

Job Mobility and the Great Recession: Wage Consequences by Gender and Parenthood

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Abstract: This study examines how inter-organizational mobility affects earnings inequality based on gender and parenthood under different macroeconomic conditions. Fixed effects regression analysis of Survey of Income and Program Participation data from 2004 to 2012 shows that earnings growth after quitting jobs for work-related reasons (e.g., to improve one's job situation) is greater for women than for men pre-recession, but the trend is driven by childless women, and mothers of children under six benefit the least among all groups of workers. However, this motherhood wage penalty disappears in the 2008 recession, as a result of the decline of wage returns to mobility for childless women. The analysis also shows that across economic conditions, the rate of layoffs or displacement is higher among men than women, but once laid off, women experience greater earnings losses than men. No motherhood penalty is found for this mobility type. These findings help us understand the longitudinal process by which the motherhood wage penalty is generated, and conditions under which a motherhood-based or gender-based wage gap becomes more pronounced.

Keywords: gender; job mobility; motherhood wage penalty; Great Recession; quitting; layoffs

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As the U.S. economy has shifted from manufacturing to service production, and flexible labor contracts have diffused across the labor market, average rates of job mobility have increased substantially (Cappelli 1999; Farber 2010; Heckscher 1995; Kalleberg 2011). One key question emerging from this trend concerns its consequences for workers' wages. Although numerous studies have examined the effect of job mobility on wages (e.g., Cha and Morgan 2010; Farber 2005; Fuller 2008), we know surprisingly little about its implications for gender earnings inequality. The few studies that examine gender-differentiated outcomes of job mobility are based on labor market data from prior to 2000 and do not consider parental status, a critical factor that affects men's and women's labor market decisions and outcomes (e.g., Keith and McWilliams 1995, 1999; Loprest 1992; Ruhm 1987). Using wage data that reflects recent labor market conditions (from 2004 to 2012), this study examines whether inter-organizational mobility affects men's and women's wages differently while paying close attention to the effect of parenthood on mobility

outcomes, given that much of the gender wage gap is attributable to the lower wages of mothers (e.g., Budig and England 2001; Budig and Hodges 2010).

Another goal of this study is to examine whether and how macroeconomic conditions affect gender-specific mobility outcomes. There has been much public discussion about gender-specific effects of the most recent recession. On the one hand, male-dominated industries or occupations (e.g., manufacturing, construction) are more sensitive to economic conditions, leading some to refer to the most recent recession as a "man-cession" (e.g., Folbre 2010). On the other hand, the recovery was slower in the public sector, in which women are concentrated, giving the recession a more prolonged effect on women (Hartman, Fischer, and Logan 2012). As I discuss below, theoretical work on the effect of the recession on women's employment outcomes has been equivocal, and the related empirical record is thin. This results in little guidance for understanding how the recession may change gender-specific or motherhood-specific outcomes of job mobility. In

this study, I seek to build an empirical foundation for understanding this problem.

To examine these questions, I use longitudinal data drawn from the 2004 (pre-recession) and 2008 (onset of recession) panels of the Survey of Income and Program Participation and estimate the effect of job mobility and the moderating effect of parental status on men's and women's earnings. The findings help us understand the longitudinal process by which the well documented "motherhood wage penalty" (e.g., Budig and England 2001) is generated and the conditions under which a motherhood-based or gender-based wage gap becomes more pronounced.

Job Mobility, Gender, and Earnings

Prior research, mostly in labor economics, has documented that the relationship between job mobility and earnings largely depends on the reasons for the move, because these reasons are associated with worker heterogeneity and changes in employer-employee skill matches (e.g., Bartel and Borjas 1981; Mincer 1986). In particular, job mobility for the purpose of career advancement leads to discrete earnings growth, presumably because only highly productive workers will have an option to change employers for this reason, or because the skill requirements of the new job better match the worker's skills. In contrast, job mobility resulting from an employer's operating decision (e.g., plant closing, layoff) or quitting jobs for familial or personal reasons (e.g., to raise children, illness) is associated with earnings losses, because those who are laid off may be less productive than the "survivors," or because being laid off or quitting jobs for non-work reasons does not improve skill matches and only leads to loss of firm-specific skills.

Gender differences in wage returns to job mobility have been explained by compositional differences in the types of job mobility: a higher proportion of men are laid off (given layoff rates are higher in male-dominated occupations or industries), but a higher proportion of women change jobs for familial reasons. The gender differences within the same type of mobility, however, are rather unclear. Some studies show that once the reasons for the moves are accounted for, gen-

der differences in wage returns to job mobility disappear, implying that the gender differences mostly stem from the composition differences in the types of mobility (e.g., Fuller 2007; Keith and McWilliams 1997, 1999). However, other studies found positive returns to voluntary job changes among men, but not among women (e.g., Bret and Stroh 1997; Dreher and Cox 2000; Ruhm 1987). Possible mechanisms suggested by these studies for the gender difference include the presence of gender biases in recruiting processes, women's family responsibilities that restrict their job choices by requiring them to consider non-pecuniary factors, such as geographical proximity to home and availability of "family friendly" work arrangements ("compensating differentials"), and women's limited social connections to the most lucrative positions.

While the uneven empirical findings may be in part due to different sample characteristics (e.g., age or occupation), or using different measures of mobility (long-term versus short-term), they may also be due to a differentiated effect between mothers and non-mothers, an aspect not considered in these prior studies. Given that most explanations for the gender differences in mobility outcomes point to women's family responsibilities, this omission is particularly curious. If family responsibilities are the major restriction in women's job search process, the adverse effect of mobility, if any, would be larger for mothers than for non-mothers.

Prior research provides several more reasons to expect a motherhood penalty in the reemployment process: even when job changes are unrelated to family responsibilities, employers may associate mothers with their caregiving roles and make biased assessments of their competence (Ridgeway 2011; Correll et al. 2007); referrals may make explicit or implicit assumptions about what jobs are suited for mothers, limiting mothers' access to job opportunities; and initiating salary negotiations may be less effective for mothers, who are subject to a stricter norm of female "niceness" (Benard and Correll 2010; Bowles et al. 2007).

In summary, I expect that, in line with previous studies, employer changes for the sake of improving one's job situation lead to earnings growth, whereas losing a job for economic reasons, such as layoffs or displacement, or quitting

a job for non-work-related reasons are associated with earnings losses for both men and women upon reemployment. However, within a single type of mobility, I expect that earnings growth from job mobility is smaller for mothers. Below, I discuss how the decreased employment opportunities of the Great Recession may moderate these relationships, which will shed light on the conditions under which these penalties are attenuated or magnified.

Job Mobility in the Great Recession

The most recent recession, which officially began in December 2007 and ended in June 2009, affected many workers and their families more greatly and more enduringly than any previous postwar recession. The number of displaced workers doubled to 15.4 million, from 8.3 million during the pre-recession period of 2005 to 2007, and the number of job openings decreased by 44 percent, which sharply reduced the reemployment rate and exacerbated earnings losses for those who lost a job during this period (Bureau of Labor Statistics 2012). A recent study reports that 36 percent of displaced workers who held their prior jobs longer than three years lost 20 percent or more of their earnings during the recession (Borbely 2011). Similarly, limited job opportunities during the recession may also have exacerbated earnings losses for those who quit jobs for external, often unavoidable reasons, such as illness or caregiving responsibilities.

In contrast, the earnings of those who change employers for job-related reasons (e.g., searching for a better job opportunity) may not have been adversely affected by the recession because of a strong self-selection in which only employees who anticipate improvement change jobs. This positive selectivity may in fact be stronger during the recession, given its more limited employment opportunities. Available jobs during the recession were also disproportionately high-skilled and high-paying positions (Bureau of Labor Statistics 2012), which could inflate the observed earnings gains of these job changers.

While it is not difficult to imagine that the recession may have affected mobility processes, it is unclear whether and how the impact may dif-

fer for men and women. Did the recession affect men's mobility processes more negatively than women's, as some observers have suggested, or did the recession's limited employment opportunities more negatively affect women and mothers, traditionally disadvantaged groups? The answers to these questions, from theory and empirical research, are unclear. First, changes in the distribution of availability of jobs across female- and male-dominated occupations provide divided predictions. On the one hand, the job loss rate was higher among male-dominated occupations, such as manufacturing and construction, accounting for 46 percent of the total displacement among long-tenured workers (Borbely 2011). On the other hand, the job loss rates among women increased sharply towards the end of the recession. Moreover, the recovery of jobs in female-dominated industries was slower than in male-dominated industries, and within the same industry, the rate of job loss was in fact higher for women than for men (Hartmann, Fischer, and Logan 2012).

Theories of discrimination or gender bias also do not provide clear answer to these questions. While traditional economic models of discrimination expect that the stronger market pressure during a recession would drive out employer discrimination (e.g., Becker 1971), many scholars have cast doubt on this logic (e.g., Blau, Brinton, and Grusky 2006; England 1994; Reskin and Roos 1990). For example, if all employers actually believe that women are less productive than other workers, enhanced market competition would not eliminate discrimination even in a fiercely competitive market. On the contrary, if employers are willing to hire women only when the demand for labor is high enough to exhaust the availability of men in the labor supply, scant employment opportunities would decrease women's reemployment chances. Similarly mothers, who are most likely to be at the bottom of the employer's "labor queue," given the prevailing perception that mothers are the least competent and committed among all groups of workers, would be least likely to be reemployed in the recession, or may be reemployed only at the lowest wage (Correll et al. 2007; Reskin and Roos 1990). Also, if mothers' family responsibilities continue to limit their job choices in the recession, these restrictions may affect their earnings more negatively in the reces-

sion than in the pre-recession, given more limited employment opportunities.

In summary, I expect that the increased number of layoffs and the decreased number of job openings during the Great Recession reduced the chance of reemployment, leading to greater earnings losses for those who lost jobs as well as for those who left jobs for non-work-related reasons. However, because of the strong tendency for those who quit jobs for better job opportunities to leave the current employer only when they anticipate improvement, I do not expect the recession to negatively affect these quitters. It is unclear from theory and empirical evidence whether and how the economic recession moderates gender-specific or motherhood-specific mobility outcomes. I seek to provide some empirical ground for answering this underappreciated question using up-to-date data that coincide with the pre-recession and recession periods, which I describe below.

Data

My analyses draw on data from the 2004 and 2008 panels of the Survey of Income and Program Participation (SIPP; U.S. Census Bureau / National Bureau of Economic Research), a national longitudinal household survey conducted by the U.S. Census Bureau. The SIPP's sample size is larger than that of any other labor market panel data, which is critical in accurately estimating a rare labor market event like job mobility. The SIPP respondents were interviewed every four months over 48- (2004 panel) or 52-month (2008 panel) periods (each dataset is called a wave). I use the 2004 panel, which covers the years 2004 through 2007, to examine the mobility effect in the non-recession years and the 2008 panel, which covers the years 2008 through 2012, for the recession data. Although the recession officially ended in 2009, the recovery of the labor market was slow even after that, and the unemployment rate remained largely at above eight percent, the highest level since the 1990s.

The span of each panel allows a comparison of the mobility effect in the pre-recession years with that in the recession years. The panel structure enables me to allow a time lag between mobility and earnings changes. Because the survey was conducted every four months, I use a four-month

lag between the mobility variables and earnings and control variables. Restricting the sample to respondents between the ages of 18 and 64 who are not contingent workers (contingent workers' earnings are not surveyed) and who report positive earnings yields an unweighted sample size of 299,072 person-months for the 2004 panel, and 336,705 person-months for the 2008 panel. All data are weighted by the final weights provided by the Census Bureau in order to produce coefficients that reflect the national target population.

Variables

The dependent variable is the natural logarithm of the month's gross earnings before any deductions. Earnings are adjusted to 2004 dollars using the Bureau of Economic Analysis personal consumption expenditures deflator. The key independent variables measure whether respondents change employers between two adjacent times (four-month lag). They are based on two survey items: "Is...still working for this employer?" and "What is the main reason...stopped working for...?" I construct three variables to measure three conventional types of job mobility based on the reasons for employer change: (1) job-related reasons ("job-related quit"); (2) layoffs or displacement ("layoff"); and (3) other, non-work-related reasons ("other quits"). The job-related quitters include those who "quit to take another job" or quit because of "unsatisfactory work arrangements (e.g., pay, hours)." The last residual category includes reasons related to schooling, retirement, illness, childcare, and other family/personal reasons. To measure the discrete earnings growth following job changes, I use the mobility variable measured four months prior to the time when earnings are measured. The reference category indicates staying with the same employer. As shown in Table 1, the aggregate rate of employer changes is four percent for both men and women in the pre-recession data. Men and women, however, are unevenly distributed across different types of mobility: men show a higher rate in layoffs (1.3 percent vs. 1 percent for women), women are represented more in the "other quits" category (1.3 percent vs. 1 percent for men), and men and women show a similar rate in job-related quits (1.7 percent for men and

Table 1: Means and standard deviations (in parentheses) for the variables used in the analysis.

	2004 Panel				2008 Panel			
	Men		Women		Men		Women	
Monthly earnings (log)	7.895	(0.838)	7.530	(0.853)	7.844	(0.894)	7.532	(0.887)
Monthly earnings (2004 US\$)	3,716	(3,950)	2,534	(2,279)	3,615	(3,547)	2,608	(2,410)
Job mobility:								
Job-related quit	0.017		0.016		0.009		0.009	
Layoff	0.013		0.010		0.016		0.010	
Other quit	0.010		0.013		0.007		0.009	
Child under 6	0.187		0.166		0.177		0.158	
Child 6-18	0.209		0.251		0.201		0.232	
Married	0.597		0.554		0.587		0.542	
Age	39.627	(11.919)	40.183	(12.125)	40.478	(12.203)	41.070	(12.500)
Education:								
High school graduate	0.294		0.258		0.259		0.218	
Some college	0.351		0.390		0.341		0.382	
College graduate	0.182		0.205		0.205		0.225	
Advanced degree	0.095		0.103		0.107		0.121	
Weekly work hours	41.462	(9.989)	36.655	(10.265)	40.247	(10.352)	36.178	(10.457)
Job tenure	7.663	(8.419)	7.024	(7.742)	8.095	(8.555)	7.649	(8.062)
Public sector	0.136		0.203		0.149		0.208	
Metropolitan	0.837		0.835		0.849		0.846	
Region								
Midwest	0.233		0.239		0.236		0.243	
South	0.357		0.362		0.359		0.373	
West	0.226		0.208		0.221		0.200	
Union	0.155		0.141		0.154		0.133	
Part-time	0.196		0.330		0.224		0.338	
Family income (log)	3.665	(7.303)	5.084	(6.387)	3.376	(7.517)	4.712	(6.718)
Occupations:								
Professional	0.169		0.267		0.192		0.283	
Service	0.134		0.186		0.149		0.203	
Sales, clerical	0.170		0.345		0.173		0.321	
Farming	0.010		0.004		0.011		0.003	
Production	0.278		0.053		0.243		0.041	
Operative	0.106		0.020		0.098		0.020	
N	150,944		148,128		167,862		168,843	

1.6 percent for women). Not surprisingly, in the recession, the rate of job-related quits and other quits drops to under 1 percent, whereas the rate of layoffs increases (for men: 1.6 percent) or remains at the pre-recession level (for women: 1 percent).

To investigate the motherhood wage penalty in the job mobility process, I use a set of dummy variables indicating whether the respondent lives with his or her own child and allow these variables

to interact with the mobility variables. Given that mothers of younger children are typically considered to have more intense caregiving responsibilities, I differentiate the presence of children based on the youngest child's age: under the age of six (preschool-age) and between the ages of six and 18 (school-age). Because the effects of job mobility and covariates are estimated using person-level fixed effects models (see below), time-invariant covariates (e.g., race) are

omitted, but the models include time-varying covariates, such as age, age squared, marital status, education (five categories), family income excluding the respondent's earnings (logged), years of job tenure, usual hours worked per week at a main job, whether the job is a part-time position, whether the worker is a union member, whether the job is in the public sector, metropolitan residency, region (four categories), occupation (seven categories), and time (i.e., waves: 12 categories for the 2004 panel and 13 categories for the 2008 panel). While the models adjust for standard covariates for wage regression, I recognize the omission of work experience, a well known correlate with wages. In SIPP data, the work experience question is asked only once at the beginning of the panel, and because the effect of time-invariant variables cannot be estimated in fixed effects regressions, the work experience variable is omitted. However, the models adjust for the effect of job tenure. Also, a supplementary analysis in which I fit the interaction effect between a linear time measure and work experience shows that the inclusion of the interaction effect rarely changes the coefficients and standard errors of the mobility variables (see Tables S1 and S2 in the supplementary appendix).

Methods

I estimate the effects of all variables, including the three types of job mobility, on the log of monthly earnings using fixed effects regression models. The models take the general form

$$y_{ij} = x_{ij}\beta + \alpha + \varepsilon_{ij} \quad (1)$$

where y is the natural logarithm of monthly earnings for individual i at time j , x is a row vector of time-varying covariates, and β is a column vector of regression coefficients. Residuals are composed of two parts: α_i represents unobserved stable characteristics of person i , and ε_{ij} is a random disturbance term, which is assumed to be normally distributed with a mean of zero and constant variance. I fit all models separately for men and women and for each panel to estimate gender- and panel-specific effects for all covariates.

Prior research suggests that worker heterogeneity may introduce bias in estimating the job mobility effect on earnings (e.g., Bartel and Borjas 1981; Gibbons and Katz 1991; also see the

discussion above). Including person-level fixed effects adjusts for all stable individual characteristics, such as time-invariant aspects of intelligence, preferences, and work habits. After adjusting for the effects of all stable characteristics, the residual variance is attributed to longitudinal change within individuals. This implies that the mobility effect is estimated by comparing times with mobility to times without mobility for each person, rather than comparing workers who stayed with the same employer with workers who changed employers. Because the mobility effect is estimated within the same person, the extent to which worker heterogeneity introduces bias is substantially reduced.

The analytic sample includes only those who are reemployed within the four-month window, and the current modeling strategy estimates the mobility effect by comparing the earnings changes observed between the two adjacent times. This analytic strategy may understate the "true" economic costs associated with job changes, because those who are reemployed within four months after displacement are a relatively fortunate group compared with those who remain unemployed in the longer term. Furthermore, the reemployment rate is lower during the recession (44 percent) than during the pre-recession period (55 percent), and this may undermine the comparability of the estimated effects of mobility between these two periods due to the selectivity associated with reemployment. That is, given the limited reemployment opportunities during the recession, those who were reemployed during the recession may be more productive employees than the average employee reemployed during the pre-recession period. If so, earnings losses estimated from the sample that includes only those who are reemployed would have a larger downward bias during the recession compared to the pre-recession. This selection bias may also affect conclusions about the changes in gender differences in the mobility effects between the recession and the pre-recession because men's reemployment rate dropped more (from 60 to 47 percent) during the recession than women's (from 50 to 42 percent). Alternatively, if the recession decreased workers' reemployment opportunities indiscriminately, the differing reemployment rates would not undermine the comparability of the estimates between the two periods.

Table 2: Person-level fixed effects regression coefficients for the effect of job mobility (4 months prior) on the log of monthly earnings, SIPP 2004

	Men 1a	Women 1b	Men 2a	Women 2b
Intercept	7.827* (0.004)	7.567* (0.006)	7.827* (0.004)	7.567* (0.006)
Job-related quit	0.023 (0.015)	0.045* (0.014)	0.032 (0.021)	0.070* (0.019)
× child under 6			-0.001 (0.030)	-0.095* (0.035)
× child 6-18			-0.057 (0.046)	-0.027 (0.031)
Layoff	-0.040* (0.016)	-0.060* (0.020)	-0.023 (0.022)	-0.064* (0.031)
× child under 6			-0.044 (0.036)	-0.011 (0.049)
× child 6-18			-0.051 (0.039)	0.025 (0.044)
Other quit	-0.063* (0.025)	-0.082* (0.020)	-0.059* (0.026)	-0.101* (0.028)
× child under 6			0.015 (0.084)	0.025 (0.049)
× child 6-18			-0.058 (0.094)	0.077 (0.043)
Child under 6	0.011 (0.009)	-0.042* (0.010)	0.011 (0.009)	-0.041* (0.010)
Child 6-18	-0.007 (0.009)	-0.010 (0.008)	-0.006 (0.009)	-0.011 (0.008)
Covariates	Included			
Observations	150,944	148,128	150,944	148,128
R ²	0.86	0.85	0.86	0.85

Notes: Robust standard errors in parentheses. All covariates are centered at their means. The full results are available in table S7 in the supplementary appendix.

* $p < 0.05$ (two-tailed).

To assess whether the different rates of reemployment between the pre-recession and recession periods influences the estimates of the mobility effects, I analyze those who are reemployed but weight the models with the inverse of the probability of inclusion in the sample.¹ With this weight adjustment, the models reflect the characteristics of all workers who experience job mobility, instead of those who change jobs *and* are reemployed within four months. In other words, this

¹The sampling weights provided by the Census Bureau are also incorporated, multiplied by this inverse-probability weight.

procedure estimates the mobility effect under the assumption that those who remain unemployed after a job separation are instead reemployed at wages equivalent to the wages of those who were reemployed but otherwise very similar. The probability of reemployment is calculated using a logistic regression model with predictor variables for age, age squared, race, parental status, marital status, educational attainment, mobility types, weekly work hours, years of job tenure, years of job tenure squared, public sector, regions, union membership, metropolitan residency, part-time status, family income, family income squared,

poverty status, and major occupation groups (see table S4 in the supplementary appendix). After the weight variables are constructed, I examine the standardized bias to check whether the reweighting procedure successfully balanced the sample characteristics between those who are reemployed and those who remain unemployed (see Morgan and Todd 2008; Rosenbaum and Rubin 1985). The results of the balance test show a substantial decrease in differences of the sample characteristics between these two groups of workers after the reweighting procedure has been applied (see tables S5 and S6 in the supplementary appendix). Addressing the selectivity associated with the reemployment process renders the mobility effects from the 2004 and 2008 panels more comparable. The details of this procedure and the results of the balance test are provided in the supplementary appendix.

Results

I present three sets of analyses. The first analysis estimates job mobility effects during the pre-recession period using the 2004 panel data. The second analysis examines the same job mobility effects during the recession using the 2008 panel data. The first two sets of results allow a simple comparison of the mobility effect in the economic recession with that of pre-recession years using the observational data. The final part of the analysis presents the models that are weighted for reemployment by performing the reweighting procedure described above. This allows me to assess whether a non-random process of reemployment may influence the differences between the estimates in the pre-recession and recession data. As I will show in this last analysis, the weighted results do not substantially differ from the results based on the original SIPP data, and so the discussion of the results is largely devoted to the first two sets of analyses.

The effect of job mobility on men's and women's earnings in pre-recession years

Table 2 presents fixed effects regression models predicting the log of monthly earnings fit to the

2004 panel data. The first two models estimate the effects of the three types of job mobility and covariates (see the list in Table 1), and the last two models add the effects of the interactions between job mobility and the parental status variables. Because all variables except the mobility and child variables are centered at their mean values, the intercept represents the log of earnings of nonparents when they stay with the same employer. The coefficients and standard errors for covariates are not shown here but are available in Table S7 in the supplementary appendix.

The estimated effects of the mobility variables show that for both men and women, employer changes for job-related reasons result in earnings growth, whereas employer changes resulting from layoffs or other personal or family-related reasons lead to earnings losses. These effects, however, also reveal interesting gender-specific patterns. At first glance, it appears that job-related quits are more beneficial for women than for men. Model 1 in Table 2 shows that earnings growth is about twice as large for women (4.6 percent; $e^{0.045} = 1.046$) as for men (2.3 percent) among job-related quitters. However, models that allow interaction effects between the job mobility and parental status variables reveal that this pattern is only true among childless women. The main effect of job-related quits in model 2b shows that childless women experience about seven percent earnings growth after job-related quitting. However, the negative interaction effect between job-related quitting and parental status shows that this wage premium is reduced substantially, by nine percentage points, for mothers with children under six. Considering the main effect and the interaction effect together, these mothers experience earnings losses of two percent compared to when they stay with the same employer.

This wage penalty is not observed for fathers of children under six. The coefficient of the equivalent interaction effect is virtually zero, and the standard errors are large. The valence of the interaction effect between job-related quitting and parental status is also negative for fathers and mothers of children between six and 18, meaning that having school-age children reduces the benefit of job-related quitting. However, the magnitude of the effect is substantially smaller (-0.057 for fathers of children under six and -0.027 for mothers of children between six and 18), com-

pared to those for mothers of children under six (-0.095) and the estimates are noisy.

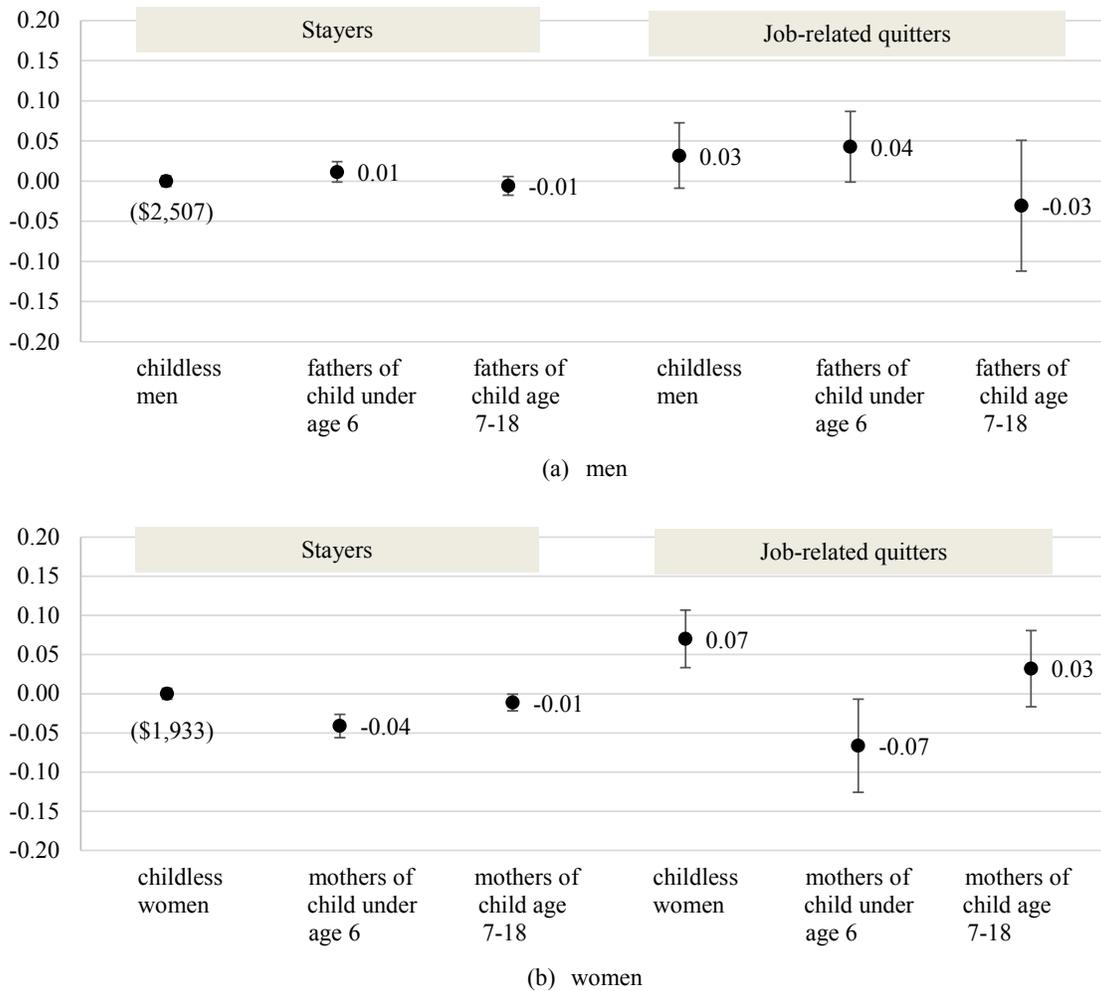
To make the comparison across parental status easier, Figure 1 graphically presents the adjusted logged monthly earnings of each group compared to those of childless stayers, based on the coefficients of job-related quits, parental status, and their interaction effects in models 2a and 2b of Table 2. The left side shows the earnings differentials for the stayer groups, and the right side shows the job-related quitter groups, all compared to the logged earnings of childless stayers (marked as 0).

Figure 1b shows that among stayers, mothers of children under six earn four percent less (i.e., $e^{-0.04} = 0.96$; see also the main effect of “child under 6” in Table 2), and mothers of children between six and 18 earn one percent less than their childless counterparts, the well known “motherhood wage penalty” documented by prior research (e.g., Budig and Hodges 2010; Glauber 2007). This is in contrast to fathers of children under six staying with the same employers, who earn one percent *more* than their childless counterparts (see Figure 1a). More important for the purpose of this study, Figure 1b shows that this motherhood wage penalty is exacerbated by job-related mobility. While childless women’s earnings increase by seven percent when changing employers for job-related reasons, comparable mothers either do not gain (for mothers of children under six) or experience a smaller gain (for mothers of children between six and 18). Consequently, the earnings gap between non-mothers and mothers widens after job-related quitting. Specifically, the earnings gap between childless women and women with children under six is 0.14 ($0.07 - [-0.07]$) logged dollars among job-changers, more than tripling the size of the gap shown among stayers (0.04). This suggests that the motherhood wage penalty is exacerbated by inter-organizational mobility. The wage gap between childless women and mothers of children between six and 18 also appears larger among job-related quitters (0.04) than among stayers (0.01), but the estimated relative earnings of mothers of children between six and 18 quitting their jobs are noisy, showing a large overlap in the 95 percent confidence intervals with those of earnings for childless women who quit jobs.

While I expected the presence of a motherhood wage penalty, its magnitude is rather surprising. The motherhood wage penalty is so large for mothers with children under six that these mothers do not gain, or may even lose slightly, by changing their jobs (compare “mothers of child under age 6” under stayers versus job-related quitters in Figure 1b). This is surprising given a strong tendency for only those who anticipate earnings gains to select into mobility. Why, then, does inter-organizational mobility exacerbate the motherhood wage penalty so much?

The effect of job mobility is estimated by fixed effects regression models, which account for all stable worker characteristics, in addition to a wide range of time-varying human capital and job-related characteristics included in the model (see the list in Table 1). This reduces the concern that this magnified motherhood wage penalty in the mobility process merely reflects mothers’ negative selection into job-related quitting (i.e., less productive mothers are disproportionately likely to enter the decision of job-related quitting). The current models, however, leave open the possibility that this magnified motherhood wage penalty is due to changes in the types of jobs that mothers and non-mothers hold through job-related quitting. Although those who reported changing employers because of a “childcare problem” or for “family/personal reasons” are assigned to a separate category (“other quits”), it is still possible that decisions based on ostensibly job-related reasons are shaped by family responsibilities, such as taking lower paying jobs for the sake of non-pecuniary benefits, such as flexibility in hours or locations or fewer business trips (see e.g., Budig and Hodges 2010). If so, this the changes to the types of jobs involved in the mobility process may explain why the motherhood wage penalty increases so much.

I investigate this possibility in a supplementary analysis by including the fixed effects of the full set of occupations (based on the 2000 Census occupation classification) at the most disaggregated level to the models, in addition to other covariates (see Table S3 in the supplementary appendix). If mothers benefit less from job-related mobility because they move to occupations that are typically lower paying but offer other “family friendly” benefits, the wage differences generated through mobility should be substantially reduced



Notes: Estimates are based on Model 2b of Table 2. The standard errors for the combined coefficients are calculated by the delta method. Error bars indicate 95% confidence intervals.

Figure 1: Differences in logged monthly earnings between childless stayers and other workers in the pre-recession data.

after adjusting for the detailed occupation effects. The results show that after accounting for occupation effects, the estimate for the interaction effect between job-related mobility and being a mother of children under six does decrease modestly from -0.095 to -0.088 (a seven percent decrease, i.e., $[0.095 - 0.088]/0.095 = 0.07$), but does not entirely disappear. Put differently, an uneven distribution of mothers and non-mothers among high- and low-paying occupations explains about seven percent of the motherhood penalty

in the mobility process, and the remaining 93 percent is due to other unmeasured characteristics, such as unobserved time-varying worker characteristics, such as job- or organization-level segregation, and discrimination in the hiring process.

Next, I turn to layoffs and other quits. While we see in Table 1 that the rates of layoffs are higher among men (13 percent) than among women (10 percent), Table 2 shows that once laid off, women experience greater earnings losses than

men: women's earnings decrease by six percent, whereas comparable men experience a four percent loss (see models 1a and 1b). The effect of layoffs is not substantively differentiated by motherhood status, as indicated by the noisy estimates of layoff-motherhood interaction effects (see model 2b). One possible reason for a more prominent gender-based, rather than motherhood-based, difference is because layoffs or worker displacements occur at higher rates in male-dominated occupations, and because reemployment in these male-dominated occupations may be more difficult for all women, regardless of parental status (because of other mechanisms such as occupational sex segregation). Thus, once women are laid off, they experience prolonged unemployment or can only find jobs in different industries or occupations, which has been associated with greater earnings losses (e.g., Cha and Morgan 2010; Neal 1995).

"Other quits" also result in greater earnings losses for women (eight percent) than for men (six percent). It is difficult to compare the effects of this type of mobility directly, however, given the different distribution of men and women across different sub-categories: women report substantively more childcare problems and injuries as reasons in this category, whereas men report more schooling and unspecified "some other reasons."

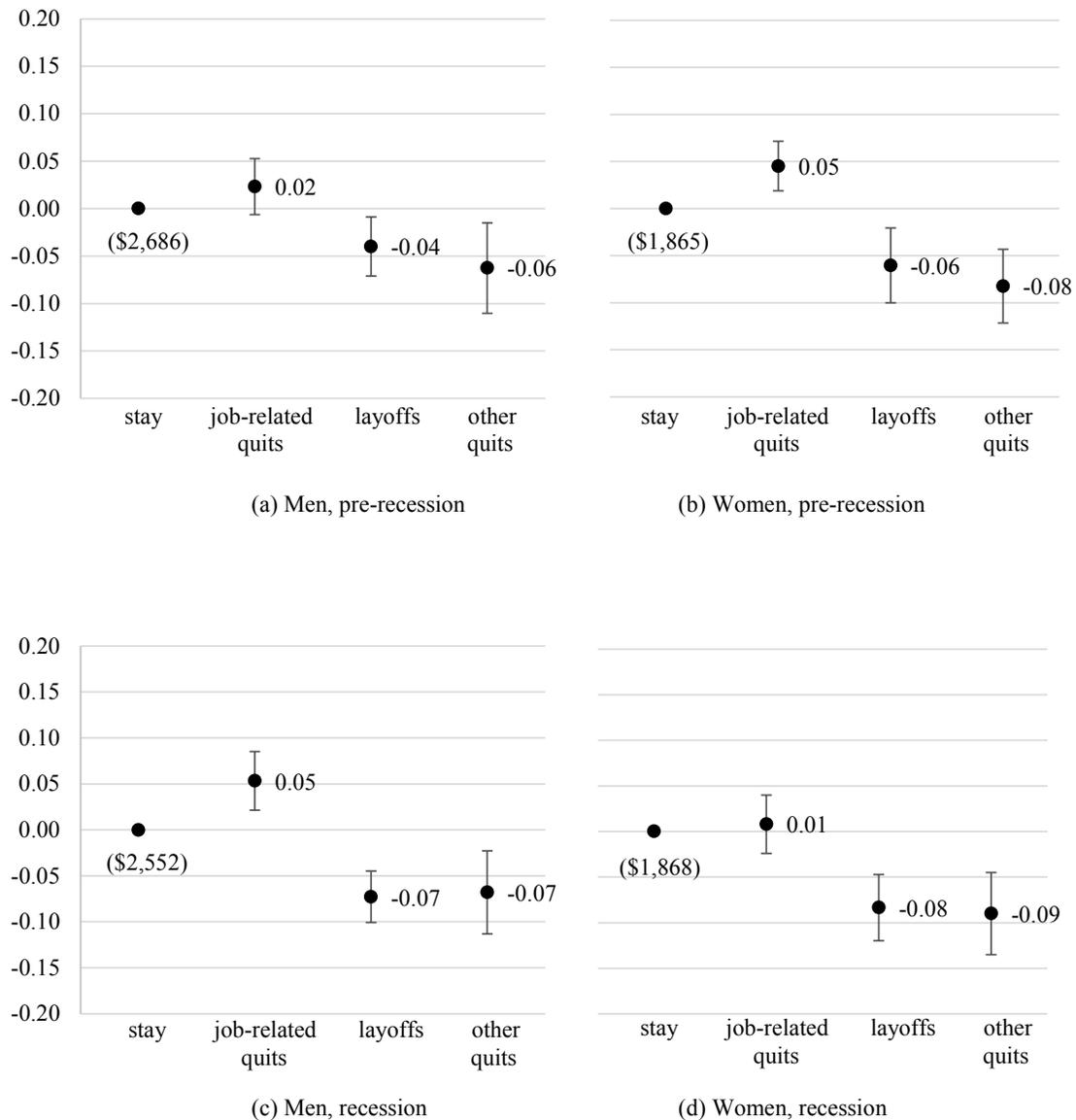
The effect of job mobility in the Great Recession

Next, I examine the effect of job mobility on earnings during the recession years. Figure 2 presents the fixed effects regression coefficients for three types of job mobility for men and women based on the 2008 panel data (Figures 2c and 2d), with the equivalent results based on the 2004 data (Figures 2a and 2b; also see Table 2) for comparison. As in the pre-recession years, job-related quitters experience wage growth, but those who are laid off or quit for other reasons experience wage losses in their next job. However, the magnitude of the mobility effect changes unevenly by gender and mobility type.

The first notable change is that men's baseline monthly earnings (earnings for stayers) decreased under the recession by five percent, from \$2,686 to \$2,552, whereas women's earnings remained similar, consistent with the common perception that the recession affected men more negatively.

Another notable change, however, is the reversal of the gender difference in the wage premium for job-related quits. Unlike the pre-recession data that show larger wage returns to job-related quitting for women, during the recession men's wage returns are larger. Specifically, men who change employers for job-related reasons experience a five percent increase in their monthly earnings, which is greater than the two percent wage premium shown in the pre-recession data (compare Figures 1a and 1c). In contrast, the wage premium for job-related quits shrunk for women from five percent to one percent (see Figures 2b and 2d). As I will show below, this decrease is due to a sharp decline in wage returns to job-related quits for childless women, who showed the largest wage growth among all groups in the pre-recession years.

As expected, wage losses for those who are laid off are larger than in the pre-recession years. Changing jobs after being laid off or displaced from a prior job leads to a seven percent decrease for men and an eight percent decrease for women during the recession (see Figures 2c and 2d). These are substantial increases from the four percent loss for men and six percent for women in the pre-recession (see Figures 2a and 2b). While earnings losses from layoffs remain larger for women than for men, men's earnings losses increased more, by 75 percent. Women showed a 33 percent increase in wage losses compared to the pre-recession. Interestingly, though not shown here, the models that fit interaction effects between parental status and being laid off show that the interaction effect between layoff and having a child under six is positive for men ($b=0.093$, $se = 0.033$), meaning that having a preschool child significantly reduces the wage losses of laid-off men (see Table S8 in the supplementary appendix). An additional analysis (not shown) finds that this "fatherhood bonus" is driven by the second half of the post-recession years (2011-2012), in which men benefitted from faster job creation in male-dominated occupations than in other occupations. Lastly, earnings losses resulting from "other quits" during the recession (seven percent for men and nine percent for women) are similar to those shown in the pre-recession data (six percent for men and eight percent for women).



Notes: Estimates are based on Models 1a and 1b of Table 2 (see also table S7 in the supplementary appendix), and Models 1a and 1b of Table S8 in the supplementary appendix. Error bars indicate 95% confidence intervals.

Figure 2: Differences in logged monthly earnings between childless stayers and job changers.

Next, I examine whether the recession changed the large motherhood wage penalty among job-related quitters shown in the pre-recession data (see Figure 3). As in Figure 1, I plot the differences in adjusted logged earnings of each group of men (Figure 3a) and women (Figure 3b) from the adjusted logged earnings of their childless counterparts (baseline group), based on the 2008

panel data (see the full models in Table S8 in the supplementary appendix). The results show that, in contrast to the pre-recession data, the motherhood wage penalty (calculated by relative hourly wages of mothers to those of childless women) among job-related quitters largely disappears, or at least decreases (compare Figures 1b and 3b). However, a closer examination

reveals that this is not because mothers experience a smaller wage penalty but because childless women benefit less from job-related quitting in the recession. As shown in Figures 1b and 3b, the wage returns to job-related quitting for mothers of children under six are similar between pre-recession and recession years (a three percentage point wage decrease after quitting in both panels; i.e., $e^{-0.03 - (-0.06)} = 1.03$). In contrast, the mobility wage premium for childless women decreased substantially to two percent during the recession, from seven percent in the pre-recession (compare Figures 3b to 1b). These results suggest that the economic recession may have not worsened the wage returns to job-related quitting for the mothers, but it did substantially bring down the wage premium that childless women showed during the pre-recession years.

This reduced wage premium for childless women is one source of the reversal of the gender difference in the wage returns to job-related quitting during the recession period. The other important source is the increased wage premium for childless men. Figure 3a shows that the wage premium for job-related quitting for childless men more than doubled in size compared to the pre-recession years (from 0.03 to 0.08 logged earnings; compare to Figure 1a). The diminished wage premium for mothers of children between six and 18 may also have contributed to reversing the trend by pulling down women's overall wage returns to job-related quitting. In the recession, the relative wages of mothers of children between six and 18 no longer show wage gains from changing jobs (-0.01 for stayers vs. -0.02 for job quitters), whereas prior to the recession they had a gain of 0.04 logged dollars, or four percent (-0.01 for stayers versus 0.03 for job quitters).

Interestingly, the recession data show a fatherhood wage penalty among job-related quitters. Figure 3a shows a 12 percent wage penalty ($e^{-0.05 - 0.08} = 0.88$) for fathers of children between six and 18 among job-related quitters, twice as large as the penalty that was shown in the pre-recession. However, the major source of this "fatherhood wage penalty" is not the decline of fathers' wage returns to job-related quits; instead, it is childless men's surprisingly high wage returns to job-related quitting. Because fathers' wages did not increase enough to keep up with the sharp increase in the wage premium for child-

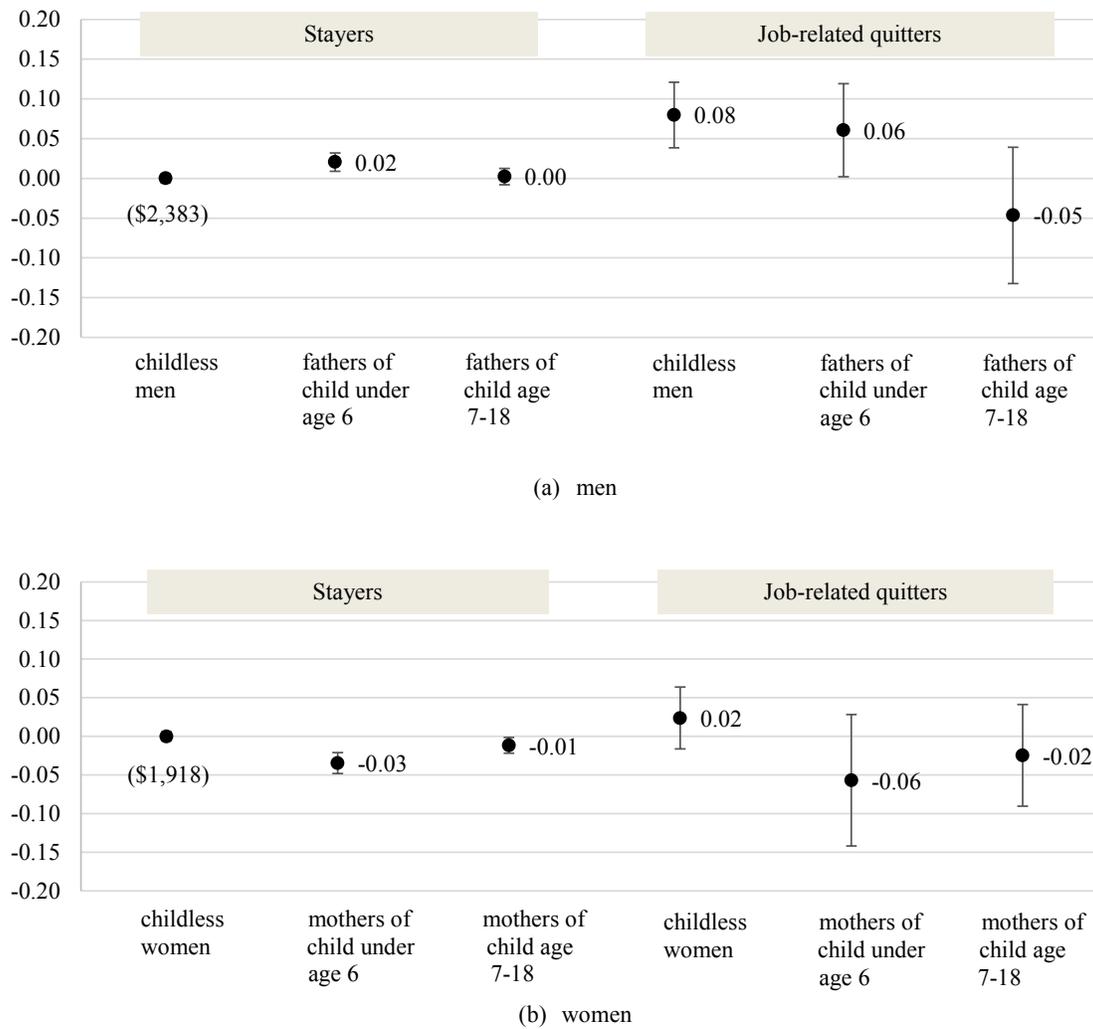
less men, the gap between fathers and childless men widened (compare Figures 1a and 3a).

While childless men's large wage returns to job-related quitting are surprising, it also bears noting that the baseline earnings (the earnings of childless stayer men) are lower by five percent during the recession compared to the pre-recession, and thus, the higher wage premium for job-related quitting among childless men does not mean that the economic recession benefitted childless men in an absolute sense. They are just relatively better off than other men, whose wages decreased much more during the recession.

In summary, the economic recession brought about several notable changes in the job mobility effects. First, it eliminated the large wage gains from job-related quitting that childless women enjoyed during the pre-recession years, reversing the gender difference in the wage premium associated with job-related quitting. Second, the recession flattened out the wage differences between mothers and non-mothers among job-related quitters, not by improving mothers' wages, but by pulling down childless women's wages. In contrast, the recession increased the wage differences between fathers and non-fathers who changed jobs for job-related reasons by pulling up the childless men's wage returns relative to the fathers'. This introduces a fatherhood wage penalty for fathers with children between six and 18 among job-related quitters. Finally, women still experience larger wage losses for layoffs under the recession, but men's earnings losses increased to a greater extent than those of women.

Selectivity associated with reemployment

So far, I have directly compared the results for the 2004 and 2008 data to discuss the effect of the economic recession on earnings. However, one may argue that the differences stem from different rates of reemployment during the recession and pre-recession. The sample includes only job-to-job transitions, meaning that those who are not reemployed immediately are not included in the analytic sample. As discussed earlier, reemployment rates are different for men and women, and also for the pre-recession and recession years. If the different rates of reemployment mean that equally productive women are



Notes: Estimates are based on Models 2a and 2b of Table S8. The standard errors for the combined coefficients are calculated by the delta method. Error bars indicate 95% confidence intervals.

Figure 3: Differences in logged monthly earnings between childless stayers and other workers in the recession data.

less likely than men to be reemployed, differences between men and women are likely to be greater if all women and men of equal productivity were reemployed with equal probabilities, because the gender difference in the observed data is based on a comparison between men and more productive women (based on observed characteristics). By the same logic, if equally productive workers are less likely to be reemployed during the recession than during non-recession years, the differences between the pre-recession and recession data may

not be due to economic conditions but instead to the heterogeneity of the workers included in the pre-recession and recession samples.

Because no data for actual productivity exist, I match as closely as possible the observed individual characteristics of those who are reemployed and those who remain jobless, by weighting the sample by the inverse probability of being reemployed (see the methods section and the supplementary appendix). In the weighted data, the mobility effect is estimated based on the coun-

terfactual assumption that those who are not reemployed were reemployed at the same wage level as those who were reemployed and have characteristics similar to theirs. The weights give more importance to younger, short-tenured, less educated, and full-time workers.

The upshot is that the results from the weighted data are substantively similar to those from the initial models, suggesting that selectivity due to the reemployment processes need not be a major concern. Nonetheless, comparing the reweighted results with those from the standard fixed effects models indicate several interesting patterns that help us understand the selection process associated with reemployment (see Table 3).² First, for both men and women, better qualified workers tend to delay their reemployment when the quit is initiated for job-related reasons, which suppresses the wage returns to mobility in the observational data. This positive selection into non-employment may reflect the fact that better skilled workers can afford to wait to be reemployed because they anticipate greater future earnings, or that those who previously held high paying jobs could afford a “break” from employment when transitioning to a new job. This positive selectivity, however, disappears in the recession data: the coefficients are similar in the weighted data and the original SIPP data.

Second, in contrast to my conjecture that negative selection (a tendency for less qualified workers to remain unemployed) would pull up the estimated wage returns to layoffs in the observational data, the estimated effects of layoffs are similar for men across the weighted and the original SIPP data, indicating no selectivity. For women, the selection pressure is actually contrary to expectation: women who are equally or better qualified, compared to those who are reemployed, often are not reemployed immediately. This positive selection is also detected for men during the recession, suggesting that even workers with strong labor market qualifications could not be immediately reemployed during recession years,

² I do not use asterisks to indicate the statistical significance for the estimates of the weighted models because the weights are based on propensity scores, which are estimated quantities, and no consensus has been reached in the counterfactual literature on calculating standard errors that adjust for the estimation errors generated by using the estimated propensity scores (see e.g., An 2010; Morgan and Harding 2006).

or in labor markets in which employment opportunities are extremely limited. The transition from negative to positive selection is observed for men who quit jobs for other reasons, suggesting that the economic recession rather indiscriminately determines unemployment, regardless of worker qualifications.

Conclusion

This article documents the varying effects of job mobility on workers’ wages by gender and parental status. At first glance, greater wage returns to job-related mobility for women in the pre-recession years appear to show that job mobility is beneficial for women and has the potential to close the gender pay gap, especially as we are moving to an economy in which job mobility occurs more frequently. However, a close examination reveals that this pattern is only true for childless women, who gain roughly seven percent in the pre-recession years, and that mothers of children under six do not benefit from this mobility. On the contrary, they experience two percent earnings losses. The earnings gap between mothers and non-mothers consequently becomes about three times wider after changing jobs. Building on prior studies on the “motherhood penalty” (e.g., Budig and England 2001; Correll et al. 2007), this study shows that job mobility exacerbates the motherhood wage penalty.

The finding that the greatest earnings gains occur among childless women who change employers for job-related reasons was unexpected, but not entirely surprising. Labor market qualifications and behaviors have converged or reversed among younger generations: the gender gap in college degrees is now reversed (e.g., DiPrete and Buchmann 2013), an increasing number of young women are entering high paying, male-dominated occupations (e.g., Cotter, Hermsen, and Vanneman 2004), and the commitment of childless women to work is perceived as equivalent to that of men (e.g., Correll et al. 2007). The political and legal environment that promotes antidiscrimination laws and policies and the diffusion of egalitarianism has also contributed to reducing blatant sex discrimination at work. The greater earnings gains for childless women may reflect these changes. In fact, Correll et al. (2007)

Table 3: Fixed effects regression coefficients for the effect of job mobility on the log of monthly earnings

			Weighted for unemployment	
	Men	Women	Men	Women
	—————2004 Panel—————			
Job-related quit	0.023 (0.015)	0.045* (0.014)	0.026 (0.016)	0.049 (0.013)
Layoff	-0.040* (0.016)	-0.060* (0.020)	-0.039 (0.017)	-0.054 (0.022)
Other quit	-0.063* (0.025)	-0.082* (0.020)	-0.084 (0.032)	-0.077 (0.021)
	—————2008 Panel—————			
Job-related quit	0.053* (0.016)	0.008 (0.016)	0.052 (0.017)	0.006 (0.017)
Layoff	-0.073* (0.014)	-0.084* (0.019)	-0.062 (0.016)	-0.067 (0.019)
Other quit	-0.068* (0.023)	-0.090* (0.023)	-0.041 (0.027)	-0.077 (0.025)

Notes: Robust standard errors in parentheses. For description of models and covariates, see the list in table 1. No asterisks are used for the coefficients in the weighted models (see footnote 2)

* $p < 0.05$ (two tailed).

found in their audit study that the callback rate for childless women is higher than for childless men or for fathers. The pace of these changes, however, has been uneven, and it has been especially slow in creating equality between mothers and non-mothers. The attrition rate for women in jobs sharply increases after the birth of a child (e.g., Stone 2007; Xie and Shauman 2003), the gender gap in caregiving responsibilities remains large (Bianchi et al. 2012), institutional support for workers with family responsibilities continues to be limited (Williams 2010), a new cultural frame that combines egalitarianism with gender essentialist beliefs about traditional motherhood is arising (Charles and Grusky 2004; Cotter, Hermsen, and Vanneman 2011), and unconscious biases against mothers persist (Correll et al. 2007). The greater gap between mothers and non-mothers found in this study may reflect these uneven changes.

However, the wage premium for childless women is substantially reduced in the economic recession, whereas wage returns for mothers of children under six remain similar, which correspondingly diminished the motherhood wage penalty.

The wage returns to job-related quitting also decreased for mothers of children between six and 18. Put simply, the economic recession flattens out women's wages across parental status, not by eliminating the penalties experienced by mothers of children under six, but by eliminating the wage premiums enjoyed by childless women and women with older children. The sensitivity to economic conditions of the wage returns for childless job-quitter women shows a vulnerability of childless women's success in labor markets in which job opportunities are limited. This is in contrast to childless men who quit their jobs for job-related reasons, who are largely immune to economic conditions. In fact, during the recession these men experience an eight percent wage increase from changing jobs, a substantial increase over their three percent increase in the pre-recession. These childless men are far better off than their stayer counterparts and also better off than fathers with children under six who quit their jobs for job-related reasons.

These findings speak to a broader question of how resource constraints in the labor market reshape mobility outcomes. They suggest

that in the new economy, where job mobility occurs more frequently, differences between mothers and non-mothers may become a more prevalent form of stratification, while diminishing the differences between men and women. However, under extreme resource constraints, such as during a prolonged economic recession, older patterns of gender inequality reemerge, as the improvement made by childless women disappears. Women's gains after job-related quits diminished to one percent in the recession from five percent in the pre-recession, whereas comparable men experience a five percent wage gain during the recession.

Consistent with this logic, wage losses for changing jobs due to layoffs are not differentiated by motherhood status in either the pre-recession or recession years. Layoffs tend to be prevalent in declining industries or occupations, which have greater resource constraints even in non-recession years. Thus, if women in general, rather than mothers alone, fare worse under conditions of resource constraint, we would expect a wage penalty for both mothers and non-mothers alike among those who are laid off. And this is precisely what the data show. Together with the pattern found for job-related quitting, this empirical pattern helps us understand the locations or conditions in which changes in the old form of gender inequality are particularly slow, and when the changes do occur, how these changes introduce a different pattern of stratification, in which motherhood, rather than gender more broadly, is the basis of the wage gap.

Increased inter-organizational mobility is often praised as empowering to workers, especially highly skilled workers who can choose their career paths without confining themselves to firm boundaries (e.g., Arthur and Rousseau 1996; Tolbert 1996). However, the findings of this study suggest that the benefits of this new opportunity are not evenly spread across the labor market. Mothers with young children, whose career outcomes are most likely to be constrained by non-market factors, are least likely to benefit from, and may even be adversely affected by, this trend. Increased job losses are the other face of this changing economy. Job losses affect a greater number of men than women, as deindustrialization hit male-dominated industries and occupations more than other segments of the labor market. However, job losses, once they occur, result in larger earnings

losses for women than for men. These findings suggest that the opportunities and costs created by the "new economy," interacting with the distribution and availability of resources in the labor market, and traditional bases of inequality, reinforce existing forms of inequality in complex ways.

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