The Added Worker Effect in Late Career

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Abstract

Households with multiple workers can theoretically insure against the job loss of one with the increased labor supply of another. This added worker effect has been documented for the average worker, but older workers may differ from these workers in their attachment to the labor force and ability to find new jobs. I study the labor supply responses of workers over age 51 to their spouses' displacements. I find that women respond little to their husbands' displacements in the short term, while men increase their probability of employment by 10 percent when their wives are displaced. I find considerable positive effects for the men and women who report the strongest enjoyment of time spent with their spouses.

1 Introduction

Involuntary job loss has significant and persistent negative impacts on a variety of household outcomes. A large literature on layoffs and plant closings shows their long-lasting effects on household finances and family stability. When such displacements[1] strike shortly before

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[1] I define displacement here as involuntary job loss due to layoff or plant closing. Some of the literature examines any such job losses while other parts study only the effects for high-tenure workers. I follow Jacobson, LaLonde, and Sullivan (1993) in using displacement to describe any job loss due to layoff or plant closing and specifying high-tenure displacement when necessary.

retirement, households have less working time to recover from the negative effects, may be less able to self-insure against the income shock, and may decide to exit the labor force entirely.

In theory, households with two potential workers can insure against some of the negative effects of displacement by increasing the labor supply of the nondisplaced worker. If households are unwilling or unable to formally insure against idiosyncratic income risk, they may adjust their behavior in response to income shocks. Following displacements, which the literature has shown to be associated with persistent decreases in income, households may optimally increase their labor supply. If they can do so, they may be able to reduce the extent to which they draw down asset holdings or reduce consumption.

While the propensity of one spouse to increase labor supply in response to the other's job loss, or "added worker effect," has been studied since at least the 1970s and documented by Stephens (2002), older workers are a unique and important group. With fewer working years in front of them, older couples that suffer a negative income shock have less time to make up lost income. Additionally, as previous research has shown that the effects of displacement are strongest for those with long job tenure (Hamermesh 1989), long-tenured older workers may be particularly negatively affected. If they also face discrimination in the job market, it may be more difficult for them to find new employment. The study of the labor responses of older workers to their spouses' job losses also allows for the potential documentation of coordination of leisure in retirement following individual income shocks.

This paper examines the labor supply responses of the nondisplaced spouse in late-career households facing displacement. I present a simple model that suggests that the added worker effect may be smaller or more negative for older workers. I then use the Health and Retirement Study (HRS) to identify displacements for a sample of older workers between 1992 and 2008. I construct monthly employment histories for HRS respondents over this period and examine the effects of a spouse's displacement on employment in each month.

I also estimate the effects of displacement on labor force participation and earnings. I find little evidence of an added worker effect for women, but I do find that men increase their probability of employment as much as 10 percent in response to their wives' displacements. Further, I find that the workers who report the strongest enjoyment of time spent with their spouses exhibit added worker effects, while those reporting less enjoyment do not.

This paper lies at the confluence of a long literature on household responses to involuntary job loss (e.g. Chan and Stevens 2001; Stephens 2002; Charles and Stephens 2004) and a literature on joint labor supply at older ages (e.g. Blau 1998; Gustman and Steinmeier 2000; Casanova 2010). The displacement literature analyzes labor force outcomes and responses for households facing displacement, but generally abstracts from the complementarity or substitutability of leisure and focuses on the income effects of job loss. I study older households because they have relatively less time remaining in their working lives to insure against displacement and because the labor-leisure choice on the extensive margin is particularly salient for them.

The paper proceeds as follows. Section 2 describes the background and existing literature. Section 3 discusses a simple two-period model of household labor supply. Section 4 explains the empirical methodology, identification strategy, and their limitations. Section 5 describes the data. Section 6 details the results of estimation. Section 7 discusses and concludes.

2 Background

Job displacements have broad implications for household welfare and have enjoyed considerable study in the economics literature. They are often viewed as plausibly exogenous shocks to income, which makes them attractive for research on household behavior. Hamermesh (1989) reviews the available research on the nature of displacements a few years before the beginning of the sample period studied in this paper. He finds that older workers are not especially at risk for experiencing displacement, but minority workers are. He also finds that displacement is associated with large losses in individual earnings, which are most noticeable for workers with long tenure prior to the displacement.

Jacobson, LaLonde, and Sullivan (1993) examine the dynamics of post-displacement employment and earnings. Using a sample of Pennsylvania workers who were separated from their jobs in the 1980s, they identify leavers who were plausibly victims of mass-layoffs because they worked for firms that experienced large decreases in employment. The years following displacement are associated with lower incomes for these workers. Ruhm (1991) also documented this effect. The literature has further examined displacements impacts on spouse labor supply (Stephens 2002), divorce (Charles and Stephens 2004), and future employment (Stevens 1997; Chan and Stevens 2001).

Previous research on the added worker effect, as in Stephens (2002), is most relevant to this paper. Although the sample used by Stephens includes workers up to age 65, he does not specifically examine the response of older workers. Using the Panel Study of Income Dynamics (PSID) Stephens (2002) examines wives labor supply responses to their husbands displacements and finds that wives increase both their hours worked and rates of employment. He documents modest increases in these margins prior to displacement and larger increases after displacement. In doing so, Stephens estimates that the households recoup more than 25 percent of the husbands lost earnings. Stephens also finds some differences in behavior according to whether the displacement results from a plant closing or a layoff. This paper does not distinguish between the two because of sample size issues. Additionally, Stephens outlines the assumptions necessary to interpret his findings as estimates of structural parameters. This paper, in contrast, will take an entirely reduced form approach.

In a resume-based audit study, Lahey (2008) finds that employers are more than 40 percent more likely to offer interviews to young workers than to old workers. If older workers face this kind of discrimination, whether taste-based or statistical, the viability of the added worker effect as an insurance mechanism is reduced. If the discrimination also extends to spouses who are already working, they may be less able to increase their hours or work more years in response to displacement. Thus, the experience of displacement for older households may be quite different from that of younger households.

The literature on joint retirement indicates that husbands and wives have a preference for joint leisure even when controlling for financial incentives. Blau (1998) uses the Retirement History Survey to show that joint retirement is more common than would be expected without this complementarity and that nonemployment of one spouse will increase the labor force exit rate or decrease the labor force entry rate of the other. Gustman and Steinmeier (2000) and Casanova (2010) both estimate structural models of joint labor supply decisions for older households. They similarly find incidence of joint retirement beyond that which is explained by financial incentives for retirement timing. This suggests complementarities in leisure that might also be relevant when a spouse is displaced. I would also like to consider the relationship of this paper to the "unretirement" literature, which studies the reentry of retired individuals into the workforce. Unfortunately, my sample contains relatively few respondents who are retired at the time when their spouses are displaced, so analysis of them is currently infeasible.

3 Theoretical Background

Stephens (2002) describes the dynamics of the added worker effect in a life cycle model of consumption and labor-leisure choice, demonstrating that the added worker effect may appear predisplacement, and may persist permanently. I have little to add in terms of describing dynamics, so instead I present a simple two-period model to motivate why late-career households may behave differently from average households in responding to displacement. While any number of stories could justify differences in the added worker effect at older ages, the crucial assumption I make in this simple model is that work effort in the early period affects expected wages in the later period. While the model presented in this draft abstracts from uncertainty, continuing work on this paper will incorporate full dynamics and uncertainty over future wages.

A unitary household seeks to maximize utility from consumption and leisure over two periods, early career and late career. There are two workers in the household, here called the husband (h) and wife (w) for simplicity, who each have an endowment of one unit of time in each period. I abstract from discounting and interest rates, as they add little to the intuition suggested by the model. I assume that utility is intertemporally additively separable and intratemporally additively separable in consumption and leisure. Thus, the household seeks to maximize lifetime utility

$$max\,\mathcal{U} = \sum_{t=1,2} u(c_t) + v(l_t^w, l_t^h) \tag{1}$$

subject to

$$c_1 + c_2 = \sum_{i=h,w} (1 - l_1^i)w_1^i + (1 - l_2^i)(w_2^i - \alpha l_1^i). \tag{2}$$

That is, leisure in the first period has a negative effect on the return to work in the second period. This assumption is effectively based in a basic human capital story, in which work in the first period develops or maintains skills valued by the market that increase productivity in the second period. Assuming an interior solution in both workers' labor supply, we have the familiar first order conditions

$$u'(c_t) = \lambda \qquad \forall t = 1, 2 \tag{3}$$

$$\frac{\partial v(l_1^h, l_1^w)}{\partial l_1^h} = \lambda[w_1^h + (1 - l_2^h)\alpha] \tag{4}$$

$$\frac{\partial v(l_1^h, l_1^w)}{\partial l_1^w} = \lambda[w_1^w + (1 - l_2^w)\alpha] \tag{5}$$

$$\frac{\partial v(l_2^h, l_2^w)}{\partial l_2^h} = \lambda w_2^h \tag{6}$$

$$\frac{\partial v(l_2^h, l_2^w)}{\partial l_2^w} = \lambda w_2^w \tag{7}$$

where λ is the multiplier on the budget constraint, the marginal value of wealth. Given a vector of wages, these conditions along with (2) pin down the interior solution.

To analyze the effects of displacement, I follow Stephens (2002) in suggesting that displacement is equivalent to low wage draws. However, as I am less interested in describing dynamics and my two periods are meant to represent large portions of a worker's career, I consider the effect of a low wage offer in one period alone and analyze comparative statics within that period. If we assume wage offers are in a region where labor supply is not backward-bending and consider only the first-order effect of each workers' own wages on his or her own labor supply (that is, ignoring feedback to one's own leisure through spouse leisure), then $\frac{\partial l_t^i}{\partial w_t^i} < 0$. If we assume complementarity of leisure, as suggested by Gustman and Steinmeier (2000) and Casanova (2010), then decreases in a spouses' wage have the dual effects of increasing the marginal value of wealth and increasing the marginal utility of leisure.

While the net effect of these income and substitution effects is ambiguous, the change in marginal value of wealth is exacerbated in period 1 by the effect of first-period labor on second-period wages. That is, if λ and l^w both increase, a more positive adjustment in l^h is necessary to bring equation (6) back to equality as compared to (4).[2] The intuition suggested by this model is that, assuming the leisure complementarities are similar in both periods, workers will be less likely to reduce their labor supply in response to spouse displacement at

[2]This also requires assumptions about the nature of one's own wage offers—if they are always really large in period 2, then this result would not hold. Future versions of this model will include a stochastic wage process.

younger ages because of the negative effects on their future labor market opportunities.

4 Empirical Methods

4.1 Specification

I analyze the response of labor force outcomes to spouse displacement in an event history framework. I estimate linear fixed effects models of the form

$$y_{i,t} = \sum_{\tau=-k}^{l} (\beta_\tau D_{i,t-\tau}) + \beta_{l+1} D_{i,<t-l} + \alpha X_{i,t} + \delta_i + \gamma_t + \epsilon_{i,t}, \tag{8}$$

where $y_{i,t}$ is some outcome of person i in time t (e.g. employment, log earnings), $spdisp_{i,t-\tau}$ is a flag for spouse displacement in period $t-\tau$, $D_{i,<t-l}$ is an indicator for spouse displacement in any period before $t-l$, $X_{i,t}$ is a vector of time-varying controls, δ_i is an individual fixed effect, γ_t is a period control, and $\epsilon_{i,t}$ is the error.

4.2 Controls

The estimates of β_τ are estimated effects of displacement on the outcome variable at each period τ around the displacement. These estimates are generated by the average within-worker difference between y near the displacement and y more than k periods before the displacement, after accounting for controls. The fixed effect, δ_i, the period controls, γ_t, and the time-varying covariates, $X_{i,t}$, effectively construct a counterfactual for each worker who has a displaced spouse. The estimates of β_τ indicate the average difference between the counterfactual and the observed outcomes for these workers. Thus, interpretation of β_τ as estimates of the effects of spouse displacement requires that the counterfactual account for other things that could impact labor market outcomes of treated workers.

In this reduced-form analysis, it is infeasible to control for everything that varies with time and affects labor market outcomes for these workers. I therefore lean heavily on those things I can observe, including age, health, and age relative to baseline retirement expectations.[3] Other time-varying work-related incentives, such as pension and social security benefits, parental health, and labor market discrimination are not explicitly controlled. However, if these unobserved effects are only correlated with spouse displacement through the included covariates, their effects will be absorbed by the controls I do use.

Thus, the age controls in particular play a large role in proxying for unobserved changes in incentives to work. They indicate the age-profile of the average worker, including the age-correlated impacts of all uncontrolled effects. As long as those effects that are not explicitly controlled are either uncorrelated with spouse displacement or are absorbed on average into the age controls, they will not bias the estimates of β_τ away from the true causal effects of spouse displacement. That is, provided the age-profile of the average worker is, in expectation, a good counterfactual for workers with displaced spouses, the estimates of interest will be unbiased.

4.3 Propensity Score Weighting

It is reasonable to believe that the assumption outlined at the end of the last section—that the average worker provides a decent counterfactual for displaced workers—does not hold. In particular, it fails to hold if the spouses of displaced workers exhibit particular trends in employment even in the absence of displacement. Although fixed effects models control some of this heterogeneity and are common in the displaced worker literature, it is important to consider their limitations in the context of constructing a counterfactual for older workers. Fixed effects control time-invariant characteristics, allowing the estimates of interest to show the dynamics of movements around within-worker averages in response to treatment. However, if outcomes are driven by heterogeneous secular trends (e.g., transitions to retirement,

[3] I further discuss these controls in Section 5 after describing the nature of the data.

health shocks) that are temporally correlated with treatment, fixed effects may control little of the relevant heterogeneity. It is important to note that time-varying heterogeneity in this context includes changes in incentives and conditions that workers anticipate but do not affect their behavior in advance and are unobserved by the econometrician.

In theory, this is a shortcoming of using fixed effects in levels and could be overcome by allowing for some manner of individual trends. However, it seems unlikely that one could estimate a sufficiently flexible individual trend without confounding estimates of the treatment effect, particularly if early-period outcomes do not generally portend anything about late-period retirement behavior. Although fixed effects cannot control for all of the relevant heterogeneity, they are certainly robust to time-invariant heterogeneity in ways that a more restricted model is not. Therefore, I use them in conjunction with the reweighting strategy described below.

Given that fixed effects cannot control for all the relevant dynamics faced by workers, it falls to the time-varying covariates described above to construct the relevant counterfactual. In essence, these controls can provide the flexible trends that the fixed effects cannot. However, they are only as relevant as the untreated observations off of which they are estimated. If there are large differences between these trends, say, across industry, and treated workers are concentrated in particular industries, they would provide an inaccurate counterfactual. Ideally, this problem could be solved with a sufficiently unrestricted model that would allow the effects of covariates to differ across groups of workers. However, both because of sample size restrictions and because it is not a priori clear how to define such groups, it is difficult to employ such an unrestricted methodology in this case.

Instead, I follow the inverse propensity weighting literature in an effort to make the sample of untreated workers observationally similar to the average displaced worker. That is, I estimate a probability of treatment in the sample period for each individual, p_i and reweight the untreated individuals by

$$\frac{\hat{p}_i}{1-\hat{p}_i}\frac{1-\bar{\hat{p}}}{\bar{\hat{p}}}. \tag{9}$$

This procedure generates a sample of untreated workers that is similar to those that are treated. Accordingly, the coefficients on the age controls (and all other controls) are estimated in such a way as to provide the counterfactual for the hypothetical worker who is observationally similar to the average displaced worker. That is, if the time-varying heterogeneity is uncorrelated with the treatment or is only correlated with the treatment through baseline variables that are effectively controlled by the reweighting, it will not bias the estimates away from the true causal effect.

In all models, I estimate confidence intervals and standard errors using a stratified block bootstrap with 1000 replications. The bootstrap randomizes over individuals within HRS strata and accounts for the Study's sampling probabilities.

5 Data

I perform the empirical analysis using nine waves of the HRS, a nationally-representative longitudinal survey of older Americans. In particular, I study the original HRS cohort, made up of individuals born 1931 to 1941, who have been interviewed biennially since 1992.[4] Each interview year, respondents answer a battery of questions about demographics, health, employment, assets, and expectations.

[4]Although other birth cohorts have been added to the study over its lifetime, these cohorts yielded relatively few useable observations because of their smaller size and shorter tenure in the study.

5.1 Sample Restrictions

I restrict my sample to individuals who are married or partnered at the first interview and remain in their unions at least through the second wave. I use data from the 1992 baseline and any interviews until the partnership dissolves or the respondents miss a wave for any reason. Because the goal is to estimate the dynamics associated with displacement and the estimation strategy employs fixed effects models, respondents must stay in the same union at least through wave 2 for me to observe them over time.[5]

I further restrict my sample to individuals whose spouses were not displaced in the three years before and two years after the first interview. This restriction is intended to allow me to measure the effects of a "first" displacement, in some sense. The literature has demonstrated that displacements tend to come in groups. My goal is to estimate the total effect of an initial displacement, including the effects of subsequent displacements. This restriction also allows me to measure baseline characteristics and consider them to be plausibly exogenous to the future displacements. On the other hand, restricting the sample in this way eliminates frequently-displaced workers from my sample, who may also be an interesting group, and it limits the analysis to those individuals whose spouses survived the recession of the early 1990s without being displaced. It also biases the raw sample to over-represent respondents whose spouses do not work, as these individuals would not be at risk for displacement around the first interview, but this effect is mitigated by the propensity score reweighting I employ.

[5]Given that Charles and Stephens (2004) find an increase in the divorce hazard following displacement, one effect of displacement is increased likelihood of exiting my sample. However, it is unclear how to interpret the added worker effect for partners who dissolve their union, so dropping these post-dissolution observations seems appropriate. The divorce hazard could be modeled explicitly, but that is currently beyond the scope of this paper.

5.2 Measures of Interest

5.2.1 Treatment

In the event of a job change between interviews, respondents are asked why they left their previous employer and why they left any intervening employment arrangements. If any of the responses given include "layoff," "plant closing," or "business closing," I consider the job separation to be a displacement. At first interview, respondents are also variously asked about their last employer and any previous employment lasting five years or more. I use answers to these question to exclude individuals displaced in the three years before the first interview, as described in Section 5.1.

5.2.2 Outcomes

As this paper is primarily interested in labor force outcomes, I collect data from the HRS on employment, labor force participation, and earnings. While I can measure employment at monthly frequency, as I will describe shortly, the latter two measures are only observed at each biennial interview. Respondents are considered labor force participants if they report any work or if they report they are not employed but looking for a job. Earnings for the previous calendar year are also measured at each interview. My primary analysis, however, concerns employment at the extensive margin, which I analyze using monthly employment histories. I use questions about the starting and ending months of jobs to determine a workers' employment status at each month. Respondents can also give exceptions to these periods–indicating months of work during periods that were otherwise indicated to have non-work, and vice versa. HRS employment histories constructed in this way have also been used by Chan and Stevens (2001) in their study of the effects of workers' job losses on their own subsequent employment.

5.2.3 Controls

I use baseline reports of planned retirement year and the year in which respondents expect to significantly reduce their work to help control for pre-existing retirement plans. These variables are measured as a calendar year, so in my month-level analysis, I include indicator variables for whether the month in question is in the planned retirement year or is after the planned retirement year. At each interview, respondents are also asked if they have a work-limiting health condition. If they report that they have such a condition and that it is permanent (lasting more than three months), they are asked when it first bothered them, when it began to interfere with work, and when it began to prevent work entirely, if applicable. I use these to construct dummy variables that indicate whether the current period is after the first month reported by the respondent for each of these three levels of limitation.

Possible other controls that are not used could include parental health and presence of a dependent in the household. The former is not directly observed, but a probabilistic measure of parental health could be constructed using information from other questions, including parents' ages and dates of death. Information on who else lives in the household, along with data on the school enrollment status of children, could be used to construct measures of other dependents.

I do not include controls for the worker's own job losses, even though there are arguments to be made both ways as to their inclusion. In the most extreme example, two spouses working for the same firm could both be displaced by the same set of layoffs or plant closings. In this case, lacking controls for own displacement, the spouse displacement would appear to be generating decreased employment for the non-displaced worker. On the other hand, if a spouse's displacement leads a worker to change jobs or begin working and the worker is subsequently displaced, we would not want to control away the effects of this displacement. In practice, these controls have negligible impacts on the estimates of interest and are excluded from the results reported in this paper.

5.3 Sample Description

The final analysis is performed on a sample of 2,802 women and 3,139 men, which is described in Table 1. Between 10 and 11 percent of the respondents experience spousal displacement while they are in the sample. This treated group tends to have spouses who are younger at first interview, reflecting the higher labor force participation and associated greater risk for displacement at younger ages. Spouses who are eventually displaced also report higher probabilities for their likelihood of losing their job within one year. It is interesting to note that, given the sample restrictions described above, none of these individuals are observed being displaced at any time up to two years after reporting this probability. Still, higher reported probabilities are correlated with displacement at longer horizons.

5.4 Propensity Score Estimation

I estimate logit models for spouse displacement in the sample period for men and women individually. I include a large set of baseline covariates including characteristics of respondents' current jobs and careers and answers to questions asked of working individuals about their expectations, including their reported probabilities of losing their jobs in the next year, being able to find new work if they lose their jobs, working after age 62, and working after age 65. Logit model estimates are presented in Table 2. Only a few of the variables are statistically significant in these models, but this is at least partially due to considerable collinearity among them. The distributions of propensity scores are displayed in Figure 1. While the treated and control distributions are clearly distinct (more so for women than for men), there is also considerable overlap between them for both sexes.

Figures 2 and 3 show quantile-quantile (QQ) plots of the distributions of particular variables for the treated group, the control group, and the reweighted control group. Each point on a QQ plot indicates a percentile pair for the two distributions in question. That is, the upper rightmost point indicates the location of the 99th percentile of the two distributions. Its location on the horizontal axis shows the position of the 99th percentile of the treated group and its position on the vertical axis indicates the 99th percentile for the control group. If the distributions for the two groups are identical at all percentiles, all points in the QQ plot will fall on the 45-degree line. Points below the line indicate higher values for those percentiles in the treated group, while those above the line indicate higher values for the control group. To the extent that the reweighted QQ plots lie closer to the 45-degree line, the distributions of these variables are more similar after the reweighting takes place.

The distributions of propensity scores are, by construction, somewhat more similar after the control group is reweighted. The largest other differences appear to be in those variables related to spouse labor force participation, and thus, displacement. After reweighting, the distributions of spouse earnings, spouse tenure, spouse experience, and spouse birth month are all right-shifted. The considerable heaping in the expectation questions appearing at the bottom of the figures reduces the usefulness of QQ plots in comparing their distributions. However, the reweighted distributions generally appear to have fewer points that are far off the 45-degree line.

5.5 Observed Displacements

Figure 4 shows the frequency of sample displacements at each age for the respondent and displaced spouse. The distributions in Panel A appear quite similar for men and women, with the generally higher levels for men reflecting the greater number of men in the sample who have a spouse displaced. Panel B shows the age distribution of displaced men to be right-shifted relative to the distribution of women. This is because the sample is defined by the age of the nondisplaced spouse and husbands are generally older than their wives in the sample. Because the age distributions of the non-displaced spouses are similar for the two sexes, the displaced wives will generally be younger than the displaced husbands.

Figures 5 and 6 present the effects of displacement on the displaced spouse. The estimates are generated by a specification like (8) with propensity score reweighting. The solid lines in Panels A and B show the predicted probabilities of employment for workers displaced at month 0. The dashed lines show what those predictions would be if those same workers had the effects of their displacements zeroed out. One way of interpreting these estimates is to note that while employment rates for displaced spouses are stable in the range of 0.7 to 0.8 in the months immediately before displacement, at the peak of recovery 24 months after displacement they are some 20 percentage points lower. However, we would expect some of these workers to be drifting out of the labor force in the absence of displacement anyway.

Panels C and D divide the solid lines by the dashed lines, normalizing to the model's predicted employment rates for the treated group in the absence of treatment. Probability of employment ticks up in advance of displacement, reflecting the fact that workers must be employed in the month of displacement to have been displaced, while overall employment rates are falling for workers in this age group. Displaced men and women both have employment rates at only 50 percent of expected levels in the 6 months following a displacement. Figure 6 demonstrates decreases in log earnings at the time of displacement and in its aftermath. Earnings recover somewhat, but the effects are understated in this regression as calendar years with no earnings are dropped from the log analysis. However, this also means that earnings are below expected levels even for those who manage to have some earnings in post-displacement years. The effects on earnings are rather imprecise as earnings are measured at only yearly frequency and likely with considerable error.[6]

[6]Future work will use the restricted administrative earnings data available with the HRS, potentially improving the precision of earnings measures.

6 Results

Figure 7 displays the results for women in graphical form. Upon initial inspection, there is little evidence of an added worker effect, as the confidence bounds include zero for all estimates in Panel A. At a rather considerable lag of more than three years, the estimated effects do increase to six percentage points, but they remain statistically indistinguishable from zero. Panel B shows predicted employment rates for treated women (solid line) along with their predicted employment rates in the absence of spouse displacement (dashed line), the model's counterfactual. Panel C divides the former by the latter and demonstrates that the increased probability of work at long horizons, though statistically not different from zero, represents an increase in expected employment of 15 to 20 percent.

One theory for the lack of an added worker effect at the time of the job loss would be that women would like to work but cannot find employment because of discrimination or other reasons. The estimates in Figure 8 are designed to partially answer this question by showing the effects of spouse displacement on labor force participation. Unfortunately, because labor force participation is only measured at interview, there are considerably fewer observations that can be used to measure this outcome. Again, the results show the same general pattern as the employment estimates and are again statistically indistinguishable from zero, suggesting that an inability to find work is not driving the results. That being said, if workers know that they will face discrimination and thus do not enter the labor force at all, we would see the same effect here.

The same estimates for men, which are presented in Figures 9 and 10, tell a somewhat different story. The coefficient estimates indicate an increase in employment probability contemporaneous with their wives' job losses that is, at least briefly, statistically different from 0. As for women in Stephens (2002), the increase in expected labor supply leads spousal job loss, presumably because the household has information about the impending displacement. While the effect fades out in the several years following the displacement, the effect at horizons of more than four years suggests a long-run effect of six percentage points, although this too is not statistically different from zero. Panel B in Figure 9 puts these estimates in context, showing what could be interpreted as delayed exit from employment at the time of spousal displacement. This amounts to a 10 percent increase in employment at the time of the displacement, and a 15 percent increase in the long run, as seen in Panel C. The comparatively-large and somewhat-discontinuous long-run effect could suggest that a more flexible model is needed to less-restrictively account for effects at long horizons. The labor force participation estimates in Figure 10 tell roughly the same story, with the potential exception of the effect two years after displacement. However, this difference could be attributed to the lack of precision for these estimates.

Imprecise estimates also plague the estimated effects on earnings, as presented for both sexes in Figure 11. In general, there appears to be no impact of spouse displacement on earnings. Since these are estimates are conditional on earnings observations being positive, one interpretation could be that all of the effect is happening at the level of the participation decision. Apart from increasing labor supply on the extensive margin, older workers neither increase hours nor find higher-paying jobs in response to spouse displacement.[7]

I further divide the sample by respondents' reported enjoyment of their time with their spouses. I divide the sample into those who report at baseline that they find such time to be "extremely enjoyable" (EE) and those who give all other responses.[8] Sample statistics for the divided groups appear in Table 3. As a general rule the EE group is more white and better educated, and has higher earnings and more assets. It is also notable that both members of the household are more likely to be in the labor force for this group.

[7] Estimates of the effects on hours, which are not presented here, were similarly noisy.

[8] The three other response categories are "very enjoyable," "somewhat enjoyable," and "not too enjoyable." Over half of the sample responded "very enjoyable," while "somewhat enjoyable" and "not too enjoyable" make up less than 19 percent. I would have preferred to compare these lowest two groups to "extremely" and "very," but there were two few responses in the low groups to make a meaningful comparison.

Figure 12 presents findings from propensity-weighted fixed effects models estimated for each group. Women in the EE group whose husbands are displaced exhibit an added worker effect lagging the displacement by six to twelve months. The effect is considerably stronger than both the non-EE group and women on average, as seen in Figure 7. They demonstrate a considerable increase in long-run propensity to work even after their husbands' employment rates have returned to expected levels. The short-run effect for men that is seen in Figure 9 seems to be almost entirely driven by the EE group, despite the fact that their wives employment rates return to expected levels more quickly. The long-run effect, on the other hand is driven by the non-EE group, as spouse displacement has effectively no impact on the employment of EE men at long horizons.

The stronger added worker effect for the EE group is somewhat surprising, as those are the individuals who theoretically have the most to gain from coordinated leisure. However, the short-run increases in labor supply could reflect a desire to more fully insure against the negative income shock and coordinate leisure further in the future. It could also suggest stronger intrahousehold cooperation, as those couples who most enjoy their time together may be more likely to share the burden of extra work. Unfortunately, given that assets, education, and any number of other variables are endogenous to enjoyment of time with spouse, it is impossible to determine from these findings whether one of these effects is driving the different result.

7 Conclusion

I study the effects of spouse displacement on a workers' own labor force outcomes for a sample of workers who were over age 51 at the time of the displacement. The analysis initially shows a temporary increase in the probability of work for men whose wives are displaced, perhaps reflecting delayed retirement. Women, on average, show no increase in employment or labor force participation at short horizons, but may increase their market labor effort three or more years beyond their spouses' displacements. An analysis of the subsample of workers who report finding time with their spouses "extremely enjoyable" reveals that the short-term effect for men is concentrated in this group and that women in this group exhibit a contemporaneous and permanent increase in employment in response to their husbands' displacement. In continuing work on this paper, I am trying to better characterize the findings for those who most enjoy time with their spouses, I am studying the effect of spouse displacement on desire to increase hours worked, even when observed hours are unaffected, and I am examining the role of "unretirement" in these effects.

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Table 1: Sample Means

	Women		Men	
	Sp Not Displaced	Sp Displaced	Sp Not Displaced	Sp Displaced
Birth Year	1936.14	1936.45	1936.09	1936.70
	(3.169)	(3.137)	(3.161)	(3.088)
Sp Birth Year	1932.97	1934.70	1939.59	1941.11
	(5.98)	(5.44)	(5.97)	(6.12)
Race				
White	0.86	0.89	0.85	0.89
	(0.34)	(0.32)	(0.36)	(0.32)
Black	0.06	0.04	0.07	0.05
	(0.25)	(0.19)	(0.25)	(0.22)
Hispanic	0.05	0.05	0.06	0.04
	(0.22)	(0.22)	(0.24)	(0.19)
Education				
Less than HS	0.20	0.19	0.20	0.22
	(0.40)	(0.39)	(0.40)	(0.42)
HS Grad	0.46	0.42	0.35	0.38
	(0.50)	(0.49)	(0.48)	(0.48)
Some College	0.20	0.24	0.20	0.18
	(0.40)	(0.43)	(0.40)	(0.38)
College Grad	0.14	0.15	0.25	0.22
	(0.35)	(0.35)	(0.43)	(0.42)
Spouse Education				
Less than HS	0.24	0.15	0.19	0.15
	(0.43)	(0.36)	(0.39)	(0.35)
HS Grad	0.37	0.36	0.43	0.50
	(0.48)	(0.48)	(0.49)	(0.50)
Some Coll	0.17	0.20	0.21	0.22
	(0.38)	(0.40)	(0.41)	(0.41)
Coll Grad	0.22	0.29	0.17	0.14
	(0.41)	(0.45)	(0.38)	(0.35)
Earnings (000s)	11.47	11.40	34.47	30.36
	(15.69)	(13.58)	(39.55)	(33.26)
Sp Earnings (000s)	27.27	31.29	12.96	15.60
	(37.00)	(23.42)	(15.97)	(16.22)
Assets (000s)	71.84	61.48	58.12	58.89
	(185.97)	(211.10)	(170.41)	(202.10)
In LF	0.59	0.63	0.85	0.87
	(0.49)	(0.48)	(0.35)	(0.34)
Sp in LF	0.69	0.91	0.64	0.81
	(0.46)	(0.28)	(0.48)	(0.39)
Planned Rtrmnt Yr	1999.26	1999.13	1999.91	2000.09
	(4.21)	(4.46)	(4.64)	(4.38)
Sp Planned Rtrmnt Yr	1998.88	1998.86	2002.27	2003.30
	(4.93)	(4.83)	(6.31)	(6.35)
Pr(Lose Job)	0.08	0.07	0.11	0.09
	(0.19)	(0.19)	(0.22)	(1.94)
Sp(Pr Lose Job)	0.08	0.13	0.08	0.14
	(0.20)	(0.23)	(1.92)	(2.41)
N	*2513*	*289*	*2797*	*342*

Sample standard deviations in parentheses. Means are weighted by inverse sampling probability. All variables measured at first interview. Earnings refer to previous calendar year. Planned Retirement year only includes positive responses, all other variables include zeros.

Table 2: Logit Estimates for Spouse Displacement in Sample Period

	Women			Men		
	Coefficient	Bootstrap Std. Error	Odds Ratio	Coefficient	Bootstrap Std. Error	Odds Ratio
Birth Month						
Linear	-0.040	0.038	0.961	0.071	0.037	1.073
Quadratic*10^{-2}	0.186	0.113	1.204	-0.209	0.113	0.811
Cubic*10^{-4}	-0.261	0.127	0.770	0.250	0.129	1.284
Quartic*10^{-6}	0.110	0.047	1.116	-0.096	0.048	0.908
Spouse Birth Month						
Linear	0.001	0.003	1.001	0.000	0.004	1.000
Quadratic*10^{-2}	-0.001	0.002	0.999	-0.001	0.004	0.999
Cubic*10^{-4}	0.002	0.002	1.002	0.002	0.002	1.002
Quartic*10^{-6}	0.000	0.001	1.000	0.000	0.000	1.000
Spouse Race						
Black	-0.351	0.327	0.704	-0.564	0.312	0.569
Hispanic	0.279	0.263	1.322	-0.524	0.350	0.592
Spouse Education						
Less than HS	-0.938	0.271	0.391	-0.044	0.306	0.957
HS Grad	-0.530	0.222	0.588	0.213	0.224	1.237
Some College	-0.333	0.220	0.716	0.048	0.227	1.049
Education						
Less than HS	0.388	0.306	1.474	0.171	0.228	1.187
HS Grad	0.154	0.250	1.166	-0.069	0.203	0.933
Some College	0.218	0.260	1.244	-0.314	0.219	0.730
Spouse Occupation						
Farm, Forestry, Fishing	-0.419	0.448	0.658	0.247	0.762	1.280
Sales, Administration	-0.163	0.222	0.850	0.456	0.199	1.577
Mech., Precision Work	-0.052	0.201	0.949	0.717	0.527	2.049
Services	-0.961	0.400	0.383	0.413	0.251	1.512
Operators	0.203	0.221	1.225	0.818	0.319	2.266
Census Region						
Midwest	0.197	0.207	1.217	-0.035	0.204	0.966
Sout	0.079	0.214	1.082	-0.088	0.185	0.916
West	-0.022	0.239	0.978	0.256	0.208	1.292
In Labor Force	0.209	0.258	1.232	-0.236	0.291	0.790
Spouse in Labor Force	1.203	0.340	3.331	0.356	0.311	1.428
Spouse Pr(Lose Job)						
Linear	0.090	0.103	1.094	0.023	0.078	1.023
Quadratic	-0.010	0.013	0.990	0.002	0.010	1.002
Spouse Pr(Find Job)						
Linear	0.136	0.069	1.146	0.187	0.070	1.205
Quadratic	-0.006	0.007	0.994	-0.019	0.007	0.981
Pr(Lose Job)						
Linear	-0.110	0.122	0.896	0.028	0.099	1.028
Quadratic	0.014	0.015	1.014	-0.012	0.015	0.988
Pr(Find Job)						
Linear	0.054	0.093	1.055	-0.082	0.066	0.921
Quadratic	-0.009	0.010	0.991	0.007	0.007	1.008

Continued

Table 2: ...Continued

	Women			Men		
	Coefficient	Bootstrap Std. Error	Odds Ratio	Coefficient	Bootstrap Std. Error	Odds Ratio
Experience						
Linear	0.005	0.021	1.005	-0.059	0.038	0.942
Quadratic*10^{-2}	-0.016	0.048	0.984	0.146	0.068	1.157
Spouse Experience						
Linear	0.020	0.050	1.020	0.023	0.022	1.023
Quadratic*10^{-2}	0.001	0.077	1.001	-0.029	0.051	0.971
Earnings						
Linear*10^{-2}	-0.003	0.003	0.997	-0.002	0.001	0.998
Quadratic*10^{-7}	0.011	0.012	1.011	0.001	0.001	1.001
Cubic*10^{-13}	-0.132	0.144	0.876	-0.002	0.002	0.998
Spouse Earnings						
Linear*10^{-3}	0.003	0.013	1.003	-0.001	0.022	0.999
Quadratic*10^{-8}	0.002	0.026	1.002	0.029	0.080	1.030
Cubic*10^{-14}	-0.078	0.155	0.925	-0.154	0.792	0.857
Assets						
Linear*10^{-4}	-0.012	0.013	0.988	0.010	0.016	1.010
Quadratic*10^{-11}	0.051	0.371	1.052	-0.122	0.401	0.885
Cubic*10^{-18}	-0.006	2.506	0.994	0.336	5.150	1.399
Spouse Pr(Work After 62)						
Linear	0.004	0.009	1.004	0.007	0.009	1.007
Quadratic*10^{-2}	-0.003	0.008	0.997	-0.006	0.009	0.994
Spouse Pr(Work After 65)						
Linear	-0.006	0.009	0.994	-0.006	0.009	0.994
Quadratic*10^{-2}	0.004	0.009	1.004	0.006	0.010	1.006
Pr(Work After 62)						
Linear	-0.019	0.010	0.981	0.002	0.007	1.002
Quadratic*10^{-2}	0.012	0.010	1.012	0.001	0.007	1.001
Pr(Work After 65)						
Linear	0.020	0.013	1.021	-0.005	0.007	0.995
Quadratic*10^{-2}	-0.022	0.014	0.978	0.001	0.008	1.001
Spouse Yrs to Planned Retirement						
Linear	-0.010	0.038	0.990	0.037	0.030	1.037
Quadratic*10^{-2}	-0.048	0.239	0.953	-0.155	0.128	0.857
Years to Planned Retirement						
Linear	-0.013	0.048	0.987	-0.041	0.033	0.959
Quadratic*10^{-2}	0.135	0.297	1.145	0.080	0.176	1.083
Spouse Job Tenure						
Linear	-0.001	0.021	0.999	-0.077	0.027	0.926
Quadratic*10^{-2}	-0.037	0.056	0.964	0.142	0.091	1.152
Job Tenure						
Linear	0.033	0.030	1.034	0.086	0.023	1.090
Quadratic*10^{-2}	-0.046	0.085	0.955	-0.212	0.064	0.809
Constant	-3.638	1.006	0.026	-3.599	0.729	0.027

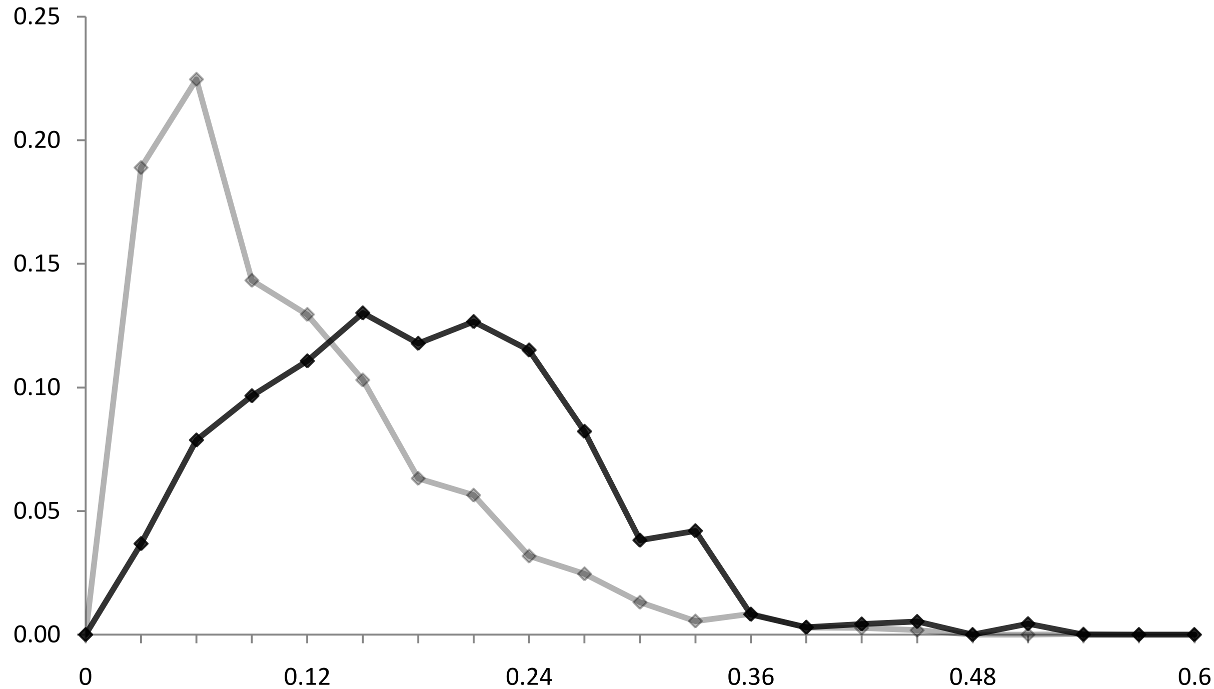
Table 3: Sample Means by Enjoyment of Leisure Time with Spouse

	Women		Men	
	All Others	Extremely Enjoy	All Others	Extremely Enjoy
Birth Year	1936.12	1936.33	1936.14	1936.20
	(3.144)	(3.230)	(3.159)	(3.157)
Sp Birth Year	1932.97	1933.68	1939.74	1939.78
	(5.78)	(6.40)	(6.02)	(5.97)
Race				
White	0.85	0.92	0.84	0.88
	(0.36)	(0.28)	(0.37)	(0.33)
Black	0.07	0.03	0.08	0.04
	(0.26)	(0.18)	(0.27)	(0.21)
Hispanic	0.06	0.04	0.06	0.05
	(0.23)	(0.19)	(0.24)	(0.22)
Education				
Less than HS	0.22	0.14	0.23	0.14
	(0.42)	(0.34)	(0.42)	(0.35)
HS Grad	0.44	0.49	0.35	0.36
	(0.50)	(0.50)	(0.48)	(0.48)
Some College	0.20	0.23	0.19	0.22
	(0.40)	(0.42)	(0.39)	(0.42)
College Grad	0.14	0.15	0.23	0.28
	(0.34)	(0.35)	(0.42)	(0.45)
Spouse Education				
Less than HS	0.25	0.18	0.20	0.15
	(0.43)	(0.38)	(0.40)	(0.36)
HS Grad	0.36	0.38	0.44	0.43
	(0.48)	(0.49)	(0.50)	(0.49)
Some Coll	0.18	0.17	0.21	0.23
	(0.38)	(0.37)	(0.40)	(0.42)
Coll Grad	0.21	0.27	0.16	0.19
	(0.41)	(0.44)	(0.37)	(0.39)
Earnings (000s)	11.15	12.37	32.66	37.19
	(15.50)	(15.41)	(35.94)	(45.01)
Sp Earnings (000s)	25.81	33.24	12.90	14.08
	(32.27)	(44.27)	(16.22)	(15.51)
Assets (000s)	66.89	82.15	55.96	63.48
	(182.23)	(206.46)	(161.46)	(200.78)
In LF	0.59	0.61	0.85	0.86
	(0.49)	(0.49)	(0.35)	(0.35)
Sp in LF	0.70	0.76	0.65	0.69
	(0.46)	(0.43)	(0.48)	(0.46)
Planned Rtrmnt Yr	1999.15	1999.53	1999.93	1999.94
	(4.14)	(4.47)	(4.61)	(4.63)
Sp Planned Rtrmnt Yr	1998.74	1999.20	2002.42	2002.39
	(4.97)	(4.77)	(6.12)	(6.75)
Pr(Lose Job)	0.07	0.08	0.12	0.10
	(0.19)	(0.19)	(0.23)	(2.10)
Sp(Pr Lose Job)	0.09	0.09	0.09	0.09
	(0.20)	(0.20)	(1.97)	(2.04)
Spouse Displaced	0.10	0.11	0.00	0.32
N	*2122*	*680*	*2227*	*912*

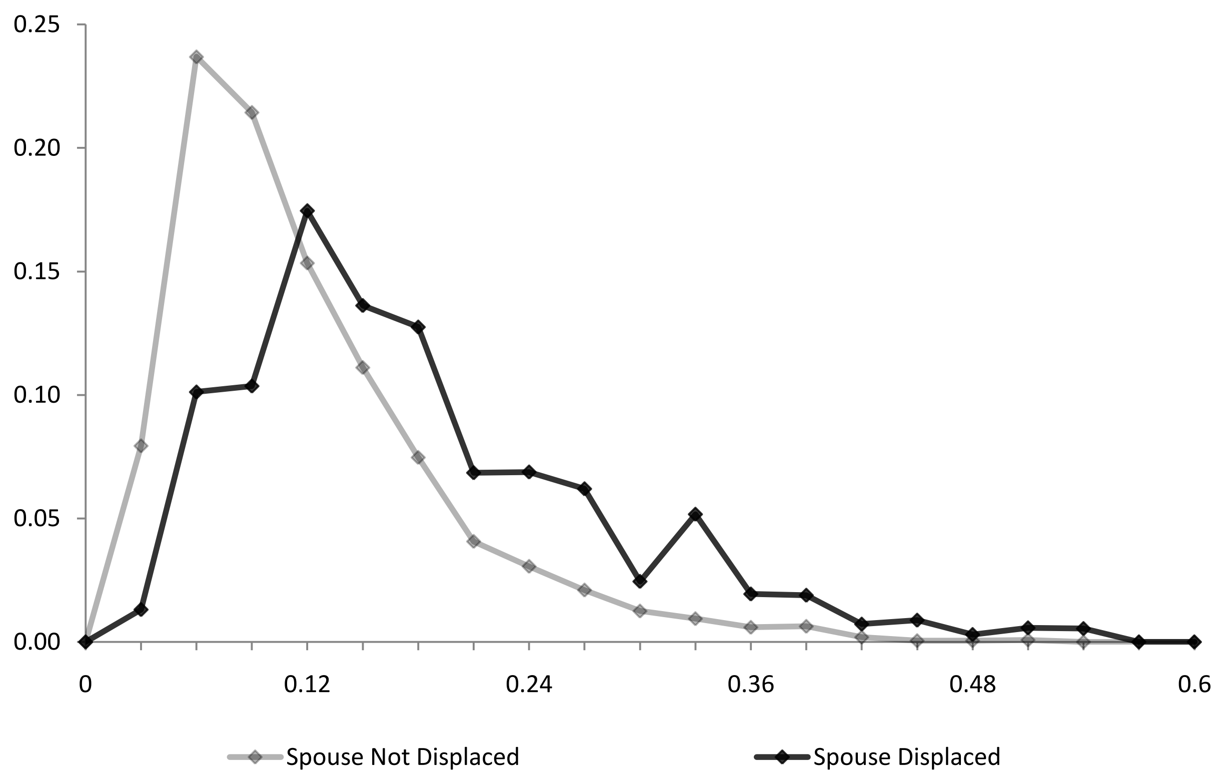
Sample standard deviations in parentheses. Means are weighted by inverse sampling probability. All variables measured at first interview. Earnings refer to previous calendar year. Planned Retirement year only includes positive responses, all other variables include zeros.

Figure 1: Distribution of Propensity Scores for Spouse Displacement

Panel A: Women

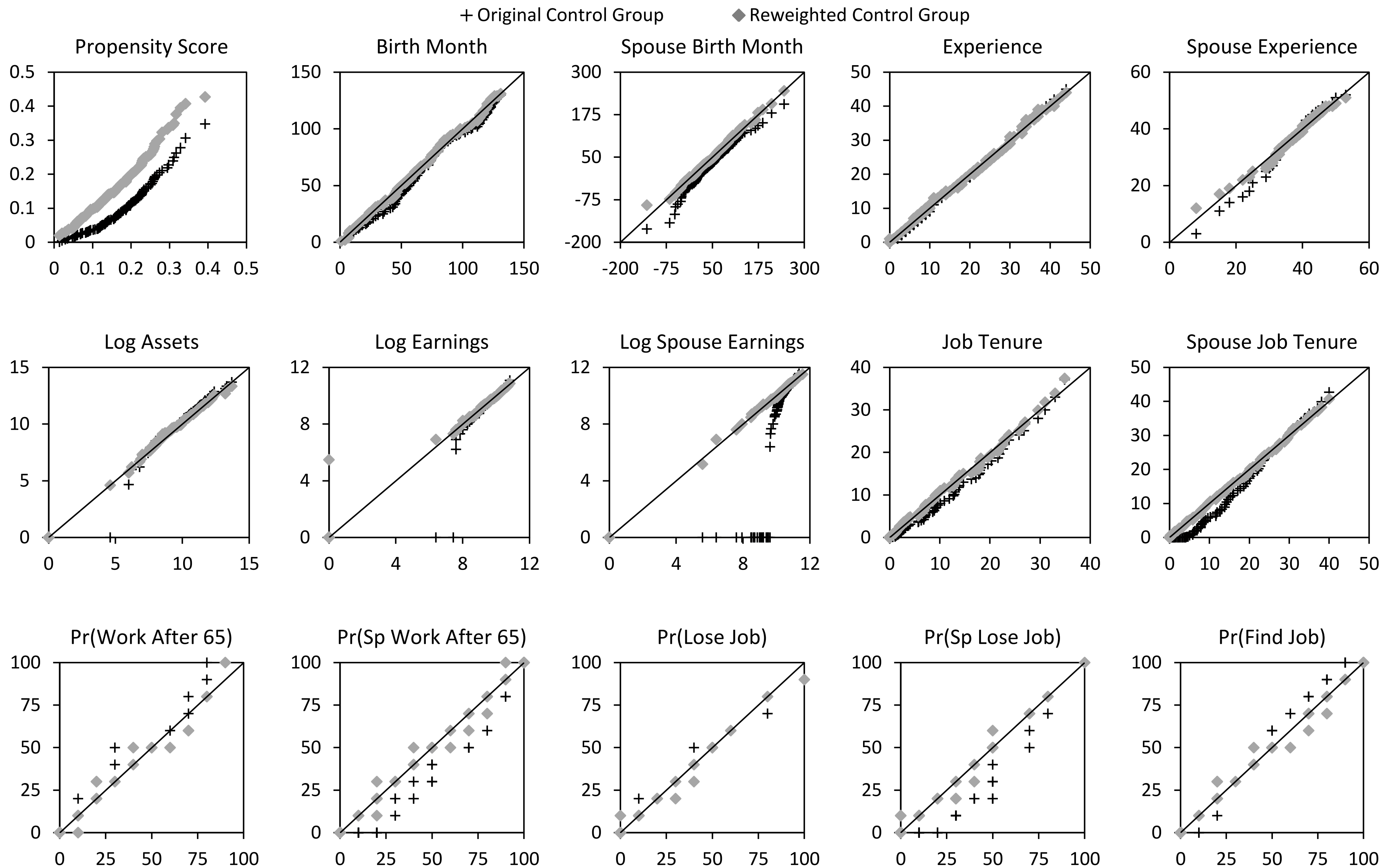


Panel B: Men



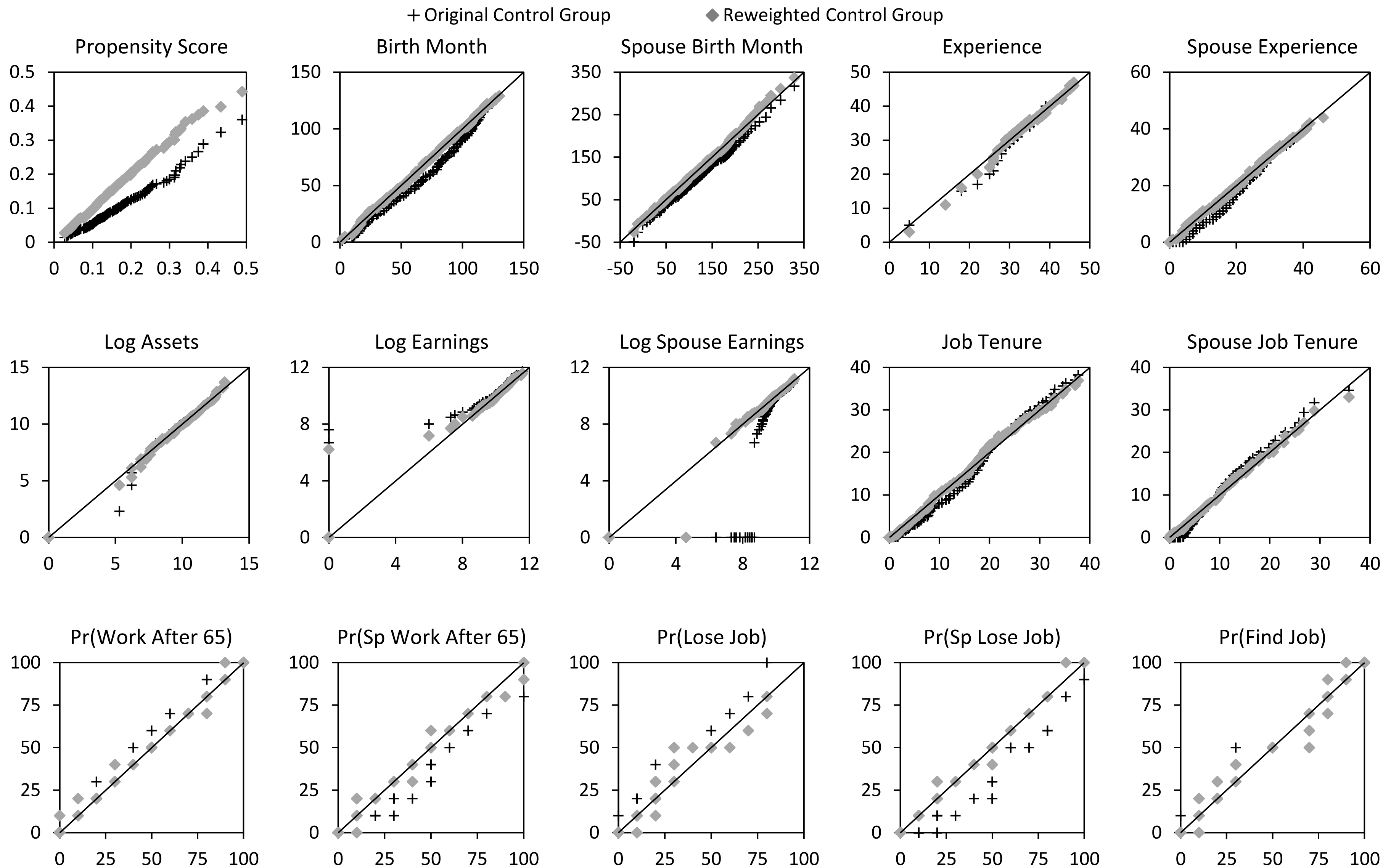
Propensity scores are estimated from logit model described in Table 2.

Figure 2: QQ Plots of Baseline Variables for Treated Group (Horizontal Axes) and Control Groups (Vertical Axes), Women



All values are measured at first interview. Distributions are weighted by inverse sampling probability. Birth month 0 is January 1931. The horizontal position of each QQ plot point shows the value of a particular percentile for the treated group, while the vertical position shows the value of that percentile for one of the control groups.

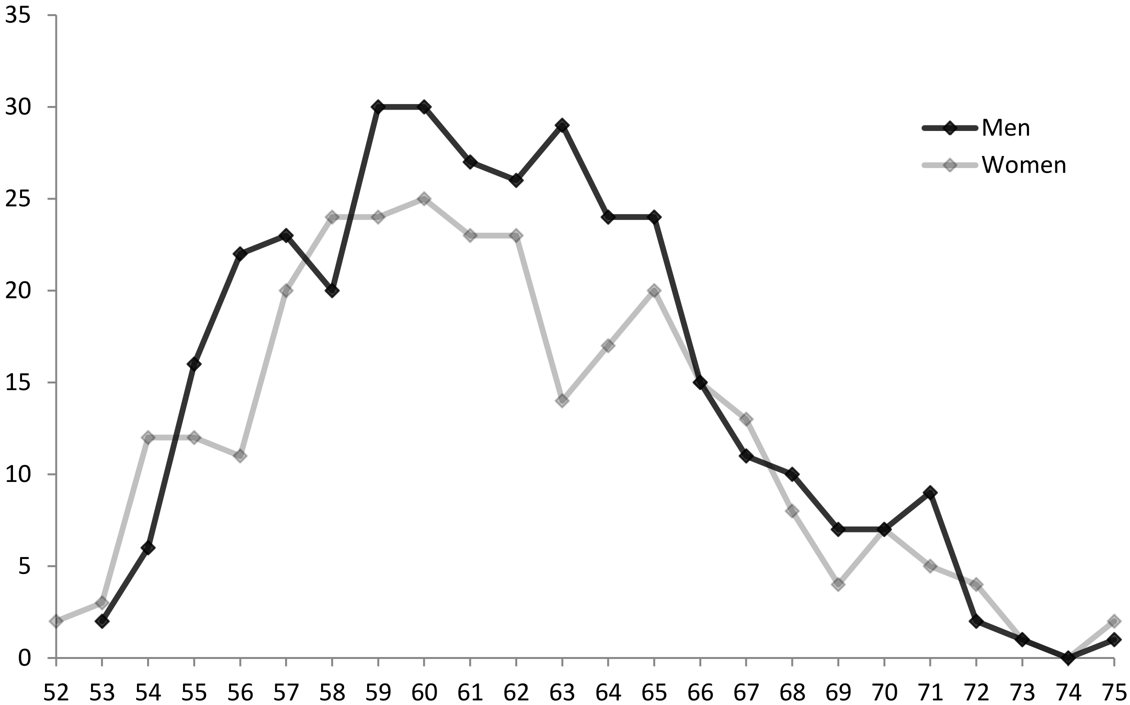
Figure 3: QQ Plots of Baseline Variables for Treated Group (Horizontal Axes) and Control Groups (Vertical Axes), Men



All values are measured at first interview. Distributions are weighted by inverse sampling probability. Birth month 0 is January 1931. The horizontal position of each QQ plot point shows the value of a particular percentile for the treated group, while the vertical position shows the value of that percentile for one of the control groups.

Figure 4: Age at Time of Displacement

Panel A: Age of Nondisplaced Spouse, by Sex of Nondisplaced Spouse



Panel B: Age of Displaced Spouse, by Sex of Displaced Spouse

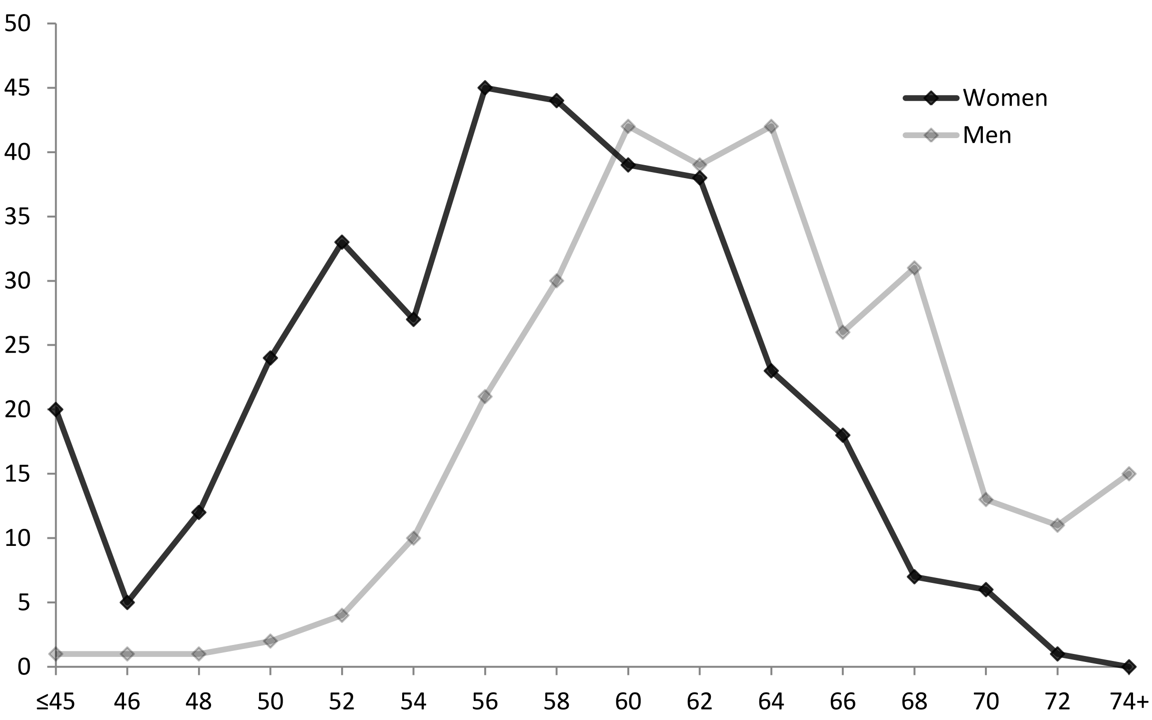
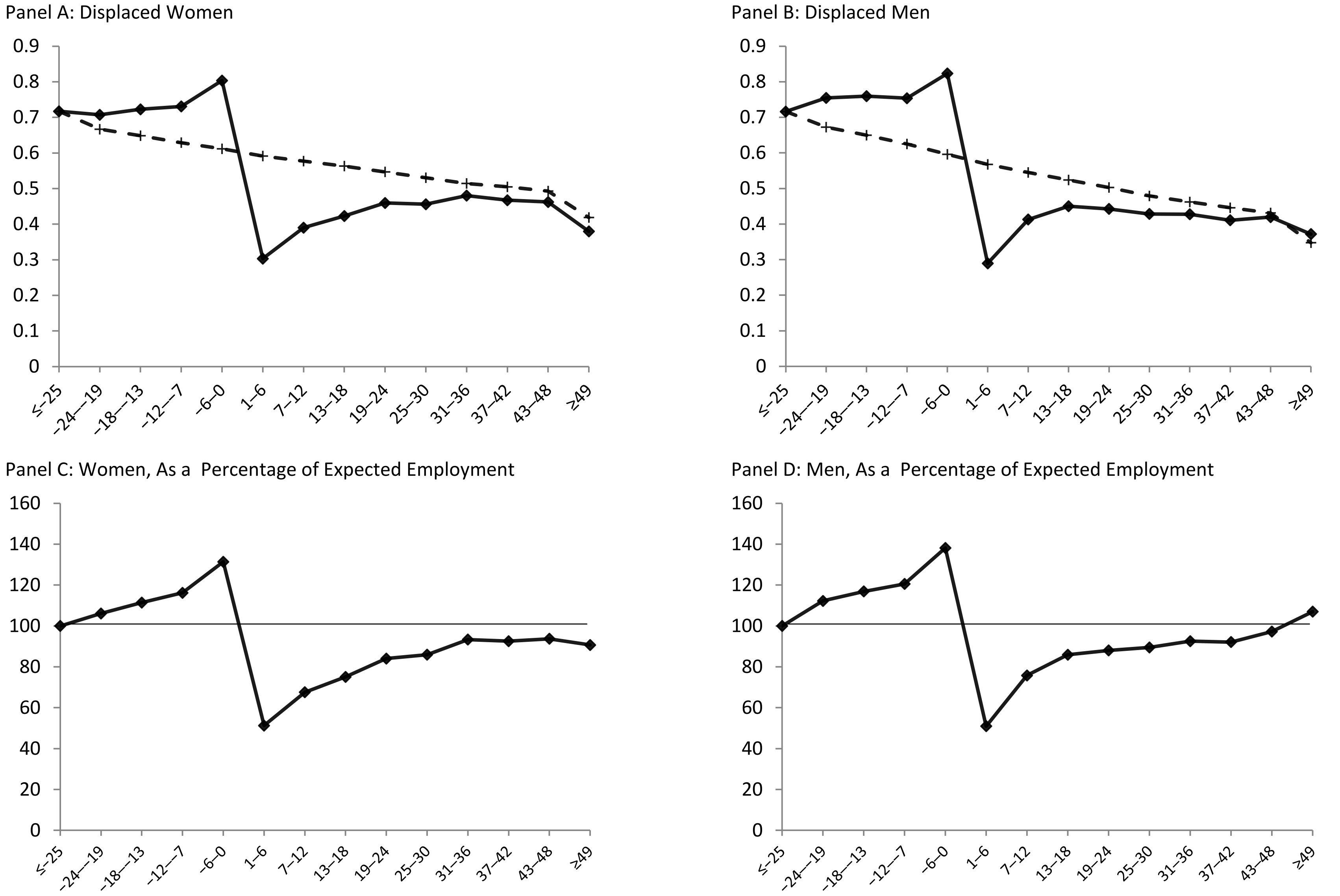


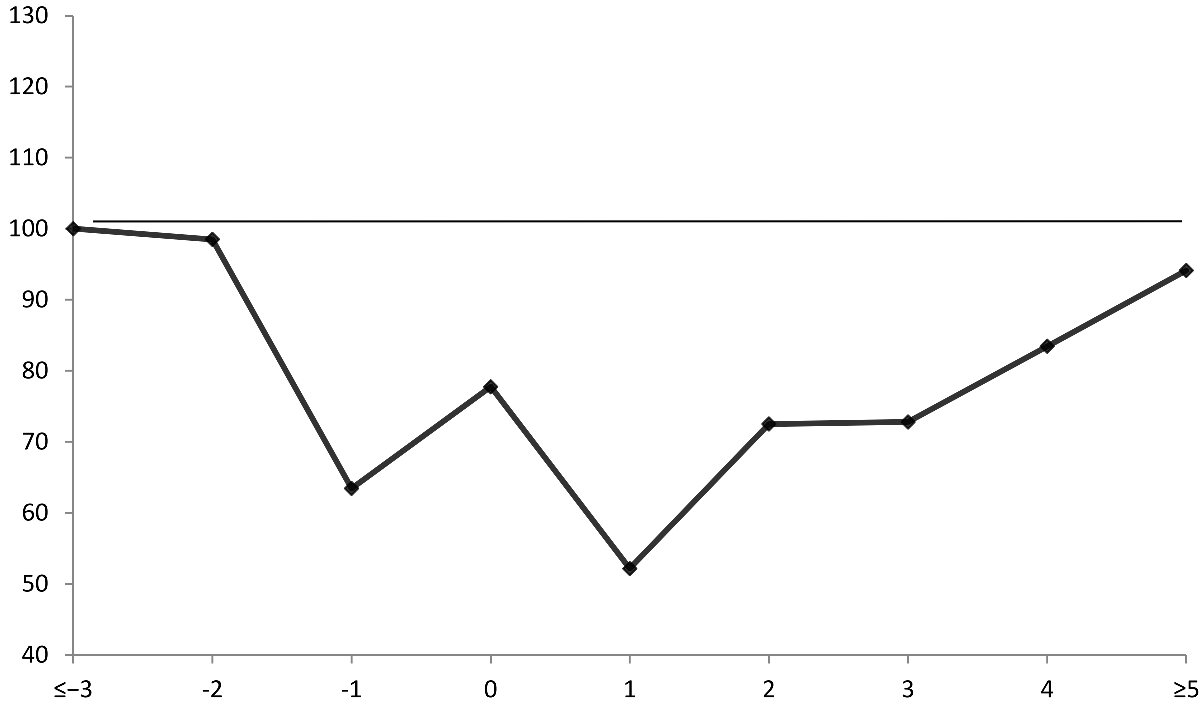
Figure 5: Predicted Probability of Employment for Workers Displaced at Month 0



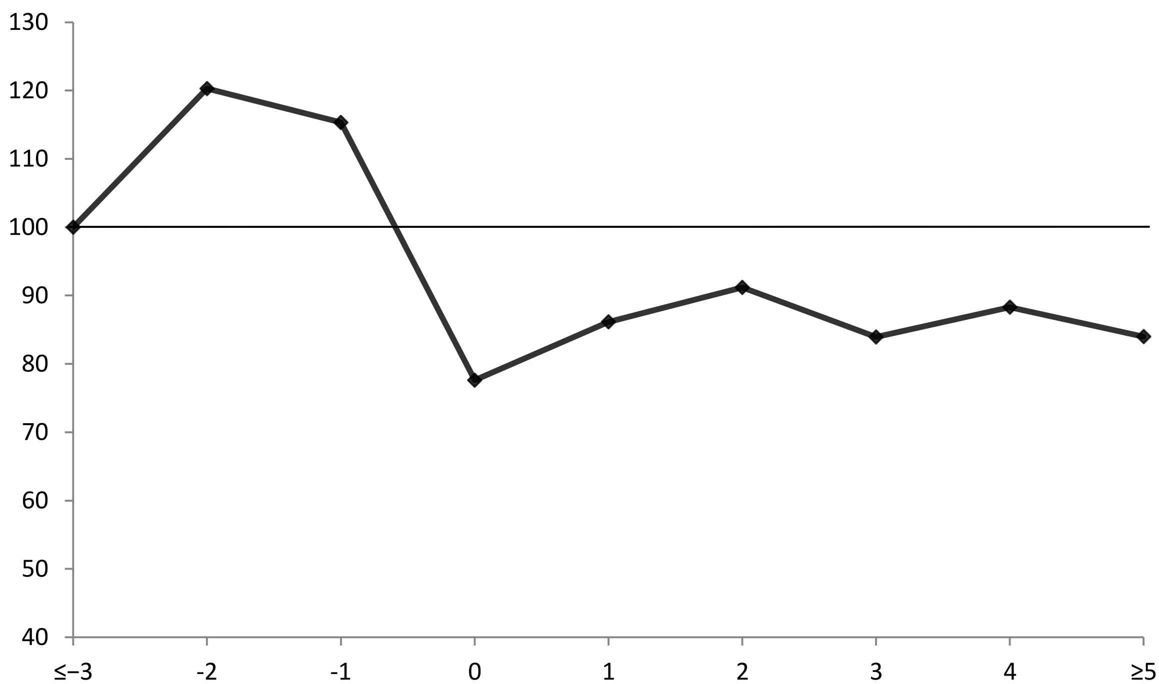
Dashed lines in Panels A and B indicate predicted probability of employment in absence of treatment. Predicted values from inverse propensity-weighted linear FE models with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 6: Earnings as a Percent of Expected Earnings for Workers Displaced in Year 0

Panel A: Women



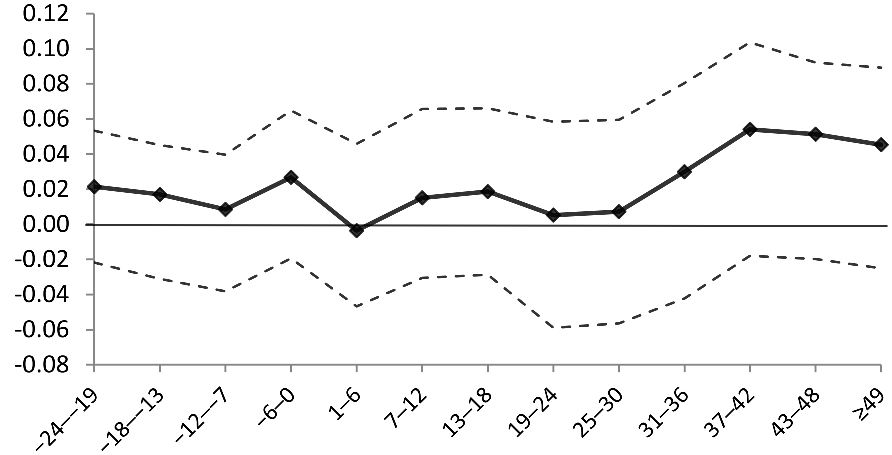
Panel B: Men



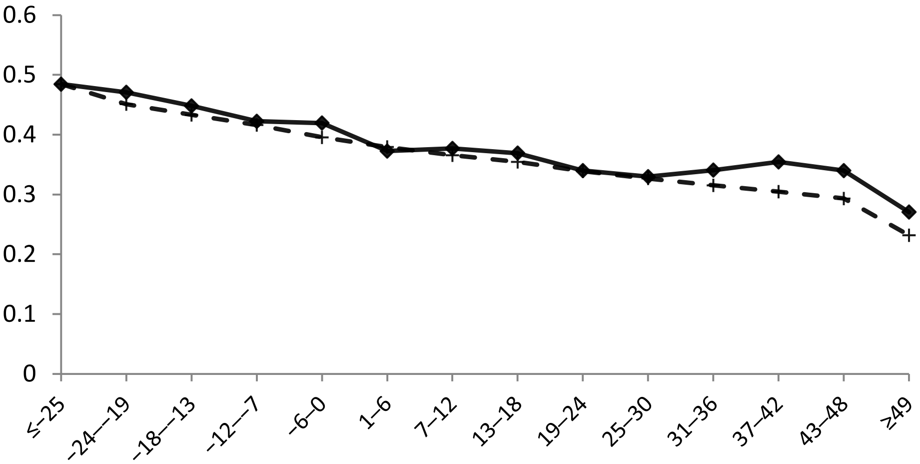
Estimates are generated by from inverse propensity-weighted linear FE models of log earnings with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year. Percent effects are calculated as 100*eβ. Effect is constrained to be 0 more than 2 years before displacement.

Figure 7: Effect of Spouse Displacement at Month 0 on Probability of Employment, Women

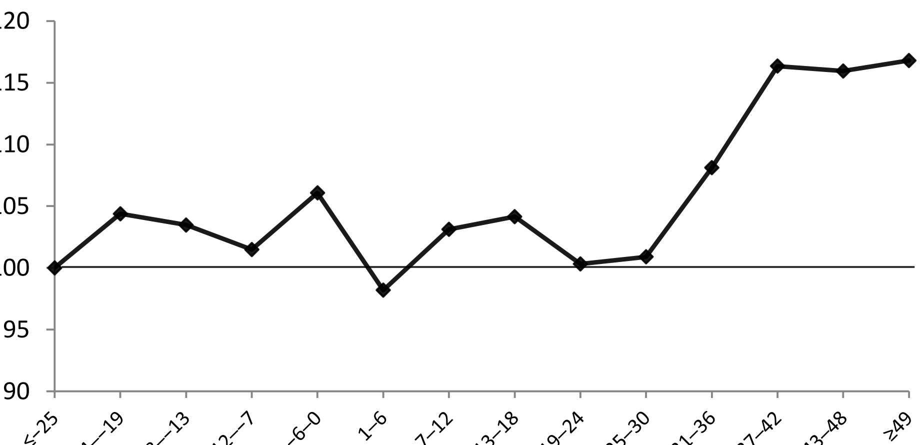
Panel A: Coefficient Estimates



Panel B: Predicted Employment Probability



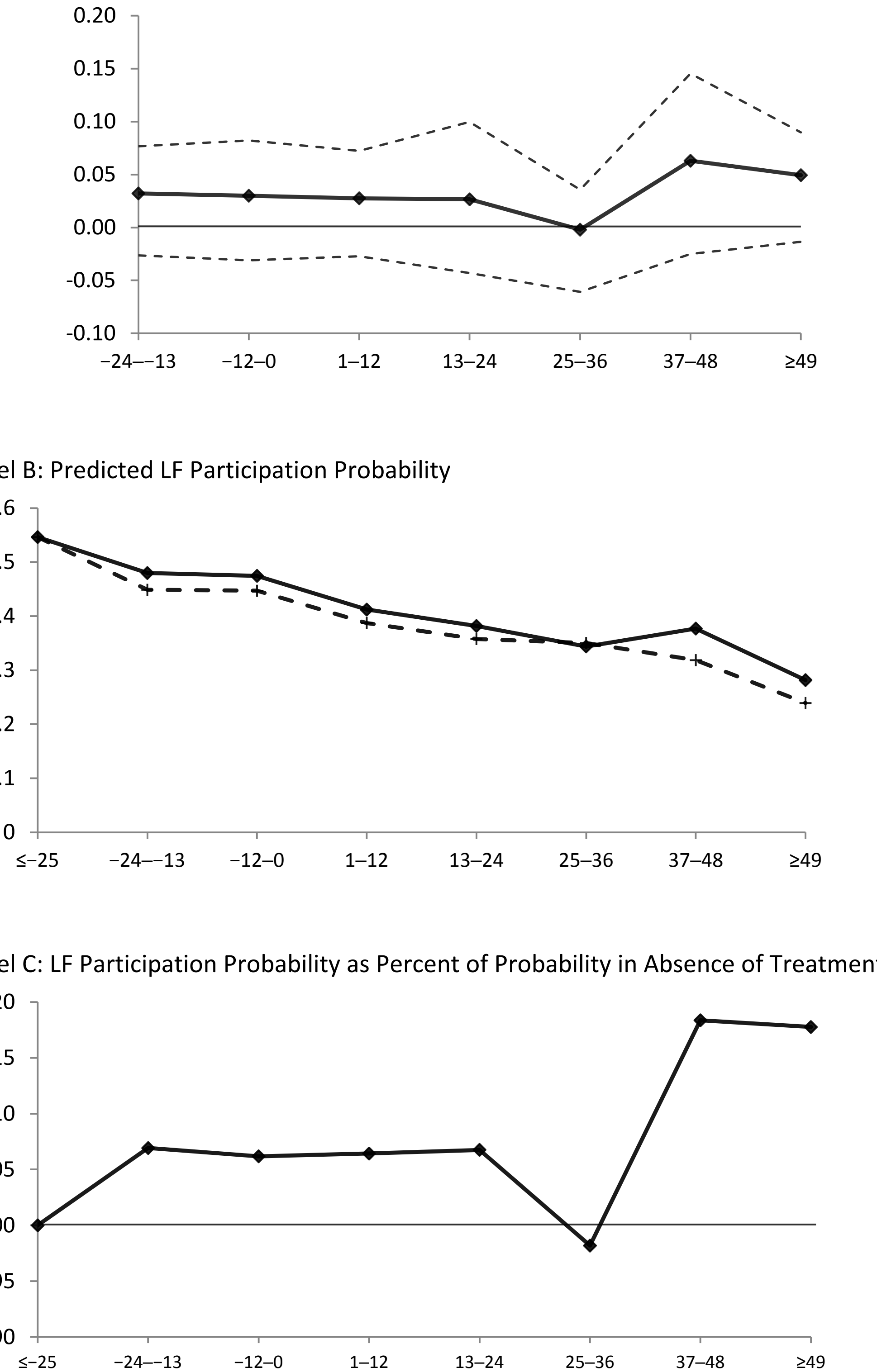
Panel C: Employment Probability as Percent of Probability in Absence of Treatment



Dashed lines in Panel A indicate bootstrapped 95% confidence interval. Dashed line in Panel B indicates predicted probability of employment in absence of treatment. Estimates generated by inverse propensity-weighted linear FE model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 8: Effect of Spouse Displacement at Month 0 on Probability of LF Participation, Women

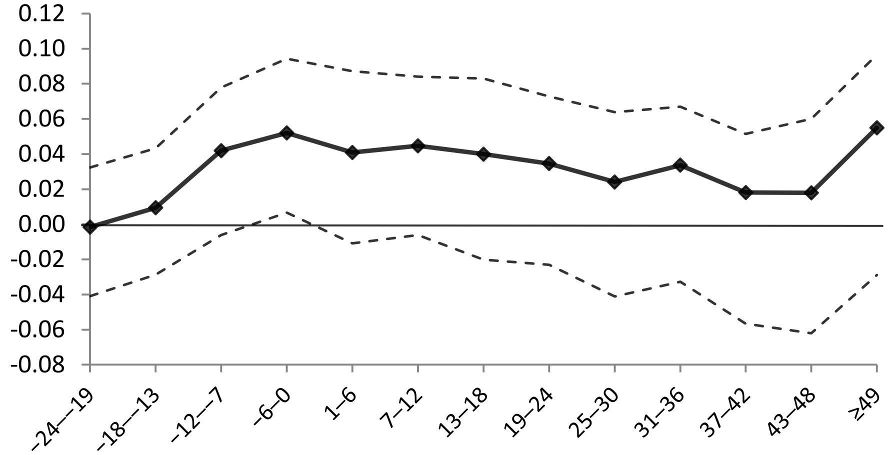
Panel A: Coefficient Estimates



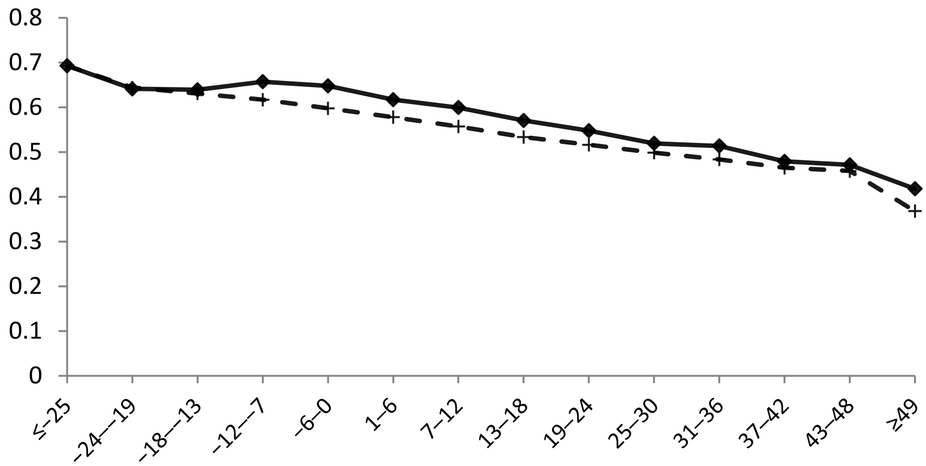
Dashed lines in Panel A indicate bootstrapped 95% confidence interval. Dashed line in Panel B indicates predicted probability of LF participation in absence of treatment. Estimates generated by inverse propensity-weighted linear FE model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 9: Effect of Spouse Displacement at Month 0 on Probability of Employment, Men

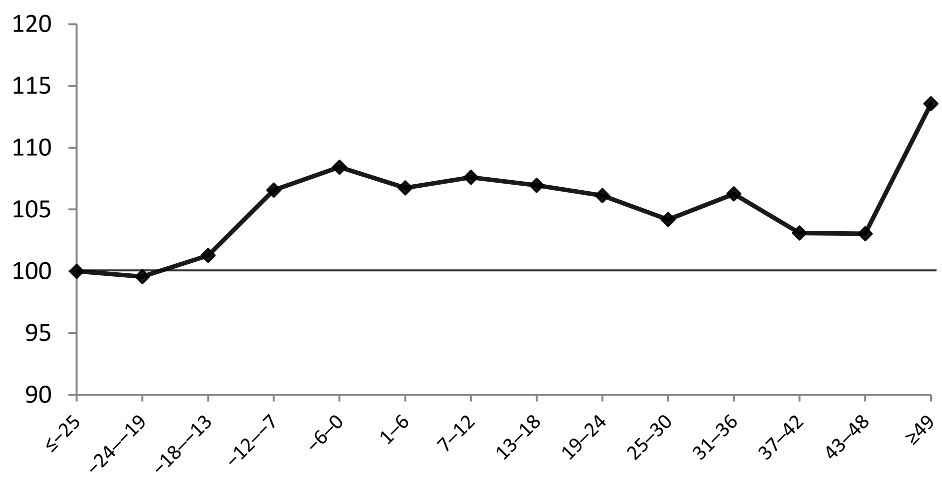
Panel A: Coefficient Estimates



Panel B: Predicted Employment Probability



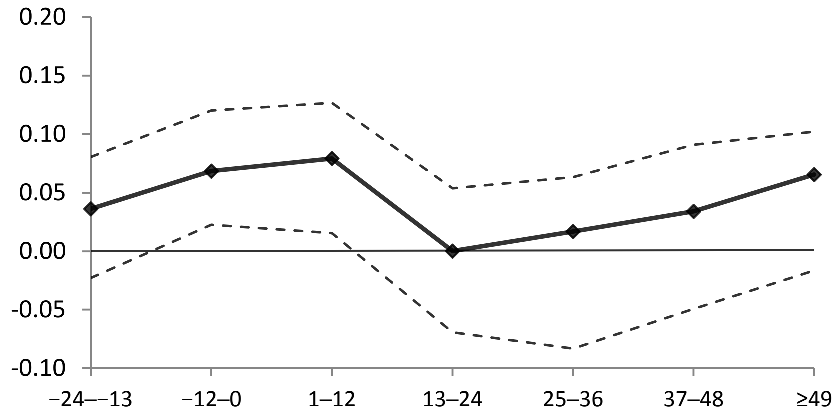
Panel C: Employment Probability as Percent of Expected



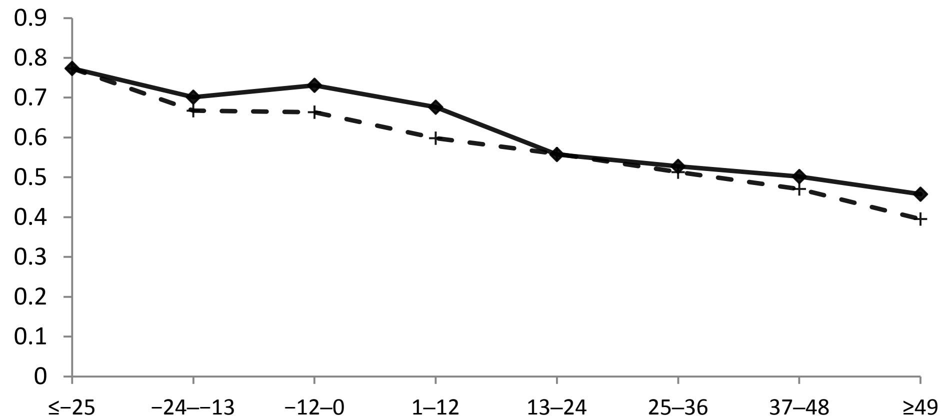
Dashed lines in Panel A indicate bootstrapped 95% confidence interval. Dashed line in Panel B indicates predicted probability of employment in absence of treatment. Estimates generated by inverse propensity-weighted linear FE model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 10: Effect of Spouse Displacement at Month 0 on Probability of LF Participation, Men

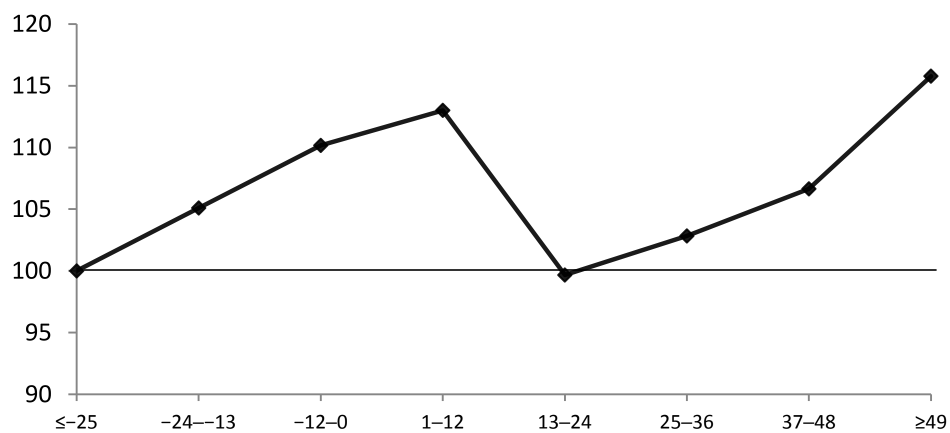
Panel A: Coefficient Estimates



Panel B: Predicted LF Participation Probability

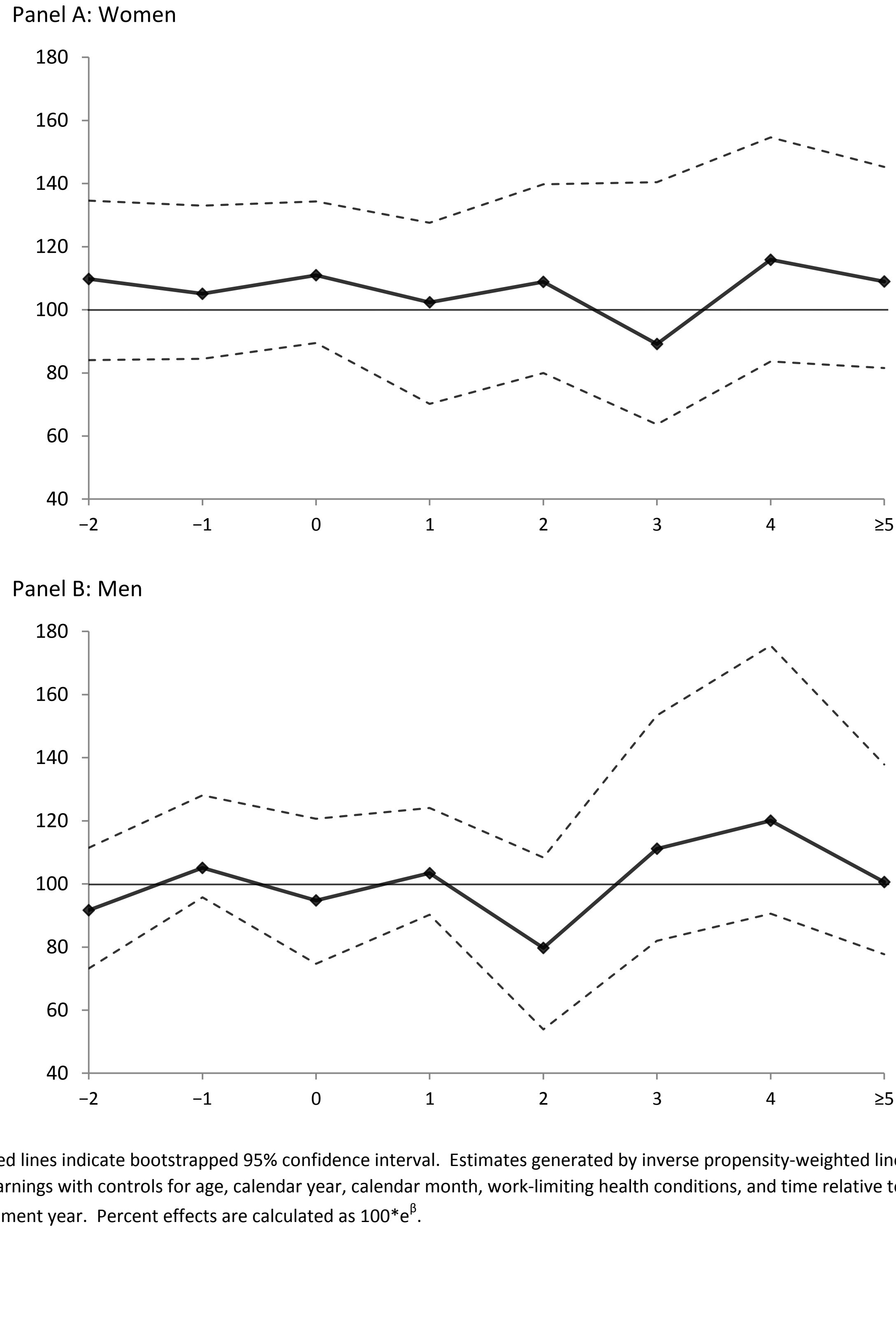


Panel C: LF Participation Probability as Percent of Expected



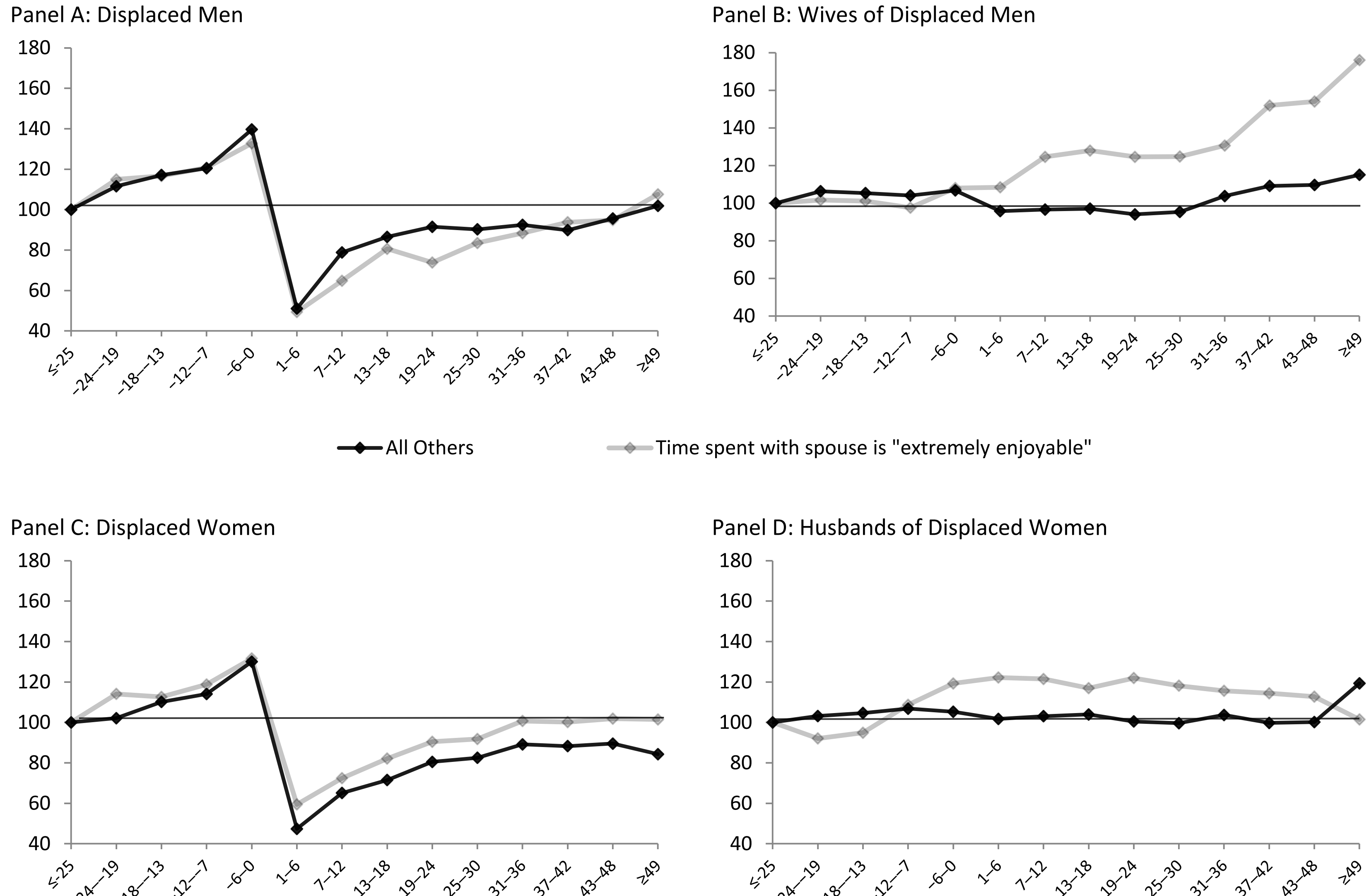
Dashed lines in Panel A indicate bootstrapped 95% confidence interval. Dashed line in Panel B indicates predicted probability of LF participation in absence of treatment. Estimates generated by inverse propensity-weighted linear FE model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 11: Effect of Spouse Displacement in Year 0 on Earnings



Dashed lines indicate bootstrapped 95% confidence interval. Estimates generated by inverse propensity-weighted linear FE model of log earnings with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year. Percent effects are calculated as $100*e^{\beta}$.

Figure 12: Employment as a Percent of Employment Absent Displacement



Estimates for each series are generated by an inverse propensity-weighted linear FE model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.