On the Job

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Is Long-Term Employment a Thing of the Past?

David Neumark
editor

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Chapter 4

Trends in Job Instability and Wages for Young Adult Men

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Although the perception of increased job instability is widespread, empirical documentation of this "fact" remains elusive. Data and measurement problems have led to a trail of conflicting findings, and the absence of clear evidence of rising instability has led some to question whether the problem lies instead with public perception. A careful review of the evidence suggests that the question may be premature. The primary sources of cross-sectional data are the tenure and pension supplements of the Current Population Survey (CPS) and the Displaced Workers Survey (DWS). Using the CPS, Kenneth Swinnerton and Howard Wial (1995) found evidence of an overall decline in job stability, whereas Francis Diebold, David Neumark, and Daniel Polsky (1997) and Henry Farber (1998) did not. Changes in the wording of the CPS tenure question and in nonresponse rates over time hamper the building of synthetic age cohorts and duration analysis and make it difficult to resolve the different findings. Adding recent CPS data and making better adjustments for changes in wording and other data problems, Neumark, Polsky, and Hansen (this volume) did find a modest decline in the first half of the 1990s among older workers with longer tenures. Similarly, using the DWS, Farber (1997) found a mild rise in involuntary job loss during the 1990s, but changes in wording and time windows make analysis difficult here as well.

Longitudinal data sets permit more direct measurement of moves between employers, and initial research on the Panel Study of Income Dynamics (PSID) appeared to provide consistent evidence of a general increase in the rate of job changing (see, for example, Rose 1995; Boisjoly, Duncan, and Smeeding 1998). But several recent papers found no such overall trend, and again the disagreement hinges on how one resolves the problem of measuring year-to-year job changes (Polsky 1999). Because employers in the PSID are not uniquely identified, a job change must be inferred using several different questions about length of tenure that have changed over the years (see Brown and Light 1992). This measurement problem does not plague the other main source of longitudinal data, the National Longitudinal Survey (NLS), which provides unique employer identification codes that are consistent over time. Although this would seem to be an important advantage for the analysis of trends in job stability, to date only one study has used the NLS for this purpose: James Monks and Steven Pizer (1998) compared two cohorts of young men and found a significant increase in job instability between 1971 and 1990.

It is somewhat puzzling that the NLS data have been underexploited in this research field. Although the term "young men" may convey a narrow segment of the population, in fact the NLS cohorts are followed from their late teens to their midthirties. Roughly two-thirds of lifetime job changes and wage growth occur during these formative years of labor market experience when long-term relationships with employers are established (Topel and Ward 1992). This observation period is particularly useful because the two NLS cohorts bracket the striking growth in earnings inequality that emerged in the 1980s (Levy and Murnane 1992). The first cohort is tracked through the years just preceding this change (1966 to 1981), and the second cohort through the years following its onset (1979 to 1994). Comparing the two cohorts thus provides an opportunity to explore whether there have been changes in job instability and whether they have contributed to the growth in earnings inequality.

In this chapter, we take another look at the NLS data. In part, we seek to subject the Monks and Pizer (1998) findings to closer scrutiny, since the history of this field suggests that differences in measurement and methods can lead to different conclusions. Monks and Pizer made a number of analytic choices that we find questionable: they did not consistently use the employer codes provided by the NLS; they neither chose an equivalent set of years for each cohort nor used the full range of years available; and they restricted their sample to full-time workers. We address these measurement issues in our analysis, model the job change process differently, and add several important covariates. Our findings suggest that, if anything, the rise in job instability is greater than that estimated by Monks and Pizer.

In addition to critically reanalyzing the NLS data, we seek to integrate our findings into the larger debate in several ways. The first is by validating the NLS data as a source of sound information on job stability. The three main data sources on job instability (CPS, PSID, and NLS) need to be reconciled so that we have a thorough understanding of the limitations of each. The recent papers by Neumark, Polsky, and Hansen (this volume) and Jaeger and Stevens (this volume) have made considerable headway on this task for the CPS and PSID. We take up this task for the NLS data, finding strong agreement between NLS and PSID estimates of instability, but less with the CPS estimates; over time the latter echoes some of the findings of Jaeger and Stevens (this volume). Since the potential bias associated with permanent attrition is always a key problem for longitudinal data, we also conduct an extensive attrition analysis. Even under the most conservative assumptions, we find that the effect of attrition on our estimates appears to be small.

Second, the focus of the field has so far been on identifying a general trend in instability for *all* workers, and this is where the controversy resides. But we also have evidence that specific groups in the labor market—less educated workers, black workers, and older men with long tenures—may in fact have experienced an increase in instability, though the results differ by whether the 1990s are included in the analysis and by whether the analysis is restricted to involuntary job loss (for example, see Diebold, Neumark, and Polsky 1997; Jaeger and Stevens, this volume; Polsky 1999). This evidence suggests that researchers should engage more carefully in group-specific analyses, which we do here by focusing on young adults in depth.

Finally, regardless of whether job instability is on the rise, it is important to ask whether the wage outcomes associated with leaving or not leaving an employer have changed. Only a few researchers have addressed this question because resolving data and measurement problems has dominated so much of the effort (but see Polsky 1999; Stevens 1997). As these problems are resolved, however, wage outcomes should increasingly become the focus of study, since wages help to inform us about the welfare consequences of instability. We therefore test for cohort differences in the wage gains that young workers capture as they engage in job shopping and then eventually settle with one employer. We find that the returns to job changing have declined and become more unequal for the recent cohort, mirroring trends in their long-term wage growth.

DATA

We use two data sets from the National Longitudinal Surveys, both of which provide nationally representative samples of young men age fourteen to twenty-two in the first survey year. From the National Longitudinal Survey of Young Men (NLSYM) we use the sample of young men born between 1944 and 1952, surveyed yearly from 1966 to 1981 except for 1972, 1974, 1977, and 1979. From the National Longitudinal Survey of Youth (NLSY) we use the sample of young men born between 1957 and 1965, surveyed yearly from 1979 to 1994. Throughout we refer to the former as the "original cohort" and to the latter as the "recent cohort." We selected non-Hispanic whites only, because attrition among nonwhites was extreme in the original cohort. We also excluded the poor white supplemental sample and the military supplemental sample from the recent cohort, because there are no comparable supplemental samples available for the original cohort. Monks and Pizer (1998) used the same two cohorts in their research but with a different sample: they included nonwhites but excluded part-time workers.

It is important to note that the NLS data are not representative of the entire population over time, unlike the other main longitudinal data set, the PSID. Instead, the NLS data comprise a representative sample of a moving eight-year age window: from the ages of fourteen to twenty-two at the beginning of the panel to the ages of thirty to thirty-eight at the end. The power of this research design lies in the fact that we observe both cohorts across a full sixteen 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.

We conducted a series of analyses to establish the representativeness and comparability of the samples, as well as the impact of differential attrition bias (for details, see Bernhardt et al. 1997). Comparing the initial year samples of the two cohorts (1966 and 1979) to corresponding CPS samples and to each other, we found no problems with representativeness or comparability. The attrition rate, however, is considerably higher for the original cohort than for the recent cohort (25.8 percent versus 7.8 percent).[1] This discrepancy is primarily due to differences in retention rules in the two panels. In the original cohort, any respondent who missed two consecutive interviews was dropped from the survey; such respondents in the recent cohort remained eligible and were pursued for future interviews with great effort.[2] The NLS revised the original base-year weights in each subsequent survey year to account for permanent attrition and nonresponse within any given year, and we use these weights throughout. However, these adjustments were made only along the main sampling dimensions (for example, race), not along the outcome dimensions that are the focus of this chapter. It may be, for example, that respondents who dropped out during the course of the sixteen-year survey period were also more unstable, so that the sample that remains is artificially stable. Later in the chapter, we investigate the extent to which the differential attrition rates between the two cohorts might have affected the cohort differences that we estimate. We also investigate the effect of attrition on wages and find that controlling for age and education removes any attrition bias in wages (as is true with other key variables such as employment status and work experience). We therefore control for age and education in all models.

Finally, about one-third of the original cohort 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 nonveterans. Of course, the veterans lost several years of experience in the civilian labor market during their 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 thirties. We adjust for this in the analyses presented here. Beyond this time lag, however, and consistent with other research (Berger and Hirsch 1983), we found no significant bias on other dimensions (for example, employment rates, hourly wages).

MEASURES

The NLS data have a distinct advantage for this field, because unique employer identification codes allow us to measure directly whether an employer change occurred over a given time span. (In the remainder of the chapter, we use the term "job change" to refer to a separation from an employer). James Brown and Audrey Light (1992) found that these employer codes are the best source of employer identification, not only for the NLS data but also compared to the other longitudinal data sets. We use the employer codes for both cohorts, in contrast to Monks and Pizer (1998), who used them only for the recent cohort and relied on other questions for the original cohort. We focus on the respondent's main "CPS" employer at the time of the survey.[3] In the original cohort, the CPS employer is 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. By successively linking pairs of years, however, we can trace a unique CPS employer over any time span as long as that employer is present in each year. We have restricted our use of the employer codes in the original cohort to match this constraint.

Four noncontiguous years were skipped in the original cohort follow-up surveys. This means that we cannot construct an unbroken series of year-to-year employer comparisons. We therefore construct 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 4.1 shows the years that we use for our analyses and defines the six comparisons being made for each cohort. Monks and Pizer (1998) also used two-year employer comparisons, but they constructed only four of them and did not select the same survey years from

Table 4.1 Years Used for Job Change Analysis

Year of NLS Survey			
Original Cohort	Recent Cohort	Year Number	Years Used for Two-year Comparison
1966	1979	1	
1967	1980	2	2 to 4
1968	1981	3	
1969	1982	4	4 to 6
1970	1983	5	
1971	1984	6	6 to 8
	1985	7	
1973	1986	8	8 to 10
	1987	9	
1975	1988	10	
1976	1989	11	11 to 13
	1990	12	
1978	1991	13	13 to 15
	1992	14	
1980	1993	15	
1981	1994	16	

each cohort. (For example, the fourth and sixth years were used as a comparison for the original cohort but not for the recent.)

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 the 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. The empirical two-year separation rate is thus calculated as the number of respondents who have left their year t employer by year t + 2, divided by the total number of respondents in the risk set in year t. After the risk set was defined, we dropped person-year observations outside the sixteen-to-thirty-four age range to ensure adequate sample sizes within age groups. The resulting sample sizes and mean number of observations contributed by respondents are given at the top of table 4.A1.

We do not disaggregate voluntary from involuntary job changes because data on this variable are missing for a significant fraction of the original cohort person-years and exploratory analysis suggests that there is bias in the missingness. But changes in job stability per se remain an important trend to document, and not only because of the current conflicting findings on this measure. Job stability can confer access to firm-specific training, internal promotion ladders, and health and pension benefits. Similarly, wage growth in the middle and later working years generally accrues from tenure with one employer, rather than from job changing, which may in fact become detrimental. Changing employers thus has potentially strong implications for skills, job security, and wages.

Our second dependent variable, wage, is measured as the respondent's hourly wage at his CPS job at the date of the interview. This measure is constructed by the NLS using direct information if the respondent reported his earnings as an hourly wage, and from questions on the weeks (or months) and hours worked in the last year if the respondent reported in other units. We focus on hourly wages rather than yearly earnings because the latter are confounded by hours and weeks worked and the number of jobs held during the year. Analyses are based on the natural log of real wages in 1992 dollars, using the Personal Consumption Expenditure (PCE) deflator. Cleaning and imputation of missing wages affected less than 6 percent of person-year wage observations in each cohort.

Later in the chapter, we examine the two-year wage changes that correspond to the two-year job changes for the subset of respondents in the risk set who were working in both years. Thus, for any two years that t and t + 2 were used to compute whether or not a job change occurred, we compute the corresponding wage change: $(\ln)\text{wage}_{t+2} - (\ln)\text{wage}_t$. We also compute the total wage growth that each individual experienced over the entire sixteen-year survey period. Total wage growth is measured by specifying a model for the individual-specific *permanent* wage profile over the sixteen years, smoothed of short-term, transitory fluctuations. Specifically, the smoothed wages are predicted hourly wages for each respondent at each age, from a mixed-effects wage model that allows a unique wage profile for each person across his or her work history (cf. Gottschalk and Moffitt 1994; Haider 1997). The appendix contains the technical details of the model.

Finally, table 4A.1 shows the independent variables that are used in this study. All the covariates are measured identically in the two cohorts, and all are time-varying—that is, they are measured at year t for any year t versus t + 2 employer or wage comparison. Although most of these variables are straightforward—see the *NLS Users' Guide* (Center for Human Resource Research 1995) for details on coding—several require elaboration. Industry and occupation are based on 1970 census codes, since these were available for both cohorts. Work experience is not measured with potential experience but rather with cumulative *actual* months worked since age sixteen. For respondents who entered the survey after age sixteen, we imputed the missing months of experience using a model based on observed experience for those who entered the survey before age seventeen. For any years in the remainder of the survey where data on months worked were missing, we imputed the average of the months worked in the surrounding two years. Finally, education is measured using information on both years of education completed and degree received.[4] Thus, respondents coded as high school graduates or college graduates must actually hold those degrees. (A GED is considered equivalent to a high school degree in this coding.)

TRENDS IN JOB INSTABILITY

The key point of interest is whether the two-year separation rates differ between the two cohorts. Figure 4.1 shows the empirical cohort differences, overall and broken down by age, education, and tenure. With no adjustments, 46.4 percent of the original cohort and 52.7 percent of the recent cohort had left their current employer two years later, a 13.6 percent proportionate increase in the rate of job changing. The next three panels illustrate the well-known fact that job instability declines with age, education, and time spent with one employer. In each case, however, the recent cohort shows a higher rate of job changing.

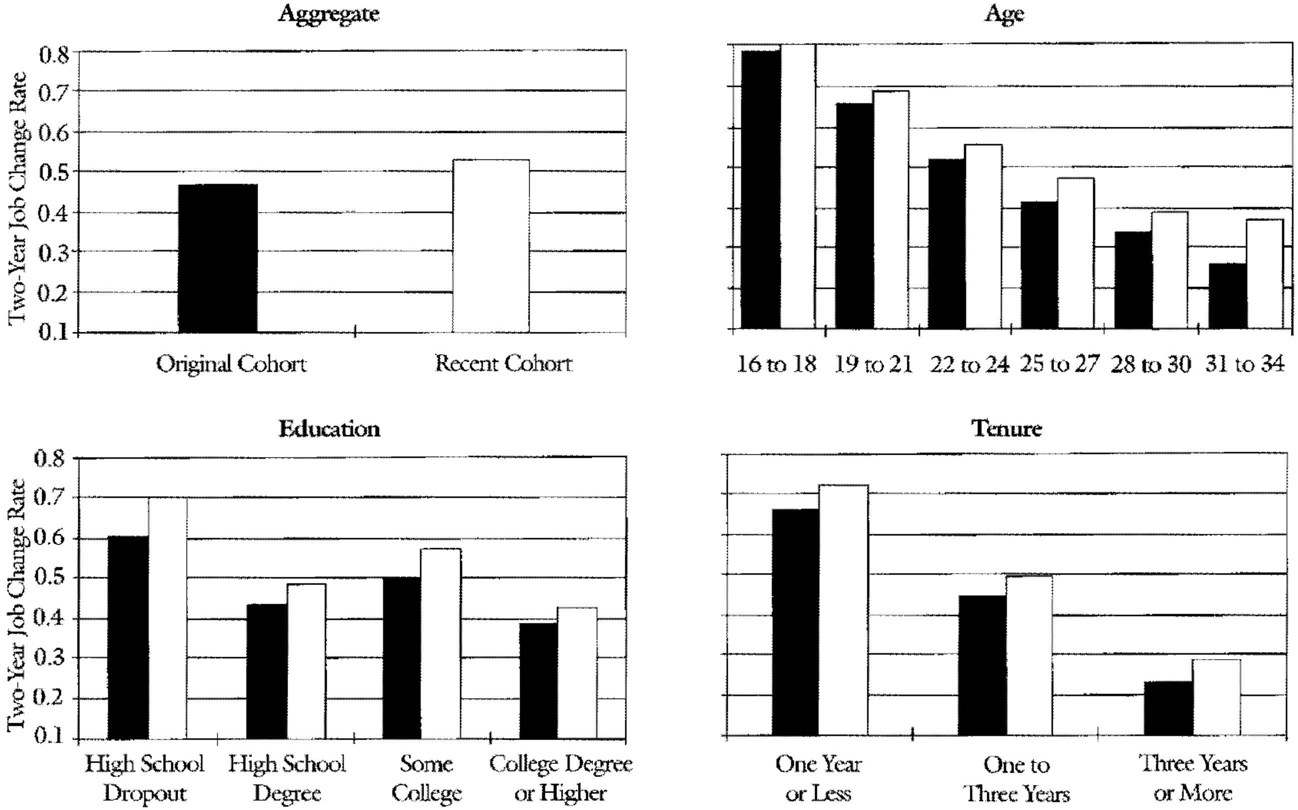
The problem is that all of these dimensions change simultaneously as the cohorts are surveyed over time. We therefore move directly to modeling the separation rates to determine whether there has been a secular increase in the rate of job changing, net of compositional shifts. Let Y_{ijt} indicate whether individual i in job j in year t has left that job by year t + 2. We specify a logistic regression model of the form:[5]

$$\text{logit}(P[Y_{ijt} = 1 \mid X_{ijt}, J_{ijt}, U_{it}, C_i, \phi_i]) = \theta_o X_{ijt} + \theta_1 J_{ijt} + \theta_2 U_{it} + \theta_3 C_i + \phi_i, \tag{4.1}$$

where $P[Y_{ijt} = 1 \mid X_{ijt}, J_{ijt}, U_{it}, C_i, \phi_i])$ is the probability that an individual in job j in year t has left that job by year t + 2 given that they have characteristics X_{ijt}, J_{ijt}, U_{it}, C_i, and ϕ_i, described later, and $\text{logit}(p) = \log[p/(1 - p)]$ is the log-odds of the probability p. Here X_{ijt} represents the time-varying characteristics of the respondent; J_{ijt} represents the 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 zero for the original cohort and one for the recent cohort. In their analysis of the two NLS cohorts, Monks and Pizer (1998) fit somewhat different models, namely, a series of probits with a different specification of the cohort difference and with fewer covariates. (In particular they excluded tenure.) We compare our results with theirs at the end of this section.

We include an individual-specific effect (ISE), ϕ_i, to capture un-

Figure 4.1 Cohort Differences in Job Separation Rates



measured characteristics of the individual that are stable over the sample period. Since the main objective of this term is to reflect the longitudinal nature of the sample, we adopt a simple specification, modeling it as independent of the other regressors (Heckman and Singer 1984).[6] The estimate of the cohort difference was robust to this specification of unobserved heterogeneity, as well as others.[7]

Table 4.2 presents the results of several versions of the above model. In model 1, we control for basic compositional differences. For example, we know that the distributions of age, education, and local unemployment differ across the two cohorts. Controlling for work experience is also important—recall that the Vietnam veterans delayed their entry into the labor market, reaching employment stability at a later age and thus "dragging down" the overall stability of the original cohort. The behavior of these "correction" variables is as expected. The odds of a job change strongly decline with age, tenure, and accumulated work experience as young workers begin to form permanent attachments to employers. Higher local unemployment has a mild positive effect on the odds of a job change.[8] Youth without a high school degree are significantly more likely to leave their current employer than are high school graduates, and those with postsecondary education are significantly less likely to do so.

In sum, after adjusting for key compositional differences, we estimate that the odds of a job change are 43 percent higher for the recent cohort. We consider this our best baseline estimate of the increase in job instability experienced by young white men in the 1980s and early 1990s, compared to their counterparts in the late 1960s and 1970s.[9]

In the next four models, we explore several alternative specifications in order to pursue different substantive questions. In model 2, we examine the impact of additional sociodemographic variables. It is not surprising that enrollment in school raises the odds of a job change, since jobs held during schooling are often short-lived. 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 28 percent 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 its longer periods of college enrollment (Morris et al. 1998); both trends are evident in CPS data as well.

In model 3, we ask whether the economywide shift toward the service sector has played a role. Service industries, as a rule, are more unstable than the public sector and the goods-producing and traditionally unionized industries (with the exception of construction, in which the nature of work is inherently transient). On both fronts, the young workers in the

(Text continues on p. 124.)

Table 4.2 Logistic Regression Estimates for Two-Year Job Separations

Variable	(1) $\hat{\beta}$	(1) $\exp(\hat{\beta})$	(2) $\hat{\beta}$	(2) $\exp(\hat{\beta})$	(3) $\hat{\beta}$	(3) $\exp(\hat{\beta})$	(4) $\hat{\beta}$	(4) $\exp(\hat{\beta})$	(5) $\hat{\beta}$	(5) $\exp(\hat{\beta})$
Intercept	1.544***	4.68	1.173***	3.23	1.436***	4.20	1.839***	6.29	.930***	2.53
	(.052)		(.060)		(.067)		(.070)		(.069)	
Recent cohort	.358***	1.43	.244***	1.28	.176***	1.19	.156*	1.17	.373***	1.45
[original cohort]	(.052)		(.052)		(.052)		(.079)		(.067)	
Age	−.146***	.86	−.063**	.94	−.037	.96	−.109***	.90	−.060	.94
	(.021)		(.022)		(.023)		(.021)		(.034)	
Age squared	.005***	1.00	.002	1.00	.001	1.00	.004***	1.00	.003	1.00
	(.001)		(.001)		(.001)		(.001)		(.002)	
Current education [high school graduate]										
Less than high school	.558***	1.75	.542***	1.72	.478***	1.61	.497***	1.64	.747***	2.11
	(.069)		(.069)		(.068)		(.068)		(.101)	
Some college	.393***	1.48	.205***	1.23	.208***	1.23	.349***	1.42	.088	1.09
	(.057)		(.060)		(.061)		(.058)		(.091)	
College degree or more	−.127*	.88	−.234***	.79	−.151*	.86	−.145*	.86	−.295***	.74
	(.064)		(.065)		(.071)		(.066)		(.087)	
Current tenure [one year or less]										
One to three	−.747***	.47	−.725***	.48	−.702***	.50	−.726***	.48	−.807***	.45
	(.042)		(.042)		(.042)		(.042)		(.059)	

(Table continues on p. 122.)

Table 4.2 *Continued*

Variable	(1) $\hat{\beta}$	(1) $\exp(\hat{\beta})$	(2) $\hat{\beta}$	(2) $\exp(\hat{\beta})$	(3) $\hat{\beta}$	(3) $\exp(\hat{\beta})$	(4) $\hat{\beta}$	(4) $\exp(\hat{\beta})$	(5) $\hat{\beta}$	(5) $\exp(\hat{\beta})$
Three or more years	−.859*** (.055)	.42	−.842*** (.056)	.43	−.811*** (.056)	.44	−.833*** (.055)	.44	−.954*** (.072)	.38
Work experience	−.008*** (.001)	.99	−.006*** (.001)	.99	−.006*** (.001)	.99	−.008*** (.001)	.99	−.008*** (.001)	.99
Local unemployment rate	.008 (.007)	1.01	.009 (.007)	1.01	−.009 (.007)	1.00	.008 (.007)	1.01	.016 (.010)	1.02
Currently enrolled	—		.447*** (.054)	1.56	.402*** (.055)	1.50	—		—	
Living in the South	—		.105* (.052)	1.11	.085 (.051)	1.09	—		—	
Married	—		−.342*** (.045)	.71	−.297*** (.045)	.74	—		—	
Industry [trades, business services]										
Construction, mining, agriculture	—		—		.115 (.066)	1.12	−.037 (.082)	.96	—	
Manufacturing, transportation, and communication	—		—		−.763*** (.051)	.47	−.927*** (.070)	.40	—	

Finance, insurance, real estate, and other professional services	—	—	−.202** (.066)	.82	−.198* (.088)	.82	—
Public administration	—	—	−1.334*** (.107)	.26	−1.456*** (.116)	.23	—
Professional, management, and technical occupations	—	—	−.147** (.053)	.86	—		—
Interaction of cohort and industry							
Recent cohort in high-level services	—	—	—		−.043 (.124)	.96	—
Recent cohort in traditional industries	—	—	—		.241** (.091)	1.27	—
Individual heterogeneity: standard deviations	1.087*** (.036)	1.080*** (.036)	1.025*** (.035)		1.029*** (.035)		1.259*** (.054)
Change in −2 log likelihood	−2133***	−137***	−427***		−459***		−734***

Note: Standard errors are identified in parentheses. Contrast categories are identified in brackets. Age is rescaled to age sixteen. Work experience is measured in months. Model 5 is fit for a subsample of respondents; see text for full explanation. For model 4, change in −2 log likelihood is relative to model 1; for model 5 it is the change relative to the null model for the subsample.

*** = significant at .001; ** = significant at .01; * = significant at .05 level.

recent cohort are disadvantaged. Mirroring the economywide trend, they are less likely to be employed in the public sector and more likely to be employed in the service sector, especially in 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 19 percent 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 that we are capturing 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 differential—the rise in instability has been felt by all education groups. (This is consistent with Monks and Pizer's [1998] finding for whites.) 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 cohort difference in job instability *within* the baseline industries of retail and wholesale trade and business services. The first interaction term indicates that the cohort difference is similar within finance, insurance, real estate, and other professional services. The second interaction term, however, shows a significantly stronger cohort difference in industries that historically have been unionized. Thus, not only are youth in the recent cohort suffering from greater reliance on the "unstable" service sector, but they are not benefiting as much when they are employed in traditionally stable industries such as manufacturing. What we are probably identifying here, albeit indirectly, is the shedding of employment and declines in unionization in the goods-producing and to some extent public sectors.[10]

Finally, we examined whether the greater instability observed in the recent cohort is simply a function of more volatile transitions to the labor market; it could be that the cohort differences in job stability are less pronounced after this transition has been completed. In model 5, we therefore reestimate model 1, but only for workers after they have finished their schooling.[11] The focus, therefore, is on the experience of the young workers once they have permanently entered the labor market. The results are consistent with those from the full sample: in particular, the estimated cohort difference remains strong and significant. (The same finding obtains if we reestimate models 2, 3, and 4.) The increased job instability we have found does not disappear once the young workers "settle down" and is therefore not just a legacy of churning in the labor market early on.

At a general level, our findings match those of Monks and Pizer (1998) in that both studies find greater job instability for the recent cohort. A direct side-by-side comparison of results is not possible: we use different (as well as more) years in our analysis, construct a somewhat different measure of job change, fit different models, and focus on a different sample. A reasonable approximation to their analysis, however, can be obtained if we restrict our sample to full-time workers only and fit a version of model 1 using a continuous linear time trend instead of a cohort dummy and including only education, age, marital status, and the unemployment rate as covariates. Monks and Pizer's (1998) estimate of this time trend for whites, as given in their table 4, is 0.017 (standard error: 0.006), and our estimate is 0.022 (standard error: 0.005), within 1.2 standard errors of their estimate.[12] Thus, there is solid agreement between the two studies to this point, and our attrition analysis in the next section can be seen as commenting on the validity of both.

VALIDATION ANALYSIS

In the context of a research field that has not been able to reach consensus on trends in job instability, the significant increase found above certainly requires a second look. On the one hand, we might expect the NLS data to yield different findings: they focus on young adult men only; they extend from the late 1960s to the early 1990s (thus capturing a longer time span); and they allow for a direct, clean measure of instability. On the other hand, other characteristics of the NLS data may be generating an artificial increase in instability. In particular, the higher attrition rate in the original cohort (25.8 percent versus 7.8 percent in the recent cohort) raises important questions about the interpretation of our findings. If respondents who attrit are also more likely to be unstable in their job change behavior, then our cohort effect for job instability may be upwardly biased by the lower rates of attrition in the recent cohort. We use two strategies to examine the potential confounding effect of attrition. First, we benchmark the NLS job change estimates against estimates based on the PSID and the CPS. This exercise is also important in its own right, since it contributes to cross–data set validation in the field. Second, we develop several model-based adjustments to our instability estimates for the impact of attrition.

We begin by comparing job change estimates from the NLS to estimates from the two other main data sets in the field. We use Polsky's (1999) series for the PSID and Stewart's (1998) series for the CPS; both address some of the well-known problems with changes in measures and question wording over time. If attrition in the original cohort introduces bias, then the job instability estimates from the original cohort will not match up well with the other data sets whereas estimates from the recent cohort will match up well (since attrition in the recent cohort was negligible).

Two factors complicate a simple comparison. First, neither the PSID nor the CPS extend back far enough in time, so they provide only two time points that we can use to compare with the original cohort. Both of these years, however, fall toward the end of the series, when the greater attrition rate in the original cohort is most likely to make itself felt. Second, the two NLS cohorts age throughout the sixteen-year survey period, and because of the skipped interview years in the original cohort, we sometimes have to use two-year instead of one-year job change rates. With these considerations in mind, table 4.3 presents the best comparisons that can be constructed, showing the specific age ranges and years used in each case. For all three data sets, the samples are white working men who are not self-employed. We also reweighted the NLS and PSID distributions to the CPS distribution within age and 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 gives the NLS-PSID comparison, using either one-year or two-year job change rates. For the NLS, these rates are once again calculated using the unique employer codes; for the PSID, the rates are calculated using information on job tenure (Polsky 1999).

Table 4.3 Comparison of Separation Rate Estimates from NLS, PSID, and CPS

Year	Age Range	Measure	Cohort	NLS	PSID[a]	NLS-PSID
1978	Twenty-six to thirty-two	Two-year rate	Original	.3668	.3652	.0016
1980	Twenty-eight to thirty-four	One-year rate	Original	.2292	.2104	.0188
1989	Twenty-six to thirty-two	Two-year rate	Recent	.4078	.4177	−.0100
1991	Twenty-eight to thirty-four	One-year rate	Recent	.2420	.2389	.0031

Year	Age Range	Cohort	NLS One-year rate	CPS[b] Fourteen-month rate	NLS-CPS
1975	Twenty-three to thirty-one	Original	.2721	.3351	−.0630*
1980	Twenty-eight to thirty-six	Original	.2108	.2591	−.0483*
1988	Twenty-three to thirty-one	Recent	.3001	.3452	−.0451*
1989	Twenty-four to thirty-two	Recent	.2942	.3198	−.0256
1990	Twenty-five to thirty-three	Recent	.2653	.3228	−.0575*
1991	Twenty-six to thirty-four	Recent	.2474	.2890	−.0416*
1992	Twenty-seven to thirty-five	Recent	.2546	.2705	−.0159
1993	Twenty-eight to thirty-six	Recent	.2713	.2727	−.0014

[a] Authors' tabulation of data from Polsky (1991).

[b] Authors' tabulation of data from Stewart (1998).

*Difference significant at .05 level.

For both, the measure is the proportion of respondents working at time t who had left their time t employer at time t + 1 or t + 2, depending on which comparison is being made. The two sets of estimates match up remarkably well: none of the differences is statistically significant. Note in particular the close agreement in 1980 for the original cohort, the next to last year of that panel when the rate of attrition peaks. 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.

The second half of the table shows our comparison of the NLS with the CPS. This comparison is more problematic because the two data sets have different measures and risk sets. Stewart's (1998) CPS measure is (1) a fourteen-and-a-half-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, as we might expect given how the measures are defined (one-year change rates for the former, fourteen-and-a-half-month rates for the latter). But the size and significance of the differences vary considerably, both within and between cohorts. Especially worrisome is the variability in the differences *within* the recent cohort, which has very little attrition. Our sense is that it would be difficult to reconcile these two data sets without considerably more analysis, along the lines of Jaeger and Stevens (this volume). It should be noted, however, that these authors also found a divergence between CPS and PSID estimates in the 1970s, though not in the 1980s and 1990s.

Our second attrition analysis is a model-based sensitivity analysis. Specifically, we make several adjustments to our estimate of the cohort difference in job stability, based on potential differences in the behavior of attriters. First, attriters may have higher levels of job instability 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 original cohort sample, and as a result the cohort effect is overstated. Our strategy in calculating the adjusted cohort effects therefore is to "add back in" the missing attriter observations. Since we are conducting a hypothetical experiment—"what would the cohort effect have been if the attriters had not attrited?"—we cannot estimate the adjusted cohort effect empirically from the data. Instead, we derive an expression for this adjusted effect that allows us both to incorporate any greater propensity among attriters to change jobs and to equalize the number of observations contributed by attriters and non-attriters.

We begin by adding several terms to model 1:

$$\text{logit}(P[Y_{ijt} = 1 \mid X_{ijt}, J_{ijt}, U_{it}, C_i, \phi_i, A_{ijt}]) = \theta_o X_{ijt} + \theta_1 J_{ijt} + \theta_2 U_{it} + \theta_3 C_i + \theta_4 A_{ijt} + \theta_5 CA_{ijt} + \phi_i. \tag{4.2}$$

The model now includes two attrition-related terms: 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. Thus, θ_4 represents the attrition effect for the original cohort. (Later we suppress the references to the characteristics X_{ijt}, J_{ijt}, U_{it}, and ϕ_i.) 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:

$$\begin{aligned}
&\text{logit}(P[Y_{ijt} = 1 \mid C_i = k]) \\
&\quad = \text{logit}(P[Y_{ijt} = 1 \mid C_i = k, A_{ijt} = 0])\, P(A_{ijt} = 0 \mid C_i = k) \\
&\qquad + \text{logit}(P[Y_{ijt} = 1 \mid C_i = k, A_{ijt} = 1])\, P(A_{ijt} = 1 \mid C_i = k) \\
&\quad = \theta_0 X_{ijt} + \theta_1 J_{ijt} + \theta_2 U_{it} + \theta_3 k + \phi_i + \theta_4 P(A_{ijt} = 1 \mid C_i = k) \\
&\qquad + \theta_5 k P(A_{ijt} = 1 \mid C_i = k)
\end{aligned} \tag{4.3}$$

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

$$\begin{aligned}
&\text{logit}(P[Y_{ijt} = 1 \mid C_i = 1]) - \text{logit}(P[Y_{ijt} = 1 \mid C_i = 0]) \\
&\quad = \theta_3 + \theta_4 [P(A_{ijt} = 1 \mid C_i = 1) - P(A_{ijt} = 1 \mid C_i = 0)] \\
&\qquad + \theta_5 P(A_{ijt} = 1 \mid C_i = 1)
\end{aligned} \tag{4.4}$$

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 since the difference in attrition rates 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, then θ_5 will be positive and this adjustment will increase the estimate of the cohort effect.

To calculate an adjusted cohort effect based on this derivation, we need to estimate two sets of quantities: θ_3, θ_4, and θ_5, and the conditional probabilities of attrition. We estimated the former using the modified logistic regression model described earlier; we obtained $\theta_3 = 0.3478$, $\theta_4 = 0.2902$, and $\theta_5 = 0.0039$. Note that attriters in the recent cohort are in fact relatively more unstable than attriters in the original cohort. We might expect this, since the recent cohort was pursued more rigorously for continued participation in the survey—any respondents who still managed to drop out of the survey are thus likely to be particularly unstable individuals.

We next estimated the conditional probabilities of attrition that we will use in our derivation. The idea here is to construct these probabilities *as though* the attriters' unobserved years had been included in the analysis. We accomplish this by defining the fraction of attriters at the level of the individual rather than at the level of person-years, so that the number of person-year observations contributed by attriters and non-attriters is equalized. There are three ways these fractions can be defined:

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 in the original cohort and 0.0545 in the recent cohort. In using these fractions, we are effectively adding the person-years that attriters would have contributed had they not dropped out of the sample.
2. *The fraction of attriters in the risk set, equalized for eligibility:* In addition to the adjustment made in (1), we also need to account 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 equalizing the proportion of attriters eligible for the risk set, yielding an adjusted attrition fraction of 0.1996 for the original cohort.
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 (all available survey years). The fraction of persons who ever worked in the full sample and who are lost to attrition is 0.2323 in the original cohort and 0.0658 in the recent cohort.

The adjustments based on each of these three methods are provided in table 4.4, along with the unadjusted estimate from model 1 in table 4.2 for comparison. Although in all cases the attrition adjustment reduces the estimated cohort effect, the reductions are modest. Under method 1, the adjusted cohort effect is 0.3172—an 11.31 percent decrease in the unadjusted value. Under method 2, the adjusted cohort effect is 0.3058—a 14.50 percent decrease in the unadjusted value. We consider this the most accurate adjustment, since it removes both types of attrition bias from the job change sample. Finally, under method 3 the adjusted cohort effect is 0.2996—a 16.23 percent decrease. We feel less comfortable with this adjustment, since it uses estimates from the job change sample (that is, θ_3, θ_4, and θ_5) and applies them to a sample that is not

Table 4.4 Attrition Adjustments to the Cohort Instability Effect

		Adjustments		
	Unadjusted	Method 1	Method 2	Method 3
Fraction of attriters				
Original cohort	.16	.16	.20	.23
Recent cohort	.06	.06	.06	.07
Cohort effect	.3577[a]	.3172	.3058	.2996
Standard error	.052	.042	.042	.042
Adjustment	—	−.0405	−.0114	−.0062
Percentage adjustment	—	11.31	14.50	16.23

[a] Taken from model 1 in table 4.2.

included in the instability analysis conducted here. Even with this most conservative adjustment, however, the recent cohort still has a 35 percent higher odds of a job change.

There are two reasons why the adjustments are modest under all methods. First, because the cohort difference in attrition only ranges from 11 percent (method 1) to 17 percent (method 3), 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 five and a half times larger in order to negate fully the size of the cohort effect.

Second, the recent cohort attrition differential (θ_5) is positive, thus offsetting 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 who missed two sequential interviews were dropped from the survey; such respondents in the recent cohort remained eligible and were energetically pursued for future interviews. 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 percent of the sample). These "hypothetical attriters" have attributes and outcomes that fall in between those of 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.

In sum, both the cross–data set comparisons and the model-based adjustments suggest that although attrition bias exists in the original cohort, it does not alter the statistical significance or the substance of our findings.

WAGE CHANGES

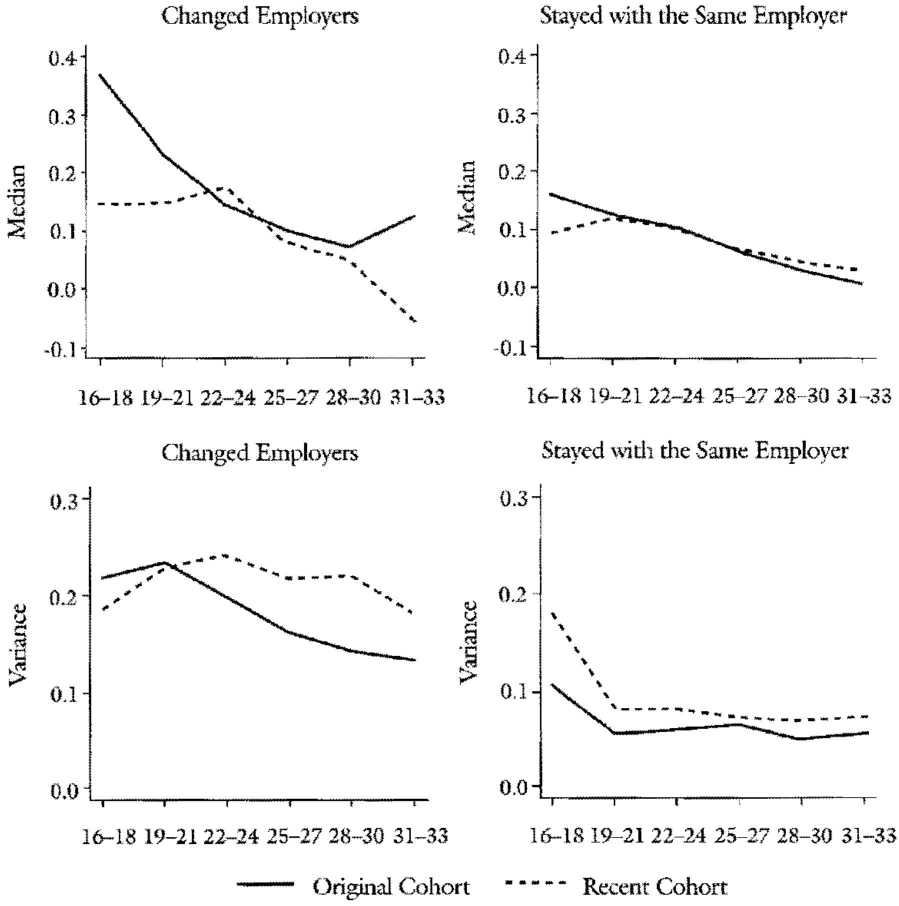
A rise in job instability among young adults in the American labor market does not necessarily signal a problem. In fact, a solid body of research has established that job shopping early in the career is highly beneficial, yielding greater wage gains than staying put with one employer (Borjas and Rosen 1980; Bartel and Borjas 1981). Roughly two-thirds of lifetime wage growth for male high school graduates occurs during the first ten years of labor market experience, and the bulk of it is the result of job changes (Murphy and Welch 1990; Topel and Ward 1992). Although it is in general true that having many employers early on does not impede wage growth (Gardecki and Neumark 1998), in the long term job instability becomes harmful to wage growth, and chronically high levels of job instability are detrimental from the outset (Light and McGarry 1998). In this context, it is important to examine how the wage returns to job shopping have changed for the recent cohort. For example, it is possible that the very nature of career development has changed in recent years. 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 outcomes—specifically, the wage gains that young workers capture as they engage in job shopping and then eventually settle with one employer.

We present a simple descriptive analysis here, not a behavioral model. There is clearly a serious endogeneity problem that must be addressed in any causal analysis of the role that job changes play in wage growth, and this kind of full-scale analysis is beyond the scope of this chapter. Our descriptive findings, however, do provide the first empirical step in establishing whether the association between job stability and wage outcomes has changed.

We continue with the sample used in the job change analysis but select that subset of respondents who were working in both years t and t + 2, so that we can construct the corresponding two-year wage changes.[13] In the top half of figure 4.2, we have plotted median wage changes for workers who left their employer and for workers who stayed with the same employer. This figure confirms that 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. After the mid-twenties, there is less to be gained from switching employers, and wage growth as a whole slows down.

The recent cohort, however, has failed to capture wage growth precisely where it is most critical: in the early stages of job shopping. This deterioration first appears between the ages of sixteen and twenty-one.

Figure 4.2 Two-Year Wage Changes, by Age and Job Change Status (Medians and Variances)



Breakdowns by education show that it is young workers moving directly from high school into the labor market who receive the lowest returns. There is also a noticeable drop in the wage gains resulting from a job change in the early thirties, and this is shared by all except those with a college degree.[14] 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, however, the recent cohort benefits more from staying with the same employer after the midtwenties, because the returns to job changing have declined so steeply at that point.

In table 4.5, we further explore the role of education in these trends, with a model of cohort differences in the wage returns to changing and not changing jobs. (Again, this regression is simply descriptive.) Substan-

Table 4.5 Wage Change Regression Results

Variable	Estimate	Standard Error	Ratio of College to High School[a]
Original cohort			
Did not change jobs			
High school or less (intercept)	.2577	.016	1.42
Some college or more	.0439	.013	
Changed jobs			
High school or less	.0850	.013	1.12
Some college or more	.1084	.014	
Recent cohort			
Did not change jobs			
High school or less	−.0227	.012	1.61
Some college or more	.0264	.014	
Changed jobs			
High school or less	−.0439	.013	3.26
Some college or more	.0915	.015	
Age (rescaled to 16 = 0)	−.0242	.004	
Age squared (rescaled to 16 = 0)	.0010	.000	
Work experience (in months)	−.0006	.000	
Adjusted R^2	.042		
N	11,139		

Note: Dependent variable is two-year change in log wages.

[a] Evaluated at variable means for age, age squared, and experience.

tive findings are summarized in the third column. For the original cohort, the education differentials in wage returns are roughly similar regardless of whether individuals change jobs or not. This is not the case for the recent cohort. Here, young adults with no college experience are getting hit the hardest when they search for jobs—and this, precisely at the time that job changing has become more prevalent. By contrast, those with college experience in the recent cohort have maintained their wage growth when they search for a job.[15]

A second potential impact of job instability is on the variability in wage changes. There has been some debate over the role of transitory wage fluctuations in the overall growth in wage dispersion over the last two decades (Gottschalk and Moffitt 1994). The rise in job instability would seem a natural candidate for explaining an increase in transitory wage variance. In the bottom half of figure 4.2, we have plotted the variances of the observed wage changes. Generally speaking, a job change results in more variable wage changes, as we might expect. The recent cohort, however, consistently shows greater variability in wage gains. This is especially pronounced among job-changers in the later age ranges, yet it is also evident among job-stayers at all ages. This suggests that transitory wage fluctuations associated with job changes are not the only force driving the increase in wage dispersion. Breakdowns by education show consistency in these trends across all education groups.

Finally, we have up to now focused on two-year wage changes and linked them to job change events. The young adult workers observed here, however, 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 4.3 plots the distribution of total wage growth between the ages of sixteen and thirty-six, using "permanent" wages that have short-term fluctuations smoothed out (see earlier discussion).

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 sixteen and thirty-six, compared to

Figure 4.3 Change in Permanent (Log) Wages from Age Sixteen to Thirty-Six

$6.69 for those in the recent cohort—a 23 percent decline (both figures in 1992 dollars). Not surprisingly, this loss of growth has been felt largely by those without a four-year college degree (Handcock and Morris 1998). Second, long-term wage growth has also become significantly more unequal in the recent cohort. 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 percentage of workers experiencing no wage growth or actual real wage declines is 1.7 percent for the original cohort but 7.2 percent for the recent cohort. This polarization becomes progressively stronger as the young workers age, and it is consistent across different levels of education.

To our minds, this figure suggests that there is a connection between trends in job instability and wage inequality, since it mirrors our findings on the wage consequences of job changing. We are currently developing models that will formally test for such a connection.

CONCLUSIONS

In this chapter, we have identified a marked increase in job instability among young white men during the 1980s and early 1990s, 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 job churning early on. It is also not limited to less educated workers. Some of the increase is associated with lower marriage rates in recent years (though it is unclear which is cause and which is effect), as well as with the trend toward longer school enrollment. The shift of the U.S. economy to the service sector—in which jobs are generally more unstable—has also played a role. But in addition, there has been a pronounced decline in job security in manufacturing industries at a time when many young men still depend on this traditional sector for employment. With these and other controls in place, only about half of the overall rise in instability is explained, indicating the presence of additional factors—perhaps linked to the respondents' employers—that we have not been able to measure.

Job instability is not necessarily a bad thing. In fact, previous research has shown that job shopping is actually the main mechanism by which young adults generate wage growth. We find, however, that this process has changed in recent years. Early job search no longer confers the same wage gains it once did, especially on those with less education. It is also yielding more unequal wage gains, and this holds true for all education groups. Our findings therefore suggest that there may be a direct link between job instability and the trends in long-term wage mobility that we and others have documented (Gottschalk and Moffitt 1994; Duncan, Boisjoly, and Smeeding 1996).

The sixteen years covered by the NLS data represent most of the job changes and wage growth that these young adults will experience during their careers. Our findings therefore suggest that public perceptions of rising job instability may not be so far off base, at least for those who entered the labor market during the late 1970s and early 1980s. Their long-term wage trajectories have also changed. Absent a dramatic shift in the American economy, the greater inequality in wage growth that they have experienced will persist over their life course.

APPENDIX

Table 4A.1 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 number of observations contributed per person	3.9	3.8	4.0
Two-year separation rate	.494	.464	.527
Age range	16 to 34	16 to 34	16 to 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
One to three years	29.9	28.8	31.2
Three or more years	30.0	31.0	28.0
Living in the 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, transportation, and communication	34.3	37.1	31.2
Wholesale and retail trade, business services	31.1	26.1	36.6
Finance, insurance, real estate, and other 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

Note: All quantities based on person-years, unless otherwise described.

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:

$$y_{it} = \mu_{it} + e_{it}, \tag{4.5}$$

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:

$$\mu_{it} = \beta_0 + \beta_1 l_{it} + \beta_2 q_{it} + \gamma X_{it} \tag{4.6}$$

where l_{it} and q_{it} are the linear and quadratic age terms, respectively, and 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 sixteen and q_{it} as the quadratic term centered on age sixteen and orthogonal to l_{it}. The random-effects component is specified as:

$$e_{it} = p_{it} + u_{it}, \tag{4.7}$$

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

$$p_{it} = b_{0i} + b_{1i} l_{it} + b_{2i} q_{it}. \tag{4.8}$$

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 individual-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.

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NOTES

1. By attrition we mean respondents who are permanently lost from the panel, not the proportion of respondents who miss the survey in any particular year.
2. This means that for the NLSY there is no formal definition of attrition, except through death. To make the two cohorts comparable in the use of the two-year "drop" rule, we define anyone in the NLSY cohort who missed both the 1993 and 1994 interviews as an attriter. This results in the 7.8 percent attrition rate for the NLSY.
3. 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 was asked to focus on the one at which he worked the most hours. Our exclusive focus on the CPS employer is important to ensure comparability across cohorts, since for the recent cohort information is gathered on up to five jobs every year.
4. The reader may notice that educational attainment is actually lower in the recent cohort. CPS data show that educational attainment among men graduating from high school in the late 1970s and early 1980s fell, probably in response to the oversupply of college-educated workers in the 1970s labor market.
5. For the original cohort, end-dates for jobs are impossible to recover consistently for all years. This induces a form of censoring—that is, interval censoring with variable interval widths—that complicates the usual duration models, so we do not consider them here.
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 that we fit by maximum likelihood (McCulloch 1997).
7. Many alternative specifications can be used to examine robustness. The fixed ISE specification (Topel and Ward 1992) is infeasible because we have a maximum of six observations per individual, and the conditional maximum likelihood estimator (Chamberlain 1984) does not identify the coefficients of time-invariant factors. We relaxed the assumption of independence by specifying a correlation between the ISE, tenure, and education. We also fitted a population-average logistic model using generalized estimating

equations instead of the ISE model (Hu, Goldberg, and Hedeker 1998). In neither case was the cohort effect appreciably changed.

8. We explored more complex specifications of the unemployment rate (for example, pulling out recessions), but none improved on this simple specification.
9. If we estimate model 1 without tenure, the recent cohort has even higher odds of a job change, reflecting the fact that tenure is endogenous in our model. There is no simple solution to this problem; excluding tenure altogether results in a serious misspecification, so we have decided to take the conservative route of including it.
10. The NLS data on union membership are not consistent.
11. Specifically, we include observations from individuals only after they are never enrolled in school again and their education level never increases again. Monks and Pizer's (1998) restriction of their sample to full-time workers probably serves as a rough approximation, but especially in a longitudinal survey, data on full-time work and on completion of school are not perfect substitutes.
12. Monks and Pizer (1998) estimated a probit model, while we estimated a logit model (both were fit with independent random effects). Probit and logit estimates are generally comparable, unless the probabilities being modeled are very low or very high. This is not the case here, since the majority of the probabilities of a job change are within the .3 to .6 range.
13. This means that we are now focusing only on "employer-to-employer" changes, in contrast to the earlier measure, which includes unemployment and out-of-labor-force as a destination state. Refitting the earlier models for the employer-to-employer subset, however, yields very similar results in terms of the cohort differential in instability.
14. 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, and the gap among thirty-one- to thirty-three-year-olds is not: by the later ages a much smaller proportion of the samples is changing jobs.
15. As a check on our findings, we fit this same model using "permanent" wages that have been smoothed of short-term variability. (See the description of the smoothing process earlier in the chapter.) The results were quite similar, with the obvious difference that a substantially greater proportion of the variance was explained using the smoothed wages.

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