NBER WORKING PAPERS SERIES

CHANGES IN THE CYCLICAL SENSITIVITY OF WAGES IN THE UNITED STATES, 1891-1987

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Working Paper No. 3854

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 1991

This research was supported by National Science foundation Grant No. SES-8707758. I have received excellent computer assistance from Jim Comer and research assistance from Linda Shumaker. Jack Wilson, Doug Pearce, Harry Holzer, Tom Kniesner, Dan Hamermesh, Bill Dickens, Alan Krueger, Lee Craig, Bob Margo, two referees, and participants at the 1989 NBER Summer Institute on Labor Studies and the Sixth World Congress of the Econometric Society at Barcelona, Spain gave me helpful comments on earlier drafts. This paper is part of NBER's research program in Labor Studies. Any opinions expressed are those of the author and not those of the National Bureau of Economic Research.

NBER Working Paper #3854
September 1991

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ABSTRACT

The conventional wisdom that nominal wages became less sensitive to the business cycle and more autocorrelated after World War II is reexamined here by considering whether these properties are artifacts of the methods used to construct prewar wage series. A replication based on these methods is more cyclically sensitive and exhibits less autocorrelation than the postwar data. Aggregation using variable instead of fixed employment weights also greatly exaggerates the cyclicality of prewar wages. These biases imply that wages are just as sensitive to the cycle today as 100 years ago, perhaps even more so.

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CHANGES IN THE CYCLICAL SENSITIVITY OF WAGES

IN THE UNITED STATES, 1891-1987

By Steven G. Allen

Nominal wages generally are believed to be less sensitive to the business cycle and more highly autocorrelated today than they were before World War II. Much attention has been devoted in recent research to reconciling increased rigidity in wages with the apparent increase in the stability of output. One possibility, suggested by John Taylor (1986), is that shocks in the postwar period have been less frequent and less severe. (Henceforth, the terms prewar and postwar should be understood to refer to World War II.) Christina Romer (1986a, 1986b) has presented evidence suggesting that the greater instability of the prewar economy is partly attributable to the poor quality of the data. She speculates that the gains from stabilization policy could have been offset by greater instability caused by rigid wages and prices. The conventional view that wage rigidity results in larger employment fluctuations has been challenged by J. Bradford DeLong and Lawrence Summers (1986) who argue that whenever there is a fall in wages and prices, expectations of further decreases develop. Despite the supply stimulus, the increase in real interest rates generated from deflationary expectations reduces aggregate demand and in many cases the net effect is contractionary. They conclude that the greater stability of the postwar economy can be attributed at least in part to more rigid wages and prices.

The belief that wages have become more rigid could not have become a part of the conventional wisdom in economics without both theoretical and econometric underpinnings. Labor markets have changed considerably since the turn of the century in terms of occupational and industrial mix, collective bargaining coverage, methods of wage payment, average job duration, workforce demographics, and government regulation. At the cost of some oversimplification, employer-employee matches today are much more likely to be long term contracts with a focus on lifetime compensation as opposed to spot market relationships with the wage rate being the key instrument for market clearing.

Even if labor market structure were the same today as at the turn of the century, the wage-setting process is likely to have adapted to stabilization policies and the widespread availability of social insurance. Expecting a return to full employment, wages need not decline in a recession, especially if the unemployed have other means of support. Such rigidity would have been irrational before World War II, especially in periods when the gold standard was being followed.

Phillips curve estimates have been used to provide empirical evidence, almost all of which is consistent with the hypothesis of increased wage rigidity, as shown in Section I below. Albert Rees (1959) is the standard source of prewar wage data used in these studies. His series is based on data from the Census of Manufactures and the Annual Surveys of Manufactures, with interpolating data from state labor bureaus for 1891-1918, the Conference Board for 1920-1931 and BLS for 1932-48. The Rees (1959) series does not incorporate revisions later made for 1890-1898 in Rees (1961) and 1920-1931 in Rees (1960), a point made by Anthony O'Brien (1985).

In addition to using the wrong Rees data, previous studies have failed to consider whether the methods used by Rees to estimate wage levels had an effect on the dynamic properties of the series. The main contribution of this study is a careful analysis of whether the dynamic properties of nominal wages are sensitive to the different ways in which prewar and postwar data on wages, output, and unemployment are constructed. Following the approach of Romer (1986a,1986b), the Rees series is replicated as closely as possible for the postwar period and then compared to the BLS series in terms of its univariate properties and Phillips curve coefficients.

I. EVIDENCE ON WAGE RIGIDITY

Viewed strictly from a univariate perspective, there is no question that wages were much less rigid in the prewar period, as shown in Table 1. The standard deviation of annual wage growth was more than three times larger in the prewar than the postwar period, even though the average growth rate was considerably larger in the postwar period. The duration of wage cycles was also much shorter in the prewar period. Wage growth in the postwar period exhibits autocorrelation for five years, whereas in the prewar period autocorrelation vanishes after one year.

The standard framework for examining the response of nominal wage growth to inflation and the business cycle is the Phillips curve. The focus on nominal wages is traditional in this literature and in many theoretical models of aggregate supply. Jeffrey Sachs (1980) is most frequently cited as evidence of increased wage rigidity between the prewar and postwar periods. His conclusions are based on Phillips curve estimates for 1894-1929 and 1952-76, along with simple comparisons of the deceleration of wage growth during contractions. The coefficient of the output gap is four to six times larger in his prewar sample than his postwar sample, whereas the coefficient of lagged inflation is about two and a half times larger in the postwar sample.[1]

[1]Other major studies published in the 1980's reach the same conclusions. Robert J. Gordon (1983) finds less cyclical responsiveness of nominal wages before 1922 along with no relationship between lagged price change and wage growth before 1950, using a single equation for 1892-1980 with time-varying coefficients. Taylor (1986) estimates a vector autoregression of wages and output and finds a stronger contemporaneous correlation between residuals in 1893-1914 than 1954-83. Lagged wages have a large effect on wage growth in his postwar sample but no effect in his prewar sample. Daniel Mitchell (1985) examines establishment-level data on nominal wage changes for 1923-1934 and 1959-1964. He concludes that there was more dispersion of wage changes and less resistance to wage cuts in 1923-1934.

The only discordant note across all recent studies is found in the work of Charles Schultze (1981, 1986), who questions whether the effect of the business cycle on wage growth has changed. Schultze (1981) develops a measure called the flexibility coefficient (the ratio of the deceleration in wage growth to the deceleration of output) and finds that it declined modestly from 1900-14 and 1923-29 to 1949-66. He concludes that wages were "already quite sticky in the late nineteenth and early twentieth centuries, but became somewhat stickier in the years after World War II (1986, p. 1153)." O'Brien (1985) shows that this conclusion is very sensitive to the choice of sample period; the difference between his prewar and postwar flexibility coefficients widens considerably if the contractions in 1920-21, 1929-33 and 1937-38 are included in the calculation. Schultze (1986) counters that his results hold if 1890-1899, 1919-22, 1933-40, and 1946-48 are added to his original sample.

The other way to find ambiguity on this issue is to refer to studies published in the 1960's and 1970's, all of which have very small samples for the postwar period. Anthony Santomero and John Seater (1978) note in their survey that "most studies that find the unemployment rate insignificant in explaining wage inflation do so primarily for the postwar years; prewar years are much more likely to show a significant relation (p. 506)." Robert A. Gordon (1975) finds a larger unemployment coefficient in the Phillips curve for 1954-1970 than for 1900-1914 or 1922-31. Because his empirical specification and choice of data series is almost identical to Sachs, the difference in results is entirely attributable to excluding various years from his prewar and postwar samples.

A decade has passed since the end of the postwar sample period in these studies, so it is natural to estimate an updated comparison. Table 2 reports estimates based on a specification similar to that used by Sachs

$$(1)\quad Dw_t = \alpha_0 + \alpha_1 X_t + \alpha_2 X_{t-1} + \sum_{i=1}^{3} \alpha_{2+i} Dp_{t-i} + \alpha_6 T + \epsilon_t,$$

where Dw_t = percentage change in average hourly earnings in manufacturing, X_t = excess demand in the labor market (G_t = output gap and U_t = unemployment), Dp_t = percentage change in price level, T = time trend, and $\epsilon_t = \rho\epsilon_{t-1} + \mu_t$. Descriptions and sources for all variables are reported in the appendix.

A few comments about differences between (1) and Sachs' estimating equation are in order. Olivier Blanchard and Summers (1986) and Robert J. Gordon (1988), among others, have included lagged excess demand variables to test for the presence of hysteresis. Expected changes in prices are proxied by including three lagged inflation rates. Experiments with a wide variety of alternative specifications (varying lag lengths, use of rolling AR forecasts as a proxy for expected inflation) showed that this was not a crucial assumption. A time trend is included to prevent trend differences from being incorporated into other coefficients. In some periods serial correlation is present, presumably reflecting autocorrelated variables that have been omitted from the equation and are independent from the other right-hand side variables. Dummy variables for wars, oil shocks, wage guidelines, or wage controls are not included because parameterization of residuals can give a false impression about precision of the estimates and is appropriate only when (1) is stable over time (see Walter Oi (1976)), precisely the hypothesis being tested here. Further, there is no objective criterion other than goodness-of-fit for defining such variables. The equation is estimated using Charles Beach and James MacKinnon's (1978) maximum likelihood procedure that produces estimates of ρ without losing any observations.

The output gap results indicate that the prewar-postwar difference in wage rigidity is modest at best. A one percentage point decrease in the output gap is correlated with a 0.32 percentage point increase in wages in the prewar period versus a 0.22 percentage point increase in the postwar period. In contrast, Sachs' output gap coefficients were 3.8 to 6.1 times larger in the prewar than the postwar sample.

Between 1891 and 1941 a one percentage point increase in the unemployment rate is associated with a 1.2 percentage point decrease in the rate of wage growth. The same change in unemployment in the postwar period is associated with a 1.0 percentage point decrease in the rate of wage growth. There is no economically or statistically meaningful difference between these two sets of estimates.

There are four noteworthy differences in the results in Table 2 for the prewar and postwar sample. First, the coefficients of the current and lagged excess demand variables in the prewar equations are almost identical in magnitude and opposite in sign, whereas lagged excess demand has no effect on wage growth in the postwar period. Second, inflation from three years ago is positively correlated with wage growth in the postwar period, but not in the prewar period. Third, residuals are autocorrelated in the postwar equation, but not in the prewar equation. These two phenomena could reflect long-term contracting in the labor market. Fourth, the standard error is much larger in the prewar equation. This presumably reflects some combination of greater measurement error and greater unpredictability of wages in the prewar period.

The difference between the results in Table 2 and those obtained by Sachs results from the choice of sample period. The same specification estimated over 1897-1929 and 1952-76 yields the following output gap coefficients (standard errors in parentheses):

	1897-1929	1952-76
Output gap	0.709(0.103)	0.128(0.046)
Lagged output gap	-0.043(0.139)	0.008(0.042)

The passage of time will no doubt make the postwar results in Table 2 as fragile as the findings of earlier studies. Arguments about what is the correct prewar sample period are not likely to be resolved.[2] Yet one issue that has not been addressed in the literature comparing prewar and postwar sensitivity of wages is whether the results are influenced by the methods used to construct the data. The remainder of this paper focuses on this issue.

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[2]The finding of increased rigidity of nominal wages for the business cycle now hinges on excluding years from the prewar sample periods, precisely the practice for which Schultze was criticized by O'Brien. The postwar coefficient increases substantially when 1977-87 are added to the sample, but remains much smaller than the coefficients for 1897-1929. The treatment of the Great Depression years does not explain why the prewar results above differ from those in Table 2. The critical factor is that the 1897-1929 period gives more weight to the experience between 1916 and 1923, when wage growth accelerated by large orders of magnitude in response to conventional fluctuations in unemployment. If an interaction term is added for 1916-23 or if the sample is restricted to 1891-1915 or 1924-1941, the coefficients for all other prewar years are quite close to those in Table 2.

II. HOW COMPARABLE ARE PREWAR AND POSTWAR DATA?

The prewar data on wages analyzed in this study are different from the postwar data in three important respects. First, the most commonly analyzed prewar data were put together from fragmentary historical records by economists who were mainly interested in trends rather than cyclical behavior. Today's wage series comes from a single monthly survey of employers and the techniques used in that survey have remained essentially the same for the entire postwar period. This raises the issue of whether differences in data construction influence cyclical properties. Second, year-to-year movements in manufacturing wages reflect not only wage changes within industries, but also changes in employment across industries. If there is a systematic tendency for the employment share of high wage industries to fall in a recession, then manufacturing wages will automatically decline even if wages within each industry stay the same. Third, the structure of the prewar and postwar economies is far from identical. Assuming that cyclical wage behavior varies across industries, occupations, and regions, this means that even if BLS had been doing monthly establishment surveys at the turn of the century, the results of those surveys would not be comparable to today's situation. The first consideration is examined in Sections II and III; the latter two, in Section IV.

A. Modern replication of Rees' series for 1890-1919

The wage series for manufacturing between 1890 and 1919 is the ratio of average annual earnings per full-time equivalent worker to the product of days per year and full-time hours per day.[3] Rees computed average annual earnings per full-time equivalent worker (payrolls divided by average employment) from the Census of Manufactures for 1889, 1899, 1904, 1909, 1914, and 1919. Estimates for the remaining years are interpolations based on reports of the state labor bureaus of Massachusetts,

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[3]Rees mainly relied on wage series compiled by the Conference Board and the Bureau of Labor Statistics for years after 1919. The procedures that Rees had to follow to splice these various series together or to reconcile them with the Census benchmarks to get consistent estimates of wage levels produce erroneous estimates of the percentage change in wages. Because the Conference Board data underrepresented the South and overrepresented large firms, Rees (1960) adjusted the Conference Board data downward by 6.4 percent in 1920 and 11.4 percent in 1932. Between 1920 and 1932 the adjustment factors are obtained through linear interpolation. Thus, the observed rate of change in wages in the Rees series after 1919 is actually the sum of the actual rate of change in the underlying data and the rate of change induced by Rees' splicing and interpolation procedures. As it turns out, this makes very little difference; the cyclical properties of the Rees series are the same as that of the Conference Board series over this period. Accordingly this study concentrates on the properties of the Rees series before 1919.

New Jersey, and Pennsylvania. The average number of days that manufacturing establishments were open is based entirely on reports from the same three labor bureaus. The hours series is compiled from the 1909, 1914 and 1919 Censuses and various BLS Bulletins. Rees set hours per day equal to average weekly hours for full-time workers divided by six.

The Rees series is based on more limited information than the modern BLS series; at a minimum this raises the noise-to-signal ratio. A more serious issue is whether Rees' estimates for 1890 through 1919 have any systematic cyclical bias. Days and earnings could be more (or less) cyclically sensitive in Massachusetts, New Jersey, and Pennsylvania than in the nation as a whole. The use of full-time hours per week underestimates cyclical adjustments in weekly hours. Rees compared his average hourly earnings estimates to the available evidence on wage rates from other sources and concluded that his series neither systematically understated nor overstated wage levels. As for cyclical patterns, Rees cautioned (p. 17):

> Although our work may have some value for cyclical problems, it must be used for such problems with great caution, for, at times, our data or our procedures would be inadequate or inappropriate for an investigation of cyclical fluctuation.

A potentially critical issue is double counting of fluctuations in full-time equivalent workers and days per worker. To illustrate the potential problems, consider the following two cases dealing with an establishment that usually operates with 40 workers for 20 days a month at a daily wage of $100. There is a recession and in each case hours are reduced by equal amounts but daily wages are unchanged. In the first case suppose that the plant closes for three months and employment is reported as zero in the months the plant was closed. The wage estimate would be $133.33 per day (based on average employment of 30 people working 180 days), creating the illusion of countercyclical wage movements. This type of bias can be offset or overridden in the reports compiled by the state labor bureaus by a current running in the opposite direction -- a possible tendency to report workers as being on the payroll even when they are not at work. Suppose instead that the recession results not in a plant closing, but in a drop in employment to 20 workers for six months (or equivalently a 50 percent reduction in hours for all workers for six months). If average employment is reported as 40 and days open as 240, the wage estimate is $75 per day, creating an illusion of procyclical wages.

Susan Carter and Richard Sutch (1990) report a much greater reliance on hours rather than employment adjustment in a sample of

Connecticut firms in the 1893 depression. Because these hours reductions were accomplished mainly by suspending operations for periods of less than one week, this finding suggests that there should be relatively few cases of employment being reported as zero, making a countercyclical bias unlikely if this practice were widespread.

Cyclically biased measurement error in the Rees series will also influence prewar estimates of autocorrelation. Random measurement error biases estimates of autocorrelation toward zero. Errors that vary with the cycle induce negative autocorrelation at the interval of the cycle. Prewar contractions before 1929 last no more than one year, but the length of expansions varies between one and four years. If the net result is negative autocorrelation at one year intervals, the prewar estimates of ρ are biased downward, which could explain why the prewar and postwar estimates of this parameter in Table 2 vary.

The properties of Rees' procedures can be gauged by developing modern counterparts to the three key components of his wage series for 1890-1919: average annual earnings, days per year, and full-time hours per day. This is a relatively straightforward task for the earnings and hours measures, but is much more difficult to do for the days measure because these data are no longer collected.

Average annual earnings for production workers in manufacturing can be computed using Rees' methods for 1972 through 1987 by setting benchmarks with the Censuses of Manufactures and interpolating with data for Massachusetts, New Jersey, and Pennsylvania from County Business Patterns for payrolls and data from the same three states from Employment and Earnings for average employment. These estimates are reported in the first column of Table 3.

The closest modern counterpart to Rees' full-time daily hours variable is average hours of workers on full-time schedules in manufacturing, reported annually in the January issue of Employment and Earnings. This is the mean of self-reported hours from the Current Population Survey for those working 35 hours or more. The corresponding daily hours figure is obtained by dividing by five.

The series indicating days in operation of manufacturing plants was used by Rees to adjust for seasonal and cyclical fluctuations in employment as well as a downward trend in days worked per year. As noted by Rees, the full-time work year for an establishment and a worker averaged 312 days, with 52 Sundays and Christmas off. For workers this had been reduced to 240 days by the 1980s -- 50 weeks of five-day workweeks less 10 holidays, with the remaining two weeks as vacation for someone with three or more years of service, according to Labor Department surveys of employee benefits. These studies, along with the Chamber of Commerce's series of paid time off work, reveal no downward trend in days worked per year in the 1970s and 1980s, so the focus of the replication is entirely on seasonal and cyclical fluctuations.

Lacking any modern data on days worked per year to use as raw material or as a basis for comparison, the replication must satisfy two standards. First, Rees' measure is markedly procyclical, so any replication that does not have this property can be rejected out of hand as providing no insights into cyclical bias in prewar wage data. This criterion is not sufficient because it says nothing about the magnitude of the relationship between days and the cycle, which need not be the same today as it was for 1890-1919. The second standard is that the replicated measure must be reasonably consistent with what a modern data collector would find if a days survey were conducted. This is established in two ways: (1) estimating the tradeoff between unemployment and days using data on the causes and duration of unemployment spells and (2) examining the change in hours worked in the 1973-75 recession in panel data sets.

Two approaches were employed to replicate the days variable. The first uses the perpetual calendar to estimate days under a specific set of assumptions about the within-year variation of employment across time and establishments. In the second approach, regressions are performed on Rees' data to obtain coefficients for transforming modern data on unemployment and output gaps into estimates of days. The advantage of the first approach is that a wage variable constructed using this measure can be used in Phillips curves estimates, whereas the latter set of estimates cannot. The advantage of the second approach is that the days estimate is guaranteed to have the same cyclical properties as Rees' measure, which need not be the case for the estimate obtained from the perpetual calendar. If the assumptions underlying the construction of any of these measures are incorrect, they will be exposed by the consistency checks described above.

In the first approach, assume that the typical employee works weekdays except for federal holidays. Annual employee days can then be estimated for the three states noted above by multiplying the number of workdays (obtained from the perpetual calendar and lists of federal holidays in various almanacs) by employment in each month and adding the monthly figures. Now assume that month to month variations in employment are entirely attributable to the opening and closing of establishments and that employment within each establishment is the same in every month that the establishment is open, so that the maximum employment figure for each year corresponds to the month when all establishments are open. Then under these highly stylized conditions, days per worker in each state equals the ratio of total employee days to maximum monthly employment. The three state estimates are weighted by average employment and reported in the second column of Table 3.

The second approach is to use the unemployment rate or output gap as an instrumental variable. Rees' days estimates for 1890-1919 were regressed on a time trend and either Romer's unemployment series (to get scaling consistent with the postwar unemployment series) or the output gap with the following results:

$$\underset{(3.979)}{\text{Days} = 300.931} - \underset{(0.413)}{1.721} * \text{Unemployment} - \underset{(0.135)}{0.228} * \text{time} \qquad R^2 = 0.391$$

$$\underset{(1.655)}{\text{Days} = 289.761} + \underset{(0.091)}{0.633} * \text{Gap} - \underset{(0.116)}{0.469} * \text{time} \qquad R^2 = 0.641$$

To estimate days for the postwar sample, the trends were ignored and constant terms in the two equations were scaled down by 40 so that both estimates generally stayed below 250 days. These series are reported in columns 3 and 4 of Table 3.

All three estimates of days per employee decline in the 1973-75 and 1981-82 contractions. These contractions are most comparable in terms of the decline in output gap to the prewar contractions of 1895-96, 1903-04, 1913-14, and 1918-19. The mean decline in Rees' days worked series for these four prewar contractions is 6.75 days, whereas the mean declines in days worked in 1973-75 and 1981-82 is 3.5, 5, and 9 days for the series in columns 2 through 4.

A more crucial check on the credibility of the IV days estimates is the magnitude of the business cycle coefficients in the days regressions. The key issue is whether the coefficients in the prewar equations are reasonable for the postwar period as well. Modern data can be used to determine whether the unemployment coefficient is plausible. Because the output gap coefficient is within an Okun's law transformation of the unemployment coefficient, its plausibility is thereby determined as well.

The specification using the Romer unemployment rate implies that a one percentage point change in overall unemployment (equivalent to 2.4 days worked out of 240) corresponds to a decline in days of 1.72. Given that fluctuations of experienced manufacturing unemployment are 1.6 times greater (based on a simple regression) than those of overall unemployment between 1972 and 1987, one might expect an estimate of 3.8(=2.4 x 1.6) to be more reasonable. However, some forms of unemployment reported by workers would not be matched by days lost in a survey of employers, including workers who quit and those who lost their job in a previous year. The percentage of the unemployed who left their job in a previous year can be approximated by using monthly data on unemployment durations for 1985, when unemployment was 7.2 percent -- almost exactly equal to the average for 1972-1987 (7.1 percent). Assuming that all unemployment spells began in the previous month and uniformly allocating persons in the 15-26 and 27-51 week categories across 3 and 5 months, 31 percent of the unemployed lost their job in a previous year. Assuming the ratio of job losers to job leavers among labor market re-entrants is the same as for other experienced unemployed persons, 81 percent of the unemployed between 1972 and 1987 were job losers. Using data in Kim Clark and Summers (1979, Table 5) the ratio of weeks of unemployment to weeks of nonemployment is 0.78. Adjusting for all these factors, the estimate of the tradeoff between the unemployment rate and days of operation drops to 1.6(=3.8 x (1-0.31) x 0.81 x 0.78). Unemployment durations tend to be longer in manufacturing than in other industries, making the true tradeoff slightly below 1.6, but still very close to the prewar estimate.

A final check on the credibility of the days estimates is a comparison of their behavior in the 1973-75 contraction to the behavior of annual hours for the same period in the Panel Survey of Income Dynamics (PSID) and the National Longitudinal Survey of Older Men (NLS), as reported in John Abowd and David Card (1989). Their PSID sample consists of men who worked every year between 1969 and 1979; the NLS sample has the same restriction for the period 1966-1975. Abowd and Card report that hours fell by 4 percent in the PSID, whereas hours fell by 7 percent in the NLS. As these samples cover individuals from all industries and occupations, the cyclical movement in hours for production workers in manufacturing is probably understated. Although part of this hours reduction could reflect a reduction in the length of the working day (e.g., lost overtime), most of it presumably comes from working fewer days. The days measures in columns 2 through 4 fell by 1, 2, and 5 percent between 1973 and 1975, suggesting that the measures in columns 2 and 3 are slightly underestimating the actual movement in days per worker.

The derived average hourly earnings measures based on Rees' approach and the BLS measure are reported in columns 5 through 8 of Table 3. The annual growth rates of the BLS and the Rees method series in column 6 are depicted in Figure 1.

B. Cyclicality comparisons

The year to year percentage changes in all three replicated wage series are much more volatile than those for the BLS measure, especially around turning points in the business cycle. Consider the replicated series in column 6 of Table 3 and the BLS measure in column 8. In 1974 both measures grew at a rate of approximately 8 percent. The growth rate of the replicated series accelerated to 12.5 percent in 1975, but then dropped to 6.8 percent in 1976. In contrast the growth rate of the BLS measure increased to 9.3 percent in 1975 and then fell to 8.1 percent in 1976. Another swing in wage inflation takes place between 1981 and 1983 for the replicated measure, going from 10.2 percent in 1981 to 6.9 percent in 1982 and then falling to 2.6 percent in 1983. Wage inflation according to BLS fell by a smaller extent during this period, from 9.9 percent in 1981 to 6.2 percent in 1982 and 4.0 percent in 1983.

Statistical evidence on the cyclicality of two of the three replicated wage series is muddied by the fact that wage growth is arithmetically related to the current and lagged values of unemployment or the output gap. An increase in the current value of unemployment or a decrease in the output gap lowers estimated days per employee, thereby raising wage growth. Some conclusions can be drawn by focusing on the replicated series in column 5 of Table 3. The correlations with current and lagged values (used because the BLS series decelerates considerably one year after the trough) of the output gap and unemployment for this and the BLS series are as follows:

	BLS	Replication
Output gap	0.181	0.231
Lagged output gap	0.433	0.727
Unemployment	-0.106	-0.224
Lagged unemployment	-0.440	-0.745

The replicated series shows stronger movement with the cyclical indicators than the BLS series over this time period. This is particularly true for its relationship with the unemployment series.

Why do the replicated series move with the cycle more than the BLS series? It is useful to consider two possible sources: payrolls and employee hours. Payroll growth in the replicated series reflects the experience of just three states. This introduces sampling error at a minimum. It also could result in biased estimates of cyclicality if the behavior of the manufacturing firms in these states is atypical. Average payroll growth at the trough of the cycle is 4.5 percentage points greater in the replicated series than the BLS series, but in the first year of each expansion wage growth in the replicated series is smaller by a roughly offsetting amount. Taken alone, this pattern of payroll growth makes the replicated series less procyclic.

Employee hours is the product of employment, days, and full-time hours per day in the replicated series, whereas in the BLS series it is the product of employment and hours per week. Recall that cyclical variations in work hours are captured with the days variable in Rees' approach and with the hours variable in the BLS series. By focusing on the product of employment and these variables one can determine whether there is systematic double-counting in any direction over the business cycle.

The growth of employee hours in the BLS data is lower at every trough and greater at every peak than in the replicated series. For illustrative purposes, consider the replicated series obtained by using unemployment as an instrument for days. At the troughs hours decline by an average of 7.9 percent in the replicated series, whereas they decline by 10 percent in the BLS series. As a result wage growth in the replicated series at the trough is about 2 percentage points lower than in the BLS series. At the peaks the difference is 2 percentage points in the opposite direction. This evidence suggests that excessive cyclicality of the prewar wage series results from overestimating employment (or days) at troughs and underestimating it at peaks.

C. Volatility comparisons

The greater volatility of the annual percentage changes in the replicated wage series is indicated by the summary statistics in Table 4. The standard deviations of the replicated series are all larger than that of the BLS series for 1973-1987, with the difference ranging from a modest 4 percent for the perpetual calendar series to 30 percent for the series where days is estimated from the output gap. The mean standard deviation of the "Rees method" series is 16 percent greater than the standard deviation of the BLS series.

Another indicator of volatility is the standard deviation of the difference of each series from its trend. The BLS measure of wage growth declined at an increasing rate in the 1980s, indicating a quadratic trend. The standard deviation of the difference between this series and its predicted trend value is 1.0 percentage points. In contrast the corresponding standard deviations of the Rees method series from their trends range between 1.9 and 2.7 percentage points, almost two to three times larger than the BLS measure.

D. Autocorrelation comparisons

Another important difference between the prewar and postwar data is the correlation between past and current wage growth. Table 4 reports autocorrelations for up to five years. The BLS series for 1973-87 shows the conventional postwar pattern with a first-order estimate of 0.76 and with autocorrelation persisting for four years. The three "Rees method" measures have less first-order autocorrelation and autocorrelation vanishes after one or two years. The pattern for the "Rees method" series obtained by instrumenting on the output gap displays a striking resemblance to the pattern in Rees' actual prewar data in Table 1 -- first-order autocorrelation of about 0.4 and no higher-order autocorrelation.

III. IMPLICATIONS FOR PHILLIPS CURVE ESTIMATES

The discussion above has shown that two of the main differences in the prewar and postwar behavior of nominal wages -- correlation with current output and past wage growth -- are at least partly attributable to the way the wage data were constructed. Valid historical comparisons of Phillips curves depend not just on comparable wage data but also on comparable unemployment and output gap data. Earlier studies use either Lebergott's unemployment series or Frickey's industrial production series to measure X_t. Romer (1986a) has shown that Lebergott's unemployment series is excessively volatile because it (1) assumes no cyclical shifts in the size of the labor force and (2) interpolates employment with output on a one-to-one basis in many cases. The pre-1914 output series constructed by Frickey is based mostly on production data for materials and goods early in the manufacturing process. Romer (1986b) shows that because of the behavior of materials inventories, the Frickey index is also excessively volatile.

To determine whether excess volatility in prewar wages and output affects the comparisons of wage rigidity made in previous studies, I estimated a restricted version of (1) using BLS and the replicated wage data in column 5 of Table 3. The key differences between the prewar and postwar estimates have been in the coefficients of the output gap (or unemployment), lagged inflation, and autocorrelation of the error term. To conserve degrees of freedom, I tested whether various combinations of lagged output or inflation terms could be restricted to zero over the BLS data for 1973-87. The restriction that the coefficients of the second and third lags of inflation and lagged output jointly equal zero could not be rejected, based on an F-statistic of 1.23 for the equation using G_t and 2.68 for the U_t equation with 3 and 7 degrees of freedom. With the additional restriction that the time trend coefficient is zero, the joint null hypothesis is still rejected (although less decisively) based on an F-statistic of 3.55 for the Gt equation and 2.00 for the U_t equation with 4 and 7 degrees of freedom. Table 5 reports results for the less parameterized specification.

The G_t and U_t coefficients are twice as large in the equation using the replicated wage series as in the equation using the BLS measure. This indicates that the methods used by Rees to estimate wage levels before 1920 create a strong upward bias in prewar estimates of the effect of output on nominal wage growth. If all of the prewar estimates in Sachs' Table 1 are biased upward by the same magnitude, then the range of his prewar output gap coefficients becomes 0.20 to 0.26, making his results comparable to the findings for 1948-1987 in Table 2 without any ad hoc arguments about which years should be included in the sample.

There is also a bias against finding autocorrelation in the replicated wage series. A strong positive autocorrelation pattern is present in the estimates obtained from the BLS measure, whereas there is a weak, negative pattern in the results for the replicated measure. This, along with the evidence in Section II, indicates that even if autocorrelation had been present in the prewar period, the pattern would never have shown up in the data.

Inflation in the previous year has a larger effect on wage growth in the replicated series than in the BLS series, duplicating yet another pattern in Table 1. Even though an F-test does not reject the joint hypothesis that inflation lagged two or three years has no effect on nominal wage growth, it is instructive to relax these restrictions. The lagged inflation coefficients in the specification corresponding to column 5 of Table 5 are as follows:

	One year	Two years	Three years
BLS	0.683(0.134)	-0.149(0.121)	0.242(0.121)
Replication	0.641(0.184)	0.121(0.290)	-0.262(0.192)

Lagged inflation has no effect on nominal wage growth after one year in the equations estimated over the replicated series. In contrast, inflation from three years ago is correlated with the growth of the BLS wage series. Once again this indicates that the lack of inertia in nominal wage growth in the prewar period is induced by the method used to estimate wage levels.

How much does the excess volatility of the output gap in the prewar period influence estimates of wage rigidity? Romer (1986b) shows that the FRB materials index has many of the properties of the Frickey output index. To gauge the impact of using an excessively volatile output measure on the business cycle coefficients, I constructed an output gap measure based on the FRB materials index and used this variable (F_t) in the wage equations in columns 3 (BLS) and 6 (replicated Rees) of Table 5. The coefficient based on the materials measure is about 20 percent smaller than the coefficient based on industrial production in both equations. If the postwar sample period is expanded to 1948-87, the materials gap coefficient is 17 percent smaller the output gap coefficient reported in Table 2.

Previous studies comparing wage rigidity in the prewar and postwar periods have essentially been comparing a prewar equation with excess volatility on both sides, much like in column 6 of Table 5, to a postwar model similar to that reported in column 2. For the 1973-87 sample period, the estimated impact of the output gap on nominal wages is 54 percent larger in the replication using excessively volatile data than in the equation using actual data. The absolute difference between the coefficients is 0.090.

This has striking implications for the comparisons of cyclical sensitivity of wages in Table 2. The G_t coefficient for 1891-1941 is within a standard deviation of the F_t coefficient in column 6. Given the biases established in Table 5, its true magnitude for 1891-1941 is likely to be between 0.205 and 0.226 -- slightly smaller or equal to the G_t coefficient for 1948-87. Further, the difference between the G_t coefficients for 1891-1941 and 1948-87 in Table 2 is amazingly close in both absolute (0.094) and comparative terms (42 percent) to the difference between the column 2 and 6 results for 1973-87 in Table 5. These findings indicate that, when consistent data are used, there is no discernable change in the response of nominal wages to the output gap over the last 100 years. If the findings for U_t for 1891-1941 in Table 2 are similarly biased, then the response of wages to unemployment is actually slightly greater today than in the prewar period.

The above conclusions are tempered by two considerations. First, the discussion at the end of Section I showed that choice of sample period affects Phillips curve estimates a great deal; the same consideration could apply here. Before 1973 the <u>County Business Pattern</u> data on payrolls pertain to the first quarter only. This factor, along with the unavailability of the full-time hours series before 1968, precludes an extension to an earlier period. Second, even though additional parameterization of the model is rejected by the data, changes in the specification do have an effect on the estimates, as one might expect with so few degrees of freedom. The addition of a time trend reduces the X_t coefficients in the replicated wage equation by about 30 percent below the values reported in Table 5, but the G_t and U_t coefficients remain 50 and 87 percent larger than in the BLS wage equation. The addition of a lagged output gap term decreases the coefficient of the current output gap in the replicated wage equations and it actually becomes smaller than the corresponding coefficient in the BLS wage equation. However, the sum of the two output gap coefficients remains much larger in the replicated wage equations.

IV. AGGREGATION

To deal with the issue of whether changes in employment shares over the business cycle influence estimates of wage rigidity, average hourly earnings series with fixed employment weights are created and compared to those with variable weights. One series is created for 1890-1914, based on seven major industries in Rees (1961) for 1890-1914 and another for 1947-1987, based on two-digit SIC industries.

Aggregation using fixed instead of variable employment weights has one noticeable effect: it reduces the standard deviation of the prewar wage growth series from 3.7 to 2.8 percentage points, thereby eliminating much of the difference in the year-to-year variability in wage growth between 1890-1914 and 1948-87. There is weak first-order correlation in the fixed weight series (0.18) versus no autocorrelation in the variable weight series over this time period.

Estimates of (1) in Table 6 point to an even more dramatic conclusion. The unemployment coefficient for the prewar sample drops by 54 percent and the output gap coefficient drops by 40 percent when average employment for the entire period is used to weight wage growth by industry. The postwar X_t coefficients are now larger than the prewar values, even though the industry wage series constructed by Rees are subject to the same biases as the aggregate series.

Looking across all of these results, there is now an indication that wages may very well be more strongly procyclical today than they were in the prewar era. Such a conclusion could be premature because the fixed-weight series for 1890-1914 is limited to seven industries and thereby omits such major sectors as printing, furniture, chemicals, and automobiles which were included in Rees' aggregate wage series. Also, it obviously omits much of the prewar period.

These two shortcomings can be overcome by focusing on industry wage series not only from Rees (1961) and BLS, but also the Conference Board (1946). These results are reported in Table 7, using the same specifications as before. By estimating separate equations by industry, one can get behind the veil of aggregation to see whether there have been any cases where wages have become more or less rigid in the postwar period. This experiment also yields information about whether changes in labor market institutions have affected wage behavior over the business cycle.

The industry wage equations constitute another piece of evidence that wages have become more cyclically sensitive since 1914. The unemployment and output gap coefficients in all seven industries are larger in the postwar period than between 1890 and 1914. No strong pattern emerges in the comparison of the 1921-1941 and the postwar estimates; the former tend to be slightly larger in most industries but the differences are rarely immense.

It is very difficult to find any noticeable prewar-postwar distinctions within any industry and impossible at this stage to link them to any institutional changes. For instance unionization is usually associated with wage rigidity. Union workers are thought to be more insulated from the business cycle than their nonunion counterparts because of multiyear contracts and their ability to use the strike-threat as leverage during contract negotiations. Thus, one would think that union organization of the iron and steel, paper, rubber, and machinery industries would be reflected in smaller postwar coefficients for at least some of those industries. This is decidedly not the case.

V. CONCLUSION

Until a few years ago the prevailing wisdom within the economics profession was that output, wages, and prices have all been less volatile in the postwar period. Romer's work has seriously challenged the part of that wisdom relating to output. This study raises the same sort of questions about wages.

Even if one ignores the issue of whether the prewar data are excessively volatile, there is little difference in the prewar and postwar estimates of the effect of the business cycle on nominal wage growth in Section I and the estimates by industry in Section IV are actually smaller in the prewar period. The comparison of the replicated Rees and the BLS wage series in Section III showed that the business cycle coefficients in the prewar equations are biased upward by at least 50 percent. Taken together, these findings indicate that wages 100 years ago were no more sensitive to the business cycle than they are today, and probably were less sensitive. The comparison of the replicated Rees and the BLS series also showed that the lack of autocorrelation in the prewar data is also partly attributable to the way in which the Rees series was constructed.

One might still ask whether a one percentage point increase in unemployment today is truly comparable to the same change 100 years ago in terms of its impact on the labor market and the personal well-being of the unemployed. This issue hinges on a number of poorly understood characteristics of the prewar period, including the average duration of unemployment, the ability of workers to transfer skills across occupations and industries, and the amount of support available from families, churches, and communities. More work by economists and historians (perhaps along the lines of Alexander Keyssar (1986)) will be needed to come to grips with this issue.

The cost of focusing on how wages react to output and employment in this study has been the cursory attention paid to price behavior. This is clearly a subject that needs to be addressed more carefully in future work, in terms of both extending the econometric analysis in Sections III and IV and carefully studying the price data in their own right. If the reaction of wages to unemployment and the variability of output and employment in the two periods have not changed, then the greater univariate volatility of wages in the prewar period presumably must come from some combination of (1) measurement error, (2) sectoral shifts, and (3) the reaction of wages to prices. The analysis in Sections II through IV has indicated that the first two factors have clearly been important. The role of the third factor needs to be evaluated more carefully.

Another limitation of this study is the use of annual data, a unit of observation that is rarely consistent with the business cycle. There are no monthly wage data for the entire country before the Conference Board series started in 1920. Data compiled by one or more state labor bureaus could be useful in this regard. For instance recent studies of monthly data by industry in Connecticut and Ohio in the 1893-94 and 1907-08 contractions by Carter and Sutch and William Sundstrom (1990) both find little evidence of cuts in nominal wages.

Based on the belief that wages in the postwar economy have become more rigid and that wage rigidity reduces economic welfare, a number of proposals have been made in recent years to impose costs on the use of particular types of payment schemes for labor services that are believed to make wages less sensitive to economic conditions (e.g. bans on multiyear collective bargaining agreements, tax incentives for profit sharing or bonus systems). Given the changes in labor market structure and countercyclical policy that have taken place over the last 100 years and the absence of any evidence of changes in wage flexibility over that period, it would seem that these proposals are based mainly on theory rather than evidence.

DATA APPENDIX

1. Average hourly earnings. The variable used in the analysis of the 1891-1941 sample period comes from Rees (1961), Table 1, p. 4, col. 1, for 1890 through 1914 and from Rees (1960), Table 1, p. 3, col. 1, for 1915 through 1941. The Conference Board data for 1921-1941 are derived from Conference Board (1946), pp. 178-9. The postwar data on average hourly earnings were derived from CITIBASE. Monthly estimates of average hourly earnings (LE6HM) are aggregated into annual averages weighted by the product of production worker employment (LPWM) and average weekly hours (LPHRM). Wage rates by industry are derived from Rees (1961), Table 13, pp. 44-50; Conference Board (1946), pp. 178-9; CITIBASE, using two-digit SIC equivalents of the series described above; and various issues of Employment and Earnings.

2. Output gap. Two output series were used in the analysis: the Nutter (1962) series, reported in Long Run Economic Growth, series A15, pp. 184-185 and the Federal Reserve Board's industrial production index. Values of the latter for 1919 through 1946 are taken from LREG, series A16, pp. 184-185, whereas values for 1947 through 1987 are estimated using variable IP in CITIBASE. Each year's monthly values of IP are converted into annual values by summing them and dividing by 12. Potential output is estimated by regressing the industrial production index on a cubic time trend, unemployment and a constant and then calculating predicted values at the mean unemployment rate. One equation was estimated for 1890 through 1941 using the G. Warren Nutter (1962) output index; another was estimated for 1947 through 1987 using the Federal Reserve Board index. The FRB materials production index comes from Economic Report of the President, 1989, Table B-49, p. 363; the output gap based on this measure was calculated in the same way as the IP measure.

3. Unemployment rate. The prewar series is a combination of Lebergott's estimates for 1890-1929, as reported in Romer (1986a), and values for 1930-1941 from LREG, series B2, pp. 212-213. The postwar series was derived from seasonally adjusted monthly data from CITIBASE for 1948-1987. Annual average unemployment is the simple average of monthly rates,

estimated as the ratio of unemployed persons (LHUEM) to the civilian labor force (LHC). The estimate for 1947 comes from LREG, series B2.

4. Price index. The prewar series values for 1886-1889 and 1914-1941 come from LREG, series B69, pp. 222-223. Values for 1890-1913 from Rees (1961), p. 4 were spliced onto this series by multiplying the data from Rees by the ratio of the 1914 values of series B69 (30.1) to the 1914 value of Rees' series (100). The postwar annual series for 1947-1987 is the simple average of the monthly values of CPI-U from CITIBASE (series PUNEW). Values for 1943-1946 from series B69 of LREG (where 1967=100) were spliced onto this series using the 1967 value of the 1982-1984 benchmark series from ERP, Table B-58, p. 373.

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Table 1. Summary statistics for percentage change in wages, 1891-1941 and 1948-1987.

	1891-1941	1948-1987
Mean	3.4	5.4
Standard deviation	7.9	2.4
Minimum	-12.2	1.8
Maximum	32.0	9.9
Autocorrelation:		
1 year	0.397	0.667
2 years	0.050	0.528
3 years	0.053	0.515
4 years	-0.074	0.374
5 years	-0.105	0.220
6 years	0.030	0.053
7 years	0.091	-0.028

Sources: See data appendix.

Table 2. Philips curve estimates with current and lagged business cycle variables, 1891-1941 and 1948-1987.

	1891-1941	1948-1987	1891-1941	1948-1987
Constant	0.008	0.017	0.024	0.060
	(0.020)	(0.010)	(0.018)	(0.014)
Trend	0.001	-0.0004	0.001	0.0002
	(0.001)	(0.0004)	(0.001)	(0.0005)
Dp_{t-1}	0.912	0.495	1.012	0.448
	(0.226)	(0.092)	(0.195)	(0.095)
Dp_{t-2}	-0.672	-0.070	-0.676	-0.066
	(0.223)	(0.095)	(0.219)	(0.091)
Dp_{t-3}	0.142	0.203	0.186	0.219
	(0.196)	(0.090)	(0.186)	(0.101)
G_t	0.316	0.222		
	(0.076)	(0.050)		
G_{t-1}	-0.205	-0.010		
	(0.088)	(0.049)		
U_t			-1.181	-0.982
			(0.242)	(0.244)
U_{t-1}			1.084	0.294
			(0.255)	(0.224)
ρ	0.128	0.464	0.006	0.557
	(0.256)	(0.190)	(0.241)	(0.193)
R^2	0.498	0.746	0.547	0.736
D.W.	1.990	2.197	2.000	2.263
SEE	0.060	0.013	0.057	0.013

Table 3. Modern replication of Rees' method for estimating average hourly earnings of production workers compared to BLS estimates.

Year	Average annual earnings: Rees method (1)	Estimated days per worker (2)	(3)	(4)	Average hourly earnings: Rees method (5)	(6)	(7)	Average hourly earnings: BLS (8)
1972	7799	247	251	251	3.72	3.66	3.67	3.82
1973	8604	248	252	253	4.09	4.02	4.01	4.09
1974	9115	247	251	250	4.41	4.33	4.36	4.42
1975	10003	246	246	241	4.87	4.87	4.97	4.83
1976	10744	249	248	244	5.17	5.20	5.27	5.22
1977	11479	246	249	247	5.54	5.48	5.52	5.68
1978	12349	246	250	249	5.91	5.82	5.85	6.17
1979	13326	248	251	249	6.39	6.31	6.35	6.70
1980	14415	248	249	246	6.95	6.93	7.01	7.27
1981	15603	248	248	245	7.64	7.64	7.71	7.99
1982	16514	243	244	239	8.20	8.17	8.35	8.49
1983	17214	245	244	241	8.37	8.38	8.51	8.83
1984	18095	248	248	246	8.57	8.58	8.65	9.19
1985	19033	248	248	246	8.98	8.97	9.06	9.54
1986	19957	248	249	245	9.39	9.34	9.48	9.73
1987	20517	248	250	246	9.68	9.60	9.75	9.91

Sources: Column 1 is obtained from the Census of Manufacturing for 1972, 1977, 1982, and 1987 with the remaining years imputed as described in text.
Column 2 is obtained from monthly employment totals for three states, as described in text.
Column 3 is predicted days using unemployment as the instrument.
Column 4 is predicted days using output gap as the instrument.
Columns 5 through 7 equal column 1 divided by the product of daily hours (described in text) and columns 2 through 4, respectively.
Column 8 is obtained from CITIBASE, as described in the data appendix.

Table 4. Comparisons of volatility and autocorrelation of wage growth between BLS and replicated wage series, 1973-87

		------Replication method for days------		
	BLS	Perpetual calendar	IV: Unemployment	IV: Output gap
Mean	6.6	6.6	6.7	6.8
Standard deviation	2.7	2.8	3.1	3.5
Minimum	1.8	2.1	2.4	1.7
Maximum	9.9	10.6	12.5	14.0
Standard deviation of difference from quadratic trend	1.0	1.9	2.2	2.7
Autocorrelation:				
1 year	0.759	0.504	0.512	0.461
2 years	0.485	0.268	0.198	0.074
3 years	0.332	-0.002	-0.088	-0.151
4 years	0.128	0.002	-0.090	-0.124
5 years	-0.158	-0.104	-0.021	-0.018

Table 5. Phillips curve estimates with BLS and replicated wage series, 1973-1987.

	BLS			Replication		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.074	0.036	0.035	0.112	0.036	0.034
	(0.020)	(0.012)	(0.011)	(0.019)	(0.008)	(0.008)
Dp_{t-1}	0.479	0.523	0.522	0.733	0.709	0.694
	(0.122)	(0.117)	(0.120)	(0.108)	(0.118)	(0.114)
U_t	-0.645			-1.347		
	(0.272)			(0.281)		
G_t		0.166			0.313	
		(0.058)			(0.075)	
F_t			0.130			0.256
			(0.047)			(0.060)
ρ	0.839	0.804	0.784	-0.313	-0.246	-0.307
	(0.216)	(0.223)	(0.241)	(0.283)	(0.288)	(0.280)
R^2	0.888	0.901	0.896	0.754	0.721	0.715
D.W.	1.832	1.966	2.031	1.463	1.710	1.724
SEE	0.010	0.010	0.010	0.015	0.016	0.016

Table 6. Business cycle coefficients, variable and fixed employment weights in wage growth estimates, 1891-1914 and 1948-1987.

Period and weighting procedure	Wage growth				Unemployment rate coefficients		Output gap coefficients	
	Mean	S.D.	Minimum	Maximum	U_t	U_{t-1}	G_t	G_{t-1}
1891-1914, variable	0.018	0.037	-0.079	0.070	-0.649 (0.175)	0.215 (0.198)	0.210 (0.093)	0.102 (0.106)
1891-1914, fixed	0.017	0.028	-0.044	0.059	-0.300 (0.133)	-0.175 (0.149)	0.127 (0.065)	0.158 (0.074)
1948-1987, variable	0.054	0.024	0.018	0.099	-0.982 (0.244)	0.294 (0.224)	0.222 (0.050)	-0.010 (0.049)
1948-1987, fixed	0.053	0.023	0.021	0.096	-0.716 (0.225)	0.203 (0.205)	0.170 (0.046)	0.000 (0.045)

Table 7. Business cycle coefficients, by industry, 1891-1914, 1921-1941, and 1948-1987.

Industry	1891-1914 G_t	1891-1914 U_t	1921-1941 G_t	1921-1941 U_t	1948-1987 G_t	1948-1987 U_t
Boots and	0.171	-0.282	0.167	-0.657	0.190	-0.939
shoes	(0.056)	(0.109)	(0.084)	(0.332)	(0.070)	(0.334)
Leather	0.114	-0.277	0.230	-0.168	0.117	-0.524
	(0.086)	(0.178)	(0.090)	(0.110)	(0.054)	(0.259)
Paper and	-0.119	-0.050	0.120	-0.656	0.144	-0.570
paper products	(0.125)	(0.311)	(0.102)	(0.367)	(0.050)	(0.244)
Rubber	-0.126	0.181	0.190	-0.887	0.170	-0.645
	(0.071)	(0.166)	(0.089)	(0.299)	(0.066)	(0.320)
Foundry and	0.200	-0.262	0.222	-1.082	0.212	-0.916
machine shops	(0.055)	(0.130)	(0.078)	(0.253)	(0.052)	(0.257)
Textiles	0.139	-0.222	0.297	-1.319	0.293	-1.437
	(0.099)	(0.206)	(0.118)	(0.397)	(0.059)	(0.270)
Iron and steel	0.275	-0.998	0.346	-1.697	0.296	-1.209
	(0.164)	(0.321)	(0.132)	(0.441)	(0.106)	(0.517)
Chemical			0.237	-1.172	0.105	-0.442
			(0.092)	(0.287)	(0.048)	(0.234)
Electrical			0.139	-0.780	0.082	-0.352
manufacturing			(0.079)	(0.273)	(0.046)	(0.229)
Furniture			0.259	-1.179	0.209	-0.982
			(0.098)	(0.317)	(0.039)	(0.188)
Lumber			0.391	-1.617	0.168	-0.634
			(0.098)	(0.329)	(0.071)	(0.341)
Printing			0.027	-0.170	0.114	-0.548
			(0.046)	(0.178)	(0.028)	(0.131)

Note: The textiles estimates reported for 1921-1941 are based on wool manufacturing, the largest of the four textile industries in the Conference Board data. Estimates for the other three textile industries (cotton, hosiery and knit goods, silk) were very similar to the estimate for wool. The paper industry estimate for 1921-1941 is based on pulp and paper manufacturing, which was a much larger part of the paper industry than paper products. The estimates for the paper products industry were almost identical to those reported in the table. The printing industry estimate for 1921-41 is based on book and job printing, which had about four times as many employees as news and magazine printing.

Figure 1. Annual percentage change in BLS and replicated Rees series, 1973-1987