

Changes Over Time in the Cost of Job Loss for Young Men and Women

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Abstract

Using data from the two cohorts of the NLSY, we examine whether income losses due to involuntary job separations have changed over time. We find that wage losses among men are similar between the two cohorts. However, women in the 1979 cohort show little evidence of wage losses while women in the 1997 cohort experience wage losses similar to those of men. We present evidence that changes in occupations across cohorts help explain these results.

Keywords: Displaced Workers, Earnings, Wage Scars

1. Introduction

A long literature has documented that involuntary job loss leads to a lasting decrease in wages and earnings, a phenomenon commonly called a wage or an earnings scar.¹ The scars associated with involuntary job separations are an empirical phenomenon, having been documented repeatedly across different datasets.² Initial falls in earnings vary from 14% to 66% with lasting effects at 7% to 39% depending on the sample and the type of earnings being measured (see Couch and Placzek, 2010). Regardless of their exact mechanism, these costs have been shown to be deep and long-lasting so separations can have long-term effects on workers (see Carrington and Fallick (2017) for a recent review of this literature).

This paper uses comparable samples of individuals from the 1980s and 2000s to investigate whether wage scars have changed over time, and particularly whether there are gender differences in changes over time. Importantly, this time period coincided with a continuation of the “grand convergence” of labor market roles for men and women (Goldin, 2014). The 1950s to 1970s saw an increase in labor force participation of married women and an increase in own-wage elasticity, partly driven by an increase in the availability of part-time work and changing attitudes about married women participating in the labor market (Goldin, 2006). The period after 1970 saw an increase in labor force attachment and career expectations among women. Future expectations around having careers and working later in life increased among new cohorts of young women, correlated with increases in education and delays in age of

¹ The term scar and scarring for this phenomenon of a permanent fall in income goes back to Ellwood (1982); while there is some discussion of terminology in the literature, we use these terms as well as “costs” to describe the pattern of post-separation changes in income without referring to a specific theory for causing this income drop.

² As with the literature, our focus is on situations where a worker is involuntarily separated from their job, such as a mass layoff, and not voluntarily separated, such as quitting. Throughout this paper, we use ‘job loss’ and ‘separation’ interchangeably to imply involuntary job separation.

marriage (Goldin, 2006). This coincided with decreases in the own-wage elasticity of labor supply for married women consistent with greater labor force attachment (Blau and Kahn (2007); Heim (2007)). Finally, there was a shift in occupations away from those traditionally associated with women, particularly into new professional positions (Goldin, 2006) and a relative decline in the gender earnings gap (Blau and Kahn, 2006).

These changes raise the possibility that earnings scars could have changed for women over time. Various hypotheses have been offered to explain the empirical patterns of wage scars (see Carrington and Fallick (2017) for a recent discussion). For example, wage scars may be the result of specific human capital which is lost when a worker is separated from a job.

Alternatively, scarring could be the result of a signaling mechanism, wherein future employers interpret the separation as a negative signal of productivity, leading to lower wages. While we do not test these hypotheses directly, they do suggest that changing gender patterns in the labor market could lead to changes in wage scars for women. Job separations would have less effect among workers with lower labor market attachment, or those with less specific human capital, or those working occupations where signals may be less important. The rise of labor market attachment, increased education, and a convergence of both occupations and earnings may mean that women are likely to have seen an increase in the scars associated with job separations as well.³

We utilize the two cohorts of the National Longitudinal Survey of Youth (NLSY) because they provide an ideal dataset for examining the changing nature of job loss. The

³ The timeframe we study also was affected by rising income inequality and rising polarization (for example, see Autor, Katz and Kearney (2006) or Acemoglu and Autor (2011)). While this could affect the labor market of all workers, research suggests that some of the changing dynamics of job polarization have a gender bias, as the analytical and people skills that women have (traditionally) had a comparative advantage in, have become more rewarded in the labor market (e.g. Bacolad and Blum (2010), Yamaguchi (2018)). If these positions are more likely to be associated with signaling or specific human capital, then that is another avenue through which we might see more changes in scarring over time for women.

longitudinal nature of the NLSY allows us to track job separations and earnings changes over time, and the similarities of the surveys makes comparison across cohorts relatively straightforward (for examples of research comparing the cohorts in other contexts, see Lovenheim and Reynolds (2011), Altonji, Bharadwaj, and Lange (2012), and Böhm (2017)). Furthermore, the timing of the cohorts is useful. The earlier cohort (NLSY79) began with individuals aged 14-22 years of age in 1979 and the more recent cohort (NLSY97) began with individuals 12-17 years of age in 1997. Therefore, in our final estimation sample we have information on workers in the 1980s and into the 1990s from the NLSY79 and workers in 2000s for the NLSY97. Thus, the timing of the cohorts captures the period of well-documented changes in the labor market just discussed.

We make several contributions to the literature. First, using similarly constructed samples of workers aged 23-31, we document that the average wage scar from involuntary job loss for younger workers has increased over time, but this increase is much more pronounced for women. The magnitudes of scars for men are similar across cohorts. In contrast, we find little evidence that women in the early cohort experienced any wage scarring while women in the more recent cohort experience scarring that is comparable to men. We demonstrate that our results are robust to a variety of specification choices and are not driven by differences in labor market conditions across samples (such as the Great Recession). Second, we find some evidence of larger annual earnings scars over time for both men and women, although there is evidence that the pattern is more similar among men and women in the recent cohort. This can partially be explained by a decrease in annual weeks worked for several years following a separation for both men and women in the 1997 cohort.

Finally, we provide evidence using Census data and data from the Displaced Worker Supplement (DWS) of the Current Population Survey (CPS) that suggests that the changes for women in wage scarring may be due to occupational changes over time. In particular, we present evidence that the pre-separation earnings of displaced women are higher in the more recent cohort, while no such change is observed for men. We argue that the pattern is consistent with women working in occupations prior to separation that are higher in the income distribution for the later cohort. A decomposition analysis shows the difference in occupations, at least as measured by the percentile in the earnings distribution, can explain about half of the change over time in the weekly earnings cost of job loss for women. We find no similar evidence for men. Thus, the evidence suggests that as women have experienced increased opportunities for employment in higher-paying occupations, it may have led them to become vulnerable to wage scars.

We begin in the next section with a review of the existing literature on wage scars, then describe our methodology and data in the following section. In section 4, we present results about the changing magnitudes of wage scars using the NLYS data. In section 5, we explore possible explanations for the changing patterns we observe for women using NLSY and DWS data before concluding in Section 6.

2. Literature Review

This paper contributes to the understanding of involuntary job loss by examining how scars may have changed over time. Given the decades of research studying the long-run effects of job loss, there are estimates of wage costs at different points in time. However, the studies are often not directly comparable due to different sources of data, differences in sample construction or empirical methodology. For example, Jacobson, LaLonde and Sullivan (1993b) use

administrative data from Pennsylvania to show that workers who collect unemployment insurance from 1975 to 1985 have long-run (6 years) losses of 24% in earnings. Couch and Placzek (2010) also use administrative data, but from Connecticut, to show that workers who collect unemployment insurance from 1993 to 2004 have long run (6 years) losses of 32% in earnings. So there is, perhaps, some evidence that costs of involuntary separations has increased. Similarly, many authors use the Panel Study of Income Dynamics (PSID) allowing some comparison over time. Stephens (2002) examines workers from 1968 to 1992 and finds that displaced workers have 20% lower earnings 5 years after job loss. Krolikowski (2018) also uses the PSID, and when he uses the most similar estimation technique to Stephens (2002) on workers from 1968 to 2009, he finds that displaced workers have approximately 30% lower earnings 5 years after job loss. Again, this provides some indication that these long run effects are becoming worse over time.

The NLSY surveys are not used nearly as frequently in the job loss literature as other datasets, such as those already mentioned or the DWS.⁴ One of the primary reasons is that the existing literature has focused predominantly on middle and late-career workers. If wage scars are due to lost specific human capital, then longer-tenured workers should be the ones that experience large decreases. Furthermore, younger workers could be less attached to the labor force and are earning lower wages than older workers, both of which may lead to smaller magnitudes in scarring. Because the NLSY cohorts start tracking individuals as teenagers and young adults, it takes many years for the samples to include observations in the age range that

⁴ For example, see Table 1 of Couch and Placzek (2010) and Table 1 of Krolikowski (2018) for a summary of work in the area with only two papers having used the NLSY79 for studying displaced workers. Those papers are Kletzer and Fairlie (2003) who primarily study wage scars and Mroz and Savage (2006) who primarily investigate future employment. Jolly and Phelan (2015, 2017) study the effects of displacement on future health insurance coverage and other benefits, but also present evidence on annual earnings losses.

prior researchers have focused upon. For example, the NLSY79 cohort is aged 49-58 in 2014 but the NLSY97 cohort is only 30-36 in 2015. Thus, it is only recently that the newer cohort is old enough to begin studying these scars, but researchers can only do so for relatively younger workers.

While the literature has typically focused on older workers, there are reasons to think that studying younger workers could be useful. For example, if wage scars are due to lost specific human capital, and specific human capital builds up relatively quickly, then even young workers could experience scarring since workers do not need many years of tenure to experience scars. Furthermore, younger workers are on a steeper part of their age-earnings profile, so it is possible that involuntary separations early in the career could have substantial effects on future wages. In fact, Kletzer and Fairlie (2003) have shown that the impacts of involuntary job separation on young workers are not much different than those for older workers. While Kletzer and Fairlie (2003) used the earlier cohort of the NLSY for their study, the information for the 1997 cohort was not included (because they were not old enough) making our paper the first paper, to our knowledge, to systematically compare how the two cohorts have fared as a result of involuntary job loss.

The existing literature has typically not emphasized gender differences or has focused attention to men. Part of the reason for this may be that most early work on displaced workers found little difference in the experiences of men and women after separation (see Carrington and Fallick (2017) for a discussion of the early literature). Another part of the explanation is the availability of data on separations for women, who tend to be underrepresented in datasets used in this literature. For example, the commonly used PSID data follows heads of households,

where a larger majority of these heads are male.⁵ Women are therefore underrepresented in average estimates using this data, which is a problem if they experience different scars.

While much of the literature has not focused on gender differences, there are exceptions, although the evidence is mixed. Kletzer and Fairlie (2003) use the 1979 cohort of the NLSY to show that involuntary separation has a smaller impact on women's wages than it does to men's wages. Jacobsen, Lalonde and Sullivan (1993a) find evidence that women experience smaller losses in wages than men immediately following a separation, but male wages recover a bit faster. Couch and Placzek (2010) find that women lose about the same as men in terms of the level of earnings, but since women's initial earnings are lower, the percentage difference is larger for women. Rodriguez and Zavodny (2001) use the PSID from 1984 to 1993 and find no differences in wage growth amongst men and women after an involuntary job displacement, while Farber (2015) finds mixed results for the earnings of women using the DWS. However, both of these papers find that job losses result in lower reemployment probabilities for women compared to men, although Rodriguez and Zavodny (2001) demonstrate that these differences only hold for married women.

3. Methodology and Data

3.1 Empirical Methodology

We focus on the effect of an involuntary job loss on future hourly income. We ground our estimation strategy in methods commonly used in the literature (often following Jacobson,

⁵ At the time of this writing, Krolikowski (2018) is the most recent published paper on displaced workers using the PSID with summary statistics on sex available; his sample is 76% male. The sample of Stevens (1997), which is the most cited paper studying these workers using the PSID with available summary statistics, is 84% male. Perhaps the most cited displacement study is Jacobson, LaLonde and Sullivan (1993a) which uses administrative data from Pennsylvania, but that sample is also approximately 75% male.

LaLonde and Sullivan (1993a)) to allow us to compare across cohorts.⁶ To be specific, we estimate the following:

$$\ln Y_{it} = \delta_1 D_{it} + \delta_2 (D_{it} \times I(97)) + \beta_1 f(A_{it}) + \beta_2 (f(A_{it}) \times I(97)) + \gamma U_t \quad (1)$$

$$+ \phi_i + \psi_t + \epsilon_{it}$$

The dependent variable, Y_{it} , is measured for individual i in year t . In our primary analysis, we consider logged real hourly wages, common in the literature. It is important to note that such a specification focuses on wages of those actually working, since missing (or 0) wages are not identified. Thus, the specification is tracking how future wages are affected by separations. Separations could also affect whether someone is working and the hours of work. Thus, we also consider annual earnings and labor supply responses as an extension of our primary focus.

The key dependent variables are found in the vector D_{it} which represents whether somebody has had an involuntary job separation, as well as the timing of that separation. This vector is made up of a dummy variable for the year of separation, as well as dummy variables for periods before and after the separation. We include dummy variables for 2 years prior to separation, 1 year prior to separation, the year of separation, each of the 4 years following the year of separation, and for 5 or more years following separation. The left-out category is 3 or more years prior to the first separation. Following the evidence in Stevens (1997), we also include a dummy variable that takes a value of 1 starting in the year of a second separation. This captures any effects on wages and earnings from additional separations.⁷ Thus, we interpret the coefficients on D_{it} as the effects of a first involuntary separation on the outcome, independent of

⁶ Recent publications using this estimation strategy to examine the effects of displaced workers include Huckfeldt (2018), Jolly (2015), and Krolkowski (2018).

⁷ Without the dummy variable for a second displacement, the estimates on the cost of displacement would be biased if one cohort has experienced more second displacements compared to the other cohort. However, this is not an issue for the main results of this paper as they do not change without this dummy variable, as discussed in Section 4.3.

any effects from additional separations. To capture differences across cohorts, we also include an interaction of our displacement variables with an indicator for being in the 1997 NLSY cohort. The coefficients on these interaction terms will then test whether the pattern of wage (or earnings) changes around displacements is different across cohorts.

For controls, $f(A_{it})$ is a quadratic in age to control for typical age-earnings profiles and U_t is the monthly unemployment rate at the time of worker i 's interview.⁸ We interact the age quadratic with the NLSY97 indicator to allow for the age-earnings profile to be different across cohorts. However, we will demonstrate that this is not driving our estimates. We also include individual fixed effects (ϕ_i) to account for time-invariant differences in the characteristics of individuals and time fixed effects to account for variation over time (ψ_t).⁹ The time fixed effects and the unemployment rate at the time of interview should correct for labor market differences over time within and across cohorts. This includes the recessions that occur in each sample, and we will present further evidence that these recessions are not driving our estimates.

We estimate equation (1) separately for men and women to explore differences by gender. In running our primary estimations on wages, we require that each worker have at least three observations and we ignore those observations with missing variables. Finally, we cluster standard errors by individual.

3.2 Data

The analysis sample for this project consists of the two cohorts from the National Longitudinal Survey of Youth data - the NLSY79 and the NLSY97. The NLSY cohorts

⁸ We use the monthly unemployment rate in addition to year fixed effects to account for the state of the labor market within the year. For example, in 2009, the unemployment rate changes by over 2% throughout the year.

⁹ The time fixed effects correspond to survey years, not necessarily calendar years. Individuals are sampled annually, but in the 1997 cohort individuals could be interviewed at the start and end of the same calendar year (e.g. January 2002 and December 2002). Technically, we create an indicator for being in the first (second, third, etc.) year of the respective sample and then include an interaction with the 1997 indicator.

oversample some individuals in the base year so we restrict our analysis to the representative sample in both cases. Next, we limit our data collection to 10 survey years for each cohort, the years 1984 to 1993 for the 79 cohort and from 2002 to 2011 for the 97 cohort.¹⁰ We do this for several reasons. As discussed later, we need specific information about the reason for job separation so that we can focus on involuntary separations. That data is available early in the 97 cohort but only starting in 1984 for the 79 cohort. Furthermore, both NLSY cohorts start on an annual survey schedule, but eventually switch to a biennial survey. Biennial data collection creates challenges to accurately tracking the timing of job separations, so we choose to use data on years for which annual data is collected (up to 1994 for the 79 cohort and 2011 for the 97 cohort). Finally, we further restrict the samples to those observations of individuals between the ages of 23 and 31. Removing the observations less than 23 helps avoid most issues of respondents finishing their education. The maximum age restriction helps to balance the age distribution of the two samples.¹¹

The most important independent variable for our study is the measurement of involuntary separations. The NLSY tracks exit from jobs and asks why workers left or were separated from their jobs. We want separations that are involuntary and relatively unexpected while also being a separation that is without cause. Therefore, following Kletzer and Fairlie (2003) we consider involuntary job separations to be those that are due to plant closings or layoffs. We code a variable equal to 1 if the worker experienced a job separation due to one of these reasons in a

¹⁰ Both cohorts experience a recession with the second experiencing a severe recession. As we discuss later, our methodological approach should account for these recessions and robustness checks indicate that these recessions are not driving our estimates.

¹¹ The NLSY79 sampled a wider ranges of ages in the base year than the NLSY97. Thus, while some individuals in the 1979 cohort would be as old as 36 in our estimation sample, only 4 observations in the 1997 cohort would be as old as 32.

year, and 0 otherwise. We do this for all such separations and track the total number, although as previously discussed, our empirical method focuses on the first separation.

In examining the cost of involuntary job loss, our primary outcome of interest is the wage rate at their primary job. However, the NLSY also collects annual earnings in the previous calendar year from all jobs, which includes wages and salary but also commission and tips. Because it is asked retrospectively (previous year's total income), we recode the variable to represent the year of a separation. For example, if the worker reports a separation in 1988 then we use data from the 1989 survey response to get the annual income in 1988 (the year of separation).¹² These dependent variables are normalized to 2017 dollars using the CPI-U. We also later consider employment, hours and weeks worked as outcomes.

3.3 Summary Statistics

In the top panel of Table 1, we present information about involuntary separations for the primary sample in our data. We have approximately 2,600 individuals in each sample of gender and cohort. The percent of the sample with a separation is fairly similar within gender across cohorts, but there is a difference across genders. Approximately 17 to 18 percent of the men experience a separation in the two cohorts, but only 10 to 12 percent of women experience a separation. The last row of the top panel shows that some individuals do experience multiple separations, which we account for in our regressions.

In the middle panel, we present summary statistics on wages, earnings, and individual characteristics for all observed years of data for all individuals in the sample. We include in this table AFQT scores (a standard measure of cognitive ability collected during adolescence) and

¹² There is missing data for individuals over time, primarily due to survey non-response in a given year. However, because the earnings data relies on two different survey years, one for the separation report and one for annual income, we have less observations when using annual earnings as the dependent variable compared to hourly wages.

percent white for discussion purposes only; these time invariant characteristics are accounted for by the individual fixed effects in our regressions. The statistics suggest that separated workers are less likely to be white and have lower AFQT scores within each combination of cohort and gender. The latter results suggest that separated workers may be disproportionately coming from lower in the ability distribution.¹³

The summary statistics on work outcomes are more challenging to interpret. For example, men in the 1979 NLSY cohort who are separated have an average hourly wage of 16.55 compared to 20.39 of individuals who were never involuntarily separated. However, some of that difference may be due to the separation itself. Similar arguments could be made for annual earnings as well as experience, tenure, hours per week and schooling if separations affect labor market decisions (we will specifically look at labor market outcomes in Section 5.1). To help interpretation, in the bottom panel we present the average characteristics of the separated workers *at the time of their separation*.¹⁴ The hourly wage at separation is higher than the average wage of separated workers across all years presented in Panel B, potentially demonstrating the wage cost of the separation. However, the pre-separation wages (as well as earnings) of individuals remain lower than the average for non-separated workers.

One final point to note is that there is evidence of differences in changes across gender cohorts in wages and earnings. For example, for men the average wage is approximately the same in both the NLSY79 and NLSY97, conditional on separation status. The average (all years) wage of separated workers is similar in both cohorts: 16.55 in the 1979 cohort compared

¹³ We have checked that AFQT scores are not driving the paper's results. We have examined this by running our estimations on subgroups of workers based on high, average and low AFQT scores and our results are similar. Similar results also hold when weighting those with lower AFQT scores to have larger weights in the comparison group. (The comparison group being those observations that have not been involuntarily separated.)

¹⁴ Technically, we calculate the maximum of the value in the year of separation or year prior to separation.

to 16.90 in the 1997 cohort. The same pattern is seen when comparing the average wages of never separated workers (20.39 versus 19.62) and when comparing the pre-separation wages (17.76 versus 17.77). In contrast, women in the later cohort seem to have higher wages within each comparison: the average (all years) wage of workers is about one dollar more for women in the 1997 cohort. Perhaps more importantly for this analysis, the average pre-separation wage is almost two dollars more in the later cohort, suggesting that women (but not men) are experiencing separations at higher wages in the later cohort.

These differences in wage and earnings can be clearly observed in the average age-wage profile shown in Figure 1 and the average age-earnings profile shown in Figure 2. In both cases, the male profile is higher than the female profile within cohort, but the male cohorts are fairly similar while the 1997 female cohort profiles are higher than the 1979 profiles. In fact, the average wage profile for women appears to have moved halfway to the male profile between the two cohorts. In our primary estimations, we allow for the profiles to be different across cohorts, although we also show that not doing so does not impact the estimates. However, the graphs do suggest that women's position within the labor market appear to have changed, which could impact the costs of involuntary separations.

4. Results

4.1 Hourly Wage Costs of Involuntary Separations

In Table 2, we present results of estimating equation (1) for men and women, first using logged hourly wages as the outcome. Column 1 presents the estimated log wage results for men. The non-interacted displacement variables represent the displacement pattern for the 1979 sample and the results follow the typical pattern found in the prior literature: wages fall after separation and are slow to recover (if they recover at all). Specifically, wages fall by approximately 10.5% in the year following a separation (relative to three years prior to the

separation), they stay between a 10 and 12% drop up to four years following the separation, and they are approximately 13% smaller five or more years after the separation. The coefficient on having a second separation is negative, consistent with a further 3.5% decrease in wages, but the results are not statistically significant. The coefficients on the interaction terms test whether this pattern is different in the 1997 cohort. While all of the coefficients on the post-separation interaction terms are negative, they are small in magnitude and not statistically significant. Thus, there is little evidence that the wage costs of involuntary separations are substantively different between cohorts for men.

The log wage estimates of equation (1) for women show a marked contrast. While the non-interacted displacement variables are generally negative (except in the year of separation), they are small in magnitude and never statistically significant. Thus, unlike for men in the early cohort, there is little evidence that women in the 1979 cohort experienced significant wage losses associated with involuntary separations. The post-displacement interaction terms, in comparison, are negative, large in magnitude and statistically significant (except for four years after displacement). The magnitude of the effect is -10.3% in year of displacement, and then varies between -14 and -18% after, with no evidence of a recovery.

The evidence in columns 1 and 2 suggests that the typical pattern of wage losses following an involuntary separation occurs for both cohorts of men, but only the later cohort for women. Figure 3 graphically presents the estimated displacement patterns by gender and cohort using the estimated coefficients in the first two columns of Table 2, clearly showing that the 1979 female cohort pattern is the outlier. Figure 3 does show a small cost of involuntary separation for women in 1979 cohort, although from Table 2 we know the result is not statistically significant. We find our 1979 results to be broadly similar to those of Kletzer and

Fairlie (2003) who do find a small statistically significant effect for the women in the NLSY79 but a much larger effect for men. While differences in sample construction likely explain these differences, our sample construction is consistent across cohorts and the results strongly suggest that wage costs of involuntary separations have increased for women over time.

Note that the graph does not mean that men and the 1997 women have the same wages. The lines are all relative to the wage three years prior to a separation for each set of workers. However, the magnitude of costs relative to that baseline appears similar for the later cohort of women as for both sets of men. Figure 3 helps visualize the results in Table 2, alternatively we could have estimated the regression separately by gender and cohort which produces identical results (see Appendix Table A-1). In sum, Table 2 and Figure 3 suggest that wages for men in both periods, and women in the 1997 cohort, fall after an involuntary separation and show little sign of recovery even five years later.

4.2 Annual Earnings Costs of Involuntary Separations

Lower wages, holding hours constant, would lead to lower annual earnings but workers with involuntary separations may also have reduced hours or employment gaps that would result in even lower annual earnings. Therefore, we also estimate effects of involuntary separations on annual earnings. In columns 3 and 4 of Table 2, we replicate our specifications but replace the log of hourly wages with the log of annual earnings as the dependent variable. Since the natural log of 0 is undefined, this is measured only among individuals with annual earnings. Thus, it captures the effects of separations on the earned income of working individuals (changes in wages and annual hours of work). In columns 5 and 6 of Table 2, we use an inverse hyperbolic sine function which produces the same coefficient interpretation as logged specifications but is identified at 0 (estimates on the non-\$0 sample are nearly identical using the log or inverse

hyperbolic sine function).¹⁵ Note that we find similar estimates using the log of (annual earnings + 1).

For men in column 3, an involuntary separation among the 1979 cohort decreases earnings during the year of separation by 27.1%, among those who have annual earnings. Part of this is directly due to the time out of labor market of the separated worker, but part is due to long-term differences in hours and pay. That can be reflected in the coefficient on the year after a separation which continues to show a statistically significant decrease of 25.9%. Unlike the wage results, there is some evidence of recovery as the effects remain negative but smaller in magnitude out in time.¹⁶ A second separation, however, further reduces annual earnings by 22.3%. The interaction terms provide some evidence that the earnings costs have increased in the later cohort for men as all of the coefficients on the post-separation interaction terms are negative. More importantly, the coefficients grow in magnitude and become statistically significant 4 to 5 years after the separation. When including those with no annual earnings (column 5), the point estimates increase as expected and the 1979 effects are strongly negative and statistically significant. The interaction terms are also negative, although not statistically significant for more than a couple years.

The results for women tend to be less precisely estimated in both cohorts, but the pattern appears to follow the male results. In the NLSY79, among those with annual incomes greater than zero (column 4), women experience large decreases in annual earnings in the year of a separation and one year after, but smaller losses in subsequent years which are not statistically

¹⁵ For annual earnings without \$0, we follow the wage specifications and censor earnings at the 1st and 99th percentiles while restricting the sample to those individuals with at least 3 observations. For the estimates including \$0 annual earnings, we do the same but without the censor at the 1st percentile.

¹⁶ The fact that annual earnings recover to some extent while hourly wages show little recovery is commonly found in the literature. See Figure 1 of Huckfeldt (2018) demonstrating the convergence of wage scars and earnings scars.

significant but with smaller magnitudes in point estimates than for men. Like men, the interaction terms hint that the effect has grown over time with all of the interaction terms negative, increasing over time (like we observe for men) but not statistically significant.¹⁷ The estimates including individuals with \$0 annual earnings (column 6) show a similar pattern, although with coefficients of a larger magnitude.

Figure 4 visually presents the results for those with annual earnings. In general, the 1997 cohort shows less recovery in annual earnings for both men and women, suggesting that part of the difference may be due to differences in hours, which we later explore. However, the graph (and estimates in Appendix Table A-1) also suggest that women's annual earnings response seems to more closely match men's in the later cohort (at least up through 3 years after separation).

4.3 Robustness Checks

The main story of our results, a changing wage cost for women and perhaps changing annual earnings costs for both men and women, are robust to a variety of specification checks. As previously discussed, the story is clearly seen when we estimate separately by gender and cohort (although by construction, the results should be the same as the interaction models, see Appendix Table A-1). Our specification also allows for the age profile to be different across cohorts, and Figures 3 and 4 suggest that this might be true for women. When we remove the interaction terms, and therefore restrict each cohort to have the same age profile, the substantive results are unchanged for both wages and earnings (see Appendix Table A-2). We also estimated models without trimming wages and earnings to the 1st and 99th percentiles. While this adds

¹⁷ Caution should be exercised when considering the coefficient on two or more separations as there are only 14 observations for the women of the 1997 cohort who have been separated twice. Excluding the control for two separations does not change the primary results regarding the pattern of scars.

some noise to the estimates, due to extreme values reported in some years by some individuals, we still find the same overall patterns as Table 2 (see Appendix Table A-3). We have also estimated median effects using quantile regressions and found similar estimates (see Appendix Table A-4).

One might be concerned that the estimates are confounded by differences across cohorts in pre-separation labor market attachment. To test for this, we considered two restrictions on the sample of separated workers. First, we restricted to those with at least 1 year of tenure prior to separation and second, we restricted to those who worked at least 25 hours a week in the job prior to separation. Neither restriction substantively changed the wage estimates in Appendix Table A-5, partly because relatively few separated workers failed these criteria.¹⁸ Finally, we also considered models where we do not condition on have multiple separations, thus allowing the displacement variables to measure the effect on wages and earnings *including* the possibility of future separations. However, doing so has no substantive effect on our findings, which is consistent with the fact that the multiple displacement indicator (and its interaction with 1997 cohort) is frequently not statistically significant in our estimates (results available upon request).

Perhaps the biggest cause for concern are the differences in labor market conditions across cohorts. Both cohorts experience a recession during our time period, both at the end of the sample period for each cohort (July 1990 to March 1991 for the 79 cohort and December 2007 to June 2009 for the 97 cohort). However, the Great Recession was a much deeper and longer-lasting economic downturn, and the difference in magnitudes could affect the costs of separations. Farber (2015) finds that the short-run effects of job displacement are cyclical while Davis and Von Wachter (2011) find that those who lose their job with a larger unemployment

¹⁸ For parsimony we only show the wage estimates in Appendix Table A-5, but annual earnings estimates are similarly unchanged since most of the sample of separated workers met these criteria.

rate suffer larger losses to their lifetime earnings. Carrington and Fallick (2017) also emphasize this as a general finding in their review of the literature. However, the inclusion of year fixed effects and monthly national unemployment rates should account for the role of these economic downturns, but we further explore whether or not our specification is handling this potential issue.

First, we identified all individuals in both cohorts who were separated during a recession. We then removed these individuals from our sample and re-estimated our results. If separations during these recessions were driving our estimates, then we would expect the results to change when we remove these affected individuals from the sample. In fact, removing them has no substantive effect on the main findings. We still see substantial wage costs for men in both cohorts and only a large wage cost for women in the 1997 cohort (see Appendix Table A-6). The annual earnings costs are also generally unchanged, although the interaction terms for women vary more than before but are still not statistically significant.

Next, we include as a control the unemployment rate at the time of separation. This variable is 0 for workers who are never separated. Similarly, it is 0 in the pre-separation period for separated workers, and then a constant in the post-separation period equal to the unemployment rate at time of separation. Thus, in this specification we are still allowing for changes in the labor market conditions within and across cohorts (year fixed effects and unemployment rate at interview) but are now controlling for the labor market condition at time of separation. As before, doing so has no substantive effects on our main findings (see Appendix Table A-7). We still find no changes in wage costs for men; however, the coefficients for women in the 1979 cohort do increase in magnitude but are never statistically significant. Ultimately, the interaction terms are large in magnitude and statistically significant consistent

with our main findings of an increase in the wage costs of involuntary separations for women across cohorts.¹⁹ Thus, the evidence suggests that our primary estimates are not driven by recessions in our sample, likely because the controls for unemployment rate at the time of interview and time fixed effects are sufficiently accounting for variations in the labor market.

5. Discussion and Evidence on the Source of Changing Scars for Women

Overall, the evidence strongly suggests that the changes over time in the wage costs of involuntary separation are isolated to women, and that they are now experiencing wage scars comparable to men. The remaining question is what has caused this change over time? While we do not have a complete answer, we provide some discussion and evidence about possible explanations. We find little evidence that changes in labor supply explain the changes in wage scarring we have identified. Instead, we provide evidence that changes in the distribution of occupational employment appear to an important part of the overall explanation.

5.1 Labor Supply Responses

One possibility is that there have been changes in labor supply responses to involuntary job losses over time for women, which could be important since differences in experience are an important component of the gender wage gap (O'Neill (2003)). Our estimates of wage costs are based on workers who continue to participate in the labor market. If workers respond on the extensive margin of labor supply, by not re-entering the labor market following involuntary separation, then our estimates miss that effect. As discussed, the evidence from the earnings estimates suggests that labor responses may be occurring in our data.

¹⁹ We also considered a specification where we drop all observations of individuals during recession periods. While this decreases the precision of the estimates due to the smaller sample, the overall message does not change and most of the results remain statistically significant. This is consistent with the idea that the time fixed effects are sufficient to handle the periods of recessions.

If women in the 1979 cohort chose to respond to involuntary separation, or the lower wages offered following a separation, by changing their labor supply in a different way than men, it could explain why estimated wage scars are different for men and women in the 1979 cohort. For example, if women were more likely to drop out of the labor market following a separation, then they would drop out of our sample since no wage is observed. This could produce small estimated wage scars for women. Over time, if women began to have labor supply responses more similar to men, for example by staying attached to the market despite lower wages, then it could produce the changing pattern of wage scars that we observe. In fact, we know that labor force participation and workforce attachment increased through the 1980s (see Blau and Kahn (2000)), and that the labor supply elasticity for married women decreased substantially from 1980 to 2000 (Haim (2007), Blau and Kahn (2007)). Thus, it seems plausible that changes in labor supply responses for women could explain the changing wage scars.

To investigate this possibility, we estimated our interacted models, separately by gender, but using different measures of labor supply as the dependent variable (see Table 3).²⁰ The first two columns of Table 3 use a binary variable that measures whether the individual is currently working a primary job (the job for which we measure wages in our wage regressions). The estimates show that men in the 1979 cohort were less likely to report working at time of interview than women. This is particularly true after 2 years since separation. The interaction terms, with larger negative coefficients for women compared to men, provide some evidence that the patterns between men and women have become more similar in the 1997 cohort. While not

²⁰ The estimates in Table 3 have more observations and individuals than our primary wage sample in Table 2 since we are expanding the sample to include those not working and there is less missing data on hours in the NLSY, particularly in the detailed work histories. Additionally, there is no censor applied nor is there a requirement for 3 observations although there is no significant change to these results when applying either or both conditions where appropriate.

statistically significant after the year of separation, summing across time would suggest a decrease of around 20 percent in the likelihood of working a year after a separation for both men and women in the later cohort, and effects of around an 8-11 percentage point decline in years 2 and 3 for both.

Employment is only one margin of labor supply that could matter, of course. It is possible that workers find employment but at lower hours, which would clearly affect annual earnings but could also be associated with lower wages if they represent more marginal jobs. The results in columns 3 and 4 present evidence on hours worked per week for those with a primary job. There is evidence of a gender difference in the 1979 cohort, with men more likely to be working fewer hours a week for several years following an involuntary separation but weaker evidence of the same pattern for women. That could be consistent with men re-entering the labor market in more marginal jobs that have lower hourly pay and lower hours, but women do not show that effect, which could explain the lack of a wage scar for women in the earlier cohort. However, this alone does not seem to fully explain the changes in wage scars over time for women. And while quite noisy, the magnitude of the interaction terms again suggests a more similar pattern between men and women in the later cohort.²¹

These estimates are measured at time of interview, so a potentially more informative measure is the amount of work during a calendar year. We use the detailed work histories in the NLSY to sum annual hours worked across all jobs and divide by 40 to get a measure of full-time equivalent weeks worked. We prefer this measure because it captures both actual weeks worked during the year and hours worked per week. Estimates using this dependent variable are

²¹ We also considered a model with the number of jobs worked at the time of interview, as workers could choose, or be forced by circumstance, to work multiple jobs. There is little evidence of a changing pattern over time for men and women on this dimension as well.

provided in columns 5 and 6. Similar to the estimates in column 1 and 2, men show a larger decrease in participation than women in the 1979 cohort following a separation. The interaction terms are negative for both men and women. When summed with the 1979 coefficients, these again tend to show a convergence between men and women. For example, men in the 1997 cohort work 14 less weeks the year after a separation, and then between 7 to 9 weeks less in later years. Women in the 1997 cohort, work about 12 weeks less in the year after separation and then between 7 and 8 less weeks in later years. Overall, the estimates provide further evidence that post-separation patterns in labor markets have become more similar between men and women across cohorts. These also help explain why wage losses did not appear to change across cohorts for men but there was some evidence that annual earnings losses did grow.

While somewhat noisy compared to the wage evidence, Table 3 suggests that patterns of the extensive and intensive margins of employment following a separation were different between men and women in the 1979 cohort and more similar in the 1997 cohort. The fact that both the wage and employment patterns following a separation appear more similar in the later cohort is an interesting fact, but causality is hard to deduce. Importantly, it does not appear that post-separation differences in employment or hours of work across cohorts and gender are driving the changes we documented in wage scars. For example, we find little change in the broad pattern of wage results when we include experience and experience squared in our estimates. If anything, there appears to be less evidence of a change in wage scars for men and more evidence of a change in wage scars for women (see Appendix Table A-8).²² This may

²² Note that these results also suggest that differences in pre-separation experience cannot explain the patterns of scars across gender and cohort. In fact, controlling for the level of experience at separation does not change the main story of our results.

suggest that other changes in the labor market are leading to more similar patterns of wage scars (and perhaps more similar patterns of labor supply responses).

5.2 Changes in Labor Market Opportunities

A strong possibility is that the labor market situations of women *prior to separation* have changed over time in a way that causes wage scars to appear in the later cohort. Such a change would need to be different for women than men, since the wage scars for men are roughly the same across cohorts.²³ The evidence in Table 1 shows that prior-to-separation wages seemed to increase significantly across cohorts for women compared to men. A plausible explanation is that there has been changes for women across cohorts in the occupations they were working prior to separation. The changing pattern of wage scars would be consistent with a change from occupations where wages would not fall (or decrease much) following a separation in the 1979 cohort to occupations where wages can fall significantly in the 1997 cohort.

Prior research has documented that the average wage gap between men and women decreased during our sample period, and that occupational upgrading was an important component of that change (Blau and Kahn, 2006). Furthermore, there is a growing literature about the importance of gender and the changing return to skills. Men have traditionally been more endowed with motor skills, whose return has decreased over time, while women have been more endowed with analytical or people skills whose return has increased over time (Bacolad and Blum, 2010, Yamaguchi, 2018). There is also evidence of women moving relatively more into jobs with non-routine analytic tasks compared to men (e.g. Black and Spitz-Oener, 2010),

²³ Note that simply higher wages for women would not necessarily predict larger scars since the same dollar decrease in wages would be a smaller percentage of the initial wage, producing smaller estimated scars. Instead, there must be a mechanism by which wages were able to fall more in percentage terms for the 1997 cohort.

which are jobs that have tended to benefit from polarization in the labor market between our sample periods (for example, see Autor, Katz and Kearney (2006)).

If women in the 1997 cohort are working in occupations prior to separation that have scarring similar to the occupations of men in both cohorts, while women in the 1979 cohort were working in very different occupations, it could explain the patterns of wage costs we have documented. For example, women in the 1979 cohort may have been working in occupations for which wage costs were unlikely to occur. This could be because they were working jobs for which specific human capital was not important, so job separation did not lead to wage losses at new positions. Alternatively, perhaps they were not working in positions where signals were important. For example, if they were more likely to work low-level positions for which employers did not find signals of productivity important or useful. If women in the later cohort are working in positions where specific human capital are important or employers value the ability to glean a signal of productivity through job histories (and men have always worked in such occupations), then women should begin to experience wage scars similar to men, which our evidence strongly suggests.

Table 4 presents evidence about occupational shifts over time for men and women. The table is sorted from high to low based on annual earnings in the occupation in 2000 calculated from the Census. We collected Census data from the 1980 and 2000 5% Census samples from IPUMS (Ruggles et al. 2019). The Census data show that women increased their share, relative to men, in 4 of the top 5 highest-paying occupation groups (columns (4) and (5)). In contrast, women saw their share of several occupations in the lowest-paying groups like Machine Operators and Administrative Support Occupations decrease relative to men. These positions may be too low in the earnings distribution for wage scars to appear, either because they are not

associated with job-specific human capital or because employers do not use job histories as signals of productivity.²⁴ Note that there is also evidence in column 3 that these shifts seem to be towards jobs with higher average weekly hours of work, perhaps possibly explaining the more similar patterns in post-separation hours across gender for the 1997 cohort in Table 3.

In columns (6) and (7), we explore the occupations from which individuals were involuntarily separated in the NLSY cohorts. While the estimates are not as informative because of relatively small samples, we see a similar pattern of changes as the overall occupational shifts in the Census. Again, we see a fairly large increase in female separations in the NLSY from higher-paying occupations such as Management Related Occupations and Professionally Specialty Occupations, both of which showed large overall shifts for women in the Census. Men show less of a shift in both occupations. There is also a sizable decrease in separations for women in the NLSY from Machine Operators which is much larger than the decrease for men. Consistent with these changes in occupation categories, and the increase in average pre-separation wages for women in Table 1, we also find evidence that separations for women are shifting up in the wage distribution more than men (see Appendix Table A-9).²⁵

5.3 Displaced Worker Supplement of the Current Population Survey

While we do not have enough observations of separated workers to further explore these occupational changes in the NLSY, we can extend our analysis using data from the Displaced Worker Supplement (DWS) of the Current Population Survey (CPS). We collected data from

²⁴ Note that being separated from a higher-paying job could produce large scars if workers must move into *new* lower-paying occupations. However, it is worth noting that the standard deviations of annual earnings are much larger for higher-paying occupation categories and thus there may be more room to experience large wage declines while staying *within* the same general occupational category.

²⁵ In theory, if women were more bound by minimum wages in the earlier cohort, then that would limit wage scarring in the early period. However, the evidence in Appendix Table A-9 shows that most of the changes in the wage distribution are happening well above the minimum wage, which was less than \$7 (in 2017\$) during the entire sample period for the 1979 cohort. Minimum wages are below the 10th percentile making it unlikely that they can explain the changes we see for women.

IPUMS (Flood et al. 2018) for two time periods: 1984 to 1990 and 2000 to 2006 to be roughly consistent with the timeframe of the 1979 and 1997 cohorts (minus the recessions at the end of each period). The data provides information about workers who had been involuntarily separated in the previous few years (5 years for the early cohort, 3 years for the later cohort). In addition to basic demographic information about the worker, we have information on when the separation happened, the worker's occupation prior to separation and the worker's current occupation, and the worker's reported current weekly earnings and weekly earnings at the job from which they were separated (see summary statistics in Table 5).

Overall, Table 4 demonstrates a relative upward shift in the occupational distribution for women both in terms of the occupations that they work (Census data) and the jobs from which they are involuntarily separated (NLSY and DWS data). In columns (8) and (9) of Table 4, we present the changes from 1984-1990 to 2000-2006 in the occupations from which workers were involuntarily separated, for men and women separately. The results indicate that the overall pattern of changes in separations we observed in the NLSY cohorts over time is not unique to that survey. The DWS data also show a relative increase for women, compared to men, in displacements from higher-paying occupations such as Management Related Occupations and Professional Specialty Occupations. The data also shows that women experienced a relative decrease in separations from occupations like Machine Operators.

Partly because of the larger sample size, the DWS data also allows us to more formally explore the role of changing occupations. In particular, we use a Blinder-Oaxaca decomposition (Blinder (1973), Oaxaca (1973)) across cohorts, separately for each gender, to help uncover the possible contribution of occupational shifts on changing wage scars. The left-hand side is the log of weekly earnings in the current job (*CurrEarn*) minus the log of earnings at the prior job

(*PriorEarn*), a measure of the weekly earnings scar associated with involuntary job loss.

Letting j index gender and $c = \{1980, 2000\}$ index the cohorts, the average weekly earnings scar can be written as: $y_{jc} = \ln(CurrEarn_{jc}) - \ln(PriorEarn_{jc})$. The average change for each gender can then be written and decomposed as:

$$\begin{aligned} E(y_{2000}) - E(y_{1980}) &= E(X'_{2000}\beta_{2000}) - E(X'_{1980}\beta_{1980}) \\ &= [E(X_{2000}) - E(X_{1980})]' \beta_{1980} + E(X_{2000})'(\beta_{2000} - \beta_{1980}) \end{aligned}$$

The first component measures how much of the change in the average weekly scar is due to changes in the observable characteristics of the cohorts (often called the “endowments effect”).

In this setup, it captures how much different the average weekly scar would be in the 1980 cohort if they had the same distribution of observable characteristics as the 2000 cohort. The second component captures how much the average weekly scar would be different if the 2000 cohort had the same coefficients (“prices”) as the 1980 cohort (called the “coefficient effect”). This captures changes over time in how observable characteristics affect average scars.²⁶

Given data on observable characteristics, including a measure of pre-separation occupation, the decomposition allows us to investigate whether changes in occupations can explain changes in female wage scars. To measure occupations, we could include a series of indicators for the broad occupation groups we have discussed, but that only captures changes across broad groups and there is substantial evidence of gender variation within these broad groups. Instead, we use data from the 1980 and 2000 Census to construct measures of the earnings power of detailed occupations. We use the 1980 Census for the early cohort and the 2000 Census for the later cohort, both capturing the beginning of the time period for each cohort.

²⁶ We chose the classic decomposition method because of ease of interpretation; however our main story is unchanged when using a more complex “three-fold” decomposition that allows for an interaction of the change in covariates and the change in coefficients.

We construct median wages in each of the approximately 350 occupations for each Census year, and then create percentiles based on these medians. We then assign these values to workers in the DWS based on the detailed occupation from which they were separated. Thus, we measure the pre-separation occupations of workers as a continuous variable that captures where the occupation falls in the earnings distribution. This allows us to identify whether movement up in the occupation-earnings distribution can help explain changing patterns of involuntary separation costs.

The top panel of Table 6 presents the average difference between log of current weekly earnings and log of prior weekly earnings. The results show that the average weekly earnings scars for women grew by approximately 2.7 percent across cohorts, while the change for men is a slight decrease in the average scar, although it is not statistically significant. While not directly comparable to the regression results using the NLSY, which look at hourly wage or annual earnings scars and use different methods, the overall pattern of increasing wage scars for women and little change for men is apparent.

Panel B shows the role of occupational earnings percentile in the decomposition results, with varying sets of additional covariates. For example, the results in column 1 of panel B for women includes only the earnings percentile of the pre-separation occupation and a quadratic in age. The endowment effect is -0.015, which suggests that differences across cohorts in the average earnings percentile explains over half of the observed growth in the weekly earnings cost of separations. That is, holding constant the coefficients, if women in the 1980s had the same occupational earnings percentile as women in the 2000s, the change over time in the weekly earnings cost of a displacement would have been half as large, approximately $-0.027 - (-0.015) = -0.012$. In column 2, we estimate the decomposition for men and there is no effect of changing

pre-separation occupations on the weekly earnings cost of a separation. That is consistent with the fact that the average occupational standing is similar across cohorts.

There is some evidence in columns 1 and 2 of a coefficient effect for both men and women, although it is larger for men. The coefficient effect may suggest that the cost of separation from higher in the income distribution has increased over time, which may be due to the widening income distribution. The difference between men and women may also be an artifact of increasing income inequality: as the income distribution has widened during the sample period, the level of earnings for a given percentile has increased for the more recent cohort. On average, men are still higher in earnings distribution in the later cohort so the coefficient effect may be capturing some of this widening. In fact, if we use the level of median wages of the occupations, instead of the percentiles, we still only find endowment effects for women. However, the coefficient effect then has a similar magnitude for men and women.

In columns 3 and 4, we add controls for race/ethnicity, marital status, tenure at displacement, and length of displacement. These controls do little to change the overall message of the decomposition. Differences across cohorts in the earnings percentile of the pre-separation occupations can still explain over half of the observed increase in the weekly earnings cost of separations for women and explain no effect for men. However, the coefficient effect has decreased in magnitude for both men and women and is now only statistically significant for men. Finally, in columns 5 and 6 we add controls for education with indicators for less than a high school degree, high school graduate, some college, a bachelor's degree, and graduate school. Doing so has no effect on the endowment effect of pre-separation occupation earnings

percentile, but it does further reduce the magnitude of the coefficient effect to the point that it is not statistically significant for men or for women.²⁷

Our decomposition results are robust to various alternative specifications. We receive similar results when we defined pre-separation occupations using levels of earnings or using categories of earnings. For example, if we define pre-separation occupations into terciles of earnings, we find a small endowment effect for being in the top tercile for men (approximately -0.003***) but a much larger effect for being in the top tercile for women (approximately -0.013***). This is consistent with shifts overtime in the percent of men and women working these jobs. In the 1980s, 36% of the separated men came from occupations in the top third of the earnings distribution, which increased to 40% in the 2000s. In contrast, only 18% of separations for women came from the top tercile of occupations in the 1980s, compared to 31% in the 2000s. Overall, the observed changes in pre-separation occupation in the DWS and the decomposition results suggest that changes in the types of occupations that women are working over our sample period may explain a significant part of the increase in costs of involuntary separations for women.

6. Conclusion

In this paper, we attempt to provide new evidence about how losses associated with involuntary job loss may have changed over time. We investigate the magnitudes of wage scars in the 1980s and 2000s, among young workers, by comparing individuals from the 1979 and 1997 cohorts of the National Longitudinal Survey of Youth. Men show no significant evidence of changes in the magnitudes of wage losses following involuntary job losses over time. In

²⁷ We show results with and without education because we only observe post-separation education levels, instead of the preferred pre-separation levels. Thus, it is possible that the education level increased due to the separation, as people go back to school. We still think these results are useful to consider because we do not think this is creating a substantial bias, but appropriate caution should be made when considering these.

contrast, women show little or no evidence of significant scarring in the 1979 cohort but then experience substantially larger wage costs of job loss in the 1997 cohort. The magnitudes of the effects for women in the 1997 cohort are comparable to those of men in both cohorts. Estimates of annual earnings are often imprecise but suggest an increasing scar for both men and women, and the pattern is more similar between men and women in the recent cohort.

We then attempt to provide some evidence about the underlying causes of the changing scars for women. Using the NLSY cohorts, we find some evidence of shifting patterns in employment following separations across cohorts. In particular, there is evidence that both men and women in the more recent cohort work less weeks per year following a separation compared to the earlier cohort. However, as with wages and earnings, the evidence suggests that the patterns become more similar across gender in the recent cohort.

We argue that the data suggests that the change in wage scars for women is likely driven by changes in the occupations that women are working prior to separation. We find evidence both in the NLSY and the Displaced Worker Supplement of the CPS that women were more likely over time to be separated from occupations that are higher in the income distribution. This shift is broadly consistent with an overall upward shift in occupations worked by women over time, relative to men, that we observe in the Census and has been documented in prior research. We then use occupational data from the Census and from the Displaced Worker Supplement to demonstrate that movement up in the occupational distribution, as measured by the percentile ranking of median earnings, can help explain part of the increase in wage scars for women as well as predicting no change in the average costs for men.

We believe that documenting the empirical facts about changes in the pattern of wage scars provides important context for this literature as well as the literature on the gender wage

gap. We believe that the evidence on changes in relative occupational position prior to separation are strongly suggestive but acknowledge that the results are not conclusive. Additional research using different datasets, or waiting for more years of the NLSY cohorts, is necessary to further test whether or not changing occupations are the primary driver of changes in wage costs for women. Further identifying how and why such changes affect the scars could be useful for understanding the nature of long-term costs from job loss.

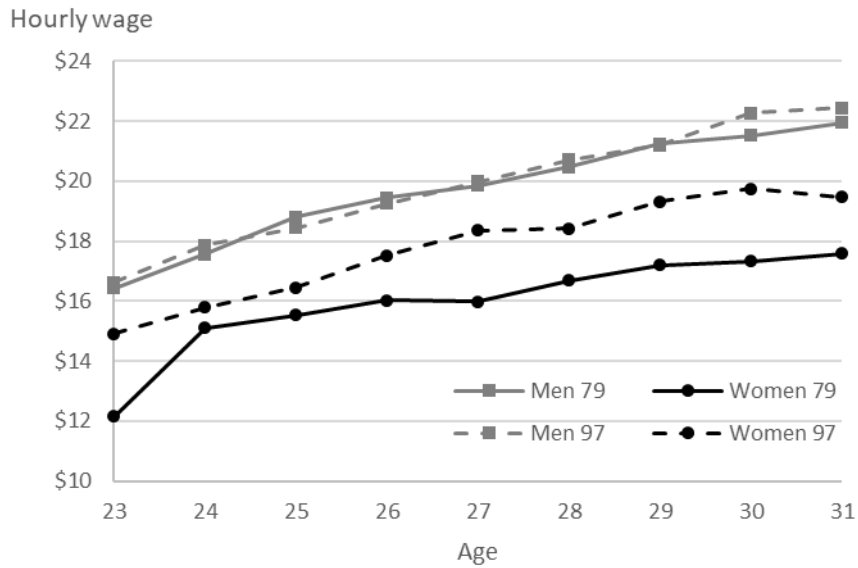
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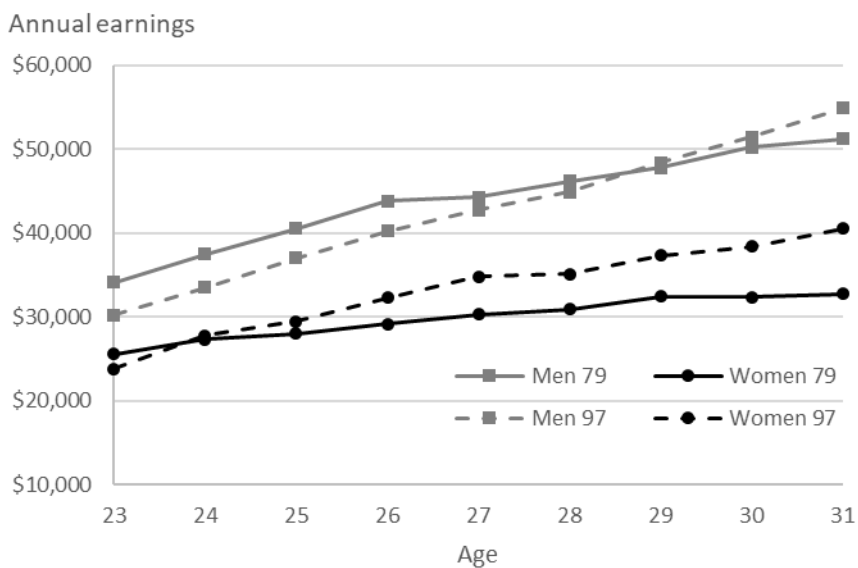
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Figure 1. Average Hourly Wage by Age, Gender and Cohort



Note: The four lines above correspond to the hourly wages for men and women of both the 1979 and the 1997 cohort for the NLSY.

Figure 2. Annual Earnings by Age, Gender and Cohort



Note: The four lines above correspond to the hourly wages for men and women of both the 1979 and the 1997 cohort for the NLSY.

Figure 3. Estimated Hourly Wage Cost of Involuntary Separation



Note: The four lines above correspond to the percentage change in hourly wages for men and women of both the 1979 and the 1997 cohort for the NLSY, relative to the wages 3+ years prior to separation. These percentage changes originate from the coefficients on involuntary separation that come from equation (1) estimated on log wages. Equation (1) has controls for individual and time fixed effects as well as age and age-squared. We also control for the unemployment rate at the time of separation as well as whether the worker has been separated twice.

Figure 4. Estimated Annual Earnings Cost of Involuntary Separation



Note: The four lines above correspond to the percentage change in hourly wages for men and women of both the 1979 and the 1997 cohort for the NLSY, relative to the wages 3+ years prior to separation. These percentage changes originate from the coefficients on involuntary separation that come from equation (1) estimated on log wages. Equation (1) has controls for individual and time fixed effects as well as age and age-squared. We also control for the unemployment rate at the time of separation as well as whether the worker has been separated twice.

Table 1: Summary Statistics

	Male				Female			
	1979 Separated	1979 Never separated	1997 Separated	1997 Never separated	1979 Separated	1979 Never separated	1997 Separated	1997 Never separated
<i>Panel A: Separations</i>								
Observations	2,589	14,846	2,118	12,815	1,637	14,645	1,364	13,168
Individuals	407	2,270	380	2,248	280	2,369	241	2,301
Percent of sample separated	0.18		0.17		0.12		0.10	
Number of separations	1.32		1.18		1.19		1.09	
<i>Panel B: Characteristics for all years</i>								
Hourly wage	16.55	20.39	16.90	19.62	13.71	16.52	14.77	17.50
Annual earnings	31672	47480	31705	40494	24061	32945	25424	32718
Age	27.13	27.11	26.11	26.09	27.13	27.10	26.14	26.03
Years of experience	7.02	8.16	6.88	7.68	5.59	6.60	5.91	6.65
Years of tenure	1.77	3.24	1.63	2.59	1.68	2.87	1.36	2.37
Years of schooling	11.86	13.22	12.26	13.45	12.41	13.43	12.97	13.99
Hours per week	41.99	43.55	38.25	39.32	36.61	37.23	35.04	35.84
White	0.55	0.65	0.58	0.69	0.60	0.63	0.53	0.66
AFQT score	35.86	53.26	39.32	52.87	37.58	53.44	41.04	53.98
<i>Panel C: Characteristics at separation</i>								
Hourly wage	17.76		17.77		14.58		16.19	
Annual earnings	33879		31533		22927		26306	
Age	26.59		26.22		26.71		26.56	
Years of experience	6.58		6.78		5.22		6.07	
Years of tenure	2.07		2.01		1.81		1.69	
Years of schooling	11.81		12.28		12.32		12.99	
Hours per week	45.48		40.67		38.00		38.31	

Notes:

1) Data in panel 2 comes from all years of individual data in the sample.

2) Data in panel 3 for separated workers comes from the maximum value for the worker in the year of their first involuntary separation or in the year *before* that separation

Table 2: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Hourly wage, primary job Men	Women	Annual earnings, all jobs Men	Women	Annual earnings (with 0s) Men	Women
2 Years Before 1st Separation	0.004 (0.027)	-0.006 (0.031)	0.027 (0.056)	0.124* (0.075)	-0.151 (0.262)	0.208 (0.306)
1 Year Before 1st Separation	0.000 (0.031)	0.015 (0.034)	-0.123* (0.067)	-0.007 (0.089)	0.400 (0.252)	0.844** (0.336)
Year of 1st Separation	-0.049 (0.031)	0.023 (0.035)	-0.271*** (0.070)	-0.264*** (0.098)	-0.451 (0.316)	-0.011 (0.386)
1 Year After 1st Separation	-0.105*** (0.037)	-0.024 (0.037)	-0.259*** (0.071)	-0.347*** (0.116)	-1.009*** (0.331)	-1.081*** (0.419)
2 Years After 1st Separation	-0.104*** (0.038)	-0.025 (0.043)	-0.165* (0.085)	-0.093 (0.105)	-0.757** (0.367)	-0.748* (0.449)
3 Years After 1st Separation	-0.121*** (0.043)	-0.031 (0.045)	-0.113 (0.087)	-0.029 (0.130)	-0.984*** (0.372)	-0.748 (0.487)
4 Years After 1st Separation	-0.108** (0.044)	-0.021 (0.050)	-0.103 (0.086)	-0.044 (0.142)	-1.085*** (0.409)	-0.569 (0.539)
5+ Years After 1st Separation	-0.128*** (0.046)	-0.016 (0.056)	-0.164* (0.094)	-0.125 (0.129)	-0.996** (0.449)	-0.822 (0.585)
Two Separations (At Least)	-0.035 (0.050)	-0.051 (0.052)	-0.223** (0.098)	-0.121 (0.174)	-0.556 (0.422)	-0.954 (0.672)
2 Years Before 1st Separation * 97 Cohort	0.008 (0.037)	-0.035 (0.041)	-0.033 (0.083)	-0.092 (0.117)	0.500 (0.363)	-0.280 (0.434)
1 Year Before 1st Separation * 97 Cohort	-0.057 (0.039)	-0.040 (0.045)	0.046 (0.094)	-0.093 (0.129)	-0.154 (0.390)	0.055 (0.470)
Year of 1st Separation * 97 Cohort	-0.031 (0.039)	-0.103** (0.045)	-0.185* (0.106)	-0.277* (0.161)	-2.386*** (0.483)	-2.919*** (0.561)
1 Year After 1st Separation * 97 Cohort	-0.043 (0.049)	-0.148*** (0.052)	-0.114 (0.108)	-0.019 (0.171)	-1.112** (0.497)	-1.426** (0.612)
2 Years After 1st Separation * 97 Cohort	-0.013 (0.053)	-0.168*** (0.054)	-0.114 (0.118)	-0.163 (0.153)	-0.566 (0.525)	-0.768 (0.664)
3 Years After 1st Separation * 97 Cohort	-0.015 (0.062)	-0.183** (0.074)	-0.171 (0.124)	-0.264 (0.192)	-0.714 (0.599)	-0.913 (0.791)
4 Years After 1st Separation * 97 Cohort	-0.024 (0.068)	-0.142 (0.088)	-0.267* (0.146)	-0.213 (0.209)	-0.268 (0.673)	-2.052** (0.903)
5+ Years After 1st Separation * 97 Cohort	-0.033 (0.075)	-0.177* (0.104)	-0.282* (0.168)	-0.168 (0.231)	-0.475 (0.761)	-0.285 (0.882)
Two Separations (At Least) * 97 Cohort	-0.105 (0.066)	0.046 (0.085)	0.059 (0.160)	0.687*** (0.261)	-1.643** (0.722)	1.088 (1.375)
Age	0.160*** (0.030)	0.094*** (0.031)	0.284*** (0.053)	0.056 (0.068)	-0.657 (6.932)	0.099 (0.294)
Age Squared	-0.003*** (0.001)	-0.001** (0.001)	-0.006*** (0.001)	-0.001 (0.001)	0.562** (0.237)	-0.001 (0.005)
Age * 97 Cohort	-0.023 (0.049)	0.050 (0.052)	0.130 (0.091)	0.288** (0.113)	-0.012*** (0.004)	0.734 (0.503)
Age Squared * 97 Cohort	0.001 (0.001)	-0.001 (0.001)	-0.003* (0.002)	-0.006*** (0.002)	-0.185 (0.431)	-0.016* (0.009)
Observations	32,368	30,814	29,579	26,736	34,368	35,677
Individuals	5,305	5,191	4,960	4,599	5,472	5,537

Notes:

This is the estimation of equation (1) on log hourly wages, log annual earnings and inverse hyperbolic sine transformation annual earnings (including 0s). Equation (1) has controls identified above as well as individual fixed effects, year fixed effects, and unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 3: Changes in Labor Market Responses Across Cohorts, by Gender

Variables	Working, primary job		Hours per week, primary job		Full-time equivalent weeks per years, all jobs	
	Men (1)	Women (2)	Men (3)	Women (4)	Men (5)	Women (6)
2 Years Before 1st Separation	0.005 (0.030)	-0.009 (0.039)	-0.917 (0.826)	0.296 (0.967)	-1.415 (1.198)	1.434 (1.435)
1 Year Before 1st Separation	-0.003 (0.031)	0.045 (0.041)	-1.822** (0.882)	-1.390 (0.999)	-0.399 (1.318)	1.646 (1.641)
Year of 1st Separation	-0.573*** (0.035)	-0.400*** (0.048)	-1.751* (0.946)	-2.125** (0.980)	-8.658*** (1.405)	-6.144*** (1.652)
1 Year After 1st Separation	-0.320*** (0.039)	-0.277*** (0.048)	-2.533** (0.995)	-1.360 (1.083)	-9.788*** (1.572)	-8.640*** (1.964)
2 Years After 1st Separation	-0.087** (0.040)	-0.072 (0.052)	-2.640** (1.080)	-2.028 (1.252)	-4.462*** (1.691)	-2.905 (2.048)
3 Years After 1st Separation	-0.095** (0.042)	0.000 (0.055)	-4.043*** (1.077)	-1.552 (1.473)	-5.165*** (1.738)	-2.225 (2.212)
4 Years After 1st Separation	-0.101** (0.045)	-0.018 (0.059)	-3.405*** (1.173)	-1.358 (1.495)	-5.937*** (1.966)	-0.719 (2.395)
5+ Years After 1st Separation	-0.065 (0.046)	-0.005 (0.064)	-3.993*** (1.240)	-1.180 (1.591)	-6.932*** (2.043)	-1.888 (2.619)
Two Separations (At Least)	-0.260*** (0.045)	-0.291*** (0.070)	-0.740 (1.190)	-0.289 (1.683)	-5.756*** (2.225)	-6.758** (3.292)
2 Years Before 1st Separation * 97 Cohort	0.016 (0.042)	0.071 (0.054)	-0.052 (1.117)	-1.480 (1.395)	2.433 (1.607)	-1.647 (2.000)
1 Year Before 1st Separation * 97 Cohort	0.105** (0.041)	0.079 (0.055)	0.737 (1.169)	1.638 (1.443)	0.204 (1.843)	-2.720 (2.216)
Year of 1st Separation * 97 Cohort	-0.186*** (0.043)	-0.301*** (0.056)	0.330 (1.194)	2.205 (1.355)	-6.689*** (1.957)	-7.803*** (2.325)
1 Year After 1st Separation * 97 Cohort	0.134*** (0.052)	0.065 (0.065)	1.378 (1.474)	-0.569 (1.550)	-4.403** (2.193)	-3.675 (2.672)
2 Years After 1st Separation * 97 Cohort	-0.012 (0.055)	-0.041 (0.071)	-1.130 (1.502)	-0.124 (1.750)	-4.136* (2.295)	-4.482 (2.870)
3 Years After 1st Separation * 97 Cohort	0.005 (0.059)	-0.079 (0.080)	1.178 (1.744)	-2.530 (2.165)	-4.101* (2.473)	-5.366 (3.340)
4 Years After 1st Separation * 97 Cohort	0.127* (0.068)	-0.084 (0.091)	-0.923 (1.977)	-0.260 (2.236)	-0.931 (2.833)	-7.315* (3.973)
5+ Years After 1st Separation * 97 Cohort	0.113* (0.068)	-0.093 (0.088)	-0.753 (2.186)	-2.349 (2.437)	-0.665 (3.249)	-5.777 (4.044)
Two Separations (At Least) * 97 Cohort	-0.178*** (0.065)	0.042 (0.113)	-0.318 (1.709)	-1.669 (2.758)	-5.690* (3.224)	1.662 (5.409)
Observations	39,400	40,177	35,484	33,666	39,400	40,177
Individuals	6,096	6,081	5,895	5,849	6,096	6,081

Notes:

This is the estimation of equation (1) but using different measures of labor force participation as the dependent variable. Equation (1) has controls identified above as well as a quadratic in age, individual fixed effects, year fixed effects, and unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 4: Changes in the Distribution of Occupations Over Time, Sorted by Decreasing Annual Earnings in 2000

	Annual earnings, 2000 Census		Weekly hours, 2000 Census	Percent change in occupation worked, Census 1980 to 2000		Percent change pre- separation occupation, NLSY79 to NLSY97		Percent change pre- separation occupation, Displaced Worker Survey 1984-1990 to 2000-2006	
Occupation	(1) Mean	(2) Std. dev.	(3) Mean	(4) Men	(5) Women	(6) Men	(7) Women	(8) Men	(9) Women
Executive, Administrative, and Managerial	81,342	89,942	43.4	0.1	1.8	-1.4	0.4	3.4	1.8
Management Related Occupations	60,782	69,446	39.9	0.5	2.2	0.7	4.2	0.8	2.5
Supervisors of production workers	58,564	40,324	43.2	0.2	0.1	0.0	0.7	0.1	0.1
Professional Specialty Occupations	54,233	64,634	37.3	2.3	5.7	0.5	5.4	4.3	5.5
Technicians	53,731	49,869	38.1	0.5	0.7	-1.3	-1.3	0.6	0.1
Other production workers	42,221	38,174	39.5	0.2	0.0	-6.2	2.1	-3.5	-1.4
Mechanics and repairers	40,651	34,031	40.1	-2.5	0.0	-0.2	0.3	-0.7	0.0
Sales	38,454	60,624	35.5	1.8	0.6	3.7	-1.5	0.2	-0.4
Construction	35,524	37,889	39.2	0.2	0.0	2.4	-0.9	0.7	0.2
Motor vehicle operators	34,608	32,776	40.9	0.2	0.2	-1.2	0.8	-0.3	-0.1
Machine operators	31,041	30,868	36.8	-2.7	-4.6	-2.8	-8.0	-4.8	-9.6
Administrative Support Occupations	30,497	29,465	34.2	1.0	-5.2	1.4	-1.7	1.1	1.7
Other transportation	26,571	31,157	35.3	-1.8	-1.1	3.2	0.3	-3.2	-0.4
Service Occupations	21,277	28,970	31.7	1.9	-0.1	3.3	0.1	1.5	-0.1
Farming, Forestry and Fishing	18,163	32,087	37.9	-0.6	-0.3	-2.0	-0.9	0.0	0.1

Notes:

1) Census calculations use the 5% Census files from IPUMS for individuals aged 18-65 and represent the change in distribution of occupations worked. The NLSY and DWS data is described in the text and represent the distribution of occupations from which workers were separated.

2) All dollar values are in \$2017.

Table 5: Sample Characteristics from Displaced Worker Supplement of Current Population Survey

	Male		Female	
	1980s	2000s	1980s	2000s
Occupational earnings percentile	0.55	0.55	0.38	0.45
Age	36.38	39.34	36.78	40.29
White	0.79	0.71	0.78	0.73
Black	0.09	0.10	0.12	0.12
Asian	0.01	0.03	0.00	0.03
Hispanic	0.09	0.14	0.07	0.10
Other minority	0.02	0.02	0.02	0.01
Married	0.69	0.64	0.54	0.49
Tenure	5.05	4.81	4.04	4.72
Displacement length	2.91	1.92	2.80	1.92
< HS	0.17	0.12	0.12	0.07
HS	0.42	0.31	0.47	0.30
Some college	0.23	0.30	0.25	0.33
BA	0.12	0.20	0.11	0.22
Grad degree	0.06	0.08	0.05	0.08
N	5701	4394	3435	3417

Notes:

1) For the CPS, the early cohort is 1984-1990 and the later cohort is 2000-2006.

Table 6: Blinder-Oaxaca Decomposition of Wage Scars Over Time by Gender, CPS Displaced Worker Data

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Average change in weekly earnings loss</i>						
	Female	Male	Female	Male	Female	Male
Early cohort	-0.152	-0.161	-0.152	-0.161	-0.152	-0.161
Later cohort	-0.180	-0.153	-0.180	-0.153	-0.180	-0.153
Average change	-0.027*	0.008	-0.027*	0.008	-0.027*	0.008
	(0.016)	(0.013)	(0.016)	(0.013)	(0.016)	(0.013)
<i>Panel B: Role of pre-separation occupational earnings percentile</i>						
Endowment effect	-0.015***	0.001	-0.014***	0.001	-0.016***	0.000
	(0.003)	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)
Coefficient effect	-0.041*	-0.073***	-0.036	-0.057**	-0.015	-0.046
	(0.023)	(0.029)	(0.023)	(0.029)	(0.024)	(0.039)
N	6,852	10,095	6,852	10,095	6,852	10,095
<i>Controls:</i>						
Age quadratic	X	X	X	X	X	X
Race/ethnicity, marital status, tenure, length of displacement			X	X	X	X
Education					X	X

Notes:

1) All values in 2017\$.

2) The early cohort is 1984-1990 and the later cohort is 2000-2006.

3) Pre-separation occupation data is drawn from the 1980 Census for the early cohort and the 2000 Census for the later cohort. Median wages were calculated for each detailed occupation in each year, and percentiles for each year were then created and merged to pre-separation occupations of the workers.

Appendix Table A-1: Costs from Involuntary Job Separation, by Cohort and Gender

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Hourly wage, primary job				Annual earnings, all jobs				Annual earnings (with 0s), all jobs			
	Men 79 Cohort	Women 79 Cohort	Men 97 Cohort	Women 97 Cohort	Men 79 Cohort	Women 79 Cohort	Men 97 Cohort	Women 97 Cohort	Men 79 Cohort	Women 79 Cohort	Men 97 Cohort	Women 97 Cohort
2 Years Before 1st Separation	0.004 (0.027)	-0.006 (0.030)	0.013 (0.024)	-0.039 (0.027)	0.026 (0.056)	0.124* (0.075)	-0.008 (0.062)	0.043 (0.089)	-0.151 (0.262)	0.208 (0.306)	0.350 (0.251)	-0.071 (0.308)
1 Year Before 1st Separation	0.000 (0.031)	0.015 (0.034)	-0.052** (0.024)	-0.022 (0.029)	-0.123* (0.067)	-0.007 (0.089)	-0.079 (0.066)	-0.087 (0.093)	0.400 (0.252)	0.843** (0.336)	0.246 (0.298)	0.898*** (0.329)
Year of 1st Separation	-0.049 (0.031)	0.024 (0.035)	-0.079*** (0.024)	-0.077*** (0.028)	-0.271*** (0.070)	-0.264*** (0.098)	-0.457*** (0.080)	-0.530*** (0.127)	-0.451 (0.316)	-0.010 (0.386)	-2.837*** (0.366)	-2.930*** (0.407)
1 Year After 1st Separation	-0.105*** (0.037)	-0.024 (0.037)	-0.144*** (0.032)	-0.169*** (0.036)	-0.259*** (0.071)	-0.347*** (0.116)	-0.372*** (0.082)	-0.361*** (0.126)	-1.009*** (0.331)	-1.081*** (0.419)	-2.121*** (0.371)	-2.506*** (0.446)
2 Years After 1st Separation	-0.104*** (0.038)	-0.025 (0.043)	-0.114*** (0.036)	-0.191*** (0.033)	-0.165* (0.085)	-0.093 (0.105)	-0.273*** (0.083)	-0.245** (0.110)	-0.757** (0.367)	-0.748* (0.449)	-1.323*** (0.376)	-1.515*** (0.490)
3 Years After 1st Separation	-0.121*** (0.043)	-0.031 (0.045)	-0.132*** (0.043)	-0.209*** (0.059)	-0.113 (0.087)	-0.029 (0.130)	-0.270*** (0.089)	-0.279** (0.141)	-0.984*** (0.372)	-0.747 (0.487)	-1.698*** (0.469)	-1.660*** (0.623)
4 Years After 1st Separation	-0.108** (0.044)	-0.021 (0.050)	-0.131** (0.052)	-0.161** (0.073)	-0.103 (0.086)	-0.044 (0.142)	-0.368*** (0.119)	-0.240 (0.155)	-1.084*** (0.409)	-0.569 (0.539)	-1.352** (0.535)	-2.621*** (0.725)
5+ Years After 1st Separation	-0.128*** (0.046)	-0.016 (0.056)	-0.156*** (0.059)	-0.188** (0.088)	-0.164* (0.094)	-0.125 (0.129)	-0.437*** (0.139)	-0.277 (0.192)	-0.996** (0.449)	-0.823 (0.586)	-1.471** (0.614)	-1.106* (0.659)
Two Separations (At Least)	-0.035 (0.050)	-0.051 (0.052)	-0.139*** (0.042)	-0.008 (0.068)	-0.222** (0.098)	-0.121 (0.174)	-0.163 (0.126)	0.547*** (0.197)	-0.555 (0.422)	-0.953 (0.672)	-2.199*** (0.587)	0.134 (1.199)
Age	0.160*** (0.030)	0.094*** (0.031)	0.139*** (0.038)	0.171*** (0.041)	0.283*** (0.053)	0.056 (0.068)	0.480*** (0.071)	0.451*** (0.089)	-4.974 (17.760)	-20.414 (20.722)	0.034 (7.524)	-6.778 (8.967)
Age Squared	-0.003*** (0.001)	-0.001** (0.001)	-0.002*** (0.001)	-0.003*** (0.001)	-0.006*** (0.001)	-0.001 (0.001)	-0.008*** (0.001)	-0.008*** (0.002)	0.562** (0.237)	0.095 (0.294)	0.376 (0.360)	0.828** (0.408)
Observations	17,435	16,282	14,933	14,532	16,446	15,201	13,133	11,535	18,565	20,135	15,803	15,542
Individuals	2,677	2,649	2,628	2,542	2,575	2,482	2,385	2,117	2,749	2,910	2,723	2,627

Notes:

This is the estimation of equation (1) separately for each gender and cohort. Equation (1) has controls identified above as well as an individual fixed effects, year fixed effects, and unemployment rate in month of interview. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-2: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender, Same Age-Earnings Profile

Variables	(1) Hourly wages, primary job Men	(2) Hourly wages, primary job Women	(3) Annual earnings, all jobs Men	(4) Annual earnings, all jobs Women
2 Years Before 1st Separation	0.004 (0.027)	-0.007 (0.031)	0.027 (0.056)	0.124* (0.075)
1 Year Before 1st Separation	0.000 (0.031)	0.015 (0.033)	-0.123* (0.067)	-0.009 (0.089)
Year of 1st Separation	-0.049 (0.031)	0.022 (0.034)	-0.271*** (0.070)	-0.268*** (0.098)
1 Year After 1st Separation	-0.104*** (0.037)	-0.026 (0.037)	-0.260*** (0.071)	-0.352*** (0.117)
2 Years After 1st Separation	-0.104*** (0.038)	-0.027 (0.043)	-0.167* (0.085)	-0.100 (0.105)
3 Years After 1st Separation	-0.120*** (0.043)	-0.033 (0.045)	-0.115 (0.087)	-0.034 (0.130)
4 Years After 1st Separation	-0.107** (0.044)	-0.021 (0.050)	-0.103 (0.086)	-0.047 (0.142)
5+ Years After 1st Separation	-0.128*** (0.046)	-0.015 (0.056)	-0.162* (0.094)	-0.119 (0.129)
Two Separations (At Least)	-0.035 (0.050)	-0.051 (0.052)	-0.224** (0.098)	-0.120 (0.174)
2 Years Before 1st Separation * 97 Cohort	0.008 (0.037)	-0.034 (0.041)	-0.034 (0.083)	-0.090 (0.116)
1 Year Before 1st Separation * 97 Cohort	-0.057 (0.039)	-0.039 (0.044)	0.046 (0.094)	-0.089 (0.128)
Year of 1st Separation * 97 Cohort	-0.031 (0.039)	-0.102** (0.045)	-0.184* (0.106)	-0.274* (0.161)
1 Year After 1st Separation * 97 Cohort	-0.044 (0.049)	-0.147*** (0.052)	-0.111 (0.108)	-0.009 (0.172)
2 Years After 1st Separation * 97 Cohort	-0.015 (0.053)	-0.167*** (0.054)	-0.110 (0.118)	-0.155 (0.153)
3 Years After 1st Separation * 97 Cohort	-0.016 (0.062)	-0.182** (0.074)	-0.166 (0.124)	-0.259 (0.191)
4 Years After 1st Separation * 97 Cohort	-0.025 (0.068)	-0.144 (0.088)	-0.266* (0.145)	-0.218 (0.208)
5+ Years After 1st Separation * 97 Cohort	-0.030 (0.075)	-0.184* (0.104)	-0.292* (0.168)	-0.204 (0.230)
Two Separations (At Least) * 97 Cohort	-0.105 (0.066)	0.044 (0.085)	0.059 (0.160)	0.696*** (0.264)
Observations	32368	30814	29579	26736
Individuals	5305	5191	4960	4599

Notes:

This is the estimation of equation (1) on log hourly wages and log annual earnings. Equation (1) has controls identified above as well as a quadratic in age, individual fixed effects, year fixed effects, and the unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-3: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender, no trimming of dependent variable

Variables	(1) Hourly wages, primary job Men	(2) Hourly wages, primary job Women	(3) Annual earnings, all jobs Men	(4) Annual earnings, all jobs Women
2 Years Before 1st Separation	0.015 (0.032)	0.051 (0.057)	0.039 (0.057)	0.192** (0.079)
1 Year Before 1st Separation	0.036 (0.044)	0.014 (0.042)	-0.138* (0.072)	0.068 (0.092)
Year of 1st Separation	-0.025 (0.038)	0.004 (0.049)	-0.263*** (0.072)	-0.205** (0.098)
1 Year After 1st Separation	-0.085** (0.043)	-0.025 (0.049)	-0.251*** (0.074)	-0.347*** (0.118)
2 Years After 1st Separation	-0.099** (0.050)	-0.022 (0.056)	-0.173* (0.090)	-0.135 (0.118)
3 Years After 1st Separation	-0.082 (0.055)	-0.066 (0.061)	-0.131 (0.090)	-0.043 (0.142)
4 Years After 1st Separation	-0.101** (0.049)	-0.063 (0.064)	-0.126 (0.089)	-0.038 (0.158)
5+ Years After 1st Separation	-0.158** (0.067)	-0.061 (0.069)	-0.162* (0.097)	-0.051 (0.146)
Two Separations (At Least)	-0.051 (0.065)	-0.072 (0.064)	-0.242*** (0.091)	-0.071 (0.185)
2 Years Before 1st Separation * 97 Cohort	0.015 (0.054)	-0.114* (0.067)	-0.082 (0.088)	-0.222* (0.128)
1 Year Before 1st Separation * 97 Cohort	-0.077 (0.068)	-0.046 (0.052)	0.051 (0.099)	-0.188 (0.140)
Year of 1st Separation * 97 Cohort	-0.038 (0.056)	-0.100* (0.060)	-0.194* (0.109)	-0.427** (0.184)
1 Year After 1st Separation * 97 Cohort	-0.133* (0.081)	-0.140** (0.066)	-0.122 (0.112)	-0.034 (0.179)
2 Years After 1st Separation * 97 Cohort	-0.036 (0.089)	-0.189*** (0.067)	-0.127 (0.123)	-0.177 (0.172)
3 Years After 1st Separation * 97 Cohort	-0.073 (0.088)	-0.171** (0.087)	-0.170 (0.128)	-0.363 (0.221)
4 Years After 1st Separation * 97 Cohort	-0.014 (0.119)	-0.131 (0.099)	-0.299** (0.152)	-0.226 (0.235)
5+ Years After 1st Separation * 97 Cohort	0.015 (0.111)	-0.207 (0.131)	-0.278 (0.170)	-0.306 (0.248)
Two Separations (At Least) * 97 Cohort	-0.094 (0.087)	0.087 (0.093)	0.063 (0.156)	0.734*** (0.259)
Observations	33153	31495	30092	27184
Individuals	5367	5255	4993	4649

Notes:

This is the estimation of equation (1) on log hourly wages and log annual earnings. Equation (1) has controls identified above as well as a quadratic in age and the interaction with the indicator for being in the 1997 cohort, individual fixed effects, year fixed effects, and unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-4: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender, Median Estimates

Variables	(1) Hourly wages, primary job Men	(2) Hourly wages, primary job Women	(3) Annual earnings, all jobs Men	(4) Annual earnings, all jobs Women
2 Years Before 1st Separation	0.004 (0.036)	-0.007 (0.031)	0.029 (0.439)	0.114 (0.417)
1 Year Before 1st Separation	0.000 (0.034)	0.015 (0.032)	-0.111 (0.467)	0.010 (0.433)
Year of 1st Separation	-0.049 (0.034)	0.024 (0.032)	-0.267 (0.479)	-0.244 (0.455)
1 Year After 1st Separation	-0.104*** (0.038)	-0.023 (0.037)	-0.257 (0.476)	-0.297 (0.531)
2 Years After 1st Separation	-0.104** (0.041)	-0.025 (0.041)	-0.160 (0.538)	-0.094 (0.507)
3 Years After 1st Separation	-0.120*** (0.044)	-0.032 (0.043)	-0.110 (0.571)	-0.013 (0.591)
4 Years After 1st Separation	-0.108** (0.046)	-0.020 (0.046)	-0.105 (0.588)	-0.016 (0.654)
5+ Years After 1st Separation	-0.128*** (0.046)	-0.015 (0.047)	-0.159 (0.594)	-0.116 (0.584)
Two Separations (At Least)	-0.035 (0.050)	-0.050 (0.052)	-0.215 (0.638)	-0.078 (0.810)
2 Years Before 1st Separation * 97 Cohort	0.008 (0.045)	-0.035 (0.044)	-0.032 (0.618)	-0.086 (0.609)
1 Year Before 1st Separation * 97 Cohort	-0.057 (0.044)	-0.040 (0.044)	0.038 (0.636)	-0.110 (0.617)
Year of 1st Separation * 97 Cohort	-0.031 (0.043)	-0.103** (0.044)	-0.173 (0.724)	-0.252 (0.731)
1 Year After 1st Separation * 97 Cohort	-0.044 (0.052)	-0.147*** (0.052)	-0.099 (0.713)	-0.030 (0.797)
2 Years After 1st Separation * 97 Cohort	-0.013 (0.056)	-0.170*** (0.054)	-0.101 (0.783)	-0.158 (0.754)
3 Years After 1st Separation * 97 Cohort	-0.015 (0.064)	-0.181*** (0.067)	-0.159 (0.855)	-0.249 (0.893)
4 Years After 1st Separation * 97 Cohort	-0.024 (0.072)	-0.142* (0.077)	-0.242 (0.974)	-0.225 (1.047)
5+ Years After 1st Separation * 97 Cohort	-0.032 (0.074)	-0.177** (0.080)	-0.250 (1.047)	-0.141 (1.015)
Two Separations (At Least) * 97 Cohort	-0.106 (0.068)	0.044 (0.097)	0.050 (1.070)	0.538 (1.668)
Observations	32,368	30,814	29,579	26,736
Individuals	5,305	5,191	4,960	4,599

Notes:

This is the estimation of equation (1) on log hourly wages and log annual earnings using quantile regression estimated at the median. Equation (1) has controls identified above as well as a quadratic in experience, a quadratic in age interacted with cohort indicator, individual fixed effects, year fixed effects, and the unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-5: Changes Across Cohorts in Hourly Wage Costs from Involuntary Job Separation, by Gender, Conditioning on Labor Force Attachment

Variables	(1) At least 1 year of tenure pre- separation	(2) At least 1 year of tenure pre- separation	(3) At least 25 hours a week pre- separation	(4) At least 25 hours a week pre- separation
	Men	Women	Men	Women
2 Years Before 1st Separation	-0.000 (0.030)	0.022 (0.035)	0.006 (0.028)	0.005 (0.033)
1 Year Before 1st Separation	0.034 (0.040)	0.077* (0.041)	-0.005 (0.032)	0.022 (0.035)
Year of 1st Separation	-0.010 (0.038)	0.075* (0.039)	-0.044 (0.032)	0.023 (0.035)
1 Year After 1st Separation	-0.128*** (0.048)	-0.041 (0.041)	-0.107*** (0.038)	-0.017 (0.038)
2 Years After 1st Separation	-0.128** (0.051)	-0.042 (0.053)	-0.109*** (0.039)	-0.013 (0.046)
3 Years After 1st Separation	-0.180*** (0.055)	-0.088* (0.051)	-0.143*** (0.043)	-0.013 (0.047)
4 Years After 1st Separation	-0.150*** (0.056)	-0.102 (0.070)	-0.125*** (0.044)	-0.002 (0.052)
5+ Years After 1st Separation	-0.146** (0.059)	-0.079 (0.068)	-0.140*** (0.046)	-0.003 (0.059)
Two Separations (At Least)	-0.076 (0.071)	-0.056 (0.071)	-0.026 (0.052)	-0.024 (0.051)
2 Years Before 1st Separation * 97 Cohort	0.001 (0.040)	-0.045 (0.054)	0.015 (0.037)	-0.042 (0.044)
1 Year Before 1st Separation * 97 Cohort	-0.069 (0.047)	-0.075 (0.058)	-0.045 (0.040)	-0.022 (0.047)
Year of 1st Separation * 97 Cohort	-0.040 (0.047)	-0.147*** (0.057)	-0.028 (0.040)	-0.078* (0.045)
1 Year After 1st Separation * 97 Cohort	-0.034 (0.064)	-0.162** (0.067)	-0.040 (0.050)	-0.138** (0.054)
2 Years After 1st Separation * 97 Cohort	-0.056 (0.070)	-0.121* (0.071)	-0.013 (0.054)	-0.174*** (0.058)
3 Years After 1st Separation * 97 Cohort	0.035 (0.081)	-0.167 (0.107)	0.006 (0.061)	-0.219*** (0.078)
4 Years After 1st Separation * 97 Cohort	-0.062 (0.091)	-0.079 (0.119)	0.001 (0.069)	-0.152 (0.097)
5+ Years After 1st Separation * 97 Cohort	-0.030 (0.100)	-0.138 (0.132)	-0.042 (0.071)	-0.183* (0.111)
Two Separations (At Least) * 97 Cohort	0.064 (0.091)	0.190** (0.095)	-0.105 (0.067)	0.032 (0.085)
Observations	30,254	29,373	32,092	30,482
Individuals	4,945	4,932	5,255	5,123

Notes:

This is the estimation of equation (1) on log hourly wages. Equation (1) has controls identified above as well as a quadratic in experience, a quadratic in age interacted with cohort indicator, individual fixed effects, year fixed effects, and the unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-6: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender, Removing Individuals Separated During a Recession

Variables	(1)	(2)	(3)	(4)
	Hourly wages, primary job Men	Hourly wages, primary job Women	Annual earnings, all jobs Men	Annual earnings, all jobs Women
2 Years Before 1st Separation	-0.005 (0.028)	-0.017 (0.033)	0.016 (0.059)	0.076 (0.079)
1 Year Before 1st Separation	-0.005 (0.032)	0.001 (0.035)	-0.133* (0.072)	-0.113 (0.096)
Year of 1st Separation	-0.054* (0.032)	-0.000 (0.036)	-0.257*** (0.071)	-0.313*** (0.108)
1 Year After 1st Separation	-0.107*** (0.038)	-0.025 (0.039)	-0.239*** (0.075)	-0.419*** (0.124)
2 Years After 1st Separation	-0.114*** (0.040)	-0.024 (0.046)	-0.160* (0.088)	-0.162 (0.115)
3 Years After 1st Separation	-0.121*** (0.045)	-0.025 (0.048)	-0.110 (0.090)	-0.132 (0.141)
4 Years After 1st Separation	-0.113** (0.045)	-0.026 (0.051)	-0.094 (0.088)	-0.118 (0.148)
5+ Years After 1st Separation	-0.134*** (0.046)	-0.020 (0.057)	-0.154 (0.097)	-0.202 (0.136)
Two Separations (At Least)	-0.033 (0.052)	-0.058 (0.055)	-0.240** (0.102)	-0.113 (0.183)
2 Years Before 1st Separation * 97 Cohort	0.009 (0.039)	-0.018 (0.046)	0.050 (0.092)	-0.035 (0.128)
1 Year Before 1st Separation * 97 Cohort	-0.066 (0.043)	-0.019 (0.048)	0.078 (0.105)	0.104 (0.144)
Year of 1st Separation * 97 Cohort	-0.019 (0.042)	-0.064 (0.048)	-0.204* (0.119)	-0.171 (0.193)
1 Year After 1st Separation * 97 Cohort	-0.014 (0.054)	-0.168*** (0.058)	-0.115 (0.126)	0.113 (0.205)
2 Years After 1st Separation * 97 Cohort	0.001 (0.057)	-0.177*** (0.060)	-0.039 (0.136)	-0.081 (0.185)
3 Years After 1st Separation * 97 Cohort	-0.019 (0.076)	-0.197** (0.089)	-0.290* (0.158)	-0.209 (0.232)
4 Years After 1st Separation * 97 Cohort	0.002 (0.080)	-0.112 (0.115)	-0.367** (0.176)	-0.131 (0.256)
5+ Years After 1st Separation * 97 Cohort	-0.012 (0.081)	-0.175 (0.114)	-0.266 (0.186)	-0.074 (0.259)
Two Separations (At Least) * 97 Cohort	-0.142* (0.078)	0.155* (0.082)	-0.130 (0.169)	1.022*** (0.239)
Observations	31618	30315	29006	26356
Individuals	5167	5096	4848	4524

Notes:

This is the estimation of equation (1) on log hourly wages and log annual earnings. Equation (1) has controls identified above as well as a quadratic in age and the interaction with the indicator for being in the 1997 cohort, individual fixed effects, year fixed effects, and unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-7: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender, Including Control for Unemployment Rate at Time of Separation

Variables	(1)	(2)	(3)	(4)
	Hourly wages, primary job Men	Hourly wages, primary job Women	Annual earnings, all jobs Men	Annual earnings, all jobs Women
2 Years Before 1st Separation	0.004 (0.027)	-0.007 (0.031)	0.026 (0.056)	0.125* (0.075)
1 Year Before 1st Separation	0.000 (0.031)	0.015 (0.034)	-0.122* (0.067)	-0.008 (0.089)
Year of 1st Separation	-0.045 (0.075)	-0.022 (0.073)	-0.389** (0.168)	-0.185 (0.229)
1 Year After 1st Separation	-0.100 (0.078)	-0.070 (0.074)	-0.377** (0.168)	-0.269 (0.230)
2 Years After 1st Separation	-0.100 (0.078)	-0.070 (0.076)	-0.282 (0.173)	-0.015 (0.230)
3 Years After 1st Separation	-0.116 (0.081)	-0.076 (0.077)	-0.230 (0.173)	0.048 (0.245)
4 Years After 1st Separation	-0.103 (0.080)	-0.066 (0.079)	-0.219 (0.173)	0.033 (0.257)
5+ Years After 1st Separation	-0.123 (0.081)	-0.062 (0.082)	-0.282 (0.177)	-0.046 (0.245)
Two Separations (At Least)	-0.035 (0.050)	-0.050 (0.052)	-0.223** (0.098)	-0.123 (0.174)
2 Years Before 1st Separation * 97 Cohort	0.008 (0.037)	-0.036 (0.041)	-0.036 (0.083)	-0.091 (0.117)
1 Year Before 1st Separation * 97 Cohort	-0.057 (0.039)	-0.043 (0.045)	0.038 (0.093)	-0.089 (0.129)
Year of 1st Separation * 97 Cohort	-0.030 (0.045)	-0.118** (0.052)	-0.222* (0.115)	-0.253 (0.170)
1 Year After 1st Separation * 97 Cohort	-0.042 (0.054)	-0.163*** (0.056)	-0.151 (0.116)	0.006 (0.183)
2 Years After 1st Separation * 97 Cohort	-0.012 (0.058)	-0.183*** (0.059)	-0.150 (0.127)	-0.141 (0.164)
3 Years After 1st Separation * 97 Cohort	-0.014 (0.065)	-0.193** (0.077)	-0.198 (0.129)	-0.249 (0.193)
4 Years After 1st Separation * 97 Cohort	-0.023 (0.072)	-0.152* (0.089)	-0.293* (0.151)	-0.199 (0.208)
5+ Years After 1st Separation * 97 Cohort	-0.032 (0.079)	-0.188* (0.107)	-0.311* (0.173)	-0.153 (0.232)
Two Separations (At Least) * 97 Cohort	-0.105 (0.066)	0.043 (0.085)	0.062 (0.160)	0.688*** (0.264)
Observations	32368	30814	29579	26736
Individuals	5305	5191	4960	4599

Notes:

This is the estimation of equation (1) on log hourly wages and log annual earnings. Equation (1) has controls identified above as well as a quadratic in age and the interaction with the indicator for being in the 1997 cohort, individual fixed effects, year fixed effects, unemployment rate in month of interview, and the unemployment rate at time of separation for separated workers. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-8: Changes Across Cohorts in Costs from Involuntary Job Separation, by Gender, Including a Quadratic in Experience

Variables	(1)	(2)	(3)	(4)
	Hourly wages, primary job Men	Hourly wages, primary job Women	Annual earnings, all jobs Men	Annual earnings, all jobs Women
2 Years Before 1st Separation	0.008 (0.027)	-0.001 (0.030)	0.015 (0.055)	0.115 (0.076)
1 Year Before 1st Separation	0.007 (0.031)	0.022 (0.033)	-0.141** (0.066)	-0.019 (0.088)
Year of 1st Separation	-0.036 (0.031)	0.036 (0.034)	-0.291*** (0.068)	-0.281*** (0.098)
1 Year After 1st Separation	-0.082** (0.037)	-0.001 (0.036)	-0.273*** (0.069)	-0.334*** (0.116)
2 Years After 1st Separation	-0.076** (0.038)	0.003 (0.043)	-0.176** (0.084)	-0.085 (0.103)
3 Years After 1st Separation	-0.091** (0.043)	-0.002 (0.045)	-0.133 (0.085)	-0.027 (0.127)
4 Years After 1st Separation	-0.081* (0.043)	0.007 (0.050)	-0.137 (0.084)	-0.064 (0.138)
5+ Years After 1st Separation	-0.097** (0.045)	0.002 (0.055)	-0.200** (0.093)	-0.172 (0.130)
Two Separations (At Least)	-0.012 (0.049)	-0.038 (0.052)	-0.220** (0.097)	-0.102 (0.172)
2 Years Before 1st Separation * 97 Cohort	0.007 (0.037)	-0.029 (0.040)	-0.035 (0.082)	-0.077 (0.116)
1 Year Before 1st Separation * 97 Cohort	-0.057 (0.039)	-0.035 (0.044)	0.047 (0.091)	-0.080 (0.125)
Year of 1st Separation * 97 Cohort	-0.022 (0.039)	-0.086** (0.044)	-0.175* (0.103)	-0.247 (0.159)
1 Year After 1st Separation * 97 Cohort	-0.035 (0.049)	-0.133*** (0.051)	-0.095 (0.105)	0.011 (0.170)
2 Years After 1st Separation * 97 Cohort	-0.007 (0.053)	-0.152*** (0.053)	-0.101 (0.115)	-0.115 (0.150)
3 Years After 1st Separation * 97 Cohort	-0.008 (0.061)	-0.157** (0.073)	-0.155 (0.120)	-0.206 (0.188)
4 Years After 1st Separation * 97 Cohort	-0.014 (0.067)	-0.116 (0.088)	-0.232 (0.141)	-0.145 (0.201)
5+ Years After 1st Separation * 97 Cohort	-0.027 (0.074)	-0.131 (0.101)	-0.253 (0.164)	-0.082 (0.220)
Two Separations (At Least) * 97 Cohort	-0.118* (0.065)	0.046 (0.083)	0.044 (0.162)	0.699*** (0.252)
Observations	32368	30814	29579	26736
Individuals	5305	5191	4960	4599

Notes:

This is the estimation of equation (1) on log hourly wages and log annual earnings. Equation (1) has controls identified above as well as a quadratic in experience, a quadratic in age interacted with cohort indicator, individual fixed effects, year fixed effects, and the unemployment rate in month of interview. Variables labeled for the 97 cohort are interacted terms of the original variable and whether the worker is from the 1997 cohort. Robust standard errors are in parentheses, asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Appendix Table A-9: Distribution of Wages and Earnings at Involuntary Separation by Cohort and Gender

	Mean	10 th	25 th	50 th	75 th	90 th
Male NLSY79	16.61	8.72	10.90	14.62	19.98	28.08
Female NLSY79	13.73	7.45	9.20	11.70	16.74	22.54
Male NLSY97	16.65	9.04	11.30	14.62	18.91	25.97
Female NLSY97	15.07	7.91	10.08	13.64	18.03	24.83
NLSY79: Male-Female	2.88	1.27	1.70	2.92	3.24	5.54
NLSY97: Male-Female	1.58	1.13	1.22	0.98	0.88	1.13
Change in Gender Gap	-1.30	-0.14	-0.48	-1.94	-2.36	-4.41
% Change in Gender Gap	-45.22	-11.36	-28.24	-66.43	-72.98	-79.54

Notes:

1) All values in 2017\$.

2) Wages are the maximum of the wage in the year of separation or year prior to separation in the NLSY.