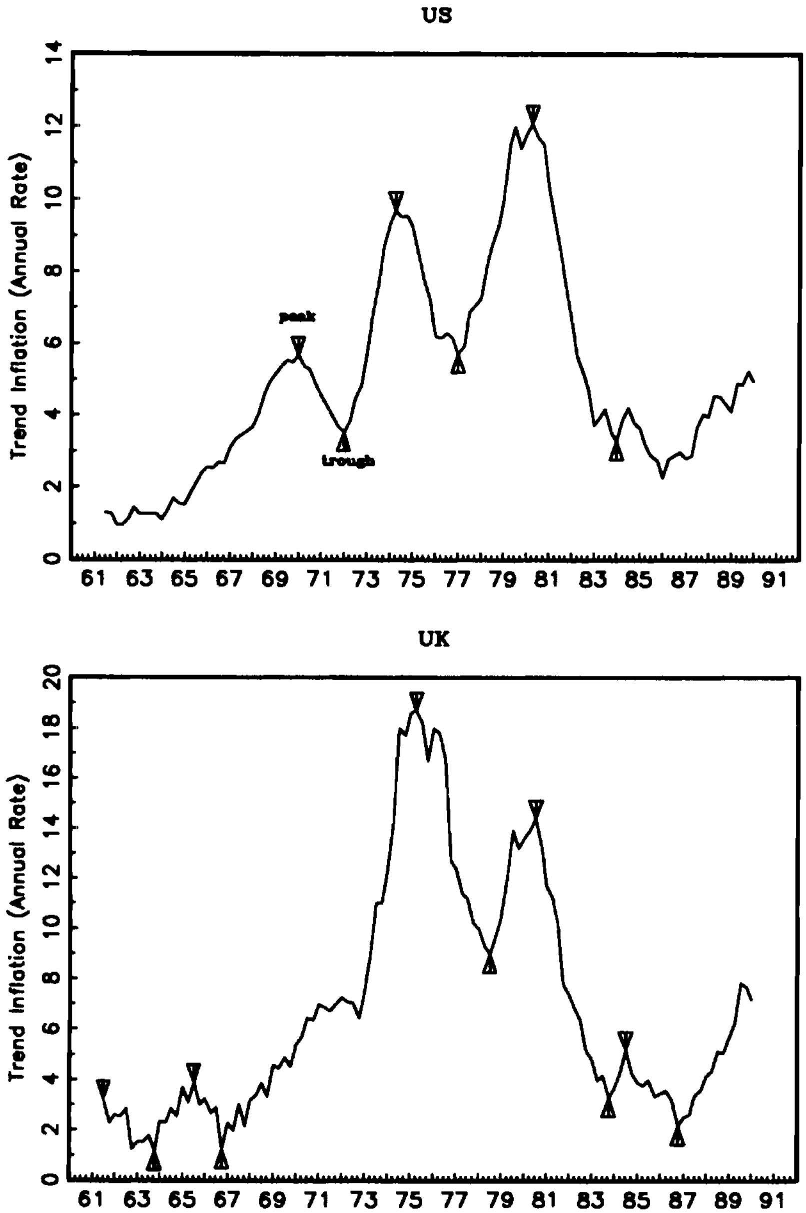
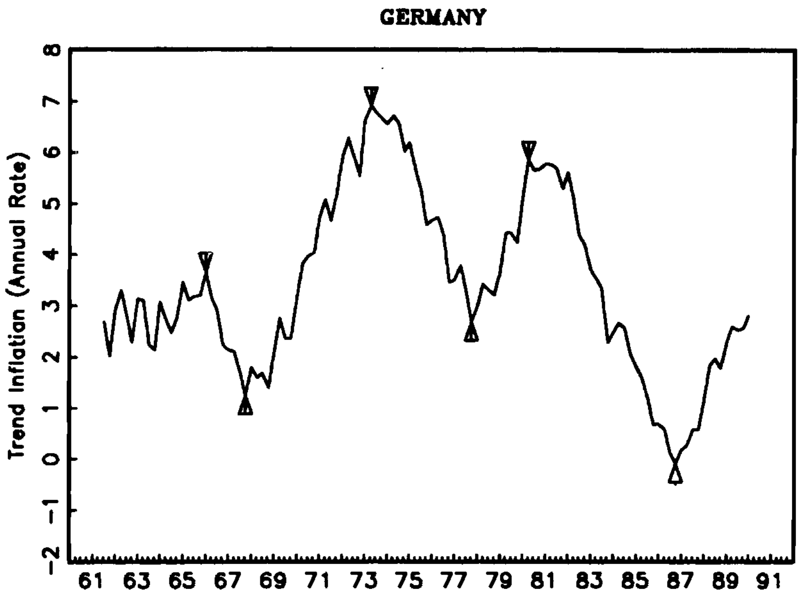
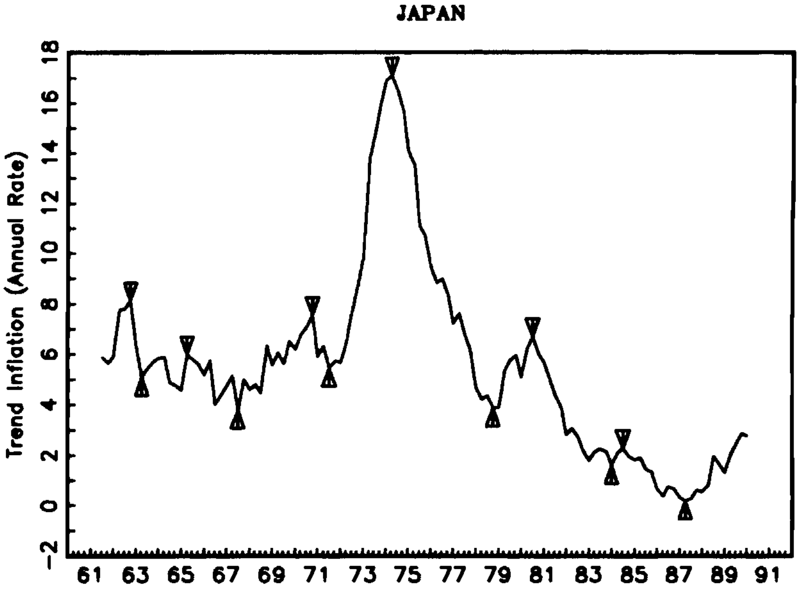


Fig. 5.1 Trend inflation and disinflation episodes


GERMANY
Trend Inflation (Annual Rate)
-2
-1
0
1
2
3
4
5
6
7
8
61 63 65 67 69 71 73 75 77 79 81 83 85 87 89 91


JAPAN
Trend Inflation (Annual Rate)
-2
0
2
4
6
8
10
12
14
16
18
61 63 65 67 69 71 73 75 77 79 81 83 85 87 89 91

tightening of monetary policy near the start of disinflation. In most cases, the motivation for tight policy is either to reduce inflation or to support the domestic currency. Declines in inflation arising primarily from favorable supply shocks, such as the 1986 decline in oil prices, are too small or too transitory to meet my criteria for disinflation. Intentional demand contractions are essentially the only source of two-point declines in trend inflation.

Indeed, a significant tightening of monetary policy is not only necessary for disinflation but also, it appears, close to sufficient. For the United States and Japan, I have compared my disinflations to the lists of monetary contractions developed by Romer and Romer (1989) and Fernandez (1992). In the United States, policy was tightened in 1968, 1974, 1978, and 1979; if the last two are treated as one episode, there is a close correspondence to the disinflations starting in 1969, 1974, and 1980. Similarly, there is a close correspondence between the six Japanese disinflations and Fernandez's dates.

5.1.3 The Sacrifice Ratio

The denominator of the sacrifice ratio is the change in trend inflation over an episode—the difference between inflation at the peak and at the trough. The numerator is the sum of output losses—the deviations between actual output and its "full employment" or trend level. The most delicate issue is the measurement of trend output, because small differences in fitted trends can make large differences for deviations.

Standard approaches to measuring trend output do not yield appealing results in this application. This point is illustrated by figure 5.2, which shows trend output in the United States and Germany calculated using a log-linear trend split in 1973 and using the Hodrick-Prescott filter. Since these methods minimize deviations from trend, they appear to understate or even eliminate recessions. In the United States, for example, output does not fall below trend during the 1980 recession. In Germany, total deviations from trend during the 1980–86 disinflation are close to zero, whereas traditional accounts of this period include a deep recession without an offsetting boom.[2]

My goal is a definition of trend output that is consistent with conventional views about the costs of various disinflations. After experimentation, I arrive at a definition based on three assumptions. First, output is at its trend or natural level at the start of a disinflation episode—at the inflation peak. This assumption is reasonable because the change in inflation is zero at a peak. The natural level of output is often defined as the level consistent with stable inflation.

Second, I assume that output is again at its trend level four quarters after the end of an episode, that is, four quarters after an inflation trough. The logic behind the first assumption suggests that output returns to trend at the trough,

2. Indeed, the HP filter almost always keeps average output over five years or so close to trend. In the United States, average output is close to trend over 1973–78 and 1978–84, which again conflicts with the usual view that these were recessionary periods.

where inflation is again stable. In practice, however, the effects of disinflation are persistent: output appears to return to trend with a lag. Four quarters is a conservative estimate of this lag in a typical disinflation. The return to trend is indicated by above-average growth rates in years after troughs. In the United States, for example, average growth in the four quarters after an inflation trough is 5.7 percent.[3]

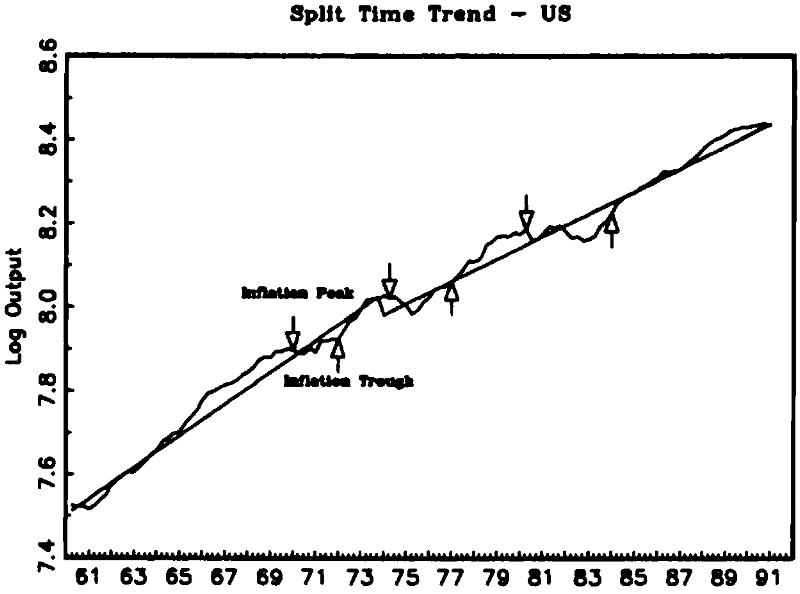
My final assumption is that trend output grows log-linearly between the two points when actual and trend output are equal. In graphic terms, trend output is determined by connecting the two points on the log output series. The numerator of the sacrifice ratio is the sum of deviations between this fitted line and log output.

Figure 5.3 plots log output and the fitted trends for the United States, the United Kingdom, Germany, and Japan. The trends are usually close to the lines one would draw by hand if doing ad hoc calculations of the sacrifice ratio.

I interpret the sacrifice ratio as the cost of reducing inflation one point through an aggregate demand contraction. This interpretation relies on two assumptions. First, shifts in demand are the only source of changes in inflation: there are no supply shocks. As discussed above, demand contractions do appear to be the main cause of the disinflations in my sample. Nonetheless, it is likely that supply as well as demand shifts occur during some episodes, and that supply shocks affect the sizes of the output losses and changes in inflation. Thus the sacrifice ratio for a given disinflation is a noisy measure of the effects of the demand contraction. This need not create a problem for my analysis, however. When I regress the sacrifice ratio on explanatory variables, the noise in the ratio can be interpreted as part of the error term.

A second assumption behind my sacrifice ratios is that trend output is unaffected by disinflation: there is no hysteresis. Recent research suggests that demand shifts can reduce output permanently (Romer 1989); that is, contractionary policy reduces trend output as well as causing temporary deviations from trend. In this case, the true undiscounted sacrifice ratio is infinite. With discounting, however, one can calculate a finite ratio with the present value of output losses as the numerator. Moreover, it is plausible that this sacrifice ratio is well proxied by the ratio computed here. My variable measures the deviation from trend output and ignores the change in the trend, but it is likely that these components of the output loss move together: a larger recession leads to a larger permanent loss. In this case, my procedure understates the sacrifice ratio in all disinflations, but accurately identifies the relative costs of different epi-

3. Both my first and second assumptions can be derived from the following model. Assume $y = a(\pi - \pi_{-1}) + by_{-1}$, where y is the deviation of output from trend. This equation is a Lucas supply function with lagged inflation proxying for expected inflation. Assume that inflation is stable before the inflation peak and after the trough. Finally, assume that b^4 is approximately zero. These assumptions imply that $y = 0$ at the inflation peak and four quarters after the trough, and that $y < 0$ between these points.



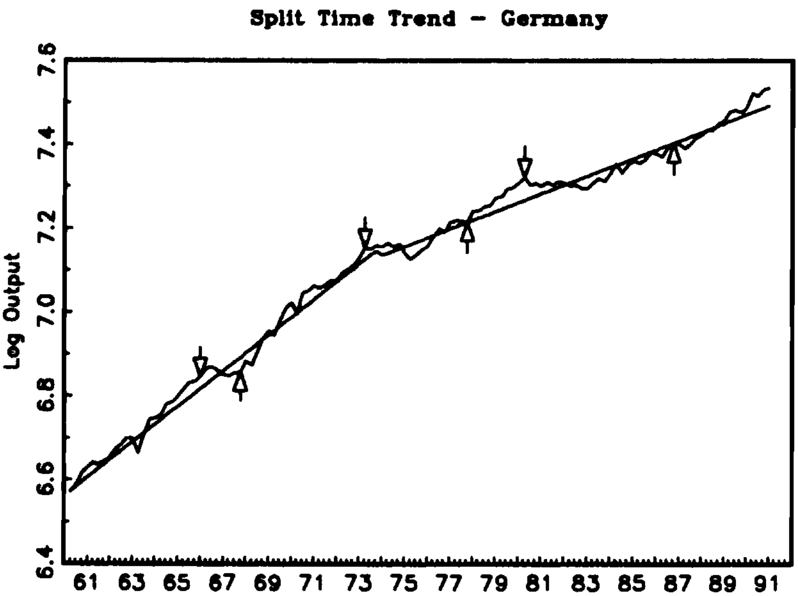
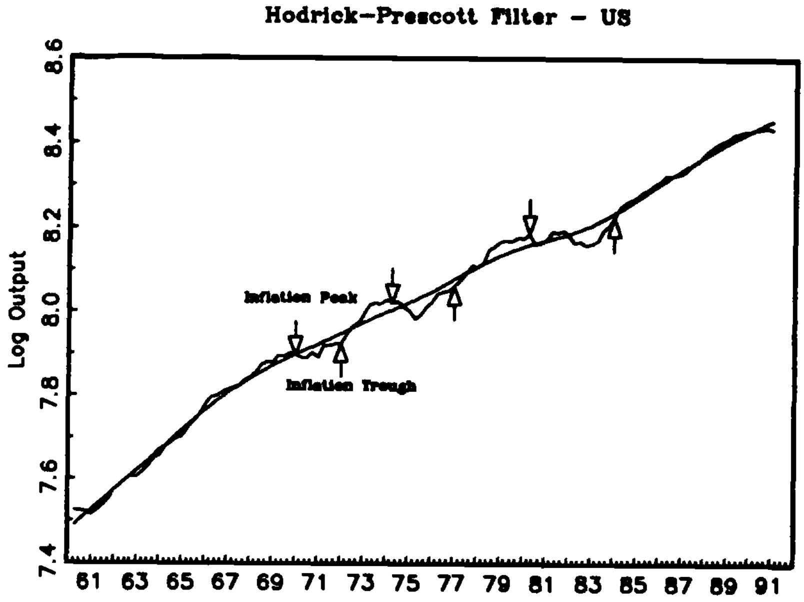
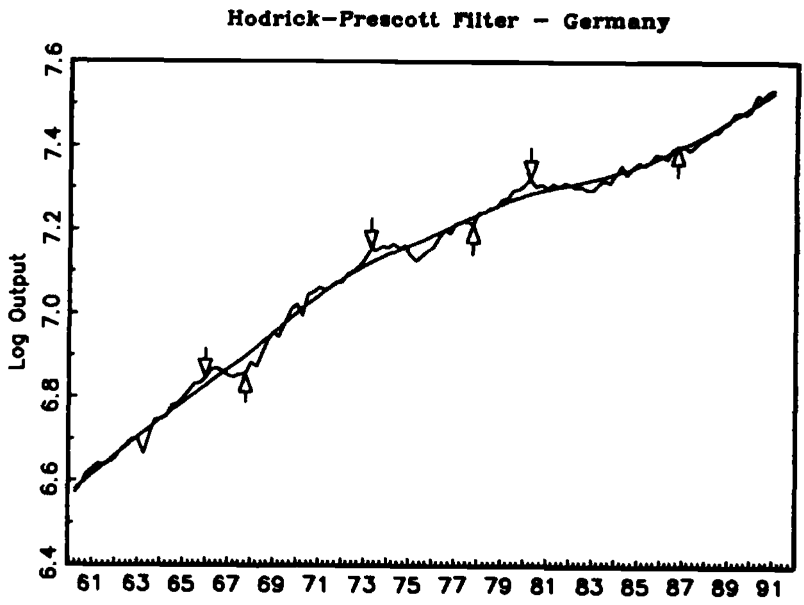


Fig. 5.2 Trend output: standard methods


Hodrick-Prescott Filter - US
Log Output
7.4
7.6
7.8
8.0
8.2
8.4
8.6
Inflation Peak
Inflation Trough
61 63 65 67 69 71 73 75 77 79 81 83 85 87 89 91


Hodrick-Prescott Filter - Germany
Log Output
6.4
6.6
6.8
7.0
7.2
7.4
7.6
61 63 65 67 69 71 73 75 77 79 81 83 85 87 89 91

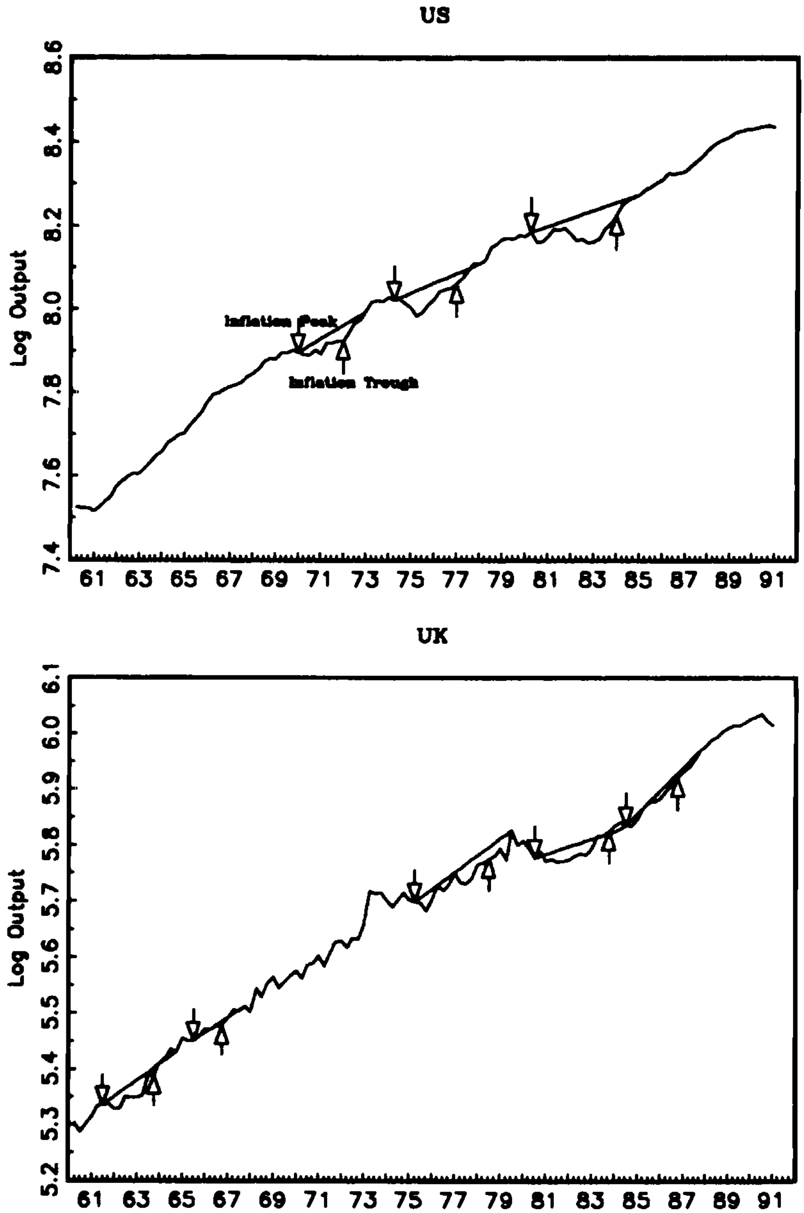
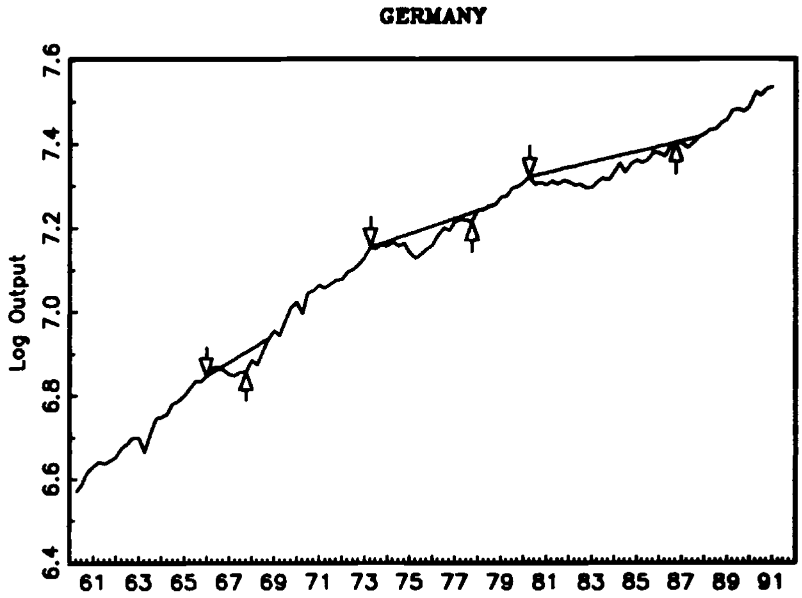
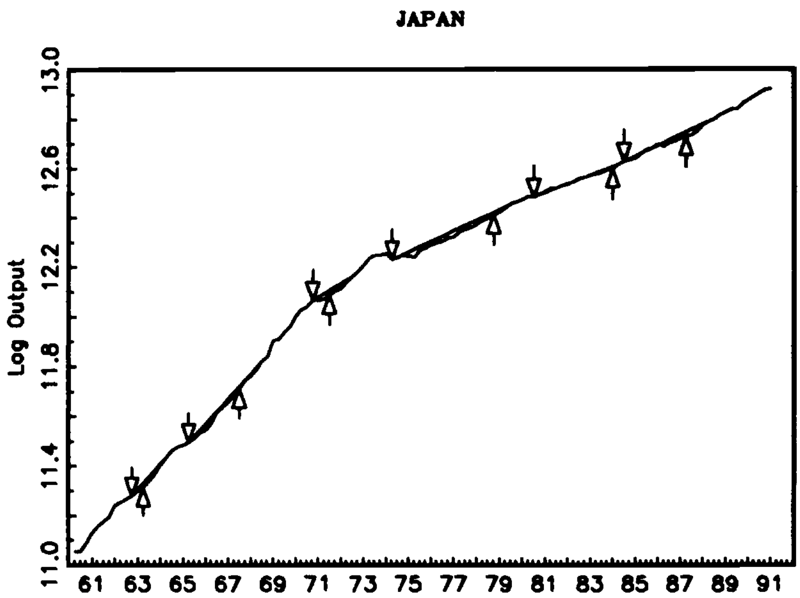


Fig. 5.3 Trend output during disinflations


GERMANY
Log Output
7.6
7.4
7.2
7.0
6.8
6.6
6.4
61 63 65 67 69 71 73 75 77 79 81 83 85 87 89 91


JAPAN
Log Output
13.0
12.6
12.2
11.8
11.4
11.0
61 63 65 67 69 71 73 75 77 79 81 83 85 87 89 91

sodes. Thus I can compare episodes without taking a stand on whether disinflation has permanent effects.[4]

5.1.4 Annual Data

For some countries, output data are available only annually. Thus I also use a version of my procedure in which a year is the basic time unit. I define trend inflation for a year as an eight-quarter moving average centered at the year—an average over the four quarters of the year and the two quarters on each side. (Quarterly inflation data are available for all countries.) Year t is an inflation peak (trough) if trend inflation at t is higher (lower) than trend inflation at $t - 1$ or $t + 1$. That is, peaks and troughs are defined with reference to a year on each side rather than four quarters. Trend output is determined by connecting output at the inflation peak to output one year after the trough. Finally, a disinflation occurs when trend inflation falls at least 1.5 percentage points, rather than two points as before. For a given country, this cutoff yields roughly the same number of episodes as I identify with quarterly data. The use of annual data dampens movements in inflation, and the resulting loss of episodes offsets the gain from the lower cutoff.

5.2 A Sample of Sacrifice Ratios

The data on inflation and output are from the International Monetary Fund's *International Financial Statistics.* I examine all OECD countries for which reliable data are available and trend inflation has stayed below 20 percent since 1960. I consider disinflations that begin in 1960 or later and end by 1991. Inflation is measured by the change in the consumer price index (CPI), and output is measured by real gross national product (GNP) or real GDP (whichever is available).

For most countries, my procedure identifies two to five disinflation episodes. The quarterly data yield twenty-eight episodes from nine countries: the United States, the United Kingdom, France, Germany, Italy, Switzerland, Canada, Japan, and Australia. The annual data yield sixty-five episodes in nineteen countries. Twenty-five episodes appear in both the quarterly and annual data sets. Tables 5.1 and 5.2 list all the episodes and their sacrifice ratios.

The average ratio across all episodes is 1.4 for quarterly data and 0.8 for annual data. For the twenty-five episodes in both samples, the averages are 1.5 and 1.1. It appears that the annual data understate output losses because time aggregation smooths the output series. Nonetheless, the two data sets yield similar pictures of the relative costs of different disinflations; for the twenty-five common observations, the correlation between the two ratios is .81.

4. In the model of note 3, hysteresis can be introduced by assuming $y^* = y_{-1}^* + cy$, where y^* is trend output and y is the deviation from trend. Under this assumption, a larger deviation implies a larger change in the trend, as suggested in the text.

Table 5.1 Disinflations: Quarterly Data

Episode	Length in Quarters	Initial Inflation	Change in Inflation	Sacrifice Ratio
Australia				
74:2–78:1	15	14.60	6.57	0.7234
82:1–84:1	8	10.50	4.98	1.2782
Canada				
74:2–76:4	10	10.60	3.14	0.6273
81:2–85:2	16	11.60	7.83	2.3729
France				
74:2–76:4	10	11.90	2.98	0.9070
81:1–86:4	23	13.00	10.42	0.5997
Germany				
65:4–67:3	7	3.67	2.43	2.5590
73:1–77:3	18	6.92	4.23	2.6358
80:1–86:3	26	5.86	5.95	3.5565
Italy				
63:3–67:4	17	6.79	5.74	2.6539
77:1–78:2	5	16.50	4.30	0.9776
80:1–87:2	29	19.10	14.56	1.5992
Japan				
62:3–63:1	2	8.11	3.00	0.5309
65:1–67:2	9	5.99	2.20	1.6577
70:3–71:2	3	7.53	2.09	1.2689
74:1–78:3	18	17.10	13.21	0.6068
80:2–83:4	14	6.68	5.07	0.0174
84:2–87:1	11	2.29	2.11	1.4801
Switzerland				
73:4–77:4	16	9.42	8.28	1.8509
81:3–83:4	9	6.15	3.86	1.2871
United Kingdom				
61:2–63:3	9	4.24	2.10	1.9105
65:2–66:3	5	4.91	2.69	−0.0063
75:1–78:2	13	19.70	9.71	0.8679
80:2–83:3	13	15.40	11.12	0.2935
84:2–86:3	9	6.19	3.03	0.8680
United States				
69:4–71:4	8	5.67	2.14	2.9364
74:1–76:4	11	9.70	4.00	2.3914
80:1–83:4	15	12.10	8.83	1.8320

For the quarterly data, the sacrifice ratios for individual episodes range from 0.0 to 3.6. The ratio is positive in twenty-seven of twenty-eight cases, suggesting that disinflation is almost always costly. There are sizable differences in average ratios across countries, as shown in table 5.3. The highest average ratios occur in Germany (2.9) and the United States (2.4), and the lowest in France (0.8) and the United Kingdom (0.8). A regression of the ratio on country dummies yields an $\bar{R}^2$ of .47.

Table 5.2 **Disinflations: Annual Data**

Episode	Length in Years	Initial Inflation	Change in Inflation	Sacrifice Ratio
Australia				
61–62	1	1.27	1.52	−0.0399
74–78	4	13.10	6.38	0.4665
82–84	2	9.48	5.46	0.7571
86–88	2	7.80	1.88	0.0824
Austria				
65–66	1	2.18	2.21	−0.5019
74–78	4	8.05	5.16	1.0824
80–83	3	5.93	1.90	1.5339
84–86	2	4.55	3.56	−0.2219
Belgium				
65–67	2	3.60	1.69	0.7376
74–78	4	10.80	7.23	0.4945
82–87	5	7.57	6.54	1.7156
Canada				
69–70	1	3.74	1.54	0.9863
74–76	2	9.08	2.57	0.3822
81–85	4	10.00	6.56	2.2261
Denmark				
68–69	1	6.13	2.94	−0.6939
74–76	2	11.40	3.95	0.5746
77–78	1	9.52	1.74	0.5776
80–85	5	10.60	7.89	1.7621
Finland				
64–65	1	7.27	3.92	−0.3582
67–69	2	7.03	5.22	0.9459
74–78	4	14.70	8.33	1.6569
80–86	6	9.92	6.95	0.6477
France				
62–66	4	5.31	3.63	−0.6765
74–76	2	11.00	3.19	1.0807
81–86	5	11.30	9.05	0.2517
Germany				
65–67	2	3.28	1.78	1.5614
73–78	5	6.31	3.91	3.9174
80–86	6	4.96	5.11	2.0739
Ireland				
64–66	2	5.41	3.37	0.9134
74–78	4	15.90	8.52	0.8147
80–87	7	15.60	13.52	0.4292
Italy				
63–67	4	5.95	4.76	2.2857
76–78	2	14.90	4.30	0.5107
80–87	7	17.60	13.40	1.6448
Japan				
62–64	2	7.55	3.78	−0.6262
74–78	4	15.20	12.51	0.4615
80–82	2	5.44	3.72	−0.1567
83–86	3	1.84	1.99	−0.6117

Table 5.2 (continued)

Episode	Length in Years	Initial Inflation	Change in Inflation	Sacrifice Ratio
Luxemburg				
75–78	3	8.79	6.03	0.5302
Netherlands				
65–67	2	5.44	2.55	1.2767
75–78	3	8.33	4.89	−0.8558
81–83	2	5.92	3.11	1.3973
84–86	2	2.85	3.35	−0.5739
New Zealand				
71–72	1	8.39	2.42	0.5396
75–78	3	13.20	3.73	1.2897
80–83	3	13.50	8.19	0.1752
86–88	2	12.30	7.62	0.1018
Spain				
62–63	1	7.37	2.13	−0.5630
64–69	5	9.95	7.28	−0.2142
77–87	10	18.40	13.86	3.4847
Sweden				
65–68	3	5.59	3.74	1.1134
77–78	1	9.53	2.85	0.3564
80–82	2	11.80	4.35	0.8707
83–86	3	7.61	4.21	−0.5350
Switzerland				
66–68	2	3.58	1.55	1.6060
74–76	2	7.90	6.87	1.3447
81–83	2	4.75	2.12	1.2618
84–86	2	3.22	2.12	−0.7917
United Kingdom				
61–63	2	3.32	2.27	1.7717
75–78	3	16.70	9.71	−0.0682
80–83	3	13.10	9.78	0.5379
84–86	2	4.51	1.84	0.4823
United States				
69–71	2	4.76	1.53	3.3666
74–76	2	8.91	3.63	1.6057
79–83	4	10.40	7.63	1.9362

There is greater variation in the ratio with annual data, and a number of negative ratios. These results probably reflect greater measurement error arising from cruder data. The $\bar{R}^2$ from a regression on country dummies is only .18. Similarly, the $\bar{R}^2$ is lower for annual than for quarterly data in most of the regressions below.

To check the plausibility of my sacrifice ratios, I have compared the ratios for the Volcker disinflation (the last U.S. episode) to previous estimates. Estimates of the Volcker ratio include 4.2 (Blinder 1987), 2.9 (Sachs 1985), 2.8

Table 5.3 **Average Sacrifice Ratios by Country**

Country	Quarterly Data	Annual Data
Australia	1.00	0.32
Austria		0.47
Belgium		0.98
Canada	1.50	1.20
Denmark		0.56
Finland		0.72
France	0.75	0.22
Germany	2.92	2.52
Ireland		0.72
Italy	1.74	1.48
Japan	0.93	−0.23
Luxembourg		0.53
Netherlands		0.31
New Zealand		0.53
Spain		0.90
Sweden		0.45
Switzerland	1.57	0.86
United Kingdom	0.79	0.68
United States	2.39	2.30

(Mankiw 1991), and 1.4 (Schelde-Andersen 1992).[5] My estimates—1.8 with quarterly data and 1.9 with annual data—are within the previous range but lower than average. Two features of my approach explain the relatively low ratios. First, I use the change in consumer price index (CPI) inflation as the denominator of the ratio; most previous authors use the GDP deflator, which yields a smaller inflation gain under Volcker. Second, I assume that output is back at trend in 1984:4 (four quarters after the inflation trough of 1983:4), whereas others assume output losses through 1985.

5.3 The Speed of Disinflation

The next three sections ask whether variation in the sacrifice ratio can be explained. This section shows that the ratio is lower if disinflation is quick.

5.3.1 Background

The optimal speed of disinflation—the choice between "gradualism" and "cold turkey"—is a central issue for macroeconomic policy. One view is that gradualism is less costly because wages and prices possess inertia, and thus need time to adjust to a monetary tightening. This view has been formalized

5. Blinder's ratio is 2.1 points of unemployment per point of inflation. An Okun coefficient of two implies an output sacrifice ratio of 4.2.

by Taylor (1983), who presents a model of staggered wage adjustment in which quick disinflation reduces output but slow disinflation does not.[6]

A contrary view is that disinflation is less costly if it is quick. Sargent (1983) argues that a sharp regime-shift produces credibility, and hence a shift in expectations that makes disinflation costless. Gradualism, by contrast, "invites speculation about future reversals," so that expectations do not adjust. Another argument for quick disinflation appears to follow from "menu cost" models of price adjustment. In these models, large shocks trigger greater price adjustment than small shocks; thus a large, one-time shift in monetary policy may be close to neutral, whereas a series of smaller tightenings reduces output substantially. (This idea has not been formalized, however.)

There is currently little evidence on these issues. The two sides make their cases by appealing to historical examples, but the interpretation of individual episodes is controversial. (See, for example, the debate over Sargent's account of the Poincaré disinflation.) This study compares the sacrifice ratio and the speed of disinflation across my sizable sample of episodes.

My basic measure of the speed of disinflation is the change in trend inflation per quarter—the total change from peak to trough divided by the length of the episode. It is not clear, however, that this variable is the right summary statistic; in particular, the numerator and denominator of speed could influence the sacrifice ratio in different ways. In models of staggered price adjustment, the ratio depends only on the length of disinflation relative to the frequency of price adjustment. A larger change in inflation over a given period increases the numerator and denominator of the ratio by the same proportion (Ball 1992). By contrast, Sargent's view suggests that the change in inflation matters: a larger change is more likely to be perceived as a regime-shift, and thus produce a shift in expectations. In my empirical work, I test for separate effects of the inflation change and the episode length.

5.3.2 Basic Results

Table 5.4 presents my basic results about the sacrifice ratio and the speed of disinflation. Columns 1 and 2 report simple regressions of the ratio on speed for the quarterly and annual data sets. In both cases, speed has a significantly negative coefficient: faster disinflations are less expensive. To interpret the coefficients, consider the difference between the fitted sacrifice ratio when speed equals one and when speed equals one quarter. These speeds correspond to a five-point disinflation carried out over five quarters or over twenty quarters (or a ten-point disinflation over ten or forty quarters). For the quarterly data set, the sacrifice ratio is 1.8 when speed is one quarter but only 0.7 when speed is one. The results for annual data are similar. Thus faster disinflation produces substantially lower output losses.

6. This result can be criticized on the grounds of theoretical robustness, as well as on the empirical grounds discussed below. See Ball (1992).

Table 5.4 **The Sacrifice Ratio and the Speed of Disinflation (dependent variable: sacrifice ratio)**

Data Set	(1) Quarterly	(2) Annual	(3) Quarterly	(4) Annual
Constant	2.196	1.512	1.045	0.131
	(0.329)	(0.307)	(0.325)	(0.232)
Speed = $\Delta\pi$/length	−1.543	−1.677		
	(0.586)	(0.637)		
$\Delta\pi$			−0.198	−0.123
			(0.061)	(0.057)
Length			0.120	0.106
			(0.034)	(0.026)
$\bar{R}^2$	.180	.085	.300	.209
Sample size	28	65	28	65

Note: Standard errors are in parentheses.

Columns 3 and 4 of the table enter the change in inflation ($\Delta\pi$) and the episode length as separate variables. The $\Delta\pi$ coefficient is significantly negative, and the length coefficient is significantly positive. That is, greater speed reduces the sacrifice ratio regardless of whether it results from a larger inflation change over a given period or from a faster completion of a given change. The $\bar{R}^2$'s for this specification are considerably higher than when $\Delta\pi$ and length enter only through their ratio.

I have also estimated the equations in columns 1–4 with the addition of dummy variables for countries. These regressions isolate the within-country comovement of the sacrifice ratio and speed. The results are similar to table 5.4, although somewhat weaker. The coefficient in the simple regression on speed is -0.9 ($t = 1.9$) for quarterly data and -1.2 ($t = 1.8$) for annual data. In the multiple regression, both coefficients are significant for the quarterly sample ($t > 2$), and the length coefficient is significant for the annual sample.[7]

5.3.3 A Potential Bias

As discussed above, the effects of supply shocks on the sacrifice ratio can be interpreted as measurement error. Measurement error in the dependent variable does not generally cause econometric problems, but it does in this application. The problem is that $\Delta\pi$ is both the denominator of the sacrifice ratio and the numerator of the independent variable speed (or, in columns 3 and 4, a separate regressor). For a given demand contraction, a favorable supply shock that increases $\Delta\pi$ will reduce the estimated sacrifice ratio and increase speed,

7. I have also experimented with a "random effects" specification, in which the error term contains a component common to episodes from the same country. Generalized least squares (GLS) estimates of this model are very close to the OLS results reported in the text.

Table 5.5 **The Speed of Disinflation: Subsamples (dependent variable: sacrifice ratio)**

Data Set	(1) Quarterly through 1972	(2) Quarterly after 1972	(3) Annual through 1972	(4) Annual after 1972
Constant	2.478	2.160	1.932	1.289
	(0.517)	(0.493)	(0.493)	(0.387)
Speed = $\Delta\pi$/length	−1.516	−1.718	−3.024	−1.044
	(0.787)	(0.953)	(1.064)	(0.789)
$\bar{R}^2$	.279	.106	.271	.017
Sample size	8	20	20	45

Note: Standard errors are in parentheses.

creating a spurious negative relation between the two. I now check whether this problem has an important effect on my results.

I take two approaches. First, table 5.5 regresses the sacrifice ratio on speed for disinflations ending by 1972 and disinflations ending after 1972. Aggregate supply was less stable in the second period, and so the negative bias arising from supply shocks should be larger. With quarterly data, however, the speed coefficients for both subsamples are close to the coefficient for the whole sample. With annual data, the coefficient is more negative in the first period than in the second, the opposite of what we would expect if supply shocks had created bias. These results suggest that my basic findings are not driven by supply shocks. Similar results arise if speed is replaced by $\Delta\pi$ and length, or if the sample is split at 1984 (before the 1986 oil price decline).

As a second approach to the supply-shock problem, I estimate the effect of speed using instrumental variables (IV), with length as an instrument. Length is clearly correlated with speed = $\Delta\pi$/length. And length is plausibly uncorrelated with the errors arising from supply shocks. For a given path of aggregate demand, a beneficial supply shock is likely to increase $\Delta\pi$, but not to reduce length: the shock does not cause disinflation to end sooner. Thus, increases in speed resulting from decreases in length should not be negatively correlated with the errors in the sacrifice ratio. (Indeed, beneficial supply shocks near the end of an episode may *increase* its length by extending the inflation decline beyond the end of tight policy. Note that a number of episodes end in the beneficial-shock year of 1986. In this case, the IV coefficient on speed has a positive rather than negative bias.)[8]

8. My approach assumes that the demand contraction during disinflation is exogenous. If policy responds to supply shocks, there are scenarios in which the negative bias in OLS extends to IV. This is the case, for example, if a favorable supply shock causes policymakers to shorten disinflation because an inflation target is met more quickly. In such a case, the identification problem appears insuperable.

Table 5.6 The Speed of Disinflation: Instrumental Variables (instrument = 1/length)

	(1) Quarterly Data	(2) Annual Data
Constant	2.077	3.633
	(0.454)	(1.320)
Speed = $\Delta\pi$/length	−1.301	−6.477
	(0.867)	(2.963)
Sample size	28	65

Note: Standard errors are in parentheses.

Table 5.6 presents instrumental variables estimates, with the inverse of length as the instrument. (1/length is more highly correlated with speed than is length.) For the quarterly sample, the coefficient on speed is close to the ordinary least squares (OLS) coefficient. For the annual sample, the coefficient is larger in absolute value, but the difference is not statistically significant. Once again, there is no evidence of negative bias in my basic results.

5.4 Nominal Wage Rigidity

5.4.1 Background

Comparisons of macroeconomic performance in different countries often emphasize differences in nominal wage rigidity (e.g., Bruno and Sachs 1985). These differences are attributed to wage-setting institutions such as the frequency of adjustment, the degree of indexation, and the synchronization of adjustment across sectors. Many authors argue, for example, that three-year staggered contracts make U.S. wages rigid, whereas one-year synchronized contracts make Japanese wages flexible. These differences are used to explain cross-country variation in the costs of disinflation, such as the high costs in the United States and the lower costs in Japan (e.g., Gordon 1982).

In contrast to this tradition, recent "New Keynesian" research has deemphasized wage-setting institutions. New Keynesians argue that monetary nonneutrality arises largely from rigidities in output prices rather than wages (e.g., Mankiw 1990). To the extent that price rigidity determines the cost of disinflation, wage-setting institutions are unimportant.

Once again, the relevant empirical evidence consists mainly of informal comparisons of a few episodes. I now investigate whether wage rigidity helps explain the variation in the sacrifice ratio in my sample.[9]

9. Bruno-Sachs and others report extensive cross-country comparisons of wage rigidity and the effects of supply shocks. To my knowledge, however, this study and Schelde-Andersen (1992) are the only papers that compare rigidity and sacrifice ratios.

Table 5.7 The Sacrifice Ratio and Wage Rigidity (quarterly results)

	(1)	(2)	(3)	(4)
Constant	2.067	1.593	1.841	1.542
	(0.400)	(0.446)	(0.302)	(0.406)
Wage responsiveness	−0.179	−0.150		
	(0.104)	(0.087)		
Contract duration			−0.313	−0.306
			(0.194)	(0.162)
$\Delta\pi$		−0.190		−0.205
		(0.059)		(0.058)
Length		0.115		0.115
		(0.033)		(0.032)
$\bar{R}^2$	.069	.352	.056	.366
Sample size	28	28	28	28

Note: Standard errors are in parentheses.

5.4.2 Basic Results

My basic measure of wage rigidity is Bruno and Sachs's index of "nominal wage responsiveness." For a given country, Bruno and Sachs assign a value of zero, one, or two to each of three variables: the duration of wage agreements, the degree of indexation, and the degree of synchronization. Higher values mean greater flexibility (that is, shorter, more indexed, and more synchronized agreements). The wage responsiveness index is the sum of the three values, and thus runs from zero to six. I regress the sacrifice ratio on the index, and also experiment with the three components.

The results for quarterly and annual data are presented in tables 5.7 and 5.8. In both cases, I report simple regressions on the responsiveness index and regressions that include $\Delta\pi$ and disinflation length. I also use the duration of agreements in place of the total index. Duration is the only component of the index that is significant by itself.[10]

The results are quite similar across specifications. The coefficients on responsiveness or duration are negative, implying that greater flexibility reduces the sacrifice ratio. The statistical significance of these results is borderline: t-statistics range from 1.8 to 2.1 for annual data and from 1.6 to 1.9 for quarterly data. The point estimates imply large effects of flexibility. In column 1 of the annual results, for example, the fitted value of the sacrifice ratio is 1.4 for a responsiveness of zero but only 0.5 for the maximum responsiveness of six. (Switzerland has a rating of zero, and Australia, Denmark, and New Zealand have ratings of six.) The results for duration show that it is the most important component of the index. In column 3 of the annual results, raising duration

10. For these regressions, the annual data set is reduced from sixty-five to fifty-eight observations, because the Bruno-Sachs index is missing for several countries.

Table 5.8 **The Sacrifice Ratio and Wage Rigidity (annual results)**

	(1)	(2)	(3)	(4)
Constant	1.369	0.657	1.280	0.649
	(0.326)	(0.388)	(0.270)	(0.356)
Wage responsiveness	−0.153	−0.130		
	(0.077)	(0.071)		
Contract duration			−0.360	−0.330
			(0.168)	(0.155)
$\Delta\pi$		−0.096		−0.111
		(0.058)		(0.057)
Length		0.096		0.099
		(0.029)		(0.028)
$\bar{R}^2$	.050	.191	.059	.208
Sample size	58	58	58	58

Note: Standard errors are in parentheses.

from zero to two reduces the fitted ratio from 1.3 to 0.6. (Zero means wage agreements of three years, and two means agreements of a year or less.)

5.4.3 A Variation

As an alternative explanatory variable, I use the index of nominal wage rigidity in Grubb, Jackman, and Layard (1983). This variable is quite different from the Bruno-Sachs index: it is constructed from a time-series regression of wages on prices and unemployment. An advantage of the Grubb index is that it flexibly measures the overall rigidity of wages—it does not rely on arbitrary assumptions about the importance of particular contract provisions. An obvious disadvantage is that the Grubb variable is endogenous. Factors that directly influence the sacrifice ratio, such as the initial level of inflation or the prevalence of incomes policies, are likely to influence the speed of wage adjustment that Grubb measures. To address this problem, I estimate the effects of the Grubb index using instrumental variables, with Bruno and Sachs's variables as instruments. This approach isolates the effects of rigidity arising from wage-setting institutions.

Table 5.9 reports results for both quarterly and annual data. Columns 1 and 2 present OLS regressions of the sacrifice ratio on the Grubb index, $\Delta\pi$, and disinflation length. For both data sets, the Grubb variable has a significantly positive coefficient. Columns 3 and 4 report instrumental variables estimates; the instruments are the three components of the wage-responsiveness variable and another Bruno-Sachs index measuring "corporatism" (the extent of unionization, the centralization of bargaining, and so on). The instrumental variables estimates are close to the OLS estimates, although the statistical significance becomes borderline. The results are reasonably stable when various subsets of the instruments are used. Overall, the results confirm the finding that wage rigidity is an important determinant of the sacrifice ratio.

Table 5.9 The Sacrifice Ratio and Wage Rigidity: Grubb Measure

	(1) Quarterly Data	(2) Annual Data	(3) Quarterly Data	(4) Annual Data
Procedure	Ordinary Least Squares	Ordinary Least Squares	Instrumental Variables	Instrumental Variables
Constant	0.731	−0.155	0.709	−0.173
	(0.324)	(0.268)	(0.358)	(0.304)
Wage rigidity	0.377	0.578	0.404	0.614
	(0.156)	(0.168)	(0.239)	(0.321)
$\Delta\pi$	−0.201	−0.112	−0.201	−0.112
	(0.056)	(0.054)	(0.056)	(0.054)
Length	0.125	0.101	0.125	0.101
	(0.031)	(0.027)	(0.031)	(0.027)
Sample size	28	58	28	58

Note: Standard errors are in parentheses.

5.5 Other Results

This section reports additional results that are either negative or inconclusive. Future research should explore these issues further.

5.5.1 Initial Inflation

Ball, Mankiw, and Romer (1988) show that trend inflation influences the output-inflation trade-off in New Keynesian models. Higher inflation reduces the extent of nominal rigidity, and thus steepens the short-run Phillips curve. Ball, Mankiw, and Romer show that cross-country evidence strongly supports this prediction. A special case of the prediction is that the sacrifice ratio during disinflation is decreasing in the initial level of trend inflation. I now test this idea with my sample of disinflations. For both quarterly and annual data, table 5.10 reports regressions of the ratio on initial inflation, π_0. I present simple regressions and regressions that include length, $\Delta\pi$, and contract duration (the best-fitting set of variables from the previous section).

The results for the two data sets are rather different. With quarterly data, the simple regression shows a negative effect of π_0, as predicted by theory. The t-statistic is 1.9. The coefficient in the multiple regression is similar in size, but statistically insignificant ($t = 1.0$). This weaker result reflects collinearity between π_0 and $\Delta\pi$: inflation tends to fall more in episodes when it is initially high. (Note that the coefficient on $\Delta\pi$ also becomes insignificant.) Overall, it is difficult to identify separate effects of π_0 and $\Delta\pi$, but the data are at least suggestive that π_0 has a negative effect.

The annual data, by contrast, provide no support for this hypothesis. In both specifications, the coefficient is not only insignificant, but has the wrong sign (positive).

The differences between the quarterly and annual results arise from differ-

Table 5.10 The Sacrifice Ratio and Initial Inflation (dependent variable: sacrifice ratio)

	(1) Quarterly Data	(2) Quarterly Data	(3) Annual Data	(4) Annual Data
Constant	2.093	1.884	0.661	0.480
	(0.386)	(0.533)	(0.307)	(0.413)
π_0	−0.067	−0.055	0.014	0.040
	(0.036)	(0.055)	(0.034)	(0.050)
$\Delta\pi$		−0.126		−0.164
		(0.099)		(0.087)
Length		0.097		0.106
		(0.037)		(0.030)
Duration		−0.331		−0.321
		(0.164)		(0.155)
$\bar{R}^2$	.086	.365	.015	.203
Sample size	28	28	58	58

Note: Standard errors are in parenthescs.

ences in the samples of countries. When the annual sample is restricted to the countries for which quarterly data exist, the results are similar to the quarterly results. It is not clear why the choice of countries is so important. It is also not clear why the effect of π_0 is weaker than the effect of trend inflation on the Phillips curve found by Ball, Mankiw, and Romer.

5.5.2 Incomes Policies

If inflation possesses inertia, there may be a role for governments to intervene directly in wage- and price-setting during disinflation. By mandating a slowdown in the growth of prices, governments can reduce inertia and thus reduce the sacrifice ratio. This logic has led to wage-price controls or other incomes policies during many disinflations.

To investigate the effects of incomes policies, I must first measure them. For each of the twenty-eight episodes in the quarterly data set, I consulted the historical record to see whether incomes policies were employed. My main sources were the OECD's *Economic Outlook* and surveys of incomes policies (Ulman and Flanagan 1971; Flanagan, Soskice, and Ulman 1983). I create two dummy variables. The first, INCM, equals one if mandatory incomes policies were employed at anytime during the episode. A mandatory policy is defined as one in which legal restrictions are placed on the majority of wages or the majority of output prices. According to my survey, such policies were imposed in six of the twenty-eight episodes (United States 1969–71, Canada 1974–76, France 1974–76, France 1981–86, United Kingdom 1965–66, and United Kingdom 1975–78). The second variable, INCV, equals one if voluntary incomes policies were introduced. These policies are broadly defined to include voluntary guideposts, jawboning, and negotiated settlements with business and labor. Voluntary policies occurred in another twelve of the episodes.

Table 5.11 regresses the sacrifice ratio on INCM and INCV, both with and without additional controls. When both dummies are included, the results are disappointing: INCV has a positive coefficient and INCM has a negative coefficient, and both are insignificant. When INCV is excluded, however, the INCM coefficient is almost significant in the regression with additional controls ($t = 1.8$). The coefficient implies that a mandatory policy reduces the sacrifice ratio by 0.6.

One interpretation of these results is that voluntary policies are ineffective but mandatory policies do reduce the sacrifice ratio. This finding is not very robust, however. The results could be checked by extending the measures of incomes policies to the larger annual sample.

5.5.3 Openness

As stressed by Romer (1991), basic macroeconomics suggests a relation between the output-inflation trade-off and the openness of the economy—the share of imports in total spending. In a more open economy, the exchange-rate appreciation arising from a monetary contraction has a larger direct effect on the price level. Consequently, inflation falls more for a given policy shift: the sacrifice ratio is smaller.

This idea receives no support from my data. Table 5.12 reports regressions of the sacrifice ratio on the imports/GNP ratio (taken from Romer). The effects of imports/GNP are very insignificant for both quarterly and annual data. These results cast doubt on Romer's argument that openness influences average inflation by changing the output-inflation trade-off.

Table 5.11 The Sacrifice Ratio and Incomes Policies (quarterly data) (dependent variable: sacrifice ratio)

	(1)	(2)	(3)	(4)
Constant	1.335	1.561	1.539	1.748
	(0.289)	(0.195)	(0.424)	(0.405)
INCM	−0.346	−0.573	−0.351	−0.593
	(0.472)	(0.422)	(0.366)	(0.328)
INCV	0.415		0.449	
	(0.391)		(0.323)	
$\Delta\pi$			−0.223	−0.206
			(0.056)	(0.056)
Length			0.112	0.112
			(0.030)	(0.031)
Duration			−0.285	−0.330
			(0.156)	(0.156)
$\bar{R}^2$	.035	.030	.443	.420
Sample size	28	28	28	28

Note: Standard errors are in parentheses.

Table 5.12 The Sacrifice Ratio and Openness

	(1) Quarterly Data	(2) Annual Data	(3) Quarterly Data	(4) Annual Data
Constant	1.363	0.828	1.615	0.469
	(0.534)	(0.302)	(0.551)	(0.402)
Imports/gross national product	0.364	−0.096	−0.390	−0.273
	(2.415)	(0.966)	(1.954)	(0.860)
$\Delta\pi$			−0.205	−0.122
			(0.059)	(0.056)
Length			0.116	0.102
			(0.033)	(0.026)
Duration			−0.306	−0.166
			(0.166)	(0.141)
$\bar{R}^2$	−.038	−.017	.339	.218
Sample size	28	61	28	61

Note: Standard errors are in parentheses.

5.6 Conclusion

This paper constructs sacrifice ratios for a sample of disinflations and asks whether variation in the ratio can be explained. I find that the ratio is lower when disinflation is quick, and when wage-setting is more flexible. Openness has no effect on the ratio, and the effects of initial inflation and incomes policies are unclear.

My analysis uses a new measure of the sacrifice ratio based on several assumptions about the behavior of trend output. Future research should check the robustness of the results to variations in my assumptions. Some evidence of robustness is provided by Schelde-Andersen (1992), whose study was carried out independently of mine. Schelde-Andersen estimates sacrifice ratios using a substantially different approach. For example, he measures the ratio using a fixed period for every country (1979–88), and he examines unemployment as well as output losses. His ranking of sacrifice ratios across countries differs considerably from my table 5.3. Nonetheless, Schelde-Andersen and I reach similar conclusions about the determinants of the ratio. In particular, he confirms my findings about both the speed of disinflation and nominal wage rigidity.

Do my results about speed imply that cold-turkey disinflation is preferable to gradualism? This conclusion is warranted only if the cost of disinflation is measured by the total output loss. The welfare loss in a given quarter might be a convex function of the output loss, as macro theorists usually assume. In this case, gradualism has the advantage of spreading the losses over a longer period. Since we do not know the shape of the social loss function, it is difficult to determine the optimal speed of disinflation. At a minimum, however, my results refute the view that gradualism makes disinflation costless.

Another possible implication of my results is that government should encourage greater wage flexibility, for example, by limiting the length of labor contracts. Such a policy is suggested by Bosworth (1981) and others, and can be justified in principle by the negative externalities from long contracts (Ball 1987). My results suggest that the welfare gains from shorter contracts are large: the recessions arising from disinflation are dampened considerably.

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Comment Benjamin M. Friedman

Laurence Ball's paper is a useful contribution to a literature with old antecedents as well as much contemporary policy import. The central questions at issue are plain: what is the cost of disinflation, and what—if anything—can a country do to reduce that cost? Ball frames these questions within the context of recent contributions by such well known researchers as Arthur Okun, Thomas Sargent, Barry Bosworth, and Robert J. Gordon, but it is also appropriate to recall the earlier incarnation of this discussion in the debate between Friedrich Hayek and Milton Friedman over "gradualism" in disinflation policies, and still prior developments as well. In the United States within the past decade or so, discussion of this issue has mostly focused on the "Volcker disinflation" of the early 1980s, the "Greenspan disinflation" that accompanied the 1990–91 recession, and the proposed (but not enacted) congressional resolution directing the Federal Reserve System to pursue a monetary policy leading to zero inflation.[1] In each case, a central question has been the costs—costs in terms of foregone output, jobs, incomes, profits, capital formation, and so on—that disinflation involves.

At least in the United States, economic thinking about these costs has followed an interesting evolution over the post-World War II era. During roughly the first half of this period, the desire to maintain a higher utilization of the economy's resources (that is, lower unemployment) without incurring ever-increasing price inflation presented macroeconomics with its primary policy objective and, in so doing, motivated the leading topic on the field's research agenda.[2] The conceptual vehicle hypothesized to make greater utilization with-

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1. See H.R. 409, 101st Cong., 1st sess.
2. The one other macroeconomic objective that commanded perhaps equal attention in the research literature of the time was raising the economy's long-run growth rate, but that idea had much less visible impact on the discussion of actual macroeconomic policy.

out ever-increasing inflation achievable was the Phillips curve: a stable and exploitable trade-off between unemployment and inflation. Given such a trade-off, an appropriate policy could permanently achieve lower unemployment than would be consistent with price stability. Prices would rise, but at a stable rate as long as unemployment was itself steady. Specifically, continually below-normal unemployment need not imply continually increasing inflation.

By the time most economists had given up on the idea of a stable and exploitable unemployment-inflation trade-off, the ongoing U.S. inflation rate had become high by historical peacetime standards. Now the question heading the research and policy agenda in macroeconomics became how to reduce that inflation while holding to a minimum the associated costs. A theory promising costless disinflation, therefore, became to its time what the theory of the stable trade-off had been in its own earlier context.

Macroeconomics again proved up to the challenge. Throughout this period, including the heyday of the stable trade-off and its aftermath, the accepted understanding had been that macroeconomic policy influenced inflation primarily, if not only, by affecting nonfinancial economic activity. Keynesian theory typically summarized the underlying dependence of inflation on the economy's utilization of its real resources via some form of explicit Phillips curve for either prices or wages, importantly including later elaborations that allowed for the role of inflation expectations. The monetarist alternative, couched primarily in terms of money growth and growth of nominal income, was necessarily more vague about the connection to real economic activity, but Friedman and Schwartz's (1963) work and the flood of subsequent studies that it inspired typically showed a substantial effect of monetary policy on real activity before the associated effect on inflation appeared.

By the late 1970s, when U.S. inflation was approaching the double-digit range, this line of thinking connecting disinflation to underutilization of resources had even achieved something of a consensus on the quantitative magnitude of the short-run trade-offs involved. When Okun (1978) surveyed a variety of estimates of the likely real costs of disinflation in the United States, the answer he found was that each one-percentage-point reduction in inflation achieved by monetary policy would require between two and six "point-years" of unemployment, with a median estimate of three point-years. Such an unfavorable trade-off—at the median, fifteen point-years of unemployment to cut the inflation rate by five percentage points—constituted a clear discouragement to an actively disinflationary policy.

By contrast, the radically different view of the way in which monetary policy affects the economy, developed during the 1970s by Lucas (1972, 1973) and Sargent and Wallace (1975), maintained that central-bank actions that are anticipated in advance affect the setting of prices and wages directly through their effect on expectations, with little or no consequence for real economic activity. Hence disinflation is costless as long as the public receives "credible" warning of the central bank's actions, as summarized in the growth of some appropriate measure of the money stock. By announcing its money-growth target in advance, and then ensuring that actual money growth followed the target closely, the central bank could thus achieve any desired reduction in inflation without consequence, adverse or otherwise, to the real economy. It is difficult to judge the exact extent of adherence to this alternative perspective by the outset of the 1980s, or the extent to which it influenced actual monetary policy decisions, but neither appears to have been negligible.

Alas, this exercise in creative optimism proved no more consistent with actual behavior than the claim of a stable trade-off that had preceded it. The Volcker disinflation destroyed the belief of most economists (and probably a still greater proportion of noneconomists) in the prospect of costless disinflation. In the wake of the early 1980s experience, featuring major disinflation accompanied by the largest business recession since the 1930s, it is far more plausible to believe that monetary policy does indeed affect price inflation primarily, if not only, by affecting nonfinancial economic activity—and, further, that the magnitude of these effects is about as was previously supposed. It is to Ball's credit that the notion of costless disinflation never enters his paper. There are few references to "credibility" either.

Although Ball's methodology based on foregone real output delivers more favorable estimates, what is striking about the Volcker disinflation is just how closely in line the resulting unemployment was with prior experience and estimates. Based on a "full employment" benchmark of 6 percent unemployment, the cost of each one percentage point of disinflation for the overall gross domestic product (GDP) deflator in the 1980s experience was about two and one-half point-years of unemployment—slightly better than Okun's median estimate of three, but well within the range of the then-conventional models that he surveyed. On the alternative assumption that the "full employment" unemployment rate was 6 percent at the beginning of the 1980s but declined to 5 percent by mid-decade, the cost per point of disinflation was slightly worse than Okun's median estimate.

By contrast, Ball's estimates based on real output movements show a sacrifice ratio of 1.9 percent for the 1979–83 U.S. experience (1.8 percent in the quarterly data), versus 3.4 percent in 1969–71 and 1.6 percent in 1974–76 (2.9 percent and 2.4 percent, respectively, in the quarterly data). But his use of the loss in real output (relative to trend) in the numerator and the slowing of the consumer price index in the denominator of his ratio presumably leads to an understatement of the costliness of the Volcker disinflation compared with analogous calculations based on unemployment and the GDP deflator. The use of output loss rather than rise in unemployment, as a measure of the cost of disinflation, raises particularly interesting substantive issues. The two measures would result in equivalent comparisons over time if such aspects of macroeconomic activity as productivity growth, labor-force participation, population growth, and the like were constant. But U.S. productivity growth improved in the early years of the 1983–90 expansion, compared with the average experience of the 1970s, and so the cost of disinflation in terms of output was less than the cost in terms of unemployment, even though labor-force growth slowed.

Given that these two measures of "sacrifice" are not equivalent in practice, the most interesting substantive question raised by the choice between them is who is doing the sacrificing. As usual in such matters, if the winners from disinflation could readily compensate the losers, this question would not arise and the output shortfall would be the obvious measure to use. Making such transfers is typically not straightforward (or even possible) in modern industrial societies, however, and so the matter of who gains at the expense of whom is very much to the point.[3]

In contrast to the Volcker disinflation, the Greenspan disinflation featured only a modest unemployment rate (never as great as 8 percent) but extraordinarily slow output growth.[4] In addition to the 2.2 percent drop in output during the 1990–91 recession, which lasted three calendar quarters, output grew by only 2.6 percent in the year and a half before the recession began. Output then expanded by only 2.9 percent in the initial year and a half after the recession ended, versus an average 9.8 percent gain in the comparable stage of the eight previous postrecession recoveries since World War II. Similarly, although slow labor-force growth held unemployment down, *employment* expanded by just 0.1 percent in the first year and a half of recovery in 1991–92, versus an average 5.4 percent gain in the eight prior recoveries. In light of Medoff's (1992a, 1992b) finding that job availability is the macroeconomic variable most highly correlated with standard consumer confidence measures as well as with voting patterns in national elections, the Greenspan disinflation presumably contributed to incumbent president George Bush's loss in the 1992 election.

Several other specifics of Ball's paper bear comment, in addition to the choice of output versus either employment or unemployment to measure cost. First, Ball explicitly focuses only on episodes in which countries have reduced inflation from "moderate" starting points. No inflation observation in his data set is as great as 20 percent. This choice presents a sharp contrast to the approach of Sargent (1982), who, in the spirit of Cagan's (1956) classic study of the demand for money, deliberately focused on hyperinflations. The basic methodological question at issue here is what can be learned about behavior under "normal" conditions from studying extreme situations. The same question is also relevant to the theoretical underpinnings of the debate over costs of disinflation, in that Lucas's (1973) original evidence for a theory of aggregate supply behavior based on producers' inability to distinguish absolute from relative price movements rested mostly on outlier observations for Argentina and Paraguay.

3. See the discussion of this subject in Friedman (1992).

4. Romer and Romer (1992) provide evidence documenting a deliberate tightening of U.S. monetary policy for purposes of disinflation beginning in 1988, and so comparisons between this episode and the Volcker disinflation are apt.

Second, the price-output dynamics that Ball posits require some explanation, to say the least. Ball's procedure assumes that output is at its natural rate at the time that inflation peaks and, subsequently, that output returns to its natural rate a year *after* inflation has bottomed. But if there is no shortfall of output at the peak, why does inflation begin to come down? And, even more puzzling, if there is still an output shortfall at the trough, why does inflation go back up? In both 1972 and 1977, for example, U.S. inflation was rising quite rapidly (see figure 5.1). Yet by Ball's estimate output was still below trend in both instances. In principle, the net impact of these peculiarities of the measurement of the output lost in disinflations could be either to enlarge or to shrink the resulting estimate of the sacrifice ratio for any specific episode. Given the small average size of Ball's estimates, however, the latter seems more likely.

Third, Ball's Hayekian finding that faster disinflations require less sacrifice raises an important problem of potential reverse causation. The basic question at issue in this regard is why central banks do what they do. As in most conventional analysis of the effect of economic policies, Ball takes monetary policy (or whatever negative aggregate demand shocks deliver disinflations) as exogenous with respect to the behavior he is investigating. But what if central banks pursue their presumed goal of disinflation more rigorously when they have reason to believe that the short-run trade-offs associated with doing so are more favorable? Ball partially addresses this question by including country dummies in his regressions, and also by testing for effects of supply shocks, but the issue remains unresolved.

Moreover, even if Ball's conclusion in this regard were persuasively robust, any attempt to find in this result evidence for sacrifice-lessening effects of greater credibility that might be associated with faster disinflation immediately runs afoul of what the specific country comparisons imply in this context. For example, Ball's calculation of a greater sacrifice ratio for Germany (2.1 percent) than for Italy (1.6 percent) in the 1980s is hardly consistent with a large role for credibility effects. Dornbusch's (1990) comparisons based on disinflation and unemployment during this period make the point even plainer: 9.6 point-years of unemployment per point of disinflation in Germany, 7.5 in the Netherlands, 4.0 in France, and 2.2 in Italy. Ball's discussion notwithstanding, comparisons like these are far more suggestive of some form of nonlinearity such that disinflation is less costly when it begins from higher initial inflation levels. (A straightforward extension of the same idea would also incorporate Sargent's finding on the end of hyperinflations.)

Finally, what about the recently fashionable talk in the United States of pressing disinflation to the point of achieving zero inflation? Ball's basic finding brings a useful note of reality to this discussion: "Intentional demand contractions are essentially the only source of two-point declines in trend inflation."

In contrast to this sobering conclusion based on the historical record, the "zero inflation" resolution recently introduced in the U.S. Congress contained not one reference to any loss of jobs, any sacrifice of output, or any shortfall of investment that a successful disinflationary policy would no doubt entail. Instead, the resolution simply stated that "zero inflation will promote the highest possible sustainable level of employment" and "the maximum sustainable rate of economic growth," and, further, that "zero inflation will encourage the highest possible rate of savings and investment." And in contrast to any mention of the higher interest rates that would have to deliver what Ball calls an "intentional demand contraction"—he concludes that "a significant tightening of monetary policy is not only necessary for disinflation but also, it appears, sufficient"—the proposed resolution merely asserted that "zero inflation will reduce interest rates to, and maintain them at, their lowest possible levels."

The problem is not that any of these claims was necessarily wrong as a description of conditions that might prevail once zero inflation were achieved but rather that they signaled no recognition whatever of the economic events that, as Ball's careful analysis shows, past experience indicates would be likely to accompany the path to zero inflation from any given beginning point. To be sure, pursuing the goal of price stability is an appropriate, indeed highly important, priority for public policy. But to do so in a disingenuous manner that obscures rather than confronts the broader implications that such a policy entails is likely, in the end, to prove destructive of just those institutions—including, most prominently, central-bank independence—that can best foster that goal.

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Comment Stephen G. Cecchetti

Laurence Ball's paper examining the impact of monetary policy on output and prices relies on several assumptions that I believe need further consideration. The purpose of this Comment is to highlight these assumptions and then show how small changes can have a large impact on certain aspects of the results. While Ball focuses his attention on the cross-sectional correlation of his estimates of the *sacrifice ratio* with various measures of the economic and political environment, I will direct my attention to the prior problem of calculating the impact of monetary policy on output and prices.

I begin with three points that I believe should guide the analysis. First, Ball presumes that information about monetary influences on the economy exists only in regard to the episodes in which inflation is reduced and output falls. This ignores the periods in which inflation increases and output rises. Why aren't we referring to *these* estimates as the "benefit ratio" from increasing inflation? While there may be an inherent asymmetry in the impact of monetary policy on output and prices, it seems unnatural to presume at the outset that there is no information on policy shifts that move toward monetary loosening. As a practical matter, the question of asymmetry is an empirical one. But it would seem that barring any evidence to the contrary, the episodes in which inflation increases should provide as much information as those in which inflation declines.

The second observation regarding Ball's approach is that it depends on the assumed dynamics of output and inflation in the absence of monetary shocks. In computing the output loss from a particular disinflation, Ball makes an assumption about trend output—namely, that it is at its trend level at the beginning of the episode and returns to the trend level four quarters after the episode. He further assumes that inflation reductions that occur during the episode are permanent. Given these two strong assumptions, that trend output is piecewise linear (in logs) and that the inflation process has no autoregressive component, some sensitivity analysis would seem useful. As we know, assumptions about stationarity and detrending often make a substantial difference in calculations of this type.

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My final observation about the Ball calculations concerns his implicit assumptions about the nature of monetary (and real) shocks. Ball assumes that a monetary shock induces the recessions he observes, and that the path of output and inflation declines during each of the episodes he examines are caused solely by the shift to tight money. But in order to measure the impact of monetary policy, we need to identify the policy shocks. This is not an easy task. How is it that we know that aggregate supply shocks are not occurring during these periods? Ball suggests that such shocks create measurement errors in his estimates of the impact of policy on output and inflation, but I believe that the difficulty is more serious.

To see why, let me give a simple example of the type of information we really need to estimate the quantity of interest. Recall that in making his measurements, Ball examines only cases in which inflation and output move in the same direction. To see how this causes problems, take a case in which an adverse supply shock hits the economy. In the absence of any change in monetary policy, there would be a temporary fall in output and a temporary rise in inflation. But in fact, the monetary authority has the option of accommodating the shock. If monetary policy becomes looser, then inflation will rise and output will *fail* to fall. We actually need to identify these shocks to estimate Ball's sacrifice ratio, otherwise we have no idea whether observed output movements are the consequence of monetary policy shifts or of other shocks to the economy.

I now proceed to use these observations to create an estimate of the sacrifice ratio that is based on a structural identification of aggregate demand and aggregate supply shocks. Once I have estimated the responses of output and inflation to these shocks in a dynamic context, I can estimate the impact of a shock using the impulse response functions, and proceed to study its sensitivity to assumptions about the dynamics of output and inflation.

The structural approach addresses my first and third concerns with Ball's methods. First, it uses information about all output and inflation movements, not just inflation declines associated with recessions. Presumably, we obtain information about the impact of aggregate demand policy on output and prices from upturns as well as downturns. In addition, by identifying the shocks explicitly, we make clear the time path of the effects. Most important, we can allow for effects that last far beyond the cyclical trough and take seriously the implication of recent research on the persistence of output fluctuations which suggests that the effect of demand shocks may be quite long-lived. I will then proceed to my second concern and investigate the importance of assumptions about detrending (or differencing) for the results. Ultimately, the question is whether the data actually give us any useful information about the response of output to policy in this context.

Following Blanchard and Quah (1989), I identify and estimate the aggregate demand shock, and hence the sacrifice ratio, by assuming that aggregate demand shocks have no long-run real effects. I study the following model:

(1) $$(1 - L)y_t = A_{11}(L)u_t + A_{12}(L)v_t$$
$$(1 - L)\pi_t = A_{21}(L)u_t + A_{22}(L)v_t,$$

where y is the log of output, π is inflation, u is an aggregate supply shock, v is an aggregate demand shock, and the $A_{ij}(L)$s are polynomials in the lag operator L. For the moment, I am assuming that inflation and output are both stationary in first differences. The innovations in (1) are assumed to be i.i.d. and uncorrelated contemporaneously. The Blanchard-Quah restriction that the aggregate demand shocks have no long-run effect on output implies that the long-run effect of v on y is zero, and so $A_{12}(1) = 0$.

Using (1) I can compute the impact of an innovation to v_t on both inflation and the level of output. For inflation, the impact of a unit innovation to v, over a horizon τ, is just the sum of the coefficients in $A_{22}(L)$:

(2) $$\frac{\Delta\pi}{\Delta v} = \sum_{i=0}^{\tau} a^i_{22},$$

where a^i_{22} is the coefficient on L^i in $A_{22}(L)$. The impact of v on the *level* of output can be computed from the coefficients in $A_{12}(L)$ as

(3) $$\frac{\Delta y}{\Delta v} \sum_{i=0}^{\tau} \sum_{j=0}^{i} a^i_{12}.$$

The relative impact of monetary policy on output and inflation is just the ratio of these:

(4) $$S_v(\tau) = \frac{\Delta y}{\Delta\pi} = \frac{\sum_{i=0}^{\tau}\sum_{j=0}^{i} a^i_{12}}{\sum_{i=0}^{\tau} a^i_{22}}$$

The estimated effect of monetary policy in (4) depends on the assumptions about the degree of differencing necessary to induce stationarity in output and inflation, as well as the horizon τ. To see why, consider the value of $S_v(\tau)$ in the limit as τ tends toward infinity. For the case in (1), where both π and y are assumed to be I(1), the impact of a shock on the level of both inflation and output will be permanent and will tend to some constant. It immediately follows that the cumulative impact of the shock in (3) will become infinite, while the impact on inflation will be finite, and so $\lim_{\tau\to\infty} S_v(\tau)$ will go to infinity.

Alternatively, if we assume that inflation is I(1), while output is stationary about a deterministic trend, since the Blanchard-Quah identification constrains the impact of an innovation to v to be zero in the long run, the result is that (after redefining S_v appropriately) $\lim_{\tau\to\infty} S_v(\tau) = 0$.

This implies that it only makes sense to examine cases in which the horizon is relatively short—I will look at five years. In addition, since the experiment in which Ball is interested is one in which inflation is permanently reduced, it makes no sense to study the case in which inflation is stationary in levels. If one were to assume that π_t were mean-reverting, then by assumption no shock is capable of reducing inflation, and the denominator of S_v would always tend toward zero.

Following in this spirit, I have estimated the value of S_v using the Blanchard-Quah identification, setting τ equal to five years, and approximating the restriction that $A_{12}(1) = 0$ by assuming that the v shocks die out completely after twenty years. Table 5C.1 reports the results for Ball's quarterly data set, which includes GDP or GNP for Australia, Canada, France, Germany, Italy, Japan, Switzerland, the United Kingdom, and the United States. The values in the table should be interpreted as the cumulative output loss experienced during a one-percentage-point decline in inflation. I study two cases, one in which inflation and output are both difference stationary, and another in which I assume that π_t is I(1) while y_t is I(0).

The estimates in table 5C.1 can be compared to those in Ball's table 5.1. Of

Table 5C.1 Estimates of the Impact of Nominal Shocks on Output and Inflation (cumulative five-year loss in gross domestic product per percentage-point decline in inflation)

	Cumulative Loss in Percent	
	y_t is I(0)	y_t is I(1)
Australia	0.08	−2.21
59:3–92:3	(1.99)	(4.92)
Canada	1.60	5.69
57:1–92:3	(2.25)	(4.74)
France	−0.76	−3.43
70:1–92:2	(1.31)	(5.50)
Germany	−1.42	8.90
60:1–92:2	(4.17)	(8.85)
Italy	0.68	2.33
60:1–92:1	(1.03)	(1.27)
Japan	−2.62	12.89
57:1–91:1	(4.09)	(7.94)
Switzerland	0.22	−2.23
70:1–92:3	(1.23)	(3.21)
United Kingdom	−0.27	1.45
57:1–92:2	(1.21)	(1.56)
United States	−0.52	8.36
59:1–92:4	(2.30)	(4.32)

Note: Standard errors are in parentheses. The standard errors are computed from the outer product of the numerical derivative of $S_v(\tau)$ with respect to the parameters, including the covariance matrix of v and u, with the covariance matrix estimated by GMM on the exactly identified system. For all of the calculations the vector moving average representation of the VAR is truncated at eighty quarters, so I assume that aggregate demand shocks die out after twenty years. Data are quarterly, from the International Monetary Fund's *International Financial Statistics*. All data are GDP, except for Germany and Japan, for which they are GNP.

the twenty-eight episodes Ball identifies, all fall well into two standard errors of the estimates calculated using my method, and a comparison of the magnitudes suggests that Ball's calculations are much closer to the ones which assume output is stationary. But the main point of the results is that the estimates are both very imprecise and dramatically different depending on the assumption about stationarity. For the United States, for example, the point estimate of the sacrifice ratio is 8.36 if output is I(1), but −0.52 if output is assumed to be I(0). (Keep in mind that as the horizon τ increases, the estimates of $S_y[\tau]$ increase without bound when y_t is I[1]). Furthermore, none of the estimates differ from zero at standard levels of statistical significance. This leads me to conclude that we know very little about the actual size of the impact of policy shocks on output, and so attempts to explain fluctuations in these numbers are unlikely to yield any convincing evidence.

I have one final comment that relates to the nature of the question being posed and how the answer is calculated. This is a more basic conceptual issue. As is clear from equation (4), the calculation of the sacrifice ratio uses the sum of the undiscounted changes in output over some horizon. It seems strange to me to add these things up in this way. Instead, I would suggest that we do something analogous to a cost-benefit analysis of inflation.

From the time path of the output and inflation changes implied by the structural vector autoregression (VAR), and an assumed discount rate, I can compute the present value of the output loss associated with a one-percentage-point decline in inflation. I can then ask what the benefit of this disinflation must be for it to be worth incurring the cost. Since the inflation decline is permanent, we can calculate the necessary inflation benefit, as a percentage of GDP per year forever, that would be needed for us to be willing to pay the output costs. This estimated benefit can then be compared to estimates of the costs of steady inflation.

For the simple case implied by equation (1), we can write this as follows. Assuming that the discount factor (measured at the same frequency as the data) is given by β, then the discounted value of the output loss—the cost—is just

$$PDV(y) = \sum_{i=0}^{\infty}\beta^i\sum_{j=0}^{i}a^i{}_{12}. \qquad (5)$$

This must be balanced against the inflation reduction. Calling ß the "benefit" of reducing inflation by one percentage point (measured as a percentage of one-year's GDP), we can write the benefit as

$$PDV(\pi) = ß\left(\sum_{i=0}^{\infty}\beta^i\sum_{j=0}^{i}a^i{}_{22}\right). \qquad (6)$$

Equating $PDV(y)$ to $PDV(\pi)$, I can solve for ß. If one percentage point of steady inflation reduces output by more than ß percent, then it is worth paying the cost to eliminate it. (This calculation has the advantage of getting rid of the need to truncate the sums in order to keep the quantities of interest finite.)

This is a simple calculation, and for the U.S. case, using an annual discount rate of 2 percent, I estimate that the output costs of disinflation equal the benefits of lower inflation when each percentage point of steady inflation has a cost, ß, of 9.05 percent (standard error = 4.86) of a year's GDP! In other words, if one percentage point of inflation costs 9.05 percent or more of GDP per year forever, then it is worth paying the price to reduce it. The reason for this high point estimate is that with this specification the output costs and the inflation benefits both last forever. But again, the lack of precision of the estimate renders it virtually useless.

To conclude, my argument is that in order to even estimate the impact of monetary shocks on output—Ball's sacrifice ratio—we require a structural model in which such shocks are identified. An important element of such a model is a set of assumptions about the time-series properties of output, inflation, and the shocks. After examining one specific model, my conclusion is that the available data are unlikely to yield a convincing set of estimates.

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