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Abstract

To adequately evaluate claims that increased worker insecurity has reduced wage growth in the 1990s, research must answer two questions: (1) Has worker insecurity increased?, and (2) Does worker insecurity reduce wage growth? Examining data on displacement rates from the Displaced Workers Surveys and data on workers' perceptions of job security from the General Social Survey, we conclude that worker insecurity has been high in the 1990s relative to what would have been expected on the basis of the falling unemployment rate. Moreover, examining the relationship between measures of displacement and aggregate wage growth using panel data covering the 50 states over the years 1979 to 1997, we conclude that worker insecurity does reduce wage growth for some classes of workers. However, we only find evidence of an effect of insecurity on wage growth for workers without college degrees and the increase in insecurity during the 1990s is limited mainly to workers with college degrees. Thus we conclude that increased worker insecurity may not have had a large effect on aggregate wage growth.

I. Introduction

Numerous policy makers and media analysts have suggested that an increase in workers' sense of job insecurity in recent years has reduced aggregate wage growth below levels that might otherwise have been expected on the basis of indicators such as the unemployment rate.[1] To evaluate such arguments researchers need to answer two distinct questions: (1) Has actual or perceived job insecurity been higher than would have been expected on the basis of the generally low unemployment rate? (2) Is job insecurity associated with lower wage growth after accounting for variation in the unemployment rate? A small, but growing, literature suggests that the answer to the first question may be at least a qualified yes. But, to our knowledge, there is no evidence on the second question, a gap in the literature we attempt to fill in this paper.

Before addressing the question of the effects of job security on wages, we begin by adding to the literature on trends in insecurity. Our tabulations of the Displaced Worker Survey (DWS) and General Social Survey (GSS) generally accord with those of Farber (1997), Aaronson and Sullivan (1998a, 1998b), Boisjoly, Duncan, and Smeeding (1998), Schmidt (1999), and Valetta (1999).[2] Specifically, we find that workers' reports of job displacement and their perceptions of insecurity in their current jobs were both somewhat higher in the mid-1990s than might have been expected on the basis of the generally low unemployment rate.[3] Insecurity was most elevated among groups, such as those with college degrees, that historically have had low rates of insecurity, resulting in a greater democratization of insecurity in the mid-1990s than in earlier years. In the most recent data, overall levels of insecurity have declined somewhat, but the trend towards democratization has continued.

After reviewing these trends in measures of insecurity, we undertake what we believe to be the first attempt to answer the second question posed above – Do elevated rates of insecurity actually decrease aggregate wage growth? Because job loss usually represents a significant economic set-

1. Prominent statements of this view by policymakers include Alan Greenspan's (1997) testimony that "atypical restraint on compensation increases ... appears to be mainly the consequence of greater worker insecurity," and Robert Reich's (1997) comment that "wages are stuck because people are afraid to ask for a raise. They are afraid they may lose their job." The New York Times (1996) collection of articles, *The Downsizing of America*, typifies media analysis.
2. Aaronson and Sullivan (1998a) review this literature as well as the related literature on job stability. Because job stability depends in part on the rate at which workers *voluntarily* quit jobs, we consider the latter literature less relevant to policymakers' thesis that insecurity lowers wage growth.
3. Gottschalk and Moffitt (1999) provide contrary evidence.

back, there is good reason to believe that workers would be concerned about an increased risk of displacement.[4] But it is not obvious what effect this would have on wages. Rather, the effect of increased insecurity on wages should depend on the form that the insecurity takes and on the nature of the wage determination process.[5]

Perhaps the most straight-forward theoretical interpretation of the proposition that higher insecurity decreases wage growth is in terms of the Shapiro-Stiglitz (1984) efficiency wage model, in which workers are deterred from "shirking" in the absence of perfect monitoring by the threat of unemployment and the loss of rents associated with higher than market-clearing wage rates. If one interprets increased worker insecurity as an improvement in the technology for detecting shirking, perhaps because dismissing workers now meets with less disapprobation or legal opposition, then lower rents would be necessary to motivate effort. In addition to lower wages, this would imply a decline in the equilibrium rate of unemployment, a prediction that, at least superficially, appears to be consistent with actual developments.[6] Alternatively, in the context of a model in which search frictions or insider power give both workers and firms some bargaining power over wages, one might interpret increased worker insecurity as an increased ability of firms to layoff workers, which would have the effect of decreasing workers' bargaining power and hence the level of their wages relative to productivity.

Not all forms of increased job insecurity need inhibit wage growth, however. In the efficiency wage model, increases in insecurity that don't depend on worker performance shouldn't lessen the need for incentives based on the fear of unemployment. Indeed, workers who believe there is a good chance they will lose their jobs regardless of their performance would likely lower their estimates of the rent associated with a continuing employment relationship, making it necessary for firms to pay *higher* current wages to induce appropriate effort.[7] Similarly, jobs that entail a higher risk of unemployment may earn compensating differentials.[8] Even in contexts in which wages are bargained, increased insecurity can have several effects on wages. For instance, in a model of

4. A large literature shows that high-seniority displaced workers typically suffer large and long-lasting earnings declines. See, for example, Podursky and Swaim (1987), Kletzer (1989), Topel (1990), Ruhm (1991), and Jacobson, LaLonde, and Sullivan (1993). Fallick (1996) and Kletzer (1997) provide recent reviews of this literature.
5. Blanchard and Katz (1997) provide an overview of theories of wage determination.
6. See Katz and Krueger (1999) for a discussion of the fall in the unemployment rate.
7. Ritter and Taylor (1997) consider the dynamics of wages in a model in which a shorter expected employment relationship induces failing firms to pay higher wages to induce appropriate levels of effort.
8. See, for example Abowd and Ashenfelter (1981) and Topel (1984).

matching followed by bargaining over wages, as in Mortensen and Pissarides (1994), an increase in insecurity interpreted as an increase in firms' ability to discharge workers raises average productivity and, therefore, equilibrium wages.[9]

Thus theory does not provide clear guidance for the direction of the effects of insecurity on wages, much less the magnitude of such effects. Nor does theory suggest that insecurity effects all workers' wage growth equally. The importance and difficulty of monitoring must vary greatly across classes of workers. Also, the importance of bargaining and rents associated with specific jobs is likely high for some workers, while for others with more general skills, wages may reflect more directly supply and demand conditions in a broad labor market of many employers. Such considerations suggest to us that job insecurity may have different effects for different classes of workers. This leads us to seek empirical evidence on these questions and to allow for the possibility that effects may vary across classes of workers.

In order to determine the effect of insecurity on wage growth we need to observe variation in some concrete measure of insecurity that can be related to wage growth. In our view, rates of job loss derived from the DWS are the most attractive alternative for measuring insecurity. Because of its relatively large size, the DWS can be used to compute rates for relatively small geographic divisions, such as the 50 states. This gives us substantial degrees of freedom to estimate the effects of insecurity on wages. There are, however, challenges associated with use of the DWS. In particular, a change in the horizon over which workers were asked about job loss complicates the interpretation of time series variation in measures derived from the DWS. To compensate, we construct a measure of displacement for high-tenure workers that minimizes such problems. We also include period-specific fixed effects in our statistical models of wage growth. An additional difficulty is that while the DWS is relatively large, the state-level samples are, in many cases, small enough to make measurement error a serious concern and a lack of consistency between estimates of displacement rates for the same year derived from different surveys cause us to suspect nonsampling forms of measurement error as well. This leads us to employ errors-in-variables and instrumental-variables methods to correct for the resulting bias as well as to experiment with alternative ways of aggregating the data.

The statistical models we find most compelling imply relatively large negative estimates of the

9. See Blanchard and Katz (1997) for the argument cast in terms of an increase in firing costs decreasing wages.

effect of displacement on overall wage rates after controlling for the unemployment rate. Unfortunately, standard errors also tend to be large, implying that the effects are only marginally statistically significant. However, when we restrict our analysis to workers without a college degree, we find substantially stronger evidence of a negative effect of insecurity on wages. In contrast, we find little evidence of a relationship between displacement and the wages of college-educated workers. Because it is college-educated workers who have experienced the greater increases in insecurity, the disaggregated results suggest a more modest effect of increased insecurity on overall wage growth in the 1990s.

II. Trends in worker insecurity

We take two approaches to evaluating the trend in worker insecurity. The first is based on workers' reports of actual job loss in the recent past. The second is based on workers' reports of their perceptions of the chances of job loss in the year ahead. We find that the two approaches yield qualitatively similar results for the time pattern of worker insecurity, though we also find that the two trends represent distinct phenomena.

Our data on workers' reports of job loss are based on the Bureau of Labor Statistics' Displaced Workers Survey (DWS) which has been conducted every two years 1984-1998 as a supplement to the Current Population Survey (CPS).[10] From 1984 to 1992, the DWS asked respondents whether they were displaced any time in the previous five years. In the more recent surveys, the retrospective window was decreased to three years. Workers who report having been displaced are asked for the reason for their displacement. The survey mentions three "standard" reasons: Plant or company closed down or moved, Insufficient work, and Position or shift abolished. There are also three possible "nonstandard" responses: Seasonal job completed, Self-operated business failed, and Some other reason.

Unfortunately, "some other reason" is an increasingly common response, accounting for a large fraction of the total growth of displacement of high-seniority workers during the 1990s. In Aaronson and Sullivan (1998a), we took the view that such workers were likely displaced, but simply found the possibly confusing wording of the other responses didn't exactly describe their circumstances. Thus we followed Farber (1997) in focussing on counts of all workers reporting displace-

10. See Hipple(1999) for tabulations and analysis of the most recent data.

ment, regardless of reason given. However, a special BLS debriefing of respondents to the February 1996 survey revealed that only 20 to 30 percent of those who initially reported "other" reasons for their job loss gave answers to the debriefing questions that were consistent with being displaced.[11] Therefore, in this paper, we restrict our attention to counts of workers reporting one of the three standard reasons for displacement.[12]

Another interpretational issue is raised by the change in the length of the retrospective time period covered from five to three years. The number of workers displaced in a five year window is naturally larger than the number displaced in a three year window, other things equal. Moreover, simply calculating annual rates doesn't eliminate the problem because people may be displaced more than once in a given time interval, but the DWS only asks about the most recent instance of displacement. Our solution to this problem is to look at rates of displacement among those with five years tenure.[13] Obviously, workers can lose only one job with five years tenure in any five or three year period. Moreover, in our regression analysis relating wages to insecurity, we include year effects, which should eliminate any bias from this change in question.[14]

As noted, we use the DWS to estimate numbers of high-seniority displaced workers. In order to calculate displacement *rates*, we need an estimate of the number of high-tenure workers at risk for job loss. To obtain such an estimate we use the CPS Outgoing Rotation Groups (ORG) to estimate the total number of workers and the various CPS Tenure Supplements to estimate the fraction with five years tenure. When necessary, we interpolate between Tenure Supplements to obtain the relevant fractions.[15] Thus displacement rates for group g in year t according to the DWS conducted in year s can be written as $r_{gst} = d_{gst} / (n_{gt} f_{gt})$ where d_{gst} is the number of workers from group g displaced in year t according to the survey in year s, n_{gt} is the number of workers in group g in year t according to the ORG files, and f_{gt} is the fraction of workers in group g in year t according

11. See Abraham (1997) and Esposito and Fisher (1998) for evidence from the reinterviews.
12. Ideally, one would like to incorporate some fraction of the "other" responses into an overall measure of displacement, but it is not clear how to construct such a series that would be consistent over time.
13. Farber(1997) proposes an alternative solution based on a calibration of the frequency of multiple displacements in three or five year periods derived from the Panel Study of Income Dynamics.
14. In our regression analysis with year effects we also consider measures of job displacement among workers with one or more years of tenure.
15. The fraction of workers with a given level of tenure changes slowly, so the interpolation introduces relatively little error. Aaronson and Sullivan (1998a) discusses the construction of displacement rates in more detail.

to the Tenure Supplements.

The overlapping, retrospective windows of the DWS surveys imply that for many years we have multiple estimates of the displacement rate. As Topel (1990) and others have noted, estimates derived from surveys closer to the year in question tend to yield higher estimates of displacement rates than those derived from later surveys. Thus, simply averaging the multiple estimates is not appropriate. Instead, we estimate a simple statistical model to combine surveys. Let r_{st} be the estimate of year t displacement derived from the DWS in year s. Then the model is $\log r_{st} = d_t + (s-t-1)\gamma + \varepsilon_{st}$ where ε_{st} is an error term assumed to have the usual ideal properties. We estimate the parameter γ to be about 0.11 which implies that each additional year of delay until the survey is conducted reduces the measured displacement rate by about 11%. We take the estimated d_t parameters as our estimates of the log of displacement in year t.

The solid line in Figure 1 shows our estimate of the overall displacement rate for workers with five or more years job tenure.[16] Evidently, this rate rose during the early 1980s recession from a little over 1.0% to a little over 2.0% and then fell steadily during the 1980s expansion back to approximately the level of 1979. The displacement rate rose again during the early 1990s recession, reaching a peak in 1991 at level slightly higher than during the previous peak in 1982. Since the early 1990s recession was by most measures much milder than that of the early 1980s, this steep increase was somewhat surprising. Moreover, the displacement rate has declined only slowly during the long economic expansion of the 1990s, despite the sharply falling unemployment rate.

The dashed line in Figure 1 shows displacement rates adjusted for variation in the unemployment rate. These estimates were obtained by augmenting the model for the survey-specific displacement rates with the national unemployment rate, $\log r_{st} = d_t + u_t\rho + (s-t-1)\gamma + \varepsilon_{st}$ where u_t is the unemployment rate and ρ is an additional parameter. The adjusted rates are estimates of the displacement rate that would have prevailed if the unemployment rate had been constant at its mean value (6.7%) over the sample period.[17] Adjusted for the unemployment rate, displacement rose significantly over the 1990s. Indeed, the average unemployment-adjusted displacement rate

16. That is, it plots $exp(\hat{d}_t)$, which is our estimate of the displacement rate that would have been recorded if there was no delay before a survey was administered.
17. In this model, we normalize d_{1979} to zero and plot $exp(\hat{d}_t + \bar{u}\hat{\rho})$ where $\bar{u}$ is the average unemployment rate and $\hat{\rho}$ is the estimate of ρ.

for the 1990s (2.1%) was 40% higher than the average for the 1980s (1.5%).

Figure 2, which displays the survey-specific displacement rates after adjustment for recall, motivates our concern below for the presence of measurement error in displacement rates.[18] Until the mid-1990s, the recall-adjusted surveys were at least fairly consistent in their estimates of displacement rates for a given year. More recently, however, results from overlapping survey windows line up less well. In particular, the rate for 1993 was significantly lower when measured in 1996 than 1994 and the rate for 1995 was significantly higher when measured in 1996 than 1998. In both cases, the differences far exceeds that expected on the basis of sampling error for the two surveys. The fact that the 1996 survey, which was involved in both large discrepancies, was undertaken near the height of media attention to job displacement issues, suggests to us the possibility that publicity and other factors can introduce nonsampling forms of measurement error into our measured displacement rates.

Figure 3 shows displacement rates separately for workers with and without a college degree. Though workers with college degrees continue to have lower rates of displacement, Figure 3 shows that such workers have seen a substantial increase in displacement during the 1990s. In contrast, displacement rates for workers without college degrees show little trend. As a result, the difference in displacement rates between college-degreed and non-college-degreed workers has shrunk dramatically over time. In Aaronson and Sullivan (1998a and 1999b), we show that this trend towards democratization in displacement is also evident in declining differentials by occupation, industry, and race. The contrasting trends in displacement shown in Figure 3 imply the importance of determining whether the effects of displacement on wage growth differ by educational attainment.

We now turn to trends in workers' prospective assessment of the chances of job loss based on the General Social Survey (GSS), which is a nationally representative survey conducted by the National Opinion Research Center.[19] On a frequent, though irregular, basis since 1977, this survey has asked the question, "Thinking about the next 12 months, how likely is it that you will lose your job or be laid off?" The allowed responses are "very likely", "fairly likely", "not too likely", and "not at all likely."[20] Generally, 30% to 40% of respondents report some degree of insecurity, answering something other than job loss being not at all likely. Most such workers report that job

18. That is, the survey specific rates shown in the graph are $exp(r_{st} + \hat{\gamma}(s - t))$ where r_{st} is the raw estimate of displacement in year t as measured by the survey conducted in year s and $\hat{\gamma}$ is approximately 0.11.
19. See National Opinion Research Center (1999) for a description of the data.

loss is only somewhat likely; the proportion answering that job loss is fairly likely or very likely never exceeds 15%.

The patterns in the GSS data are summarized in Table 1 which presents the estimated coefficients of an ordered probit model for the responses to the job loss question.[21] The model assumes that workers' responses are determined by a latent variable that can be interpreted as an index of job security and which is a linear function of workers' industry, occupation, sex, race, marital status, education, census region, and survey year as well as a normally distributed error term. Each worker characteristic is represented by a series of indicator variables for each possible value of the characteristic relative to a base case which is a worker who is white, male, aged 25 to 34, married, a high school graduate, residing in the Pacific region, doing clerical work in the government sector and was interviewed in 1988. The table also shows the estimated marginal effect on the probability of reporting each of the responses of changing from the base case to the particular level of the characteristic shown.

Table 1 reveals a number of interesting relationships between insecurity and workers' cross-sectional characteristics. In particular, older workers are more secure than younger workers and whites are substantially more secure than blacks. Sex matters less, but female workers are slightly less secure than males. Workers who are married are more secure than those who are not. Construction and manufacturing workers are less secure than workers in other industries while professional and managerial workers are more secure than those in other occupations. Finally, education substantially reduces insecurity.

The heavy solid line in Figure 4 displays the coefficients on the year dummies from the GSS ordered probit model since 1977.[22] Like the displacement rates in Figure 1, workers' perceptions of job security clearly declined during the recession of the early 1980s, then rose during the

20. The qualitative nature of these responses makes their interpretation as absolute probabilities impossible. Recently, Manski and Straub (1999) have reported a survey methodology that reveals respondents subjective probabilities of events such as job loss. Unfortunately, this survey is too new to be useful for our current purposes.

21. Denoting the possible responses by $j = 0, 1, 2, 3$ where $j = 0$ corresponds to "not at all likely," and $j = 1, 2, 3$ to the progressively more likely responses, then the model assumes that for individual i with a vector of characteristics x_i

$$Prob\langle j_i | x_i \rangle = \begin{cases} \Phi(\mu_0 - x_i\beta) & \text{if } j_i = 0 \\ \Phi(\mu_{j_i} - x_i\beta) - \Phi(\mu_{j_i - 1} - x_i\beta) & \text{if } 0 < j_i < 3 \\ 1 - \Phi(\mu_3 - x_i\beta) & \text{if } j_i = 3 \end{cases}$$

where $\Phi(x)$ is the standard normal cumulative distribution function and μ_j $j = 0, 1, 2$ and β are parameters to be estimated.

expansion that followed. The GSS security index fell again during the recession of the early 1990s, though in contrast to the results for displacement rates, the lows associated with the 1990s recession were not as low as those associated with the 1980s recession. However, like the displacement results, the recovery in the job security index after the 1990s recession was quite slow considering the improvement that was occurring in the unemployment rate.

The lighter solid line in Figure 4 shows the time variation in the GSS security index adjusted for variation in the unemployment rate. Analogously to the adjusted displacement rates, these were obtained by augmenting the model generating the index with the national unemployment rate. Our results indicate that workers' perceptions of their job security at a given level of the unemployment rate have declined quite steadily over the period since 1977.

We have seen that the qualitative patterns in the adjusted displacement rate and GSS security indices are somewhat similar. Thus a natural question is whether the variation in the displacement rates fully explains the variation in the GSS security index. The dashed lines in Figure 4, which further augment the GSS index model with displacement rates, suggest that the answer to this question is no. The lighter dashed line shows the security index adjusted for unemployment and the displacement rate for workers with five or more years tenure that was discussed above. The heavier dashed line shows the index adjusted for a similarly constructed displacement rate for workers with at least one year tenure. If unemployment and displacement rates fully explained workers' perceptions of job security, then the adjusted indices represented by the dashed lines should be constant over time. However, both lines fall in the 1990s, suggesting that while related, the rise in the displacement rate and the fall in the index of workers' perception of security are separate phenomena.

Figure 5 illustrates the increased democratization of insecurity by allowing for a separate set of year dummies for workers with and without a college degree.[23] Results indicate that college-degreed workers have experienced a much sharper drop in perceived job security. Until the late 1980s, workers with college degrees reported considerably higher levels of job security. Since then, however, the security index for college educated workers has been somewhat lower than that for those without college degrees. Our previous work (Aaronson and Sullivan (1998a and 1998b), suggests that this trend towards democratization is also found in narrowing differentials by occu-

22. Higher values of the index correspond to a perception of greater security. The value for 1988 is normalized to zero.
23. The figures are relative to the left out category – noncollege educated workers in 1988.

pation and industry.

To summarize, while not justifying some of the extreme claims found in the popular news media, our results for both displacement and perceptions of job security are consistent with the arguments of policymakers who claim job insecurity increased in the 1990s. In particular, once we control for the level of the unemployment rate, insecurity seems to have risen appreciably in the 1990s. Moreover, the increase in insecurity has been most pronounced for groups, such as those with college degrees, that had historically had lower levels of insecurity. As a result, insecurity is now a considerably more democratic phenomenon.

III. The impact of insecurity on wages

To evaluate the effect of insecurity on wage growth we estimate standard, panel-data regression models relating wage growth at the state level to displacement and unemployment rates also at the state level:[24]

$$(1) \qquad \Delta w_{it} = \alpha_i + \beta_t + u_{it}\theta + \hat{d}_{it}\delta + \varepsilon_{it}$$

where u_{it} is the standard, state-level measure of unemployment reported by the BLS and $\hat{d}_{it}$ is a state-level version of the national displacement rate obtained by estimating

$$(2) \qquad r_{ist} = d_{it} + (s - t - 1)\gamma + \varepsilon_{ist}$$

where r_{ist} is the displacement rate for state i in year t measured by the survey taken in year s. Finally, Δw_{it} is the change in the demographically-adjusted log wage of state i from year $t-1$ to year t.[25]

Model (1) is in the spirit of Blanchflower and Oswald's (1994) "wage curve" specification with

24. One might also think of examining the relationship between wage levels and perceptions of insecurity at the *individual* level using the GSS data. We did this and obtained very large negative estimates of the "effect" of insecurity on wages. However, we suspect that at the individual level the causation runs mainly the other way. Thus it is more appropriate to analyze the relationship between aggregate wages and insecurity measures.

25. The adjusted log wage is obtained as the state-specific intercepts of year-specific regression models for log wages in the ORGs containing education, a quartic in experience interacted with sex, education, martial status, and full-time status. This specification for log wages follows that of column 3 of table 2 in Blanchard and Katz (1997).

the displacement rate as an additional variable. However, we follow the subsequent literature, in particular Blanchard and Katz (1997), in using an hourly wage measure, rather than annual earnings, derived from the ORG, rather than the March CPS. With such a specification we find that the estimated coefficient on the lagged wage is very close to unity in specifications in which it is allowed to be free. Thus for simplicity we work in wage changes. We also follow the previous literature in including state- and year-specific fixed effects. State effects account for, among other things, differences in industry mix. Year effects account for, among other things, inflation expectations and productivity growth (assuming these are national in scope) as well as changes in survey questions.

As we have already noted, measurement error in the displacement rates used to estimate model (1) is a significant concern. First, at the state level, the sample sizes underlying the DWS-based estimates of the survey-specific displacement rates are not enormous.[26] Second, as was noted in the last section, discrepancies between surveys covering the same years suggest some random subjectivity across survey responses. If such measurement error takes the classical form, estimation of model (1) by ordinary least squares will likely lead to the coefficient on displacement being biased towards zero as well as collateral damage to the consistency of the other parameter estimates. Statistical significance of parameter estimates will also be reduced. Moreover, the effects of measurement error are likely to be magnified by the inclusion of state- and year-specific fixed effects. Ordinary least squares estimation of model (1) including the fixed effects is equivalent to ordinary least squares estimation of $\Delta\tilde{w}_{it} = \tilde{u}_{it}\theta + \tilde{d}_{it}\delta + \tilde{\varepsilon}_{it}$ where, for example, $\tilde{d}_{it} = \hat{d}_{it} - \bar{d}_i - \bar{d}_t + \bar{d}$ and $\bar{d}_i$, $\bar{d}_t$, and $\bar{d}$ are the state-specific, year-specific, and overall means of the measured displacement rate. This differencing reduces the overall variation in the data, likely reducing the signal-to-noise ratio that determines the effects of measurement error.

Given the likely importance of measurement error, we considered a number of strategies for estimating model (1). The first choice concerns the level of aggregation. This has both a cross-sectional and time dimension. In the cross-sectional dimension, the relevant choices are the 50 states and the nine census regions. In this case, we found that the increase in degrees of freedom from using state level data more than offset the disadvantage from having each observation measured

26. On average, approximately 100,000 workers are asked whether they were displaced, or about 2,000 per state. Sample sizes for many states are, however, considerably smaller.

with more error. Thus all the results reported below are obtained from state level data.[27]

In the time dimension, there are also two obvious choices – annual and survey-period data. In the latter case, u_{it} and $\hat{d}_{it}$ are interpreted as the average values of unemployment and displacement over the years covered in the survey conducted in year t and Δw_{it} is the annualized rate of change in the wage rate over the years covered by the survey. Annual data, of course, yield more degrees of freedom. However, in the time dimension, we find that these additional degrees of freedom do not compensate for the greater measurement error that we find. Thus we focus mainly on results derived from survey-period data, though we present some results using annual data as well.

No matter how we aggregate the data, measurement error remains a concern. However, given estimates of the reliability of the data, we can obtain consistent estimates of the parameters of model (1). To simplify notation, let the true model be $y_{it} = x_{it}^* \lambda + \varepsilon_{it}$ where ε_{it} is an error term with the usual ideal properties and x_{it}^* is the true data. Let the observed data be $x_{it} = x_{it}^* + \mu_{it}$, where $Cov[\mu_{it}] = \Sigma_{it}$. This is slightly different than the usual textbook treatment of in that the measurement error variance varies by observation. However, as long as we maintain the assumptions that the $(y_{it}, x_{it}^*, \varepsilon_{it})$ are independent and identically distributed and that a law of large numbers applies to the μ_{it}, trivial modifications of the standard arguments show that consistent estimates of λ can still be obtained from

(3) $$\hat{\lambda}_{EIV} = \left[\sum_{it} x'_{it}x_{it} - \sum_{it}\Sigma_{it}\right]^{-1} \left(\sum_{it} x'_{it}y_{it}\right).$$

In the case in which the Σ_{it} are diagonal, the above error-in-variables estimator can be expressed in terms of the average reliabilities of the variables:

(4) $$\hat{\lambda}_{EIV} = \left[\sum_{it} x'_{it}Rx_{it}\right]^{-1} \left(\sum_{it} x'_{it}y_{it}\right)$$

where R is the diagonal matrix with average reliabilities down the diagonal. That is
$$R_{kk} = r_k = \left(\sum_{it} x_{kit}^2 - \sum_{it}\sigma_{ki}^2\right) / \left(\sum_{it} x_{kit}^2\right).$$

The most important source of sampling variability in our displacement data is from the estimates of the number of workers displaced. This estimated number is the product of the proportion of workers displaced and the sum of the weights, the latter of which is not stochastic. Thus the over-

27. Estimates based on regional-level data are consistent with the state-level estimates presented below in the sense that one cannot reject the hypothesis that the true coefficients are equal. Standard errors, however, tend to be significantly larger when based on region-level data.

all reliability of our displacement rates is approximately the same as the reliability of the proportion in the monthly CPS answering "yes" to the DWS question and reporting one of the standard reasons. For a given state and survey, the standard estimate of the sampling variance is $\hat{p}_{is}(1-\hat{p}_{is})/n_{is}$ where $\hat{p}_{is}$ is the proportion displaced and n_{is} is the sample size. To obtain the average reliability, we average these estimated sampling variances over the sample of states and years and then divide by the total variance of the measured displacement rates in the sample of states and years. Our estimates of this average reliability in the annual data are a little less than 0.5, suggesting that about half the variation is due to sampling error. However, with the survey-period data, the reliability estimates based on this estimator rise to between 0.8 and 0.9 depending on whether we compute the displacement rate for workers with at least five years or at least one year of tenure.

The reliability estimates just described account only for measurement error arising from the finite samples underlying the state-level displacement rates. However, Figure 2 suggested that there may also be nonsampling forms of measurement error that affect the displacement rate estimates we use in model (1). For the annual data, we can estimate the reliability in a way that accounts for both sampling and nonsampling forms of measurement error. Specifically, the standard estimates of the sampling variability of the d_{it} and $\hat{d}_{it} = \hat{d}_{it} - \tilde{d}_i - \tilde{d}_t + \tilde{d}$ associated with the regression model (2) reflect both kinds of potential measurement error. Such estimates depend on the estimated variance of the error term in (2), which would be zero if different surveys always yielded the same estimates of the displacement rate in a given year. To the extent that sampling or nonsampling errors cause the estimates from different surveys to differ, the estimates of the variances of the $\hat{d}_{it}$ and $\hat{d}_{it} = \hat{d}_{it} - \tilde{d}_i - \tilde{d}_t + \tilde{d}$ associated with the regression model (2) are larger. When we re-estimate the reliability of our annual displacement rates in this way, we obtain estimates of only about 0.25, which implies that nonsampling forms of measurement error are also important.

Table 2 reports our basic findings for the effect of overall displacement rates on overall wage growth. The first five columns of results are based displacement rates for workers with five or more years of tenure and the second set on displacement rates for workers with one or more years of tenure. Within each group, the first three columns of results are based on annual data and the last two on survey-level data. In each case, we first present OLS estimates and then errors-in-variables (EIV) corrected estimates. In the case of the annual data, the assumed reliability is obtained from the estimated sampling variabilities derived from model (2) and thus reflect both sampling and nonsampling measurement error. The survey-level data, on the other hand, reflect only the finite samples underlying the survey-level displacement rates. Finally, for the annual data, we also present instrumental variable estimates. These are based on the sample of years for which we have multiple estimates of the displacement rate. For these data, we use the estimate based on the first survey following the year as the measure of displacement and the estimate from the survey two years later as an instrument.

Using the annual measures of displacement, unemployment and wage growth, one cannot reject the hypothesis that the effect of insecurity on wage growth is zero. It is worth noting, however, that as we discuss below, the magnitudes of the EIV and IV displacement coefficients are economically large. Unfortunately, the standard errors associated with the estimates are comparably large, which implies that we cannot determine how important displacement is for wage determination using the annual data.

Stronger evidence of the importance of displacement rates for overall wage growth is, however, obtained from the survey-period data. These results allow us to reject the hypothesis that the true effect of insecurity is zero at moderate levels of confidence. The magnitudes of the coefficients are also economically meaningful. For instance, the results in the last section suggested that five year displacement rates were elevated in the 1990s by roughly 40%. The estimate of -0.0054 (0.0019) shown in the fifth column suggests that such higher rates of displacement would be associated with more than two tenths of a percentage point lower wage growth, which would be quite an important effect when maintained over several years. The estimate of -0.0043 (0.0020) based on the one year tenure sample implies a similar negative effect on wage growth in the 1990s. Finally, the results in the second row of Table 2 imply that the unemployment rate remains a robust determinant of wage growth even after controlling for displacement rates – t-statistics for the unemployment rate remain above ten and the coefficient estimates shown are quite similar to those obtained by Blanchard and Katz (1997) in a similar specification without the displacement rate.

However, this is not the full story. The impact of job security and unemployment rates vary substantially across groups of workers. Katz and Krueger (1999) document such variation in the effects of the unemployment rate on wage growth. The results in Table 3 show that there is similar variation in the effects of displacement on wage growth. In particular, the wage growth of workers with college degrees tends to be less sensitive to displacement rates than those of workers without college degrees. Using the five year tenure restriction, the EIV estimates of the effect of displacement using the education-specific displacement and unemployment rates are -0.0054 (0.0047) and -0.0090 (0.0023) for college-graduates and non-college-graduates respectively. Moreover, using the one year tenure restriction, the EIV estimate of the effect of displacement is only -0.0004 (0.0058) for college-graduates, while that for non-college-graduates remains high at -0.0090 (0.0023). Using aggregate, rather than education-group-specific measures of displacement and unemployment make relatively little difference to the estimates of the effects of displacement rates, but do change the unemployment coefficients in a way similar to that reported in Krueger and Katz (1999).

The results of Table 3 for workers without college degrees are statistically significant and relatively robust, while those for workers without college degrees are neither statistically significant nor robust. Because it is mainly college-educated workers who have seen increases in displacement rates, the results in Table 3 have important implications for the growth in aggregate wages. Based on the five year tenure sample, the point estimates for the effect of displacement on wage growth still imply a large effect on aggregate wage growth. However, given the standard error associated with the college-educated coefficients, one cannot be confident that the effect is very large. Moreover, the point estimates based on the one year tenure sample suggest very little effect on wage growth for college-educated workers, which would imply little effect on aggregate wages. So the evidence in Table 3, while establishing the importance of displacement rates for the growth of wages of non-college-educated workers, does not support the inference that the rise of displacement in the 1990s has had an important restraining effect on aggregate wage growth.

In Table 4, we explore the robustness of the results in Tables 2 and 3 to outliers. In particular, for overall, college-graduate, and non-college-graduate samples, we compare our OLS results to those obtained by least squares weighted by employment totals, median regression, and the robust regression procedure of Huber that represents a compromise between OLS and median regression. The latter two techniques give essentially no weight to large outliers and are thus robust to such data anomalies. Weighted regressions also provide a check that a few wild observations derived from small states and a based on few observations are not driving the results. The results in Table 4 generally indicate a fair degree of robustness in the results in Tables 3 and 4. Point estimates, while sometimes smaller than the corresponding OLS estimates, are relatively similar in magnitude. Moreover, the qualitative conclusion that the results are marginally statistically significant for the overall sample, insignificant for the college-educated sample, and strongly significant for the non-college-educated sample, continue to hold.[28]

Finally, we should note that we experimented with extensions of model (1) in which forms of the index of workers' perceptions of job security derived from the GSS were included in addition to the unemployment and displacement rates. Such specifications are motivated by our finding in Figure 4 that the trend in workers' perceptions of their security is a distinct phenomena from workers' reports of job displacement. Unfortunately, the GSS data only identify workers' Census region and even if we did know workers' states, sample sizes in the GSS would be too small to construct useful state-level data. Thus we were forced to analyze the data at the region-level. The resulting estimates of the effect of GSS-derived insecurity measures generally had very large standard errors and were far from statistical significance.[29] We conclude that while potentially interesting, the separate effects of worker perceptions on wage growth cannot be determined given the data currently available to us.

IV. Conclusions

We have shown that, relative to what would have been expected on the basis of the generally low unemployment rate, both displacement rates and workers' perceptions of their chances of job loss have been elevated during much of the 1990s. Adjusted for variation in the unemployment rate, the displacement rate for workers with at least five years tenure was approximately 40% higher in the 1990s than the 1980s. However, such findings by themselves do not prove that increased job insecurity has been an important factor in restraining aggregate wage growth. To adequately evaluate such claims, one needs evidence on the effect of insecurity on wage growth.

Our results provide the first evidence that higher levels of insecurity do reduce wage growth. However, we find evidence of this effect only for workers without college degrees. On the one hand, for such workers, the magnitude of the effect of variation in displacement rates on wage growth is quite large. On the other hand, the level of the displacement rate does not appear to be an important factor in aggregate wage determination for workers with college degrees. One might speculate that this difference in the importance of insecurity arises because of differences between college and non-college workers in the importance of general skills relative to rents associated with specific jobs. For workers with college degrees, the value of their general skills may depend more on the balance of supply and demand in a wide labor market of many employers than on fac-

28. None of the coefficients in Table4 have been adjusted for the effects of measurement error which explains why they are lower than our preferred estimates. Measurement error corrections generally increase standard errors by approximately the same factor as point estimates. So, statistical significance is little effected.

29. Including GSS-based measures of worker' perceptions of job security also did not materially change the estimates of the effects of unemployment and displacement rates.

tors specific to their current jobs. Thus the increased risk of the loss of their current job may have less effect on their wage growth. In contrast, for many workers without college degrees, the loss of rents from current jobs may be a greater concern.

One the one hand, we found that while college graduates experienced a relatively major increase in insecurity, insecurity does not appear to have an effect on their wage growth. On the other hand, while insecurity does effect the wage growth of non-college graduates, we found that such workers have not seen sharp increases in insecurity. Thus the increase in worker insecurity in the 1990s likely had only a modest effect on aggregate wage growth.

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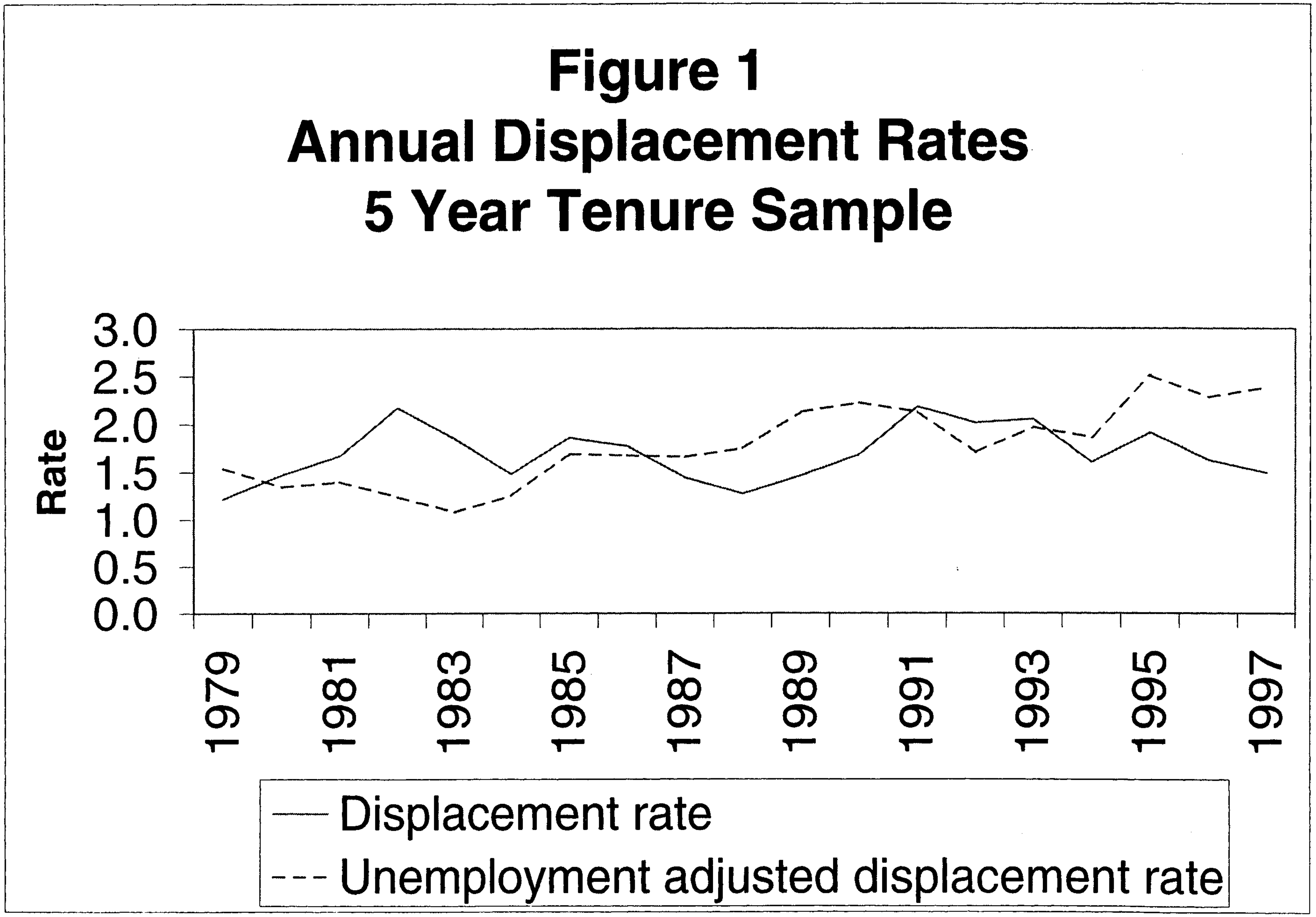


Figure 1
Annual Displacement Rates
5 Year Tenure Sample
Rate
3.0
2.5
2.0
1.5
1.0
0.5
0.0
1979
1981
1983
1985
1987
1989
1991
1993
1995
1997
Displacement rate
Unemployment adjusted displacement rate

Figure 2
Displacement Rates, by Survey
5 year tenure sample

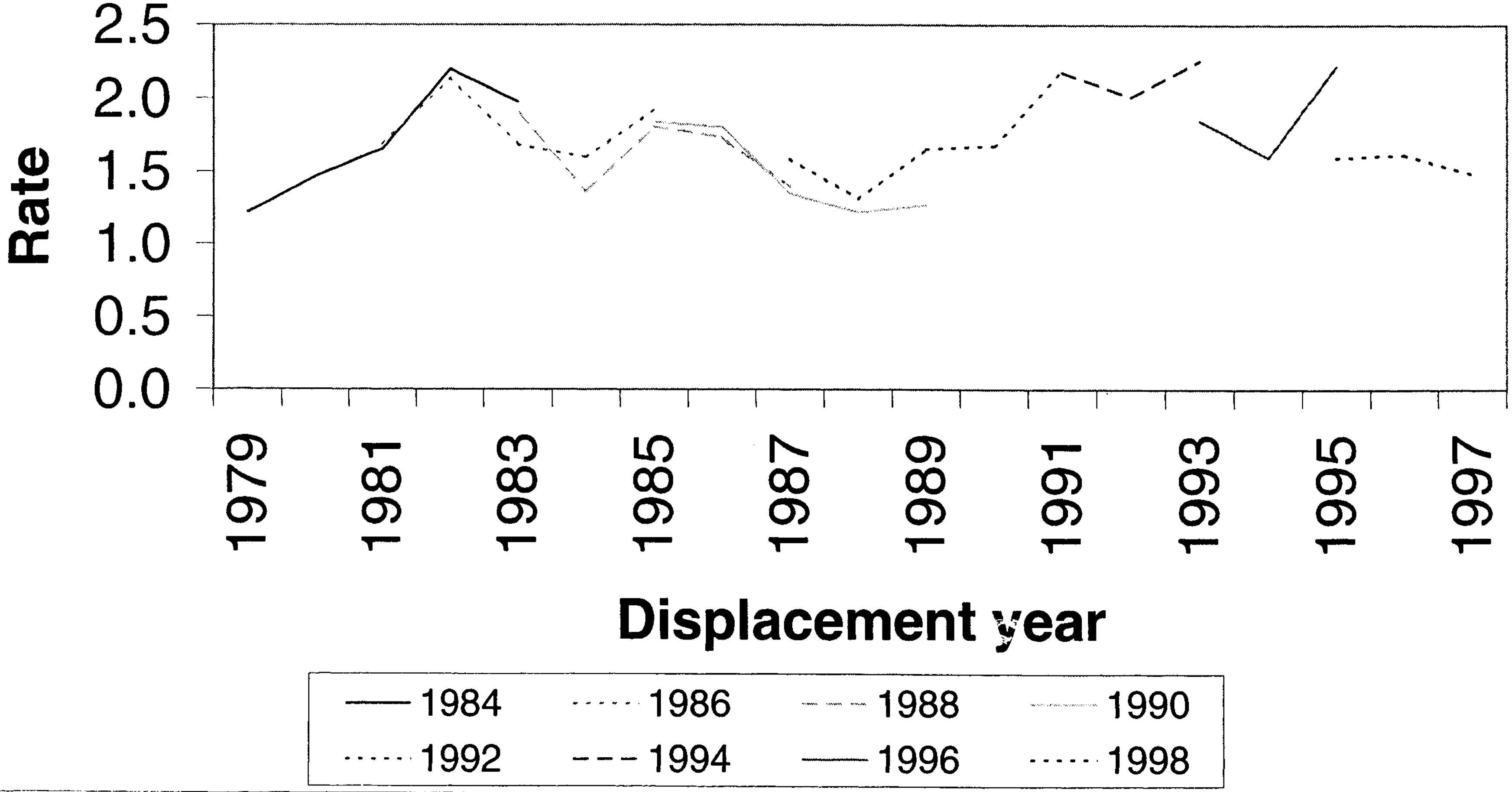
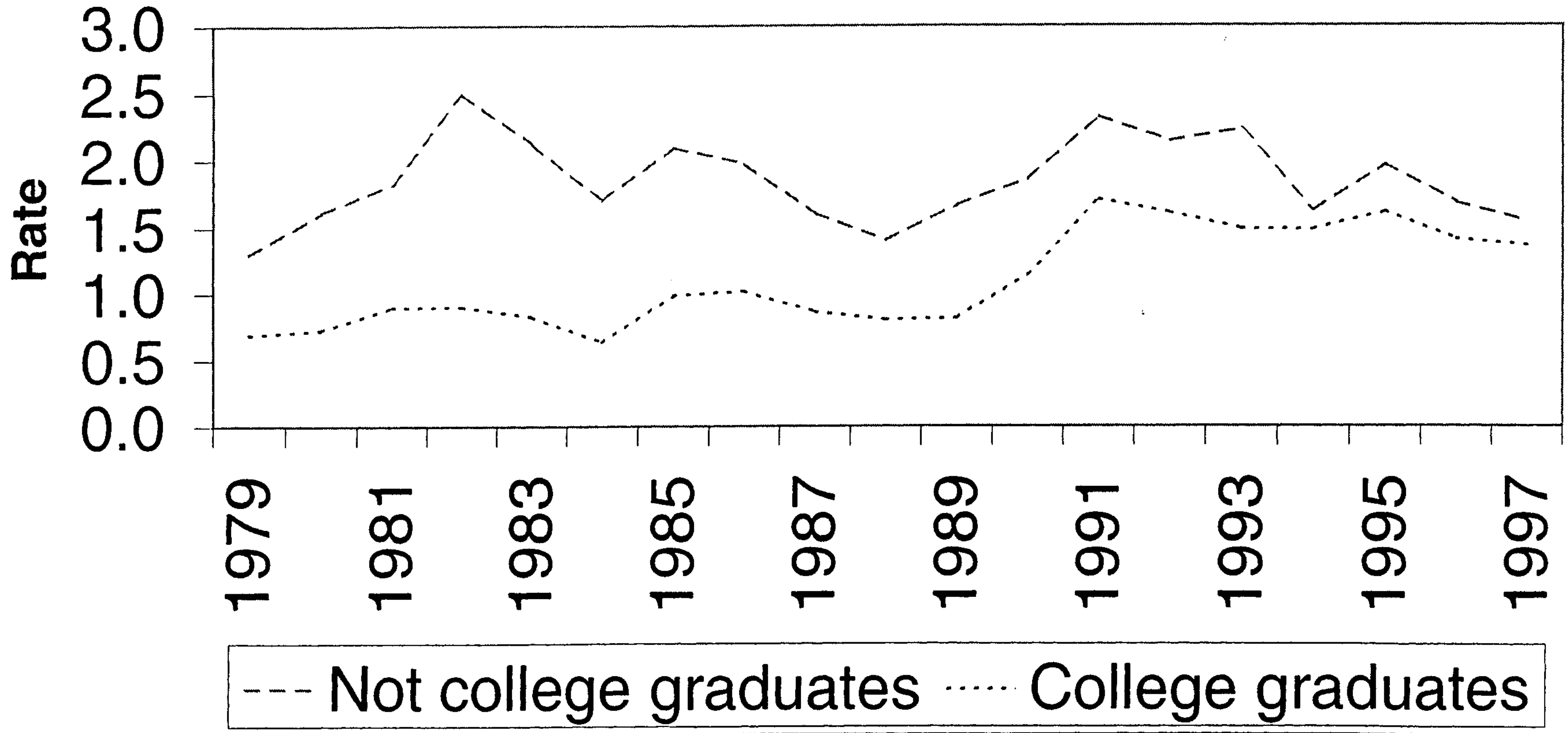


Figure 3
Displacement Rates, by Education
5 Year Tenure Sample



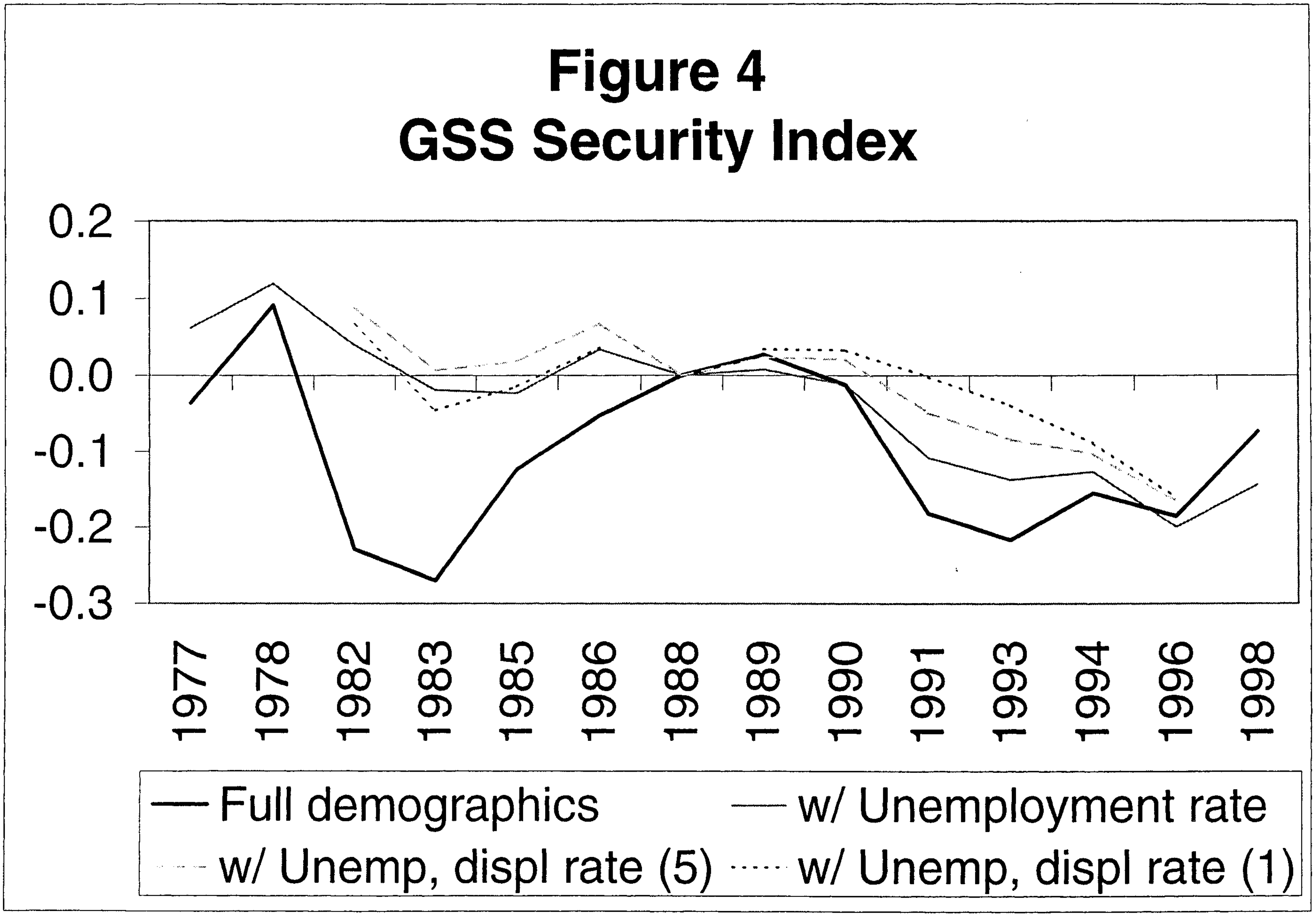


Figure 4
GSS Security Index
0.2
0.1
0.0
-0.1
-0.2
-0.3
1977
1978
1982
1983
1985
1986
1988
1989
1990
1991
1993
1994
1996
1998
Full demographics
w/ Unemployment rate
w/ Unemp, displ rate (5)
w/ Unemp, displ rate (1)

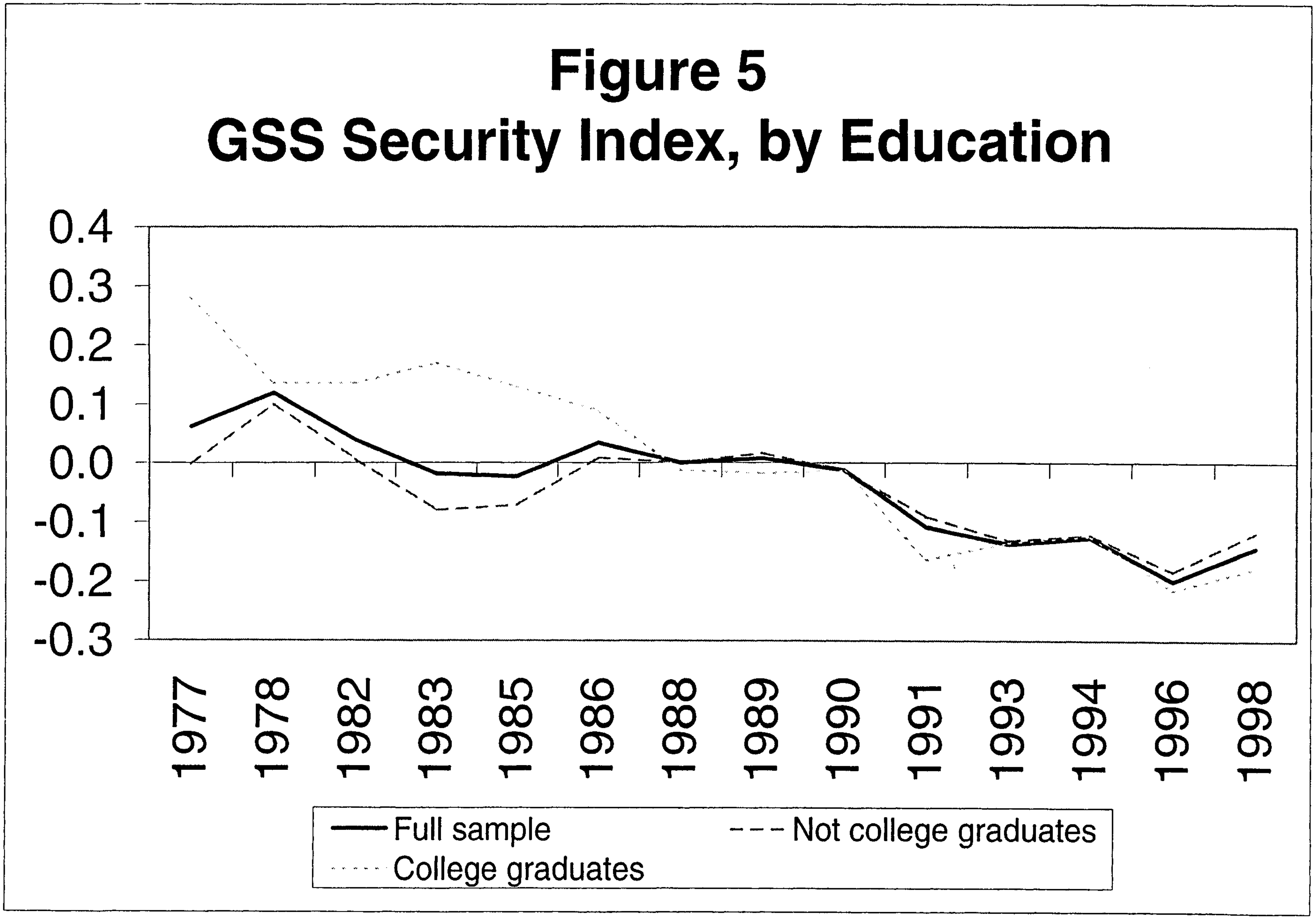


Figure 5
GSS Security Index, by Education
0.4
0.3
0.2
0.1
0.0
-0.1
-0.2
-0.3
1977
1978
1982
1983
1985
1986
1988
1989
1990
1991
1993
1994
1996
1998
Full sample
Not college graduates
College graduates

Table 1
Likelihood of losing your current job in the next year [1]

	Coefficient	Std err		Marginal effect on base case probability: Not at all likely	Not too likely	Fairly likely	Very likely
1977	-0.036	0.068		-0.012	0.007	0.003	0.002
1978	0.092	0.070		0.030	-0.019	-0.006	-0.006
1982	-0.229	0.068	*	-0.083	0.046	0.018	0.019
1983	-0.270	0.067	*	-0.098	0.054	0.021	0.023
1985	-0.123	0.067		-0.043	0.025	0.009	0.009
1986	-0.053	0.069		-0.018	0.011	0.004	0.004
1989	0.027	0.076		0.009	-0.006	-0.002	-0.002
1990	-0.012	0.076		-0.004	0.003	0.001	0.001
1991	-0.181	0.073	*	-0.065	0.037	0.014	0.014
1993	-0.216	0.072	*	-0.078	0.044	0.017	0.017
1994	-0.155	0.064	*	-0.055	0.032	0.012	0.012
1996	-0.186	0.064	*	-0.066	0.038	0.014	0.015
1998	-0.073	0.065		-0.026	0.015	0.005	0.005
Agriculture	0.076	0.094		0.026	-0.016	-0.005	-0.005
Construction	-0.302	0.067	*	-0.112	0.059	0.025	0.028
Manufacturing	-0.222	0.052	*	-0.081	0.044	0.018	0.019
Trans, comm, and utilities	-0.061	0.062		-0.021	0.012	0.005	0.005
Wholesale trade	-0.050	0.080		-0.018	0.010	0.004	0.004
Retail trade	-0.129	0.055	*	-0.046	0.026	0.010	0.010
FIRE	0.061	0.065		0.021	-0.013	-0.004	-0.004
Services	-0.080	0.048		-0.028	0.016	0.006	0.006
Professional, technical	0.144	0.045	*	0.048	-0.030	-0.010	-0.009
Managerial	0.248	0.046	*	0.080	-0.051	-0.016	-0.014
Sales	0.101	0.053		0.034	-0.021	-0.007	-0.006
Craftsman	-0.016	0.050		-0.005	0.003	0.001	0.001
Operative or laborer	-0.159	0.045	*	-0.058	0.032	0.012	0.013
Service worker	0.129	0.045	*	0.043	-0.027	-0.009	-0.008
Female	-0.033	0.027		-0.012	0.007	0.002	0.002
Black	-0.268	0.037	*	-0.099	0.053	0.022	0.024
Other race	-0.103	0.063		-0.037	0.021	0.008	0.008
Age 18-24	0.019	0.041		0.006	-0.004	-0.001	-0.001
Age 44-65	0.130	0.028	*	0.044	-0.027	-0.009	-0.008
Never married	-0.101	0.032	*	-0.036	0.021	0.008	0.008
Divorced	-0.040	0.035		-0.014	0.008	0.003	0.003
Separated	-0.205	0.061	*	-0.075	0.041	0.016	0.017
Widowed	-0.189	0.073	*	-0.069	0.038	0.015	0.016
High school dropout	-0.135	0.037	*	-0.048	0.027	0.010	0.011
College graduate	0.016	0.036		0.005	-0.003	-0.001	-0.001
Graduate school graduate	0.071	0.051		0.024	-0.015	-0.005	-0.005
New England	0.101	0.061		0.034	-0.021	-0.007	-0.006
Mid Atlantic	-0.013	0.044		-0.004	0.003	0.001	0.001
East North Central	0.093	0.042	*	0.032	-0.019	-0.006	-0.006
East South Central	0.048	0.056		0.016	-0.010	-0.003	-0.003
South Atlantic	0.029	0.042		0.010	-0.006	-0.002	-0.002
West North Central	0.056	0.052		0.019	-0.012	-0.004	-0.004
West South Central	-0.024	0.049		-0.008	0.005	0.002	0.002
Mountain	-0.095	0.055		-0.034	0.019	0.007	0.007
Intercept 1	0.524	0.074	*				
Intercept 2	1.436	0.075	*				
Intercept 3	1.862	0.076	*				
log likelihood		-20,221					

Notes:
[1] * = significant at the 5% level. Sample size is 11, 014.

Table 2
The effect of displacement on hourly wage growth
CPS outgoing rotation samples, 1979-1997
Basic estimates

	5 year tenure sample					1 year tenure sample				
	Annual			Survey period		Annual			Survey period	
Log displacement rate	-0.0018	-0.0123	-0.0029	-0.0035	-0.0054	-0.0029	-0.0188	-0.0196	-0.0034	-0.0043
	(0.0014)	(0.0104)	(0.0143)	(0.0012)	(0.0019)	(0.0020)	(0.0133)	(0.0133)	(0.0016)	(0.0020)
Log unemployment rate	-0.0435	-0.0343	-0.0472	-0.0480	-0.0465	-0.0425	-0.0311	-0.0342	-0.0485	-0.0479
	(0.0037)	(0.0092)	(0.0127)	(0.0026)	(0.0028)	(0.0038)	(0.0102)	(0.0117)	(0.0026)	(0.0027)
F-statistic on instrument			6.1					18.8		
Reliability ratio		0.22			0.83		0.28			0.93
Sample size	910	910	583	408	408	918	918	611	408	408
Method	OLS	EIV	IV	OLS	EIV	OLS	EIV	IV	OLS	EIV
State fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year or survey fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Table 3
The effect of displacement on hourly wage growth
CPS outgoing rotation samples, 1979-1997
By Education group and survey period, alternative aggregations

	5 year tenure sample						1 year tenure sample					
	College graduates			Non-college graduates			College graduates			Non-college graduates		
Log displacement rate	-0.0012	-0.0054		-0.0042	-0.0093		-0.0001	-0.0004		-0.0066	-0.0090	
-- education-specific	(0.0011)	(0.0047)		(0.0014)	(0.0030)		(0.0013)	(0.0058)		(0.0017)	(0.0023)	
Log displacement rate			-0.0007			-0.0041			-0.0002			-0.0043
-- overall			(0.0020)			(0.0014)			(0.0026)			(0.0018)
Log unemployment rate	-0.0276	-0.0257		-0.0486	-0.0450		-0.0258	-0.0257		-0.0475	-0.0461	
-- education-specific	(0.0036)	(0.0042)		(0.0029)	(0.0034)		(0.0036)	(0.0040)		(0.0029)	(0.0030)	
Log unemployment rate			-0.0408			-0.0509			-0.0412			-0.0514
-- overall			(0.0042)			(0.0028)			(0.0043)			(0.0029)
Wald p-stat, college vs. no college:												
Log displacement rate				0.092	0.484	0.164				0.002	0.168	0.195
Log unemployment rate				0.000	0.000	0.045				0.000	0.000	0.049
Reliability ratio		0.50			0.72			0.57			0.90	
Sample size	380	380	408	408	408	408	407	407	408	408	408	408
Method	OLS	EIV	OLS	OLS	EIV	OLS	OLS	EIV	OLS	OLS	EIV	OLS
Survey and state fixed effect	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Table 4
The effect of displacement on hourly wage growth
CPS outgoing rotation samples, 1979-1997
Robustness of results[1]
By displacement survey period

	All				College graduates				Not college graduates			
	OLS	WLS	Median	Robust	OLS	WLS	Median	Robust	OLS	WLS	Median	Robust
5 year tenure sample												
Log displacement rate					-0.0012	-0.0016	-0.0008	-0.0009	-0.0042	-0.0029	-0.0056	-0.0032
-- education-specific					(0.0011)	(0.0011)	(0.0017)	(0.0011)	(0.0014)	(0.0013)	(0.0016)	(0.0012)
Log displacement rate	-0.0035	-0.0024	-0.0027	-0.0020								
-- overall	(0.0012)	(0.0012)	(0.0016)	(0.0011)								
Log unemployment rate					-0.0276	-0.0291	-0.0330	-0.0313	-0.0486	-0.0469	-0.0469	-0.0470
-- education-specific					(0.0036)	(0.0036)	(0.0069)	(0.0036)	(0.0029)	(0.0026)	(0.0036)	(0.0025)
Log unemployment rate	-0.0480	-0.0453	-0.0496	-0.0475								
-- overall	(0.0026)	(0.0023)	(0.0034)	(0.0023)								
1 year tenure sample												
Log displacement rate					-0.0001	0.0004	-0.0006	0.0004	-0.0066	-0.0064	-0.0055	-0.0048
-- education-specific					(0.0013)	(0.0015)	(0.0018)	(0.0013)	(0.0017)	(0.0017)	(0.0025)	(0.0016)
Log displacement rate	-0.0034	-0.0036	-0.0019	-0.0016								
-- overall	(0.0016)	(0.0016)	(0.0022)	(0.0015)								
Log unemployment rate					-0.0258	-0.0279	-0.0325	-0.0275	-0.0475	-0.0448	-0.0492	-0.0465
-- education-specific					(0.0036)	(0.0035)	(0.0056)	(0.0035)	(0.0029)	(0.0026)	(0.0040)	(0.0026)
Log unemployment rate	-0.0485	-0.0447	-0.0513	-0.0481								
-- overall	(0.0026)	(0.0023)	(0.0032)	(0.0024)								

Notes:
[1] All regressions include survey and state fixed effects. Weighted regressions use state employment weights. Median regression standard errrors are bootstapped with 500 replications. Sample size is 408 state-years for the noncollege and total samples. For the college graduate results, the 5 year tenure sample includes 380 state-years and the 1 year tenure sample includes 407 state-years.