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The Reversal of the Employment-Population Ratio in the 2000s: Facts and Explanations

ABSTRACT The decline in the employment-population ratios for men and women over 2000–07, just before the Great Recession, represents a historic turnaround in U.S. employment trends. The decline is disproportionately concentrated among the less educated and younger groups within the male and the female populations and, for women, especially among unmarried women without children. About half of the decline among men can be explained by declines in wage rates and by changes in nonlabor income and family structure, but the decline among women is more difficult to explain and requires distinguishing between married and unmarried women and between those with and without children, as these subgroups have experienced quite different wage and employment trends. Neither changes in taxes nor changes in government transfers appear likely to explain the employment declines, with the possible exception of the Supplemental Nutrition Assistance Program. Other influences such as the minimum wage and health factors do not appear to play a role, but increases in incarceration may have contributed to the decline among men.

There are many indicators of trends and cycles in the labor market. The unemployment rate is the primary indicator used in analyzing cyclical changes, but for long-term trends the employment-population ratio is the best indicator of the quantity of labor supplied. When one compares one cyclical peak with the next, thus holding the unemployment rate more or less fixed, the employment-population ratio necessarily reflects the labor force participation rate, which is the common measure of labor supply. Long-term trends in the employment-population ratio can therefore likewise be taken as reflecting trends in labor supply.

This study examines the decline in the employment-population ratio from 2000 to 2007, just before the Great Recession began. The ratio for the overall working-age population (that is, for both men and women aged 16–64) stood at 74.1 percent in 2000 and at 71.8 percent in 2007. The decline was greater among the younger and less educated of both sexes. This drop in the ratio represents a historic reversal from its upward trend over the previous 30 years and hence constitutes a major change in the U.S. labor market.

The employment-population ratio has been much discussed recently, both in the press and among researchers and policymakers, because it underwent a further, even sharper decline during the Great Recession, falling 9.0 percentage points to 65.8 percent at its low point in January 2010 (several months after the official trough), a tremendous decline by historical standards.[1] It has recovered only slowly since then, to about 67 percent in 2011. Behind this trend is a decline in the labor force participation rate—a contribution to the decline in the unemployment rate but not a particularly welcome one.

The factors already at work in the decline in the employment-population ratio before the Great Recession may in part explain this slow recovery since. Indeed, James Stock and Mark Watson (2012) predict that, should the long-term downward trend in the ratio continue, future recessions are likely to be deeper and future recoveries slower. More immediately, if the long-term decline continues, the employment-population ratio may not return to its 2007 value even when the recovery is judged complete.

The reversal of the employment-population ratio in the 2000s has received little formal study. In a session at the American Economic Association meetings in January 2012, Henry Farber reported his finding that changes in the age-sex-education composition of the population could explain no more than a quarter of the decline, and Robert Shimer, noting the greater rate of decline among youth, speculated that rigid wages or intertemporal substitution between the pre- and post-2000 periods could be partly responsible.[2] David Autor (2010) finds that changes in the ratio over 1979–2007 as well as over the subperiod after 2000 are positively correlated with changes in wages, suggesting a conventional labor supply explanation. Diane Macunovich (2010) finds a significant decline in female labor supply from 1999–2001 to 2007–09, particularly among unmarried women without children, but also finds that conventional explanatory variables (wage rates, number of children per household, and others) account

1. Many public discussions cite figures including the population 65 and over. For this larger population, the ratio fell from 63 percent to 58 percent over the same period.

2. Video of the session is available at www.aeaweb.org/webcasts/2012/index.php.

for very little of this change. Stephanie Aaronson and others (2006) examine the aggregate labor force participation rate through 2005, finding that demographic, cyclical, and structural factors probably contributed to the recent downturn in that rate.

Trends in the labor supply of women have been extensively studied. The recent literature has noted that although female labor supply has historically exhibited strong growth, that growth slowed in the 1990s, prompting some observers to ask whether it has plateaued (Goldin 2006). Discussions of the slowdown have mainly focused on whether wage elasticities of labor supply and other coefficients in female labor supply equations have changed over time and are responsible. Francine Blau and Lawrence Kahn (2007) find that the wage elasticity for married women declined noticeably from the 1980s to the 1990s, bringing it closer to that for men. More relevant to the post-2000 period are studies such as those by Kelly Bishop, Bradley Heim, and Kata Mihaly (2009), Heim (2007), and Macunovich (2010), who examine whether wage elasticities were falling after 2000. Among these studies, those whose sample period ends in 2002 or 2003 find falling wage elasticities, whereas the one study (Macunovich 2010) that ends in 2007–09 finds a slight increase after 2000. Complicating inference from these studies is that in each case the ending year was at a different point in the business cycle than the beginning. More relevant for present purposes is whether trends in one aspect of labor supply—the employment-population ratio—can be explained by changes in observed variables rather than changes in coefficients.[3]

Another strand of the literature for women has focused on a so-called opt-out revolution among well-educated and professional married women, whose labor force participation rates fell in the 2000s.[4] This line of argument speculates that more-educated women are increasingly deciding to stay at home to engage in childrearing rather than engage in market work. Some research has investigated this hypothesis, but very little attempts to search specifically for variables that might have caused the decline (Antecol 2011, Bousey 2008, Macunovich 2010). Claudia Goldin (2006) notes that it may take several years to see whether recent cohorts of

3. As noted above, Macunovich (2010) finds that little of the change for women through 2007–09 could be explained by observable variables. Hotchkiss (2006), using a model without wages in the labor supply equation, likewise finds that observables could explain little of the change in female labor force participation through 2005.

4. Claudia Wallis, "The Case for Staying Home," *Time,* March 22, 2004. www.time.com/time/magazine/article/0,9171,993641,00.html (accessed August 5, 2012).

more-educated women exhibit opt-out patterns over the remainder of their working lives.

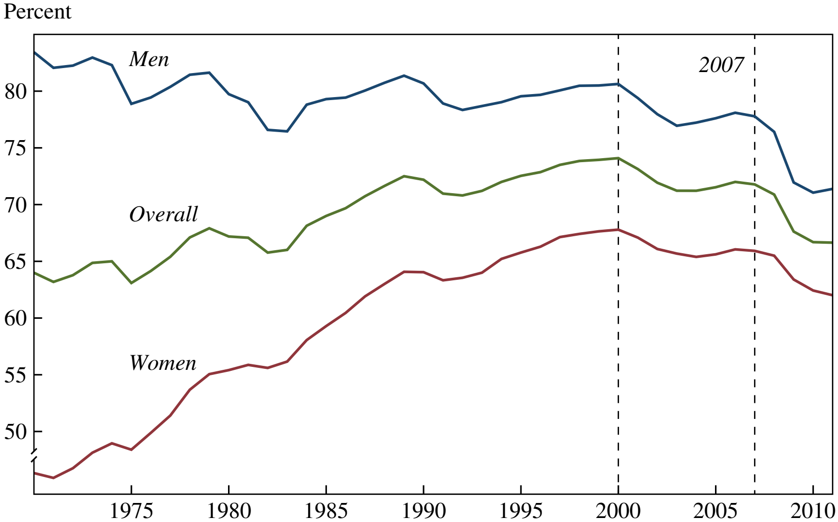
In this paper I conduct an analysis of the decline in the employment-population ratio through 2007, with two parts. First, I describe in detail the patterns of this decline, including those by time period as well as by demographic group (as defined by age, sex, education, and race) and other characteristics. This analysis reveals that the decline is disproportionately concentrated among the young and the less educated of both sexes. The decline is particularly strong among unmarried women without children. Second, I conduct an investigation into the proximate causes of the decline. About half of the decline of the male employment-population ratio can be explained by declines in wage rates and changes in nonlabor income and family structure. The factors responsible for the decline in the ratio among women are more difficult to explain and require separate examination of wage and employment trends for married and unmarried women and for those with and without children, as these subgroups exhibit different patterns of employment and wage change. I also find that neither changes in taxes nor changes in government transfers appear likely to explain the employment declines, with the possible exception of the Supplemental Nutrition Assistance Program (the food stamp program), nor do other influences such as the minimum wage or health factors.

I. Trends and Patterns

The Bureau of Labor Statistics (BLS) publishes statistics on the employment-population ratio drawn from the monthly interviews of the Current Population Survey (CPS), which asks all respondents aged 16 and over about their employment status during the week preceding the interview. The middle line in figure 1 shows the trend for the civilian noninstitutional population aged 16–64 from 1970 to 2011.[5] The trend in the ratio was positive, with intermittent cyclical variation, from 1970 to about 1999 or 2000. At that point it reversed course and began the decline that is the object of interest here. As noted in the introduction, the ratio declined by over 2½ percentage points between 2000 and 2007, then plummeted as the Great Recession began. The departure from the historical trend is dramatic and clear from the figure.

5. This study does not examine those over 64, whose labor supply decisions are likely driven by factors other than those that influence the working-age population. The employment-population ratio for the elderly increased over the period.

Figure 1. Employment-Population Ratios, Overall and by Sex, 1970–2011

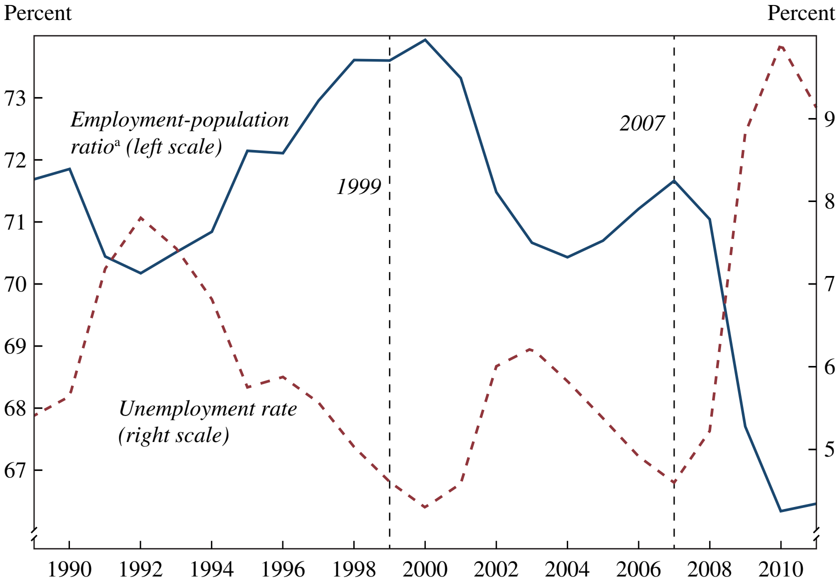


Source: Bureau of Labor Statistics.
a. Data refer to the civilian noninstitutional population aged 16–64.

The trend in the overall ratio masks quite different trends by sex, as the figure also shows. The ratio for men declined, on average, between 1970 and 1983, after which it remained stable until 2000, when it began a further decline. Its decline from 2000 to 2007 was 2.7 percentage points. The ratio for women, in contrast, secularly increased from 1970 to 2000, consistent with the well-known trend growth of female employment over this period. After 2000 it stopped growing and declined slightly, falling by 1.7 percentage points by 2007. The decline was therefore smaller in magnitude for women than for men, but the deviation from the pre-2000 trend was greater.

This study will focus on the period 2000–07, as compared with that of the 1990s, and will investigate possible causes of the reversal of the trend in the employment-population ratio from the first period to the second. An immediate issue in such an investigation is whether to attempt to explain both the trend and the cycles in the ratio, for it is clear from figure 1 that the ratio behaves procyclically. Here the focus will be on the trend and not the cycle, at least to the extent possible. To this end I select as endpoints years when the economy was roughly at the same point (the peak) in the cycle.

Figure 2. Employment-Population Ratio and Unemployment, 1989–2011



Source: Bureau of Labor Statistics.
a. Data refer to the civilian noninstitutional population aged 16–64, male and female.

Figure 2 traces both the unemployment rate and the overall employment-population ratio since 1989. The unemployment rate in 2007 stood at 4.60 percent in March 2007, and in the previous expansion it came closest to this rate in March 1999 (4.61 percent).[6] Therefore, I focus on the change in the ratio between those points in time, a period that exhibits the same magnitude of decline as discussed above for 2000–07 (2.7 percentage points for men and 1.7 percentage points for women). For the period of the late 1980s and the 1990s, the lowest March unemployment rate was recorded in 1989, when it stood at 5.41 percent (it was even higher for all earlier years in the 1980s), somewhat higher than in March 1999. Never-

6. These figures differ slightly from BLS figures for the population aged 16–64 because they are computed on the sample used for model estimation below, which has some exclusions. Also, it is worth noting that the natural rate of unemployment as estimated by the U.S. Congressional Budget Office (2012) was exactly the same in all four quarters of 1999 and 2007.

theless, I take the period from March 1989 to March 1999 as illustrating the trend over the 1990s. Over that period the employment-population ratio for men fell by a modest 0.9 percentage point and that for women rose by 3.6 percentage points.[7]

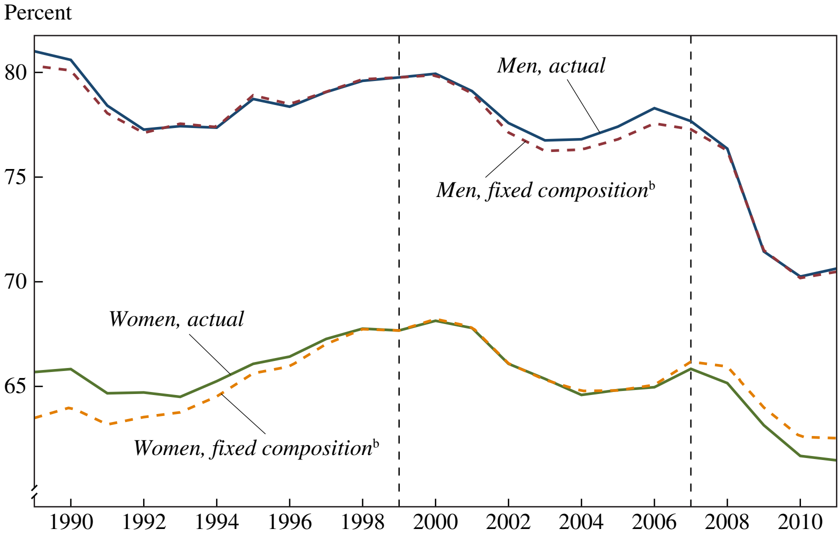
Movements in the overall employment-population ratio can result either from shifts in the demographic composition of the population or from shifts in the ratios for one or more such groups. Although shifts in composition are likely to be more important over periods longer than those studied here, they could also be of some importance over the 1989–2007 period and could affect the interpretation of the trends in the aggregate ratio I have thus far shown. I therefore briefly analyze the overall ratio, looking for shifts in overall composition before turning to a more thorough analysis of shifts within demographic groups. For this exercise I use the March CPS in each of the years 1989 to 2007, which collected information on the employment and labor force status of all individuals 16 and over as well as their age, level of education, race, and sex. Classifying the population into four age groups (16–24, 25–39, 40–54, and 55–64), four education groups (less than a high school diploma, high school graduates, some college, college degree or more), and three race groups (white, black, and other) allows a determination, using a standard shift-share decomposition, of how the proportions of the population in the resulting 48 demographic groups for each sex affected their aggregate employment-population ratio trends.[8] Figure 3, which plots for each sex both the actual ratio and the ratio holding composition constant at its 1999 value, shows that only small fractions of the changes in the ratios were a result of changes in composition. Slight compositional changes are observed for men during the early 1990s and during the 2000s downturn, and a somewhat larger but still small change is seen for women from 1989 to 1999.

Having established that most of the decline in the employment-population ratio from 1999 to 2007 was not a result of changes in composition, I next use the March CPS to describe the patterns of the decline in the ratio by demographic characteristic. The first and third panels

7. Again, some of the studies mentioned in the introduction studied labor supply trends through 2002, 2003, or even 2005. Clearly the unemployment rate was much higher, and the employment-population ratio much lower, in those years, but partly for cyclical reasons. As noted before, this makes it difficult to draw inferences about trends from those studies.

8. The decomposition used is $y_{t+1} - y_t = \sum_g p_{gt}(y_{g,t+1} - y_{gt}) + \sum_g (p_{g,t+1} - p_{gt})y_{g,t+1}$, where y_{gt} is the employment-population ratio for group g in year t, p_{gt} is the proportion of the population in group g in year t, and groups $g = 1, \ldots, 48$ are the demographic groups. A decomposition using weights in the other years yields almost identical results.

Figure 3. Employment-Population Ratios, Actual and with Fixed Demographic Composition, 1989–2011[a]



Source: Bureau of Labor Statistics and author's calculations.

a. All series refer to the civilian noninstitutional population aged 16–64.

b. Ratio that would have prevailed if the composition of the population by age, education, and race had remained constant at 1999 proportions throughout the period.

of table 1 show the patterns of change from 1999 to 2007 by age, education, and sex, using the same age and education categories used for the composition exercise.[9] The magnitudes of the changes vary greatly across the cells, but some patterns can be detected. Reading down the columns, one observes that the largest employment-population declines occurred, with some exceptions, among those under 40 years old, and that the decline was more monotonic for women than for men. Among those under 40, the declines were usually sharper for those under 25. Reading across the rows, one also notes a correlation with education, with declines generally larger for those with no college than among those with at least some college. Those who were both young and less educated generally experienced the largest declines (for example, over 4 percentage points). On the other hand, declines in the ratio, even if

9. Standard errors are very small and not shown. The sample size per cell is never less than 400 and ranges up to 7,500, with most in the 1,500-to-4,000 range.

Table 1. Changes in Employment-Population Ratios by Sex, Age, and Education, 1999–2007 and 1989–99[a]
Percentage points

	Education			
Age	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*
		Men		
1999–2007				
16–24 years	−7.9	−4.1	−0.9	−3.7
25–39	−0.4	−3.6	−2.3	1.0
40–54	−3.7	−2.6	−0.7	−0.2
55–64	−1.6	−3.6	−2.3	0.5
1989–99				
16–24 years	−3.7	−1.4	−1.5	3.1
25–39	1.2	0.5	−0.7	−1.1
40–54	−2.6	−3.2	−3.8	−1.2
55–64	−2.1	−2.3	0.5	1.7
		Women		
1999–2007				
16–24 years	−7.7	−7.4	−1.8	−4.3
25–39	−5.7	−4.2	−1.9	−3.0
40–54	1.6	−0.4	−1.8	−1.9
55–64	3.2	2.9	4.2	7.0
1989–99				
16–24 years	1.0	4.1	−1.5	−0.0
25–39	7.1	2.2	2.4	−0.1
40–54	1.0	1.2	1.9	2.7
55–64	0.6	4.4	3.8	2.5

Source: CPS data and author's calculations.
a. CPS data are weighted using the CPS Basic Weight.

smaller in magnitude, are also often observed for those aged 40–54 and for those with a college degree or more, in the latter case particularly for women (perhaps consistent with the opt-out revolution). Thus, the decline did not occur exclusively among the young and less educated.[10]

The patterns for 1989–99 are different, as should be expected. For men the ratio generally declines, but for most subgroups this decline is smaller

10. Separate tabulations by full-time and part-time status show that essentially all of the decline for men came from those transitioning from full-time work to no work, whereas the decline for women was roughly equally split between moves from full-time and from part-time work.

Table 2. Changes in Rates of School Attendance among 16- to 24-Year-Olds, by Sex and Education, 1999–2007 and 1989–1999[a]
Percentage points

	Men		*Women*	
Education	*1999–2007*	*1989–99*	*1999–2007*	*1989–99*
No high school diploma	0.9	4.7	3.6	5.9
High school graduate	1.5	–5.5	4.6	–2.9
Some college	–1.0	–1.3	1.8	5.5
College degree or more	–0.3	2.1	6.0	3.3

Source: CPS data and author's calculations.
a. CPS data are weighted using the CPS Basic Weight.

in magnitude than for the 1999–2007 period, and there is a slight tendency for the difference to be greater for the younger and the less educated. For women the contrast is greater, with almost all categories showing positive trends in the ratio in this period. The difference in trends is particularly strong for younger and less educated women.

Comparisons by race (appendix table A.1) show roughly the same patterns of decline for whites, blacks, and those of other races. The magnitudes vary considerably across racial groups, but the smaller sample sizes for some categories may play a role. Some of the largest declines are seen among black men and women, but for many age-education groups they are smaller than for white men and women than for blacks.

For the very young, some of the declines in employment may simply reflect increases in school attendance. The CPS asks its respondents aged 16–24 who report that they are not employed whether they are attending school. Table 2 shows increases in school attendance from 1999 to 2007 for men with a high school diploma or less and for all women. However, with only a couple of exceptions, the increases are smaller than during the 1989–99 period.

Some of the papers referenced in the introduction note the importance of marital status for labor supply trends, especially those of women, and the analysis below will also find major differences with respect to marital status. The employment-population ratio declined over 1999–2007 by 1.6 percentage points for married men but by almost double that, 2.9 percentage points, for unmarried men. For women the contrast was even greater: the ratio declined by only one-third of a percentage point for married women but by 2.9 percentage points for the unmarried. Thus, for both sexes, the majority of the decline was among the unmarried, not the married.

Table 3. Changes in Employment-Population Ratios by Sex, Marital Status, Age, and Education, 1999–2007[a]
Percentage points

	Education			
Age	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*
		Men		
Married				
16–24 years	−6.8	−3.9	−3.4	−12.8
25–39	0.6	−1.9	−0.2	−0.2
40–54	−2.3	−1.0	−1.0	0.1
55–64	−0.9	−3.3	−2.0	−0.4
Unmarried				
16–24 years	−7.3	−3.5	−0.2	−2.1
25–39	−1.5	−4.7	−4.2	2.7
40–54	−4.6	−4.6	0.5	−0.6
55–64	−0.6	−2.2	−0.7	6.7
		Women		
Married				
16–24 years	−4.6	−11.1	1.0	−0.9
25–39	−6.1	−4.4	0.6	−2.9
40–54	3.6	1.0	−1.3	−1.9
55–64	4.8	1.1	3.3	7.7
Unmarried				
16–24 years	−7.9	−6.9	−2.1	−5.2
25–39	−4.7	−5.0	−6.0	−2.3
40–54	−1.4	−3.4	−2.6	−1.8
55–64	1.4	6.2	6.0	5.3

Source: CPS data and author's calculations.
a. CPS data are weighted using the CPS Basic Weight.

Table 3 shows the patterns of decline by marital status for each age-education category. From 1999 to 2007, married men's employment-population ratios still declined more for the youngest (16–24) and less educated groups, but the ratios for unmarried men declined more for older, less educated men. For women, although the relatively greater declines are concentrated in the younger and less educated groups among both the married and the unmarried, they are almost always considerably greater for the latter. An additional finding (not shown in the tables) is that the greater declines for unmarried women are concentrated among those without children, for whom the ratio declined by 3.5 percentage points between 1999 to 2007, compared with only 0.4 percentage point among unmarried women with children.[11] Unmarried women without children constitute about one-third of all women aged 16–64.

II. Labor Supply Models and Evidence

The workhorse model in labor economics for explaining changes in individual employment and hours of work has been the static labor supply model. In that model, enshrined in most labor economics textbooks, individuals choose whether to work at all, and how many hours to work, as a function of the market wage rate they face and the amount of nonlabor income available to them. The theoretical effect of the market wage rate on hours of work is ambiguous in sign, but that on the decision whether to work at all is unambiguously positive, whereas the predicted effect of nonlabor income on both hours and the decision to work is negative.

The empirical literature on the model is vast. Mark Killingsworth (1983) exhaustively reviewed the literature from the 1960s and 1970s; Richard Blundell and Thomas MaCurdy (1999) and Costas Meghir and David Phillips (2010) have conducted updated reviews. Unfortunately, the bulk of this literature focuses on hours of work and not on the employment decision. For hours of work, the conventional wisdom from this literature is that wage elasticities are zero or negative for prime-age men and significantly positive for women, and that income elasticities are negative for both, and greater in magnitude for women, but often not very large for either. The conclusions for men have been challenged over the years, for example, by Chinhui Juhn, Kevin Murphy, and Robert Topel (1991), and most recently by Michael Keane (2011) and by Keane and Richard Rogerson (2012). The latter study argues explicitly that wage elasticities are likely higher for the employment decision (what the authors call the "extensive margin") than for the hours decision (the "intensive margin") and are very important for the aggregate labor supply elasticity (see also Rogerson and Wallenius 2009). For women, it has long been recognized that the extensive margin is particularly important; this finding goes back to early labor supply work that separated it from the intensive margin (Mroz 1987). Meghir and Phillips (2010) also examine wage elasticities for labor force participation and find them to be larger for women than for men, but not that large even for women. It is also well known that the increase in labor supply of women over time has been particularly strong on the extensive margin.

11. Again, Macunovich (2010) found the same result.

Another literature of relevance is that on separating demand from supply influences on trends in wage differentials among men and women (Katz and Autor 1999, Acemoglu and Autor 2011). Although this literature is rarely referenced in the labor supply literature, its main focus on the correlation between wage changes and "quantity" changes—most often measured by total hours of work in a skill group—has implications for wage elasticities of labor supply. The main conclusions from that literature are that the last four or five decades have seen a trend-like expansion of the relative demand for more-skilled workers, and that with the exception of the 1970s, relative supply has shifted outward only modestly—and may even have shifted inward. This conclusion is based on the general finding of a positive correlation of wage changes with hours changes across education and experience groups, implying a positive wage elasticity of labor supply, even for men. A recent paper focusing just on the employment-population ratio within the same framework (Autor 2010) reaches the same conclusions for that ratio, finding a positive correlation with changes in wages both over 1979–2007 and over the 2000s alone.

The empirical literature on the standard labor supply model has reached many other general conclusions as well. For married women, it has been established that the husband's earnings are an important factor in her labor supply decision (Blau and Kahn 2007). The presence of young children, which tends to depress the labor supply of women, is also important, as is marital status, with unmarried women tending to work more. For men, marital status is also correlated with labor supply (at least as measured by hours of work), with married men working longer hours. The presence of young children is generally found to have less of an impact, if any, on the labor supply decisions of men than of women.

A related but important literature focuses on the impact of taxes and transfer programs on labor supply. The early literature on the effect of taxes was covered by Killingsworth (1983), and the later literature by the reviews of Blundell and MaCurdy (1999) and Meghir and Phillips (2010). All of these studies concluded, to varying degrees, that responses to changes in taxation were consistent with the literature on labor supply in general: very modest for prime-age men and somewhat larger for women.[12]

12. A related literature is that examining the effects of taxes on taxable income. See the original contribution by Feldstein (1995), the recent review by Saez, Slemrod, and Giertz (2012), and the recent contribution of Romer and Romer (2012). Moffitt and Wilhelm (2000) apply the methodological framework initially developed by Feldstein to hours of work.

This view has been challenged recently by Keane (2011), who argues that properly specified life cycle models that incorporate returns to human capital imply larger wage elasticities. A similarly large literature focuses on the different transfer programs. My own review of the early literature (Moffitt 1992) found rather significant responses of single-mother labor supply to the availability of benefits from the Aid to Families with Dependent Children (AFDC) program, and research on later reforms of that program shows even larger responses (Grogger and Karoly 2005). But my review found very small effects of most other means-tested transfer programs, and a more recent review (Ben-Shalom, Moffitt, and Scholz forthcoming) is consistent with this view. The literature reaches less of a consensus on the effects of social insurance programs: very divergent estimates of the effects on work incentives of the Social Security retirement program, the Social Security disability insurance program, and unemployment insurance (UI) have been reported. The effects of UI have figured prominently in the discussion of the Great Recession, but not as much in the discussion of earlier labor supply trends.

III. Influences on Labor Supply: Wages, Other Income, and Demographics

The approach taken here in exploring the various influences on labor supply is to first examine the traditional determinants appearing in the literature—wages and nonlabor income, but supplemented with demographic determinants (marital status, presence of children, and others)—to determine whether they can explain the reversal of the trend in the employment-population ratio from 1999 to 2007 relative to 1989–99, including the patterns by age-education subgroup identified above. Section IV considers the effects of taxes and transfers. The primary data source for the analysis is again the March CPS data from 1989, 1999, and 2007, which come from random samples of approximately 145,000, 132,000, and 206,000 individuals, respectively. The household interviews collected information on all individuals aged 16 and over, from whom I select only those between the ages of 16 and 64. In addition to information on employment status in the survey week, which is used to construct a dichotomous variable for whether an individual is employed, and on demographic characteristics, I collected information on earnings and weeks of work in the calendar year before the interview week as well as on all forms of nonlabor income and other labor income received by members of the family in that prior year.[13]

The modeling approach is kept as simple as possible to increase transparency. Observations on individuals from the three yearly surveys are pooled into one data set, and ordinary least squares (OLS) regressions are estimated explaining employment status as a function of wages, nonlabor income, and demographic variables (probit regressions are also tested). Whether changes in those variables can explain the changes in the employment-population ratio from 1989 to 1999, and from 1999 to 2007, is the question then addressed, not only for aggregate changes in the ratio but also for the pattern of age-education changes shown in table 1. All equations are estimated separately by sex.

A difference between this study and much of the recent work on female labor supply referenced in the introduction is that the coefficients in the employment status regression are held fixed for all three years rather than allowed to change from year to year. In the literature, separate equations are often estimated by year, and then the change in labor supply (more often hours of work than employment status, however) from one year to the next is decomposed into the portion that can be explained by changes in the variables in the regression and the portion explained by the rest—changes in the coefficients on the variables and in the intercept. Here, because the focus is only on the former portion, constant coefficients are imposed.

The equation estimated on the pooled data for each sex can be written as follows:

$$E_{it} = V_{it}\gamma + X_i\beta + \varepsilon_{it}, \tag{1}$$

where E_{it} is a dummy variable equal to 1 if individual i in year t ($t = 1989$, 1999, or 2007) was employed and zero if not, V_{it} is a vector of variables (wages, nonlabor income, family structure) that change over time and whose explanatory power is being assessed, X_i is a vector of age-education-race dummy variables treated as fixed effects, and ε_{it} is an error term. The predicted change in the employment-population ratio between year t and

13. Following most of the literature, I exclude individuals in group quarters and those with zero weights. All analyses are weighted. The number of observations in the male sample, pooled over all three years, is approximately 120,000; that for females is approximately 129,000.

year $t + 1$ is therefore $[V_{t+1}(X_i = x) - V_t(X_i = x)]\gamma$ for age-education-race group x, and the predicted change for the population as a whole is the weighted sum of these changes over all age-education-race groups. This fixed-effects model is equivalent to a first-differenced model, although estimated on individual rather than grouped data. The predictions can be compared with actual changes in the employment-population ratio by group and overall.

III.A. Wages

The CPS interview asks respondents to report earnings, weeks worked, and average hours of work per week in the preceding year. The last of these variables is particularly prone to measurement error and leads to the well-known problem of "division bias," so I instead compute weekly wages by dividing earnings by weeks worked.[14] The main results use weekly wages of all workers, but analyses reported in the appendix use the wages of full-time, year-round (40 or more weeks per year, 35 or more hours per week) workers only, as a further test of whether variation in hours worked or weeks worked affects the weekly wage estimates (many other studies, such as Acemoglu and Autor 2011, also use this measure). Persons in group quarters, the military, the self-employed, and those with allocated earnings are excluded from the wage sample, again as in the studies just referenced.[15] Weekly wages are expressed in 2007 dollars using the personal consumption expenditures (PCE) deflator.

Table 4 shows changes in the logarithm of the real weekly wage by age and education for men and women, for comparison with the employment-population changes shown in table 1. For men, these changes are roughly positively correlated with employment changes from 1999 to 2007, but considerably less of a relationship is observed from 1989 to 1999. However, there is also a positive relationship between the difference in wage changes across the two periods and the difference in employment changes,

14. The division bias problem is presumably less important here because hours of work are not used as the dependent variable. Nevertheless, measurement error in hours worked could be correlated with the error term in the employment equation. I report below how the results change when hourly wages are used.

15. Allocated earnings values in the data are values that are imputed by the Census Bureau in cases where earnings are missing or have implausible values. The exclusion of those with allocated earnings makes no difference to the results. In addition, following Acemoglu and Autor (2011, p. 1162), I trim weekly wages at top and bottom, both to eliminate outliers and to eliminate those affected by top coding. However, rather than trim at fixed real weekly wage values for all years, as they do, I trim the top and bottom 5 percent of the distribution. All wage regressions are estimated using March CPS Supplement weights.

Table 4. Changes in Log Real Weekly Wages by Sex, Age, and Education, 1999–2007 and 1989–99[a]
Log points

	Education			
Age	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*
		Men		
1999–2007				
16–24 years	–0.014	–0.027	0.007	–0.036
25–39	0.003	–0.028	–0.037	0.018
40–54	–0.035	–0.018	0.003	0.075
55–64	–0.030	–0.010	–0.018	0.004
1989–99				
16–24 years	0.117	0.076	0.031	0.188
25–39	0.003	0.023	0.027	0.137
40–54	–0.004	–0.031	–0.004	0.101
55–64	–0.039	0.016	–0.019	0.138
		Women		
1999–2007				
16–24 years	–0.111	0.033	–0.008	0.036
25–39	0.038	0.028	0.071	0.043
40–54	0.049	0.047	0.048	0.088
55–64	–0.105	0.117	0.022	0.160
1989–99				
16–24 years	0.174	0.095	–0.009	0.075
25–39	0.099	0.100	0.050	0.144
40–54	0.099	0.095	0.119	0.160
55–64	0.217	0.100	0.238	0.264

Source: CPS data and author's calculations.
a. CPS data are weighted using the CPS Basic Weight.

with some of the largest reductions in wage changes from the earlier to the later period occurring among younger and less educated individuals, which is where the employment changes were also the largest. For women, the relationship is much weaker: most age-education groups experienced wage increases, not decreases, from 1999 to 2007, although it is also the case that the wage increases were typically even larger from 1989 to 1999.

In estimating the model with wages, a well-known problem, extensively addressed in the labor supply literature, is that wage rates are not observed for nonworkers and must be imputed. I follow the fixed-effects approach described in equation 1 by first regressing the log of real weekly wages on the X_i vector (age-education-race dummy variables, separately by sex) separately for each of the three years in question: 1989, 1999, and 2007. Because the March CPS in those years reports earnings and weeks worked in the preceding calendar year, I select the sample and estimate these regressions using the 1990, 2000, and 2008 CPS, respectively. I then impute log weekly wages to all individuals in the March 1989, 1999, and 2007 CPS using the estimated equation for the respective year and enter this variable into the V_{it} vector. The coefficient on predicted log weekly wages is thus identified by the covariance between the change in employment probabilities and the change in predicted wages conditional on the age-education-race group, averaged over the groups. Put differently, this is the individual-data equivalent of a first-differenced grouped-data regression in which the change in the mean employment-population ratio in each group is regressed on the change in the log real weekly wage for that group, conditional on the other variables in the V_{it} vector (nonlabor income and demographic characteristics).[16]

For purposes of the analysis here, I do not investigate the source of the change in wages; the literature on changes in the wage structure over the last several decades is replete with alternative explanations for differential wage movements by education, experience, and sex. In addition, I implicitly assume that wage changes are the result of shifts in labor demand for different groups, rather than shifts in the labor supply curve. If the latter occur, some of the wage coefficients could be negative, and the results will show this. The object of this exercise is to determine how far one can go with a traditional labor supply model in explaining changes in the employment-population ratio, not to estimate a general equilibrium model of the labor market.

Another well-known problem since the work of James Heckman (1974) is that the wages of workers alone may be a biased measure of what nonworkers would earn, and for the issue studied here, changes in employment over time may result in biased estimates of the effects of wage changes if only workers' wages are used, because those who enter or exit employment may have systematically different wages than those who do not. For the main results reported, I employ a semiparametric version of the traditional

16. Estimation on the individual data is more efficient because it makes use of within-group covariances of the variables in the V_{it} vector. Formally, either the individual-data approach or the grouped-data approach is equivalent to an instrumental variables procedure where "year" is the variable included in the wage equation—because it is estimated separately by year—but excluded from the employment-population regression, which restricts all parameters to be the same over all years. This equivalence is demonstrated by Moffitt (1993) in a discussion of the work of Browning, Deaton, and Irish (1985).

Heckman (1979) approach, one not requiring the normality assumption. Reduced-form, first-stage OLS estimates of the employment equation in each year (leaving out the wage) are used to predict probabilities of employment, and a polynomial in those predicted probabilities is then entered into the wage equation estimated on workers only. The selection bias effect is identified because the employment equation contains variables—nonlabor income and some demographic variables—that are excluded from the wage equation. The predicted wage from this equation, obtained by setting the predicted probability equal to 1 (which is equivalent to setting the normal-distribution-based λ to zero), is then used in the employment equation.

As a sensitivity test, I also use the method of imputing wages to nonworkers employed by Juhn and others (1991) and by Juhn (1992), modified slightly as suggested by Blau and Kahn (2007). I also estimate the model with no adjustment for selection bias at all.

III.B. Nonlabor Income

The typical difficulty in constructing a variable for nonlabor income is that few types of such income are exogenous. Means-tested transfer income is inversely related to labor income and therefore to employment, and hence is endogenous, and most social insurance program benefits, such as unemployment insurance and Social Security, are likewise negatively related to employment (Social Security at certain ages is an exception). For this reason the typical labor supply study restricts the nonlabor income variable to include interest, dividends, and rent, which are contemporaneously independent of labor market activity. However, these types of capital income are the result of past accumulation of capital, which is no doubt related to earnings as well. Moreover, large fractions of the population receive no capital income at all. A third type of income sometimes included is earnings by other family members. The leading example is spousal earnings. However, this variable is also likely endogenous if the spouses coordinate their labor supply decisions.

Solving this old and difficult problem is beyond the scope of this study, so here I simply include interest and dividends in the measure of nonlabor income, excluding rent received for data availability reasons.[17] I also conduct sensitivity tests including earnings received by other family

17. The Census imputes rent received for many observations, with the result that a large fraction of the data has negative values for this form of income. In addition, very few families receive any income at all from this source.

members. The nonlabor income variable is converted to a weekly amount and expressed in 2007 PCE dollars.

III.C. Demographic Variables

As noted in the review of labor supply models above, the presence of children, marital status, and other family structure variables have been shown in the literature to have strong effects on labor supply, albeit quite different ones for men and women. Here I construct a three-category marital status variable—married, single, or divorced-widowed-separated—and include variables for the number of young children (those aged 0 to 5) and older children (6 to 18). Also included are variables indicating whether the individual is the head of the household or an unmarried parent (essentially an interaction between marital status and children). These variables are potentially endogenous, but I do not address this issue.

III.D. Results

Table 5 shows the results of the main model for men and women.[18] The wage coefficient for men is 0.06 and is statistically significant at conventional levels, implying that a 10 percent increase in the log weekly wage would raise the employment-population ratio by 0.6 percentage point. This corresponds to an elasticity of approximately 0.08, not large but consistent with the labor supply literature showing fairly inelastic labor supply curves for men. The wage elasticity for women is also positive but insignificant. This result simply reflects the lack of correspondence between the wage and employment changes from 1989 to 2007 shown in tables 1 and 4. Further results for women that separate the estimates by marital status, and yield different estimates, are discussed below.

The other variables have the coefficient signs and significance levels expected from the literature. Nonlabor income has a negative effect on labor supply, the presence of young children reduces the employment probabilities of women, that of older children also reduces women's employment but increases it for men, and married men are more likely to work than unmarried men, whereas women exhibit the opposite relationship. For

18. The standard errors shown are not adjusted for the two-stage nature of the estimation. Bootstrapped standard errors are preferred, but those estimates are biased and inconsistent if used with weighted data. Instead, the model was estimated without weights, and the standard errors with and without bootstrapping were compared: the bootstrapped standard errors were two to four times the unadjusted errors. This would not affect the significance levels of the wage coefficients in table 5 at conventional levels. The standard errors on the other coefficients were unaffected.

Table 5. Regressions of Employment on Wages, Nonlabor Income, and Selected Demographic Variables[a]

	Regression coefficient	
Independent variable	*Men*	*Women*
Log of real weekly wage in dollars	0.060*	0.009
	(0.008)	(0.015)
Weekly nonlabor income (thousands of dollars)	–0.001*	–0.001*
	(0.000)	(0.000)
No. of own children aged 0–5	0.000	–0.120*
	(0.002)	(0.003)
No. of own children aged 6–18	0.006*	–0.027*
	(0.001)	(0.002)
Married[b]	0.072*	–0.042*
	(0.005)	(0.005)
Divorced, widowed, or separated	0.019*	0.004
	(0.006)	(0.005)
Head of household	0.079*	0.096*
	(0.006)	(0.005)
Unmarried parent	0.025*	0.034*
	(0.009)	(0.006)

Source: Author's regressions.

a. The dependent variable is a dummy variable set equal to 1 if individual i in year t (t = 1989, 1999, or 2007) was employed and zero if not. Estimation is by OLS using pooled data from the 1989, 1999, and 2007 CPS (for men and women separately) and including a full set of age-education-race interactions. Standard errors are in parentheses. Asterisks indicate statistical significance at the 10 percent level.

b. The last four independent variables reported are dummy variables. The omitted marital status category is "single."

both sexes, household heads are more likely to work, as are unmarried parents, another common finding in the literature.

Table 6 compares the actual mean changes in male and female employment-population ratios in each of the two sample periods with those predicted by the estimated models.[19] For men, the model explains all of—in fact, more than—the small decline in the 1989–99 period, but only about half of the decline in the 1999–2007 period. For women, the model explains a little over half the rise in the ratio in the first period but virtually none of the decline in the second.[20]

Table 7 shows how the explanatory variables in the model changed in each period, providing some insight into the sources of the model

19. Standard errors are not shown because the sample sizes (see note 13) are so large as to make them quite small.

20. Separate model estimates for the 1989–99 and 1999–2007 periods show substantial differences in elasticities. Indeed, the women's wage elasticity in 1999–2007 is negative, reflecting the fact that women's wages rose over that period and their employment declined.

Table 6. Actual and Predicted Changes in the Employment-Population Ratio, by Sex, 1989–1999 and 1999–2007[a]
Percentage points

	1989–99		1999–2007	
Sex	*Actual*	*Predicted*	*Actual*	*Predicted*
Male	–0.6	–1.7	–2.5	–1.3
Female	4.2	2.3	–1.5	0.1

Source: Author's calculations.
a. The predicted change in the employment-population ratio between year t and year $t + 1$ is calculated as described in section III.

predictions. In the 1999–2007 period, the predicted decline in the male employment-population ratio is accounted for by the decline in wages, the number of older children, the fraction married, the fraction divorced or widowed or separated, and the fraction that are heads of household. Multiplying each of these variables by its regression coefficient shows that the wage decline dominates the other influences in importance, followed by the decline in the fraction married. For women, virtually every variable changed in a direction that would increase rather than decrease employment: wages increased whereas nonlabor income, the number of younger and of older children, and the fraction married declined. This explains why no decline in employment was predicted for women in table 6.

Table 8 shows how well the model captures the age-education patterns of employment decline from 1999 to 2007 reported in table 1. The model

Table 7. Changes in the Variables Explaining Employment, 1989–99 and 1999–2007

	Change in variable mean			
	Men		Women	
Independent variable	*1989–99*	*1999–2007*	*1989–99*	*1999–2007*
Log of real weekly wage in dollars	–0.028	–0.101	0.223	0.072
Weekly nonlabor income (thousands of dollars)	0.585	–0.241	0.549	–0.203
No. of own children aged 0–5	–0.031	–0.008	–0.034	–0.007
No. of own children aged 6–18	0.004	–0.027	0.008	–0.026
Married	–0.028	–0.014	–0.031	–0.012
Divorced, widowed, or separated	0.016	–0.002	0.008	–0.003
Head of household	–0.014	–0.006	–0.012	–0.007
Unmarried parent	0.007	–0.000	0.006	–0.001

Source: Author's calculations.

Table 8. Predicted Changes in the Employment-Population Ratio, by Sex, Age, and Education, 1999–2007
Percentage points

	Education			
Age	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*
		Men		
16–24 years	–0.9	–2.0	–1.3	–1.7
25–39	–0.7	–1.9	–1.4	–0.4
40–54	–1.5	–1.4	–0.6	–0.1
55–64	–1.8	–0.6	–0.7	–0.3
		Women		
16–24 years	0.3	–0.6	0.3	0.0
25–39	–1.0	–0.6	–0.4	–1.4
40–54	0.1	–0.2	0.2	–0.3
55–64	–0.3	0.3	–0.2	0.4

Source: Author's calculations.

captures very little of the pattern of greater declines for younger and less educated women, but this is not surprising given its lack of overall explanatory power. The model captures some of the relatively greater decline for younger men (except for those with less than a high school education) but captures the greater decline among the less educated only for older men. The model is therefore only partly successful, at best, at capturing these patterns.

III.E. Further Exploration of Patterns by Marital Status and Presence of Children

The lack of explanatory power of the model for women, together with the descriptive evidence, noted previously, that the decline in the employment-population ratio for women was concentrated among the unmarried and, within that group, among those without children, suggests that disaggregation of the sample by marital status and presence of children may be warranted. In fact, an inspection of the wage data for women reveals that the log weekly wage fell for married women over 1999–2007 but rose for unmarried women. Among the latter, wages rose for those with children and fell for those without children. Although this does not necessarily imply that the models estimated above are misspecified, it is worth investigating whether the coefficients on wages and other variables are different for the different groups.

Table 9. Wage Coefficients and Actual and Predicted Changes in the Female Employment-Population Ratio, by Marital and Parental Status, 1989–99 and 1999–2007[a]

Sample	Wage coefficient	Change in employment-population ratio (percentage points) 1989–99 Actual	1989–99 Predicted	1999–2007 Actual	1999–2007 Predicted
Married	0.076* (0.016)	5.2	4.5	–0.3	−1.1
Unmarried	–0.077* (0.012)	2.8	3.4	–2.9	−3.0
With children	0.045 (0.046)	12.6	3.4	–0.3	2.0
Without children	0.066* (0.008)	–0.1	−1.1	–3.5	−2.7

Source: Author's calculations.

a. Equations are estimated for each sample using the same specification as in table 5. Standard errors are in parentheses. Asterisks indicate statistical significance at the 10 percent level.

To this end, table 9 reports estimations of the model for married and unmarried women separately, and for unmarried women with and without children separately.[21] For married women the wage coefficient is 0.076, a statistically significant result and quite different from that estimated for all women combined (table 5). Married women's wages rose strongly in the 1990s and, as just noted, fell from 1999 to 2007, so the model is much more successful in explaining both the growth of employment in the first period and the decline in the second (in fact, the decline is overpredicted in the second).[22] But the lion's share of the female employment decline occurred, in any case, among the unmarried, and here the estimated model yields an implausible negative wage elasticity when estimated over that subsample.[23] This estimate is a simple result of the fact that wages for all unmarried women rose over the period, while their employment fell. How-

21. Marital status and childbearing are likely to be endogenous variables, at least to some extent, and any bias arising from their endogeneity could be made worse by this stratification. If there are unobserved variables affecting marital status, childbearing, and employment, and especially if the composition of different marital status and childbearing groups is changing over time, bias could arise. This issue should be addressed in future research.

22. Models for married women were also estimated including the husband's wage. The results did not change the general tenor of the results and are not presented.

23. A negative wage elasticity could result from some type of endogeneity, or from a supply shock instead of a demand shock. However, it would be surprising if either of these occurred only for this subgroup of women and not for any other women or for men.

ever, a further disaggregation of unmarried women into those with and those without children yields quite different results. Both wage elasticities are now positive, and for unmarried women without children, where the majority of the employment decline occurred, the model predicts an employment decline not far from the actual decline. This prediction arises because wages fell for this group, as noted above. Indeed, the wage decline dominates the influence of all the other variables in the model in magnitude.[24] The model does a poor job of explaining the small decline in employment for unmarried women with children, however, predicting instead an increase of some magnitude.

These results suggest that further investigation is warranted into the reasons for the differences in wage elasticities and in wage changes for women of different marital and parental status. The fraction of women who were married and had children was declining over the period, and this could be related to the employment changes, for example. It is also something of a puzzle why wage rates moved in such different directions for some of the demographic subgroups, who presumably operate in roughly the same labor market. This and other issues need to be explored.

IV. Influences on Labor Supply: Taxes and Transfers

As noted in the literature review above, increases in taxes and transfers are often hypothesized to reduce labor supply and employment. The question addressed in this section is whether prima facie evidence exists for such effects specifically between 2000 and 2007.

IV.A. Taxes

There were no changes in federal income taxes during 2000–07 that would have induced a decline in labor supply over the period, and as this section will show, many of the changes that did occur would suggest the opposite. The Economic Growth and Tax Relief Reconciliation Act of 2001 provided for lower marginal tax rates at all income levels to be phased in gradually over 2001–06, repealed the phase-out of itemized deductions and the personal exemption by 2008, and made some tax rate

24. The wage elasticity for the two groups combined is positive because the wage increase for unmarried women with children was particularly large but their employment decline was small, whereas unmarried women without children saw a modest decline in wages but a large decline in employment. Thus, the wage and employment changes for the two groups combined are negatively correlated.

Table 10. Average Tax Rates, Federal Income Tax, by Selected Income Category, 2000–07[a]
Percent

		Adjusted gross income[b]			
Year	*All returns*	*\$1–\$10,000*	*\$30,000–\$50,000*	*\$50,000–\$100,000*	*\$100,000–\$200,000*
2000	16.1	4.5	9.4	12.2	17.3
2001	15.2	2.8	9.1	11.7	16.6
2002	14.1	2.5	8.0	10.6	15.8
2003	13.0	2.5	7.6	9.6	14.0
2004	13.3	2.4	7.6	9.2	13.6
2005	13.6	2.5	7.5	9.1	13.3
2006	13.8	2.7	7.4	9.0	13.1
2007	13.8	2.7	7.3	9.0	12.8

Source: *SOI Bulletin,* various issues.
a. The average tax rate is total federal income tax due (after credits) as a share of adjusted gross income.
b. Income is in nominal dollars.

reductions retroactive. The Jobs and Growth Tax Relief Reconciliation Act of 2003 accelerated some of those reductions and reduced capital gains tax rates, and the Working Families Tax Relief Act of 2004 accelerated the provisions of both prior acts.

The relevant tax rate for the employment decision, as opposed to the marginal-hours-worked decision, is the average tax rate (ATR), which the Internal Revenue Service defines as total federal income tax due (after credits) as a share of adjusted gross income. Table 10 shows ATRs for all returns and for selected nominal income ranges in each of the years 2000 to 2007. The ATR fell overall and for most income ranges over this period, which should have led to an increase in the employment-population ratio rather than its opposite.

Other taxes during this period either did not change, increased only slightly, or fell. The federal payroll tax rate below the maximum taxable earnings amount remained at 7.65 percent over the period, unchanged from its value in 1990. The taxable maximum itself (which is indexed to inflation) did rise, however, leading to a slight increase in the payroll ATR for those with higher nominal incomes. The phase-in and phase-out tax rates for the earned income tax credit were unchanged from 1996 to 2008, although the income level for the maximum credit and for the complete phase-out moved up, increasing work incentives for lower earners and decreasing them for higher earners. Capital gains and dividend tax rates generally fell, and estate and gift tax rates were reduced and exemption levels raised.

IV.B. Transfers

The federal system of transfers includes both programs that provide means-tested transfers and social insurance programs, where eligibility and benefits are based on past earnings contributions. The leading means-tested programs in terms of expenditure and caseloads are Medicaid, Supplemental Security Income (SSI), Temporary Assistance for Needy Families (TANF, formerly AFDC), the Supplemental Nutrition Assistance Program (SNAP, since 2008 the official name of the food stamp program), and the federal housing aid programs. The leading social insurance programs are the Social Security retirement (Old Age and Survivors Insurance) program, Medicare, UI, and the Social Security disability insurance (DI) program.

Theoretically, most of these programs might be expected to reduce work incentives among their beneficiaries and hence to reduce work effort, although their governing laws and regulations include many specific provisions that could go either way and will not be discussed here. More important is the empirical literature on the existence and size of those disincentives; that literature is quite large for some programs and quite small for many others. The literature was recently reviewed by Yonatan Ben-Shalom, Moffitt, and John Scholz (forthcoming), who find the evidence on work disincentives to be modest for most programs and very sparse for some. There is virtually no research evidence on the work disincentives of the current TANF program, for example, and very little for SSI. However, the TANF caseload is extremely small and very unlikely to contribute to the widespread employment-population declines seen in the data, and SSI affects only the aged and the disabled. A few studies have examined the work disincentives of expansions of the Medicaid program and have shown zero or negligible effects (Gruber 2003, Ham and Shore-Sheppard 2005). SNAP appears to create work disincentives that are quite small, primarily because the benefit in question (food coupons) is not sufficiently large to provide much additional income as a result of reduced work effort (Currie 2003). A recent study of the federal housing aid programs shows them to create significant work disincentives, lowering employment by about 4 percentage points (Jacob and Ludwig 2012). However, once again, housing subsidy recipients are a restricted set of the population.

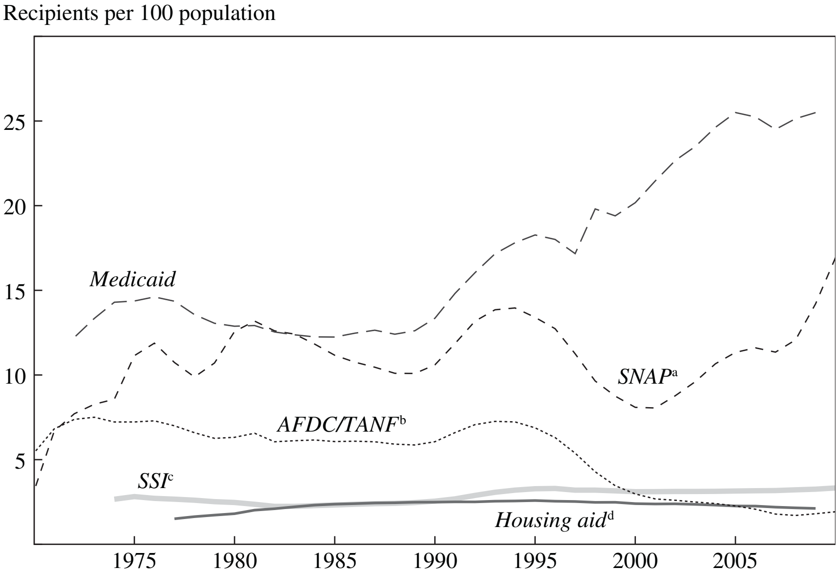
As for the social insurance programs, a vast literature analyzes the effects of the Social Security retirement program on retirement ages and the labor supply of the elderly, but they are outside the scope of this study. Evidence on the program's indirect impact on labor supply by the nonelderly is too sparse to allow any reliable conclusions. The few recent studies of the effect of Medicare on labor supply (such as French and Jones 2011) suggest the possibility of nontrivial work disincentives but, once again, only for those 65 or older. Research on the disincentive effects of Social Security's DI program has been increasing because of the recent growth in its caseload and expenditure. No consensus on its work disincentive effects has yet emerged, however: studies using traditional benefit-employment correlations (such as Autor and Duggan 2003) show larger disincentives than studies using rejected applicants as a control group (Bound, Lindner, and Waidmann 2010).[25] A very large literature examines the work disincentives of the UI program but reaches very little consensus on the magnitude of the effects. If one considers only the effects of the basic UI program and not of its extensions during economic downturns—there was no difference in UI extended benefits in 2000 and 2007—the most cited study is the recent work of Raj Chetty (2008), which implies nontrivial work disincentives of the program on lengths of unemployment spells.

More important for present purposes is whether any of these programs grew in size over 1999–2007 and, if so, whether there was any significant change in their structure, eligibility, or benefits over that period. Figure 4 shows that among the major means-tested transfer programs, only Medicaid and SNAP saw substantial growth in caseloads during this period; the SSI caseload also grew, and the housing caseload declined, but both only slightly; the TANF caseload also declined. The growth of Medicaid during this period was a result of legislation in 1999 creating the State Children's Health Insurance Program (SCHIP, later renamed CHIP), which expanded coverage to children. This expansion should have had only an indirect impact on adult work effort. In addition, the growth of the Medicaid program began much earlier, in the late 1980s, as a result of expansionary reforms for coverage of children and pregnant women, which continued to have an impact over succeeding years.

More relevant to adult work effort is the rise in the SNAP caseload per capita, which began around 2000 and continued to grow thereafter (and accelerated after the onset of the Great Recession). The reasons for this growth relate to administrative reforms intended to increase the participation rate of eligible families, which historically had been only around 60 percent. Beginning in the late 1990s and early 2000s, the U.S. Department of Agriculture began strongly encouraging states to make it easier

25. However, von Wachter, Manchester, and Song (2011) have found that even the rejected-applicant methodology shows growing work disincentives over time as a result of changes in the composition of the caseload.

Figure 4. Caseloads of Means-Tested Federal Transfer Programs, 1970–2010



Source: Author's data set compiled from various government sources.

a. Supplemental Nutrition Assistance Program.

b. Temporary Assistance for Needy Families replaced the Aid to Families with Dependent Children program in 1996.

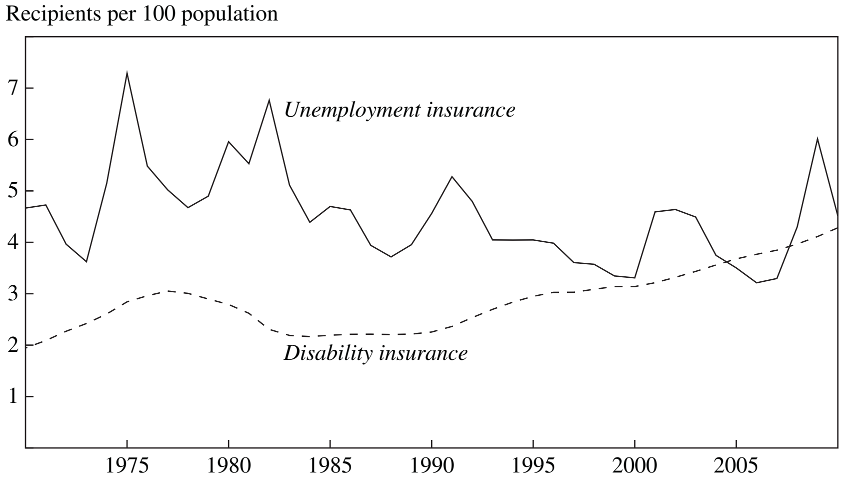
c. Supplemental Security Income.

d. Includes Section 8 subsidized housing, public housing, and other programs.

to participate in the program. These activities included extensive outreach programs to inform low-income communities about the program; simplified eligibility criteria that reduced the paperwork requirements for application; reduced recertification requirements requiring less reporting and less frequent reestablishment of eligibility; and relaxed asset test requirements (Leftin and Wolkwitz 2009). These reforms have been found to be a major cause of the increase in the caseload (Klerman and Danielson 2009).

Earlier research (for example, Currie 2003) has found SNAP to create very small work disincentives, if any. However, it is possible that the marginal individuals brought into the program by these administrative reforms had higher levels of initial employment and perhaps stronger employment reductions from receiving program benefits. Although no direct evidence on this question is available, one can identify where in the income distribution the increases in participation from 2000 to 2007 occurred. As table 11 indicates, these increases were concentrated among the very poorest

Figure 5. Unemployment and Disability Insurance Caseloads, 1970–2010



Source: Author's data set compiled from various government sources.

families, those with incomes less than 25 percent of the poverty line.[26] Although in principle the low incomes of these families may be partly the result of program participation, it seems unlikely that their employment would be very high in the absence of participation, as might have seemed more possible if the participation increases had occurred somewhat higher in the distribution. In any case, more research on this question is needed.

Figure 5 shows the growth in caseloads per capita of the two social insurance programs for the nonelderly, UI and DI. The UI caseload was approximately the same in 2000 and 2007, not surprisingly since the unemployment rate was the same in those years as well, and no structural reforms of the program took place between those years. The DI program, on the other hand, continued a pattern of growth that had begun in 1990 (and which continued into the Great Recession and its aftermath).[27] Much research has been conducted on the causes of this growth. Declining wages for the low-skilled population may be one factor responsible, but changes

26. Recall that the official definition of income excludes SNAP benefits.

27. The rate of applications is much more volatile than the slow-moving caseload stock shown in the figure (David Autor, personal correspondence). Moreover, applications are also correlated with work disincentives because applicants typically do not work while awaiting an award decision, which could be as much as 5 years later. The application rate per 1,000 persons aged 25–64 in the population was 8.25 in 1999 and 13.72 in 2007.

Table 11. SNAP Households by Poverty Status, 2000 and 2007

Gross household income (as percent of poverty line)	*Percent of all households receiving SNAP benefits* 2000	2007
0–25	16.8	23.6
26–50	16.2	15.3
51–75	25.3	17.8
76–100	30.3	30.7
101–130	10.4	10.5
131 and over	1.0	2.1
All households	100.0	100.0

Sources: U.S. Department of Agriculture (2001, 2008).

in administrative procedures that effectively allow more eligible individuals into the program are thought to be another (Bound and others 2010).

A reexamination of the disincentive effects of the DI program is beyond the scope of this paper, but a relevant piece of evidence that can be gleaned from the CPS is whether the magnitudes and pattern of increases in DI receipt across age-education groups (receipt of DI benefits is reported in the survey) match up with the patterns of employment decline. Table 12 shows how DI receipt reported in the CPS changed between 1999 and 2007 for the different groups of men and women; these data provide little basis for concluding that DI has played much of a role in the employment decline. Some of the youngest and least skilled groups have seen increases in receipt, but the magnitudes are tiny compared with the declines in the employment-population ratio shown in table 1. Many older groups have actually seen declines in receipt. Although the fraction of the caseload composed of somewhat younger men has increased in recent decades (von Wachter and others 2011), the caseload is still dominated by older men: in the CPS data set used here, the share receiving DI exceeds 3 percent in some of the subgroups within the 55–64 age group, but never more than half a percent among those under 40. Thus, the patterns of receipt of this type of transfer do not match up well with the age patterns of the employment-population ratio decline.[28]

28. DI receipt can be as much a result of employment declines as its cause. Therefore, if the pattern of receipt were to match up well with the pattern of employment declines, no causal conclusions could be drawn. However, the failure of them to match up well constitutes legitimate evidence in the opposite direction, that the program is unlikely to have played a major role.

Table 12. Changes in Share of Population Receiving Disability Insurance Benefits, by Sex, Age, and Education, 1999–2007
Percentage points

	Education			
Age	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*
		Men		
16–24 years	0.04	0.19	–0.03	0.00
25–39	–0.18	–0.09	–0.07	–0.08
40–54	–0.14	0.33	–0.06	0.08
55–64	0.41	–0.21	0.70	–0.62
		Women		
16–24 years	0.06	0.17	–0.08	0.00
25–39	–0.06	–0.17	0.01	–0.24
40–54	0.28	0.09	0.17	0.11
55–64	0.22	0.59	0.11	0.14

Source: Author's calculations.

In summary, few changes in the tax and transfer system during 1999–2007 are likely to have contributed to the decline in employment-population ratios. Income tax rates fell rather than increased, and no other significant changes in the tax system occurred. Most transfer programs did not experience programmatic reform, and those that experienced significant caseload growth over the period are unlikely to have played a major role in the employment declines, although more study of SNAP in this regard is warranted.

V. Other Possible Influences on Labor Supply

Several other factors that may have influenced the decline in employment-population ratios are worthy of consideration. These include changes in time use, health status, incarceration, and the minimum wage.

V.A. Time Use

Time use could be a contributing explanation if the decline in employment-population ratios was accompanied by an increase in nonmarket work, household production, or time devoted to childcare. Although such a shift would itself require an explanation, it would obviously suggest a concrete direction for exploration. Unfortunately, examining this hypothesis specifically for the 1999–2007 period is severely limited by lack of data. Modern analyses of time use begin with the American Time Use Survey (ATUS), whose first year was 2003. As I have emphasized, 2003 witnessed the trough of a business cycle and a peak of unemployment (see figure 2), and consequently one should expect to see a decline in employment-population ratios and probably an increase in nonmarket work between 1999 or 2000 and that year. However, no time use survey is available for 1999 or 2000 in any case. Before the 2003 ATUS, the most reliable recent survey was conducted in 1985, far too early to draw conclusions for the time period under consideration here. Further, Mark Aguiar and Erik Hurst (2007) have shown that nonmarket work time actually declined over 1985–2003—the wrong direction for explaining a downward trend in employment. A 1994 survey exists but is widely regarded as fairly unreliable, often providing counterfactual results, and its time use categories are not completely comparable to those used in the ATUS. Aguiar and Hurst also analyze that survey, however, and again find that nonmarket work declined from 1994 to 2003, albeit by a smaller amount than from 1985 to 2003.

Aguiar, Hurst, and Loukas Karabarbounis (2011) use the ATUS to chart nonmarket work time from 2003 to 2007 and beyond (their paper is more focused on trends in the Great Recession). Their data show strong increases in market work and declines in nonmarket time from 2003 to 2007. Again, however, this was unquestionably a cyclical recovery period, and this direction of effect is exactly what one would expect for that phase of a cycle.

Somewhat better recent data are available on time spent in childcare. Garey Ramey and Valerie Ramey (2010) analyze data for 1975–2008 and find an upward trend in childcare time. Although this is evidence in support of the hypothesis that changes in time use contributed to the decline in employment, the pattern by level of parental education in the childcare trend was exactly the opposite of that in the employment-population ratio demonstrated previously. Whereas the declines in employment were disproportionately concentrated among the less educated—and, as the previous section showed, among the unmarried—the increases in childcare time were concentrated among higher educated, married individuals.

V.B. Health

A decline in the overall health of a population is another factor that could contribute to a decline in employment. Although one would naturally expect health to improve over time for a population with growing income per capita, it need not necessarily improve over shorter periods, particularly for the most disadvantaged subgroups. Measurement is a difficult issue here as well: the use of medical records to determine trends in specific morbidity rates is subject to bias, because improved medical procedures generally result in greater detection of disease. The most commonly used measure of health status is a self-rated measure from survey questions asking whether an individual's health is excellent, good, fair, or poor. This question is used in the CPS as well as in the National Health Interview Survey and others. Unfortunately, time trends in the fractions of the population reporting these different health status categories differ dramatically across surveys—rising in some, falling in others, and stationary in others, including over 2000–07 (Salomon and others 2009). In any case, the CPS, in particular, shows improvements in health in almost all age, education, and race categories, so it is unlikely that this factor is a significant one in the employment-population trends under study here.

V.C. Incarceration

The dramatic increase in the incarceration rate of disadvantaged men is well known and is another trend that could be related to the decline of male employment.[29] The number of prisoners in federal or state prisons per 100,000 residents rose by almost five times between 1975 and 2009 (Pettit 2012). Much of the increase was among less educated men: the fraction of non-Hispanic white men with less than a high school education who were incarcerated rose from 3.5 percent in 1980 to 8.3 percent in 2008, and that for non-Hispanic black men rose from 9.6 percent to 29.6 percent over the same years (Pettit 2012, table 3.1). Changes in sentencing and parole policy account for most of the rise (Raphael 2010).

However, these increases in incarceration do not necessarily align well with the employment declines studied here: incarceration has risen steadily since the late 1970s or early 1980s, and if anything, the rise slowed in the 2000s. In addition, as appendix table A.1 indicates, the employment declines were not particularly concentrated among the non-Hispanic black population. Further, the CPS figures on employment include only the noninstitutionalized population and hence exclude men in prisons and jails. In a simple supply-and-demand framework, one would expect a reduction in the aggregate labor supply of less educated men to lead to an increase in equilibrium wages and consequently to an increase, not a decrease, in employment among those men remaining.

Nevertheless, the potential impact of long-term increases in incarceration could be felt through its impact on the employment rates of men after they leave prison. Substantial evidence indicates that past incarceration reduces the probability of being hired (Raphael 2010), and indeed, many

29. I thank Steven Davis for suggesting this avenue of investigation.

employers have explicit rules against such hiring. Becky Pettit (2012, table 1.4) uses data on the age distribution of prisoners over time to estimate cohort rates of cumulative imprisonment by ages 30–34 and finds those rates to have risen dramatically over time: the share of white male school dropouts who had ever been incarcerated rose from 14.4 percent in 1999 to 28.0 percent in 2009; the rate for black male dropouts rose from 46 percent to 68 percent. Although these rates were already increasing in earlier decades, it is possible that they rose to such high levels in the 2000s that the negative impact on male employment rates was particularly large.

V.D. Minimum Wage

Wage rigidities may also account for some of the 1999–2007 employment decline; if so, they would also explain why wages do not play a more important role in the labor supply models estimated in this paper.[30] Markets for low-wage labor, at least those for the very unskilled, are typically nonunionized and fairly competitive, so it is unclear whether wage rigidities are important for the groups that have been shown here to have experienced the largest employment declines. The only significant source of such rigidities is the minimum wage. However, trends in the national real minimum wage go in exactly the wrong direction to explain the differing employment trends in the 1990s and the 2000s before 2007. The real federal minimum wage declined from 1974 to 1989, rose from 1989 to 1997, and then declined again from 1997 to 2006. It has risen dramatically since then, beginning with an increase from $5.15 per hour to $5.85 per hour in July 2007, but that increase occurred after the March 2007 CPS, too late to have had any effect on the trends studied here.

VI. Summary and Implications for Postrecession Employment

The decline in the employment-population ratios for men and women during 1999–2007, before the Great Recession, represents a historic turnaround. The decline was disproportionately concentrated in the less educated and younger groups within both the male and the female populations and, within the latter, among unmarried women, especially those without children. A standard model that emphasizes the role of wage rates and nonlabor income can explain about half the decline for men but none of

30. Shimer (2011 and elsewhere) has suggested that wage rigidities could play an important role in movements in the labor force participation rate. However, his emphasis is on explaining cyclical movements rather than trends, which are the focus here.

it for women, whose wages rose, on average and across all subgroups, over the period. However, separate examination of trends in wages and employment for married and unmarried women, and for unmarried women with and without children, finds a more important role for wages. The decline in female employment was by far the largest for unmarried women without children, and wages for that group declined over 1999–2007. However, the different trends in wage rates and other determinants of employment for these different demographic subgroups raise many questions that need to be explored.

Most other possible influences on the employment-population ratio also appear unlikely to have contributed to the 1999–2007 decline. Federal income tax rates fell rather than rose, other federal tax rates did not rise, and federal transfer programs did not change in structure or in patterns of growth that line up with the employment declines, although further study of the Supplemental Nutrition Assistance Program and the Social Security disability insurance program would be worthwhile. Changes in health status, the minimum wage, and other factors also appear to have played no role, although rising rates of incarceration among disadvantaged and younger men may have contributed. Whether changes in time use and home production accompanied the employment declines is not answerable with the available data but remains a possibility. Further analysis of possible contributors to the employment decline is clearly needed.

In 2008, with the onset of the Great Recession, the employment-population ratio plummeted, falling to approximately 72 percent for men and 63 percent for women in 2009. It has exhibited a slow recovery since that time. Given the downward trend in the ratio before the Great Recession, a natural question is whether it will return to its 2007 level after the recovery is complete, or only to a lower level. The model estimated in this paper can be used to forecast employment-population ratios in 2011, the latest year for which CPS data are available. The results suggest that the ratios may fully return to their 2007 levels. The online appendix to this paper describes the details of the calculation.[31]

31. Online appendixes and replication files for the papers in this volume may be accessed on the *Brookings Papers* website, www.brookings.edu/about/projects/bpea, under Past Editions.

APPENDIX

Supplementary Tables and Estimates

Table A.1 shows the changes in the employment-population ratio from 2007 to 2001 by race as well as by age and education.

Tables A.2 and A.3 show the estimated wage coefficients for the main model for subgroups of men and women, respectively, for the years 1989, 1999, and 2007 (estimated from the 1990, 2000, and 2008 March CPS, respectively). Demographic variables for marital status and headship are included on the presumption, supported by much of the literature, that much of the correlation between these variables and wage rates is causal. The predicted probability of being in the wage sample—which is not only the predicted probability of working during the year but also that of not satisfying any of the exclusions from the sample noted in the text—is taken from a first-stage reduced-form OLS employment status regression.[32] The identifying variables are those for children and nonlabor income. The coefficient on the predicted probability is negative for men, which is consistent with positive selection (that is, nonworking men have lower wage rates than working men). However, the coefficient is positive for women, which is generally interpreted as implying that women with higher market wage rates have even higher reservation wages.

Table A.4 shows estimates of the wage coefficient from different methods of imputing wages and estimating the model, along with the predicted changes in employment probabilities for comparison with those in table 6. The first row shows the effect of using no adjustment for selection bias in the wage equation. The wage coefficient for men is negative in this case, whereas that for women is positive. For men, the declining employment rate leads to an increase in the wage conditional on working, and this leads to a negative correlation between the wage change and the change in employment. For women, selection operates in the opposite direction, leading to a positive bias. However, the predicted changes in employment probabilities for the two time periods are very close to those in table 6.

The second row shows the effect of imputing wages to nonworkers using a method closely related to that of Blau and Kahn (2007), who themselves adopted a method used by Juhn (1992) and Juhn and others (1991). The latter two studies imputed wages of those who had worked 1–13 weeks to nonworkers, whereas Blau and Kahn estimated regressions separately for those who worked less than 20 weeks and those who worked 20 weeks or

32. A quadratic predicted probability was also tested but yielded very similar results.

Table A.1. Changes in Employment-Population Ratios by Sex, Age, Education, and Race, 1999–2007 and 1989–99[a]
Percentage points

	White				*Black*				*Other*			
Age	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*	*No high school diploma*	*High school graduate*	*Some college*	*College degree or more*
						1999–2007						
Men												
16–24	−7.83	−4.30	−1.75	−1.53	−6.80	−0.66	4.27	1.77	−11.31	4.98	3.39	−13.19
25–39	−1.23	−2.82	−1.59	0.93	4.20	−5.91	−8.14	2.63	−10.57	−4.41	3.94	3.74
40–54	−2.25	−1.95	−1.02	−0.14	−11.61	−7.85	3.43	−0.81	−7.20	−0.25	−0.75	1.03
55–64	−1.10	−2.52	−1.02	0.34	−2.47	−6.64	−12.45	4.93	−5.22	−13.23	−3.51	1.52
Women												
16–24	−8.85	−6.56	−1.33	−4.79	−3.43	−11.66	−6.96	9.10	−1.71	0.05	3.64	−4.40
25–39	−7.39	−4.19	−0.98	−2.54	−0.59	−3.35	−6.61	−3.05	−0.76	−0.35	1.47	−0.91
40–54	1.03	0.37	−2.09	−2.58	−0.41	−3.52	3.31	2.86	9.64	−1.00	−8.70	1.67
55–64	2.88	2.34	2.99	7.94	2.29	5.51	11.06	9.92	8.73	14.95	18.50	−10.53
						1989–99						
Men												
16–24	−4.63	0.36	−0.48	4.68	−0.43	−10.75	−7.77	−29.86	5.34	3.11	1.88	6.34
25–39	0.99	0.41	0.05	−0.86	−5.87	2.47	−1.73	1.38	16.25	3.37	−4.48	−1.29
40–54	−2.91	−2.79	−3.06	−1.15	−4.68	−1.25	−8.50	−2.43	7.26	−7.00	−4.09	0.35
55–64	−0.69	−2.11	0.79	1.92	−5.23	−6.23	3.64	3.61	−5.28	6.88	−17.85	−10.38
Women												
16–24	0.65	1.54	−0.10	3.71	6.10	15.74	−9.05	−28.84	−9.31	16.78	8.64	−12.74
25–39	4.95	2.24	1.60	0.84	15.05	4.14	7.82	1.30	11.90	−2.32	−5.21	−9.66
40–54	0.44	1.68	1.76	3.36	0.00	−3.63	4.04	−1.69	12.14	5.48	−0.02	−2.04
55–64	−0.11	5.90	5.55	1.81	3.65	−3.48	−5.46	4.09	−3.80	−36.55	−23.14	16.38

Source: CPS data and author's calculations.
a. CPS data are weighted using the CPS Basic Weight.

Table A.2. Estimates of Log Weekly Wage Regressions, Men

	1989		*1999*		*2007*	
Characteristic	*Wage coefficient*	*Standard error*	*Wage coefficient*	*Standard error*	*Wage coefficient*	*Standard error*
Married	0.1877	0.0113	0.2245	0.0129	0.2666	0.0151
Divorced, widowed, or separated	0.1032	0.0155	0.1402	0.0163	0.1858	0.0176
Head of household	0.3820	0.0229	0.4422	0.0305	0.4951	0.0428
Single parent	0.0267	0.0271	0.1088	0.0285	0.1549	0.0290
Predicted probability of being in wage sample	–1.1896	0.1392	–1.8974	0.2001	–1.9502	0.2754
Age 16–24, no HS diploma, white	–0.7807	0.1858	–1.0682	0.0936	–1.1286	0.0823
Age 16–24, no HS diploma, black	–1.0142	0.1908	–1.3750	0.1154	–1.2176	0.1166
Age 16–24, no HS diploma, other	–0.7240	0.2071	–1.4232	0.1206	–1.3357	0.1320
Age 16–24, completed HS, white	–0.0725	0.1913	–0.2415	0.1074	–0.2962	0.0917
Age 16–24, completed HS, black	–0.3372	0.1904	–0.5868	0.1059	–0.5805	0.0894
Age 16–24, completed HS, other	–0.3502	0.2026	–0.4196	0.1372	–0.4186	0.1210
Age 16–24, some college, white	–0.2089	0.1901	–0.3887	0.1041	–0.4275	0.0853
Age 16–24, some college, black	–0.3045	0.1965	–0.6508	0.1057	–0.5535	0.1064
Age 16–24, some college, other	–0.3856	0.1949	–0.4888	0.1325	–0.9444	0.1045
Age 16–24, college degree or more, white	0.1594	0.1945	0.0463	0.1187	0.0070	0.1005
Age 16–24, college degree or more, black	–0.2279	0.2721	0.1036	0.1810	–0.0627	0.1809
Age 16–24, college degree or more, other	–0.1372	0.2228	–0.4181	0.1789	–0.1714	0.1745
Age 25–39, no HS diploma, white	–0.1160	0.1884	–0.3707	0.1002	–0.3985	0.0882
Age 25–39, no HS diploma, black	–0.4222	0.1900	–0.7051	0.1059	–1.0228	0.1153
Age 25–39, no HS diploma, other	–0.1656	0.2069	–0.3123	0.1210	–0.3310	0.1162
Age 25–39, completed HS, white	0.2310	0.1889	0.0629	0.1030	0.0150	0.0921
Age 25–39, completed HS, black	0.0410	0.1910	–0.0472	0.1069	–0.3868	0.0831
Age 25–39, completed HS, other	0.0602	0.1959	0.0132	0.1183	–0.1987	0.0997
Age 25–39, some college, white	0.3312	0.1887	0.2305	0.1051	0.1551	0.0970

(continued)

Table A.2. Estimates of Log Weekly Wage Regressions, Men (*Continued*)

	1989		*1999*		*2007*	
Characteristic	*Wage coefficient*	*Standard error*	*Wage coefficient*	*Standard error*	*Wage coefficient*	*Standard error*
Age 25–39, some college, black	0.2169	0.1928	0.1367	0.1126	–0.0799	0.0997
Age 25–39, some college, other	0.0709	0.1960	0.0106	0.1194	0.1203	0.1063
Age 25–39, college degree or more, white	0.4249	0.1872	0.3472	0.0996	0.3584	0.0907
Age 25–39, college degree or more, black	0.3921	0.1962	0.3168	0.1137	0.1440	0.0947
Age 25–39, college degree or more, other	0.3332	0.1923	0.3431	0.1054	0.3953	0.0930
Age 40–54, no HS diploma, white	–0.1180	0.1865	–0.4039	0.0955	–0.5339	0.0762
Age 40–54, no HS diploma, black	–0.2537	0.1901	–0.6206	0.1083	–0.7880	0.0893
Age 40–54, no HS diploma, other	–0.1630	0.1977	–0.6319	0.1277	–0.5065	0.1146
Age 40–54, completed HS, white	0.2310	0.1863	–0.0054	0.0948	–0.1292	0.0760
Age 40–54, completed HS, black	0.1363	0.1902	–0.2991	0.1005	–0.4074	0.0799
Age 40–54, completed HS, other	0.1196	0.2031	–0.4566	0.1180	–0.2116	0.0893
Age 40–54, some college, white	0.3078	0.1860	0.1361	0.0950	0.1009	0.0786
Age 40–54, some college, black	0.2178	0.1976	–0.1928	0.1012	–0.1067	0.0853
Age 40–54, some college, other	0.2850	0.2010	–0.0272	0.1135	–0.0442	0.1025
Age 40–54, college degree or more, white	0.3483	0.1853	0.1812	0.0913	0.2142	0.0730

Age 40–54, college degree or more, black	0.5071	0.1938	0.1630	0.1059	0.1255	0.0873
Age 40–54, college degree or more, other	0.2330	0.1931	–0.0001	0.1116	0.1902	0.0866
Age 55–64, no HS diploma, white	–0.3785	0.1874	–0.7183	0.0981	–0.8851	0.0923
Age 55–64, no HS diploma, black	–0.4913	0.1936	–1.0092	0.1424	–1.1409	0.1248
Age 55–64, no HS diploma, other	–0.4312	0.2484	–0.6960	0.1737	–1.1763	0.1950
Age 55–64, completed HS, white	–0.1015	0.1861	–0.4461	0.0937	–0.3865	0.0743
Age 55–64, completed HS, black	–0.1324	0.2109	–0.7722	0.1379	–0.7293	0.1071
Age 55–64, completed HS, other	–0.0316	0.1964	–0.5361	0.1418	–0.6273	0.1208
Age 55–64, some college, white	0.0906	0.1874	–0.3180	0.0949	–0.2760	0.0750
Age 55–64, some college, black	–0.3234	0.2499	0.0103	0.1204	–0.5539	0.1072
Age 55–64, some college, other	0.0434	0.2067	–0.0457	0.1476	–0.0766	0.1017
Age 55–64, college degree or more, white	0.0971	0.1873	–0.1990	0.0969	–0.0671	0.0758
Age 55–64, college degree or more, black	0.1726	0.2130	–0.2771	0.1360	–0.0395	0.1205
Constant	6.6765	0.1915	7.2328	0.1117	7.1682	0.1098
No. of observations	27,168		20,883		21,456	

Source: Author’s regressions.

Table A.3. Estimates of Log Weekly Wage Regressions, Women

	1989		*1999*		*2007*	
Characteristic	*Wage coefficient*	*Standard error*	*Wage coefficient*	*Standard error*	*Wage coefficient*	*Standard error*
Married	–0.0581	0.0137	0.0041	0.0160	0.0042	0.0153
Divorced, widowed, or separated	0.0269	0.0150	0.0163	0.0147	0.0226	0.0153
Head of household	0.1145	0.0144	0.0374	0.0180	0.0456	0.0198
Single parent	–0.0581	0.0156	–0.1114	0.0158	–0.1491	0.0172
Predicted probability of being in wage sample	0.8119	0.0737	0.8558	0.1176	0.7332	0.1272
Age 16–24, no HS diploma, white	–1.1240	0.1335	–1.2940	0.0987	–1.2590	0.1064
Age 16–24, no HS diploma, black	–1.1174	0.1414	–1.1194	0.1189	–1.2303	0.1235
Age 16–24, no HS diploma, other	–1.0255	0.1618	–1.0065	0.1272	–1.1757	0.1318
Age 16–24, completed HS, white	–0.9653	0.1355	–1.1790	0.0996	–1.0876	0.1074
Age 16–24, completed HS, black	–0.8195	0.1401	–1.0959	0.1067	–1.1134	0.1180
Age 16–24, completed HS, other	–0.7396	0.1600	–1.0354	0.1317	–1.0214	0.1314
Age 16–24, some college, white	–0.9456	0.1361	–1.1845	0.0985	–1.1692	0.1071
Age 16–24, some college, black	–0.8072	0.1452	–1.1283	0.1114	–1.0229	0.1175
Age 16–24, some college, other	–0.8674	0.1529	–1.0853	0.1152	–1.0516	0.1251
Age 16–24, college degree or more, white	–0.5456	0.1394	–0.8665	0.1075	–0.7993	0.1167
Age 16–24, college degree or more, black	–0.7490	0.2256	–0.6739	0.1873	–0.5644	0.1452
Age 16–24, college degree or more, other	–0.5420	0.1601	–0.3117	0.2798	–0.4001	0.1732
Age 25–39, no HS diploma, white	–0.6771	0.1341	–0.9470	0.0976	–0.9617	0.1065
Age 25–39, no HS diploma, black	–0.7218	0.1405	–0.9274	0.1066	–1.0077	0.1221
Age 25–39, no HS diploma, other	–0.5363	0.1480	–0.9742	0.1357	–1.0658	0.1340
Age 25–39, completed HS, white	–0.5845	0.1341	–0.7839	0.0968	–0.8043	0.1061
Age 25–39, completed HS, black	–0.6033	0.1361	–0.7942	0.1011	–0.8074	0.1097
Age 25–39, completed HS, other	–0.5210	0.1433	–0.8295	0.1109	–0.7907	0.1193
Age 25–39, some college, white	–0.4044	0.1344	–0.6818	0.0977	–0.6860	0.1090
Age 25–39, some college, black	–0.4818	0.1379	–0.6219	0.1007	–0.6556	0.1122

Age 25–39, some college, other	–0.3395	0.1487	–0.7177	0.1107	–0.6469	0.1171
Age 25–39, college degree or more, white	–0.1140	0.1340	–0.3256	0.0974	–0.3515	0.1093
Age 25–39, college degree or more, black	–0.1662	0.1387	–0.3714	0.1040	–0.3985	0.1159
Age 25–39, college degree or more, other	–0.0080	0.1388	–0.2509	0.1038	–0.2465	0.1121
Age 40–54, no HS diploma, white	–0.5621	0.1341	–0.8503	0.0975	–0.8812	0.1052
Age 40–54, no HS diploma, black	–0.5529	0.1416	–0.8556	0.1137	–0.7786	0.1246
Age 40–54, no HS diploma, other	–0.4577	0.1498	–0.6909	0.1325	–0.9073	0.1206
Age 40–54, completed HS, white	–0.4517	0.1334	–0.6823	0.0965	–0.6872	0.1055
Age 40–54, completed HS, black	–0.4322	0.1369	–0.7375	0.1007	–0.7132	0.1088
Age 40–54, completed HS, other	–0.4148	0.1436	–0.6229	0.1173	–0.7345	0.1169
Age 40–54, some college, white	–0.2884	0.1342	–0.5074	0.0969	–0.5098	0.1067
Age 40–54, some college, black	–0.2795	0.1420	–0.4427	0.1025	–0.4769	0.1100
Age 40–54, some college, other	–0.1629	0.1550	–0.4200	0.1194	–0.6584	0.1179
Age 40–54, college degree or more, white	–0.0546	0.1338	–0.2070	0.0960	–0.1959	0.1045
Age 40–54, college degree or more, black	0.0868	0.1440	–0.1078	0.1034	–0.1854	0.1110
Age 40–54, college degree or more, other	0.0531	0.1482	–0.2745	0.1118	–0.1912	0.1170
Age 55–64, no HS diploma, white	–0.5055	0.1357	–0.6665	0.1079	–0.8936	0.1145
Age 55–64, no HS diploma, black	–0.6226	0.1487	–0.8128	0.1231	–0.7931	0.1299
Age 55–64, no HS diploma, other	–0.7527	0.1677	–0.7625	0.1601	–0.7394	0.1445
Age 55–64, completed HS, white	–0.3706	0.1334	–0.5900	0.0979	–0.6323	0.1036
Age 55–64, completed HS, black	–0.4422	0.1528	–0.5853	0.1221	–0.7429	0.1136
Age 55–64, completed HS, other	–0.2559	0.1680	–0.6155	0.1365	–0.6108	0.1351
Age 55–64, some college, white	–0.2647	0.1366	–0.3870	0.0984	–0.4488	0.1047
Age 55–64, some college, black	–0.1017	0.1624	–0.3880	0.1363	–0.4772	0.1181
Age 55–64, some college, other	–0.2622	0.1838	–0.2828	0.1853	–0.6122	0.1524
Age 55–64, college degree or more, white	–0.0161	0.1404	–0.1893	0.1016	–0.1638	0.1045
Age 55–64, college degree or more, black	–0.3783	0.2900	0.0139	0.1473	–0.2094	0.1453
Constant	5.8969	0.1348	6.2695	0.1077	6.4409	0.1065
No. of observations	26,993		21,053		21,908	

Source: Author’s regressions.

Table A.4. Wage Coefficients and Model Employment Predictions for Alternative Model Specifications[a]

	Men			*Women*		
		Predicted change in employment-population ratio (percentage points)			*Predicted change in employment-population ratio (percentage points)*	
Specification	*Wage coefficient*	*1989–99*	*1999–2007*	*Wage coefficient*	*1989–99*	*1999–2007*
No wage selection adjustment	–0.116	–0.7	–0.8	0.051	2.8	0.3
	(0.025)			(0.017)		
Wage imputation based on weeks worked	0.762	4.7	0.2	0.639	7.7	5.1
	(0.015)			(0.009)		
Full-time, full-year weekly wages	0.019	–1.1	–0.4	0.030	2.1	–0.2
	(0.005)			(0.022)		
Hourly wages	0.056	–1.7	–1.1	–0.069	2.5	–0.4
	(0.008)			(0.036)		
Probit for employment equations	0.175	–1.5	–1.3	0.025	2.3	0.0
	(0.024)			(0.042)		
	(0.063)[b]			(0.009)[b]		

Source: Author's regressions.

a. Numbers in parentheses are standard errors except where noted otherwise.

b. Probit marginal effect.

more, and then imputed wages to nonworkers from the less-than-20-weeks regression. The present study estimated separate wage regressions for those with less than 20 weeks worked, those with 20–39 weeks worked, and those with 40 or more weeks worked. Wages of nonworkers are imputed from the predicted values from the less-than-20-weeks regression, and wages for workers are predicted from a fixed-weighted average of the wages predicted from the three regressions, using as weights the fractions of the sample that worked less than 20, 20 to 39, and 40 or more weeks in 1999.[33] As the table shows, this method yielded much larger wage coefficients than did the conventional method. The predicted changes in employment probabilities are always positive and considerably further from the actual changes than the predictions from the conventional selection bias method.

The third row shows the effect of restricting the sample in the wage equation to workers with 40 or more weeks of work per year and at least 35 hours worked per week, a common method of eliminating variation in weeks and hours worked from the wage measure and obtaining something closer to an hourly wage (see, for example, Acemoglu and Autor 2011). The method runs the danger of selecting on an endogenous variable (weeks of work) but, perhaps more important, results in a rather restricted set of observations for the wage sample.[34] In some of the age-education cells, the fraction of the sample that is included in the wage regression is less than 10 percent, and very often it is less than 25 percent (recall that observations with allocated earnings are also excluded). This not only makes selection bias adjustments more fragile but also leads to imputing wages from what may be a rather atypical sample to the full sample. This method yields somewhat smaller wage elasticities for men and somewhat larger ones for women. The predicted changes in employment are slightly worse for men and very slightly better for women.

The fourth row shows the effect of using log real hourly wages instead of log real weekly wages in the model, using the CPS variable for average hours worked in the preceding year. The male wage coefficients and employment predictions from this model are very close to those using the weekly wage, but the results for females are quite different, with a negative wage elasticity. This may be the result of a bias related to division bias. The employment status predictions are slightly better than in the main model, mostly because wages of women were rising over the period while

33. I thank Steven Davis for suggesting this method.
34. I thank Steven Davis for emphasizing this point.

employment rates were falling, and a negative wage elasticity can explain this better than a positive one.

The final row shows the effect of using a probit model for the first-stage reduced-form employment status regression and for the final employment status regression containing the predicted wage. The results are quite similar to those using the main model, which used OLS.

ACKNOWLEDGMENTS Comments from David Autor, Steven Davis, Alexandre Mas, the editors, and the Brookings Panel participants are appreciated. Nicole Lott and Lu Xu provided excellent research assistance.

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Comments and Discussion

COMMENT BY

STEVEN J. DAVIS U.S. employment rates drifted down from 2000 to 2007, fell precipitously in the wake of the financial crisis and recession of 2008–09, and showed little or no sign of recovery as of this writing in late 2012. These developments fully erased the large employment rate gains achieved in the 1980s and 1990s. They constitute a very serious setback in economic performance, with long-term negative repercussions for human capital, real wages, living standards, and tax revenue net of government transfers.

In this paper Robert Moffitt focuses on employment rate changes from 1999 to 2007 and from 1989 to 1999. The selection of time periods reflects a desire to examine developments between business cycle peaks so as to highlight longer-term forces. Table 1 of the paper shows widespread employment rate declines across age-education groups from 1999 to 2007 for men, with steeper declines for the younger and less educated. The only exception to the downward drift among men is the essentially flat employment rates for the college educated aged 25 and older. Women aged 16–24 and 25–39 also show notable employment rate declines from 1999 to 2007, especially among the less educated. In contrast, women aged 55–64 experienced sizable employment rate gains across education groups. On the whole, men experienced larger employment rate declines than women from 1999 to 2007, but the decline relative to pre-1999 trends was greater for women.

After documenting these facts, the paper turns to simple empirical models that relate employment rate changes to real weekly earnings, nonlabor income, marital status, and family structure. This part of the paper produces few robust conclusions about the proximate determinants of employment rate changes. As it turns out, the chief empirical results are highly sensitive to the wage measure and, for women, to basic aspects of the regression specification.

The specification sensitivity for women comes through clearly in a comparison of tables 6 and 9. When the same regression specification is imposed on all women, the fitted empirical model accounts for none of the overall drop in female employment rates between 1999 and 2007 (table 6). However, letting the regression coefficients vary by marital status and, for unmarried women, by the presence of children yields a very different picture. The predicted changes implied by the flexible specification account well for the actual employment rate changes for married women and for unmarried women without children over 1989–99 and over 1999–2007 (table 9).

The performance of the flexible specification for unmarried women with children is harder to assess. Moffitt writes that the flexible model "does a poor job of explaining the small decline in employment for unmarried women with children, however, predicting instead an increase of some magnitude." But it is unclear whether the differences between the actual and the predicted changes for this group are statistically significant. Both quantities in these comparisons are subject to sampling variability. The samples for unmarried women with children are smaller than for the other groups in table 9 (personal communication with the author). The table reports a large standard error on the estimated wage elasticity for this group, and a footnote to the discussion of table 5 suggests that the reported standard errors are too small in any event. In sum, the flexible specification appears to perform well in accounting for changes in women's employment rates from 1989 to 1999 and from 1999 to 2007—with the possible exception of unmarried women, for whom the evidence is not very informative.

These results hold when using the wage measure featured in the main text. A key challenge in constructing the wage measure is how to handle nonworking persons, for whom there is no contemporaneous wage observation. Ignoring this issue would lead to potentially biased wage measures—and biased estimates of employment responses—because changes over time in observed wages may not accurately reflect changes over time in market opportunities for nonworking persons or for all persons. To address this issue, the main text uses the predicted wage for employed persons from a wage equation that incorporates a selection correction for employment status. The selection correction is identified by excluding nonlabor income and the two variables relating to number of children from the wage equation, while including these variables in the employment probability equation. Both equations contain other controls.

The paper says little about why these selection-corrected wage measures could be expected to adequately adjust for changes over time in the market wage opportunities of nonworking persons. For unmarried men, it seems unlikely that the children variables provide much leverage for estimating the selection correction. For women, it is useful to recall that the underlying wage measure is average weekly earnings during the calendar year, so the wage measure is affected by the number of hours worked per week. The presence of children, especially young children, is likely to have a strong effect on desired work hours and work intensity for many women, and therefore on their set of relevant market wage opportunities. Thus, I do not see a good case for excluding the children variables from the wage equation for women. In future work that uses selection-corrected wage measures to explain employment changes over time, I hope to see a vigorous defense of the exclusion restrictions and a fuller case in favor of the resulting selection-corrected wage measures.

The paper also considers an approach that imputes wages to nonworkers based on the wages of persons with 1–19 weeks of work during the year.[1] Appendix table A.4 reports selected results for this alternative wage measure. For both men and women, it yields estimated wage elasticities that are an order of magnitude larger than the ones reported in the main text (tables 5 and 9). That is, the most important response coefficient in the model is extremely sensitive to the construction of the wage measure. It is unclear what to make of this sensitivity.

There is another head-scratching aspect of table A.4. Recall the generally downward movement in men's wages from 1999 to 2007 for the wage measure featured in the main text (top panel of table 4). This pattern coupled with the large wage elasticity reported in the second row of table A.4 leads me to expect a strong negative predicted employment rate change for men from 1999 to 2007. Instead, the second row of table A.4 reports a positive predicted change over this period. It is puzzling that the alternative wage measure yields a much larger estimated wage elasticity in the employment rate regression *and* generates a much smaller predicted employment rate change. More work is needed to understand what lies behind this result, even at a mechanical level.

Section V of the paper considers several additional factors that are potentially important for understanding longer-term employment rate changes. I share Moffitt's view that the Supplemental Nutrition Assistance

1. See the appendix to the paper for a full explanation of how wages are measured under this approach.

Program, the Social Security disability insurance program, and rising rates of incarceration and imprisonment have had potentially important effects on the employment rates for some demographic groups, and that their roles warrant further study. More generally, I see a strong need for additional research to improve our understanding of the reasons for the worrisome declines in U.S. employment rates during the 21st century.

COMMENT BY

ALEXANDRE MAS Why did job growth slow before the Great Recession? Between 1999 and 2007 the employment-to-population ratio fell by 1.3 percentage points. For prime-age men the decline in the ratio appears to be roughly a continuation of a 30-year trend, but the decline among young adults accelerated, and the female employment rate declined slightly after decades of increases. Understanding these changes may help clarify the current labor market situation. If the factors that caused this decline were still present after 2007, it could help explain the slow labor market recovery from the Great Recession. Yet surprisingly little research has been done on this question.

This paper by Robert Moffitt makes a significant contribution by documenting the patterns of wages and employment rates over the 1989–2007 period and interpreting these patterns within a labor supply framework. The focus on labor supply in explaining aggregate movements in employment puts this paper at the heart of the current macroeconomic debate. Its empirical methodology draws from a long literature on the estimation of labor supply functions. Such a paper is of value, particularly if, like this one, it is systematic, transparent, and carefully executed. I expect that this paper will be influential.

The goal of the paper is to assess whether a "standard" formulation of the static labor supply model on the extensive margin can explain the change in the employment-to-population ratio over the 1989–99 and 1999–2007 periods. In the most stripped-down model, the employment decision depends on individual taste and on a set of variables that determine the labor supply decision, such as family structure, nonlabor income, and the wage. Implicit in the model is an assumption that there is a distribution of reservation wages. Workers seek (and successfully obtain) employment when the wage rises above their reservation wage, or when their reservation wage falls, for example following marriage or childbirth. The market is always in balance, and demand shocks affect a worker's decision to participate through the wage alone. For example, if a worker is out of the labor force following a shock to the manufacturing sector, that outcome results from the individual's optimization in response to a lower available wage. During booms people choose to work because they are rewarded for this effort. During slumps there is a lower reward for work, so some people choose leisure.

Movements along an aggregate labor supply curve are certainly not the only explanation for why the employment-to-population ratio may have fallen. Unemployment can arise if workers are off their supply curve because of disequilibrium in the labor market or because the market is imperfect. Indeed, the assumption that demand shifts affect employment only though the wage is quite strong given the available evidence (for example, Blanchard and Katz 1992) that wages take time to adjust to shocks. However, a complete examination of all the alternative hypotheses is beyond the scope of this (or any) study. A careful examination of the labor supply hypothesis alone is of value.

This paper is most closely related to the literature on the secular downward trend in the male employment rate over the 1970s and 1980s (such as Juhn 1992 and Juhn, Topel, and Murphy 1991).[1] These studies have employed a supply-and-demand framework to analyze changes in wages and measures of labor supply to explain aggregate changes in labor force participation and employment. The secular decline in male labor force participation has many possible explanations, but the patterns of wages and employment are suggestive of a labor supply explanation: over 1970–80, those education-experience subgroups who experienced relative wage declines also had relatively lower participation.

At first blush it does not appear that this explanation can account for the decline in the employment-to-population ratio over the 1999–2007 period, particularly for women. Demographics and measures of family structure did not change very much over this period, and for women the average wage increased somewhat. What casual observation misses, however, is that the observed wage is not necessarily the same as the wage offer available to the nonemployed, which is unobserved. Therefore, any conclusion that is not based on careful econometric analysis that explicitly accounts for the wage of nonworkers is incomplete. However, it is

1. It is also related to the literature on rising female labor force participation (for example, Blau and Kahn 2007), although that literature has tended to emphasize changes in labor supply parameters over time, whereas this paper considers whether a model with fixed parameters has explanatory power.

not straightforward to impute these wage offers, and that is at the heart of what this paper does.

The paper's main analysis has three components. The first is to impute offer wages for nonworkers in the Current Population Survey, the second is to estimate the parameters of a labor supply model, and the third is to use these estimates to predict the effect of changes in demographic composition, family structure, nonlabor income, and wages on the employment-to-population ratio. I will comment on each of these.

The paper considers three different approaches to measuring wages. The most basic approach assigns the average weekly wages of full-time, year-round workers in an education-age-race-sex cell to all individuals (employed or not employed) in the same cell. The obvious drawback to this procedure is that nonworkers may be very different from fully employed workers, and it is this problem that the other approaches seek to address. The first of these is a semiparametric version of the Heckman selection model, and the second is based on Francine Blau and Lawrence Kahn (2007) and Chinhui Juhn (1991). Both have their disadvantages. I will discuss some of these below, with the caveat that these critiques are not specific to this paper but apply to the methodology employed by the broader literature. This paper's objective is not to improve on the methods used to estimate labor supply relationships at the extensive margin, but to assess whether existing methods can successfully explain the wage and employment patterns observed in the data. Employing multiple approaches is of considerable value precisely because each of them has its drawbacks.

The first step of the selection model is to fit a linear probability model of employment as a function of a set of demographic characteristics. In the second step the predicted probability of employment is entered flexibly into a wage equation that includes all of the variables from the first-stage model other than nonlabor income and number of children. This is a commonly employed correction, but it relies on the assumption that nonlabor income and the number of children in a family are independent of the wage, conditional on the explanatory variables in the model. This exclusion restriction must be accepted as a matter of faith; however, one can tell stories for why this assumption may not be justified. Nonlabor income as defined in this paper is a function of past accumulation of wealth, and workers may have higher nonlabor income precisely because they have (and have had) a high wage. There is also a literature that emphasizes the wage gap between women with and without children (for example, Waldfogel 1998). Such a gap could come about because the benefit of having a job with a more flexible schedule compensates for the lower wage. So although the approach may produce unbiased estimates of the mean wage, the assumptions necessary to accomplish this give one pause.

The second procedure, based on the approach of Blau and Kahn (2007) and Juhn (1992), involves estimating separate wage equations for workers with less than 20 weeks worked, 20 to 39 weeks worked, and more than 40 weeks worked. The wages that nonworkers would receive if they were working are assigned according to the predicted values from the regression for the first group, while the wages for workers are assigned as a weighted average of the predicted wages of the three models based on the weeks-worked share in the education-age-race-sex cell. This approach results in very high employment elasticities with respect to the wage for both men and women, but as Moffitt notes, this correlation may be mechanical. Average weekly wages for workers who work less than 20 weeks per year are lower than those who work more weeks. Within an education-age-race-sex cell, this procedure assigns a lower wage to nonworkers than to workers, and it appears that at least some of the higher estimated wage elasticity is a result of this difference.[2] A second issue with this approach is that it is not clear that the wages of nonworkers are best approximated by the relatively low wage earned by workers working less than 20 hours per week, particularly for women. Caveats aside, the Heckman method shows some evidence that on average, female nonworkers would obtain higher wages than workers if they were in the labor market (that is, workers are negatively selected).

The Heckman approach shows that weekly wages for females increased by about 7 percent in real terms between 1999 and 2007, with the increases occurring broadly across experience and education groups. For men, imputation matters quite a bit. Unadjusted constant-dollar weekly wages for men stayed roughly constant between 1999 and 2007 (male median weekly wages in the Current Population Survey declined by about half a percent in real terms over this period), but imputing the selection-adjusted wage to nonworkers results in a 10 percent decline in the male weekly wage over this period. A decline of this magnitude can account for a substantial share of the decline in employment even with a small wage elasticity.

2. I suggested this approach, which has been frequently employed in the literature, in my discussion of this paper at the Brookings Panel conference without fully appreciating the potential for this automatic correlation. In this paper this procedure is conducted as a sensitivity check and is not the preferred method. In personal correspondence Robert Moffitt indicated the potential for a mechanical relationship to me.

Once the wages of nonworkers have been imputed, the next step is to estimate the parameters of the labor supply model. Although the model is estimated with microdata, in the paper's main approach wages are constant within education-age-race-sex cells, so this is effectively a cell-level model. The paper implicitly assumes that changes in the wage within these groups are due to group-specific demand shifts along a stable supply curve, and not to shifts in the labor supply function or other factors. This is not an innocuous assumption.

The endogeneity of the wage presents a problem for the two-step estimation procedure, which is not designed to be internally consistent. Within this framework any "unexplained" change in employment could be the result of a shift in the labor supply function, but then the employment-wage relationship that was estimated to predict employment is not necessarily a labor supply curve. For example, one of the paper's conclusions is that the reduction in the employment rate among women over 1999–2007 is largely unexplained, suggesting that there could have been a leftward shift in female labor supply over this period (indeed, female wages rose). One indication that there might be an issue is that the estimated elasticities vary quite a bit across specifications and samples and sometimes have puzzling patterns. For example, in the paper's table 5 it appears that female labor supply is inelastic, whereas labor supply for men has a positive (but small) elasticity. The estimated elasticity for unmarried women (table 9) is negative. It is interesting that in most specifications the estimated wage elasticity is small for both men and women. This could be due to bias or, taking the estimates at face value, to the wage changes occurring over this period being viewed as highly persistent, thus leading to income effects.

Although estimating wage elasticities is difficult, the focus on the substitution effect is something of a red herring for explaining changes in female labor supply on the extensive margin, since their wages were rising somewhat. One angle for future research is to consider the entire distribution of wages rather than just the mean. If there is a mean-preserving spread in wages in an education-age-race-sex cell, the model will predict no change in employment because of wage changes for any wage elasticity. But workers whose wages are declining may still be withdrawing from the labor force, resulting in a lower employment-to-population ratio on average. It would be worthwhile to consider not just the changes in mean wages but changes at lower percentiles of the residual distribution as well.

Perhaps this paper is trying to do too much by both estimating the labor supply parameters and using those estimates to predict aggregate employment changes in the same sample. A less ambitious but still worthwhile alternative would be to impose a set of parameters, or at least a range of wage elasticities, to simulate whether changes in wages and family characteristics are large enough to explain the changes in the employment-to-population ratio.

The paper proceeds to use the estimated parameters to predict changes in employment as a function of worker characteristics. It does this in a disciplined way, using the same parameters to assess the 1989–99 period as for the 1999–2007 period. One of the main conclusions is that although the decline in the employment-to-population ratio can be partly explained in the case of men, the labor supply factors do not predict the decline in the employment rate for women in the 2000s. In fact, the observed shifts in demographic characteristics, family structure, and wages largely point toward an increase rather than a decrease in aggregate employment. The decline in female employment, although small, is a reversal of a decades-long secular increase. This is very interesting and raises a number of questions for future research. Has the secular increase in female labor supply come to an end? Are there demand factors that were missing in this analysis? Were there unobserved wealth shocks? Was there a shift toward household production? Why were the declines in employment greatest among unmarried women? These are some of the questions raised by this paper that I hope will motivate future research.

Another fascinating trend highlighted by this paper is the decline in the employment-to-population ratio of younger workers. The magnitude of this shift suggests that something significant changed in the early 2000s. This paper shows that school attendance been on the rise, particularly for women, suggesting a shift from work toward other productive activities. On the other hand, educational attainment has increased much more slowly. Some studies have analyzed this shift (for example, Aaronson, Park, and Sullivan 2007), but the factors underlying the declines in the employment-to-population ratio for the group aged 16–24 deserves more attention.

I will not say much on the role of changes in transfers and taxes, but I will note that the analysis here is thorough and quite convincing. The paper observes that changes in tax and benefit programs over the 1999–2007 period should not have motivated workers to leave the labor force in large numbers. There were, however, major changes in benefits programs and tax rates in the mid- to late 1990s, including welfare reform and the expansion of the Earned Income Tax Credit. These changes likely led to increased labor force participation in the late 1990s, particularly among single women with children. These policy-driven gains in employment during a booming labor market for a group of workers who had been only marginally attached to the labor market may have been wiped out following the 2001 recession. A related question for further consideration is whether the decline in the employment-to-population ratio reflects the fact that 1999 and 2007 were not truly comparable points in the business cycle. Both these years are business cycle peaks, but GDP growth was more rapid in 1999 than in 2007. So although the focus of this paper is on trends, perhaps the observed decline in the employment-to-population ratio reflects some degree of cyclical variation in employment.

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GENERAL DISCUSSION Martin Feldstein wondered how single people who were out of the labor force for an extended period derived an income. Given Steven Davis's emphasis on the large and growing number of incarcerated persons, Feldstein speculated that criminal activity might account for much of that unexplained income.

Valerie Ramey suggested two factors at opposite ends of the age continuum that might help explain the drop in the employment-population ratio. The first was the increase in the ratio of elderly people to prime-age workers. Ramey suggested that within an extended family, the task of caring for the sick and the elderly tends to fall on the women, and in particular on those with the least favorable job opportunities, who then substitute income from interfamily transfers—perhaps a larger share of any inheritance—for income from formal sector work. The second factor related to what she and Garey Ramey in a 2010 Brookings Paper had called the "rugrat race": a 2007 study by Shirley Porterfield and Anne Winkler in the *Monthly Labor Review* found that teenagers are increasingly substituting volunteer opportunities for paid work in order to pad their résumés for college application.

Robert Hall reasoned that including an endogenous wage variable in an equation explaining changes in the employment-population ratio made econometric sense only if one assumed that all the disturbances in that ratio were on the demand side. But given that the most obvious explanation for the paper's findings was shifts in labor supply within demographic groups, that assumption seemed to him invalid. A reduced-form model that simply dropped the wage term and treated the other right-hand-side variables as exogenous, Hall argued, would avoid both that problem and the problem of measurement error in wages that the discussants had noted.

Bradford DeLong agreed with Davis that it was unlikely that the observed wage differences during 1999–2007 fully captured the shift in labor market opportunities as perceived by workers. He thought that this failure might be particularly large for those in households with two potential earners, in which labor supply decisions are affected not only by the wages each partner can command but also by each partner's likelihood of finding a good job match. DeLong also thought it significant that Moffitt had not found any determinants of the fall in the employment-participation ratio between 1999 and 2007, and hence could not use any such relationship to find a changing factor whose acceleration after 2007 could be invoked to explain the decline in the post-2007 employment-population ratio.

Michael Klein wondered whether Feldstein's point might be broadened to suggest that a shift of workers to the underground economy could explain much of the decline in the employment-population ratio. Kristin Forbes corroborated, reporting anecdotally that it was becoming difficult in Boston to hire nannies and housekeepers who would accept being paid above the table. Typically, the reason was not that the prospective hires wanted to avoid taxes, but rather that they wanted to maintain eligibility for certain small-scale welfare programs. Some of those programs, Forbes noted, were state programs and thus would not be reflected in the macro data that Moffitt used.

David Romer sided with Hall in arguing that it might be simplest to omit the wage rate from Moffitt's equation, but on different grounds. He pointed out that the change in average wages had been quite small in recent decades, such that if it were responsible for substantial changes in labor force participation, the implied labor supply elasticity would have to be so large as to imply that one hundred years ago no one would have chosen to work.

Hilary Hoynes predicted that the paper would stimulate much further work on the topic, as it showed that the decline in the employment-population ratio did not have one single overarching explanation but rather was the combined result of different factors affecting different groups—young and old, men and women, married and unmarried, and so on. Although she considered herself a labor economist and therefore, according to Moffitt's distinction, a student of trends, she was surprised that the paper had nothing to say about the "jobless recovery" during the period in question. Hoynes noted that the decade of the 2000s differed from other business cycles in that the 2001 recession produced only a small decrease in the employment-to-population ratio, yet the ratio never recovered.

Scott Winship thought the paper could benefit from distinguishing between trends in labor force participation and trends in the employment-population ratio. The former trend, he suggested, might be explained fairly simply by demographic changes. Since 2000, labor force participation for men aged 25–54 had tracked closely the long-term secular decline in participation for that group; for men aged 16–24 the participation rate had declined significantly as the school enrollment rate had risen sharply; and for men aged 55–64 the participation rate followed the long-term trend until 1996, and then rose as many college-educated baby-boomers began deciding to postpone retirement. For women, Winship argued, the story was also likely to be relatively simple. Once the trends in labor force participation were thus explained, all that remained to account for the trends in the employment-population ratio was to explain the trends in the unemployment rate, which might require a different approach.

Erik Hurst reported findings from his own research using regional variation to explain changes in the employment-population ratio in the 2000s: regions that experienced a housing boom during the 2000–07 period also experienced faster labor force participation growth during that period. Conversely, those regions experiencing a manufacturing decline during the 2000–07 period experienced large labor force participation declines. The two effects roughly offset each other in the aggregate statistics during the boom years. However, when the recession hit, the housing jobs went away. These findings led Hurst to conclude that if the housing boom had not occurred, the effect of the manufacturing decline on aggregate employment during the 2000–07 period would have been even larger, particularly among the less skilled

Iván Werning noted that Moffitt's model erred in predicting the participation rate for women and for men about by the same absolute magnitude. To him this hinted that some explanation common to both men and women might underlie the discrepancy.

Robert Gordon argued that the paper needed to control for the intensity of aggregate demand, which had differed between 1989 and 1999 and between 1999 and 2007. He suggested that a combination of three factors explained why the labor market was weaker in 2007 than in 2000: a somewhat higher unemployment rate, the lower labor force participation rate that had already been mentioned, and stagnant real wages. To Gordon, the relatively larger change in labor force participation than in unemployment indicated that workers' decisionmaking about whether to keep looking for a job had structurally changed. Finally, Gordon pointed out that because the paper defined the labor force participation to exclude workers older than 64, it could not explain the effect of baby-boomers' retirement decisions.

Returning to the subject of youth employment, Christopher Nekarda reported that his colleague at the Federal Reserve Board, Christopher Smith, had found the decreased employment among teenagers since the mid-2000s to be due in equal parts to supply and to demand factors. On the supply side was rising school attendance, as Winship had noted, and on the demand side, teenagers were facing increased competition with immigrants and adults for the jobs that teenagers had traditionally held. Nekarda also suggested looking at the labor polarization literature to incorporate demand-side effects.

Robert Shiller offered another possible supply-side factor: the decline in labor unions might have led to a decrease in the attractiveness of many jobs to workers, as employers exploited their own increased bargaining power by eliminating nonpecuniary benefits such as coffee breaks.

Henry Aaron agreed that labor market conditions had changed significantly over the time period the paper studied, with relatively tight demand before about 1999 and persistent looseness of demand thereafter. He thought decisions about whether to seek work might be influenced by past as well as present labor market conditions, and he suggested incorporating some measure of the former into the analysis.

Benjamin Kay asked whether the paper's measure of wages included health care and other nonpecuniary benefits. He suspected that total compensation increased rather than decreased when these are included, so that the observed response of employment was actually of the wrong sign. He conceded that the income elasticity of labor supply might differ for benefits than for money wages, but if so, the paper should address that.

The discussion thus far prompted Steven Davis to clarify his criticism of the paper's procedure for imputing wages: the paper did not impute wages only to those without jobs; it imputed them for all who did not work full-time, full-year. The result was that wages were imputed rather than obtained directly for from 43 to 99 percent of all individuals in some groups. He saw this as a first-order issue apart from whether or not wages were the appropriate right-hand-side variable. On the latter question, Davis added that earlier models that incorporated wage changes—for example, that in a 1991 Brookings Paper by Chinhui Juhn, Kevin Murphy, and Robert Topel—had been successful at predicting employment changes.

Responding to the discussion, Robert Moffitt agreed with Davis's last point and added that others, such as Claudia Goldin, had used the wage model successfully in studying the employment behavior of women, both in the aggregate and for women of different skill levels. But he conceded that it is obviously not a sufficient model since demand fluctuations do have an effect. He pointed out further that much of the literature on income inequality, in particular the work of Lawrence Katz, David Autor, and Thomas Lemieux, also takes a demand-driven approach.

Moffitt also agreed with Davis's comment in his formal discussion that the prison population needed to be accounted for, although he surmised that present incarceration was a less important factor than past incarceration: incarceration likely tends to remove the least qualified job seekers from the labor pool, thus raising the average qualifications of the remainder; on the other hand, it was well known that former inmates have great difficulty finding jobs. Finally, Moffitt said he would consider how to address Aaron's observation about the recent change in labor markets from hard to soft but did not immediately see a way to incorporate it into his model.