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ABSTRACT

To determine whether there has been a secular rise in job instability among young adults over the past 3 decades, a study compared two National Longitudinal Survey cohorts of young white men. The first cohort entered the labor market in the late 1960s and early 1970s, the second during the late 1970s and early 1980s. The study examined longitudinal data on work history and schooling and found a significant increase in the rate of job changing across the two cohorts. The trend toward lower marriage rates and longer transitions into the labor market explained some increase. The economy's shift toward the service sector played an important role, although declines in stability occurred in traditionally unionized industries as well. The overall rise in instability resulted in shorter median tenures. Although greater job instability and shorter tenures are not necessarily a bad thing, findings indicated young workers in recent years failed to capture the all-important wage gains that were associated with job changing in the past. This deterioration in wage gains was felt largely by less educated workers, but inequality in these gains also increased for all education groups. In combination, findings suggested a decline in the long-term economic welfare among those who entered the labor market in the 1980s. (Appendixes contain the following: 43 references; comparison of estimates of job change rates; adjusting for attrition; permanent wage estimation; 5 tables; and 5 figures.) (Author/YLB)

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Institute on Education and the Economy

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Abstract

In this paper, we take up the question of whether there has been a secular rise in job instability among young adults over the past three decades. We compare two NLS cohorts of young white men – the first cohort entering the labor market in the late 1960s and early 1970s, and the second during the late 1970s and early 1980s. Using longitudinal data on work history and schooling, we find a significant increase in the rate of job changing across the two cohorts. Some of the increase is explained by the trend toward lower marriage rates and longer transitions into the labor market. The economy's shift toward the service sector has also played an important role, although declines in stability have occurred in traditionally unionized industries as well. The overall rise in instability has resulted in shorter median tenures. While greater job instability and shorter tenures are not necessarily a bad thing – job changing is often beneficial to early wage growth – we find that young workers in recent years have failed to capture the all-important wage gains that were associated with job changing in the past. This deterioration in wage gains has been felt largely by less educated workers, but inequality in these gains has also increased, for all education groups. In combination, our findings suggest a decline in the long-term economic welfare among those who entered the labor market in the 1980s.

Table of Contents

ERIC

I. Background

When the *New York Times* ran its eight-part series on "The Downsizing of America" two years ago, the response was overwhelming (The New York Times 1996). The newspaper was flooded with letters and set up an Internet site to accommodate the need for more information and for people to share similar experiences of being downsized, reengineered, and restructured. Other media took up the ball, reporting on downsizing in their local communities but also commenting on the *Times* articles themselves. The series clearly captured a strong anxiety in the national psyche about increasing job insecurity, an anxiety that had built up during the numerous layoffs of the early 1990s.

While the perception of increased job insecurity is widespread, empirical documentation of this 'fact' remains elusive. The primary sources of cross-sectional data on job stability are the CPS tenure and pension and benefit supplements, and the Displaced Worker Survey. Using the CPS, Swinnerton and Wial (1995) find evidence of a secular decline in job stability whereas Diebold, Neumark, and Polsky (1994), Farber (1995), and Stewart (1997) do not. Changes in the wording of the CPS tenure question and in non-response rates over time hamper the building of synthetic age cohorts and duration analysis and make it difficult to resolve the conflicting findings. Neumark, Polsky, and Hansen (1997) add recent CPS data and attempt to better adjust for changes in wording and other data problems. They find that job stability declined modestly in the first half of the 1990s among older workers with longer tenures, which fits the typical picture of downsizing among managerial ranks. Using the Displaced Worker Survey, Farber (1996) finds a mild increase in the overall rate of involuntary job loss in the 1990s using these data, but changes in the wording of the question over time make analysis difficult here as well.

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In part because of these measurement problems, the focus has recently shifted toward the analysis of longitudinal datasets, which allow more direct calculation of job change rates. Initial research on the PSID appeared to provide consistent evidence of a general increase in the rate of job changing (Marcotte 1996, Rose 1995, Boisjoly, Duncan and Smeeding 1994). But several recent papers find no such overall trend, and the disagreement hinges on how one measures year-to-year job changes (Polsky 1996, Stevens 1996a). Because employers in the PSID are not uniquely identified, a job change must be inferred using several different questions about length of time with current employer, and some of the wording has changed over time – see Brown and Light (1992) for a detailed discussion. Monks and Pizer (1997) present the only other analysis of NLS data besides ours that we know of, and they find an increase in job instability among young workers between 1971 and 1990. We expand on their research by analyzing the wage outcomes of job instability, the attrition effect in the NLS data, and the comparability of the NLS data to the PSID and CPS.

While there is no consensus about overall trends, several recurring themes emerge from this research. First, it does appear that the rate of *involuntary* job loss has increased over the past two decades, which captures well the public anxiety about lay-offs and plant closings. Second, there is some agreement that less educated workers, black workers, and young workers have experienced an overall increase in job instability. This second point has perhaps not received enough attention. Such groups have traditionally been at the periphery of the labor market and may therefore enjoy fewer protections against the effects of changed employment and business practices. The experience of young workers in recent years may be especially telling: they are the first generation to enter and establish their careers in the post-industrial labor market, thus highlighting in what ways, if any, the employment relationship has changed.

In this paper, we take up the question of whether there has been a secular rise in job instability among young adults over the past three decades. We compare two NLS cohorts of young white men – the first cohort entered the labor market in the late 60s and early 70s, the second entered the labor market during the late 70s and early 80s. Using longitudinal data on work history and schooling, we first ask whether the rate of job changing has increased across the two cohorts. After finding evidence of such an increase, we then trace its effect on median tenure. Job instability and shorter tenures are not necessarily a bad thing: in the early years, job shopping is often highly beneficial to wage growth and career development. We therefore test for cohort differences in the wage gains that young workers capture as they engage in job search and then eventually settle with one employer.

Roughly two-thirds of life-time job changes and wage growth occur during the years we observe for these cohorts. These are the formative years of labor market experience when long-term relationships with employers are established. Our findings thus capture most of the economic and career mobility that these young men can expect to achieve over their lifetime.

II. Data, Attrition, and Measures

Data

We compare two datasets from the National Longitudinal Surveys. The first is the National Longitudinal Survey of Young Men: a representative sample of young men was interviewed in 1966 and tracked until 1981, reinterviewed yearly in that time span except for 1972, 1974, 1977, and 1979. The second is the male sample of the National Longitudinal Survey of Youth: a representative sample of young men was interviewed in 1979 and has been interviewed yearly since then, with 1994 the most recent available year. Throughout, we refer to the former as the "original cohort" and to the latter as the "recent cohort". The initial baseline sample selection for both cohorts is as follows. We selected non-Hispanic whites only, because attrition among non-whites was extreme in the original cohort. We also dropped the poor white supplemental sample and the military supplemental sample from the recent cohort (further selections are done below).

It is important to stress that the NLS data are not representative of the entire population over time, unlike the other main longitudinal dataset, the PSID. Instead, the NLS data comprise a representative sample of a moving 8-year age window: from the ages of 14-21 at the beginning of the panel to the ages of 30-37 at the end. The power of this research design lies in the fact that we observe both cohorts across a full 16 years, at exactly the same ages, with comparable information on schooling, work history, and job characteristics. This enables us to isolate the impact of potential differences in the economic context of their early career development: the original cohort entered the labor market in the late 1960s at the tail of the economic boom, while the recent cohort entered the labor market in the early 1980s after the onset of economic restructuring.

There are few previous studies that compare these two datasets, so we have conducted a series of analyses to establish the representativeness and comparability of the samples, and the impact of differential attrition bias. We found no problems with the representativeness and comparability of the initial starting-year samples of the two cohorts. The starting age distributions differ slightly, so we control for age in all analyses. While the pattern of missed interviews during the survey span differs between the two cohorts, detailed recovery of data has minimized bias on that account (details can be found in Bernhardt, et al. (1997)). We also investigated a potential problem that had been noted in previous research concerning wage dispersion trends in the recent cohort (Gottschalk and Moffit 1997), and were able to document the validity of the NLSY data.

Finally, one of the best known characteristics of the original cohort data is that about one third of the respondents served in the Vietnam War at some point during the survey years. Surprisingly, the timing and rate of attrition is similar for veterans and non-veterans: a majority of the veterans returned to the survey after their military service, behaving much like the general population in terms of attrition after their return. Of course the veterans lost several years of experience in the civilian labor market during their years of military service. They therefore show a clear time lag in their entry into the labor market, with shorter tenures and less accumulated work experience by their early 30s. We adjust for this in the analyses below. Beyond this time lag, however, we found no significant bias on other dimensions (e.g. employment rates, hourly wages, occupation). It appears that the veterans were able to make up most, if not all, of the lost ground, a finding consistent with other research (Berger and Hirsch 1983).

Attrition

The attrition rate is considerably higher for the original cohort than for the recent cohort (25.8% vs. 7.8%). There is consensus that attrition has not compromised the representativeness of the recent cohort, but findings for the original cohort are mixed (O'Neill 1982, Parsons 1987, Rhoton 1984, Falaris and Peters 1996). NLS revised the original base-year weights in each subsequent survey year to account for permanent attrition and non-response within any given year, and we use these weights throughout. However, these adjustments were only intended to compensate for potential non-response bias along the main sampling dimensions (race, geographic residence, family income), not along the outcome dimensions that are the focus of this paper. A plausible scenario would be that the respondents lost to attrition in the original cohort were more unstable than those who were retained, leading to an underestimate of the job change rate in the original cohort, and an overstatement of the cohort difference. We therefore investigated the extent to which the differential attrition rates between the two cohorts might have affected the cohort differences that we observe.

We used two strategies to examine the effect of attrition. First, we compared job change estimates from the NLS to estimates from two other well-known datasets: the PSID (from Polsky 1997) and the CPS (from Stewart 1998). Our goal was to benchmark both cohorts against other datasets to see whether the comparisons yielded consistent results. If attrition in the original cohort biases the estimates of job instability, then we would expect that job change estimates from the original cohort will not match up well with the other datasets, while estimates from the recent cohort will match up well (since attrition in the recent cohort was negligible). We found nearly perfect agreement between NLS and PSID estimates for both cohorts. This is our preferred comparison, since the measures and risk sets are directly analogous between the two datasets. The results of the comparison between the NLS and CPS were much less clear. The problem here is that the two datasets have different measures and different risk sets, making reconciliation difficult (see Jaeger and Stevens (1998) for a similar attempt to reconcile the PSID and CPS). A detailed discussion of our cross-dataset comparison can be found in Appendix A.

Our second strategy was to make several direct adjustments to our estimate of the cohort difference in job stability, to remove the potential effects of attrition bias. These adjustments are given in Section 3 and discussed in detail in Appendix B. We find that while attritters in the original cohort do have a higher rate of job changing (net of basic controls), adjusting for this

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difference does not significantly lower our estimate of the cohort difference – the strongest adjustment lies within one standard error of the unadjusted estimate and reduces the cohort effect by only 10%. We also investigated the effect of attrition on wages, our second outcome variable. Here we found that controlling for age and education removes any attrition bias in wages (as is true with other key variables such as employment status, labor force participation, tenure, and work experience). We therefore control for age and education in the wage change models in Section 4.

In sum, both the cross-dataset comparisons and the direct adjustments suggest that while attrition bias does exist in the original cohort, it does not alter either the statistical significance or the substance of the findings we report below.

Measures

The NLS data have a distinct advantage for this research area because they provide unique employer identification codes. [1] We can therefore unambiguously measure whether an employer change occurred over a given time span – rather than having to infer a job change, as must be done with other datasets. Specifically, we focus on the respondent's main "CPS" employer at the time of the survey. The CPS employer is identified in the same way across both cohorts in all survey years: if the respondent held more than one job at the time of the survey, he is asked to focus on the one at which he worked the most hours, either through an explicit question or via the interviewer's instructions. The respondent is then asked, "For whom did you work?" and the name of the employer is entered. In the original cohort, the CPS employer is

[1] Brown and Light (1992) find that these employer codes contain a negligible amount of error and are the best source of employer identification in the NLS datasets and also the best measure available from all the main longitudinal datasets.

then assigned an employer code that is unique across all interview years. In the recent cohort, unique identification of the CPS employer is only possible between any two consecutive years.[2] By successively linking pairs of years, however, we can trace a unique employer over any time span *as long as that employer is present in each year.* Therefore, we only capture continuous work with an employer in the recent cohort, and we have restricted our use of the employer codes in the original cohort data to match this definition.

As mentioned, four years were skipped in the original cohort's survey. This means that we cannot construct a continuous series of year-to-year employer comparisons across the span of the survey. We have therefore taken a very precise approach, by constructing a series of two-year employer comparisons. These are strictly matched between the two surveys, so that we are comparing job changes at exactly the same ages and at exactly the same time during the survey period. There are six such comparisons for each cohort and they are evenly spaced across the survey time span. Table 1 shows the years that we use for the analyses below and defines the six comparisons being made for each cohort; Table 2 shows the resulting sample characteristics.

We define a job separation as follows. For each two-year comparison, the risk set in year t is all employed respondents, not self-employed or working without pay, who are also observed in year $t+2$. If the respondent is unemployed or out-of-labor force in year $t+2$, an employer separation occurred. If the respondent is employed in year $t+2$, then the employer code for the CPS employer in year t is compared to the CPS employer code in year $t+2$. An employer separation occurred if these codes differ. Given these definitions, the empirical two-year separation rate is calculated as the number of respondents who have left their year t employer by

[2] The fact that we focus only on the CPS job is important. As noted in Neumark (1997), for the recent cohort information is gathered on up to five jobs every year, which is not true for the original cohort. In order to ensure strict comparability in the job change measure across the two cohorts, it is therefore important to only consider the main CPS job, which is uniquely identified and tracked in the same way for both cohorts.

year $t+2$, divided by the total number of respondents in the risk set in year t. In the discussion below, we will often use the term "job changing" for convenience, but it is important to remember that we are measuring separations from employers, not job changes within employers.[3]

III. Trends in Job Instability

The key point of interest is whether the two-year separation rates differ between the two cohorts. Figure 1 shows the empirical cohort differences, overall and broken down by age, education, and tenure. Without any adjustments, 46.4% of the original cohort and 52.7% of the recent cohort had left their current employer two years later, a 13.6% proportionate increase in the rate of job changing. The next panel illustrates the well-known fact that age is one of the central determinants of job instability. In general, roughly two-thirds of life-time job changes occur in the first ten years of labor market experience for the average male worker (Topel and Ward 1992). The recent cohort, however, has a clear disadvantage in attaining employment stability with age. Education also has a strong impact on job changing, as can be seen in the next panel.[4] High school dropouts have the highest separation rate, while the other education groups have relatively similar, and lower, rates. But the recent cohort again shows a higher rate of job changing in all education groups. Tenure also affects job instability, as shown in the final panel: the longer a worker stays with his current employer, the less likely he is to leave that employer.

[3] Unfortunately, we cannot disaggregate voluntary from involuntary job changes, because the missing data on this variable in the original cohort are both frequent and very likely biased.

[4] We are here looking at the *final* education level reached by the respondents, so that we do not confound education with current enrollment.

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Here again, however, the higher levels of instability for the recent cohort are consistent throughout the tenure distribution.

All of these dimensions (age, education, tenure) change simultaneously as the cohorts are surveyed over time. We therefore move directly to modeling the separation rates to determine whether there has been a "real" increase in the rate of job changing, net of compositional shifts in these variables. Similar to other research in this area, we estimate a logistic regression model of the form:[5]

$$\text{logit}(P_{ijt}) = \theta_0 X_{ijt} + \theta_1 J_{ijt} + \theta_2 U_{it} + \theta_3 C_i + \phi_i ,$$

where P_{ijt} is the probability that individual i in job j in year t has left that job by year $t+2$, X_{ijt} represents time-varying characteristics of the respondent, J_{ijt} represents time-varying characteristics of the job, including tenure, U_{it} represents the local unemployment rate in the individual's labor market in year t, and C_i represents a cohort indicator variable, coded 0 for the original cohort, 1 for the recent cohort. Finally, there is likely to be residual heterogeneity among individuals in the probability of making a job change which we do not capture in these terms of the model. A common solution is to fit the above models with random effects (Light and Ureta 1992, Farber 1992, Topel and Ward 1992, Heckman and Singer 1984). We therefore include a simple random effect, ϕ_i, that captures the deviation of individual-specific intercepts from the average fixed-effect intercept.[6] The standard deviation of the individual effects are statistically significant in all versions of the model estimated below, but there is very little impact

[5] We do not fit a duration model because end-dates for jobs are impossible to recover consistently for both cohorts for all years.

[6] We model the ϕ_i as conditionally independent given the other regressors and following a mean zero Gaussian distribution. This is a generalized linear mixed-effects model (McCulloch 1997) and we fit it using the statistical package Splus with a macro from Norleans (1995).

on most of the fixed-effects parameter estimates.[7] Thus our substantive findings, in particular our estimate of cohort differences in stability, are robust to the effects of unobserved heterogeneity that is uncorrelated with the other regressors.

The main point of interest here is to estimate the secular changes in the rate of job separation across the two cohorts. This can be modeled by fitting a continuous time trend, or by fitting a cohort indicator variable (or a more complicated set of discrete effects). If we were willing to assume that all cohort and aging effects were adequately controlled for by the human capital and job-related variables, then we could fit a time trend that captures secular changes in the job separation rate between 1967 and 1993. We are not willing to make this assumption, however, and have therefore taken the more conservative route of modeling the aggregate difference between the two cohorts with an indicator variable. Note, however, that the cohort indicator is also a time period indicator (1967-1980 compared to 1980-1993) and therefore captures the aggregate difference in job stability between the two periods.

It is also important to understand the role that tenure plays in this model. As the workers in the recent cohort begin to change employers more frequently than the original cohort, their tenures become progressively shorter. So toward the end of the time period studied, part of the cohort difference will actually be absorbed by the effect of tenure. Thus we are very likely underestimating the true difference between the cohorts: to wit, if we estimate model 1 without tenure, the recent cohort has a 43% higher odds of a job change. There is unfortunately no simple solution to this problem. Excluding tenure altogether results in a seriously misspecified model (Mincer and Jovanovic 1981, Blau and Kahn 1981, Borjas and Rosen 1980), and so we have decided to take the conservative route of including tenure.

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[7] The exception is that the effect of tenure is somewhat dampened in strength, which confirms the common rationale given for fitting random effects.

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Table 3 presents the results of several versions of the above model. In model 1, we begin with a basic specification that makes several key corrections. For example, we know that the age and education compositions differ between the two cohorts, and that the unemployment rates also differ at several time points. Controlling for work experience from the outset is important as well. And as mentioned earlier, the Vietnam veterans in the original cohort had a delayed entry into the labor market, thus reaching employment stability at a later age and "dragging down" the overall stability of the original cohort. The behavior of these "correction" variables is as expected. The odds of job changing clearly and strongly decline with age, tenure, and accumulated work experience, as young workers begin to form permanent attachments to employers. Higher unemployment has a mild positive effect on the odds of a job change.[8] And those without a high school degree are significantly more at risk of leaving their current employer than high school graduates.

The estimated difference between the two cohorts after controlling for compositional differences in these variables is strong: the odds of a job change are 34% higher for the recent cohort. However, we have not yet fully accounted for the higher attrition rate in the original cohort. If "less stable" respondents were significantly more likely to drop out of the study (net of the model 1 controls), then our estimate of the cohort difference would be biased and too large. We therefore calculated several adjustments to the raw log-odds estimate of the cohort effect of .289. The adjusted estimates range from .281 to .262 (described in detail in Appendix B). To our minds the most accurate adjustment lies in the middle of these two, .268, which represents a 7.7% reduction of the unadjusted estimate.

[8] We explored more complex specifications of the unemployment rate (e.g. pulling out recessions) but none improved on this simple specification.

In sum, after adjusting for key compositional differences and for attrition, we estimate that the odds of a job change are 31% higher for the recent cohort. We consider this our best baseline estimate of the increase in job instability experienced by young adult men in the 80s and early 90s, as compared to their counterparts in the late 60s and 70s.

In the second model, we examine the impact of several additional socio-demographic variables. Enrollment in school raises the odds of a job change, not surprising since jobs held during schooling are likely short-term and temporary. The geographic effect of living in the South works in the expected direction, as does the stabilizing effect of marriage. The impact of these three variables on the cohort difference is strong: the odds of a job change are now 21% higher for the recent cohort – still substantial, but clearly lower. Most of this reduction is driven by lower marriage rates in the recent cohort, and the trend toward longer periods of college enrollment. What is being tapped here is that the transition to the labor market as a whole is taking longer than it did in the past (Bernhardt, et al. 1997).

In the third model, we ask whether the economy-wide shift towards the service sector has played a role. Service industries as a rule are more unstable than the goods-producing and traditionally unionized industries (excepting construction, where the nature of work is inherently transient), while the public sector is more stable. On both fronts, however, the young workers in the recent cohort are disadvantaged. Mirroring the economy-wide trend, they are less likely to be employed in the public sector and more likely to be employed in the service sector, especially low-end, high-turnover industries such as retail trade and business services. Controlling for these compositional shifts further reduces the cohort difference, so that the job change odds are now 16% higher for the recent cohort, about half of the baseline estimate.

In these first three models, all of the variables are constrained to have the same effect for both cohorts, so we are essentially measuring the impact of compositional shifts in the variables, not changes in their impact. We did test whether the rise in job instability for the recent cohort was particularly pronounced for those with less education. Surprisingly, we found no such disproportionate effect – the rise in instability has been felt by all education groups. There is, however, a further twist to the industry story. In model 4, we fit an interaction between the cohort effect and the industry effect. The cohort dummy now captures the significant cohort difference in job instability *within* the baseline industries of retail and wholesale trade and business services. The first interaction term then indicates that the cohort difference is similar within the higher-level industries of FIRE and professional services. The second interaction term, however, shows a stronger cohort difference within industries that historically have had higher unionization rates. Thus not only are youth in the recent cohort suffering from greater reliance on the "unstable" service sector, but they are also not benefiting as much when they are employed in traditionally stable industries such as manufacturing. What we are very likely identifying here, albeit indirectly, is the shedding of employment and declines in unionization in the goods-producing and to some extent public sectors.[9]

We have performed one final analysis in order to see whether the greater instability observed in the recent cohort is simply a function of a longer and more volatile transition to the labor market. The above models have been based on the full work history of each respondent and therefore include the early years of considerable job churning and job hopping (the late teens and early 20s). We know that the recent cohort takes longer to complete the transition to stable employment (Bernhardt, et al. 1997), and it could be that the cohort differences in job stability

[9] The NLS surveys do not have consistent data on union membership.

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are less pronounced after this transition is completed. In Table 4, we have therefore estimated the same models from above, but only for workers after they have finished their schooling.[10] The focus, therefore, is on the experience of the young workers *once they have permanently entered the labor market*. The results are quite consistent with those from the foregoing model. In particular, the estimated cohort differences in each specification remain strong and significant. Thus, our finding of a rise in job instability does not disappear once the young workers "settle down" and is therefore not just a legacy of churning in the labor market early on.

In short, the combination of the above findings suggest that we have identified a strong, secular increase in job instability among young adult workers in recent years. The consistency of this finding throughout is striking. When one group of workers changes jobs more frequently than another, their tenures, on average, will be shorter. Figure 2 shows how the median tenures of the two cohorts have been affected.[11] In the first panel, the greater job instability of the recent cohort begins to make itself felt in shorter tenures after the mid-20s, and a substantial gap has emerged by the early 30s. In the second panel, we exclude the Vietnam veterans from the original cohort, and the difference becomes even stronger. Again, this is because those serving in Vietnam lost several years of labor market participation and are therefore several years behind in building a long-term relationship with one employer. Finally, breakdowns of the median trends by education (not shown) yield a remarkably consistent picture across all education groups.

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[10] Specifically, we include observations from individuals only after they are never enrolled in school again and their education level never increases again.

[11] Because end-dates of jobs are not consistently recorded in the NLS data, we cannot do a full analysis of completed tenure spells.

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The upshot is that by their early 30s, 32% of the original cohort but 38% of the recent cohort had tenures shorter than two years. Conversely, 30% of the original cohort but only 24% of the recent cohort had tenures of seven years or longer. The reader should understand that even if the two cohorts suddenly became identical in their rate of job changing, this relative difference in tenure length would persist over time. In order to make up the lost ground, the recent cohort would actually have to become *more* stable, but the results from our models suggest no such trend.

IV. Wage Changes

A rise in job instability among young adults in the American labor market does not necessarily signal a problem. In fact, a solid body of research has established that job shopping early in the career is highly beneficial, yielding greater wage gains than staying put with one employer (Borjas and Rosen 1980, Bartel and Borjas 1981). It is important to appreciate the strength of this relationship. Roughly two-thirds of lifetime wage growth for male high school graduates occurs during the first 10 years of labor market experience, and the bulk of it is the result of job changes (Murphy and Welch 1990, Topel and Ward 1992). But the story is more complicated. It is in general true that having many employers early on does not impede wage growth (Mincer and Jovanovic 1981, Gardecki and Neumark 1997). In the longer term, however, job instability becomes harmful to wage growth, and chronically high levels of job instability are detrimental from the outset (Light and McGarry 1994). In other words, there is an optimal progression of initial job shopping, followed by stable employment with one firm.

In this context, it is important to examine how the wage returns to job search have changed for the recent cohort. For example, it is possible that the very nature of career

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development has changed in recent years – that workers increasingly build their careers and attain wage growth *across* employers. The recent cohort might be changing jobs more frequently and accumulating less tenure with one firm, but nevertheless be able to capture consistent wage growth over time. Thus our appraisal of the rise in job instability must in the end focus on the wage consequences – specifically, the wage gains that young workers capture as they engage in job shopping and then eventually settle with one employer.

Our measure of wages is the natural log of deflated hourly wages (using the PCE deflator). For any two years t and $t+2$ that were used to compute whether or not a job change occurred, we compute the corresponding wage change: $(\ln)wage_{t+2} - (\ln)wage_t$. We are therefore measuring two-year wage changes, and we separate them into two groups: those that resulted from a job change, and those that did not.

In Figure 3, we have plotted median wage changes, for workers who changed their jobs and for workers who stayed with the same employer. First note that these graphs mirror the age dynamic that has been documented by previous research. Early in the career, job changing pays off more than staying with an employer – in fact, these wage gains are substantially higher than any experienced later on. What is being reflected here is the well-known shape of the age-earnings profile, which slopes steeply upward in the early stages of the career and then slowly reaches a plateau of small, incremental wage gains. After the mid-20s, there is less to be gained from switching employers, and wage growth as a whole slows down.

The striking message from these graphs, however, is that the recent cohort has failed to capture wage growth precisely where it is most critical: in the early stages of job search and job changing. This first appears between the ages of 16 and 21. Breakdowns by education show that it is young workers moving directly from high school into the labor market who have borne the

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brunt of the burden. There is also a noticeable drop in the wage gains resulting from a job change in the early 30s, and this is shared by all but the most educated (those who have completed a four-year college degree).[12] By contrast, when young workers stay with the same employer, there is little difference in the *absolute* wage gains captured by the two cohorts. In *relative* terms, though, the recent cohorts benefits more from staying with the same employer after the mid-20s, because the returns to job changing decline so steeply at that point.

A second way to look at the impact of job instability is to examine the variability in wage changes. This measure has been neglected in previous research, but it is an important one, because we know that there has been an economy-wide increase in the dispersion of wages over the last several decades. Figure 4 therefore plots the variances of the observed wage changes. It should come as no surprise that generally speaking, a job change results in more variable wage changes. Switching employers can result in substantial wage gains but also in wage losses; staying put with one employer is unlikely to generate such polar outcomes. On both fronts, however, the recent cohort has clearly seen an increased variability, and this is especially pronounced among job changers in the later age ranges. Moreover, breakdowns by education show strong consistency in these trends across all education groups.

These are purely descriptive findings, and it is worthwhile to confirm them with a simple model of cohort differences in the returns to changing (and not changing) jobs. Regression results using the raw two-year wage changes are given in the first panel of Table 5, and the substantive findings are summarized in the third column. In general, there is a clear college premium in wage gains, and not surprisingly, that premium has grown in recent years. More

[12] In these graphs statistical significance effectively ends up being a function of sample size. So for example, in the job change panel, the gap in the early age ranges is statistically significant, while the gap among 31-33 year olds is not – by the later ages, a much smaller proportion of the samples is changing jobs.

interesting is that for the original cohort, it doesn't matter so much whether one changes jobs or not: the education differentials are similar. This is not the case for the recent cohort. Here young adult workers without any college experience are getting hit the hardest when they engage in job search – they are, in fact, experiencing real wage declines in comparison to the past. And this pronounced deterioration in the returns to job changing for less educated workers comes at the same time that job changing has become more prevalent. By contrast, those with college experience are seeing unprecedented wage gains when they engage in job search.

In the second panel of Table 5, we have fit the same model, but this time using wages that have been smoothed of year-to-year variability. This enables us to isolate *permanent* differences in wage changes and to avoid confounding them with trends in *transitory* wage variability.[13] Short-term wage gains show a considerable amount of fluctuation from one year to the next and this might obscure systematic patterns in those gains (Gottschalk and Moffit, 1994). As it turns out, the substantive results are quite similar across both panels, though the education differentials are somewhat dampened in strength when we use the smoothed wages (as one might expect). The R^2 is much higher in the second panel, reflecting the fact that these factors explain a significant portion of the systematic variation in wage changes.

Finally, we have up to now focused on two-year wage changes and linked them to job change events. But in truth, the young adult workers observed here have experienced an entire chain of wage changes. Even small differences in single wage changes can cumulate into substantial differences over time. What happens, then, when we examine the total wage growth observed for each individual? Figure 5 plots the distribution of total wage growth between the

[13] Specifically, these "smoothed" wages are predicted hourly wages for each respondent at each age, from a mixed-effects wage model. The model effectively allows a unique wage profile for each person across his full work history, smoothing out fluctuations in wages that can result from temporary disturbances such as the business cycle. See Appendix C for a description of the model.

ages of 16 and 36, again using the permanent wages that have short-term fluctuations smoothed out.

Two important trends emerge from this figure. First, young workers who entered the labor force in the 1980s experienced significantly lower *total* wage growth when compared to their predecessors. Translated into real terms, the typical worker in the original cohort saw his hourly wage increase by $8.65 between the ages of 16 and 36, whereas the increase was only $6.69 for those in the recent cohort (both figures in 1992 dollars). Again, this loss of growth has been felt largely by those without a four-year college degree. Second, long-term wage growth has also become significantly more unequal across the two cohorts. The distribution of wage gains for young workers in the 80s and 90s has become flatter and more spread out, and in particular, the bottom tail has gotten thicker. There remain some workers who experience high levels of wage growth, but there are now substantially more workers who have minimal or even negative wage growth. We estimate that the percent of workers experiencing no wage growth or actual real wage declines is 1.7% for the original cohort but 7.2% for the recent cohort. This polarization becomes progressively stronger as the young workers age and is consistent across different levels of education.

In sum, workers who entered the labor force in the late 70s and early 80s have seen both stagnant and more unequal wage growth than those who entered in the late 60s and early 70s. The stagnation has been driven largely by the failure of non-college graduates to fully capture the benefits of early job shopping. The greater inequality has been driven by an increasing polarization in the wage gains associated with job changes, especially those that result from switching employers later in the career.[14] We are currently developing models that will formally

[14] See Polsky (1996) and Stevens (1996b) for additional evidence along these lines.

link the total wage growth experienced by these young adult workers to both the rising prevalence of and the declining returns to job changing (Bernhardt et al. 1997).

V. Conclusion

In this paper, we have identified a marked increase in job instability among young adult white men during the 1980s and early 1990s, as compared to the late 1960s and 1970s. The robustness of this finding to different controls is striking. It does not disappear, for example, once the young workers "settle down" and is therefore not just a legacy of churning in the labor market early on. Some of the increase is explained by the trend toward longer transitions into the labor market and lower marriage rates. Changes in the structure of the U.S. economy have clearly also played a role, in particular the shift to a service-based economy and the decline in employment and unionization in the manufacturing sector. With all of these controls in place, however, only half of the cohort effect is explained. For all workers, one immediate effect of the rise in instability is that median tenure has gotten shorter.

Job instability and shorter tenures are not necessarily a bad thing. We know from pervious research that job shopping is actually the main mechanism by which young adults generate wage growth early in the career. In this context, our findings are particularly discouraging. The process of job search and job changing, so important in the past, no longer confers the same benefits it once did, especially to those with less education. As a result, the increase in job instability in recent years is generating lower and more unequal wage growth. Combined with the research by Gottschalk and Moffit (1994), Duncan, Boisjoly, and Smeeding (1996), Gittleman and Joyce (1996), Baker (1997) and others, our findings begin to establish a clear link between changes in job stability and declines in both short-term and long-term wage mobility.

The 16 years covered by the NLS data examined here represent most of the job changes and wage growth that these young adult workers will experience during their career. Our findings thus have troubling implications for the long-term economic welfare of men who entered the labor market during the late 1970s and early 1980s. The growth in wage inequality that has captured so much public attention is not just a temporary phenomenon for these men, it has changed their long-term trajectories. Absent a truly dramatic shift in the American economy, the greater inequality in wage growth that we have documented here will persist over their life course.

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Appendix A. Comparison of NLS, PSID, and CPS Estimates of Job Change Rates

In what follows, we compare job change estimates from the NLS to the PSID series calculated by Polsky (1997) and the CPS series calculated by Stewart (1998). We chose these two series because they attempt to address some of the well-known problems in the PSID and CPS with changes in measures and question wording over time. Our goal is to test whether PSID and CPS comparisons to the original cohort differ from PSID and CPS comparisons to the recent cohort. If attrition in the original cohort had a strong bias in terms of job instability (i.e. less "stable" respondents were much more likely to drop out of the study), then we would expect that estimates from the original cohort will not match up well with the other datasets, while estimates from the recent cohort will match up well (recall that attrition in the recent cohort was negligible).

Two factors make this comparison somewhat difficult. First, neither the PSID nor the CPS extend back far enough in time, so that only two time points can compared with the original cohort. Second, the nature of the NLS data also work against a full-scale comparison: the two cohorts age throughout the 16-year survey period, yielding a shifting age range over time. As well, the skipped interview years in the original cohort means that we sometimes have to use two-year instead of one-year job change rates. With these two considerations in mind, Table A presents the best comparisons that we could construct. For all three datasets, the samples are white men who are not self-employed. We also reweighted the NLS and PSID distributions to the CPS distribution within age/education cells, so that the analysis is not confounded by differences in composition – in practice, this reweighting has a minor effect.

The first half of the table shows that the NLS and PSID estimates match up remarkably well. For both cohorts, the differences in estimates are quite small and not statistically significant. To our minds, this is a solid indicator that the greater attrition rate in the original cohort is not driving our finding of changes in job stability over time (a second indicator is given in Appendix B). We should also note that this is our preferred comparison: the PSID job change measures are directly analogous to the NLS measures, and the PSID is replenished to remain representative over time and therefore serves as a useful benchmark.

The second half of the table shows our comparison of the NLS with the CPS. The drawback to this comparison is that the two datasets have different measures and risk sets. The CPS measure is (1) a 14.5-month job change rate that (2) is inferred using several decision rules for (3) respondents who worked at least one week in the previous year and who were not students or recent graduates. By contrast, the NLS measure is (1) a one-year job change rate that (2) is calculated directly for (3) respondents who were working during the week of the previous year's survey. The results of comparing across these different measures are not clear. As a rule, the NLS estimates are lower than the CPS estimates, which one might expect given how the measures are defined. But the magnitude and significance of the differences varies considerably, both within and between cohorts. Especially worrisome is the variability *within* the recent cohort, which has very little attrition. Our sense is that it would be difficult to come to reconcile these two datasets without considerably more analysis, along the lines of Jaeger and Stevens (1998).

Table A. Comparison of job change rate estimates from NLS, PSID, and CPS

		Cohort	NLS	PSID†	NLS - PSID
1978	ages 26-32, 2-year rate	Original	0.3668	0.3652	0.0016
1980	ages 28-34, 1-year rate	Original	0.2292	0.2104	0.0188
1989	ages 26-32, 2-year rate	Recent	0.4078	0.4177	-0.0100
1991	ages 28-34, 1-year rate	Recent	0.2420	0.2389	0.0031

		Cohort	NLS	CPS‡	NLS - CPS
1975	ages 23-31	Original	0.2721	0.3351	-0.0630 *
1980	ages 28-36	Original	0.2108	0.2591	-0.0483 *
1988	ages 23-31	Recent	0.3001	0.3452	-0.0451 *
1989	ages 24-32	Recent	0.2942	0.3198	-0.0256
1990	ages 25-33,	Recent	0.2653	0.3228	-0.0575 *
1991	ages 26-34	Recent	0.2474	0.2890	-0.0416 *
1992	ages 27-35	Recent	0.2546	0.2705	-0.0159
1993	ages 28-36	Recent	0.2713	0.2727	-0.0014

† Authors' tabulation of data from Polsky (1997).

‡ Estimated 14-month job change rate for CPS compared to one-year job change rate for NLS. Authors' tabulation of data from Stewart (1998).

* Difference significant at .05 level.

Appendix B. Adjusting for Attrition

This appendix focuses on the possibility that the higher attrition rate in the original cohort may lead us to overestimate the cohort difference in job instability. Differential attrition rates between the two cohorts can potentially bias the estimated cohort effect in our job change models through two mechanisms. First, attriters may have higher levels of job stability than non-attriters. Second, attriters may also be less likely to be eligible for the risk set that defines the job change sample. In both cases, attriters do not contribute enough "unstable" observations to the job change sample, and as a result the cohort effect is overstated. It is possible to adjust the estimated cohort effect by making the assumption, in technical terms, of having data that are missing at random but not completely at random, in the sense of Rubin (1976) and Little (1995). Substantively, it means that we assume the effect of eventual attrition on job separation rates during the observed person-years is the same as the effect during the unobserved years, after conditioning on a person's observed past job stability (i.e. tenure) and other covariates. The adjustment to the cohort effect then consists of estimating and removing the bias associated with the "effect" of attrition on job change and risk set eligibility.

The first stage in the adjustment is to estimate the attrition effect – the difference between attriters and non-attriters in the odds of a job change. We model this by adding terms to the logistic regression model 1 in section 3:

$$\text{logit}(P_{ijt} | C_i, A_{ijt}) = \theta_o X_{ijt} + \theta_1 J_{ijt} + \theta_2 U_{it} + \theta_3 C_i + \theta_4 A_{ijt} + \theta_5 CA_{ijt} + \phi_i,$$

where the dependence of the logit on the other regressors is surpressed. The model now includes two attrition-related effects: A_{ijt}, a dummy variable indicating whether person i in job j in year t attrits after year $t+2$ given that he has not attrited before, and CA_{ijt}, the interaction between attrition and cohort. The interaction term allows the separation rates for attritters to differ by cohort, and its inclusion means that the coefficient on the main attrition dummy, θ_4, represents the attrition effect for the original cohort. As with the models in section 3, we include random effects, ϕ_i, that capture any systematic deviation of person-specific intercepts from the average fixed-effect intercept. Again, the parameters are estimated using restricted maximum likelihood (REML).

Under this model, the log-odds of a two-year job change for a randomly chosen person-year with given characteristics from cohort k is:

$$\begin{aligned}\text{logit}(P_{ijt} \mid C_i = k) &= \text{logit}(P_{ijt} \mid C_i = k, A_{ijt}=0)\, P(A_{ijt}=0 | C_i = k) + \text{logit}(P_{ijt} \mid C_i = k, A_{ijt}=1)\, P(A_{ijt}=1 | C_i = k) \\ &= \theta_o X_{ijt} + \theta_1 J_{ijt} + \theta_2 U_{it} + \theta_3 k + \phi_i + \theta_4 P(A_{ijt}=1 \mid C_i = k) + \theta_5 P(A_{ijt}=1 \mid C_i = 1)\text{k}\end{aligned}$$

The attrition-adjusted cohort effect is then simply represented as:

$$\text{logit}(P_{ijt} \mid C_i = 1) - \text{logit}(P_{ijt} \mid C_i = 0) = \theta_3 + \theta_4[P(A_{ijt}=1 \mid C_i = 1) - P(A_{ijt}=1 \mid C_i = 0)] + \theta_5 P(A_{ijt}=1 \mid C_i = 1)$$

The first term (θ_3) represents the cohort effect for a non-attriter. The second term represents the differential odds that an attriter experiences a job separation before being lost, multiplied by the difference in attrition rates between the two cohorts. If attriters are more unstable, θ_4 will be positive, and as the difference in attrition is negative, the adjustment will lower the estimate of the cohort effect. The third term represents the differential in the attrition effect for the recent cohort, multiplied by the attrition rate in the recent cohort. If those who attrit in the recent cohort are more unstable than those who attrit in the original cohort, θ_5 will be positive and this adjustment will increase the estimate of the cohort effect. The estimates we obtain are: $\theta_4 = 0.3213$; $\theta_5 = 0.0248$.

The second part of the adjustment is to estimate the conditional probabilities of attrition in the equation above. The idea here is to construct these conditional probabilities *as though* the unobserved years had been included in the analysis – so that the adjusted effect represents what would have been measured if the attriters had not been lost. The conditional probabilities can thus be seen as a reweighting factor. For this reason, we can not use the observed fractions of person-years in the job-change sample because the fraction of person-years contributed by attriters is too low: it excludes the unobserved person-years for attriters after they are lost. There are several methods by which these fractions can be adjusted based on the fraction of *respondents* who attrit in each cohort, as this implicitly allows attriters and non-attriters to contribute proportionately in terms of person-years:

1. *The fraction of attriters in the risk set.* The fraction of respondents in the job-change risk set who eventually attrit is 0.1603 (375/2340) in the original cohort and 0.0545 (124/2276) in the recent cohort. In this adjustment, we are effectively adding the person-years that attriters would have contributed, had they not dropped out of the sample.

2. *The cohort-equalized fraction of attriters in the risk set.* In addition to the adjustment made in (1), we also need to adjust for the fact that recent cohort attriters were more likely to make it into the job change risk set than original cohort attriters. We do so by adding the number of original cohort attriters necessary to equalize the proportion of attriters eligible for the job change sample between the two cohorts: 115 additional attriters for the original cohort, yielding an adjusted fraction of 0.1996 (490/2455). This is the most accurate adjustment, since it removes both types of attrition bias from the job change sample – and it is precisely differences in stability based on this sample that we are trying to adjust.

3. *The fraction of attriters in the full sample.* Finally, the strongest adjustment would use the fraction of attriters for each cohort in the full sample. The fraction of persons at risk in the full sample who are lost to attrition is 0.2323 (610/2625) in the original cohort and 0.0658 (157/2385) in the recent cohort. We feel less comfortable with this adjustment, since it uses estimates from the job change sample and applies them to a sample that is not included in the job instability analysis conducted in this paper. It is difficult to gauge the extent to which this inference to the full sample is valid – four years were skipped in the original cohort's survey, resulting in an unbalanced panel across the two cohorts. Indeed, this was the motivation for the restricted job change sample: it uses exactly the same years for exactly the same age ranges in both cohorts.

The adjustments based on each of these three methods is provided in Table B. While in all cases the attrition adjustment reduces the estimated cohort effect, the reductions are modest. Using method 1, the adjusted cohort effect is 0.2811 +/− 0.0766, where the bounds represent a 95% confidence interval. This is a 2.88% decrease in the unadjusted value and corresponds to 0.21 of a standard error (the standardized value). Using method 2, which we consider the most accurate, the adjusted cohort effect is 0.2684 +/- 0.0778. This represents a 7.73% decrease in the unadjusted value and corresponds to 0.52 of a standard error. Finally, using method 3 the adjusted cohort effect is 0.2618 +/- 0.0796. This represents a 10.45% decrease and corresponds to 0.67 standard errors. Again, in our opinion the statistical grounds for making this adjustment are less strong, but even with this caveat, 90% of the cohòrt effect remains.

There are two reasons why the adjustments are modest under all methods. First, the cohort difference in attrition only ranges from 11% (method 1) to 17% (method 3), so the proportional reweighting is not substantial in any of the methods. Under these conditions, the estimated attrition effect (θ_4) would have to be about 5.5 times larger in order to fully negate the size of the cohort effect.

Second, the recent cohort attrition differential (θ_5) is positive, so that it offsets the negative adjustment made by the main attrition effect. That attriters in the recent cohort are more "unstable" than attriters in the original cohort makes sense, given the difference in retention rules in the two panels. In the original cohort, any respondents missing two sequential interviews were dropped from the survey, while such respondents in the recent cohort remained eligible and were pursued for future interviews with great effort. Those who did manage to drop out of the recent cohort therefore likely represent "hard core" attriters. We found support for this conjecture by examining respondents in the recent cohort who would have been dropped from the survey under the rules used in the original cohort (about 9% of the sample). These "hypothetical attriters" have attributes and outcomes that fall in between the "hard core" attriters and the retained sample. This result suggests that the additional respondents lost to attrition in the original cohort are a moderate group.

Table B. Adjusting for attrition in the job change analysis

	Unadjusted	**Method 1***	**Method 2***	**Method 3***
Attrition Fraction (original:recent)	16.0:5.4	16.0:5.4	20.0:5.4s	23.2:6.6
Cohort effect (95% CI)	0.2892 (+/- 0.078)	0.2811 (+/- 0.076)	0.2684 (+/- 0.078)	0.2618 (+/- 0.080)
Adjustment		0.0081	0.0207	0.0274
percentage change		2.88	7.73	10.45
standardized change		0.21	0.52	0.67

* Method 1: using risk set attrition fractions; Method 2: using cohort-equalized risk set attrition fractions; Method 3: using full sample attrition fractions

Appendix C: Permanent Wage Estimation

We use the following model to smooth an individual's wages of short-term fluctuations: a set of fixed effects to capture the average curve of the wage profile over age, a set of random effects to isolate the heterogeneity in permanent wage gains among individuals, and a residual term to represent the transitory components of wage change within each individual profile.

The permanent and transitory components of wage-profile heterogeneity are specified as follows:

$$y_{it} = \mu_{it} + e_{it},$$

where y_{it} is the log of the real wage of individual i in year t. The average wage profile μ_{it} is specified by:

$$\mu_{it} = \beta_o + \beta_1 l_{it} + \beta_2 q_{it} + X_{it}\gamma_{it},$$

where l_{it} and q_{it} are the linear and quadratic age terms respectively, X_{it} represents individual and age specific covariates. In this application these are education and experience. The coefficients β_o, β_1, β_2, and γ_{it} are average level ("fixed-effect") parameters. We have parameterized l_{it} as the age of individual i in year t centered on age 16 and q_{it} as the quadratic term centered on age 16 and orthogonal to l_{it}. The random effects component is specified as:

$$e_{it} = p_{it} + u_{it},$$

where we define p_{it} as the permanent component and u_{it} as the transitory component. Specifically,

$$p_{it} = b_{0i} + b_{1i} l_{it} + b_{2i} q_{it}.$$

Thus p_{it} is a random quadratic representing the deviation of the individual-specific wage profile from the average wage profile. Under this parameterization, b_{0i}, b_{1i}, and b_{2i} represent the deviations from their fixed-effects counterparts. We model b_{0i}, b_{1i} and b_{2i} as samples from a mean-zero trivariate Gaussian distribution. We suppose u_{it} is mean-zero and allow the variance of u_{it} to vary by calendar year to capture any business cycle effects.

The individual-specific wage profile is the combination of the average wage profile and the individually-specific deviation: $\mu_{it} + p_{it}$. The parameters in our model are estimated using restricted maximum likelihood (REML). In addition to being asymptotically efficient under the assumption of Gaussianality, this approach produces asymptotic standard errors and covariances for the fixed and random parameter estimates. This approach provides the best linear unbiased estimator (BLUE) for the individual-specific wage profiles.

Table 1. Years used for job change analysis

Panel	Year of Survey		Six matched pairs
	Original cohort	Recent cohort	
1	66	79	
2	67	80	2-4
3	68	81	
4	69	82	4-6
5	70	83	
6	71	84	6-8
7		85	
8	73	86	8-10
9		87	
10	75	88	*
11	76	89	11-13
12		90	
13	78	91	13-15
14		92	
15	80	93	*
16	81	94	

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Table 2. Characteristics of sample for job change analysis

	Pooled sample	Original cohort	Recent cohort
Number of persons	4,616	2,340	2,276
Number of person-years	18,077	8,811	9,266
Mean # of observations contributed	3.9	3.8	4.0
Two-year separation rate	.494	.464	.527
Age range	16-34	16-34	16-34
Mean age	24.9	25.0	24.8
Mean work experience, in months	82.1	80.2	84.2
Enrolled in school	22.0 %	18.9 %	25.3 %
Current education			
Less than high school	16.4	16.5	16.4
High school degree	39.2	34.8	44.0
Some college	23.0	24.8	20.9
College degree or more	21.4	23.9	18.7
Current tenure			
One year or less	40.1	40.2	39.9
1 – 3 years	29.9	28.8	31.2
3 or more years	30.0	31.0	28.0
Living in South	29.2	29.7	28.2
Married	49.9	60.3	38.4
Industry			
Construction, mining, agriculture	14.2	13.6	14.8
Manufacturing, transport. & communication	34.3	37.1	31.2
Wholesale & retail trade, business services	31.1	26.1	36.6
FIRE and professional services	15.7	17.3	14.0
Public Administration	4.7	5.9	3.4
Professional, managerial, technical occupations	26.4	28.4	24.2
Finished with education	59.8	58.9	60.9

Table 3. Logistic regression results for two-year job separation: All workers

	Model 1				Model 2				Model 3				Model 4			
Variable	B	S.E.	Sig	Exp(B)	B	S.E.	Sig	Exp(B)	B	S.E.	Sig	Exp(B)	B	S.E.	Sig	Exp(B)
Intercept	1.239	.088	***	3.454	0.903	.097	***	2.466	1.157	.103	***	3.180	1.710	.099	***	5.529
Recent Cohort (original cohort)	0.289	.040	***	1.335	0.189	.042	***	1.208	0.145	.043	***	1.156	0.138	.069	*	1.148
Age	-0.121	.018	***	0.886	-0.042	.020	*	0.959	-0.023	.021		0.977	-0.087	.019	***	0.917
Age squared	0.004	.001	***	1.004	0.001	.001		1.001	0.000	.001		1.000	0.004	.001	***	1.004
Current education (High school graduate)																
Less than high school	0.522	.060	***	1.685	0.512	.061	***	1.668	0.465	.063	***	1.592	0.483	.062	***	1.621
Some College	0.408	.050	***	1.503	0.239	.054	***	1.270	0.240	.056	***	1.271	0.329	.052	***	1.390
College degree or more	-0.068	.056		0.934	-0.172	.058	**	0.842	-0.107	.066		0.899	-0.117	.060		0.890
Current tenure (one year or less)																
1-3 years	-0.626	.037	***	0.535	-0.607	.038	***	0.545	-0.587	.040	***	0.556	-0.007	.039		0.993
3 or more years	-0.709	.045	***	0.492	-0.704	.047	***	0.495	-0.657	.048	***	0.518	0.005	.047		1.005
Work Experience	-0.015	.001	***	0.985	-0.013	.001	***	0.987	-0.014	.001	***	0.986	-0.768	.001	***	0.464
Local unemployment rate	0.010	.006		1.010	0.006	.006		1.006	-0.004	.007		0.996	-1.185	.006	***	0.306
Currently enrolled					0.408	.049	***	1.504	0.365	.051	***	1.441				
Living in South					0.060	.046		1.062	0.038	.047		1.039				
Married					-0.298	.040	***	0.742	-0.265	.041	***	0.767				
Industry (wholesale & retail trade, business services)																
Construction, mining, agriculture									0.070	.060		1.073	-0.125	.074		0.882
Manufacturing, transportation & communication									-0.627	.046	***	0.534	-0.850	.063	***	0.427
FIRE & professional services									-0.207	.061	***	0.813	-0.160	.081	*	0.852
Public administration									-1.103	.099	***	0.332	-1.331	.107	***	0.264
Professional, managerial, technical occupations									-0.091	.049		0.913				
Interaction of Cohort & Industry																
Recent cohort in high-level services													-0.044	.113		0.957
Recent cohort in traditional industries													0.235	.083	***	1.265
Individual heterogeneity (standard deviation)	1.205	.150	***		0.683	0.154	***		0.620	.152	***		0.630	0.15	***	
Change in -2 log likelihood	-2025.22		***		-330.71		***		-402.53		***		100.375			

***=significant at .001, **=significant at .01, *=significant at .05

Contrast categories are identified in parentheses. Age is rescaled to age -16. Work experience is measured in months. See text for explanation of interaction effect.

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Table 4. Logistic regression results for two-year job separation: Workers who have finished schooling only

	Model 1				Model 2				Model 3				Model 4			
Variable	B	S.E.	Sig	Exp(B)	B	S.E.	Sig	Exp(B)	B	S.E.	Sig	Exp(B)	B	S.E.	Sig	Exp(B)
Intercept	1.272	.182	***	3.569	0.638	.146	***	1.893	0.924	.156	***	2.519	1.066	.160	***	2.904
Recent Cohort (original cohort)	0.293	.060	***	1.341	0.237	.056	***	1.267	0.198	.057	***	1.219	0.154	.096		1.167
Age	-0.137	.036	***	0.872	-0.034	.029		0.967	-0.032	.030		0.968	-0.051	.030		0.950
Age squared	0.006	.002	***	1.006	0.002	.001		1.002	0.002	.001		1.002	0.003	.001	*	1.003
Current education (High school graduate)																
Less than high school	0.678	.103	***	1.970	0.711	.093	***	2.036	0.668	.096	***	1.951	0.667	.094	***	1.948
Some College	0.428	.098	***	1.535	0.157	.083		1.170	0.168	.086	*	1.184	0.158	.085		1.171
College degree or more	0.077	.087		1.080	-0.100	.078		0.905	0.005	.090		1.005	-0.050	.083		0.951
Current tenure (one year or less)																
1-3 years	-0.540	.056	***	0.583	-0.601	.052	***	0.548	-0.576	.053	***	0.562	-0.589	.053	***	0.555
3 or more years	-0.678	.063	***	0.507	-0.671	.059	***	0.511	-0.614	.061	***	0.541	-0.622	.060	***	0.537
Work Experience	0.018	.001	***	1.018	-0.022	.001	***	0.978	-0.020	.001	***	0.980	-0.021	.001	***	0.979
Local unemployment rate	0.008	.010		1.008	0.001	.008		1.001	-0.007	.009		0.993	-0.005	.009		0.995
Living in South					0.081	.062		1.084	0.060	.064	***	1.061				
Married					-0.259	.052	***	0.772	-0.232	.054	***	0.793				
Industry (wholesale & retail trade, business services)																
Construction, mining, agriculture									0.116	.077		1.122	-0.051	.099		0.950
Manufacturing, transportation & communication									-0.551	.062	***	0.577	-0.724	.087	***	0.485
FIRE & professional services									-0.193	.091	*	0.825	-0.283	.124	*	0.754
Public administration									-1.095	.137	***	0.335	-1.263	.148	***	0.283
Professional, managerial, technical occupations									-0.127	.067		0.881				
Interaction of Cohort & Industry																
Recent cohort in high-level services													0.104	.172		1.110
Recent cohort in traditional industries													0.301	.113	**	1.351
Individual heterogeneity (standard deviation)	1.435	.100			0.690	0.110	***		0.620	.110	***		0.880	0.10	***	
Change in -2 log likelihood	-10404.5		***		-227.73		***		-241.98		***		70.783			

***=significant at .001, **=significant at .01, *=significant at .05

Contrast categories are identified in parentheses. Age is rescaled to age -16. Work experience is measured in months. See text for explanation of interaction effect.

Table 5. Wage change regression results†

	Using raw wages			Using permanent wages		
Variable	**Estimate**	**s.e. + sig.**	**Ratio of college to high school§**	**Estimate**	**s.e. + sig.**	**Ratio of college to high school§**
Original cohort						
Did not change jobs						
High school or less (intercept)	0.2609	.014 ***	1.45	0.2541	.003 ***	1.29
Some college or more	0.0331	.010 ***		0.0285	.002 ***	
Changed jobs						
High school or less	0.0607	.010 ***	1.21	0.0027	.002	1.30
Some college or more	0.0884	.011 ***		0.0331	.002 ***	
Recent cohort						
Did not change jobs						
High school or less	0.0146	.010	1.54	-0.0133	.002 ***	1.66
Some college or more	0.0623	.011 ***		0.0437	.002 ***	
Changed jobs						
High school or less	-0.0208	.010 *	3.55	-0.0316	.002 ***	2.30
Some college or more	0.1140	.011 ***		0.0563	.003 ***	
Age (rescaled to 16=0)	-0.0228	.003 ***		-0.0179	.001 ***	
Age squared (rescaled to 16=0)	0.0008	.000 ***		0.0004	.000 ***	
Work experience (in months)	-0.0008	.000 ***		-0.0005	.000 ***	
Adjusted R^2	.044			.349		
N	17,954			17,954		

* Significant at .05 level

*** Significant at .001 level

† Dependent variable is two-year change in log wages, raw and permanent; see text for explanation.

§ Evaluated at variable means for age, age-squared, and experience.

Figure 1. Cohort differences in job separation rates

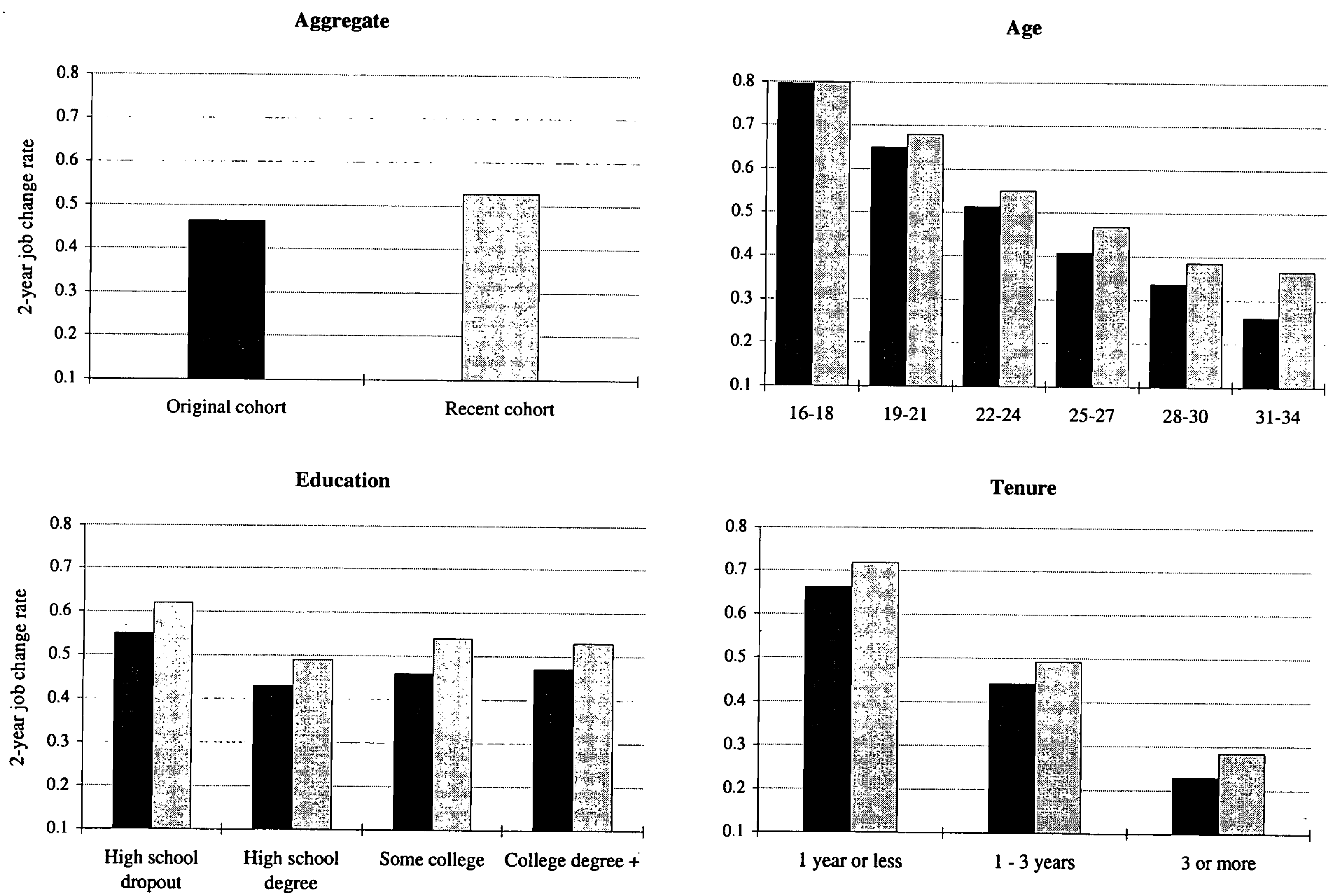

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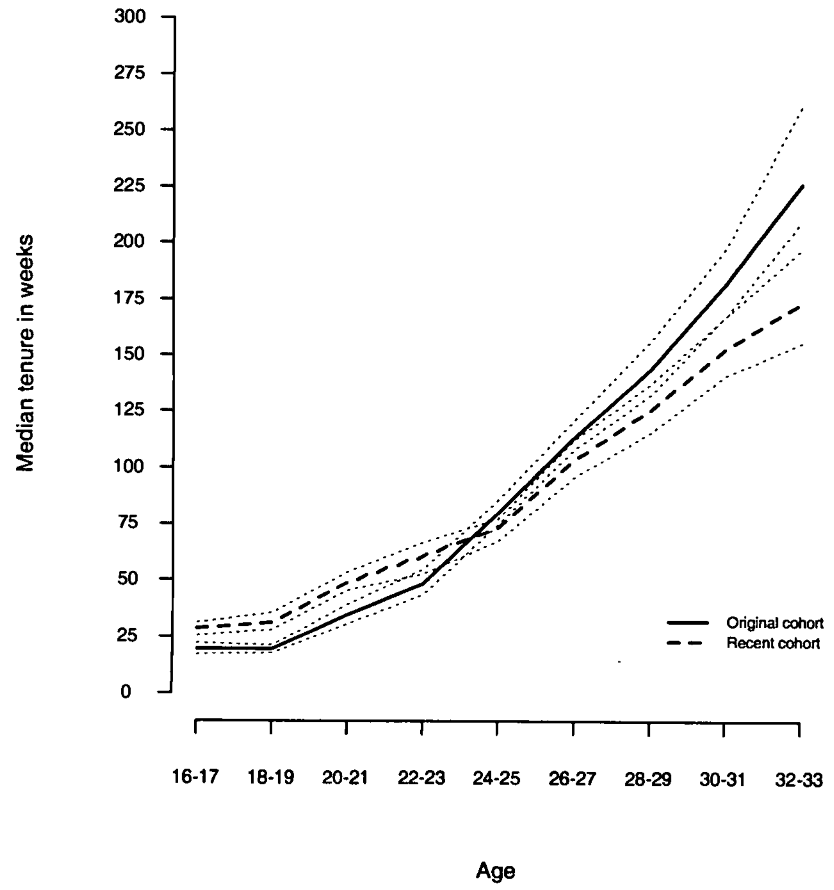
Figure 2. Median tenure by age

Full sample



Excluding Vietnam veterans from original cohort

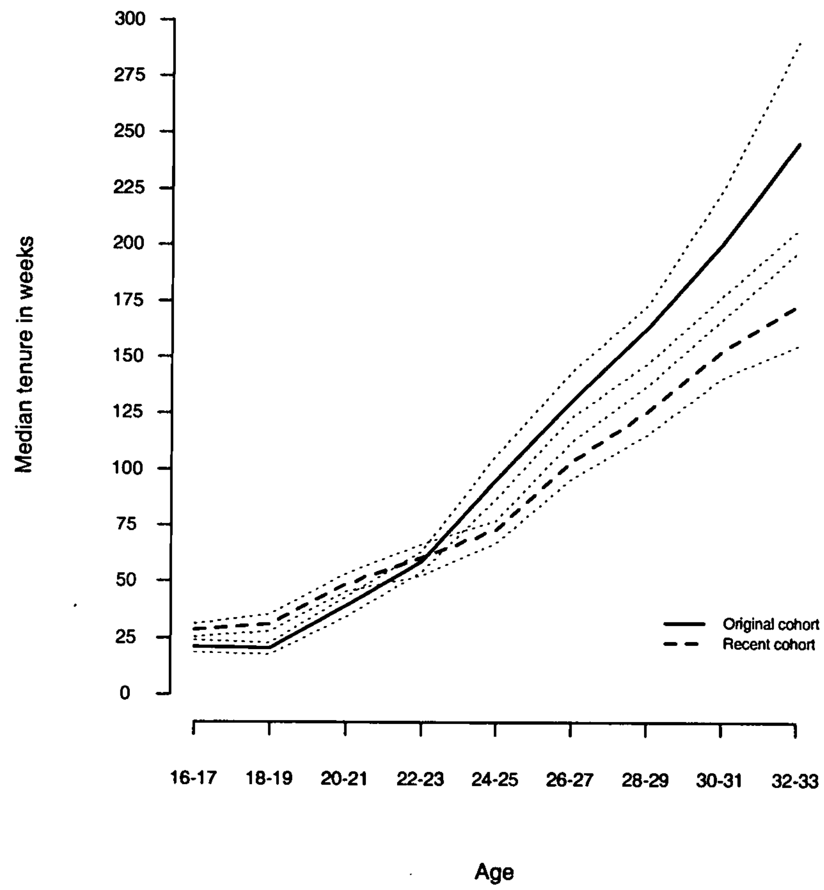


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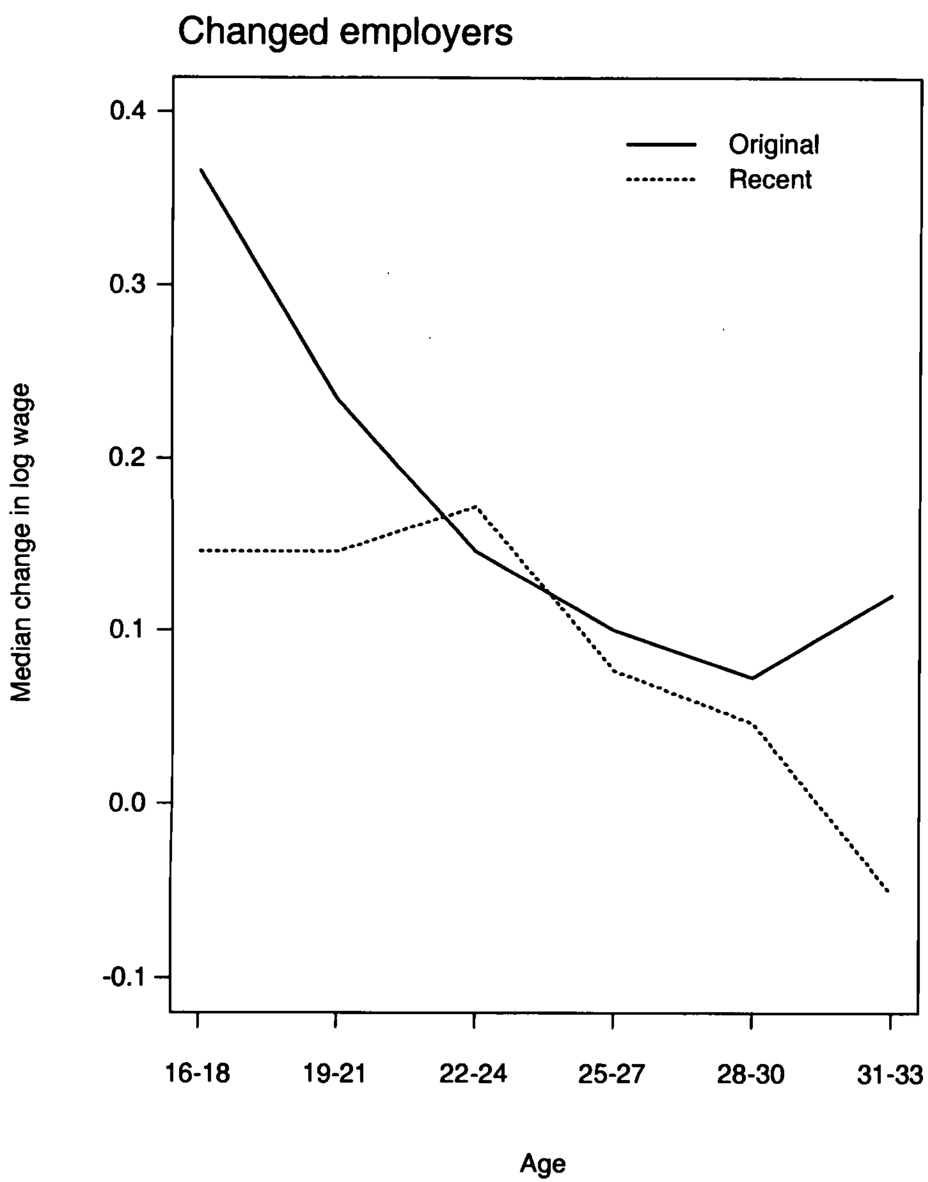
Figure 3. Median change in log wages



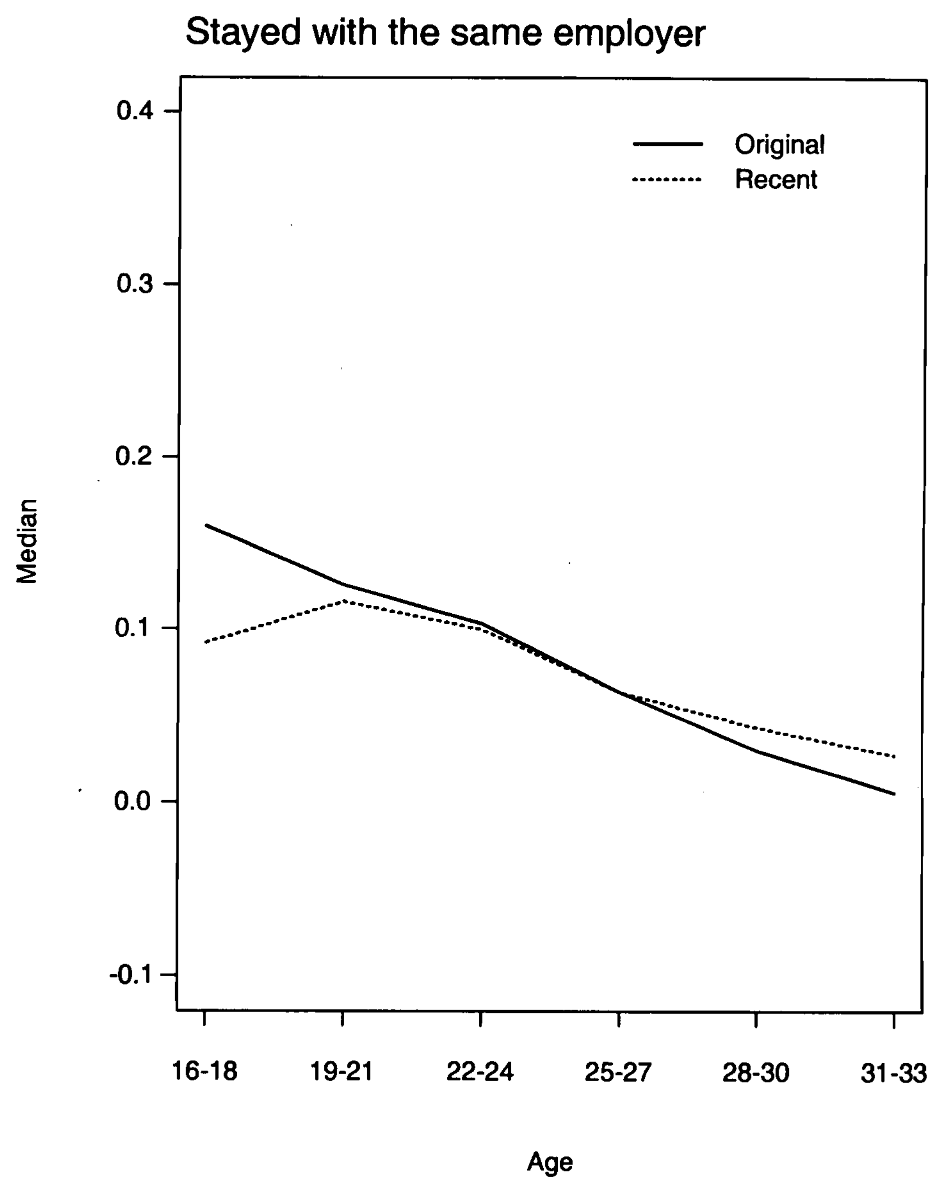


Figure 4. Variance of change in log wages

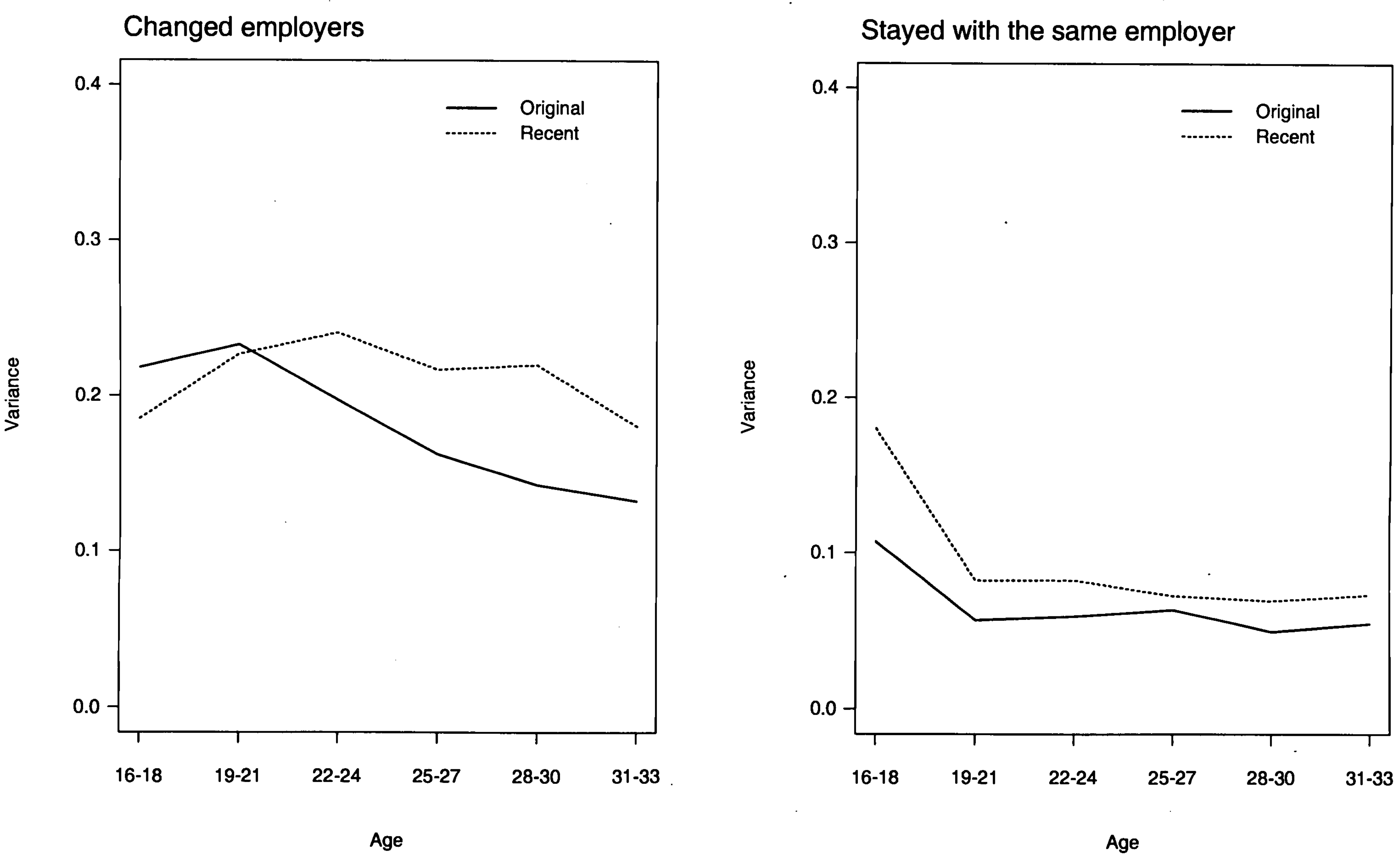


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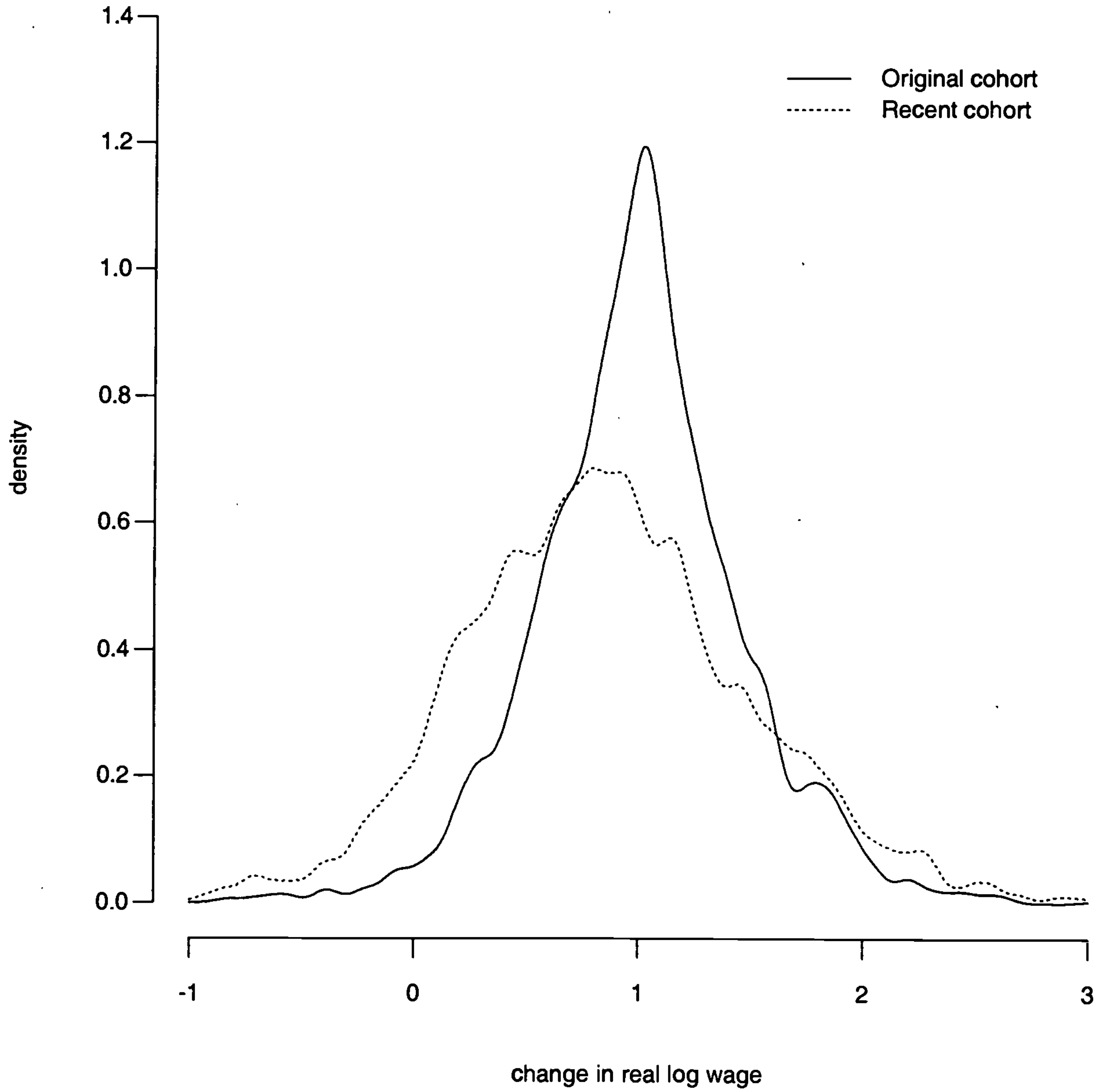
Figure 5. Permanent change in wages from age 16 to 36



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