CHANGES IN MALE LABOR SUPPLY AND WAGES

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In the 1980s, both wages and labor supply of poorly educated men fell substantially relative to those of educated men. Some observers have interpreted this positive association between changes in wages and labor supply as reflecting movement along stable labor supply curves. The author casts doubt on this interpretation by showing that the wage elasticity necessary to account, by itself, for the observed labor supply decline would greatly exceed elasticity levels typically found in prior studies. Analysis of Census data shows little relationship between changes in relative wages at the state level and changes in male labor supply. Also, panel data analysis shows no strong correlation between long-run changes in individual hours and wages. The small implied labor supply elasticities suggest that very little of the labor supply changes of men during the 1980s can be related to changes in relative wages.

Recent decades have seen a large decrease in the labor market participation of less-educated men in the Unites States. During the same period, the wages of less educated men have declined relative to the wages of women and other men (Levy and Murnane 1992; Bound and Johnson 1992). The contemporaneous correlation of changes in relative wages and relative participation has led researchers to speculate that the two phenomena are related. The hypothesis expounded by Juhn (1992) and Welch (1997) is that the declines in participation experienced by less educated and low-wage men are due to the wage declines experienced by these groups: as different socioeconomic groups have moved along stable labor supply curves, the groups with large wage declines have responded to those declines by substantially reducing their participation.

However, changes in relative wages are unlikely to be the only factor influencing changes in labor supply. Other possible explanations for declining labor market participation of men are declining marriage rates, increases in the wages and labor supply of women, more generous welfare packages, greater payoff from crime, and an increase in the disutility received from work. Changes in these factors are unlikely to be uniform and may have a greater impact on less skilled men. Thus, the contemporaneous declines in wages and participa-

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A data appendix with additional results and copies of the computer programs used to generate the results presented in the paper are available from the author at the Department of Economics, University of California, Los Angeles, 405 Hilgard Ave, Los Angeles 90095.

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tion for less educated men may be related to any of these forces.

This paper uses both repeated cross-sections from Census data and individual-level panel data to estimate the labor supply response of white men to long-term changes in wages. These data allow me to relax some of the assumptions made in previous literature and to use within-group identification of labor supply responses. Previous papers have studied this issue by fitting cross-sectional labor supply functions to the changes over time. Because cross-sectional approaches have many problems, I take two alternative approaches. First, if increases in wage inequality have led to a decline in the labor supply of unskilled men relative to the more skilled, then states with larger increases in inequality should also have experienced greater relative declines in labor supply of unskilled men. I use Census data to test for the presence of such a pattern. Second, one implication of the hypothesized strong link between wages and labor force participation is that individuals with large relative increases in wages will also have large relative increases in labor supply. I test that prediction using panel data from the Panel Study of Income Dynamics (PSID). In the analysis, I focus on changes during the 1980s, because changes in relative wages were much larger in that decade than in the 1970s or 1990s.

Understanding the labor supply responses to changes in relative wages is important for at least two reasons. First, to understand the effects of changes in tax laws on labor supply, knowledge of labor supply elasticities is necessary. Estimates of wage elasticities for men in the literature have tended to be very small and even negative. The implied elasticities if relative labor supply changes during the 1980s arose solely from movement along labor supply curves are much larger than those typical estimates, and they imply large effects of tax changes on male labor supply. Second, from the perspective of labor supply estimation, these issues are important because sources of exogenous wage variation are invaluable in estimating labor supply elasticities. Thus, if changes in relative wages can be treated as exogenous to labor supply, they provide a promising source of identification in future research. Indeed, they have been used for this purpose already (see, for example, Blundell, Duncan, and Meghir 1998; Pencavel 1997). The analysis in this paper sheds light on whether this assumption is likely to be satisfied and, hence, on whether one can confidently use long-term changes in relative wages to identify labor supply responses.

Empirical Methodology

Using Cross-Sectional Variation

The first approach is to use Census data to examine whether states that have had the biggest changes in relative wages have also had the biggest changes in relative hours per worker. It is well known that there are substantial differences across Census regions (see, for example, Karoly and Klerman 1994; Topel 1994) and across metropolitan areas (Borjas and Ramey 1995) in the level and trend of measures of wage inequality.[1] This is also true at the state level. The approach I use is to see how this cross-state variation in relative wage changes across groups corresponds to the cross-state variation in relative labor supply changes. The advantage of this approach is that one can allow the labor supply of different groups of workers to have been affected in different ways by unobservables over time. Consider the labor supply of group g at times t and $t+10$ in states defined by j:

$$(1)\qquad \begin{aligned} h_{gjt} &= \beta w_{gjt} + f_{gt} + \mu_{gj} + \eta_{jt} + \varepsilon_{gjt} \\ h_{gjt+10} &= \beta w_{gjt+10} + f_{gt+10} + \mu_{gj} + \eta_{jt+10} + \varepsilon_{gjt+10} \end{aligned}$$

[1]Topel (1994) found that changes in relative wages in the West between 1972 and 1990 were about 50% greater than changes in the Northeast and three times greater than changes in the South. Borjas and Ramey (1995) showed that changes in relative wages were variable across metropolitan areas between 1976 and 1990, with, for example, the return to education increasing sizably in Los Angeles but only very slightly in Pittsburgh.

Now, by differencing:

$$(2) \qquad (h_{gjt+10} - h_{gjt}) = \beta(w_{gjt+10} - w_{gjt}) + (f_{gt+10} - f_{gt}) + (\eta_{jt+10} - \eta_{jt}) + (\varepsilon_{gjt+10} - \varepsilon_{gjt})$$

Thus, by including fixed group effects and state effects in the differenced regression, one can get a consistent estimate of β in the case where the group-specific effect is not fixed and can differ arbitrarily across groups and over time. Note that if there were no cross-state variation, one could not allow the group-specific effect to change over time. One must make the assumption that any state-group effect is constant across time. However, it is not necessary to assume that the state-group effect is uncorrelated with wages, as it is differenced out of the regression. Thus, this methodology is robust with respect to mismeasurement of cost of living changes, to changes in federal tax rates, and to changes in nonwage benefits across groups. The state effects capture any state-specific changes—the state business cycle, for example, or state-specific changes in the cost of living—that may affect labor supply changes. They also capture any fixed differences in group composition across states.

Researchers in the labor supply literature have drawn a distinction between labor supply responses to shifts in wage profiles and labor supply responses to movements along a given wage profile.[2] Long-term trends in relative wages represent shifts in the career wage profiles faced by different kinds of individuals. The identification scheme here is appropriate because the labor supply responses are identified using variation in shifts in wage profiles across states. Further detail about the Census data and the estimation methodology is provided later in the paper.

[2]Blundell and MaCurdy (1999) and Pencavel (2002) provide thorough accounts of how the interpretation of the labor supply elasticity depends on the estimation specification.

Using Panel Data from the Panel Study of Income Dynamics

This approach involves using panel data to examine whether, within groups, the people who have had the biggest increases in wages have also had the biggest increases in hours. The wage change measure is the change in the wage over a ten-year period. The emphasis on long-term changes is important because it is likely that the behavioral responses to short- and long-term wage changes are quite different. The changes in the labor supply measures are over the same ten-year period. As with equation (2), one can allow the labor supply of different groups of workers to have been affected in different ways by unobservables over time. Consider the labor supply of person i who is a member of group g at times t and $t + 10$:

$$(3) \qquad h_{igt} = \beta w_{igt} + f_{gt} + \mu_{ig} + \varepsilon_{igt}$$
$$h_{igt+10} = \beta w_{igt+10} + f_{gt+10} + \mu_{ig} + \varepsilon_{igt+10}$$

Now, in differences:

$$(4) \qquad (h_{igt+10} - h_{igt}) = \beta(w_{igt+10} - w_{igt}) + (f_{gt+10} - f_{gt}) + (\varepsilon_{igt+10} - \varepsilon_{igt})$$

Thus, by including fixed group effects in the differenced regression, it is possible to get a consistent estimate of β in the case where the group-specific effect is not fixed and can differ arbitrarily across groups and over time. A necessary assumption is that the individual effect is constant across time. This methodology is robust with respect to mismeasurement of cost of living changes but may be sensitive to changes in federal or state taxes that differ among individuals within groups. Given that the control variables include a quartic in actual labor market experience, the identification here is from a mixture of wage profile shifts and movement along a given profile. That is, some of the individual-level wage changes will reflect permanent changes in the individual's earning capacity, and other changes will reflect movement along the individual's career wage profile. Since the labor supply elasticity from the latter should exceed that from the former, the estimates from equation (4) will tend to overstate the labor supply effects of changes in relative wages.[3] Further details about the PSID data and the estimation methodology are presented later in the paper.

Wages and Participation in the Census Data

The Census data I use come from the 5% samples of the 1980 and 1990 Censuses. I restrict the sample to individuals who are aged between 20 and 60. Persons in school or the military during the survey week are omitted, as are persons who are living in group quarters. I exclude persons who have self-employment income. The wage measure used is average hourly earnings, where earnings include wage and salary earnings only.[4] Earnings are topcoded at \$75,000 in 1980 and \$140,000 in 1990. Half of 1% of observations are topcoded in 1980, and 0.8% are topcoded in 1990. I replace the topcoded value by the topcode times 1.33 in both years. I restrict the sample to white men.[5]

A generic problem in labor supply estimation arises because the market wage is not observed for non-participants. A large proportion of the relative changes in hours and weeks worked across different groups arises because of changes at the extensive rather than the intensive margin. My approach to this problem is similar to that of Juhn (1992). I impute wages of nonworkers by using the wage distribution of workers who work 1–13 weeks. First, individuals are placed in one of 20 age-education groups. These are defined by the interactions of 5 education groups (high school dropout, high school graduate, some college, college graduate, more than college) and four age groups (21–30, 31–40, 41–50, 51–60). I impute the wage for non-workers as being the average wage of individuals who worked 1–13 weeks in that group in that state. For 0.2% of non-workers, there is no person who works 1–13 weeks in that group in that state. For these individuals, I impute wages as being the average wage of persons who work 1–13 weeks in that group in that Census division. Juhn (1992) demonstrated that in the CPS this approach to imputation appears to work well.

The labor supply measures refer to the hours worked in the previous calendar year and the wage measure is the log of average hourly earnings in the previous calendar year. I include four different labor supply measures. The first, HOURS, is annual hours worked in the previous calendar year. The second, WEEKS, is the number of weeks worked in the previous calendar year. The third, Full Time/Full Year (FTFY), is the proportion in the group who worked at least 50 weeks and usually worked 35 or more hours per week. The fourth, ANNUAL PARTICIPATION, is the proportion of people in the group who worked for even one hour in the year. The value of using several participation measures is that each captures a different facet of labor supply and each has its own strengths and weaknesses. Rather than rely on one measure, I prefer to draw inferences from the effects of wages on all four. Like other researchers in this literature, I make no distinction between periods of non-employment that are classified as unemployment and periods spent out of the labor force.

There were some substantial differences in changes in labor supply across states during this period. Wyoming and West Virginia had large decreases in labor supply, with average annual hours worked falling by 146 hours in Wyoming and by 80 hours in West Virginia between 1980 and 1990. Average hours worked also fell in New Mexico, Oklahoma, Louisiana, North Dakota, Texas, Montana, and Kansas over

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[3]Browning, Deaton, and Irish (1985) showed that the intertemporal labor supply elasticity is greater than the Marshallian wage elasticity that corresponds to shifts in wage profiles.

[4]All monetary amounts are deflated to 1979 dollars using the Personal Consumption Expenditures price index for Gross Domestic Product.

[5]The sample is restricted to white men because there are inadequate numbers of non-whites in age-education groups to obtain unbiased estimates. Prior research has shown that grouping estimators can perform poorly when group sizes are very small (Devereux 2002).

this period. The other states show increases in average hours worked, with the largest increases being about 70 to 100 hours in New Jersey, Maryland, Delaware, Florida, New York, and Virginia. Changes in the other labor supply measures approximately parallel the changes in annual hours worked.

Changes over Time at the National Level

In this section, I describe the relationship between labor supply and wages at the national level and I discuss some of the previous literature on this topic. Some descriptive statistics on wages and participation by education group for the United States as a whole are presented in Table 1. These statistics show clearly some of the well-known characteristics of relative wages and relative participation. Hours worked are higher for persons with more education. Similarly, wages increase with education.

Juhn (1992) and Welch (1997) brought cross-sectional labor supply relationships to bear on the time series changes in relative wages using the CPS. Juhn (1992) estimated the labor supply curve across the cross-section of the wage distribution for the years 1970–72 prior to the start of her period of interest. Welch (1997) estimated the labor supply equation using the entire sample period between 1967 and 1992. Juhn's and Welch's results are valid to the extent that the cross-sectional estimates are valid. Both authors acknowledged that their cross-sectional estimates may overstate how much individual participation responds to changes in individual wages. For example, highly motivated individuals are likely to have both high wages and high participation rates. Given that we cannot control fully for "motivation" in the labor supply equation, we may observe a positive relationship between wages and labor supply in the cross-section, even if wages have no causal effect on labor supply. This problem will also be serious when wages are instrumented by age and education, as it is likely that unobservables like motivation differ systematically by age and education group.

All the empirical work in this paper is carried out using cell means.[6] In the absence of other endogeneity problems, measurement error in reports of hours and earnings leads to bias in regressions using microdata. Mean-zero measurement error can be averaged away by taking means over cells with sufficient numbers of observations. I have estimated cross-sectional regressions of labor supply on wages using the 1980 and 1990 Census. In each year, I take the mean of each variable for each of the 20 age-education groups. In the regressions, the mean labor supply for each group is regressed on the mean of the log wage for the group. The means from each group are weighted by the number of underlying Census observations in the group. The wage elasticity is calculated as the coefficient on the log wage divided by the mean of the dependent variable. The elasticities are in Table 2a. For the 1980 sample, I find wage elasticities of 0.182 (0.056) for hours, 0.139 (0.051) for weeks, and 0.405 (0.079) for FTFY in the national sample. The elasticities from 1990 are also reported in Table 2a and are somewhat similar in magnitude. These elasticities are substantial given that this is a sample of white men.

Between 1979 and 1989, hours worked fell for the less-educated groups and rose for the groups with college degrees. As can be seen in Table 1, these changes in labor supply measures closely parallel the changes in relative wages over this time period. In Figure 1, I plot the changes in the average log wage of each group against the change in annual hours worked. There is clearly a strong positive correlation between a group's wage change and its hours change. The relationships are similar for the other labor supply measures.

The statistical analysis in the third row of Table 2a quantifies this effect more pre-

[6]Note that the wage measure is the mean of the log wages within each cell rather than the log of the mean wage within the cell.

Table 1. Wages and Labor Supply by Education Group.

Education Group	*Year*	*Annual Hours*	*Weeks*	*FTFY*	*Proportion Participating*	*Hours of Participators*	*Log Wage*
High School Dropouts	80	1,667.740	39.253	0.569	0.850	1,963.160	1.906
	90	1,582.150	36.732	0.521	0.816	1,937.980	1.703
	Change	–85.590	–2.521	–0.048	–0.034	–25.180	–0.203
High School Graduates	80	1,956.290	45.378	0.712	0.943	2,074.420	2.006
	90	1,944.100	44.322	0.700	0.928	2,094.520	1.860
	Change	–12.190	–1.056	–0.012	–0.015	20.100	–0.146
Some College	80	2,018.880	46.332	0.747	0.956	2,112.550	2.062
	90	2,064.990	46.260	0.759	0.950	2,173.200	2.006
	Change	46.110	–0.072	0.012	–0.005	60.650	–0.056
College Graduates	80	2,050.750	47.230	0.779	0.969	2,115.490	2.235
	90	2,137.790	47.392	0.791	0.968	2,208.060	2.262
	Change	87.040	0.163	0.012	–0.001	92.570	0.027
Post-College Education	80	2,102.270	47.179	0.735	0.971	2,164.320	2.371
	90	2,222.500	47.575	0.766	0.970	2,291.580	2.519
	Change	120.230	0.395	0.031	–0.001	127.260	0.148

Notes: The means are calculated from the 5% samples of the 1980 and 1990 Census. FTFY is the proportion of individuals who work 50 or more weeks and 35 or more hours per week in the calendar year.

cisely. Each column presents weighted least squares estimates from the regression of the change in labor supply for each of the 20 groups on the change in the log wage. The means from each group are weighted by the number of underlying Census observations in the group. The elasticities are quite substantial, with an estimated wage elasticity of 0.32 in the hours equation, 0.18 in the weeks equation, and 0.24 in the FTFY equation. One implication of these numbers is that in order for changes in labor supply to be explained by movement along a labor supply curve, the own wage elasticity for men must be about this magnitude.

The estimates in Table 2a are consistent with the previous literature. Consider first the results for estimates that use weeks worked as the labor supply measure. The Table 2a estimates show that the cross-sectional elasticity for weeks worked is about 80% the size of the differenced elasticity, implying that if the cross-sectional relationships were fit to the time-series changes, wage changes would almost fully explain changes in weeks worked over time by different groups. This is what Juhn (1992), who focused on weeks worked, found for white men. Turning to the results for FTFY, the cross-sectional estimates in Table 2a for FTFY imply that changes in wages should have led to larger changes in FTFY than actually occurred. Welch (1997), who concentrated on FTFY as the labor supply measure, obtained the same result.

Table 2b includes results for men who worked at least one hour in the calendar year. The advantage of restricting the sample to participators is that it obviates imputing wages; the disadvantages are that changes at the extensive margin are ignored and the sample composition changes over time as participation rates change. The cross-sectional results for participators are generally similar to those for the full sample. However, the differenced elasticities are smaller for participators, reflecting the fact that changes in labor supply across groups are not as large when one conditions on participation (because of the large changes at the extensive margin).

It is noteworthy that the strongly positive relationships between wages and labor supply in the studies mentioned above contrast with the generally small or negative wage elasticities for men in the previous litera-

Table 2a. Labor Supply Elasticities at the National Level (Weighted Least Squares Estimates).

Description	*Hours*	*Weeks*	*FTFY*	*Participation*
Cross-Section: 1980	0.182	0.139	0.405	0.007
	(0.056)	(0.051)	(0.079)	(0.040)
Cross-Section: 1990	0.206	0.139	0.307	0.050
	(0.055)	(0.049)	(0.075)	(0.036)
Differenced	0.319	0.175	0.235	0.111
	(0.054)	(0.051)	(0.110)	(0.029)
Means	1,946.14	44.38	0.70	0.93

Table 2b. Labor Supply Elasticities at the National Level (Participators; Weighted Least Squares Estimates).

	Hours	*Weeks*	*FTFY*
Cross-Section: 1980	0.178	0.132	0.400
	(0.025)	(0.023)	(0.070)
Cross-Section: 1990	0.158	0.090	0.258
	(0.024)	(0.020)	(0.056)
Differenced	0.194	0.057	0.121
	(0.034)	(0.030)	(0.096)
Means	2,100.60	47.90	0.76

There are 20 observations in each regression. All elasticities are evaluated at the mean of the dependent variable. The elasticities are calculated as the coefficient on the log wage divided by the mean of the dependent variable. The means of the labor supply variables are given in the table. Robust standard errors in parentheses allow for arbitrary forms of heteroskedasticity.

ture (Killingsworth 1983; Blundell and MaCurdy 1999). Also, the responsiveness is somewhat inconsistent with the fact that male hours have steadily fallen over the century as wages have steadily risen. Given the strong assumptions implicit in the cross-sectional approaches, the alternative approaches implemented in this paper provide an important check on the robustness of these results.

Using Cross-State Variation

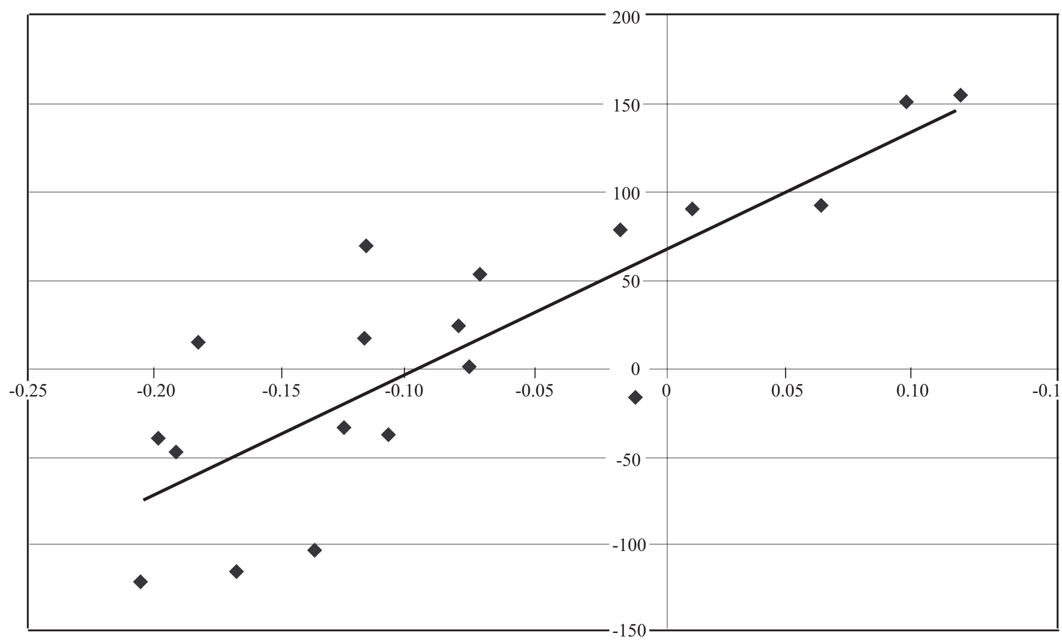
As discussed in the methodology section, cross-state variation in relative wages and relative hours can be used to identify labor supply responses in the presence of unobservable factors that have affected the labor supply of particular groups. To carry out this analysis, I calculate the mean value of the variables for the 20 groups in each of 48 states (dropped are Alaska and Hawaii) in each year. I then implement an estimator that corrects the OLS regression of the group-state cell means for small sample bias. The estimator and the logic behind it are described below.

The Estimator

On average, there are 2,048 observations per group-state cell. Even though these cell sizes are relatively large, prior research suggests that OLS estimates from regressions of the cell means may suffer from small sample biases with cell sizes of this magnitude (Devereux 2002).[7] In the following discussion, I distinguish between population means and sample means. The sample mean of hours for a particular cell is the mean of the hours of all members of that cell included in the sample. The popu-

[7]The results using national groups presented above are weighted least squares results. The national groups are so large that the corrected least squares results are basically identical to the least squares results.

Figure 1. Relationship between Wage and Hours Changes at the National Level.



The horizontal axis measures the log wage change of each age-education group between 1979 and 1989. The vertical axis measures the hours change of each group.

lation mean for that cell relates to the mean of hours for all members of the underlying population who are in that cell.

Consider the regression at the level of population means:

$$\Delta h^* = \Delta x^*\beta + \varepsilon. \tag{5}$$

Here, h^* refers to the mean of h within each cell and x^* is a vector of the means of x (where x includes the log wage and other variables included in the regression). However, because the cells are not arbitrarily large, one calculates from the data the sample means $\bar{x}$ and $\bar{h}$ rather than x^* and h^*. Assume that measurement error has the following structure:

$$\bar{h} = h^* + u \tag{6}$$

$$\bar{x} = x^* + v$$

$$\begin{pmatrix} \bar{h} \\ \bar{x} \end{pmatrix} \sim N \left(\begin{matrix} h^* \\ x^* \end{matrix} \,\middle|\, \begin{matrix} \sigma_{00} & \sigma \\ \sigma & \Sigma \end{matrix} \right)$$

This measurement error may have two components. First, reporting error in individual reports of earnings and hours implies that the means will be incorrect in finite samples. The individual-level reporting error issue is particularly important here, because in addition to the usual attenuation bias that arises from classical reporting error, there is non-classical reporting error induced by the correlation between reporting error in hours and in earnings divided by hours. In cells with few observations, such reporting error will induce a spurious correlation between changes in the wage and changes in hours. This correlation would bias down the own wage elasticity in an OLS labor supply regression. Second, even if individual observations were correctly reported, there will be sampling error in finite samples. As the cell size gets larger, laws of large numbers imply that the sample mean will become arbitrarily close to the population mean.

Deaton (1985) showed that one can consistently estimate β using the estimator

$$(7) \quad \tilde{\beta} = (\Delta\bar{x}'\Delta\bar{x} - 2N\hat{\Sigma})^{-1}(\Delta\bar{x}'\Delta\bar{y} - 2N\hat{\sigma}),$$

where N is the total number of cells and $\hat{\Sigma}$ and $\hat{\sigma}$ are sample estimates of the relevant population parameters. I refer to this as corrected least squares.

Intuitively, this estimator subtracts the variance of measurement error from the moment matrix of $\bar{x}$. Likewise, it subtracts the covariance of the measurement error in $\bar{y}$ and $\bar{y}$ from the total covariance of $\bar{y}$ and $\bar{x}$. Those effects are particularly important in this application because the definition of the wage as earnings divided by hours implies that measurement error in wages and hours may be correlated.

The variances and covariances of measurement error are estimated by their empirical counterparts. I estimate the elements of $\hat{\sigma}$ by calculating the covariance between log hours and each of the $\bar{x}$ variables in each cell in each time period. Likewise, I calculate the rows and columns of $\hat{\Sigma}$ by estimating the variances and covariances of the $\bar{x}$ variables within each cell in each year. In both cases, I take the average of the covariances across all the cells and the two years and I divide by the average number of observations in each group.

Three issues must be addressed in practice. First, in many specifications, I include indicator variables for state, group, or both. These indicator variables are not measured with error. As suggested by Deaton (1985), I deal with this issue by setting the appropriate elements of σ and rows and columns of $\hat{\Sigma}$ to zero. Second, I weight the regressions by multiplying each sample mean by the square root of the number of observations in that cell. Equivalently, the variances and covariances are multiplied by the average number of observations per cell. Third, the standard errors are calculated under the assumption that $\hat{\Sigma}$ and $\hat{\sigma}$ are known. I make this assumption because the sampling variance of estimates of Σ and σ diminishes in proportion to (Nn), where n is the average number of individuals in each cell. Given that there are almost 2 million observations in each year, allowing for sampling error in the variance estimates has negligible effects on the standard errors.

I report estimates both using weighted least squares and using the corrected least squares estimator. In both cases the variance-covariance matrix is calculated allowing for arbitrary forms of heteroskedasticity and non-zero covariance between observations that are in the same group in the same Census division.

Results

The estimation results are reported in Tables 3 and 4. It is standard in labor supply estimation to include controls for other income to capture negative income effects on hours worked. Thus, in Table 4, I add asset income as an additional control.[8] In all specifications the observations are weighted by the number of individuals within each state-group. In the specifications in Table 3a, all white men are included and wages are imputed for nonworkers as described in the data section. I will discuss the corrected least squares estimates—the weighted least squares estimates tend to be slightly smaller.

The first two rows of Table 3a contain cross-sectional regressions of labor supply on the log wage, and a set of state indicators. As expected, the elasticities are very similar to the cross-sectional elasticities from national data. In the third row, the change in labor supply is regressed on a constant, the change in the log wage, and a full set of state indicators. There are no controls for group indicators, so the source of variation used to estimate the wage elasticity is changes in relative wages across the groups over time. Given that this is the same type of variation used in the national estimation, one would expect the results to be similar to those from the differenced regressions in Table 2a.

The estimated elasticities are somewhat smaller in magnitude than in Table 2a, but

[8]Asset income is defined as interest, dividend, and net rental income.

Table 3a. Labor Supply Elasticities at the State Level.

	Hours	*Weeks*	*FTFY*	*Participation*
Weighted Least Squares				
Cross-Section: 1980	0.180 (0.021)	0.135 (0.019)	0.398 (0.031)	0.004 (0.015)
Cross-Section: 1990	0.205 (0.021)	0.137 (0.018)	0.310 (0.028)	0.045 (0.013)
Differenced	0.244 (0.025)	0.142 (0.022)	0.180 (0.042)	0.092 (0.013)
Differenced (Group Effects)	0.028 (0.020)	0.047 (0.020)	0.057 (0.028)	0.009 (0.015)
Corrected Least Squares				
Cross-Section: 1980	0.181 (0.021)	0.135 (0.019)	0.399 (0.031)	0.004 (0.015)
Cross-Section: 1990	0.205 (0.021)	0.137 (0.018)	0.310 (0.028)	0.046 (0.013)
Differenced	0.256 (0.026)	0.148 (0.022)	0.187 (0.044)	0.097 (0.014)
Differenced (Group Effects)	0.045 (0.024)	0.057 (0.022)	0.065 (0.032)	0.015 (0.017)

Table 3b. Labor Supply Elasticities at the State Level (Participators).

	Hours	*Weeks*	*FTFY*
Weighted Least Squares			
Cross-Section: 1980	0.181 (0.010)	0.132 (0.009)	0.396 (0.027)
Cross-Section: 1990	0.162 (0.009)	0.093 (0.008)	0.267 (0.022)
Differenced	0.169 (0.017)	0.058 (0.013)	0.108 (0.039)
Differenced (Group Effects)	0.035 (0.017)	0.061 (0.012)	0.086 (0.033)
Corrected Least Squares			
Cross-Section: 1980	0.182 (0.010)	0.133 (0.009)	0.397 (0.027)
Cross-Section: 1990	0.163 (0.009)	0.093 (0.008)	0.268 (0.022)
Differenced	0.179 (0.018)	0.061 (0.014)	0.112 (0.041)
Differenced (Group Effects)	0.061 (0.023)	0.075 (0.015)	0.102 (0.042)

There are 960 observations in each regression. Each regression includes a full set of state indicators. All elasticities are evaluated at the mean of the dependent variable (listed in Table 2). The elasticities are calculated as the coefficient on the log wage divided by the mean of the dependent variable. Robust standard errors, in parentheses, allow for arbitrary forms of heteroskedasticity and for observations in the same group and Census division to be correlated.

they are much more precisely estimated. In the hours regression, the wage elasticity is 0.26. The wage elasticities for the other labor supply measures are somewhat smaller but still substantial considering that this is a sample of white men. The addition of group fixed effects dramatically reduces the size of the wage effect on all four labor supply measures. For example, the wage elasticity in the annual hours regression is 0.05 and is statistically different from the elasticities both from cross-sectional regressions and from the differenced regression without group controls. The wage elasticities are similarly small for the other three labor supply measures.

The specifications above include both working and non-working men. As explained above, I used that mixed sample in the interest of comparability with the relevant literature. To capture changes at both the intensive and extensive margins, Juhn (1992) used all men and studied weeks worked; toward the same end, Welch (1997) concentrated on the proportion who work full-time/full year. Of course, there are problems in interpreting labor supply parameters for hours and weeks that are estimated on a sample that includes non-working individuals. For this reason, I have also carried out the hours and weeks estimation with a sample restricted to working men.[9] For hours, I find an uncompensated wage elasticity of 0.06 (0.02) in the specification with group dummies and an elasticity of 0.18 (0.01) in the specification without group dummies. The equivalent elasticities for weeks are 0.08 (0.01) and 0.06 (0.01). These results show that the elasticities with group dummies are very low, just as in the sample that included non-participating men.

Interestingly, the wage elasticities from the specifications without group dummies are lower than in the sample that includes non-working men. (For hours, compare 0.18 [0.02] in Table 3b to 0.26 [.03] in Table 3a; for weeks, compare 0.06 [.01] in Table 3b to 0.15 [0.02] in Table 3a.) These modest elasticities for participators suggest a weak relationship between changes in wages and changes in labor supply for participators. Since they come from a specification that does not allow for differential supply shifts across groups, they imply that the observed relationship between changes in labor supply and wages at the intensive margin is consistent with modest labor supply elasticities, even in the absence of supply shifts.

Table 4 contains estimates in which the specifications in Table 3 have been augmented by the addition of asset income.[10] The uncompensated wage elasticities from this specification are very similar to the wage elasticities from the specification in Table 3. The marginal propensity to earn (calculated as the average wage times the coefficient on other income) is typically small and statistically insignificant. Thus, for all the measures, the compensated wage elasticity is not very different from the uncompensated elasticity. As in Table 3, the addition of group fixed effects radically reduces the size of the wage effect on all four labor supply measures. The results imply that while labor supply changes were positively correlated with wage changes over the 1980s, differences in the pattern of relative wage changes across states had little predictive power for labor supply changes. Thus, the estimates suggest the occurrence of labor supply curve shifts across groups that implied changes in labor supply that were not strongly related to changes in wages.

One objection to this interpretation is that differences in relative wage changes across states represent sampling error rather than actual differences in wage changes. This is unlikely to be an issue, because the number of observations in each group-state cell is large (an average of 2,048 observa-

[9]The reported results ignore selection. I have also estimated models that use a selection correction in the spirit of Heckman (1979). I find no evidence of selection bias. My ability to test for selection bias is limited, however, by the unavailability of variables that can be included in the participation equation and omitted from the hours equation.

[10]To conserve space, I include only the weighted least squares estimates. As with the specifications in Table 3, the corrected least squares estimates are very close to, but always slightly higher than, the weighted least squares estimates.

Table 4a. Labor Supply Elasticities at the State Level (Weighted Least Squares Estimation).

	Hours	*Weeks*	*FTFY*	*Participation*
Differenced				
Wage Elasticity	0.247	0.164	0.266	0.082
	(0.028)	(0.024)	(0.044)	(0.015)
Marginal Propensity to Earn * 100	–1.727	–0.291	–0.017	0.003
	(6.791)	(0.136)	(0.004)	(0.002)
Compensated Wage Elasticity	0.264	0.167	0.266	0.082
	(0.084)	(0.025)	(0.044)	(0.015)
Differenced (Group Effects)				
Wage Elasticity	0.028	0.046	0.058	0.008
	(0.020)	(0.020)	(0.028)	(0.015)
Marginal Propensity to Earn * 100	5.961	–0.068	0.004	–0.003
	(7.249)	(0.134)	(0.004)	(0.002)
Compensated Wage Elasticity	–0.031	0.047	0.058	0.008
	(0.075)	(0.020)	(0.028)	(0.015)

Table 4b. Labor Supply Elasticities at the State Level (Participators, Weighted Least Squares Estimation).

	Hours	*Weeks*	*FTFY*
Differenced			
Wage Elasticity	0.189	0.101	0.227
	(0.020)	(0.015)	(0.043)
Marginal Propensity to Earn * 100	–11.998	–0.525	–0.023
	(4.897)	(0.082)	(0.004)
Compensated Wage Elasticity	0.302	0.106	0.227
	(0.062)	(0.015)	(0.043)
Differenced (Group Effects)			
Wage Elasticity	0.037	0.062	0.089
	(0.017)	(0.011)	(0.032)
Marginal Propensity to Earn * 100	15.998	0.088	0.007
	(5.405)	(0.075)	(0.004)
Compensated Wage Elasticity	–0.123	0.061	0.089
	(0.058)	(0.011)	(0.032)

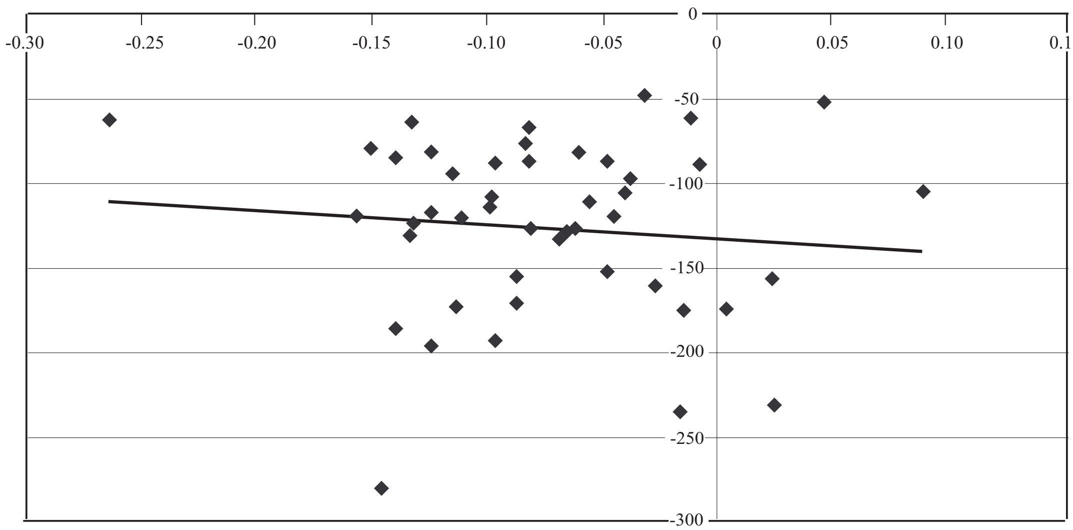
There are 960 observations in each regression. Each regression includes a full set of state indicators. All elasticities are evaluated at the mean of the dependent variable (listed in Table 2). The elasticities are calculated as the coefficient on the log wage divided by the mean of the dependent variable. The marginal propensity to earn is calculated as the coefficient on asset income multiplied by the mean wage. The mean of the wage is $9.368 for the full sample and $9.382 for the sample of participators. The mean of other income is $787.74 for the full sample and $749.62 for the sample of participators. Robust standard errors, in parentheses, allow for arbitrary forms of heteroskedasticity and for observations in the same group and Census division to be correlated.

tions per group-state cell) and the estimation method corrects for any remaining variance resulting from sampling error. A second objection is that there is not sufficient variation across states in wage change differences across groups. However, as indicated by the small standard errors in the specifications with group controls, there is substantial variation in changes in relative wages across states.[11]

Figures 2–4 provide some graphical evidence of the relationship between wage

[11]A table detailing the extent of variation is available from the author on request.

Figure 2. Relationship between State-Level Wage and Hours Changes (High School Dropouts, Aged 31-40).



The horizontal axis measures the deviation in the log wage change of the group from the average log wage change in the state. The vertical axis measures the deviation in the annual hours change of the group from the average annual hours change in the state.

and hours changes across states. The figures plot, for selected groups, the relationship between the deviation in the log wage change of each group from the average log wage change in the state and the deviation in annual hours from the average annual hours in the state. It is clear from these figures that there is no consistent pattern relating hours and wage changes across states. The absence of any clear pattern is consistent with the small elasticities found in the statistical analysis.

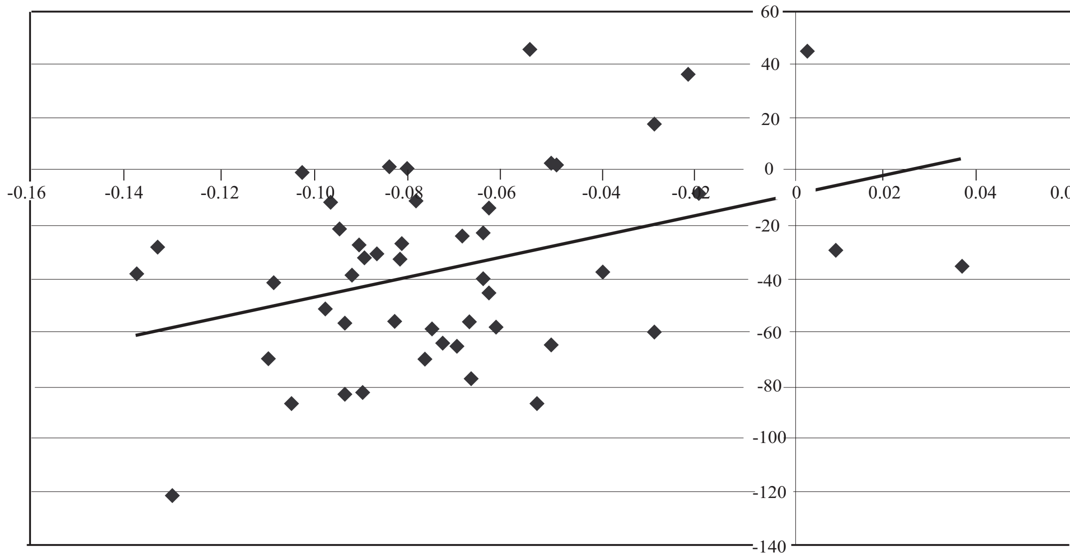
Grouping by Cohort

In the analysis so far, grouping has been by age. An alternative is to follow cohorts over time. In this section, 15 groups are defined by the interactions of the 5 education groups (high school dropout, high school graduate, some college, college graduate, more than college) with three birth cohorts (men aged 21–30, 31–40, and 41–50 in 1980). Thus, individuals in 1990 are aged 31–40, 41–50, and 51–60. As before, the means from each group-state cell are weighted by the number of underlying Census observations in the cell.

The results are in Table 5. For brevity, I only include the differenced estimates and I exclude specifications with asset income. As with the age-based groups, the wage elasticities are not much affected by the inclusion of asset income. The elasticities are generally slightly higher than in the age-based groups. For the full sample, the corrected least squares elasticities range from 0.02 and 0.1 in the specifications with group indicators. In the sample of participators the equivalent elasticities range from 0.1 to 0.17.

The slightly higher wage elasticities estimated from the cohort-based groups may result from the fact that they are measuring some combination of the intertemporal wage elasticity and the standard uncompensated wage elasticity. If changes in relative wages are foreseen by agents, then they do not constitute a change in wealth, and hence the marginal utility of wealth does

Figure 3. Relationship between State-Level Wage and Hours Changes (High School Graduates, Aged 31-41).



The horizontal axis measures the deviation in the log wage change of the group from the average log wage change in the state. The vertical axis measures the deviation in the annual hours change of the group from the average annual hours change in the state.

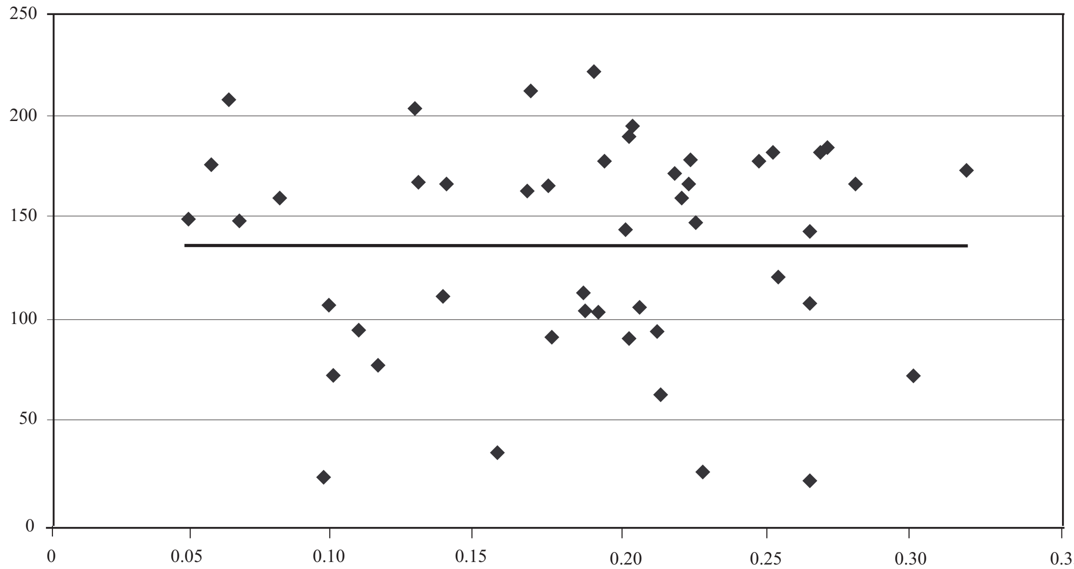
not change. Thus, changes in hours are responses to movement along a wage profile rather than shifts in the profile and the wage elasticities are intertemporal elasticities. However, if the change in relative wages is not foreseen by agents, then the wage changes do lead to shifts in the marginal utility of wealth, and the estimated elasticities do not have this straightforward implication.

On the other hand, when we compare the labor supply of agents of the same age in 1980 and 1990, we are comparing two cohorts with different permanent incomes resulting from the wage changes. Thus, to the extent that the changes in relative wages are permanent, the elasticities are capturing hours changes that result from shifts in the wage profile. The elasticities from the age-based groups are therefore more likely to be capturing the standard uncompensated wage elasticity. Given this consideration, it is not surprising that they are smaller. Pencavel (2002) provided an excellent discussion of how estimated elasticities may be interpreted as uncompensated elasticities or intertemporal elasticities depending on the exact specification. His conclusion that the uncompensated wage elasticity is very small for men is consistent with the findings of this paper.

Migration

If the composition of a group changed between 1980 and 1990, this may bias results. Since groups are defined at the state level, migration between states or immigration from abroad is a potential source of concern. Ideally, in order for the estimates to be interpreted as labor supply elasticities, there should be no migration across states. One advantage of the cohort-based strategy described in the previous section is that it allows a natural specification check for the effects of migration. The Census asks respondents about their state of residence five years previously. Thus, I can exclude from the 1990 sample all cases in which the individual resided in a different

Figure 4. Relationship between State-Level Wage and Hours Changes (Men with Post-College Education, Aged 31-40).



The horizontal axis measures the deviation in the log wage change of the group from the average log wage change in the state. The vertical axis measures the deviation in the annual hours change of the group from the average annual hours change in the state.

state in 1985. When I do this, the estimates are robust with respect to the exclusion of all individuals who did not reside in the same state in 1985 (the wage elasticities are typically slightly smaller when the movers are omitted, but the differences are not large).[12] I do not report results, because the estimates are very similar to those in Table 5. Thus, it is unlikely that migration across states between 1980 and 1990 seriously biases my results.

[12]One would expect migration to bias upward the labor supply elasticities. If the wage of a particular group rises in a certain state, individuals in that group will tend to move to that state. The individuals who move to the state will tend to be individuals with strong preferences for work. Thus average labor supply in the group will rise both because individuals in the group work more and because new entrants to the group in that state are hard-working types.

Wages and Labor Supply in the Panel Study of Income Dynamics

The Panel Study of Income Dynamics (PSID) is a longitudinal data set that has followed approximately 5,000 families from 1968 to the present. In the analysis using the PSID, I use both the random sample and the poverty sample. The results reported are not weighted by the weights in the PSID, but the weighted results are almost identical.

The sample selection criteria are as follows. Respondents must be present for at least one year during the 1979–81 period and must also be in the sample ten years later, during the 1989–91 period. The wage measure used is annual earnings divided by annual hours. To create a sample that is comparable to the Census sample, I restrict the sample to white men who are no more than 50 years of age in the first year. The minimum age of people in the sample is 20, and stu-

Table 5a. Labor Supply Elasticities at the State Level (Cohort-Based Groups).

	Hours	*Weeks*	*FTFY*	*Participation*
Weighted Least Squares				
Differenced	0.564	0.413	0.713	0.225
	(0.026)	(0.023)	(0.044)	(0.018)
Differenced (Group Effects)	0.046	0.047	0.083	0.00011
	(0.020)	(0.019)	(0.031)	(0.015)
Corrected Least Squares				
Differenced	0.573	0.419	0.723	0.229
(0.026)	(0.023)	(0.044)	(0.018)	
Differenced (Group Effects)	0.072	0.060	0.099	0.018
(0.024)	(0.023)	(0.036)	(0.017)	
Means	1,979.29	44.92	0.71	0.93

Table 5b. Labor Supply Elasticities at the State Level (Cohort-based groups, Participators)

	Hours	*Weeks*	*FTFY*
Weighted Least Squares			
Differenced	0.337	0.189	0.485
	(0.014)	(0.010)	(0.032)
Differenced (Group Effects)	0.073	0.070	0.125
	(0.024)	(0.020)	(0.050)
Corrected Least Squares			
Differenced	0.343	0.192	0.492
	(0.014)	(0.010)	(0.032)
Differenced (Group Effects)	0.137	0.099	0.170
	(0.038)	(0.030)	(0.075)
Means	2,119.76	48.11	0.77

There are 720 observations in each regression. All regressions include a full set of state indicators. All elasticities are evaluated at the mean of the dependent variable. The elasticities are calculated as the coefficient on the log wage divided by the mean of the dependent variable. The means of the labor supply variables are given in the table. Robust standard errors, in parentheses, allow for arbitrary forms of heteroskedasticity and for observations in the same group and Census division to be correlated.

dents are omitted. Furthermore, I only include individuals with positive actual labor market experience. I delete cases in which the reported annual hours are greater than 4,000, as these are likely to be seriously mismeasured. I also delete cases with wages of less than $1 per hour in 1979 dollars. Finally, I delete a few cases with inconsistent information on hours, weeks worked, and earnings, such as persons reporting zero weeks and positive hours and persons with positive hours and zero earnings. I also delete cases with missing education information. I treat topcoded wages in the same fashion as with the Census data, that is, I multiply the topcoded value by 1.33.[13]

Exploiting Individual Variation in Wage Changes

The estimating equation relates changes in labor supply for individual i between t and $t+10$ to the change in the individual's log wage over that period, and to a set of control variables x (in this case, education indicator variables and a quartic in actual [not potential] labor market experience).

[13]There is only one such case, and I get similar results if I delete it.

Table 6. Labor Supply Elasticities from the Panel Study of Income Dynamics.

Year (Observations)	*Annual Hours*	*Weeks Worked*	*FTFY*
1979 (1,434)	–0.006	0.027	–0.061
(0.030)	(0.017)	(0.074)	
1980 (1,476)	–0.022	0.003	–0.038
(0.026)	(0.016)	(0.067)	
1981 (1,464)	0.017	0.012	0.001
(0.027)	(0.016)	(0.068)	
Means	2,233.87	46.82	0.69

All regressions are estimated using changes over ten years, where the beginning year is the year listed in the Table. All regressions include indicator variables for education and a quartic in actual labor market experience. All elasticities are evaluated at the mean of the dependent variable. The elasticities are calculated as the coefficient on the log wage divided by the mean of the dependent variable. FTFY equals 1 if the individual works at least 48 weeks and 35 hours per week in the calendar year, and zero otherwise. Robust standard errors, in parentheses, allow for arbitrary forms of heteroskedasticity.

$$(8) \qquad (h_{it+10} - h_{it}) = \beta(w_{it+10} - w_{it}) + \alpha' x + (\varepsilon_{it+10} - \varepsilon_{it})$$

The wage measure used is annual earnings divided by annual hours. Thus, by construction, the wage measure has a spurious negative correlation with hours resulting from measurement error in hours. The strategy of taking means over large numbers of observations used with the Census data cannot be applied to these individual-level data. Therefore, I take advantage of the panel structure of the data. I use the average of the log wage at $t - 1$ and the log wage at $t + 1$ as an instrument for the log wage at t. Similarly, I use the average of the log wage at $t + 9$ and the log wage at $t + 11$ as an instrument for the log wage at $t + 10$. Thus, the instrument for the change in the log wage between t and $t + 10$ is the difference between these two average wages. An advantage of this instrument is that it preserves the individual-specificity of the wage changes used in the analysis.

Several caveats should be borne in mind. First, measurement error in earnings and hours may be serially correlated, and this would bias the estimates. Second, the fixed effects assumption may be violated, and changes in wages of individuals may be related to changes in skills, motivation, or other factors that have direct effects on the choice of hours. Third, in a model of uncertainty, forecast errors may be correlated with wage changes.

There is no obvious way to impute wages for non-workers, so I restrict the sample to men who participate in both periods. The analysis is carried out using two stage least squares. Table 6 contains the wage elasticities.[14] The estimates are similar for all three 10-year periods. The wage elasticities are all close to zero and are frequently negative. The estimated elasticities are a bit smaller than those from the Census analysis, but given the standard errors, one should not make too much of the differences. While both identification strategies use within-group identification, it is still meaningful that two very different sources of identification on two different data sets produce similarly small estimates. As such, these results strengthen the earlier conclusion that the wage effects on labor supply are too small to fully explain the changes in men's relative labor supply during the 1980s.

[14]The PSID has an alternative wage measure for a subset of workers who are paid hourly or are salaried. I have tried using this wage measure as an instrument for the wage measure used in the analysis. The results are very similar to the reported results.

Why Might Labor Supply Curves Have Shifted in for Less-Skilled Men?

Given the conclusions of this paper that the uncompensated wage elasticity for white men is close to zero and thus wage changes alone cannot explain the changes in relative labor supply, what factors can explain the implied shifts in labor supply? Possible candidates are declining marriage rates, changes in female wages and labor supply, shifts in earnings opportunities from crime, and changes in transfer income. I briefly consider these possible explanations below.

Crime rates stabilized during the 1980s as incarceration rates increased. Freeman (1996) estimated that the propensity for criminal activity by non-institutionalized men increased by between 80% and 163% between 1977 and 1992. There are no reliable estimates of earnings from crime. However, Freeman reports that two Boston surveys showed that the proportion of youths who said that they had "chances to make illegal income several times a day" roughly doubled between 1980 and 1989. The expansion of the drug market may have increased earnings from criminal activity over this period. Over the same period, the probability of incarceration increased, and this should have tended to make crime less attractive. Thus, given the incompleteness of our knowledge about crime, it is not possible to tell whether changes in criminal opportunities for different groups shifted labor supply curves over this period.

Another possible cause of shifting labor supply curves is declining marriage rates. Married men tend to work more than single men. This could be a causal effect of being married or could reflect the sorting of men into marriage. During the 1980s, marriage rates in the Census data fell for all education groups except the group with post-college education. The proportion married fell from 0.77 to 0.66 for high school dropouts, from 0.74 to 0.68 for high school graduates, from 0.72 to 0.70 for the group with "some college," and from 0.71 to 0.70 for the college graduates, and it rose from 0.77 to 0.79 for the group with post-college education. The disproportionate decline in marriage rates for the least educated could imply a shifting inward of labor supply curves for this group. This can only be suggestive, as marriage rates are likely to be affected by wages and labor supply, and establishing any causal relationships between these outcomes might prove difficult.

The wages and labor supply of women changed enormously over this period. Among married women, labor force participation increased by about 10% for the wives of all five male education groups. Earnings of wives also increased for all five groups, with the biggest increases (in absolute and percentage terms) being experienced by the highly educated groups. In particular, the wages of wives of college-educated men increased about 15% while the wages of wives of less-educated men stagnated. The increasing earnings of married women would have tended to shift labor supply curves of men through income effects. However, if wives' incomes are to be invoked as an explanation for why labor supply curves have shifted in for poorly educated men only, it must be shown that the effects of wife's income differ across groups.

I have examined this possibility using the sample of married men in the Census. I take the differenced state-level hours regression without group indicators and augment it with the change in wife's earnings. I allow the effect of the change in wife's earnings to differ by husband's education. The coefficient on wife's earnings is significantly negative for high school dropouts, approximately zero for high school graduates, and significantly positive for the other education groups. Adding wife's earnings to the specification reduces the estimated wage elasticity for married men from 0.278 (0.014) to 0.083 (0.016). Given that wife's earnings are surely endogenous, one should not take these estimates too literally. However, they do indicate that changes in the wages and labor supply of women may have led to shifts in the labor supply curves of men.

One source of non-labor income available to working-age men is disability benefits. Over this period, these benefits did not become more generous, and so it is unlikely that they are a major reason for shifts in labor supply curves (Juhn 1992). In the Census, the proportion who reported having a disability that inhibited working rose from 0.17 to 0.19 among high school dropouts and remained fairly flat for the other groups. This increase in reported disabilities among the least-educated could reflect changes in attitude toward work. However, once again, reported disability may be endogenous to labor supply. I conclude that although there are many possible reasons for shifts in labor supply curves over this period, pinning down which were most important constitutes a challenging research agenda.

Conclusions

Labor supply estimation using two different data sources and identification strategies provides one consistent result: the relationship between wage changes and labor supply changes over the 1980s was very small. This relationship is much weaker than repeated cross-sectional estimates in the literature suggest. In particular, the relationship is not strong enough to suggest that labor supply changes during the 1980s reflect movement along a stable labor supply curve. The results suggest such movements along supply curves were accompanied by inward shifts in labor supply among low-skill men. Researchers should be careful when using changes in relative wages over time as instrumental variables in labor supply estimation.

Recent research has suggested that the declines in labor supply by less educated men during the 1980s represented a labor supply response to changes in relative wages. In this paper, I have shown that the labor supply elasticities required to be consistent with this mechanism are large, particularly to explain changes at the extensive margin. These large elasticities are out of line with the vast majority of previous estimates in the literature. Using two alternative identifying assumptions, I have estimated elasticities that are close to zero. These results suggest that tax changes have a negligible effect on the labor supply of men below retirement age. Policy-makers should be cautious when drawing conclusions about labor supply elasticities that are obtained by correlating trends in wages and participation rates over relatively short periods of time.

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