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for Displaced Workers**

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Choosing a Control Group for Displaced Workers

Pawel Krolikowski

The vast majority of studies on the earnings of displaced workers use a control group of continuously employed workers to examine the effects of initial displacements. This approach implies long-lived earnings reductions following displacement even if these effects are not persistent, overstating the losses relative to the true average treatment effect. This paper's approach isolates the impact of an average displacement without imposing continuous employment on the control group. In a comparison of the standard and alternative approaches using PSID data, the estimated long-run earnings losses fall dramatically from 25 percent to 5 percent. Model simulations reinforce these empirical findings.

JEL codes: E24, J63, J64.

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1 Introduction

Job displacement, defined as an involuntary loss of job through layoff, business closure, or insufficient demand, affects many workers in the United States. According to the Displaced Worker Survey, from January 2011 through December 2013, 4.3 million workers were displaced from jobs they had held for at least 3 years. An additional 5.2 million were displaced from jobs they had held for less than 3 years (Bureau of Labor Statistics, 2014). There is now a well-established literature documenting that the earnings and wage losses of displaced workers are large and extremely persistent, remaining even as long as 20 years after the displacement event.¹

Jacobson, LaLonde and Sullivan (1993*b*) (henceforth JLS) is a seminal paper in this literature. JLS used Pennsylvania administrative data from 1974 to 1986 and restricted their treatment and control groups to workers with 6 years of tenure at the beginning of 1980. They further restricted their control group to workers who remain continuously employed from 1980 to 1986. Since the contribution of JLS, the vast majority of the literature has taken their approach and used a control group of continuously-employed workers.² One reason the literature has focussed on a control group of workers who do not separate from their employer is to, in the spirit of human capital theory, isolate the portion of earnings potential that is destroyed when an individual involuntarily loses a specific job. For this purpose it is natural to use a control group of workers who remain at their employer and therefore do not lose this match-specific human capital.

This paper argues that the standard approach may be somewhat misleading when it comes to estimating the average treatment effect of displacement and the role of specific human capital. The major issues with the standard approach are that it looks at the effects of displacement using a control group of workers who are continuously employed in future sample periods and it imposes no prior displacements in the treated group.³ Consequently,

¹The literature in this area is long and distinguished. Earlier literature reviews include Hamermesh (1989), Fallick (1996), and Kletzer (1998). There has been much work in the area since then culminating in contributions such as von Wachter, Song and Manchester (2011) and Davis and von Wachter (2011).

²In some studies, such as Stevens (1997), researchers look at the consequences of the first displacement out of many displacement events using a control group of never-displaced workers. The analysis in the present paper applies equally well to these studies. For the purposes of the present work, the crucial ingredients in this prior work are that the control group experiences no displacements in future sample periods (positively-selected control group), and that, in the predisplacement period, treated workers do not experience displacement (selection on treatment event). The former is true in JLS because the control are continuously employed throughout the observation period and is true in Stevens' work because the control are the never-displaced. The latter is true in JLS due to the tenure requirement and is true in papers like Stevens (1997) because the event is, by definition, the first displacement. Stevens (1997) also looks at the effect of the last displacement. I address this approach in Appendix A.1.2.

³With the control group selection, the key issue is that the control group experience no nonemployment

even if the earnings effects of displacement are not long-lived, this approach attributes earnings declines from future job instability to the initial displacement event, thereby overstating the earnings losses associated with displacement. Put another way, this standard approach overstates the earnings losses of displaced workers relative to the true average treatment effect of displacement (downward biased).⁴

This paper proposes an alternative approach that, in essence, constructs the treatment group from individuals displaced in a particular year and the control group from individuals who do not experience a displacement this year, without conditioning on any events in other years. Compared to the standard approach, this alternative delivers an estimate for a different quantity: It estimates the effects of a job displacement relative to what would have happened had that displacement event not taken place in that year, acknowledging the risk of job loss in other periods. I show that, in simulated environments, this approach alleviates the major issues with the standard approach and delivers the correct average treatment effect. Moreover, this study highlights that even when interested in the role of specific human capital for earnings outcomes, the alternative approach is preferred over the standard approach. The estimates of displaced worker earnings losses from the standard approach include the downward bias highlighted above and so this approach overstates the role of specific human capital.

As with the simulated results, empirical results using the Panel Study of Income Dynamics (PSID) point to a systematic tendency for the predominant approach to yield estimates of the earnings losses associated with displacement that dramatically overstate the estimates of the alternative approach. In line with previous estimates of displaced worker earnings losses, the PSID data suggest annual earnings losses of over 25 percent, even 10 years after displacement, when one uses the standard method proposed by JLS. The alternative approach yields losses of around 5 percent 10 years after displacement. The difference between the two sets of estimates is economically and statistically significant.

Jacobson, LaLonde and Sullivan were aware of the distinction at the heart of the present paper. In their 1993 book they discussed this issue and were careful in how they defined (displacement) in future periods. As long as the treatment group are also continuously employed (not displaced) prior to the displacement event, conditioning on past employment (no displacements) in the control group is not an issue.

⁴There is also the more subtle issue that, since displacement tends to affect earnings adversely, using a control group of workers continuously employed during the observation period conditions on positive future earnings outcomes in this group. In a context where unobserved heterogeneity is important, this conditioning might be particularly problematic as it might imply a control group that is positively selected on these unobservable factors. This paper shows that even without this positive-selection issue, the standard approach overstates the earnings losses associated with displacement.

their control group.⁵ Indeed, they mentioned that the two approaches may give substantially different results if the probability of displacement in other periods, conditioning on no displacement in the current period, is high.⁶ They suggested that their estimates of the earnings consequences of displacement will tend to be larger than those implied by the approach outlined in this paper. It seems that as the displaced worker literature has proceeded, some of these earlier concerns have faded. In many regards, the current paper returns to, and investigates, the original remarks made by Jacobson, LaLonde and Sullivan (1993a).⁷

In addition to correctly estimating the earnings consequences of displacement, which is important in its own right, having an accurate and complete picture of the time-path of earnings around displacement serves to better inform and direct theoretical considerations aimed at explaining this phenomenon.⁸ As an example, if one documents a *permanent* reduction in earnings following displacement, as obtained with the standard approach in the PSID, this casts doubts on explanations appealing solely to specific human capital destruction or a loss in a particularly good-quality match, and grants more credence to stories focussing on loss of rents, information revelation or health effects. Similarly, documenting a persistent, but *temporary*, reduction in earnings following displacement, as obtained with the alternative approach in the PSID, brings human capital and matching explanations back into the picture. As researchers pursue the most promising theoretical frameworks to explain the earnings effects of displacement, it is important that the empirical benchmark is well understood and the average treatment effect is correctly measured.

Jung and Kuhn (2015) is a closely related paper. That paper uses a search and matching model with a life-cycle component to match the earnings losses of displaced workers. For agents within their model, the authors show theoretically that the JLS approach imposes upward bias (“selection effect”) on the magnitude of displaced worker earnings losses. They attribute this upward bias to the fact that workers in the control group are selected because they do not experience future nonemployment. The current paper adds to this work in two dimensions. First, I point out that, in addition to the positively-selected control group,

⁵See, for example, their discussion in Section 4.1 of their book, including Notes 1 and 2 at the end of Chapter 4.

⁶With the PSID data used in the present paper, I verify that this is a non-negligible concern in Section 6.2.

⁷Fallick, Haltiwanger and McEntarfer (2012) are concerned about the difference between a control group of stayers and a control group of non-displaced separators. Consequently, they present earnings results for “all separators,” “distressed separators,” (job separators from firms undergoing large employment losses) and “job stayers.” Flaaen, Sorkin and Shapiro (2015) use a control group of workers not displaced in a particular quarter. Neither paper compares the two approaches outlined in this paper using the same data.

⁸See Carrington and Fallick (2014) for a recent summary of the possible causes of earnings losses following displacement.

the standard approach imposes selection on the treatment event by conditioning on no job loss prior to the displacement event in the treated group. This places restrictions on the predisplacement earnings of the treated group. Second, I show that the critique of the standard approach is relevant in observed data using the PSID, whereas Jung and Kuhn (2015) show that this issue matters in the context of their model. In fact, I show that the empirical significance of these issues is potentially larger than what is suggested by the theoretical results of Jung and Kuhn (2015).

In the empirical job training literature, Sianesi (2004) and Fredriksson and Johansson (2008) both address some of the present issues. In that context, unemployed individuals can participate in job training on an ongoing basis. As a result, selecting a control group of workers who never participate in job training may choose individuals who have left unemployment for a job. In this context, this amounts to conditioning on future (successful) outcomes in the control group. Sianesi mentions that this evaluation problem arises in ongoing programs “that individuals sooner or later will join provided they are still eligible...” The current paper highlights that estimating the effects of displacement falls under the same evaluation problem: Sooner or later workers may experience displacement. The standard approach uses workers who are continuously employed (never displaced) and this can amount to selecting on favorable outcomes in the control group. The alternative approach in this paper uses a control group of individuals who can experience displacement in other years to deliver the correct average treatment effect.

The rest of the paper is organized as follows. Section 2 presents a simple framework that highlights the major issues. Even in a stylized environment that features no long-run effect from job loss, the “continuously-employed” approach results in estimated long-run earnings losses, equal to the probability of job loss. The “not-displaced-today” approach correctly estimates the average treatment effect and finds no long-run earnings consequences from displacement. In Section 3 I develop a more sophisticated search and matching model with human capital acquisition that delivers observed wage and employment dynamics. In this more realistic context, as well, the standard approach overstates the earnings losses relative to the alternative approach. Section 4 outlines the PSID data used in this analysis and presents some summary statistics. Section 5 outlines the two approaches used in the empirical analysis. Section 6 presents results for the two specifications and confirms the findings of the model simulations. This section also presents evidence that individuals not displaced in a given year are at risk of displacement in other years. Section 7 summarizes and briefly discusses the implications of the findings for a broader literature on event-studies.

2 Intuition

This section illustrates the bias than can result from following the standard approach and using a control group of continuously-employed workers to study the effects of initial displacements. The alternative approach is shown to deliver the correct average treatment effect. The models in this section are meant to be extremely simple to provide clear insights into the observed results. Section 3 presents a more sophisticated model that features realistic wage and employment dynamics.

2.1 Immediate Earnings Recovery

Suppose that all workers receive an identical wage w when employed, and a wage of zero when not employed. Suppose further that unemployed workers find a job with certainty but have to wait till next period to begin employment. Individual employment risk is governed by an exogenous separation shock which occurs with probability δ every period.⁹

In this context, define “displacement” as the exogenous separation shock. I simulate wage and employment data for 10,000 agents for a total of 200 periods and discard the first 100 periods. I initialize δ of the population to be unemployed in the first period. For the purposes of this simulation, I set w to 10. The probability of separation in a given period, δ , is set to 0.15. With this simulated data, I estimate JLS-type equations for earnings losses using the standard and alternative approaches discussed in the introduction.

In this simple example, the treatment effect of displacement in time period k on wages in time period k is

$$\mathbb{E}[w_k|D_k = 1] - \mathbb{E}[w_k|D_k = 0] = 0 - w = -10$$

where D_k indicates the displacement dummy in period k . The treatment effects does not vary with individual i as there is no heterogeneity. Notice that the treatment effect of displacement in period k on earnings in periods not equal to k (denoted by $-k$) is

$$\mathbb{E}[w_{-k}|D_k = 1] - \mathbb{E}[w_{-k}|D_k = 0] = \mathbb{E}[w_{-k}] - \mathbb{E}[w_{-k}] = 0$$

where the first equality follows from the fact that displacements have no persistent effect on earnings. In words, this says that there is no permanent wage reduction from job loss;

⁹This shock occurs before this period’s payment, so an individual may receive zero earnings in more than one period consecutively: He loses a job, finds a job with certainty, but experiences the separation shock before he is paid in the new job.

unemployment events are exogenously stipulated, and when a worker finds a job, his wage is the same as before the separation event.

For the alternative approach I estimate the following equation:

$$w_{it} = \alpha + \sum_{k=-6}^{10^+} D_{it}^k \delta_k + \epsilon_{it} \quad (1)$$

where w_{it} are earnings of individual i at time period t , D_{it}^k refer to any displacement event and are equal to 1 if individual i , at time period t , was displaced k periods ago, and α is an intercept. Notice that since the displacement dummies refer to any displacement event, an individual that is displaced twice can have more than one dummy “on” at a particular time. For example, suppose someone gets displaced in periods $t = 5$ and $t = 10$. In period 8, the individual has $D_{i,8}^3 = 1$ and $D_{i,8}^{-2} = 1$. Also notice, that the specification pools the last dummy to include periods 10 or more years after a displacement.

The “not-displaced-today” line in the top panel of Figure 1 presents the δ_k coefficients from estimating the above equation. Notice that this approach correctly predicts a treatment effect of $-w = -10$ on impact, and no effect before and after the displacement event. Using simple equations, Appendix A highlights that this is the result one would obtain with a control group of workers not displaced this year, but possibly experiencing displacement in other years. Intuitively, this approach takes into account all displacements and hence estimates the effects of an average displacement instead of the first displacement, as in the standard approach. This approach also uses earnings observations from people displaced, for example, once to identify the earnings effects of a second or third displacement, thereby implicitly including individuals displaced in other years in the control.

As an alternative, suppose we use only the first displacement event, so that the D_{it}^k dummies in equation (1) refer only to the first displacement.¹⁰ Notice that the control are those individuals at least 6 years before their first displacement and hence, implicitly, includes no displaced individuals. The “continuously-employed” line in the top panel of Figure 1 presents the δ_k coefficients from estimating this alternative specification. Notice that this approach correctly gives $-w = -10$ as the on-impact effect but incorrectly predicts losses equal to $-\delta w = -0.15 \times 10 = -1.5$ thereafter. Notice that the expected earnings of an individual in any given period are $(1 - \delta)w$ since the worker earns w with probability $(1 - \delta)$ and zero with probability δ . In this context, the standard approach overstates the long-run

¹⁰As explained in the introduction, for the critique outlined here of the standard approach, looking at the first displacement of many such events is the same as looking at one displacement event in a shorter observation window like in JLS.

earnings losses by $\delta w = 1.5$. This is precisely because the control group have not experienced displacement yet and the event is the first displacement. The intuition is that the treatment group experience displacement with probability δ every period after their first displacement (and not before), and the control group do not experience separations. As a result, the treated group appear to have permanently lower earnings following the displacement.¹¹

2.2 One-Period Delay in Earnings Recovery

Now perturb the previous simplified world in only one way: Suppose that the true earnings recovery is $0.5w$ (conditional on employment) in the period after displacement and then is back to w two periods after displacement (conditional on employment). In other words, displacement has a short-lived impact on earnings, but the long-run earnings losses are still exactly zero. In this simple environment, the expected wage for an individual who is employed (with probability $(1 - \delta)$) in a given period is:

$$\mathbb{E}[w|emp] = 0.5w\delta + w(1 - \delta)$$

This is because an employed individual can either be employed or unemployed last period. He experiences unemployment last period with probability δ , and this period will have wage $0.5w$. Alternatively, he was not unemployed last period with probability $(1 - \delta)$ and earns w this period because he is at least two periods after any potential displacement.

In this simple example, the average treatment effect of displacement in time period k on wages in time period k is

$$\mathbb{E}[w_k|D_k = 1] - \mathbb{E}[w_k|D_k = 0] = 0 - \mathbb{E}[w|emp] = -9.25$$

The treatment effect of displacement in time period k on wages in time period $k + 1$ is

$$\mathbb{E}[w_{k+1}|D_k = 1] - \mathbb{E}[w_{k+1}|D_k = 0] = [\delta \cdot 0 + (1 - \delta)5] - [\delta \cdot 0 + (1 - \delta)10] = -4.25$$

where $\mathbb{E}[w_{k+1}|D_k = 1]$ is equal to the expected earnings of an individual displaced last year. That is, with probability δ this person earns nothing in period $k + 1$ because they are displaced again, and with probability $(1 - \delta)$ they earn 5 because they were separated last period. By similar reasoning one can find the expression for $\mathbb{E}[w_{k+1}|D_k = 0]$, which is equal to the expected earnings of an individual not displaced last year. Notice that in all periods

¹¹Including individual fixed effects in this analysis does not alter these conclusions.

other than k and $k + 1$, the average treatment effect of displacement is zero because earnings losses persist for only one period.

The “not-displaced-today” line in the bottom panel of Figure 1 shows the results of estimating equation (1) with any displacement as the event.¹² On impact the earnings losses are exactly -9.25 ($= \mathbb{E}[w|emp]$), and in the period after displacement, they are -4.25 ($= -0.5w(1 - \delta)$). This method correctly captures the fact that there are no long-run earnings losses due to displacement, and estimates the treatment effect on impact relative to workers who are employed but not necessarily employed for more than one period. This is the correct average treatment effect.

The “continuously-employed” line in the bottom panel of Figure 1 also shows the δ_k coefficients from estimating equation (1) with the first displacement as the event. Notice that this approach correctly predicts no effect before displacement but incorrectly implies a treatment effect of $-w = -10$ on impact. In the period just after displacement, the model predicts earnings losses of -5.75 ($= -0.5w(1 + \delta)$), and, strikingly, losses of -2.14 ($= -0.5w\delta(3 - \delta)$) thereafter. These are both inflated compared to the actual losses: -4.25 ($= -0.5w(1 - \delta)$) and 0, respectively. This method incorrectly predicts long-run earnings losses for the same reasons as before: A continuously-employed control group and no prior displacements in the treatment group. Notice that, compared to the case where earnings recover immediately, the long-run earnings losses are even greater when the actual earnings recovery is more protracted ($2.14 > 1.5$).¹³

2.3 Many-Period Delay in Earnings Recovery

To anticipate the results from the live data in Section 6.1, here I present a scenario where the earnings recovery following displacement is protracted. The basic setup is the same as in the previous examples, but now separation has a long-lasting effect on earnings.¹⁴ I choose

¹²In this exercise I remove agents who have less than 10 earnings at the start of the sample, i.e., those who are unemployed in the first period and those who were unemployed in the period before the sample starts. This is as in Stevens (1997), where individuals at the start of the sample period who experienced a displacement in the 10 years before the survey started are dropped. For the purpose of the simulation, if I did not remove those with 5 earnings in the first period, the continuously-employed group would include such individuals, as they were displaced before the start of the sample. Hence, the average earnings without displacement is slightly under 10, and the treatment effect is slightly smaller than the current result (in absolute terms).

¹³Appendix A.2.2 shows explicitly why using any displacement as the event delivers the correct average treatment effect in this one-period-delay example.

¹⁴To be specific, the sequence of earnings (conditional on employment) following separation is: $0.52w$, $0.56w$, $0.60w$, $0.64w$, $0.67w$, $0.70w$, $0.73w$, $0.76w$, $0.79w$, $0.82w$, $0.85w$, $0.88w$, $0.91w$, $0.94w$, $0.97w$, and w . The recovery takes 16 periods. In the period of separation, the agent receives $0.45w$. This accommodates the annual time period. The separation probability, δ , is taken to be 0.15.

the earnings recovery postdisplacement to match the trajectory found in the data using the not-displaced-today approach, as appears in Section 6.1 (Figure 4).

Figure 2 presents the results of estimating both empirical approaches using data simulated from this simple numerical example. As in the previous examples, the standard approach implies larger earnings losses than the not-displaced-today approach at all horizons. Even 10 years after the displacement event, the standard approach yields large earnings losses, whereas the not-displaced-today-approach delivers a complete recovery.¹⁵ Also, notice that the difference between the two estimates grows with the time since the displacement event.

2.4 Intuition Summary

The approach that uses the continuously employed as a control group and focusses on the first displacement, incorrectly predicts long-run earnings losses in an environment with no actual earnings losses. In a simple environment where earnings recover immediately, this approach overpredicts the earnings losses by δ , the probability of separation in any given period. As the earnings recovery becomes more protracted, this approach produces even larger bias. An approach that looks at an average displacement, and in essence uses a control group of workers that are not displaced today, gives the correct average treatment effect.¹⁶ It is important to note that, even if the object of interest is the portion of wages attributable to being employed by a single employer, in this simple environment the standard approach overstates the role of specific human capital, while the alternative approach correctly predicts no job-specific returns.

3 Model with Human Capital Acquisition

This section develops a more sophisticated model featuring search and matching with human capital dynamics. The model presented here is similar in spirit to Ljungqvist and Sargent (1998). The goal of this exercise is to present a relatively straightforward way of incorporating realistic wage and employment dynamics alongside large and persistent earnings losses that are associated with displacement and labor market frictions. The model is also similar to a

¹⁵In this exercise I did not remove those individuals with less than 10 earnings at the start of the sample as in the previous example. This means that some individuals recently displaced are in the control group and hence bring down the average earnings of the control group from $w = 10$. This also explains why the not-displaced-approach yields positive long-run effects of displacement: Workers many years after their most recent separation have slightly higher earnings than this control group.

¹⁶Appendix A includes simple intuition for an approach recently used by Davis and von Wachter (2011) and shows how it relates to the two other approaches outlined here.

directed search model by Delacroix and Shi (2006) because workers only move slowly up the job ladder due to persistence in human capital embodied in outside offers. In that context, however, there are no *ex ante* sources of heterogeneity as in the present model.

3.1 Setup

In this environment unemployed workers encounter jobs at an exogenous probability, p_U . Workers in the model possess different amounts of human capital, denoted by h , the dynamics of which will be described below. All unemployed workers receive utility from leisure that is proportional to their human capital. Employed workers receive a flow payment, $w(h)$, and produce flow output. Employed workers participate in on-the-job search and encounter jobs at a different probability, p_E . All workers are endowed with linear utility (risk neutrality). When workers accept jobs, output is determined by a simple production function, h^γ , where γ denotes the concavity of the production function with respect to human capital. Employment relationships have an exogenous probability, p_s , of ending every period, which I will interpret as a layoff event. All workers are subject to a probability of dying between periods, η .

All unemployed workers accept their first job offer.¹⁷ When a worker is employed, human capital grows with certainty at rate μ .¹⁸ When employed workers encounter an outside job offer, they draw human capital from $F(h'|h)$, which depends on their current human capital level. One interpretation of this is that outside offers will come from jobs that are related to your current job tasks and capabilities. In the event of a layoff a fraction of human capital, α , is exogenously destroyed.

The timing of the model is as follows. At the outset of a period, unemployed workers encounter jobs at random. When an unemployed worker contacts a job, the worker and firm consummate the match. New matches wait until next period to produce with human capital h , where δ denotes the discount factor. For workers who were employed at the outset of the period, the timing for workers and firms is as follows. First, the firm and worker bargain over the wage. Second, production occurs, and the firm pays the worker. Third, the exogenous separation shock and the death shock occur with probabilities p_s and η , respectively. Fourth, human capital acquisition occurs. Finally, employed workers receive outside offers with probability, p_E . If an employed worker receives a favorable outside offer, he moves to the poaching firm. In this model, the human capital at the outside firm is

¹⁷In the calibration, even at the minimum level of human capital, it will be beneficial for matches to consummate.

¹⁸The model can also easily accommodate human capital depreciation while out of work, but this does not substantively affect the results presented here.

observable before transition decisions are made. In this sense, matches are inspection goods, as opposed to experience goods.

At the beginning of each period, every worker-firm pair bargains over the wage that the firm pays the worker for production. This model features a linear surplus-sharing rule, so that the worker (firm) receives a fraction, β ($1-\beta$), of the total match surplus. If an employed worker receives a favorable outside offer, he moves to the poaching firm and renegotiates his wage using unemployment as his outside option.¹⁹

3.2 Bellman Equations

This subsection deals with the formal recursive equations of the model. The value of work satisfies the following equation:

$$\begin{aligned}
 W(h) = & w(h) + \delta \underbrace{(1-p_E)(1-p_s)(1-\eta)W((1+\mu)h)}_{\text{Match continues}} + \delta \underbrace{p_s(1-\eta)U((1-\alpha)h)}_{\text{Exogenous separation shock}} \\
 & + \delta p_E(1-p_s)(1-\eta) \int \underbrace{\max\{W(h'), W((1+\mu)h)\}}_{\substack{\text{Worker stays at current firm,} \\ \text{or goes to new firm}}} dF(h'|h)
 \end{aligned} \tag{2}$$

The flow payoff from working is the wage, $w(h)$. The second term captures the situation where the worker does not die, does not experience a separation shock, and encounters no outside offers. In this case, the match continues with additional human capital in the next period. The third term captures the event of a separation shock, so that the worker flows into unemployment and a fraction of their human capital, α , is exogenously destroyed. The final term represents the expected payoff to meeting an outside employer. In this event, the worker can either stay at his current firm or move to the poaching firm, depending on the level of human capital in the new job.

The value for unemployment satisfies

$$U(h) = bh + \delta(1-p_U)(1-\eta)U(h) + \delta p_U(1-\eta)W(h) \tag{3}$$

The flow payoff is the value of unemployment insurance, which is set proportional to the individual's current human capital level. In the future, the worker can either remain unemployed or encounter a new job. In the latter case, employment begins with human capital h

¹⁹Nagypal (2007) also uses this convenience in an on-the-job search model. In the setup of Postel-Vinay and Robin (2002) workers can use the surplus at their previous firm as an outside option.

next period. Note that there is no human capital depreciation when unemployed, and the loss in human capital is deterministic when laid off (fraction α), although it would be a simple modification to the framework to allow for depreciation. It could easily be incorporated without changing the implications of the present paper.

The value of a filled job to the employer satisfies the following equation:

$$\begin{aligned}
J(h) = & h^\gamma - w(h) + \underbrace{\delta(1-p_E)(1-p_s)(1-\eta)J((1+\mu)h)}_{\text{Match continues}} \\
& + \delta p_E(1-p_s)(1-\eta) \int \underbrace{\mathbb{I}\{W((1+\mu)h) \geq W(h')\}}_{\substack{\text{Worker turns down} \\ \text{poaching firm}}} J((1+\mu)h) dF(h'|h) \quad (4)
\end{aligned}$$

The firm gains the output, h^γ , when matched with a worker and pays out wage, $w(h)$. The next term captures the continuation of the match with more human capital. The last term captures the situation where the worker gets an outside offer but turns it down, leaving the firm with the worker and added human capital.

3.3 Solving the Model

Solving the model involves solving for the surplus of a relationship, defined by $S(h) = W(h) + J(h) - U(h)$. I work through the details in Appendix B, and present the result here:

$$\begin{aligned}
S(h) = & h^\gamma + \underbrace{\delta(1-p_E)}_{\substack{\text{No outside} \\ \text{offer}}} \underbrace{(1-p_s)}_{\substack{\text{No separation} \\ \text{shock}}} \underbrace{(1-\eta)}_{\substack{\text{No death} \\ \text{shock}}} \underbrace{[S(h^+) + U(h^+)]}_{\text{Match continues}} \\
& + \delta p_E(1-p_s)(1-\eta) \int \left[\underbrace{\mathbb{I}\{\beta S(h^+) + U(h^+) \geq \beta S(h') + U(h')\}}_{\substack{\text{Worker turns down} \\ \text{poaching firm}}} \underbrace{[S(h^+) + U(h^+)]}_{\text{Match continues}} \right. \\
& \left. + \underbrace{\mathbb{I}\{\beta S(h^+) + U(h^+) < \beta S(h') + U(h')\}}_{\substack{\text{Worker leaves} \\ \text{current firm}}} \underbrace{[\beta S(h') + U(h')]}_{\text{Match at new firm}} \right] dF(h'|h) \quad (5) \\
& + \underbrace{\delta p_s(1-\eta)U((1-\alpha)h)}_{\substack{\text{Exogenous} \\ \text{separation shock}}} - \underbrace{U(h)}_{\substack{\text{Worker's outside} \\ \text{option}}}
\end{aligned}$$

where I have used h^+ to denote $(1+\mu)h$ to conserve space. Notice that this is a functional equation in $S(\cdot)$ and $U(\cdot)$, so iterating on this and the value of unemployment in equation (3) solves the model, where one can replace $W(h)$ in the value of unemployment with $\beta S(h) + U$

due to the linear surplus-sharing rule. Appendix B also shows how to solve explicitly for the wage, $w(h)$.

3.4 Calibration

This section discusses the process for the state variable, the calibration strategy, and the results of the calibration exercise for targeted moments.

3.4.1 Calibration Methodology

The model period length is one month. Newly born agents start out with human capital level h_0 , and then human capital accumulation follows this law of motion:

$$\ln h' = \begin{cases} \ln h & \text{for unemployed workers who remain unemployed or find work} \\ \ln(1 + \mu)h & \text{if employed worker does not switch jobs} \\ \rho_x \ln h + \epsilon'_h & \text{if employed worker transitions to new job (E} \rightarrow \text{E)} \\ \ln(1 - \alpha)h & \text{if job loss occurs (E} \rightarrow \text{U)} \end{cases} \quad (6)$$

This makes clear that workers who remain employed with the same employer from period to period see their human capital rise by fraction μ . Workers who switch employers draw a task that is related to their current task and choose whether to switch jobs or not. Unemployed workers do not lose human capital, although transitioning from employment to unemployment results in a fraction of human capital, α , being exogenously destroyed.

I use this human capital process and the fraction of human capital lost upon job loss, α , to target the earnings losses associated with displacement as measured by Davis and von Wachter (2011) (henceforth DV).²⁰ First, a larger α implies that more human capital is destroyed during a separation, and therefore this increases the on-impact earnings losses associated with displacement. Second, a larger ρ_h implies a slower earnings recovery after displacement, as it takes longer to find equally suitable job tasks. Third, a larger σ_{ϵ_h} implies a faster recovery, as individuals are faced with a larger dispersion in wages in outside jobs.

To compare the simulated and observed data, the simulated monthly wage information is aggregated into annual earnings data and the following equation is estimated, which is

²⁰The results presented in that paper are very similar to the earnings losses found with PSID data in Section 6.1 using either the DV empirical approach or the not-displaced-today approach of this paper.

equivalent to equation (1) in DV:

$$e_{it}^y = \alpha_i^y + X_{it}\beta^y + \sum_{k=-6}^{10^+} D_{it}^k \delta_k^y + u_{it}^y \quad (7)$$

where the superscript y denotes the displacement year, the outcome variable e_{it}^y is annual earnings of individual i in year t , α_i^y represents an individual fixed effect, X_{it} is a quartic polynomial in age of worker i at year t , D_{it}^k are dummy variables equal to one in the worker’s k th year before or after his displacement and zero otherwise, and the error u_{it}^y represents random factors. Note that $k = 1$ denotes the displacement year and $k = 0$ denotes the final year of positive earnings from the predisplacement employer. I omit time fixed effects because the model of this paper does not feature time variation in aggregate earnings. Exactly as in DV, I make the baseline seven and eight years before displacement. Although DV estimate this equation separately for each displacement year y , in the model presented in this paper all years are identical, so y is averaged over arbitrary years.

Sample selection follows DV exactly. I only include individuals in the treatment and control group if they have positive earnings in year y . I impose an identical tenure restriction on the sample: The worker must have positive earnings from the employer in question in $y-3$, $y-2$, and $y-1$. Furthermore, a worker “separates” from an employer in year y when he has earnings from the employer in $y-1$ but not in y and the worker experiences a separation into nonemployment in year $y-1$. Conditioning on job loss is important because a worker may not have earnings from his previous employer in year y because of an E-E transition. These workers are not included in the treatment or the control groups.²¹ I cannot impose the same “mass layoff” definition as DV because the model features one-worker firms.

For year y , the treatment group includes those workers displaced in year y , $y+1$ and $y+2$. Including workers from three years serves to smooth the estimated earnings effects of job displacement from year to year. The control group includes individuals with the same tenure requirement who remain with their employer in years y , $y+1$, and $y+2$. For the control group, $D_{it}^k = 0$ for all t so that the dummy variables reflect the change in earnings relative to this control group.

The human capital level for newborns, h_0 , is chosen to target the experience-earnings profiles documented in the decennial Census data by Elsby and Shapiro (2012). Although these profiles differ somewhat by education level, on average workers experience an increase

²¹DV cannot condition on separation into nonemployment because they use administrative earnings data; however they impose separation from a firm undergoing a “mass-layoff.” This restriction limits the amount of quitters in the treatment group.

of 1.1 in their log earnings over the first 30 years of labor market experience. As I increase the newborn level of human capital, h_0 , the rise in earnings associated with 30 years of experience in the model declines, allowing the model to match these empirical profiles.

I use the growth in human capital within a job, μ , to target the empirically estimated returns to tenure. I use the instrumental-variables approach proposed by Altonji and Shakotko (1987) to assess this in the model. This involves regressing log wages on a constant, time, tenure, tenure squared, and an indicator for the first year on the job and instrumenting for tenure by constructing within-spell deviations.²² Altonji and Williams (2005) present analyses that imply a return to 10 years of tenure of about 11 percent.

I use the exogenous separation probability, p_s , to target an average unemployment rate of 5.5 percent. Targeting a 22 percent job-finding rate, using p_U , implies that this exogenous separation probability is 1.3 percent. This is consistent with the average layoff probability using the Current Population Survey (CPS), as in Elsby, Hobijn and Sahin (2013). The observed average E-E transition probability is targeted using the contact rate for the employed, p_E . Raising the number of encounters employed workers have with outside firms raises the probability that workers experience E-E switches. Intuitively, this implies that E-E flows in the model are monotonically increasing in p_E . The PSID data imply that an average of 1.8 percent of employed persons change employers each month.²³

The concavity of the human capital production function, γ , is based on previous human capital literature. It is close to the estimate found in Song and Jones (2006) (0.58) and below the estimate in Heckman, Lochner and Taber (1998) (0.81), but consistent with earlier work on human capital technology (See, for example, Ben-Porath, 1967; Heckman, 1976; Brown, 1976). The value of leisure, b , is chosen so that this flow payoff from leisure is 71 percent of average labor productivity (ALP) in the economy. This target is in between the wide range found in Shimer (2005) (0.4) and Hagedorn and Manovskii (2008) (0.955), and equal to the value found in Hall and Milgrom (2008). The monthly probability of death, η , is chosen to target life expectancy of 40 years.

²²I use average wages over employment spells in year t as a measure of the annual wage. I use the tenure (in years) in the eighth month of each year t as the measure of tenure. As in Altonji and Shakotko (1987), I include a cubic in experience in the specification.

²³See Krolkowski (2015) for average flows using monthly labor-force-status data from the PSID for survey years 1988 to 1997.

3.4.2 Calibration Outcomes

The current version of the model has seven free parameters and eight moments. The seven free parameters are ρ_h , σ_{ϵ_h} , h_0 , μ , α , p_E , and b .²⁴ The moments include the observed earnings recovery around displacement,²⁵ the (log) rise in earnings over the first 30 years of experience, the empirical returns to tenure, the average E-E probability, and the flow unemployment payoff as a fraction of average labor productivity.

Table 1 summarizes the baseline parameters and the targeted empirical outcomes. Table 2 displays the simulated moments at the calibrated parameter values and shows that the model matches the calibration targets well. The model delivers a realistic worker environment by matching average worker flows, the payoff to unemployment, returns to tenure, and life-cycle earnings growth.

Figure 3 compares the observed earnings trajectory of displaced workers and the one implied by the model. The empirical results are from DV (Figure 4, expansion results), and the model results are the estimated coefficients δ_k from equation (7) as a fraction of average predisplacement earnings of the treatment group in the four years prior to displacement. The model delivers an on-impact earnings reduction of around 30 percent and a relatively healthy recovery over the next 10 years.²⁶

Figure 3 also shows the estimates from using the standard approach to measuring the earnings losses of displaced workers. These estimates come from estimating equation (7) for all years and specifying that the displacement dummies, D_{it}^k , refer only to the first displacement.²⁷ On impact, the standard approach implies similar earnings losses to the approach of DV. Over the next 10 years, however, the standard approach suggests virtually no recovery in earnings. As with the intuitive examples, the difference between the two estimates grows with the time since the displacement event. As we will see in Section 6.1, these results from simulated data are remarkably similar to the estimated earnings losses in the PSID using the two specifications.

²⁴The parameters p_s and p_U are chosen externally to match the average unemployment probability and the average job-finding rate, respectively, and the concavity of the production function, γ , is calibrated to 0.65, in accordance with the human capital literature.

²⁵In essence this is four moments: the estimated coefficients δ_{-6}^y , δ_{-1}^y , δ_1^y , and δ_{10+}^y from equation (7) averaged over y .

²⁶To remain consistent with DV, I use their approach to measure the earnings losses associated with displacement in this exercise. Using the “not-displaced-today” approach outlined above gives similar results, although the earnings losses are slightly larger compared to the DV approach. Appendix A relates the two approaches and provides intuition for why the DV approach might deliver smaller losses.

²⁷I estimate the equation with a constant but no individual fixed effects.

4 Data

The empirical analysis uses an unbalanced panel version of PSID waves from 1968 to 2009, incorporating both the nationally representative sample and the poverty oversample.²⁸ The strength of the PSID for this study is that workers have long histories, allowing precise estimation of individuals' fixed effects, and individual time trends. The sample consists of household heads who, from their first observation as household heads, have at least three consecutive observations, and only observations for individuals between the ages of 18 and 65 are used. The final sample includes 14,543 household heads with an average of 14 years of observations for each household head, yielding 208,615 observations.

Job displacements are determined from a question that asks respondents with low levels of current job tenure: "What happened to that employer (job)?" (the individual's previous job). The two categories of responses used to identify displacements are "plant closed/employer moved" and "laid off/fired." In the final sample, there are 7,779 displacements with 5,035 first displacements.

As is well documented by previous authors, the year of displacement is measured with error in the PSID. The respondent's answers about earnings and employment refer to the previous calendar year. For the first 16 waves of the PSID, the survey asks what happened to the last job for those reporting a job tenure of less than one year. Subsequent surveys ask what happened to the previous job if the current job started on or after January 1 of the previous calendar year. Due to the timing of the interviews, job displacements may have occurred either during the previous calendar year or during the first few months of the current calendar year. For this study, a recorded displacement is assumed to have occurred during the survey year. Finally, as is common with displaced-worker studies using the PSID, household heads who report a displacement in the 1968 wave are excluded from the analysis because this displacement may have occurred any time in the 10 years prior to the survey.

Summary statistics for never-displaced household heads and displaced household heads are presented in Table 3. Consistent with previous literature, displaced individuals tend to be younger than the never-displaced at the time of the shock. Around 65 percent of household heads report never being displaced, and 35 percent report at least one displacement.

²⁸Both samples are used to increase sample size, and individual weights are used to maintain national representativeness and to deal with nonrandom attrition. The baseline results are similar with unweighted observations using only the nationally representative sample. The analysis excludes the Latino and immigrant supplement samples. Household-head-earnings data are available, using supplement files, for all years except 2005.

5 Empirical Methodology

Authors have used different specifications to assess the earnings losses of displaced workers. The standard approach focuses on the first displacement and, with some slight variations, takes the following form:

$$e_{it} = \alpha_i + \gamma_t + \lambda_i t + X_{it}\beta + \sum_{k=-m_l}^{m_u^+} D_{it}^{first,k} \delta_k + \epsilon_{it} \quad (8)$$

where e_{it} are the annual earnings of household head i at time t , α_i and γ_t represent individual and time fixed effects respectively, λ_i allow for individual linear time trends, and $D_{it}^{first,k}$ equal one if individual i was displaced for the first time k periods ago at time t . X_{it} captures a quartic in worker wage. The analysis treats those m_l periods before a worker’s first displacement as part of the control group, and those m_u or more years after their first displacement are captured in the last dummy variable. I set m_l to 4 and m_u to 11.²⁹ Here the ‘+’ denotes that those m_u periods or more after displacement are pooled to estimate the coefficient on the last dummy. Note that the control group for this regression consists of workers who do not experience a displacement in the relevant window because the control group is composed of individuals who are at least m_l years before their first displacement. Alternatively, these are individuals who are not displaced yet. Similarly, the treatment group have no displacements before the event of interest, which is the first displacement. This specification finds its roots in JLS and has been used for decades in studies such as Stevens (1997), Stephens (2001), and Couch and Placzek (2010). I refer to this specification as the “continuously-employed” specification.

I include observations with zero earnings in my analysis and report losses as a fraction of average earnings prior to displacement.³⁰ These observations constitute a significant portion of the observations in my analysis.³¹

The alternative approach does not use the first displacement event, but rather uses information on any displacement. Mechanically, this means that the D_{it}^k dummies now refer to

²⁹The astute reader may notice that in the simulations in Section 2 m_l was set to 6 and m_u to 10. That is because the simulations have the largest fall in earnings in year “0” and in the data this occurs in year “1” so I include an additional postdisplacement dummy in the empirical exercise, and adding one more predisplacement dummy does not substantively affect the empirical results.

³⁰Sometimes authors estimate this equation after removing observations with zero earnings. This is because, to capture the portion of wages lost due to breaking the relationship with one employer, individuals have to be observed working again. Estimating the equations where observations with zero earnings are excluded does not alter the conclusions of this paper.

³¹As an example, using the PSID sample in this paper, in 1980 almost 12 percent of the sample report zero labor income in the previous year.

any displacement, not just the first displacement. As an example, if individual i experiences displacement in 1980 and 1985, the dummy variables $D_{i,1983}^3 = 1$ and $D_{i,1983}^{-2} = 1$. This makes clear that in a given year, for individuals who experience more than one displacement, more than one displacement dummy will be turned “on.” As with the first specification, I express these losses as a fraction of the treatment group’s average predisplacement annual earnings. I refer to this specification as the “not-displaced-today” specification.

6 Empirical Results

This section presents results for the two different earnings specifications outlined in this paper using observed PSID data. I also document the probability of displacement for individuals not displaced in a particular year, highlighting that these individuals are at risk of displacement in other years.

6.1 Displaced Worker Earnings Losses

The two approaches outlined in the previous section yield potentially different results. Using the PSID, I estimate both equations and present the results in Figure 4, along with the 95 percent confidence interval for each approach. Standard errors are calculated using the bootstrap approach (percentile bootstrap). I bootstrap at the person level to preserve any serial correlation structure that the data might exhibit.³² The point-estimates here are the δ_k coefficients from equation (8) divided by the average earnings of the treatment group over the four years prior to initial displacement.³³ With both approaches I use data from 1968 through 2009.

Looking at the point-estimates, the two specifications paint a different picture of the earnings consequences for displaced workers. On impact, the standard and alternative approaches yield comparable estimates of earnings losses: around 30 percent. This suggests that the choice of empirical model is not crucial for the on-impact effect of displacement on

³²I could also calculate the standard errors clustering at the individual level. The resulting confidence intervals are almost identical to the ones presented here. I choose the bootstrap approach here for readability. In particular, when I look at the confidence interval for the difference between the standard and alternative approaches, the bootstrap is easier to implement and is more transparent.

³³To implement this regression I first-difference the data and then estimate the equation with an individual fixed effect and year dummies. This is preferred to double differencing the data to remove the individual fixed effect if the residuals from the estimated double-differenced equation are serially correlated (see Wooldridge (2002), Section 10.6.3). Estimating the equation $\Delta^2 \hat{\epsilon}_{it} = \hat{\rho}_1 \Delta^2 \hat{\epsilon}_{i,t-1} + error_{it}$ for the first-displacement approach yields $\hat{\rho}_1 = -0.85(0.003)$. This suggests double-differencing the data is not the appropriate way to proceed.

earnings, although, as the intuitive examples suggest, the standard approach yields a larger on-impact effect. The recovery, however, is strikingly different for the two specifications. The alternative approach suggests a 20 percentage-point earnings recovery in the 10 years following displacement, with the earnings losses being indistinguishable from zero nine years after the displacement event. In contrast, the standard specification suggests that earnings remain permanently lower after displacement, showing almost no recovery even 10 years following the initial displacement event. As with the intuitive examples, the difference between the two estimates grows with the time since the displacement event. The results mimic very closely the results from the model with human capital in Section 3 (Figure 3). Although both empirical specifications point to tremendous costs associated with job displacement, the not-displaced-today approach depicts a healthy recovery in earnings in the decade following displacement, whereas the continuously-employed approach suggests a permanent decline in earnings. Six years after the displacement event the selection effects account for around 60 percent of the total earnings losses. This is larger than the 30 percent figure that Jung and Kuhn (2015) find in their theoretical setup, and points to substantial selection effects using the standard approach with observed data.

The confidence intervals in Figure 4 are large, but they are not appropriate for performing statistical inference on the difference between the two approaches. Figure 5 plots the difference between the two approaches, with the 95 percent confidence interval calculated using the bootstrap (percentile bootstrap). As before, I bootstrap at the person level to preserve any serial correlation structure that the data might exhibit. In the short-run the two approaches give earnings losses that are indistinguishable. At longer horizons, however, the standard approach implies larger earnings losses than the alternative approach, and the pointwise differences are statistically significant.³⁴

6.2 Displacement Probabilities

The main message of the intuitive examples in Section 2 is that conditioning on no job loss in the control group for displaced workers builds in a wedge between the earnings of treated and control individuals. However, *ex ante* it is not clear that those not displaced in a given year are at risk of job loss in other periods. To document this fact, I estimate

³⁴Looking at the confidence interval around the difference between the two approaches helps with establishing statistical significance because

$$\mathbb{V}[A - B] = \mathbb{V}[A] + \mathbb{V}[B] - 2\text{COV}[A, B] \tag{9}$$

and the covariance between the estimates of the two approaches is large.

equation (7) for every year, y , but with a dummy for displacement on the left-hand-side, as opposed to earnings, and without the individual fixed effect and age quartic (and no constant). The coefficients on the displacement dummies now estimate the probability of displacement around a year, y , for individuals who are displaced and not displaced in a given year. To obtain one estimate, I average over the coefficients from all years y , running from 1972 to 1997.

Figure 6 shows the probability of displacement for those in the treatment and control groups for all years around year y . The figure shows that those not displaced in a particular year are in fact at risk of job loss in other periods. In the year after the event, nondisplaced workers have a persistent 2 percent probability of experiencing displacement. Although this is below the average probability of displacement among the treated, who were displaced in year y , it is only slightly smaller than the average probability of displacement in the sample, around 2.5 percent. In fact, as soon as the restriction of no displacements in the control group is relaxed, in year $y + 3$, the probability of displacement jumps up to 2 percent. In their book, Jacobson, LaLonde and Sullivan (1993a) mention that their approach may give substantively different results if the probability of displacement in other periods, conditioning on no displacement in the current period, is high. This figure serves as evidence that this may indeed be the case, confirming that choosing a control group of continuously-employed workers may overstate the earnings losses associated with displacement.

The figure also shows a substantial probability of displacement among the treated in the years before y . This highlights that conditioning on the first displacement in the standard approach results in selection on the treatment event, or “conditioning on good luck” predisplacement in the treatment group.

7 Conclusion

This paper implements two empirical specifications to assess the earnings consequences of worker displacement. The standard approach, which has dominated the field, uses those not displaced during the whole period of observation as a control group, and focuses on the first displacement event. Another implementation uses a control group of individuals who are not displaced in a particular year, but could be displaced in other years, and looks at any displacement. This paper shows that the two specifications identify different parameters and give different notions of the recovery of displaced worker earnings. Since displacements tend to affect earnings adversely, using those not displaced during the period of observation as a control group conditions on future (favorable) earnings outcomes. Hence, this approach

implies significantly larger earnings losses than the alternative specification. This is shown with simple, intuitive examples, as well as in the context of a model with human capital acquisition and realistic wage and employment dynamics. Empirical results using the PSID confirm these theoretical findings. In the PSID data, the standard approach yields annual earnings losses of over 25 percent, even 10 years after displacement. The alternative approach yields losses of around 5 percent 10 years after displacement, losses that are statistically indistinguishable from zero. The difference in estimates between the two approaches are economically and statistically significant.

Intuitively, the standard approach yields significantly different results from the not-displaced-today approach when the incidence of the event is large and the event has a persistent effect on the outcome variable. The issues presented here apply in a broader context of event-studies where the control group are chosen to never experience the “event.”

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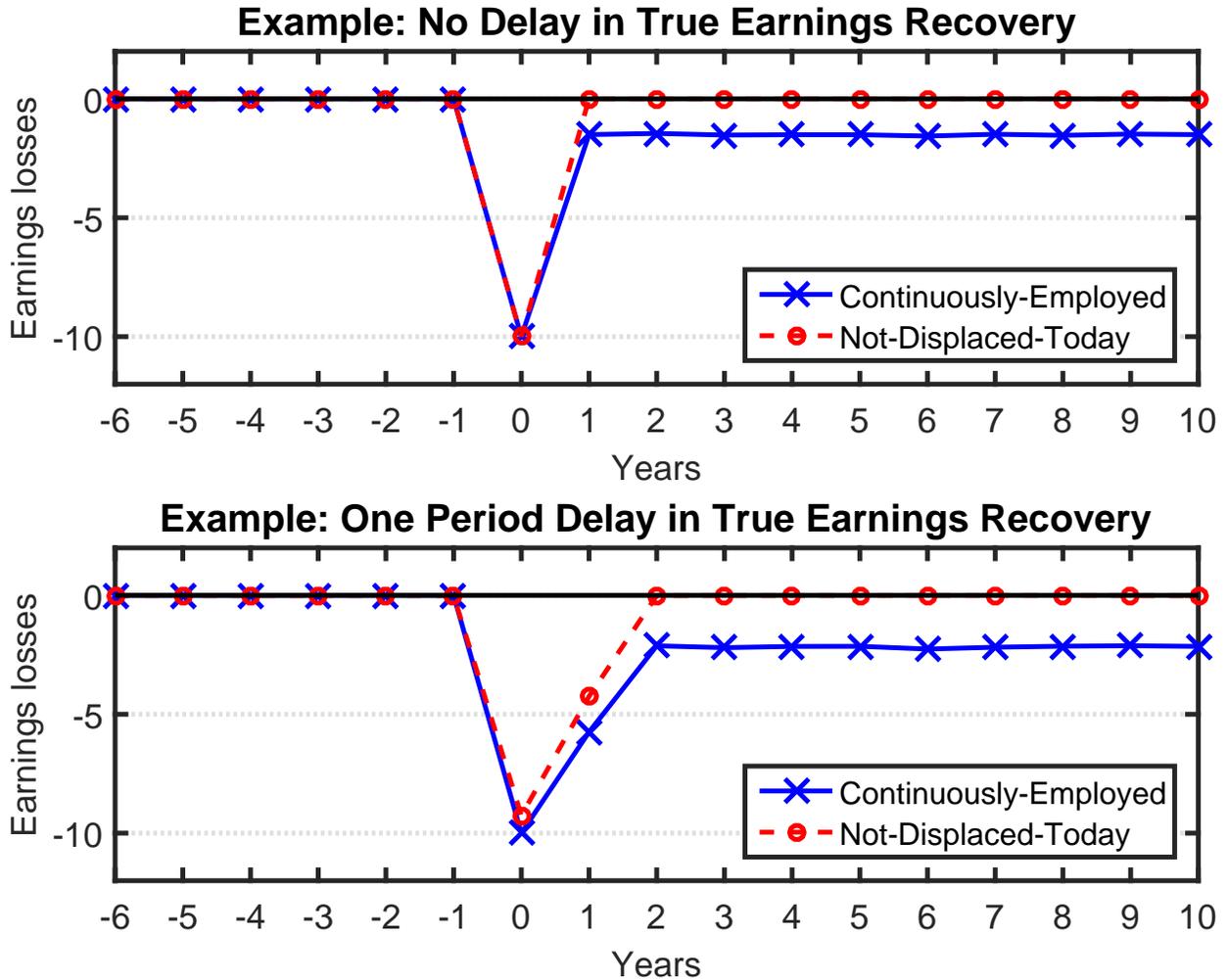


Figure 1: Simulation Results: Continuously-Employed and Not-Displaced-Today Approaches

Note: In the example where the earnings recovery following job loss is immediate, the continuously-employed approach overstates the long-run earnings losses by $\delta w = 1.5$. The not-displaced approach correctly predicts an on-impact effect of $-w = -10$ and long-run earnings losses of zero. In the example where the true earnings recovery takes one period, the continuously-employed approach overstates the long-run earnings losses by $-0.5w\delta(3 - \delta) = -2.14$. The not-displaced approach correctly predicts an on-impact effect of $-\mathbb{E}[w|emp] = -9.25$ and long-run earnings losses of zero.

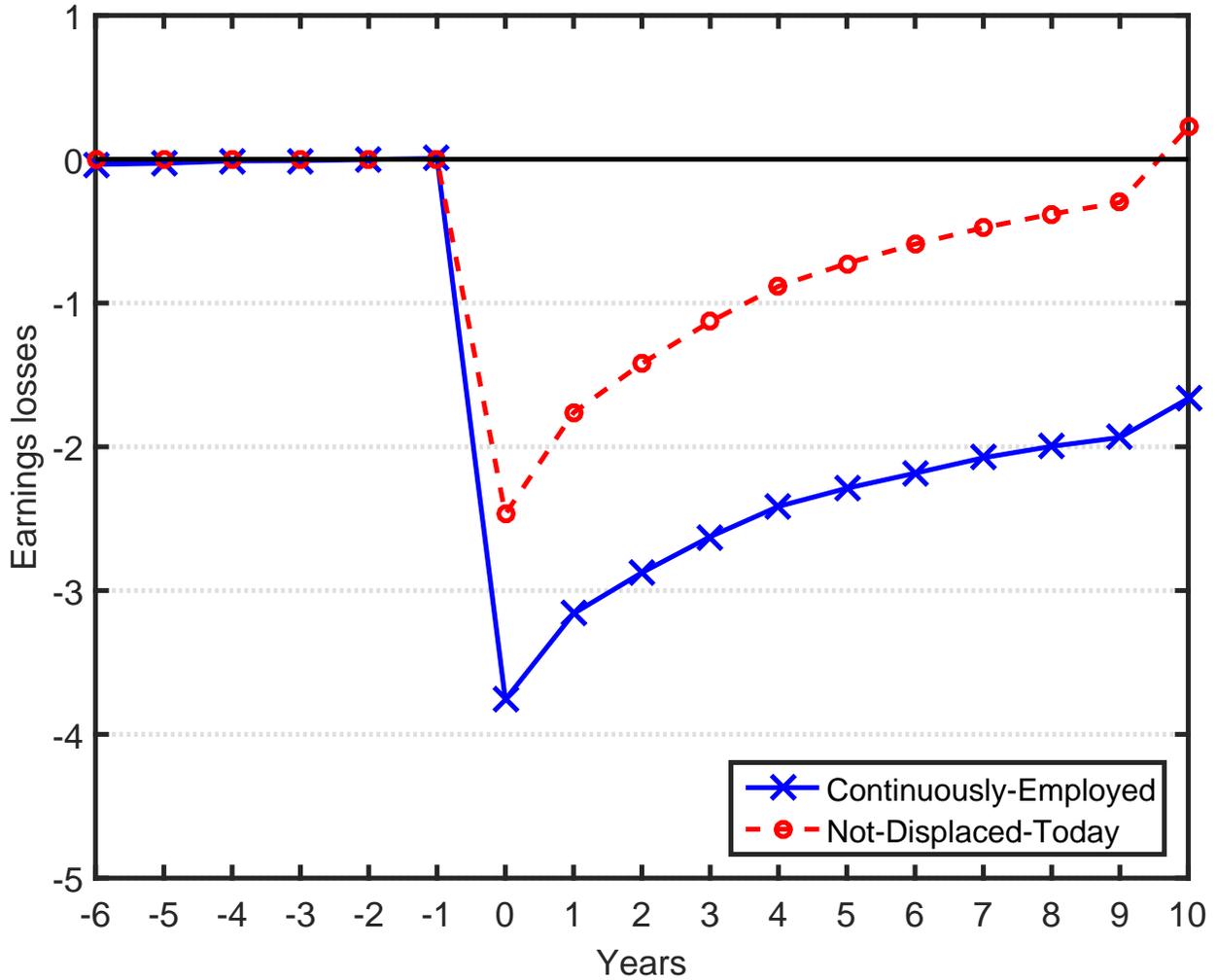


Figure 2: Simulation Results: Sixteen Period Earnings Recovery

Note: As with the live data, the continuously-employed approach implies much larger earnings losses relative to the not-displaced-today approach for all horizons. Even 10 years after the displacement event, the continuously-employed approach delivers large earning losses, whereas the not-displaced-today approach delivers a complete recovery. The earnings recovery was chosen to match the shape of the not-displaced-today estimates in the data, found in Figure 4. In this exercise I did not remove those individuals with less than 10 earnings at the start of the sample as in the one-period-delay example. This means that some individuals recently displaced are in the control group and hence bring down the average earnings of the control group from $w = 10$. This also explains why the not-displaced-approach yields positive long-run effects of displacement: workers many years after their most recent separation have slightly higher earnings than this control group.

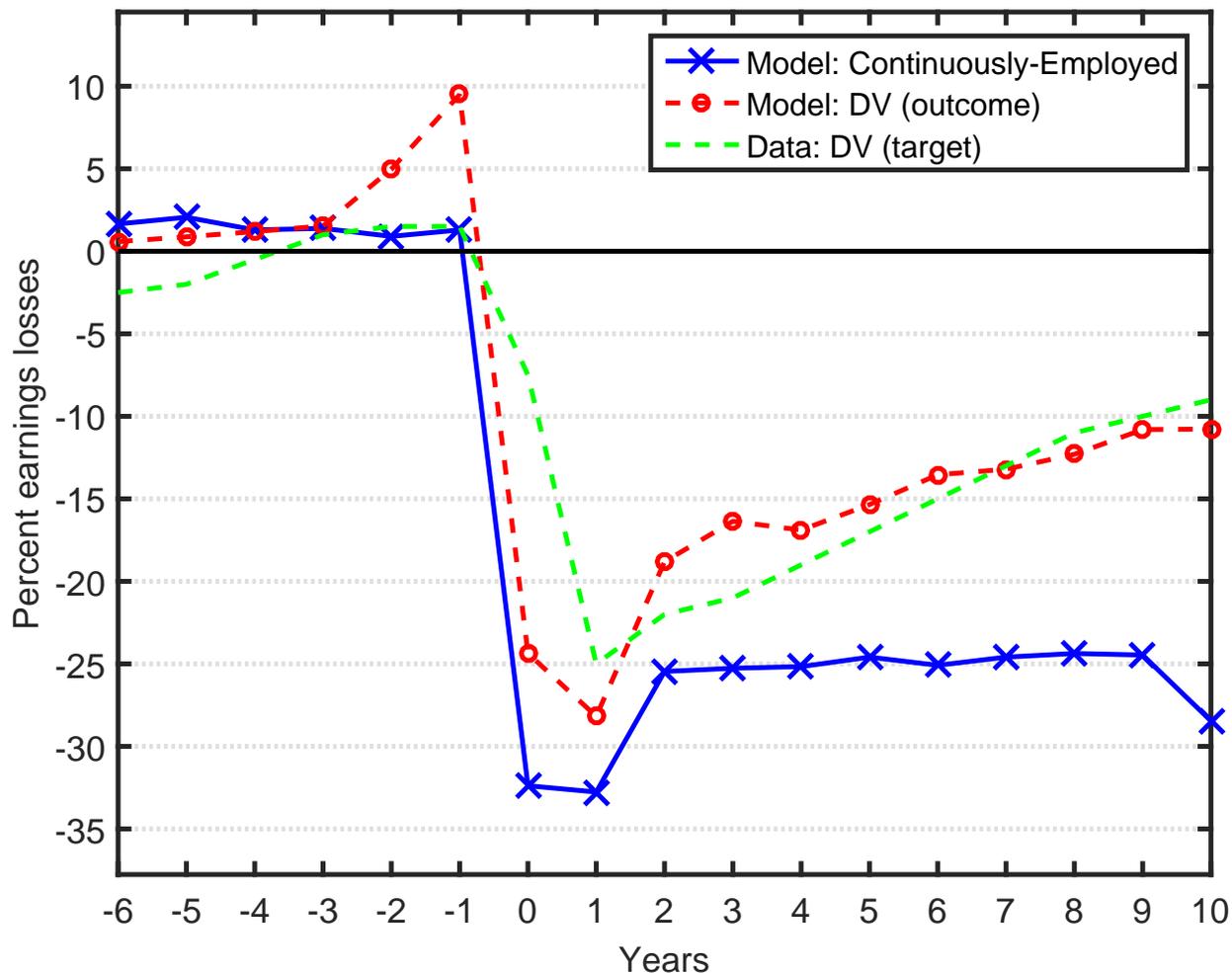


Figure 3: Earnings Losses in the Model with Human Capital

Note: The model with human capital accumulation matches the observed earnings losses around displacement. The empirical time-path is from Davis and von Wachter (2011) (Figure 4, expansion results), and the model results are the estimated coefficients δ_k from equation (7) as a fraction of average pre-displacement earnings of the treatment group in the four years prior to displacement. The approach using a control group of continuously-employed workers provides similar estimates of the earnings losses in the short run but suggests no recovery in the next 10 years. The “continuously-employed” results come from estimating equation (7) for all years and where the displacement dummies, D_{it}^k , refer only to the first displacement. Equivalently, this is similar to equation (8) but without the time fixed effects and the individual linear time trends since these features do not exist in the model. The earnings losses from these two specifications are remarkably similar to the results with observed PSID data in Figure 4. ‘DV’ stands for Davis and von Wachter (2011).

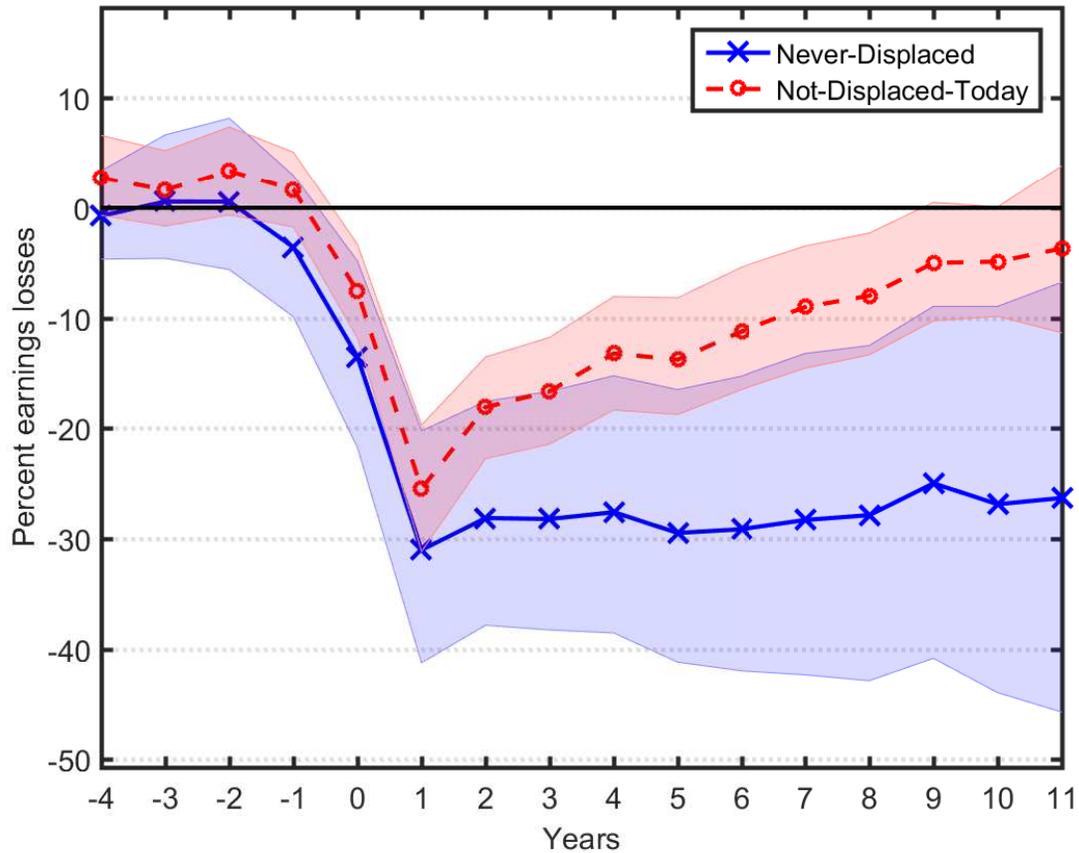


Figure 4: Effect of Displacement on Head’s Income: Continuously-Employed versus Not-Displaced-Today Specifications Using PSID Data

Note: The estimated long-run earnings losses fall dramatically from 25 percent when using the standard approach to 5 percent when using the alternative approach. Both equations include dummies four years before the displacement shock and 11+ years after the displacement shock. The control group for the not-displaced-today approach are those not displaced in a given year, but can be displaced in other years. The control group for the continuously-employed approach are those who are at least four years before their first displacement, i.e. those not having experienced a displacement yet. The analysis applies individual weights from the PSID, but the results are similar with unweighted observations using only the Survey Research Center (SRC) sample. Shaded regions correspond to 95 percent confidence intervals, with standard errors computed using the bootstrap method with 1,000 replications (percentile bootstrap).

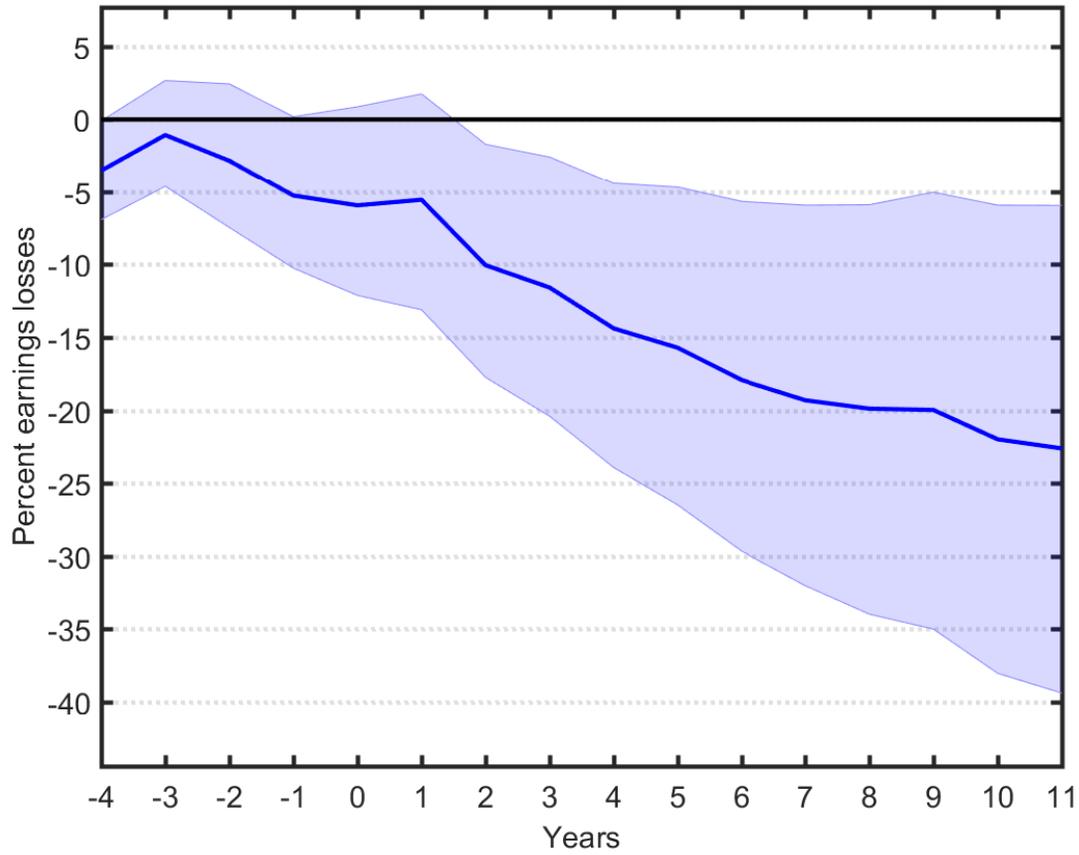


Figure 5: Bootstrapped Confidence Interval for Difference Between Continuously-Employed and Not-Displaced-Today Specifications

Note: The difference between the continuously-employed and not-displaced-today approaches is economically and statistically significant at longer horizons. The solid line is the difference between the continuously-employed and not-displaced-today approaches as in Figure 4. The shaded area represents the bootstrapped 95 percent confidence interval (percentile bootstrap). I bootstrap at the person level to preserve any serial correlation structure that the data might exhibit. I use 1,000 bootstrap replications.

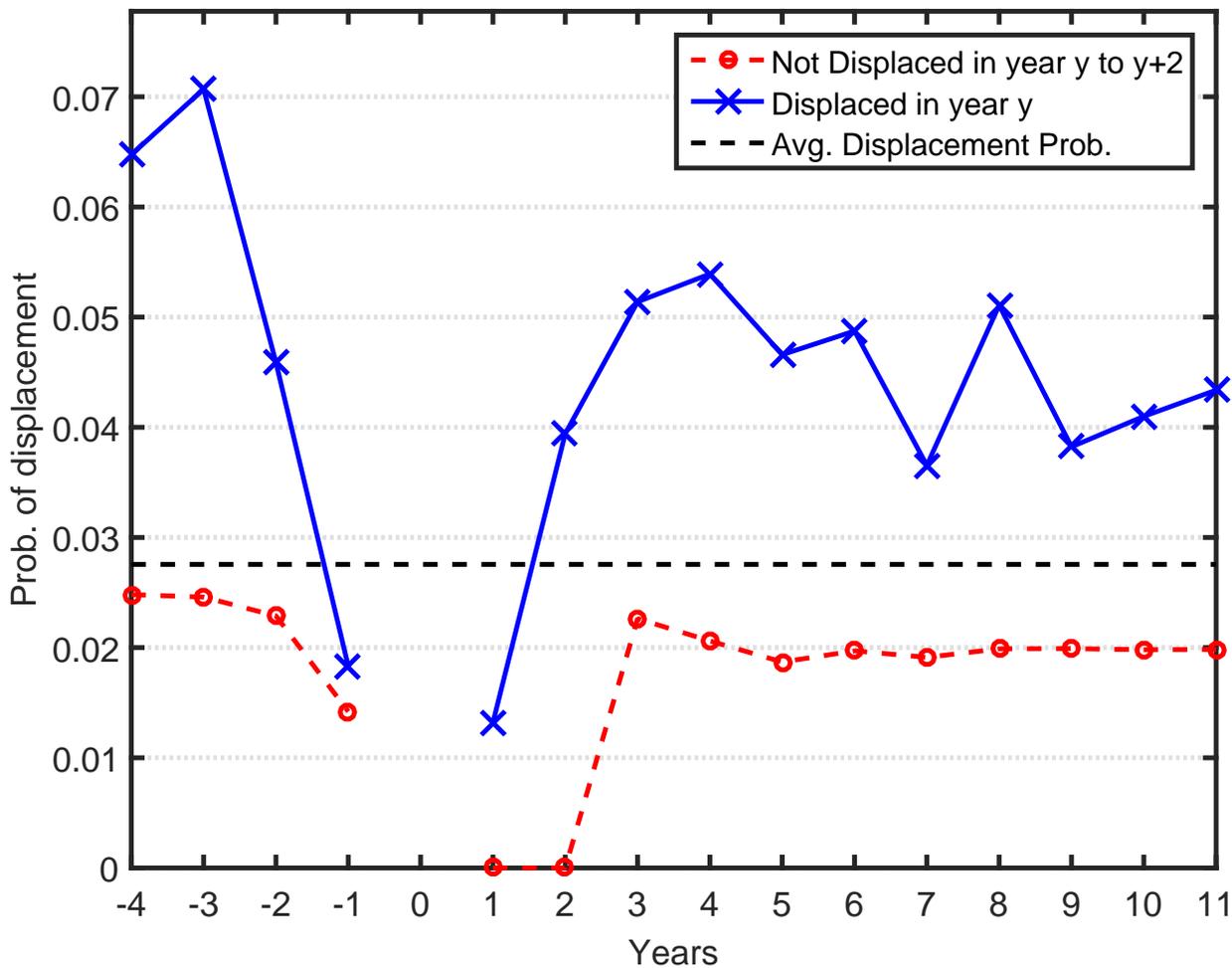


Figure 6: Probability of Displacement for Treated and Control

Note: Those not displaced in year y through $y + 2$ have a significant probability of displacement in other years. This figure shows the probability of displacement for individuals displaced in year y , $y + 1$ or $y + 2$, and those not displaced in years y through $y + 2$, averaged over 1972 to 1997 in the PSID. To raise sample sizes, just like in Davis and von Wachter (2011), the treated are individuals who are displaced in years y , $y + 1$ or $y + 2$. In the figure '0' refers to their actual year of displacement. I have omitted the estimates in this year because the estimate for the treated is equal to one (these individuals are by definition displaced in year y), and this distorts the figure's vertical scale.

Table 1: Calibrated Parameters for the Model with Human Capital

Parameter	Meaning	Calibrated Value	Main Source of Identification
ρ_h	Persistence of HK offers	0.67	Earnings recovery in DV
σ_{ϵ_h}	Std. dev. of HK offers	0.25	Earnings recovery in DV
h_0	HK level for newborns	0.36	Experience-earnings profile
γ	Concavity of production function	0.65	HK literature
μ	HK growth on the job	0.0018	Returns to tenure
α	Fraction HK lost upon job loss	0.33	On-impact earnings loss in DV
p_E	Contact probability (E)	0.031	E-E flow probability
p_U	Contact probability (U)	0.22	U-E flow probability
p_s	Exo separation probability	0.013	5.5% unemployment rate
b	Value of leisure	0.67	b/ALP
β	Worker's bargaining power	0.5	Benchmark
δ	Discount factor	0.9959	5% annual interest rate
η	Exo death probability	0.0021	40 year life expectancy

Note: Calibrated parameters of the human capital model (Section 3) at monthly frequency. ‘HK’ stands for human capital, and ‘ALP’ stands for average labor productivity. The citations and values of these empirical moments appear chiefly in Table 2, along with Figure 3. ‘DV’ stands for Davis and von Wachter (2011).

Table 2: Calibration Targets

Moment in the data	Data	Model
Rise in (log) earnings after 30 years experience	ES: 1.1	1.1
Returns to 10 years of tenure	AW: 11%	11%
E-E flow probability	Kr: 1.8%	1.8%
U-E flow probability	Kr: 22%	22%
Unemployment rate	5.5%	5.5%
b/ALP	HM: 0.71	0.72

Note: The model with human capital matches the empirical targets well. The middle column of this table presents the value of the moment in the data and the citation. The column on the right presents the value of the equivalent moment in the model at the calibrated parameter values. ‘ALP’ stands for average labor productivity. All probabilities are at the monthly frequency. See Figure 3 for the earnings time-path around displacement in the data and the model. ‘ES’ stands for Elsby and Shapiro (2012), ‘Kr’ stands for Krolikowski (2015), ‘AW’ stands for Altonji and Williams (2005), and ‘HM’ stands for Hall and Milgrom (2008).

Table 3: Summary Statistics

	Never Displaced	First Displacement	Any Displacement
Head’s age	40.3	33.4	35.0
Head’s annual earnings (\$)	42,231	34,086	32,114
Head’s hourly earnings (\$)	23.1	17.9	17.3
Fraction of household heads	64.7	35.3	35.3

Note: Unweighted tabulations using unbalanced data from the 1968-2009 PSID surveys. Dollar figures are in 2007 dollars using the CPI-U-X1. Averages for the never-displaced individuals are calculated using every observation for these individuals. Averages for displaced individuals are calculated using the observation from the year of the shock. Pre-displacement wages and earnings are taken from two years prior to the shock.

A Appendix: Equations for Simple Intuition (For Online Publication)

This appendix presents theoretical results that confirm the simple intuition presented in Section 2, as well as intuition for a recent approach used by Davis and von Wachter (2011).

A.1 Immediate Earnings Recovery

A.1.1 Alternative and Standard Approaches

To find the treatment effect using the two different empirical approaches, I will use conditional expectations. Let time period 0 be the period of displacement, and let k represent the time since displacement. Notice that the expected earnings of an individual in any given period are $(1-\delta)w$ since the worker earns w with probability $(1-\delta)$ and zero with probability δ . Then the time-path of earnings for individuals that experience displacement in period 0 ($D_0 = 1$) in this simple model is:

$$\begin{aligned}\mathbb{E}[e_k|D_0 = 1] &= (1 - \delta)w, \forall k < 0 \\ \mathbb{E}[e_k|D_0 = 1] &= 0, k = 0 \\ \mathbb{E}[e_k|D_0 = 1] &= (1 - \delta)w, \forall k > 0\end{aligned}\tag{10}$$

The time-path of earnings for individuals not displaced at time 0 is:

$$\begin{aligned}\mathbb{E}[e_k|D_0 = 0] &= (1 - \delta)w, \forall k < 0 \\ \mathbb{E}[e_k|D_0 = 0] &= w, k = 0 \\ \mathbb{E}[e_k|D_0 = 0] &= (1 - \delta)w, \forall k > 0\end{aligned}\tag{11}$$

To obtain the treatment effect when using those not displaced at time 0 as the control group, subtract (11) from (10) to yield:

$$\begin{aligned}\mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= 0, \forall k < 0 \\ \mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= -w, k = 0 \\ \mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= 0, \forall k > 0\end{aligned}\tag{12}$$

Notice that this approach correctly predicts a treatment effect of $-w$ on impact, and no effect before and after displacement. In particular, this method correctly captures no long-run earnings losses due to displacement.

Now consider the empirical approach where the control group is made up of individuals who are continuously employed, and the event is the first displacement. The time-path of earnings for those that are displaced at time 0 for the first time is as follows:

$$\begin{aligned}\mathbb{E}[e_k|D_0^{first} = 1] &= w, \forall k < 0 \\ \mathbb{E}[e_k|D_0^{first} = 1] &= 0, k = 0 \\ \mathbb{E}[e_k|D_0^{first} = 1] &= (1 - \delta)w, \forall k > 0\end{aligned}\tag{13}$$

Notice that these individuals have wage w before the displacement because the event is their *first* displacement. The time-path for those never experiencing a separation is trivial:

$$\mathbb{E}[e_k|ND = 1] = w, \forall k\tag{14}$$

To obtain the treatment effect with this approach, subtract (14) from (13), yielding:

$$\begin{aligned}\mathbb{E}[e_k|D_0^{first} = 1] - w &= 0, \forall k < 0 \\ \mathbb{E}[e_k|D_0^{first} = 1] - w &= -w, k = 0 \\ \mathbb{E}[e_k|D_0^{first} = 1] - w &= -\delta w, \forall k > 0\end{aligned}\tag{15}$$

Notice that this approach correctly predicts $-w$ on impact, but incorrectly predicts losses after the displacement event equal to $-\delta w$. The intuition is that each member of the treatment group experiences displacement with probability δ every period after displacement and we have conditioned on them not experiencing displacement in the pre-period, and the fact that the never-displaced control group never experience job loss.

A.1.2 Since Last Displacement

This section outlines the bias that can result when using the last displacement as the event as opposed to the first displacement or any displacement as in the standard and alternative approaches. Here I estimate equation (1) where the dummies refer to the last displacement.

Figure 7 shows the estimated coefficients. Intuitively, now the control group are individuals who are at least more than 6 years before their last displacement and thus this includes displaced individuals. This means that the control group have earnings of $(1 - \delta)w = 8.5$ because some of them are out of work and have no earnings. Prior to their last displacement, the treated also have displacements so the estimated effect is zero. On impact, displaced workers only lose $(1 - \delta)w = 8.5$ because the control group have average earnings of 8.5. After their last displacement, the treated group have no more displacements and, since the

control group do experience displacements, the earnings actually rise above zero to $\delta w = 1.5$. The true long-run earnings effects of displacement in this simple example is zero. In this sense, using the last displacement as the event can understate the earnings losses associated with displacement.

A.2 One-Period Delay in Earnings Recovery

A.2.1 Simple and Alternative Approaches

Now perturb the previous simplified world in only one way: suppose that the true earnings recovery is $0.5w$ (conditional on employment) in the period after displacement and then back to w two periods after displacement (conditional on employment). In other words, displacement has a short-lived impact on earnings, but the long-run earnings losses are still exactly zero. In this simple environment, the expected wage for an individual who is employed (with probability $(1 - \delta)$) in a given period is:

$$\mathbb{E}[w|emp] = 0.5w\delta + w(1 - \delta) \quad (16)$$

This is because an employed individual can either be employed or unemployed last period. He experiences unemployment last period with probability δ and this period will have wage $0.5w$. Alternatively he was not unemployed last period with probability $(1 - \delta)$ and earns w this period because he is at least two periods after any potential displacement.

As before, the time-path of earnings for those displaced at time period 0 is as follows:

$$\begin{aligned} \mathbb{E}[e_k|D_0 = 1] &= (1 - \delta)\mathbb{E}[w|emp], \forall k < 0 \\ \mathbb{E}[e_k|D_0 = 1] &= 0, k = 0 \\ \mathbb{E}[e_k|D_0 = 1] &= (1 - \delta)0.5w, k = 1 \\ \mathbb{E}[e_k|D_0 = 1] &= (1 - \delta)\mathbb{E}[w|emp], \forall k > 1 \end{aligned} \quad (17)$$

because the individual is not employed at time $k = 0$ and then, conditional on employment in the next period, receives $0.5w$. The time-path of earnings for individuals not displaced at

time 0 is:

$$\begin{aligned}
\mathbb{E}[e_k|D_0 = 0] &= (1 - \delta)\mathbb{E}[w|emp], \forall k < 0 \\
\mathbb{E}[e_k|D_0 = 0] &= (1 - \delta)w + 0.5w\delta = \mathbb{E}[w|emp], k = 0 \\
\mathbb{E}[e_k|D_0 = 0] &= (1 - \delta)w, k = 1 \\
\mathbb{E}[e_k|D_0 = 0] &= (1 - \delta)\mathbb{E}[w|emp], \forall k > 1
\end{aligned} \tag{18}$$

In the period of displacement the individual is guaranteed to be employed, but with probability δ he was unemployed last period and therefore only obtains $0.5w$ in period 0, and with probability $(1 - \delta)$ he was employed last period and earns w in period 0. In period 1, the individual was employed last period, and therefore earns w if he is employed.

To obtain the treatment effect when using those not displaced at time 0 as the control group, subtract (18) from (17) to obtain:

$$\begin{aligned}
\mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= 0, \forall k < 0 \\
\mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= -\mathbb{E}[w|emp], k = 0 \\
\mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= -0.5w(1 - \delta), k = 1 \\
\mathbb{E}[e_k|D_0 = 1] - \mathbb{E}[e_k|D_0 = 0] &= 0, \forall k > 1
\end{aligned} \tag{19}$$

Notice that the approach (correctly) predicts a treatment effect of $-\mathbb{E}[w|emp]$ on impact, a treatment effect of $-0.5w(1 - \delta)$ in the period after displacement, and no effect in other periods.³⁵ In particular, this method correctly captures no long-run earnings losses due to displacement, and estimates the treatment effect on impact relative to workers who are employed, but not necessarily employed for more than one period. This is the treatment effect of interest because the treated individual is not necessarily employed for more than one period.

Now consider the empirical approach where the control group is made up of individuals who are continuously employed, and the event is the first displacement. The time-path of

³⁵The average treatment effect of a separation is $-\mathbb{E}[w|emp]$ on impact, since we are comparing the wage of a separated worker to the average employed worker.

earnings for those that are displaced at time 0 for the first time is as follows:

$$\begin{aligned}
\mathbb{E}[e_k | D_0^{first} = 1] &= w, \forall k < 0 \\
\mathbb{E}[e_k | D_0^{first} = 1] &= 0, k = 0 \\
\mathbb{E}[e_k | D_0^{first} = 1] &= (1 - \delta)0.5w, k = 1 \\
\mathbb{E}[e_k | D_0^{first} = 1] &= (1 - \delta)\mathbb{E}[w|emp], \forall k > 0
\end{aligned} \tag{20}$$

To obtain the treatment effect with this approach, subtract (14) from (20), which yields:

$$\begin{aligned}
\mathbb{E}[e_k | D_0^{first} = 1] - w &= 0, \forall k < 0 \\
\mathbb{E}[e_k | D_0^{first} = 1] - w &= -w, k = 0 \\
\mathbb{E}[e_k | D_0^{first} = 1] - w &= -0.5w(1 + \delta), k = 1 \\
\mathbb{E}[e_k | D_0^{first} = 1] - w &= -0.5w\delta(3 - \delta), \forall k > 0
\end{aligned} \tag{21}$$

Notice that the outcome predicts $-w$ on impact, and losses of $-0.5w(1 + \delta)$ in period $k = 1$ (as opposed to $-0.5w(1 - \delta)$) and long-run losses of $0.5w\delta(\delta - 3) < 0$ in all future periods. So this method incorrectly predicts long-run earnings losses, for the same reason as before: a combination of “conditioning on good luck” post-displacement in the control group and “conditioning on good luck” pre-displacement in the treatment group. Moreover, notice that the earnings losses predicted using this method are larger than in the previous scenario where the recovery in earnings was immediate.³⁶

A.2.2 Why Does Using Any Displacement Work?

It should be clear from the simulations in Section 2.2 and the simple equations above, that using any displacement as the event delivers the true average treatment effect of displacement. It is still not clear why exactly this approach works. Notice that estimating equation (1), where the D_{it}^k dummies refer to any displacement, suffers from omitted variable bias. In particular, this equation does not include the interaction between D_{it}^0 and D_{it}^{-1} which can be “on” simultaneously. The equation without the omitted variable should be

$$w_{it} = \alpha + D_{it}^0\delta_0 + D_{it}^{-1}\delta_{-1} + D_{it}^0D_{it}^{-1}\beta + \epsilon_{it} \tag{22}$$

³⁶In fact, the long-run earnings losses appear to be larger the more protracted the actual earnings recovery.

where I have omitted all dummies that have $k \neq \{-1, 0\}$ because they are zero in this context. From simple econometrics, we know that

$$plim\left(\hat{\delta}_i\right) = \delta_i + \zeta\beta \quad (23)$$

where ζ is the coefficient from regressing $D_{it}^0 D_{it}^{-1}$ on the other regressors in (24):

$$D_{it}^0 D_{it}^{-1} = \zeta_0 + D_{it}^0 \zeta_0 + D_{it}^{-1} \zeta_{-1} + \epsilon_{it} \quad (24)$$

In this simple example, we can estimate equations (22) and (24) directly and obtain that $\hat{\delta}_0 = -10$, $\hat{\delta}_{-1} = -5$, $\hat{\beta} = 5$ and $\hat{\zeta}_0 = \hat{\zeta}_{-1} = 0.15 = \delta$. Plugging into equation (23) we get that $\hat{\delta}$ is the desired $[-9.25; -4.25]$. Somewhat serendipitously, the omitted variable bias in equation (1) serves to include displaced individuals in the control group and thus address the problem with the standard approach.

A.2.3 One Caveat

I performed one more exercise worth mentioning. Suppose that earnings recover immediately upon re-employment, but the probability of separating increases for one period, if one was separated last period. It turns out that in this case the alternative approach understates the earnings losses associated with displacement in the short-to-medium run. Although this is not ideal given that a displacement today does induce higher displacement probabilities in the future, most studies find that the employment probabilities of displaced workers recover in around four years. So, the alternative approach will correctly estimate the long-run earnings losses associated with displacement which come from a reduction in pay rather than hours or employment.

A.3 Intuition Using Yet Another Approach

This section presents simple intuition for the method used in Davis and von Wachter (2011) (henceforth DV). These authors pick a particular year, y . The treatment group is composed of individuals displaced in year y and the control group is composed of individuals not displaced in year y . The displacement dummies are created so that D_{it}^k is zero for all t and all k for the control group, and for the treatment group equal to 1 if individual i , at time period t was displaced k periods ago and zero otherwise.

This approach is similar to the not-displaced-today approach outlined in the main text and equations (10) and (11) can be used for the time-path of earnings for the displaced and

not-displaced, respectively, when the earnings recovery is immediate. However, with this approach, those not displaced this period are the control and since these individuals have all their dummies turned to zero, the constant in equation (1) is $w(1 - \delta)$ as opposed to w in the not-displaced-today approach. Moreover, there is nothing in the procedure that allows the earnings of the control group to be different in the treatment period since all their dummies are set to zero. In this sense, although the control group are individuals who are not displaced this period, the way the estimation is operationalized means that the intercept of equation (1) refers to an average worker (who may or may not be employed) since the control group can experience displacements outside of year y that are not picked up by the D_{it}^k dummies. Figure 8 shows the δ_k coefficients from estimating equation (1) for an arbitrary year with this DV approach when the earnings recovery is immediate. On-impact the earnings losses are $w(1 - \delta) = 8.5$, and zero in all other periods.

For the case where the earnings take one period to recover, equations (17) and (18) still apply, but the DV approach compares the treated (equation (17)) to a worker who is not displaced this period, but could be displaced in other periods. This amounts to comparing displaced workers to an average worker, represented by a convex combination of (17) and (18) with weight equal to δ on equation (17). The result is that on impact, this approach delivers earnings losses of $-(1 - \delta)w(1 - 0.5\delta)$ (instead of $-\mathbb{E}[w|emp] = -w(1 - 0.5\delta)$), and $-(1 - \delta)0.5w(1 - \delta)$ (instead of $-0.5w(1 - \delta)$) in the period after displacement. In all other periods the treatment effect is (correctly) estimated to be zero. Figure 8 shows the δ_k coefficients from estimating equation (1) for an arbitrary year with this DV approach when the earnings recovery takes one period. Notice that the on-impact dip in earnings is -7.9 , which is exactly equal to $(1 - \delta)$ times the losses on-impact using the not-displaced-today (-9.25). Again, this is because the DV approach compares displaced workers to workers who may experience displacements outside the year of interest that are not picked up by the displacement dummies. In the period after displacement, the earnings losses are -3.6 , which are the losses in the not-displaced-today approach (-4.25) times $(1 - \delta)$. Hence, comparing the DV approach to the not-displaced-today suggests that the DV approach will understate the earnings losses associated with displacement by a factor of $(1 - \delta)$.

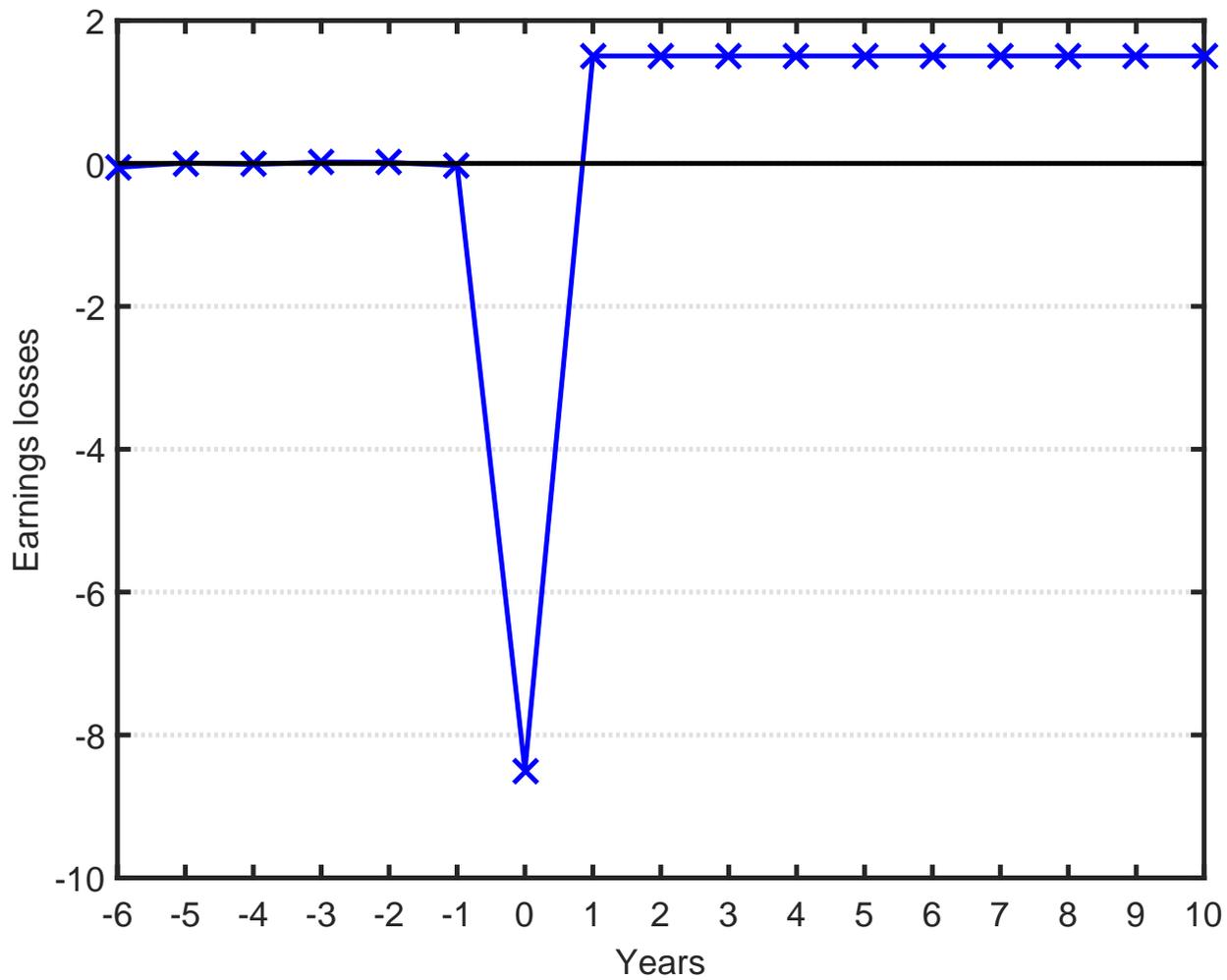


Figure 7: No Delay in True Earnings Recovery: Since-Last-Displacement Approach

Note: Looking at the last displacement event understates losses after displacement by a factor of $\delta w = 1.5$. This exercise estimates equation (1) using the last displacement as the event in the simple simulation with an immediate recovery in earnings. This is an approach that Stevens (1997) uses.

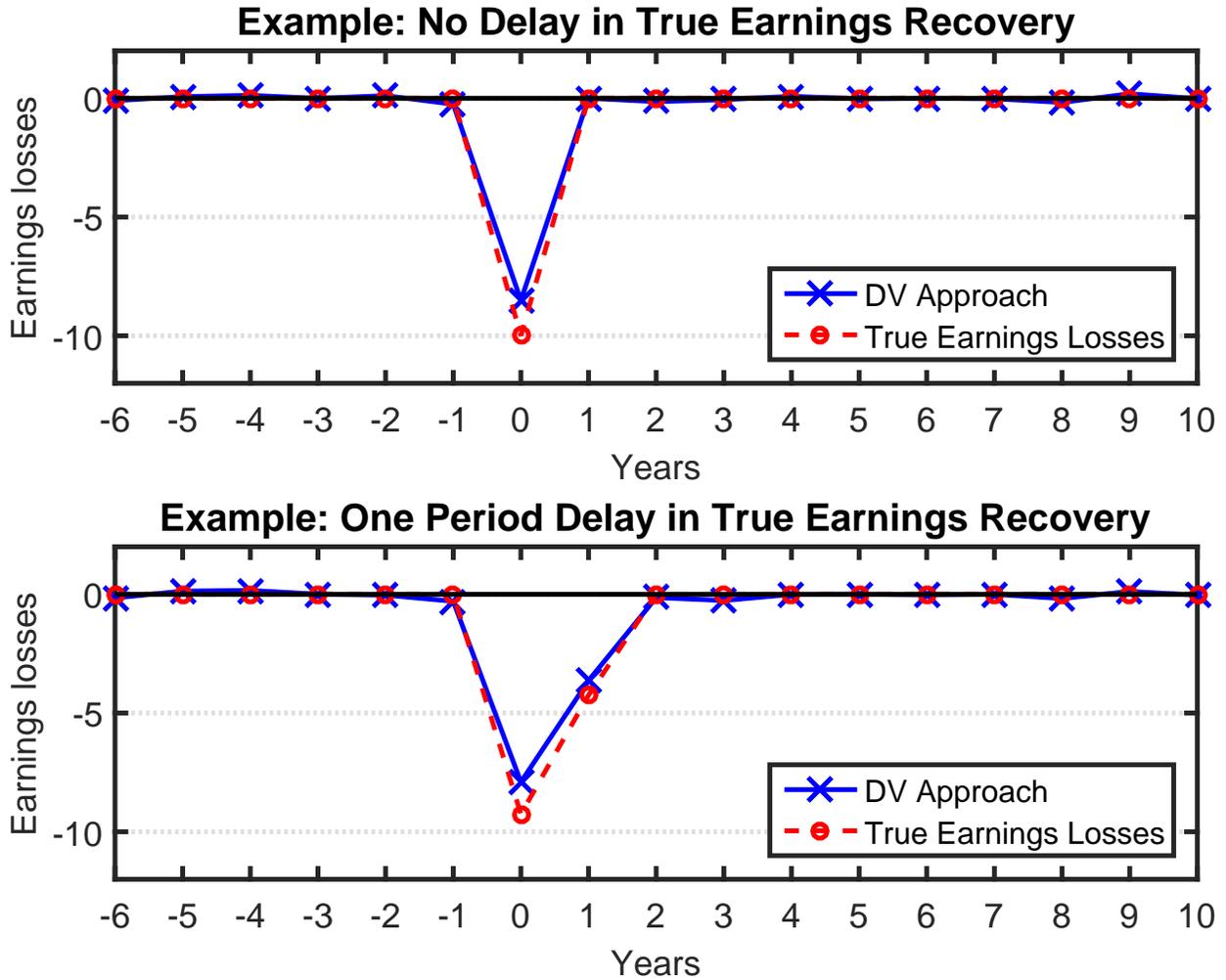


Figure 8: Examples with DV approach

Note: The approach used by Davis and von Wachter (2011) understates losses by a factor of $(1 - \delta)$ compared to the not-displaced-today approach. Nevertheless, the approach correctly predicts zero long-run earnings losses. The results are slightly more variable due to smaller sample sizes for one year of the DV approach. In particular, the slight dip in earnings in year ‘-1’ is due to simulation error.

B Appendix: Surplus/Wage Equation and Numerical Details

This section details the derivation of the surplus equation and the wage equation used in the main text, as well as briefly describing the numerical approach.

B.1 The Surplus Equation

To solve the model, first solve for the surplus equation using equations (2) and (4) to substitute for $W(h)$ and $J(h)$, respectively.

$$\begin{aligned}
S(h) &= J(h) + W(h) - U(h) \\
&= h^\gamma + \delta(1 - p_E)(1 - p_s)(1 - \eta)[J((1 + \mu)h) + W((1 + \mu)h)] \\
&\quad + \delta p_E(1 - p_s)(1 - \eta) \int \left[\mathbb{I}\{W((1 + \mu)h) \geq W(h')\}[J((1 + \mu)h) + W((1 + \mu)h)] \right. \\
&\quad \left. + \mathbb{I}\{W((1 + \mu)h) < W(h')\}W(h') \right] dF(h'|h) \\
&\quad + \delta p_s(1 - \eta)U((1 - \alpha)h) - U(h)
\end{aligned} \tag{25}$$

Now use the fact that with the surplus sharing rule $W(h) = \beta S(h) + U(h)$ and $J(h) + W(h) = S(h) + U(h)$ to substitute out for these two quantities. This yields:

$$\begin{aligned}
S(h) &= h^\gamma + \delta(1 - p_E)(1 - p_s)(1 - \eta)[S((1 + \mu)h) + U((1 + \mu)h)] \\
&\quad + \delta p_E(1 - p_s)(1 - \eta) \int \\
&\quad \left[\mathbb{I}\{\beta S((1 + \mu)h) + U((1 + \mu)h) \geq \beta S(h') + U(h')\}[S((1 + \mu)h) + U((1 + \mu)h)] \right. \\
&\quad \left. + \mathbb{I}\{\beta S((1 + \mu)h) + U((1 + \mu)h) < \beta S(h') + U(h')\}[\beta S(h') + U(h')] \right] dF(h'|h) \\
&\quad + \delta p_s(1 - \eta)U((1 - \alpha)h) - U(h)
\end{aligned} \tag{26}$$

Notice that this is a functional equation in $S(\cdot)$ and $U(\cdot)$ so iterating on this and the value of unemployment in equation (3) is all you need to do. Notice that you need to replace $W(h)$ with $\beta S(h) + U$ in equation (3) and solve for $U(h)$ to yield:

$$U(h) = \frac{bh + \delta p_U(1 - \eta)\beta S(h)}{1 - \delta(1 - \eta)} \tag{27}$$

B.2 The Wage Equation

To retrieve the wage notice that

$$\begin{aligned}
\beta S(h) &= W(h) - U(h) = w(h) + \delta(1 - p_E)(1 - p_s)(1 - \eta)W((1 + \mu)h) \\
&\quad + \delta p_s(1 - \eta)U((1 - \alpha)h) \\
&\quad + \delta p_E(1 - p_s)(1 - \eta) \int \max\{W(h'), W((1 + \mu)h)\} dF(h'|h) - U(h) \\
&= w(h) + \delta(1 - p_E)(1 - p_s)(1 - \eta)[\beta S((1 + \mu)h) + U((1 + \mu)h)] \\
&\quad + \delta p_s(1 - \eta)U((1 - \alpha)h) \\
&\quad + \delta p_E(1 - p_s)(1 - \eta) \int \max\{\beta S(h') + U(h'), \beta S((1 + \mu)h) + U((1 + \mu)h)\} dF(h'|h) \\
&\quad - U(h) \\
\therefore w(h) &= \beta S(h) - \delta(1 - p_E)(1 - p_s)(1 - \eta)[\beta S((1 + \mu)h) + U((1 + \mu)h)] \\
&\quad - \delta p_s(1 - \eta)U((1 - \alpha)h) \\
&\quad - \delta p_E(1 - p_s)(1 - \eta) \int \max\{\beta S(h') + U(h'), \beta S((1 + \mu)h) + U((1 + \mu)h)\} dF(h'|h) \\
&\quad + U(h)
\end{aligned}$$

This is a function of $S(\cdot)$ and $U(\cdot)$ so once you have those functional equations you can back out the wage.

B.3 Numerical Details

I solve the model numerically using a contraction mapping in a discretized state space. I discretize the AR(1) process for human capital (h) onto 249 grid points using the Tauchen method. I solve the value functions on a grid, and in the simulation interpolate for points off the grid using linear interpolation. I do not allow the state variable to take values above and below the minimum and maximum values on the grid, although in practice this does not affect the results because the probability of the state variable falling outside the grid remains extremely small.

Given the optimal decisions of workers and firms, the model generates simulated data at a monthly frequency. In particular, I simulate 5,000 agents for 600 months (50 years). To remove the effects of initial conditions, I simulate the model for 3600 months and then discard the first 3000 months of the sample. This simulation provides a time-path of wages and annual earnings, as well as an employment history.

I calibrate the parameters of the model using simulated method of moments. The procedure minimizes the distance between the summary statistics of the simulated data and the summary statistics of real data. Specifically, if θ represents the vector of structural parameters, \hat{g} represents the moments of the actual data, and $g(\theta)$ represents the moments of simulated data, then the simulated minimum distance estimator is defined as:

$$\hat{\theta} = \arg \min_{\theta} \mathcal{L}(\theta) = \arg \min_{\theta} [g(\theta) - \hat{g}]' W [g(\theta) - \hat{g}] \quad (28)$$

Here $g(\theta)$ represents a non-linear transformation of the structural parameters by the model and a transformation of the simulated data to achieve moments that match observed moments.

The optimization is implemented using a coarse grid search across the relevant state space to obtain areas where the loss function might be minimized. Once the initial points are evaluated, I use MATLAB's Nelder-Mead optimization routine, *fminsearchbnd*, from each candidate solution to find the minimum objective function value in that region of the state space. The global minimum is taken as the minimum of all these local minima.