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Chapter 10

Another Look at Whether a Rising Tide Lifts All Boats

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President John F. Kennedy made famous the saying, "A rising tide lifts all boats." The American experience of the 1960s and 1970s—decades during which periods of rapid economic growth were accompanied by improved living standards for the disadvantaged—amply supported this view. Subsequent decades, however, did not, and the steadily declining real earnings of low-wage workers during the economic expansions of the 1980s and early 1990s have led many to question the ability of economic growth to ameliorate economic and social ills for the disadvantaged and perhaps even for the median worker. This paper assembles evidence on the cyclic nature of a number of important economic and social indicators since the early 1970s. A bottom-line finding of our paper is that President Kennedy's shibboleth continues to hold true: the benefits of strong economic growth for the disadvantaged are at least as great as they are for the more advantaged, and the costs of a downturn are borne disproportionately by the disadvantaged.

Table 10.1 provides a brief summary of economic and social outcomes during the business-cycle peak of 1989, the labor-market trough of 1992, and the peak of 2000.[1] Although a number of factors contribute to the patterns revealed in table 10.1, the results provide a rough indication of the effect of the business cycle on economic and social outcomes for various groups. The table clearly indicates that good things tend to happen in good times. For example, the unemployment rate of African Americans fell to its lowest level ever recorded in the economic expansion that culminated in 2000. In addition, between 1992 and 2000, the average real income of the bottom 20 percent of households grew more rapidly (15 percent) than did that of the middle 20 percent (12 percent), while the income of the wealthiest 20 percent of households grew the fastest (25 percent). In the 1989 to 1992 downturn, families at the bottom experienced the greatest relative decline in income (8 percent). The poverty rate also rose in that recession and fell in the subsequent growth period. Extreme poverty—defined as having income less than half the poverty line for one's household size—also moves with the business cycle, although it is less sensitive to business conditions because individuals in extreme poverty are less connected to the labor market. Undesirable

TABLE 10.1 / Economic and Social Outcomes over Recent Business Cycles (Percent)

	Peak, 1989	Trough, 1992	Peak, 2000
Economic indicators:			
Unemployment rate (percent):[a]			
Overall	5.3	7.5	4.0
White	4.5	6.6	3.5
Black	11.4	14.2	7.6
Hispanic	8.0	11.6	5.7
Poverty rate (percent):[b,c]			
Overall poverty rate	12.8	14.8	11.8
Extreme poverty (less than half the poverty line)	4.9	6.1	4.6
Average household income ($):[b,d]			
Bottom 20 percent	9,433	8,654	9,940
Middle 20 percent	38,862	36,373	40,879
Top 20 percent	114,912	108,189	135,401
Federal surplus (+) or deficit (−) as a percentage of GDP[e]	−2.8	−4.7	1.7
Social indicators:			
Violent crimes per 1,000 people:[f]			
Household income < $7,500	. . .	84.7	57.5
Household income ≥ $75,000	. . .	41.3	22.9
Welfare-utilization rate (percent):[g]	4.4	5.3	2.1
High school dropout rate (percent):[h,i]	12.6	11.0	11.8
Single-parent rate (percent):[h,j]	24.4	26.7	27.7

[a]U.S. Bureau of the Census (1995).
[b]Income and poverty data for 2000 are not yet available, so 1999 data are used. Income is in 1999 dollars, deflated by the Consumer Price Index for All Urban Consumers.
[c]U.S. Bureau of the Census (2000).
[d]U.S. Bureau of the Census (2000).
[e]*Budget of the United States Government* (2001, tables 1.1 and 10.1). Surplus-to-GDP ratio for 2000 is an estimate.
[f]U.S. Department of Justice (2000). Criminal-victimization data are for 1993 and 1999 and pertain to people aged 12 and older.
[g]Committee on Ways and Means (2000); and U.S. Bureau of the Census (2000). Welfare-utilization figures for 2000 are for June.
[h]Data for 1998 are used, more recent data not being currently available.
[i]*Digest of Education Statistics* (1999, table 108). The high school–dropout rate is for sixteen- to twenty-four-year-olds.
[j]U.S. Bureau of the Census (1998) and earlier reports to be found at *http://www.census.gov/population/socdemo/ms-la/tabch-1.txt*.

social outcomes, such as criminal victimization and welfare participation, also appear to improve during expansions.

On the other hand, the high school dropout rate moves mildly countercyclically, perhaps in response to greater labor-market opportunities (see, for example, Card and Lemieux 2000), and the single-parent rate has been growing secularly. Nevertheless, the picture that emerges from table 10.1 is that of a rising tide continuing to lift all boats, the dinghies at least as much as the yachts, while a falling tide submerges many of the least seaworthy vessels.

The properties of employment, earnings, income, and real wages over the business cycle have been the most thoroughly studied in the literature. The first section reviews and extends the evidence on the cyclic pattern of employment, earnings, and real wages. The results point to a strong procyclic pattern of employment and work hours—lower-skilled individuals are particularly likely to find employment and work longer hours when the labor market tightens. In addition, real wages are mildly procyclic. We also find that changes in unemployment have a larger effect on family earnings and other outcomes at later stages of a recovery or recession, and we find some evidence of asymmetry over the cycle: the harmful effect of a 1 percentage point increase in unemployment during a downturn exceeds the helpful effect of a decline of equal magnitude during an upturn. Consequently, a less volatile economy (that is, one with fewer downturns) is predicted to lead to better long-run outcomes than a more volatile economy with the same average growth rate.

The cyclic pattern of wage data is difficult to interpret because the composition of employment changes over the course of a business cycle. In the second section, we use the Panel Study of Income Dynamics (PSID) to examine the cyclic pattern of real wages for a balanced sample of individuals, following earlier work by Gary Solon, Robert Barsky, and Jonathan Parker (1994). Like theirs, our findings suggest that real-wage gains accrue during a tight labor market even for a fixed set of workers; changes in the composition of the workforce tend to attenuate only slightly (if at all) the cyclic wage effect found in unbalanced samples. However, the responsiveness of wages to unemployment may have declined in the last two decades.

A more difficult question is whether the benefits of a high-pressure economy are lasting. Do they extend beyond the boundaries of a particular business cycle? If the benefits prove to be persistent—for example, by changing the long-run mix of jobs—then a high-pressure economy has even more going for it than is commonly appreciated. Arthur Okun (1973, 208), for example, presented evidence suggestive of "an upgrading of workers into more productive jobs in a higher-pressure economy." The discussant of Okun's paper was none other than Alan Greenspan, who was skeptical of the long-term benefits of a high-pressure economy. "It is by no means clear to me," Greenspan (1973, 257) remarked, "that class A employment [jobs with career ladders] can be promoted sustainably through high-pressure economic expansions." The results that we present in our second section provide suggestive evidence that a high-pressure economy makes it somewhat more likely that workers will move from dead-end jobs to jobs with upwardly sloping seniority profiles.

The third section broadens our examination of the importance of cycles by looking at the effects on crime, welfare participation, health, and education. Interestingly, work injuries, which are typically procyclic, declined considerably in the economic upturn of 1992 to 2000.

Finally, the effect of strong economic growth on government finances has received little attention in the literature. Table 10.1 suggests that the federal government's financial position is particularly buoyant when the economy grows. The federal-government budget deficit swelled to a seemingly intractable 4.7 percent of GDP at the depth of the 1992 downturn, while a surplus equal to 1.7 percent of GDP is estimated for 2000. Although cause and effect are difficult to distinguish, these figures suggest that the very strong procyclic nature of federal-government revenues may carry important implications for the economy. Indeed, the Treasury seems to be a major beneficiary of a strong economy.

In the fourth section, we consider the effect of the business cycle on the level and distribution of government expenditures across spending categories. We focus on the state- and local-government level to exploit regional variability in economic conditions. The evidence indicates that all components of state- and local-government spending are procyclic, with capital spending (for example, highways and parks and recreation) generally more procyclic than current spending (for example, health and education). An important, and striking, exception is welfare spending, levels of which are not only procyclic, but more strongly so than any other category of government spending. Since average individual needs for public assistance are countercyclic, the procyclic nature of total welfare spending indicates that public generosity per welfare recipient is powerfully procyclic. Hence, the cyclic nature of public finances reinforces the notion that the affluence associated with good economic times expands society's resources and thereby provides benefits to all income groups.

THE EFFECT OF THE BUSINESS CYCLE ON EMPLOYMENT, HOURS, AND EARNINGS

Previous Literature

The properties of employment, earnings, and wages have received the most attention in the literature. In particular, prior research has examined the effect of business cycles and local labor-market conditions on employment outcomes (Bartik 1991, 1993a, 1993b, 1996; Blanchard and Katz 1992; Holzer 1991; Hoynes 2000a), real wages (Bils 1985; Blank 1990; Keane, Moffitt, and Runkle 1988; Solon, Barsky, and Parker 1994), racial differences in labor-market outcomes (Bound and Holzer 1993, 1995), labor-market outcomes of disadvantaged youths (Acs and Wissoker 1991; Bound and Freeman 1992; Cain and Finnie 1990; Freeman 1982, 1991a, 1991b; Freeman and Rodgers 2000), and family income, poverty, and

income inequality (Bartik 1994; Blank 1989, 1993; Blank and Blinder 1986; Blank and Card 1993; Cutler and Katz 1991; Freeman 2001). These studies almost universally find that labor-market outcomes are procyclic, with greater sensitivity among lower-skilled groups.

The studies of disadvantaged youths relate labor-market outcomes to local (typically, metropolitan statistical area, or MSA) unemployment rates. That literature has consistently found that higher local unemployment rates lead to reductions in employment and earnings (Acs and Wissoker 1991; Bound and Freeman 1992; Cain and Finnie 1990; Freeman 1982, 1991a, 1991b; Freeman and Rodgers 2000), with larger effects for blacks, younger workers, and less-educated workers (Acs and Wissoker 1991; Freeman 1991a). Using micro data, David Ellwood (1982) finds that extended spells of nonemployment among teenagers have a small effect on future employment prospects but a large, adverse effect on future wages.

Other studies have examined how MSAs' labor-market conditions affect employment and wages in the population (Bartik 1991, 1993a, 1993b, 1994, 1996; Bound and Holzer 1993, 1995). These studies estimate the effect of the growth and changing composition of MSA employment on area employment and earnings. The results differ somewhat across studies, but they generally show that changes in labor demand lead to larger changes for blacks, younger persons, and those with lower education levels. The patterns seem to hold for men and women. William Wilson (1996) carefully documents the decline in employment among low-skilled males since the late 1960s, a decline that he attributes to the fall in availability of jobs in central cities.

Hilary Hoynes (2000a) examines the effect of business cycles on the employment, earnings, and income of persons in different demographic groups defined by sex, education, and race. The business-cycle effects are identified using variation across MSAs in the timing and severity of shocks. The results consistently show that individuals with lower education levels, nonwhites, and low-skilled women experience greater cyclic fluctuation than do high-skilled men. The results are the most striking when examining comprehensive measures of labor-force activity, such as the likelihood of full-year, full-time work. Government transfers and the earnings of other family members decrease the differences between groups, as business cycles have more skill-group-neutral effects on family income than on individual earnings. The evidence further suggests that the 1992 recession led to more uniform effects across skill groups than did earlier cycles.

Studies of family income and poverty have typically used either national (Blank 1989, 1993; Blank and Blinder 1986; Cutler and Katz 1991) or regional (Blank and Card 1993) variation in unemployment rates or GNP as cyclic indicators. Such studies find a consistent negative relation between unemployment rates and inequality and poverty. In particular, Rebecca Blank (1989) disaggregates household income into many components and finds earnings and capital income to be procyclic and some transfer income to be countercyclic. Overall, she finds greater variation in income over the cycle for those who are young, male, and nonwhite. More recently, Richard Freeman (2001) used a pooled cross-state time-series model to examine the effect of earnings, unemployment, and inequality on poverty. He finds that decreases in unemployment or increases in real wages lead to declines in poverty.

Distinct from the literature on labor-market outcomes are empirical studies, dating back at least to John Dunlop (1938), that examine the cyclic nature of real wages. More recently, panel data have been brought to bear on this issue. This literature primarily uses aggregate measures of business cycles (national unemployment rates or GNP growth) and examines the degree to which real wages fluctuate with the business cycle and whether changes in the composition of the workforce over the cycle (for example, more low-paid new entrants during upturns) confound procyclic movements in wages. Katherine Abraham and John Haltiwanger (1995) provide a thorough review of this literature. A growing body of evidence uses panel data to hold the composition of the workforce constant over the cycle by focusing on a fixed sample of workers. Mark Bils (1985) uses the National Longitudinal Survey and concludes that composition changes have only a small effect on the cyclic nature of real wages, while Solon, Barsky, and Parker (1994, 3) use the PSID and conclude that "the apparent weakness of real wage cyclicality in the United States has been substantially exaggerated by a statistical illusion," namely, changes in the composition of the workforce. Solon, Barsky, and Parker attribute the difference between their conclusion and Bils's to his focus on young men, which misses changes in the age composition of the workforce. Our reading of the evidence is that real wages have moved slightly procyclically since 1970, although we agree with Abraham and Haltiwanger (1995, 1262) that "the cyclicality of real wages is not likely to be stable over time."

An important issue that arises throughout the literature is whether one should use national, regional, or metropolitan-area controls for business cycles. The main appeal of using the national cycle is that it is measured relatively precisely and reflects movements in the aggregate economy. However, there are two principal weaknesses of using an aggregate cycle measure: first, it may pick up the influences of unmeasured aggregate variables; second, it suffers from low power because the number of aggregate cycles is small. Furthermore, the use of an aggregate measure of the cycle does not exploit regional differences in the business cycle. In contrast, using regional or metropolitan-area variation in labor-market conditions leads to a substantial increase in the size of the estimation sample. This will, in general, lead to more precise estimates and allows for the estimation of models with unrestricted time effects. The time effects control for the unmeasured aggregate variables that are a concern in the aggregate models. Furthermore, some argue that labor-market outcomes are more influenced by local variables than by national variables (Blanchflower and Oswald 1994; Bartik 1994). However, using state or metropolitan areas introduces measurement error in the unemployment rate. In fact, the Current Population Survey (CPS), the main data set used in this area, is not designed for reliable estimates of most MSAs. Two other issues that argue against using metropolitan samples are that

the boundaries of these areas change (perhaps endogenously) over time and that metropolitan areas do not cover the entire United States.

Another estimation issue that arises is whether the data should be specified as a Phillips curve or a wage-curve relation. The Phillips curve relates the *change* in the dependent variable (for example, log wages) to the *level* of the unemployment rate. The wage curve relates the level of the dependent variable to the level of the unemployment rate. In first-differences, the wage curve relates the growth of wages to the change in the unemployment rate over the corresponding time period. The wage-curve specification assumes that wages are higher when (or where) unemployment is low. The Phillips curve specification assumes that wages are growing when unemployment is low.

David Blanchflower and Andrew Oswald (1994) promote the wage-curve relation. David Card (1995) and Olivier Blanchard and Lawrence Katz (1997) test whether wage data are more consistent with the Phillips curve or the wage curve and conclude that a Phillips curve provides a better description of wage data. Interestingly, Blanchard and Katz interpret the Phillips curve as a wage curve, in which the wage in year $t - 1$ is proxying for the reservation wage. Because a wage-curve specification has a more natural theoretical interpretation and fits the data (hours as well as wages) that we use better than the Phillips curve specification does, in the subsequent analysis we estimate wage-curve specifications. Our main qualitative conclusions are likely to be similar if a Phillips curve specification were used instead.

Our findings for the labor market, presented in this section and the next, extend the literature by providing estimates through 1999—allowing us to analyze the effect of the sustained recovery and to examine whether this cycle is different from earlier cycles. We also examine the effect of cycles in more detail by exploring whether changes in unemployment have different effects in booms and busts or whether the length of the current boom or bust has an effect independent of the unemployment rate.

Estimating Effects of Cycles Using Time-Series Data

A natural starting point is to estimate the simplest model using annual time-series data on average annual hours worked and unemployment rates. We use a sample of persons aged twenty to fifty-five from the March CPS covering the years 1975 to 1999 to calculate average annual hours worked in each year and combine it with Bureau of Labor Statistics (BLS) data on annual unemployment rates. The March CPS is an annual demographic file that includes labor-market and income information for the previous year, at the individual and family levels. The sample size is approximately 150,000 persons per year.[2]

Annual hours worked is averaged over workers and nonworkers and thus reflects changes in the employment rate as well as in the intensity of work. We use this "prime-age" sample to minimize the effects of early-retirement and early-schooling decisions. We estimate a specification that regresses the year-to-year change in the log of average annual hours worked (ΔLNHRS) on the year-to-year change in the unemployment rate (ΔUR). We chose this specification after exploring several different ones. The first-differenced specification consistently provided a better fit to the data than did a Phillips curve specification (that is, change on level). We use the first-differenced specification throughout the paper. The coefficient estimates and standard errors for the time-series model are

$$\underset{}{\Delta \text{LNHRS}} = \underset{(0.002)}{0.010} - \underset{(0.001)}{0.015(\Delta \text{UR})} - \underset{(0.0001)}{0.0003(\text{year})}, \quad R^2 = 0.87.$$

A 3 percentage point decrease in the unemployment rate—about the size of the reduction experienced in the recovery since the 1992 trough—is associated with a 4.5 percent increase in average hours worked. To put this magnitude in perspective, note that annual work hours averaged 1,538 over this time period and ranged from a low of 1,378 to a high of 1,675. At the overall average, hours would increase by almost seventy, or two weeks of full-time work a year.

Using the National Bureau of Economic Research (NBER) national-business-cycle dating, we can allow the effect of the unemployment rate to differ in expansions (EXP) and contractions (REC). The coefficient estimates and standard errors are

$$\begin{aligned}\Delta \text{LNHRS} = {} & \underset{(0.003)}{0.011} - \underset{(0.002)}{0.014(\Delta \text{UR} \times \text{EXP})} - \underset{(0.002)}{0.017(\Delta \text{UR} \times \text{REC})} \\ & - \underset{(0.0002)}{0.0004(\text{year})}, \quad R^2 = 0.87.\end{aligned}$$

This suggests that a given change in the unemployment rate has a larger effect in a recession than in an expansion, although the differences are not statistically significant.[3]

To examine how stable the relation is over time, we add a dummy for POST89 and interact it with the change in the unemployment rate. The estimates for that model are

$$\begin{aligned}\Delta \text{LNHRS} = {} & \underset{(0.003)}{0.005} - \underset{(0.004)}{0.009(\text{POST89})} - \underset{(0.001)}{0.015(\Delta \text{UR})} \\ & + \underset{(0.003)}{0.004(\Delta \text{UR} \times \text{POST89})} - \underset{(0.0003)}{0.0003(\text{year})}, \quad R^2 = 0.90.\end{aligned}$$

Although not precisely estimated, the results show that the effect of a change in the unemployment rate has decreased in the last cycle.

An alternative cyclic indicator to the unemployment rate is the Federal Reserve Board's capacity-utilization rate (CU). The capacity-utilization rate captures the concept of sustainable practical capacity and is equal to an output index divided by a capacity index. We were motivated to look at capacity utilization because James H. Stock and Mark W. Watson (1999) and others have highlighted the fact that the price Phillips curve is much more stable if one uses the capacity-utilization rate in place of the unemployment rate. The basic time-series first-difference model using capacity utilization yields the estimates:

$$\Delta \text{LNHRS} = \underset{(0.004)}{0.006} + \underset{(0.001)}{0.004(\Delta \text{CU})} + \underset{(0.0003)}{0.0001(\text{year})}, \quad R^2 = 0.69.$$

A 3.5 percentage point increase in the capacity-utilization rate—the increase in the last recovery—is associated with a 1.4 percent increase in average annual hours. (We have divided the capacity-utilization rate by 100 in the regression, with the result that a 3.5 percentage point increase is equal to 0.035.) This suggests a weaker effect compared to that of a change in the unemployment rate.

Like the effect of a change in the unemployment rate, the marginal effect of a change in the capacity-utilization rate is larger in recessions:

$$\begin{aligned}\Delta \text{LNHRS} = {} & \underset{(0.005)}{0.012} + \underset{(0.001)}{0.003(\Delta \text{CU} \times \text{EXP})} + \underset{(0.001)}{0.006(\Delta \text{CU} \times \text{REC})} \\ & - \underset{(0.0003)}{0.0002(\text{year})}, \quad R^2 = 0.73.\end{aligned}$$

Here, the coefficients are significantly different at the 10 percent level. Unlike the Phillips curve, however, the first-difference log of annual hours shows essentially the same degree of stability with either capacity utilization or the unemployment rate as the cyclic indicator. Adding the POST89 dummy and interaction to the model generates the following estimates:

$$\begin{aligned}\Delta \text{LNHRS} = {} & \underset{(0.004)}{-0.001} - \underset{(0.006)}{0.017(\text{POST89})} + \underset{(0.001)}{0.005(\Delta \text{CU})} \\ & - \underset{(0.002)}{0.002(\Delta \text{CU} \times \text{POST89})} + \underset{(0.0004)}{0.001(\text{year})}, \quad R^2 = 0.80.\end{aligned}$$

As with the unemployment rate results, we find that the effect of a change in the capacity-utilization rate has decreased in the last decade, although this change is insignificant.

While these results provide a simple summary of the data, the use of aggregate data is somewhat limiting. In particular, the cyclic indicators (unemployment rates and capacity-utilization rates) may, to some degree, pick up other unmeasured aggregate variables. In the next subsection, we extend our analysis by presenting models that take advantage of regional variation in the timing and severity of cycles. This increases the power of the empirical analysis and, by including year dummies, controls for the effect of unmeasured aggregate variables that cut across regions.

Employment, Earnings, and Wages and MSA-Specific Cycles

The aggregate regression will yield biased estimates if there are omitted factors that are correlated with the unemployment rate and that affect labor-market outcomes (for example, nationwide government-policy changes). We follow other recent papers in the literature (for example, Freeman and Rodgers 2000; Hoynes 2000a) by using MSA-level data to take advantage of the substantial variation in business cycles across regions in the United States and account for time effects. As previously, we start with a sample of persons aged twenty to fifty-five from the March CPS. The analysis uses data from the 1977 to 2000 CPS surveys, which cover the years 1976 to 1999. In each year, we calculate various labor-market outcomes for each MSA identified in the CPS sample. In particular, we calculate the fraction employed at some time during the year (called the annual EPOP, or employment-to-population rate) and mean values for hours worked, earnings, and hourly wages. Our measure of average wages is confined to workers, while the other outcome variables do not condition on work status. We also examine family outcomes, including mean family earnings, income, and poverty rates.[4]

Because we are ultimately interested in examining whether responsiveness to cycles varies across groups, we form demographic groups defined by education (less than twelve years, twelve years, thirteen to fifteen years, and sixteen or more years), race (white, nonwhite), and sex.[5] The regressions are based on cell-level data where the cells are defined by MSA, year, and demographic group. All regressions are estimated by weighted least squares, using as weights the number of observations in each cell.

We will rely on the unemployment rate as our main measure of the cycle. The MSA-level unemployment rates are available on an annual basis beginning in 1976 from the BLS Local Area Unemployment Statistics division.[6] Instead of using the national NBER dates of business cycles, we use the timing of the cycles at the census-division level. Specifically, we assigned cycle peaks and troughs for each of the nine regions by examining the local minimums and maximums in the division-level unemployment rates. Each MSA was assigned the cycle dates corresponding to the census division in which it is located.

These data allow us to estimate equations of the following form:

$$\Delta \log(y_{jmt}) = \alpha_j + \lambda_m + \theta_t + \gamma \Delta \mathrm{UR}_{mt} + \varepsilon_{jmt},$$

where y_{jmt} is the mean labor-market outcome (such as mean real hourly wages) for demographic group j in MSA m in time t, and UR_{mt} is the unemployment rate in MSA m in period t. The regression also includes unrestricted effects for demographic group (α_j), MSA (λ_m), and time (θ_t). The identification of the key parameter, γ, comes from differences in the timing and severity of cycles across MSAs.

For comparability to the earlier aggregate analysis, we first relate the change in the log of average annual hours at the MSA-year-group level to the change in

TABLE 10.2 / Determinants of Change in MSA Log Average Annual Hours Worked, March CPS, 1976 to 1999

	(1)	(2)	(3)	(4)	(5)
ΔUR (national)	−.016 (.001)	−.016 (.001)			
ΔUR (MSA)			−.012 (.001)	−.012 (.001)	−.008 (.001)
Additional controls (fixed effects)	None	Demographic group, MSA	None	Demographic group, MSA	Demographic group, MSA, year
Observations	44,773	44,733	44,733	44,733	44,733

Notes: Authors' tabulations of the 1977 to 2000 March CPS. The sample consists of persons aged twenty to fifty-five with positive CPS weights. The observations are MSA-demographic group-year cells. The demographic groups are defined by *race* (nonblack, black), *gender* (male, female), and *education* (< twelve, twelve, thirteen to fifteen, sixteen+). The dependent variable is the first-difference of log average annual hours worked in the MSA-demographic group-year cell. It is a weighted regression with the number of CPS observations in the cells as the weight. The unemployment rate, UR, is measured as a percentage of the labor force. Standard errors are in parentheses.

the aggregate unemployment rate. These results, shown in column 1 of table 10.2, utilize nationwide time-series variability in the cycle. The MSA cell-level analysis generates essentially the same estimates that we find for the country as a whole. Column 2 adds fixed effects for demographic group and MSA, which does not substantively change the estimates. Column 3 replaces the national unemployment rate with the MSA-level unemployment rate. The results show that the coefficient on the change in the unemployment rate is about one-quarter lower at the MSA level: a 3 percentage point reduction in the mean unemployment rate is associated with a 3.6 percent increase in average annual hours in an MSA. This smaller effect may be due to measurement error in the MSA unemployment rate or to different responses to local and national labor-market shocks. Column 4 adds fixed effects for demographic groups and MSAs, which does not alter the results. Adding time effects in column 5, however, reduces the effect of the unemployment rate by another third. This suggests that there are factors not being controlled for that are associated with higher unemployment rates and lower average annual hours worked.[7] All the remaining estimates in this section are from models that control for year, MSA, and demographic group.

Going beyond the use of annual hours as the labor-market measure, table 10.3 presents estimates for the full set of individual labor-market and family-outcome variables. The estimates in panel A are of the same specification as that used in column 5 of table 10.2. These estimates show that labor-market outcomes are strongly procyclic. The results indicate that annual earnings are more procyclic than are annual hours and that real hourly wages are less procyclic than are

TABLE 10.3 / Estimating the Effect of Cycles on Labor-Market Outcomes across MSAs Using the CPS, 1976 to 1999, Basic Specifications

Key Explanatory Variable	Change in Annual EPOP (Level)	Change in Log Annual Hours	Change in Log Real Annual Earnings	Change in Log Real Hourly Wage (Workers)	Change in Log Real Family Earnings	Change in Log Real Family Income	Change in Family Poverty Rate (Level)
A. Base case							
ΔUR	−.0030	−.0077	−.0097	−.0043	−.0121	−.0090	.0022
	(.0006)	(.0014)	(.0020)	(.0017)	(.0024)	(.0019)	(.0009)
B. Asymmetries in effect of unemployment rate							
ΔUR × recession	−.0043	−.0136	−.0144	−.0020	−.0173	−.0128	.0045
	(.0011)	(.0023)	(.0033)	(.0028)	(.0040)	(.0032)	(.0015)
ΔUR × expansion	−.0022	−.0040	−.0067	−.0057	−.0087	−.0066	.0008
	(.0008)	(.0018)	(.0026)	(.0022)	(.0031)	(.0025)	(.0012)
p-value for test of equal coefficients	.132	.001	.075	.310	.097	.141	.062

Note: Authors' tabulations of the 1977 to 2000 March CPS. The sample consists of persons aged twenty to fifty-five with positive CPS weights. The observations are MSA-demographic group-year cells. The demographic groups are defined by *race* (nonblack, black), *gender* (male, female), and *education* (< twelve, twelve, thirteen to fifteen, sixteen +). The dependent variable is the change in the (log or level) of the mean of the variable within the MSA-year-demographic cell. MSA unemployment rates are used. The model also includes fixed effects for demographic group, MSA, and year. It is a weighted regression with the number of CPS observations in the cells as the weight. The change in the unemployment rate, denoted ΔUR, is measured as a percentage of the labor force. Annual EPOP is the proportion of the population that worked during the year. Poverty rate is measured as a proportion of families. Standard errors are in parentheses.

wages or earnings. Average wages are particularly difficult to interpret when using pooled cross-sectional data since the mean is taken over a changing population if the composition of the workforce changes. We will address this issue in the next section using panel data from the PSID.[5]

The last three columns in table 10.3 examine the cyclic nature of family outcomes. An analysis of families may differ from one of individuals in that families contain varying numbers of potential workers with differences in propensities for intrafamily substitution of labor-market activity. Furthermore, family income and poverty status depend on government transfers, which are strongly procyclic. The basic estimates show that, as expected, family earnings and income are strongly procyclic and poverty rates countercyclic. Family income is less cyclic than family earnings (presumably owing to countercyclic transfers). The results suggest that the 3 percentage point reduction in unemployment rates in the economic recovery of the 1990s led to a 0.6 percentage point reduction in the poverty rate. The actual decline in the family-level poverty rate was 2.6 percentage points, so either other factors were at work, or the relation has become stronger over time.

Panel B of table 10.3 allows the effect of the unemployment rate to differ in recessions and expansions. As explained above, the cycles are dated using the nine census divisions. There seems to be an asymmetrical effect of unemployment in recessions and expansions. For employment, hours, and earnings, the effect of a change in unemployment rates is larger in recessions. The only exception is real hourly wages, which have a larger (but not statistically different) effect in expansions. These results imply that recessions tend to inflict a sharp amount of pain in a short period but that upswings lead to gradual improvements.

The models estimated in table 10.4 explore how the cyclic nature of labor market exerience varies across education groups and over time. Panel A of table 10.4 adds to the base model a set of interactions of the four education groups with the change in the unemployment rate. The results show that lower-education groups (especially those without a high school diploma) are much more responsive to cycles than are higher-education groups. For example, the results for annual earnings show that a 1 percentage point increase in unemployment leads to a 2.5 percent reduction in annual earnings for those with less than a high school education, compared to a 1 percent decline for those with a high school education, a 0.5 percent decline for those with some college, and a 0.1 percent decline for those with a college degree or more. In further results not shown here, nonwhites are more cyclic than whites, and women are less cyclic than men, the latter result due in part to women's behaving as added workers.

Referring back to table 10.3, in the sample as a whole, family income exhibits slightly less cyclic variation compared to family earnings (−0.012 versus −0.009). This tendency is present for all education groups but is much more pronounced for those with lower education levels. For example, among those with less than a high school education, a 1 percentage point increase in unemployment is associated with a 2.6 percent decline in family earnings but a 1.6

TABLE 10.4 / Estimating the Effect of Cycles on Labor-Market Outcomes in MSAs Using the CPS, 1976 to 1999, Exploring Differences across Education Groups and Time

	Change in Annual EPOP (Level)	Change in Log Annual Hours	Change in Log Real Annual Earnings	Change in Log Real Hourly Wage (Workers)	Change in Log Real Family Earnings	Change in Log Real Family Income	Change in Family Poverty Rate (Level)
A. Differences across education groups							
ΔUR × less than high school	−.005	−.020	−.025	−.005	−.026	−.016	.006
	(.001)	(.003)	(.004)	(.003)	(.005)	(.004)	(.002)
ΔUR × high school graduate	−.004	−.008	−.010	−.005	−.014	−.011	.003
	(.001)	(.002)	(.003)	(.002)	(.004)	(.003)	(.001)
ΔUR × some college	−.002	−.005	−.005	−.002	−.009	−.009	.003
	(.001)	(.002)	(.003)	(.003)	(.004)	(.004)	(.002)
ΔUR × college graduate or more	−.0002	.0004	−.001	−.005	−.002	−.001	−.001
	(.001)	(.002)	(.003)	(.003)	(.004)	(.004)	(.002)
B. Structural break in effect of unemployment rate							
ΔUR	−.003	−.008	−.012	−.005	−.013	−.010	.002
	(.001)	(.002)	(.003)	(.002)	(.003)	(.003)	(.001)
ΔUR × Post89	−.001	.001	.006	.002	.003	.002	.001
	(.001)	(.003)	(.004)	(.003)	(.005)	(.004)	(.002)

Notes: Authors' tabulations of the 1977 to 2000 March CPS. The sample consists of persons aged twenty to fifty-five with positive CPS weights. The observations are MSA-demographic group-year cells. Demographic groups are defined by *race* (nonblack, black), *gender* (male, female), and *education* (< twelve, twelve, thirteen to fifteen, sixteen +). The dependent variable is the change in the (log or level) of the mean of the variable within the MSA-year-demographic cell. MSA unemployment rates are used. The model also includes fixed effects for demographic group, MSA, and year. It is a weighted regression with the number of CPS observations in the cells as the weight. The change in the unemployment rate, denoted ΔUR, is measured as a percentage of the labor force. Annual EPOP is the proportion of the population that worked during the year. Poverty rate is measured as a proportion of families. Standard errors are in parentheses.

percent decline in family income, while, among those with a college degree, a 1 percentage point increase in unemployment leads to a 0.2 percent decline in family earnings and a 0.1 percent decline in family income. Thus, adding nonlabor income to family earnings significantly reduces the differences in cyclic responses across demographic groups. This pattern was also found by Blank (1989), Blank and Card (1993), and Hoynes (2000a) and seems to be due to the effects of countercyclic income-transfer programs such as public assistance and unemployment compensation.

Panel B of table 10.4 tests for a structural break in the effect of unemployment rates. In particular, we examine whether the most recent cycle (captured by the dummy POST89) differs from the earlier period 1976 to 1989. As does the earlier aggregate regression, the point estimates here generally show that sensitivity to the cycle decreased slightly in the 1990s. However, these differences are not statistically significant. It is possible, of course, that our simple structural break in 1989 does not capture what is a more complicated time structure to the cyclic nature of unemployment. Our results are also consistent with those of Freeman (2001), who finds little change in the effect of the unemployment rate on poverty over time, conditional on changes in inequality and wage growth. These results suggest that the decline in the poverty rate in the 1990s is only partially a result of the tight labor market. Freeman's analysis suggests that factors such as declining inequality and the rising median wage in the latter part of the 1990s, apart from low unemployment, also played a role.

Does the Duration of the Recession/Expansion Have an Effect?

The specifications used earlier assume that a percentage point change in the unemployment rate has a uniform effect on labor-market outcomes independent of the tightness of the market or point in the expansion or recession. Table 10.5 extends the analysis by including two additional variables: the duration of the recession and the duration of the expansion. The duration of the recession is measured as the number of years since the most recent peak (if in a recession, 0 otherwise). The duration of the expansion is measured similarly as the number of years since the most recent trough (if in a expansion, 0 otherwise). These duration variables are constructed using business-cycle dates specific to each of the nine census divisions. This specification is a simple way in which to incorporate the dynamic effects of the business cycle on labor-market outcomes.

Panel A of table 10.5 repeats the estimates of the base-case specification in table 10.3 for comparison. Panel B adds the duration variables to the base-case specification. Adding these variables does not significantly change the importance of the unemployment rate. The point estimates on the duration variables show that, holding the change in unemployment rates constant, increasing the length of the recession by a year leads to a worsening of labor-market outcomes and that increasing the length of the expansion leads to an improvement in

TABLE 10.5 / Estimating the Impact of Cycles on Labor Market Outcomes Across MSAs Using the CPS, Exploring the Role of Length of Recession/Expansion

	Change in Annual EPOP (Level)	Change in Log Annual Hours	Change in Log Real Annual Earnings	Change in Log Real Hourly Wage (Workers)	Change in Log Real Family Earnings	Change in Log Real Family Income	Change in Family Poverty Rate (Level)
A. Base case							
ΔUR	−.0030	−.0077	−.0097	−.0043	−.0121	−.0090	.0022
	(.0006)	(.0014)	(.0020)	(.0017)	(.0024)	(.0019)	(.0009)
B. Adding duration of recession/expansion							
ΔUR	−.0028	−.0071	−.0094	−.0048	−.0108	−.0083	.0019
	(.0007)	(.0014)	(.0021)	(.0017)	(.0024)	(.0020)	(.0009)
Duration of recession (years)	−.0012	−.0025	−.0006	.0031	−.0039	−.0023	.0016
	(.0009)	(.0019)	(.0028)	(.0023)	(.0033)	(.0027)	(.0013)
Duration of expansion (years)	−.0002	.0003	.0008	.0007	.0017	.0009	.0000
	(.0005)	(.0011)	(.0016)	(.0013)	(.0019)	(.0015)	(.0007)
p-value for test of equal and opposite coefficients	.236	.380	.949	.207	.620	.313	.313
p-value for joint significance of duration variables	.43	.31	.22	.91	.20	.43	.38
C. Also interacting duration of recession/expansion with change in unemployment rate							
ΔUR	−.0024	−.0039	−.0022	−.0033	−.0030	−.0000	.0022
	(.0014)	(.0029)	(.0042)	(.0035)	(.0050)	(.0041)	(.0019)
ΔUR × duration of recession (years)	−.0006	−.0034	−.0043	.0007	−.0051	−.0047	.0006
	(.0006)	(.0013)	(.0019)	(.0015)	(.0023)	(.0018)	(.0009)
ΔUR × duration of expansion (years)	.0000	−.0004	−.0019	−.0009	−.0024	−.0025	−.0003
	(.0004)	(.0009)	(.0013)	(.0011)	(.0016)	(.0013)	(.0006)
p-value for joint significance of duration interactions	.39	.02	.08	.35	.08	.03	.45

Notes: Authors' tabulations of the 1977 to 2000 March CPS. The sample consists of persons aged twenty to fifty-five with positive CPS weights. The observations are MSA-demographic group-year cells. The demographic groups are defined by *race* (nonblack, black), *gender* (male, female), and *education* (< twelve, twelve, thirteen to fifteen, sixteen+). The dependent variable is the change in the (log or level) of the mean of the variable within the MSA-year-demographic cell. MSA unemployment rates are used. Duration of recession and expansion corresponds to the cycles in the census division in which the MSA resides. The model also includes fixed effects for demographic group, MSA, and year. It is a weighted regression with the number of CPS observations in the cells as the weight. The change in the unemployment rate, denoted ΔUR, is measured as a percentage of the labor force. Annual EPOP is the proportion of the population that worked during the year. Poverty rate is measured as a portion of families. Standard errors are in parentheses.

labor-market outcomes. The recession effects are much larger than the expansion effects, probably reflecting the fact that recessions are typically shorter and more intense than expansions. The test statistics reported in the table, however, indicate that the duration variables are jointly and individually insignificant.

Panel C of table 10.5 presents estimates in which we include interactions between the change in the unemployment rate and the duration of the expansion and contraction (as well as including the main effect of the change in the unemployment rate). This specification allows the effect of a given change in the unemployment rate to differ with years into the expansion or recession. These results show important and statistically significant effects. Consider, for example, annual earnings. A 1 percentage point reduction in the unemployment rate in the second year into an expansion leads to a 0.6 percent increase in mean real earnings ($0.002 + 2 \times 0.0019$), while the same reduction in the eighth year into an expansion leads to a 1.8 percent increase in mean real earnings ($0.002 + 8 \times 0.0019$). These results could explain why such large improvements in earnings and family income were experienced toward the end of the 1990s.

COMPOSITION OF WORKFORCE AND JOBS OVER THE CYCLE

Balanced and Unbalanced Samples of Workers from the PSID

Employment of and hours worked by less-skilled workers in particular tend to rise during an upturn in the economy, as indicated by table 10.4. Even within narrowly defined demographic groups, the composition of the workforce could change over the business cycle. If lower-paid workers are induced to join the labor force during an upswing, then the cyclic wage effects estimated previously will be understated—that is, the average wage will be pulled down by lower-paid new entrants. To explore the effect of a change in the composition of the workforce on the cyclic behavior of real wages, we extend the analysis of Solon, Barsky, and Parker (1994) in tables 10.6 and 10.7. These researchers examined the cyclic nature of real wages for a *balanced* set of workers to prevent composition changes from affecting their results.

Table 10.6 uses the PSID to explore the cyclic nature of real wages for a balanced sample of individuals. Annual earnings data are currently available for 1967 to 1996, collected in the 1968 to 1997 waves of the survey. Following Solon, Barsky, and Parker, we initially restricted the sample to male household heads aged twenty-five to fifty-nine who were *continuously* employed at least one hundred hours each year from 1967 to 1987. Using this sample, we calculated mean log real hourly earnings each year, denoted $\ln(W_t)$.[9] We regressed the year-over-year change in $\ln(W_t)$ on the change in the national unemployment rate and a linear time trend.[10] Results are reported in column 1 of table 10.6. Column 2 reports the same estimated regression model but uses the change in log real GNP as a cyclic indicator instead of the unemployment rate. Because the sample

TABLE 10.6 / Aggregate First-Differenced Wage-Curve Estimates for Balanced Sample of Men, PSID, 1968 to 1987 and 1977 to 1996 (Dependent Variable: Annual Change in Mean Log Real Wage)

	Sample			
	1967/1968 to 1986/1987		1976/1977 to 1995/1996	
	(1)	(2)	(3)	(4)
Intercept	.241 (.038)	.180 (.041)	.155 (.080)	.12((.074
Change in annual unemployment rate	−.013 (.002)	. . .	−.006 (.006)	. . .
Change in log real GNP	. . .	.572 (.128)	. . .	.564 (.24'
Year	−.003 (.0005)	−.002 (.0005)	−.002 (.0009)	−.001 (.00(
Durbin-Watson statistic	1.34	1.93	2.65	2.59
R^2	.75	.70	.19	.33

Notes: Sample size is twenty years. Observations on 363 continuously employed male house hold heads born 1928 to 1942 were used to calculate mean log earnings each year from 196: to 1987, and observations on 335 continuously employed male household heads born 1937 t 1951 were used each year from 1976 to 1996. Sample was limited to individuals who worke at least one hundred hours each year and did not have major assigned data for hours or labor income. The unemployment rate is measured as a percentage of the labor force. Standa errors are in parentheses.

of individuals underlying these regressions is fixed, any effect of compositi changes over the business cycle is removed. Columns 3 to 4 extend this analy: for a similarly defined sample of men who were continuously employed frc 1976 to 1996.

For comparison, table 10.7 presents analogous estimates for an unbalanc sample. In column 1, the dependent variable is the mean log real hourly wage. varying, and less-restrictive, sample of men was used to calculate the depende variable each year; to be included in the sample in year t, the individual need to work 100 or more hours in year t and be older than age sixteen in year Column 2 also uses an unbalanced sample, but first-differences and regressi adjusts the micro wage data. Specifically, we estimate the following model weighted least squares:

$$\Delta\ln(W_{it}) = \theta_{68} + \ldots + \theta_{88} + \beta X_{it} + \varepsilon_{it},$$

where $\Delta \ln(W_{it})$ is the change in the log real wage from year $t - 1$ to t, $\theta_{68} \ldots$ (are coefficients on year dummies, and X_{it} is potential work experience (a labor-market outcomes. The recession effects are much larger than the expansion effects, probably reflecting the fact that recessions are typically shorter and more intense than expansions. The test statistics reported in the table, however, indicate that the duration variables are jointly and individually insignificant.

Panel C of table 10.5 presents estimates in which we include interactions between the change in the unemployment rate and the duration of the expansion and contraction (as well as including the main effect of the change in the unemployment rate). This specification allows the effect of a given change in the unemployment rate to differ with years into the expansion or recession. These results show important and statistically significant effects. Consider, for example, annual earnings. A 1 percentage point reduction in the unemployment rate in the second year into an expansion leads to a 0.6 percent increase in mean real earnings (0.002 + 2 × 0.0019), while the same reduction in the eighth year into an expansion leads to a 1.8 percent increase in mean real earnings (0.002 + 8 × 0.0019). These results could explain why such large improvements in earnings and family income were experienced toward the end of the 1990s.

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	Sample			
	1967/1968 to 1986/1987		1976/1977 to 1995/1996	
	(1)	(2)	(3)	(4)
Intercept	.241 (.038)	.180 (.041)	.155 (.080)	.120 (.074)
Change in annual unemployment rate	−.013 (.002)	. . .	−.006 (.006)	. . .
Change in log real GNP	. . .	.572 (.128)	. . .	.564 (.249)
Year	−.003 (.0005)	−.002 (.0005)	−.002 (.0009)	−.001 (.0008
Durbin-Watson statistic	1.34	1.93	2.65	2.59
R^2	.75	.70	.19	.33

Notes: Sample size is twenty years. Observations on 363 continuously employed male household heads born 1928 to 1942 were used to calculate mean log earnings each year from 1967 to 1987, and observations on 335 continuously employed male household heads born 1937 to 1951 were used each year from 1976 to 1996. Sample was limited to individuals who worked at least one hundred hours each year and did not have major assigned data for hours or labor income. The unemployment rate is measured as a percentage of the labor force. Standarc errors are in parentheses.

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$$\Delta \ln(W_{it}) = \theta_{68} + \ldots + \theta_{88} + \beta X_{it} + \varepsilon_{it},$$

where $\Delta \ln(W_{it})$ is the change in the log real wage from year $t - 1$ to t, $\theta_{68} \ldots \theta_8$ are coefficients on year dummies, and X_{it} is potential work experience (ag

TABLE 10.7 / Aggregate First-Differenced Wage-Curve Estimates for an Unbalanced Sample of Men, PSID, 1968 to 1987 and 1977 to 1996 (Dependent Variable: Annual Change in Mean Log Real Wage)

	Sample			
	1967/1968 to 1986/1987		1976/1977 to 1995/1996	
	(1)	(2)	(3)	(4)
Intercept	.178	.120	−.011	−.028
	(.036)	(.029)	(.061)	(.054)
Change in annual unemployment rate	−.012	−.014	−.008	−.008
	(.002)	(.002)	(.005)	(.004)
Year	−.002	−.002	.0002	−.0002
	(.0004)	(.0004)	(.0008)	(.0006)
Regression adjusted	No	Yes	No	Yes
Durbin-Watson statistic	2.43	2.35	1.93	1.97
R^2	.71	.81	.14	.21

Notes: Sample size is twenty years. Sample each year includes all individuals aged sixteen or older who worked at least one hundred hours that year and did not have major assigned data for hours or labor income. In columns 1 and 3, the dependent variable is the change in the mean log hourly wage for the sample available in those years. Micro data sample size is 50,461 in column 1 and 47,914 in column 3. In columns 2 and 4, a two-step procedure was used. In the first step, the year-over-year change in each individual's log wage was regressed on year dummies and potential experience. In the second-step regression (reported here), the year dummies were regressed on the change in the unemployment rate and a time trend. The sample restrictions used in the first-step regression are identical to those used to calculate the dependent variable in columns 1 and 3, except that individuals were required to have worked in adjacent years. The unemployment rate is measured as a percentage of the labor force. Standard errors are in parentheses.

minus education minus 6). In the second-step regression reported in column 2, the coefficients on the year dummies are regressed on the unemployment rate in year *t*. Notice that, because the regression model uses wage growth as the dependent variable, any wage gains from entering the labor market (which may be due to changes in the composition of jobs or employees) is missed in this specification, although these effects would be reflected in the column 1 results. Columns 3 and 4 report analogous results for the period 1977 to 1996.

Preliminarily, it is reassuring to note that our point estimates for the period 1968 to 1988 are very close to those found by Solon, Barsky, and Parker, even though we made a few changes in the way in which we handled the data (for example, applying sample weights, trimming outliers).[11] A 3 percentage point decline in the unemployment rate—about the magnitude observed in the expansion of the 1990s—is associated with a 4 percent increase in real wages.

The results for the balanced sample indicate a slightly stronger wage response to unemployment than do the results for the raw means in the unbalanced sample, but the differences among all three estimates (balanced sample, unbalanced means, and regression adjusted) are trivial. These results suggest that the mildly procyclic pattern of real wages displayed in tables 10.3 and 10.4 is unlikely to be severely biased by a changing sample composition, especially in the light of the fact that those results condition on demographic groups and education.[12]

Solon, Barsky, and Parker, however, concluded that the balanced sample provides stronger support for procyclic wage behavior than do the unbalanced, unadjusted data. The reason for this difference is that they weighted the wage data by hours worked in the regression corresponding to the one in column 1 of table 10.7 because it is common to use total payroll divided by total hours worked as a measure of the hourly wage in macro models. We suspect that the hours weighting matters because, as shown previously, hours move with the business cycle, especially for less-skilled workers. Thus, changes in the composition of the workforce appear to be less important for the cyclic nature of real wages than are shifts in the share of hours worked by existing workers in different wage categories. For these models to be comparable to the types of models estimated in tables 10.3 and 10.4, however, we did not weight by hours. Moreover, the balanced data in table 10.6 are not weighted by hours worked. If we do weight the hourly wage by hours worked in the unbalanced sample, however, we find that wage movements are about 50 percent more procyclic in the balanced sample than in the unbalanced one, a finding similar to that of Solon, Barsky, and Parker.

A more important difference between Solon, Barsky, and Parker's results and ours is suggested by the regressions for the period 1977 to 1996. In the balanced panel, we find that the procyclic pattern of wages is statistically insignificant when the unemployment rate is used as the cyclic indicator and about half as large as that found for the period 1968 to 1988. When real GNP growth is used as the cyclic indicator, however, the responsiveness of wages to economic growth in the latter period is quite close to that found in the earlier period. If the change in capacity utilization is used as the cyclic variable (not shown here), the results are in between: the coefficient on capacity utilization falls by a quarter in the latter period.

The coefficient on the unemployment rate is on the margin of statistical significance in the unbalanced samples. Interestingly, in contrast to Solon, Barsky, and Parker's results, in this period unemployment has a smaller magnitude in the balanced sample than in the unbalanced one. These results suggest that unemployment is becoming a less effective measure of labor-market tightness and that composition effects might even move in the opposite direction. Since real-wage growth was particularly strong in the period 1997 to 2000, it would be interesting to see whether the results for the unemployment rate continue to hold when new wage data are available in the PSID. These results also highlight the added power obtained from identifying cyclic effects using regional differences in unemployment changes; the coefficient on unemployment in column 3 of table

10.6, for example, is about equal in magnitude to that found with the MSA-level data, but, here, it is statistically insignificant, whereas it was significant when the disaggregated data were used.

We have not reported the corresponding regressions for women because the number of continuously employed female household heads and married women over a twenty-year period in the PSID is fairly small (144 per year from 1968 to 1988 and 193 per year from 1977 to 1996). Nonetheless, when we estimated the analogous models for women, the results provided even less evidence of procyclic wage movement. For the balanced sample, for example, the coefficient on the unemployment rate was statistically insignificant, small, and *positive* in the period 1968 to 1988 and statistically insignificant, small, and *negative* in the period 1977 to 1996.[13] Likewise, if we estimate separate models by sex using the CPS data in the previous section, we find that wages move procyclically for men and neither pro- nor countercyclically for women.

Over the entire period 1968 to 1996, wages move procyclically in the PSID for men and acyclically (but not statistically significantly) for women. If we estimate the model in column 2 of table 10.7 for the pooled twenty-nine-year period, for example, the coefficient on the unemployment rate for men is −0.011 (SE = 0.003) and for women is −0.002 (SE = −0.004).

We should also note that we found relatively minor differences in the cyclic nature of wages for different education groups using the PSID data, similar to the findings of Solon, Barsky and Parker (1994) and Swanson (2001). In the period 1968 to 1988, male high school dropouts exhibited more cyclically sensitive wages than did those with a high school diploma or a college degree. In the period 1977 to 1996, however, change in unemployment was not significantly related to wage growth for any of the education groups we examined.

Composition of Jobs

To control for shifts in the composition of *jobs*, we estimated a wage curve using data from the BLS's employment cost index (ECI). Because the ECI is calculated for a fixed set of jobs, these results are unaffected by changes in the composition of jobs over the business cycle. Specifically, we regressed the proportionate change in the ECI (December to December) less the proportionate change in the Consumer Price Index for All Urban Consumers(CPI-UX1) on the change in the unemployment rate and a linear trend. We used data on the ECI for total compensation in the private sector in the years 1980 to 1999. The coefficient estimates (and standard errors) were as follows:

$$\Delta(\text{real ECI growth}) = \underset{(0.64)}{-0.75} - \underset{(0.002)}{0.002}\,\Delta(\text{UR}) + \underset{(0.0003)}{0.0004}(\text{year}), \quad R^2 = 0.16.$$

Changes in unemployment are notably uncorrelated with changes in the ECI less inflation. Abraham and Haltiwanger (1995) reach the same conclusion using

quarterly ECI data from 1976 to 1993 and employment growth as a cyclic indica tor.

On the one hand, these results suggest that whatever cyclicality does exist ir wages is a result of changes in the mix of jobs. On the other hand, the PSID, th CPS, and the ECI suggest that real wages were not very sensitive to the unem ployment rate in the 1980s and 1990s.

Do Workers Move to Jobs with Steeper Seniority Profiles in Good Times?

Okun (1973, 237–38) argued that, in a high-pressure economy, employers ar more likely to provide jobs that offer "a schedule of wage increases at regula intervals, fringe benefits and seniority privileges" and paid vacation to improv worker attachment. His empirical support was based primarily on the fact tha high-wage industries such as manufacturing and construction tend to have rela tively strong procyclic employment swings. We use the PSID to test more di rectly the hypothesis that workers tend to gravitate toward jobs with rising se niority profiles in a tight labor market.

Specifically, for each of twelve major industries, we estimated a tenure slop by estimating a separate log wage equation that included years of tenure, poten tial experience and its square, sex, race, and years of schooling as explanator variables, using the 1976 cross-sectional wave of the PSID.[14] The estimated tenur slopes ranged from virtually 0 in entertainment and recreational services, min ing, and personal services to 2.8 percent higher pay per year of tenure in financ insurance, and real estate. We computed separate wage regressions for seve major occupations as well. The resulting returns to tenure ranged from 0 fo farmers to 2.2 percent per year for professional and technical workers. The esti mated tenure slopes provide an indication of the extent to which jobs in particu lar industries and occupations offer upward-sloping seniority profiles.

We then assigned each individual in the PSID the tenure slope correspondin to his or her industry in year t and computed the change in each individual' industry-tenure slope between year t and year $t-1$. We regressed this variabl on the annual change in the national unemployment rate, sex, race, and potenti experience and its square. The estimated coefficient on the change in the unem ployment rate for the full sample is reported in column 1 of table 10.8, an columns 2 and 3 report separate estimates for workers broken down by whethe they earned more or less than the median wage in year $t-1$. Columns 4 to provide the corresponding estimates using the change in the occupation-base returns to tenure as the dependent variable.

The results provide some suggestive evidence that workers tend to gravitat toward jobs in sectors with steeper tenure-earnings profiles when the labor ma ket becomes tighter. The coefficient on the unemployment-rate change for th full sample is negative for both industry and occupation slopes, although onl the former is statistically significant. Moreover, the significant shift in emplo

TABLE 10.8 / The Cyclic Nature of Seniority Profiles (Dependent Variable: Change in Tenure Slope Associated with Change in Industry or Occupation)

	Δ Industry-Based Seniority Returns			Δ Occupation-Based Seniority Returns		
	All (1)	High Wage (2)	Low Wage (3)	All (4)	High Wage (5)	Low Wage (6)
Change in unemployment rate (/1000)	−.016 (.009)	−.005 (.011)	−.031 (.017)	−.012 (.008)	−.014 (.008)	−.007 (.016)

Notes: Based on the PSID. Returns to seniority for each major industry and occupation were estimated from a standard human-capital log wage equation using the 1976 wave of the PSID. The year-over-year change in returns for each individual (based on industry or occupation) was then regressed on the change in unemployment rate, sex, race, and experience and its square (see the text). The coefficient on the change in the unemployment rate (times 1,000) is reported here. High wage is the subsample above the median wage in year $t - 1$; low wage is the subsample below the median wage in year $t - 1$. *Occupation* and *industry* refer to the current job, while *wage* (and *median wage*) refers to the preceding calendar year. Sample size for regressions using industry-level tenure returns is 115,751; sample size for regressions using occupation-level returns is 125,411. The industry analysis includes the years 1973 to 1997, and the occupation analysis includes the years 1970 to 1997. Standard errors are in parentheses.

ment toward industries with steeper tenure profiles in good times appears to be driven primarily by the behavior of lower-paid workers. The following calculation puts the magnitude of this effect in context and suggests that it is quite small in the aggregate. If the unemployment rate falls by 3 percentage points, the tenure profile is, on the basis of column 1, predicted to rise by 0.005 percent per year. The average industry-based tenure slope is 1.9 percent per year across the whole sample, so a tighter labor market would increase the average slope by only a trivial fraction. Because only a minority of workers change industrial sectors in a year, perhaps it is unreasonable to expect a very large effect in the aggregate. In any event, the results do suggest that workers tend to gravitate toward jobs in sectors with steeper earnings profile when the labor market tightens.

OTHER OUTCOMES

Crime

Crime rates dropped throughout the economic expansion of the 1990s. Between 1991 and 1997, the total crime index dropped 17 percent. As shown in table 10.1, violent crimes dropped by 32 percent in families with incomes less than $7,500 and by 45 percent in families with incomes over $75,000. Because crime rates are higher for those in lower income families, the number of violent crimes per thousand people fell more for lower income families. Studies have found that

crime rates respond to labor-market opportunities, both changes in unemployment rates and changes in wage levels. This literature, summarized recently by Freeman (1999), shows that unemployment rates have a modest effect on crime but that wages may be more closely correlated with criminal activity. Richard Freeman and William Rodgers (2000) use the Uniform Crime Reports to create a state-level crime-rate series with which to explore the relation between crime rates and unemployment at the state level. They find that crime rates fell most rapidly in the states where unemployment fell the most. In particular, they find that a 1 percentage point decrease in unemployment is association with a reduction of 1.5 percent in the number of crimes per youth. Thus, the 3 percentage point reduction in the unemployment rate in this expansion would have reduced crime per youth by about 5 percent. H. Naci Mocan and Daniel Rees (1999), studying criminal activity among juveniles, find that a 1 percentage point reduction in unemployment rates leads to a 0.4 percent reduction in the probability of selling drugs and a 0.3 percent reduction in the probability of committing robbery.

Welfare Participation

The 1990s have seen unprecedented reductions in welfare participation in the United States. Looking back at table 10.1, the percentage of the population receiving welfare fell from 5.2 percent in 1992 to 2.9 percent in 2000, the lowest rate in over thirty years. Overall, the national welfare caseload has fallen by more than half since its peak in 1993. Welfare caseloads tend to be countercyclic, with increases in periods of higher unemployment rates. However, the correlation between the business cycle and welfare participation appears to be stronger in the current cycle. As discussed in the *Economic Report of the President* (Council of Economic Advisers 1999), significant changes in welfare programs and family structure may have masked the effect of cycles in the 1970s and 1980s.

A recent literature has explored the role that the strong labor market has played in the declining welfare caseload in the 1990s (for a survey, see Blank, forthcoming). Several studies use pooled cross-state data on welfare caseloads and economic conditions and estimate models that control for state fixed effects, time effects, and state-specific time trends. The estimates in these studies vary somewhat, but, overall, they find that labor-market conditions are important determinants of the welfare caseload (Blank 2001; Council of Economic Advisers 1997; Ziliak et al. 2000). The Council of Economic Advisers study estimates the relative contribution of the unemployment rate and welfare reform to the per capita welfare caseload and finds that a 1 percentage point decline in each of two successive years leads to a 4 percent decline in the caseload in the second year. Rebecca Blank and James Ziliak et al. find statistically significant but somewhat smaller effects of labor-market conditions than did the Council of Economic Advisers study. Hoynes (2000b) finds that improvements in local labor-market conditions lead to lower caseloads through increases in exits from welfare and reductions in recidivism.

We agree with Blank (forthcoming), who suggests that one should use caution in making conclusions that are based on the current research. She points out that many factors that could have affected welfare caseloads changed in the mid-1990s—including large expansions in the earned income tax credit, minimum-wage increases, welfare reform, and the strengthening of the labor market—and argues that our ability to determine the relative importance of these factors is limited by the fact that the changes were coincident.

Health and Work Injuries

A surge in work-related injuries is usually an undesired side effect of higher productivity growth in an expanding economy (Smith 1972). Injuries are expected to rise when unemployment falls because work intensity increases and because many inexperienced workers are hired. The anticipated rise in work-related injuries has not occurred in the latest business cycle, however. Since 1992, the number of work-related injuries and illnesses fell by an impressive 25 percent, from 8.9 to 6.7 per 100 full-time-equivalent workers. All major types of injuries have declined. Since 1992, missed-workday cases are down by 36 percent for sprains and strains, 31 percent for broken bones, and 30 percent for carpal tunnel syndrome. Workplace fatalities are down by 13 percent, which suggests that the trend toward safer working conditions is not a mere reporting phenomenon.

A regression of the change in the overall injury and illness rate per one hundred workers on the change in the unemployment rate using data from 1973 to 1991 yields

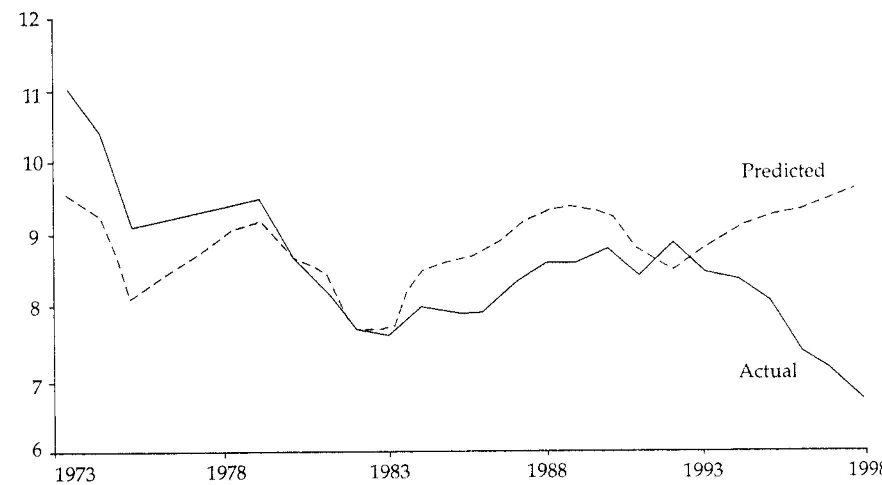
$$\Delta(\text{injury rate}) = \underset{(0.05)}{-0.11} - \underset{(0.04)}{0.35}(\Delta\text{UR}), \quad R^2 = 0.83,$$

which is remarkably similar to Smith's (1972) estimate of the cyclic nature of injury rates based on historical data. But, if the equation is estimated with the small number of post-1991 years, the correlation is positive and has a p-value of 0.12.

Figure 10.1 displays the injury and illness rate in each year and a prediction of the rate based on its pre-1992 relation with the unemployment rate. In 1998, the latest year for which data are available, there were 5.9 million cases of work-related injuries and illnesses in the private sector, the vast majority of which were injuries. If the pre-1992 relation between injuries and unemployment had held, one would have expected 3 million more injuries and illnesses in 1998 than the number actually recorded.

Interestingly, Leslie Boden and John Ruser (1999) find an inverse relation between the state unemployment rate and work injuries across states in the 1990s,

FIGURE 10.1 / Actual and Predicted Work-Related Injury and Illness Rate



Source: Bureau of Labor Statistics and authors' calculations.

Note: The predicted rate is based on an OLS regression of the injury rate on the unemployment rate, using annual data from 1973 to 1991.

controlling for year and state fixed effects. This result suggests that injuries stil move procyclically but that this effect is masked by an even stronger, nationwid downward trend in injuries in the 1990s.

The cause of the trend toward fewer work-related injuries is something of a mystery. The decline in injuries is not due to shifts in employment toward job in safer industries. Instead, injuries and illnesses declined within most industries. For example, the incidence rate fell by 22 percent in manufacturing, 3 percent in construction, and 27 percent in services. The decline also does no correspond well to the timing of changes in federal Occupational Safety an Health Administration (OSHA) policy and is just as strong in states that ru their own OSHA programs as in those that are under federal OSHA programs.

Whatever the reason for the decline in injuries, the economic implications ar sizable. W. Kip Viscusi's (1993) survey of compensating differentials finds tha workers are willing to forgo at least $33,000 in earnings (in current dollars) t take a job that entails no risk of injury as compared with one that carries certain risk of injury, all else equal. If this figure is correct, the 3 million fewe injuries and illnesses than predicted in 1998 would be implicitly valued b workers at around $100 billion—a bounty that is not included in the GNP. Thi amounts to a $1,000 raise per private-sector worker.

Education and Training

As indicated in table 10.1, the likelihood that youths will drop out of high school rises during the expansionary portion of the business cycle and falls during the contractionary portion. A countercyclic pattern of school enrollment has been carefully documented in studies by Alan Gustman and Thomas Steinmeier (1981), Audrey Light (1995), and David Card and Thomas Lemieux (2000), for example. This pattern likely arises because the opportunity cost of attending school is lower when the economy is depressed and good jobs are scarce. Likewise, the opportunity cost of attending school rises when jobs are plentiful. It is unclear, however, whether those who are induced to leave school early during good times return to school later on.

The cyclic pattern of job training is less well documented and mixed. For noncollege graduates, Lisa Lynch (1992) finds that, in the early 1980s, higher local unemployment increased the probability of participation in an apprenticeship program but decreased the probability of receiving on-the-job training from an employer. For a larger sample that included college graduates as well (again in the early 1980s), higher local unemployment increased the probability that young people who had completed school would take some off-the-job training.

PUBLIC FINANCES, THE BUSINESS CYCLE, AND THE DISADVANTAGED

The many beneficiaries of a strong economy include all levels of government. Rising incomes generate tax revenues that strengthen government finances, thereby facilitating expanded spending programs, tax cuts, debt reduction, or some combination of these three. Since the government may reasonably be expected to act as the agent of the citizenry, the benefits of government fiscal strength are received by citizens, although not necessarily in equal measure by all.

To the extent that economic expansion is associated with greater government spending, it follows that the disadvantaged are likely to receive many of the associated benefits. Government-funded activities provide services from which the disadvantaged benefit quite out of proportion to the tax obligations that they incur to finance them. There are two reasons why economic fluctuations over the business cycle may have particularly strong implications for the disadvantaged: governments with greater revenue sources tend to spend money on programs that particularly benefit the disadvantaged, and rising incomes generally trigger tax obligations that increase most rapidly among higher-bracket taxpayers. To put the same matter differently, economic downturns entail spending cuts that negatively affect the disadvantaged without providing commensurate tax relief.

This section considers evidence on the pattern of government activity over the business cycle and the likely effect of that activity on disadvantaged citizens. For this purpose, it is particularly useful to analyze data on American "states" (understood to include the District of Columbia) since states differ in the timing and magnitude of their economic cycles and thereby offer fifty-one separate glimpses into the effect of the business cycle on public finances. Moreover, state and local fiscal activity is important in its own terms, representing by now a significant fraction of the economy. Finally, there are interesting questions concerning the effect of economic cycles on the ability of subnational governments to pursue countercyclic policies, particularly insofar as these policies affect the disadvantaged.

The Scope of Government

It is useful to review the scope of government activity in order to identify the potential effect of cycle-induced fiscal-policy swings. Table 10.9 presents information on state- and local-government expenditures in the United States over the period 1952 to 1996. In 1996, state and local governments spent a total of $1.398 trillion, which represented 25.9 percent of personal disposable income in that year.[15] The largest single category of state- and local-government spending was that on public education, representing $399 billion, or 29 percent of the total. The next largest was welfare spending, at $193 billion, or 14 percent of the total, followed by health and hospitals, at $111 billion, or 8 percent of the total. Insurance-trust expenditures were also 8 percent of total spending in 1996, followed by highways at 6 percent and police and fire protection at 4 percent.

The U.S. federal government has somewhat different spending priorities that reflect its own political situation. Its on-budget 1996 spending of $1.260 trillion included $266 billion for national defense, $226 billion for income security (much of it transferred to the states and included in their totals), $174 billion for Medicare, $119 billion for health, $52 billion for education, training, employment, and social services, $40 billion for transportation, $22 billion for natural resources and the environment, $18 billion for justice, and, of course, numerous other categories of expenditure.

The effects of American business cycles are evident from the simple patterns revealed in table 10.9. Against a backdrop of steadily growing government, cyclic expansions are associated with growing state and local expenditures, while recessions are associated with spending slowdowns at the state and local level. For example, the recession of the early 1980s is reflected in the very modest growth ($36 billion, nominal) of total state and local expenditures between fiscal year 1981 and fiscal year 1982, a figure that is small compared to nominal growth in other years and striking in the light of the high rate of inflation during that time period. In order to identify more carefully the effect of business cycles on state and local fiscal activity, it is useful to examine the information available by comparing the experiences of American states over the cycle.

TABLE 10.9 / State and Local Expenditures by Function, Fiscal Years 1952 to 1996 ($Millions)

Year	Total	Public Education	Highways	Welfare	Health & Hospitals	Police & Fire Protection	Administration[a]	Insurance Trust[b]	Utility & Liquor Store[c]	Other[d]
1952	30,863	8,318	4,650	2,788	2,185	1,525	1,193	1,698	3,067	5,439
1953	32,937	9,390	4,987	2,914	2,290	1,636	1,263	1,711	3,316	5,430
1954	36,607	10,557	5,527	3,060	2,409	1,783	1,375	2,423	3,482	5,991
1955	40,375	11,907	6,452	3,168	2,524	1,923	1,452	2,764	3,886	6,299
1956	43,152	13,220	6,953	3,139	2,772	2,067	1,560	2,376	4,065	7,000
1957	47,553	14,134	7,816	3,485	3,119	2,278	1,725	2,749	4,429	7,818
1958	53,712	15,919	8,567	3,818	3,462	2,483	1,843	4,168	4,693	8,759
1959	58,572	17,283	9,592	4,136	3,724	2,624	2,003	4,784	4,901	9,525
1960	60,999	18,719	9,428	4,404	3,794	2,852	2,113	4,031	5,088	10,570
1961	67,023	20,574	9,844	4,720	4,086	3,104	2,237	5,299	5,523	11,636
1962	70,547	22,216	10,357	5,084	4,342	3,254	2,338	4,888	5,453	12,615
1963	74,698	23,729	11,150	5,420	4,638	3,398	2,439	4,987	5,736	13,201
1964	80,579	26,286	11,664	5,766	4,910	3,588	2,567	5,094	6,184	14,520
1965	86,686	28,563	12,221	6,315	5,361	3,855	2,773	4,950	7,058	15,590
1966	94,906	33,287	12,770	6,757	5,910	4,152	2,974	4,782	7,282	16,992
1967	105,978	37,919	13,932	8,218	6,640	4,548	3,313	5,278	7,350	18,780
1968	116,234	41,158	14,481	9,857	7,546	5,033	3,647	5,653	8,170	20,689
1969	131,600	47,238	15,417	12,110	8,520	5,694	4,105	6,053	8,820	23,643
1970	148,052	52,718	16,427	14,679	9,669	6,518	5,451	7,263	9,447	25,880
1971	170,766	59,413	18,095	18,226	11,205	7,531	6,243	9,793	10,300	29,960
1972	190,496	65,814	19,021	21,117	13,023	8,584	7,056	10,548	11,398	33,935
1973	205,466	69,714	18,615	23,582	13,844	9,584	7,934	11,074	13,035	38,084
1974	226,032	75,833	19,946	25,085	15,945	10,326	8,844	12,667	14,406	42,980
1975	269,215	87,858	22,528	28,155	18,846	12,048	10,154	21,209	17,285	51,132
1976	304,228	97,216	23,907	32,604	20,686	13,429	11,247	27,954	19,542	57,643
1977	324,554	102,780	23,058	35,905	23,039	14,857	12,453	26,149	24,190	62,123
1978	346,786	110,758	24,609	39,140	24,951	16,108	16,618	23,526	26,277	64,799
1979	381,867	119,448	28,440	41,898	28,218	17,354	18,448	23,504	30,845	73,712
1980	434,073	133,211	33,311	47,288	32,174	19,212	20,443	28,796	36,190	83,448
1981	487,048	145,784	34,603	54,121	36,101	21,283	20,001	36,583	43,016	95,556
1982	522,760	154,573	34,545	58,050	40,259	23,387	22,224	39,466	47,971	102,285
1983	566,567	163,876	36,655	60,484	44,118	25,516	24,508	47,335	52,811	111,264
1984	600,222	176,108	39,419	66,414	46,330	27,464	26,355	40,153	55,062	122,917
1985	657,888	192,686	44,989	71,479	49,581	29,873	28,890	44,191	59,798	136,401
1986	717,430	210,819	49,368	75,868	53,508	32,272	31,803	46,538	65,297	151,957
1987	775,318	226,658	52,199	82,520	56,972	35,594	34,896	50,815	68,440	167,224
1988	826,849	242,683	55,621	89,090	61,940	38,030	37,419	51,879	70,048	180,139
1989	890,863	263,898	58,105	97,879	67,757	39,703	40,923	54,994	73,510	194,094
1990	975,940	288,148	61,057	110,518	74,635	43,763	44,836	63,321	77,801	211,861
1991	1,063,270	309,302	64,937	130,402	81,110	46,568	48,461	74,159	81,004	227,327
1992	1,146,853	326,275	66,689	154,642	88,112	48,903	50,334	90,276	84,361	237,261
1993	1,207,125	342,595	68,134	167,046	94,651	51,943	52,402	98,908	84,361	247,085
1994	1,260,642	353,287	72,067	179,829	100,430	54,768	55,715	95,462	91,163	257,921
1995	1,347,763	378,273	77,109	193,110	105,946	58,064	60,018	107,340	94,235	273,668
1996	1,397,634	398,859	79,092	193,480	110,813	62,392	62,145	108,751	95,608	286,495

Source: Department of Commerce, Bureau of the Census.

Notes: Duplicative intergovernmental transactions are excluded.

[a]Includes financial, judicial and legal, general public building, and other government administration.

[b]Includes employee retirement, unemployment compensation, workers' compensation, and other social programs.

[c]Includes utility capital outlay, water supply, electric power, gas supply, transit, and liquor store.

[d]Includes interest, sewerage, utilities, and other categories.

Evidence of the Effect of Business Cycles

This section analyzes information on the spending activities of American state and local governments over the period 1977 to 1997. The following specification is useful in evaluating the effect of economic conditions on government fiscal activity:

$$S_{ijt} = Y_{jt}^{\beta_{1i}} N_{jt}^{\beta_{2i}} e^{\delta_{1ij} + \delta_{2it} + t\delta_{3ij} + u_{ijt}},$$

in which S_{ijt} is spending in category i by state j in year t, Y_{jt} is personal income in state j in year t, N_{jt} is the population of state j in year t, and β_{1i} and β_{2i} are parameters to be estimated. The dummy variable δ_{1ij} captures time-invariant state-specific effects on levels of spending in category i, δ_{2it} captures time effects, and δ_{3ij} captures state-specific growth rates in spending on i.

Taking first-differences of logs, the prior equation becomes

$$\Delta \ln S_{ijt} = \beta_{1i}\Delta \ln Y_{jt} + \beta_{2i}\Delta \ln N_{jt} + (\delta_{2it} - \delta_{2it-1}) + \delta_{3ij} + \varepsilon_{ijt},$$

in which ε_{ijt} is the residual, equal to $u_{ijt} - u_{ijt-1}$.

The first two rows of table 10.10 present estimates of the variant of this equation in which the S_{ijt} variable equals total spending by state and local governments. The data analyzed in this regression are drawn from the U.S. Census of Governments and consist of annual observations for the fifty states plus the District of Columbia over the period 1977 to 1997. The Census of Governments does attempt to verify that intergovernment transactions (such as spending by state governments that takes the form of transferring money to local governments) are counted just once in this tabulation. All the regressions include time dummy variables (not reported) to capture the $\delta_{2it} - \delta_{2i(t-1)}$ term. The regression reported in the first row of table 10.10 omits state dummy variables, thereby implicitly imposing the condition that the underlying expenditure growth rate δ_{3ij} be the same for all states, while the regression reported in the second row of table 10.10 includes state dummy variables and therefore does not impose this equality.

The results indicate that total spending responds positively to higher income and higher population levels. The estimated 0.18 coefficient in the first row of table 10.10 implies that a doubling of state income is associated (at a 1-year frequency) with 18 percent higher state spending. The 0.67 coefficient likewise indicates that a doubling of state population is associated with 67 percent greater spending. The inclusion of state dummy variables in the regression reported in the second row of table 10.10 changes these estimates only modestly.

It is hardly surprising that state- and local-government spending responds positively to personal-income growth since greater affluence is typically associated with demand for greater government services.[16] Furthermore, there is the widely documented "flypaper" effect—that government receipt of cash windfalls or other revenue sources tends to be accompanied by greater spending (Ladd 1993; Hines and Thaler 1995; Strumpf 1998). It is, however, instructive to compare the spending results in the first two rows of table 10.10 with those in rows 3 and 4, in which the dependent variable is the capital component of state- and local-government spending. In these regressions, income and population growth are again associated with greater spending, the difference being that the coefficients on income are now much larger. In the specification without state dummy variables, the estimated coefficient of 0.81 implies that a doubling of state income is associated with 81 percent greater capital expenditures. These results are consistent with the interpretation that governments fail to undertake worthwhile capital projects unless their finances are particularly strong and, in particular, that they cut back on capital projects in bad economic times.[17] Comparing the estimated income coefficients from the capital-spending regressions reported in rows 3 and 4 of table 10.10 with the estimated income coefficients from the equations for noncapital ("direct") spending reported in rows 5 and 6 makes this particularly apparent.

The results reported in table 10.10 are robust to a variety of alternative specifications of the estimating equations. In particular, replacing the change in personal income with the change in state unemployment rate yields results with opposite signs—that is, signs that again imply that state spending increases during strong economic times. Changing the dependent variable to the first-differ-

TABLE 10.10 / State and Local Expenditures and the Business Cycle

Dependent Variable	Δ ln Income	Δ ln Population	State Dummies	R^2
Δ ln total spending	.180	.670	N	.91
	(.045)	(.078)		
	.171	.670	Y	.91
	(.047)	(.098)		
Δ ln capital spending	.807	.870	N	.29
	(.166)	(.286)		
	.758	1.314	Y	.30
	(.173)	(.363)		
Δ ln direct spending	.154	.675	N	.90
	(.047)	(.081)		
	.141	.680	Y	.91
	(.048)	(.102)		

Notes: Data consist of annual observations of spending by a panel of U.S. states (plus the District of Columbia) over the period 1977 to 1997. All regressions have 1,020 observations. Dependent variables are first-differences of logs of indicated spending categories. All regressions include year dummy variables (not reported), and those with Y in the "State Dummies" column also include state dummy variables (likewise not reported). "Δ ln Income" is the first-difference of the log of state personal income, and "Δ ln Population" is the first-difference of the log of state population. Standard errors are in parentheses.

ence of the log of per capita spending (and omitting the population variable on the right-hand side) has only a small effect on the estimated coefficient on the change in log income. Inclusion of state dummy variables, but omission of year dummy variables, changes the results very little. Replacing OLS with a minimum-absolute-distance regression method, which is more robust to outliers, generates similar findings. And weighting the regressions by state population likewise had little effect on the results.

Further evidence of the effect of the business cycle on spending patterns is presented in table 10.11, in which five separate spending categories—health and hospitals, education, highways, police, and parks and recreation—are distinguished. All categories respond positively to personal-income growth, with the estimated short-run income elasticity highest for parks and recreation (0.95 in

TABLE 10.11 / Business-Cycle Effects on Major Spending Categories

Dependent Variable	Δ ln Income	Δ ln Population	State Dummies	R^2
Δ ln spending on health and hospitals	.260	1.296	N	.72
	(.107)	(.184)		
	.207	1.732	Y	.74
	(.109)	(.229)		
Δ ln education spending	.243	.865	N	.84
	(.053)	(.092)		
	.217	1.053	Y	.85
	(.054)	(.114)		
Δ ln highway spending	.415	.879	N	.23
	(.138)	(.238)		
	.383	1.144	Y	.24
	(.144)	(.302)		
Δ ln spending on police	.353	.889	N	.83
	(.073)	(.126)		
	.320	1.081	Y	.83
	(.076)	(.159)		
Δ ln spending on parks and recreation	.949	1.582	N	.67
	(.223)	(.386)		
	.823	2.712	Y	.68
	(.230)	(.484)		

Notes: Data consist of annual observations of spending by a panel of U.S. states (plus the District of Columbia) over the period 1977 to 1997. All regressions have 1,020 observations. Dependent variables are first-differences of logs of indicated spending categories. All regressions include year dummy variables (not reported), and those with *Y* in the "State Dummies" column also include state dummy variables (likewise not reported). "Δ ln Income" is the first-difference of the log of state personal income, and "Δ ln Population" is the first-difference of the log of state population. Standard errors are in parentheses.

the regression without dummy variables capturing state-specific growth trends) and lowest for education (0.24). The result that these spending categories respond positively to economic expansions is robust to replacing the change in income with the change in the state unemployment rate as well as replacing the dependent variables with per capita measures, weighting the regressions, using the minimum-absolute-distance regression method, and other straightforward specification checks.

Appendix table 10A.1 reports the results of reestimating the effect of the business cycle on major spending categories, using changes in state unemployment rates (in place of changes in log income) as indicators of business-cycle movements. As the results reported in the table indicate, higher unemployment rates are associated with reduced spending, the strongest effect appearing for welfare spending.[18] The standard errors for these regressions are rather large, in part reflecting the imprecision of using state unemployment figures in place of personal income as measures of desired demand for public spending. Nevertheless, the patterns are consistent with the procyclic nature of major government-spending programs evinced elsewhere. Other specification checks produced results that are similar to those reported in tables 10.10 and 10.11.[19]

The disadvantaged benefit (along with the rest of the population) from greater government expenditures in these major categories, although the extent to which various groups benefit from the services provided by marginal expenditures is difficult to assess. The evidence on welfare spending reported in table 10.12 suggests, however, that there may be a tendency to direct marginal resources in ways that benefit the disadvantaged.

Of perhaps the most consequence for the disadvantaged are government income-maintenance programs, and it is for this spending category that the results are the most striking and at first glance perhaps the most paradoxical. Table 10.12 presents these regressions. The results reported in the first two rows of table 10.12 indicate that total welfare spending responds positively to changes in local income, with a coefficient implying that a doubling of state personal income is associated with a 134 percent rise in total welfare spending. The relatively large standard errors make it impossible to reject the hypothesis that welfare spending is unaffected by changes in personal income, but the coefficient point estimates are positive. Similar results appear in other specifications, specifically, replacing changes in personal income with changes in state unemployment rates, defining dependent variables in per capita terms, removing year dummy variables, among others.

What makes these results striking is the countercyclic nature of caseloads in major welfare programs such as Aid to Families with Dependent Children (AFDC) since economic downturns are responsible for greater numbers of individuals eligible for, and receiving, welfare payments.[20] In order for total (major-program) welfare spending to respond positively to aggregate income growth, benefit levels per recipient must be strongly affected by the condition of local finances, in combination with the creation of new welfare programs at times when budgets make such initiatives feasible. Is it possible for benefit levels to exhibit such a strong dependence on local economic prosperity?

TABLE 10.12 / Welfare Spending and the Business Cycle

Dependent Variable	Δ ln Income	Δ ln Population	State Dummies	R^2
Δ ln total welfare spending	1.344	2.667	N	.12
	(1.404)	(2.417)		
	1.135	4.661	Y	.12
	(1.468)	(3.075)		
Δ ln spending on cash assistance	2.157	2.139	N	.05
	(1.662)	(2.861)		
	1.975	4.104	Y	.06
	(1.739)	(3.643)		
Δ ln spending on welfare institutions	3.732	1.144	N	.02
	(2.140)	(4.258)		
	3.656	3.039	Y	.04
	(2.295)	(6.361)		
Δ ln spending on categorical assistance	1.246	2.338	N	.11
	(1.592)	(.078)		
	1.113	3.724	Y	.11
	(1.665)	(3.489)		
Δ ln spending on other public assistance	.005	9.775	N	.07
	(3.881)	(6.170)		
	−.767	13.414	Y	.08
	(4.055)	(7.839)		

Notes: Data consist of annual observations of spending by a panel of U.S. states (plus the District of Columbia) over the period 1977 to 1997. All regressions have 1,020 observations. Dependent variables are first-differences of logs of indicated spending categories. All regressions include year dummy variables (not reported), and those with *Y* in the "State Dummies" column also include state dummy variables (likewise not reported). "Δ ln Income" is the first-difference of the log of state personal income, and "Δ ln Population" is the first-difference of the log of state population. Standard errors are in parentheses.

Were these purely cross-sectional regressions performed at an international level, one might not find the results so puzzling. A much smaller fraction of the population is living in poverty in the United States than in, say, India, yet per capita welfare spending in the United States vastly exceeds per capita welfare spending in India. The difference is obviously due to the much greater affluence of the United States and its ability therefore to afford generous transfer programs. Even within the United States, there are persistent differences between states in ability and willingness to pay for income-maintenance programs; these differences tend to be of the form that high-income states with smaller recipient populations nevertheless spend more per capita on income maintenance than do low-income states. The cross-sectional relation for the fifty states and the District of Columbia in 1997 is

$$\ln(\text{total welfare spending}) = \underset{(3.329)}{9.924} + \underset{(0.998)}{1.420}(\ln \text{income}) - \underset{(1.020)}{1.313}(\ln \text{population}), \quad R^2 = 0.06.$$

Further evidence can be obtained by examining categories of welfare expenditures, the determinants of which are estimated in the equations reported in rows 3 to 10 of table 10.12. Cash assistance, welfare institutions, and categorical assistance all appear to respond strongly to changes in state income.[21] In all these cases, the estimated coefficients on changes in log income exceed 1, although in none of these cases is the estimated coefficient significantly different from 0. Nevertheless, the positive sign pattern is striking. Other types of welfare assistance show very little association with state income.

One possibility is that the one-year nature of the first-differences of the spending equations imparts some kind of bias that obscures what might otherwise be the expected negative relation between welfare spending and state income. A long first-difference, covering the years 1977 to 1997 for the fifty states and the District of Columbia, produces

$$\Delta\ln(\text{total welfare spending}) = \underset{(2.869)}{0.232} + \underset{(2.667)}{1.332}(\Delta \ln \text{income}) - \underset{(2.602)}{0.422}(\Delta \ln \text{population}), \quad R^2 = 0.01,$$

which is reassuringly close to the panel results (although the standard errors are quite large).

One is left, therefore, with the impression that strong local economies contribute in a very important way to government spending and, therefore, economic outcomes for the disadvantaged. This analysis is necessarily somewhat imprecise about the magnitude of the associated benefits by income group, and it omits consideration of tax-rate cuts during economic expansions and other ways in which the disadvantaged benefit from strong government finances. But the central fact is that greater affluence is shared through government operations. This implies that the costs of economic downturns are likewise shared, which makes such downturns particularly burdensome for those who stand to lose the most from reduced government activity.

CONCLUSION

The results reported in this paper highlight the value of maintaining strong, steady macroeconomic growth. Although growth does not cure all social problems, it serves to alleviate many of them. Hours worked and family income, in particular, tend to rise when business conditions improve. The real-wage rate, however, is only weakly procyclic, although there is no evidence that wages tend to fall during periods of economic growth. Nevertheless, workers tend to gravitate toward jobs in sectors with steeper seniority-wage profiles when times are good and tend to gravitate toward dead-end jobs when times are bad. The main labor-market lift from a rising economic tide comes about by expanding opportunities to work more hours in better jobs, particularly for lower-paid individuals.

Why does a rising tide provide so many more marginal workers the opportunity to work? The most obvious interpretation is that many of those who are unemployed, out of the labor force, or underemployed are constrained: they would like to work (or work more hours) at the going wage but are unable to find employment. A cyclic upturn increases aggregate demand for labor. It also appears that wage rates are relatively sticky, both during a downturn and during an upturn. Consequently, for the most part, adjustments to employment and hours worked end up clearing the market rather than wages. This simple model of the economy is consistent with the "wage-curve" relations documented in this paper.

Freeman (2001) notes that, despite its benefits, a rising tide has important limits when it comes to reducing poverty. He estimates that, at the end of the 1990s, "close to 60% of adults in poor families were unlikely to be able to benefit much from the labor market" because they were disabled, retired, or had family obligations that prevented them from working enough to lift their families out of poverty. For this group, it is important to emphasize that the public provision of goods and services, including income transfers, also tends to rise with a rising tide. Thus, the potential exists to assist individuals who, for whatever reason, are not in the job market.

Two dark clouds associated with a sunny economy were noted. First, work injuries typically move procyclically—although the expansion of the 1990s is an exception to that pattern. Second, an upturn in the business cycle historically coincides with a slight increase in the high school–dropout rate. The latter tendency suggests that focusing public policy on dropout prevention and skill development—especially among those who are at a high risk of dropping out of high school—during an upturn could make a good deal of sense.

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APPENDIX

TABLE 10A.1 / Effect of the Unemployment Rate on Major Spending Categories

Dependent Variable	Δ Unemployment	Δ ln Population	State Dummies	R^2
Δ ln spending on health and hospitals	−.692	1.280	N	.15
	(.309)	(.192)		
	−.795	2.169	Y	.20
	(.309)	(.291)		
Δ ln education spending	−.149	1.220	N	.27
	(.160)	(.092)		
	−.206	1.771	Y	.30
	(.161)	(.152)		
Δ ln highway spending	−.747	1.452	N	.17
	(.430)	(.266)		
	−.769	2.119	Y	.19
	(.438)	(.413)		
Δ ln spending on police	−.439	1.418	N	.21
	(.222)	(.138)		
	−.487	1.933	Y	.23
	(.226)	(.213)		
Δ ln total welfare spending	−2.106	.476	N	.09
	(4.507)	(2.803)		
	−1.853	.565	Y	.10
	(4.644)	(4.378)		

Notes: Data consist of annual observations of spending by a panel of U.S. states (plus the District of Columbia) over the period 1977 to 1997. All regressions have 1,020 observations. Dependent variables are the first-differences of logarithms of indicated spending categories. All regressions include year dummy variables (not reported), and those with *Y* in the "State Dummies" column also include state dummy variables (likewise not reported). "Δ Unemployment" is the first-difference of the unemployment rate (where the unemployment rate is measured as a proportion rather than a percentage), and "Δ ln Population" is the first-difference of the log of state population. Standard errors are in parentheses.

NOTES

1. Although, according to the National Bureau of Economic Research, the recession officially ended in March 1991 *(http://www.nber.org/March91.html)*, the unemployment rate did not peak until 1992. Consequently, we use 1992 as the trough year.
2. Specifically, we use the March CPS for survey years 1976 to 2000, which provides annual labor-market information for the following calendar year.

3. If cycles were symmetrical, in that the increase in unemployment rates in the recession equaled the decrease in unemployment rates in the expansion, then this estimated asymmetrical effect of unemployment rates would suggest that, after repeated cycles, average annual hours would be lower than at the beginning of the period. However, we are not observing such a steady-state economy; instead, we are looking at a finite slice of time, and, during this time period, unemployment rates have been trending downward.
4. Some standard adjustments to the data are implemented. The earnings data are top coded at $50,000 through 1981, $75,000 from 1982 to 1984, $100,000 from 1985 to 1988, and about $200,000 from 1989 on. Following Lawrence Katz and Kevin Murphy (1992), the earnings of top-coded individuals are adjusted to be 1.45 times the top-coded value. Beginning in 1996, instead of giving each top-coded observation the value of the top code, the CPS assigns the mean among the sample of top codes (by demographic group). The earnings figures can be as high as $600,000 in this period. We make no adjustment for top coding in these years. There is no apparent top coding of family earnings or family income. Real earnings and income are constructed using the deflator implied by the CPI-UX1.
5. The nonwhite group includes both blacks and white Hispanics.
6. The Federal Reserve provides an aggregate measure of only the capacity-utilization rate, so we cannot use this variable in the MSA-level analysis.
7. We also estimated models that included MSA linear time trends and unrestricted demographic group times year effects. Although including these variables improved the model fit considerably, they consistently had no significant effect on the estimated unemployment-rate effects.
8. Note that, for wages, earnings, and income, we use the change in the log of the mean outcome. It would be more consistent with an underlying individual model to take the mean of the log of the measure. However, since we are not, in general, conditioning on working, we cannot take the mean of log income or earnings owing to the prevalence of zeros.
9. For comparison to Solon, Barsky, and Parker (1994), earnings were deflated by the GNP deflator. Hourly earnings were derived as the ratio of annual labor income to annual hours worked. Individuals with assigned earnings or hours data were eliminated from the sample. Unlike Solon, Barsky, and Parker, we windsorized the hourly wage data (that is, rolled extreme values back) at $2.13 and $100 per hour in 1996 dollars and used sample weights to adjust for the low-income oversample; our results were not very sensitive to these changes.
10. When we tested the wage-curve specification against a Phillips curve specification, the PSID data preferred the wage curve; that is, if we include the current unemployment rate in the equations in table 10.6, it is statistically insignificant, while the change remains significant (in column 1).
11. For example, for column 1 of table 10.6, Solon, Barsky, and Parker find a coefficient of −.0135, and we find −.0129, and, for column 2 of table 10.7, we both find −.0140. The standard errors are also close.

12. If we use the log of the mean hourly wage (as was done in the previous section) in th model in column 1 of table 10.7 instead of the mean of the log hourly wage, th results are quite similar.
13. The coefficient (standard error) in the earlier period is 0.0046 (0.0054) and in the lat period is −0.0033 (0.0063). The estimates for the unbalanced sample are similarl insignificant, as are estimates that use GNP growth as the cycle variable.
14. To check the robustness of our results, we also used the 1985 wave of the PSID an the May 1979 CPS Pension Supplement to estimate the tenure profiles. Our finding were qualitatively similar when the 1985 PSID sample was used but much less sy tematic when the CPS data were used.
15. Data reported in table 10.9 represent fiscal years, which for almost all states run fro July 1 to June 30. Personal disposable income in the United States in the last tw quarters of 1995 plus the first two quarters of 1996 was $5.398 trillion. Figures in tab 10.9 are nominal, and therefore not adjusted for inflation.
16. See, for example, the estimated income coefficients in Anne Case, James Hines, ar Harvey Rosen (1993), James Poterba (1994), and the studies surveyed by Dani Rubinfeld (1987). It is for this reason that we use annual-income growth, rather th unemployment changes, as a measure of business-cycle conditions.
17. See the evidence provided in Poterba (1994, 1995).
18. Unlike in the preceding tables, the unemployment rate in these regressions is me sured as a proportion, so a value of 0.01 corresponds to a 1 percentage point rise state unemployment. Consequently, the estimates in the first column of table 10 imply that a 1 percentage point rise in state unemployment (for example, the une ployment rate rising from 2.5 to 3.5 percent) corresponds to declines of 0.0069 perc in health and hospital spending, 0.0075 percent in highway spending, and 0.02 p cent in welfare spending.
19. Other specification checks included adding additional lags of personal-inco changes to the main regressions and (separately) distinguishing large from small p sonal-income changes to test for asymmetrical reactions over the business cycle. consistently new patterns emerged from adding additional lags of personal-inco changes to the main regressions, although estimated coefficients on lagged-inco terms were often significant. When the sample is divided in half according to the s of income changes and the regressions run separately on each subsample, the resu are similar to those reported for the whole sample. The estimated income coefficie tend to be somewhat larger for the sample with observations with slow inco growth, suggesting that business-cycle downturns have a greater effect on the e mates than do periods of rapid economic growth, but the differences are not sign cant in a statistical sense.
20. In this context, it is noteworthy that AFDC and Transitional Assistance for Ne Families (TANF) represent relatively small fractions of total welfare spending by s and local governments. In fiscal year 1994, state and local governments spent $ billion on AFDC, representing 12 percent of their total cash and noncash payments $98.6 billion) to persons with limited income. AFDC spending was roughly equa the sum of total state and local spending on foster care, general assistance, and s

plemental security income (Burke 1996). Hence, business-cycle influences on AFDC expenditures might have a modest influence on total welfare spending by state and local governments.

21. Welfare institutions consist of the provision, construction, and maintenance of nursing homes and welfare institutions owned and operated by state and local governments for the benefit of needy persons; examples include public nursing homes, orphanages, homes for the elderly, and indigent-care institutions. Categorical-assistance programs include direct payments to beneficiaries under the supplemental security income program, AFDC or TANF, and Medicaid.

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