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RECONSIDERING THE CONSEQUENCES OF WORKER DISPLACEMENTS: FIRM VERSUS WORKER PERSPECTIVE

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ABSTRACT

Prior literature has established that displaced workers suffer persistent earnings losses by following workers in administrative data after mass layoffs. This literature assumes that these are involuntary separations owing to economic distress. This paper examines this assumption by matching survey data on worker-supplied reasons for separations with administrative data. Workers exhibit substantially different earnings dynamics in mass layoffs depending on the reason for separation. Using a new methodology to account for the increased separation rates across all survey responses during a mass layoff, the paper finds earnings loss estimates that are surprisingly close to those using only administrative data.

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A significant literature using administrative data finds large and persistent earnings losses for displaced workers (e.g., Jacobson, LaLonde, and Sullivan (1993) and Davis and von Wachter (2011)). This literature assumes that the when a firm is contracting by 30% or more, the workers who separate do so because of economic distress at the firm. It is not obvious, however, that all separations in these events are due to economic distress. For example, even when a firm is contracting by 30% or more, some workers might be quitting to better jobs. Such quits would lead to a downward bias in estimates of the earnings effects of displacements.

To quantify this bias, this paper links worker survey data to administrative data. Survey data provides information about workers' perceptions about their reason for separation, and thus allows us to distinguish between cases where workers' stated reason agree and disagree with the assumption that all separations in a mass layoff are displacements. The central difficulty lies in how to interpret worker survey responses. Specifically, does the survey response represent the proximate or ultimate reason for separations? We interpret the proximate reason as being what the worker reports in the survey. Some separations reported as a quit, however, may have been caused by economic distress at the firm. For example, the worker might have searched for a job in anticipation of the layoff. In this case, the researcher would not want to take the survey response at face value and infer an error in the administrative data. Hence, we interpret the firm contraction to be the ultimate reason for separation if, in the absence of the contraction, the worker would not have separated.

This paper develops an approach to combine information in administrative and survey data to assess the magnitude of possible biases in the administrative data, while distinguishing between the proximate and ultimate reasons for separations in the survey data. The main idea is to compare the separation probabilities and earnings losses of workers reporting particular survey reasons at relatively stable firms (i.e., neither growing nor shrinking), to those at firms contracting by 30% or more. We interpret the difference in the separation probabilities and the composition-adjusted earnings losses as reflecting the causal role of the firm contracting on both the probability of separating as well as earnings losses.

Our approach provides estimates of the share of separations at firms that are contracting by 30% or more that are caused by the separation, which we refer to as latent displacements, as well as the earnings losses in the separations caused by the contraction. In terms of separations, when a firm contracts, only 55% of the survey responses indicate that the worker's proximate cause of separation was firm-level distress. Our approach finds, however, that much of this misalignment between survey and administrative measures represents workers reporting proximate rather than ultimate reasons for separations, so that about 85% of the separations were caused by the firm contraction. In terms of earnings losses, we uncover substantial heterogeneity in earnings losses depending on the alignment between survey and administrative data. Our estimate of the latent earnings losses of displaced workers, however, is quite similar to the estimate using administrative data alone. Thus, we conclude that the magnitude of the biases in administrative data measures of displacement are small.

We also use our link between survey and administrative data to revisit the treatment of earnings histories with zeros. A common practice in the displaced worker literature is to drop earnings histories with a sufficient number of zeros post-displacement. The reason is that in typical administrative datasets the researcher does not know whether the zeros are because of non-participation or unemployment. Our survey-administrative data link allows us to include only the zeros of workers who are unemployed and thus still looking for work. Including these zeros approximately doubles the magnitude of the persistent earnings losses.

The paper that is most similar to this one is von Wachter, Handwerker, and Hildreth (2012). They link the Displaced Worker Survey (DWS) to California administrative data and assess the alignment between the survey data and the administrative data and see how earnings losses vary depending on the alignment. Our use of the Survey of Income and Program Participation (SIPP) allows us to ask different questions than von Wachter, Handwerker, and Hildreth (2012) can ask with the DWS. Specifically, workers only appear in the DWS if they report being displaced. There are thus two reasons why a worker might not appear in the DWS. First, a worker may have forgotten that they were displaced. Second, they might have remembered that they separated, but did not perceive it as a displacement. Because respondents are asked retrospectively about the last three years, what they remember about the event might depend on what subsequently happened to them. This saliency bias is precisely what von Wachter, Handwerker, and Hildreth (2012) work hard to address. The SIPP has the benefit that workers are asked about separations that occur in the last quarter and so there is less concern about recall bias (in particular, recall that conditions on outcomes). In addition, SIPP respondents can report a reason for separating that is not displacement.

This paper unfolds as follows. Section 1 describes our administrative and survey datasets and the procedure for linking them. Section 2 documents what workers say when the administrative data would label them as displaced, and what the administrative data says when workers say they were displaced. Section 3 describes how we estimate earnings changes following a separation. It also shows that there is substantial heterogeneity in earnings changes conditional on a worker being displaced, depending on what reason the worker gives for separating. Section 4 describes how we combine the information in (mis)alignment between the survey and administrative data to measure the earnings losses associated with the firm contracting. It also reports the results of implementing our method. Section 5 reports the results of including the zeros of workers still looking for work. Section 6 concludes.

1 Survey and administrative data

1.1 Data description

We use two datasets: the Survey of Income and Program Participation (SIPP) and the administrative Longitudinal Employer Household Dynamics (LEHD).

The SIPP is a U.S. Census Bureau survey. It is a nationally representative series of panels with a sample size of between 14,000 and 36,000 households per panel. We use the 2001 and 2004 panels, which span the years 2000 to 2006. Each SIPP panel is conducted in waves and rotation groups, with each wave consisting of a 4-month period during which an interviewer contacts a household. The sample is divided into four rotation groups, where one rotation group is interviewed each month. During the interview, the household is asked information about the previous 4 months.

The SIPP contains information on up to two jobs held by each person in the household, along with the starting and (potential) ending dates of those jobs. If a respondent identifies that a job has ended, they are prompted to identify the reason that the job has ended from a list of 14 possible answers (see Table 1 for the complete list). In addition, it provides information on labor force participation. Those identified as not working are asked to identify the reason.

The LEHD dataset is built from administrative unemployment insurance records.[1] It contains

[1] We have access to all 49 states (plus the District of Columbia) that participate in the LEHD program, and the data we have available runs through 2011. See Abowd et al. (2009) and McKinney and Vilhuber (2008) for discussion of the background and contents of the LEHD files. Over 90 percent of payroll employment is covered by the unemployment insurance system.

unique person identifiers that allow us to follow workers across employers. Similarly, it contains unique employer identifiers that allow us to follow employers over time and construct employer growth rates. The unit of analysis on the employer side is the state-level enterprise identification number (SEIN). While multiple establishments may have the same SEIN in a particular state, the definition of the enterprise does not cross state lines.

1.2 Matching procedure

We link the jobs in the SIPP to jobs in the LEHD. While there is a bridge between *people* in the SIPP and the LEHD, there is not a bridge between *jobs*.[2]

To align with the interest in the displaced worker literature in high-tenure workers we look at SIPP jobs with at least 12 months of tenure. We then link a SIPP job that ends to an LEHD job on three features.

1. The LEHD job has four consecutive quarters of positive earnings that exceed a minimal threshold (earning the minimum wage at 70% of full-time equivalent hours);

2. In the four quarters following the survey-reported separation, the worker has at most minimal earnings from the employer (earnings fall below the threshold defined in the previous bullet);[3] and

3. The LEHD job ends either in the quarter that the SIPP job is reported to end, or one quarter before or after the SIPP job is reported to have ended.

The first requirement means that both jobs meet a tenure threshold and are both plausibly full time. The second requirement attempts to capture permanent separations. The third requirement follows from our interest in comparing reasons for separations, rather than the reporting of separations. The window around the separation allows for the possibility that workers continue to receive paychecks after a separation. In cases where this procedure yielded more than one match we gave priority to the job with the highest earnings in the quarter prior to the separation.

For linking continuing jobs (i.e., jobs that do not end) in the SIPP to the LEHD we follow a similar procedure to above, except that we do not impose the requirement that the job end.

[2] This person-level bridge has been used before (e.g. Celik et al. (2012)).

[3] This four quarters of minimal earnings is similar to Schoeni and Dardia (1996, pg. 5), which they also use to alleviate concerns about recalls.

Appendix A provides additional details on the criteria, as well as the resulting match rates. The main sample frame consists of person-quarters in the SIPP that have been matched to the LEHD. A given person might appear multiple times in the dataset. We impose two additional sample restrictions. First, we require that the worker be between the age of 25 to 74 in the calendar year of the observation. Second, we require that the employer have at least 50 workers three quarters prior to the candidate quarter (see appendix Tables A1 and A2 for comparisons of our sample selection criteria to other studies).

One might worry that some of what we label as a mass layoff is simply that the employer ID has changed. In Appendix B we detail how we used employee flows to clean spurious shutdowns and other employer ID changes.

2 Alignment between survey and administrative measures

The literature has used both administrative and survey measures to study displaced workers. Both measures present measurement challenges. On the one hand, the administrative measure might capture separations that are unrelated to economic distress. On the other hand, the survey measure might be contaminated by self-serving reports, or might not capture the concept that the researcher is interested in. This section first details both measures and then documents the alignment between the two measures in our linked administrative-survey data.

2.1 Administrative data measure

The standard approach in administrative data is to classify separations based on information from net worker flows. In particular, a large net contraction is taken as evidence of firm distress, and the event as a whole is referred to as a mass layoff. We follow the practice in this literature of defining a mass layoff as occurring when employment falls by 30% or more. Table A2 highlights the commonality across papers using administrative data of this definition, which originates with Jacobson, LaLonde, and Sullivan (1993). While this cutoff may seem arbitrary, the findings in this paper suggest that it does a reasonable job of mainly picking up separations that are caused by the employer contraction, in a sense that we develop more formally in Section 4.

We use a one-year window around when the worker separated to measure the employer growth rate. Specifically, we measure the firm's employment three quarters before the separation and one quarter after the separation. If the decline in employment over this period exceeds 30%, then we label this a mass layoff. This one-year time window is in line with recent literature, i.e. Andersson et al. (2014) and Davis and von Wachter (2011). In contrast, Jacobson, LaLonde, and Sullivan (1993) allow for a six year window.

In Appendix C we discuss other ways of measuring separations due to economic distress that have appeared in the literature.

2.2 Survey data measures

The SIPP provides information from workers about their perceptions of the circumstances surrounding the separation. Researchers typically use the worker-reported reasons from the survey to classify separations into those owing to economic distress at the firm, and other reasons.

Because of the multiple survey response options in the SIPP and the relatively small sample sizes, we classify the survey responses into three groups: *distress*, *quit* and *other*. We map the following four reasons for separation to *distress*: 1) On layoff,[4] 2) Employer went bankrupt, 3) Employer sold business, and 4) Slack work or business conditions. To identify worker *quits*, we narrow in on the employer-to-employer transitions that is the subject of interest in the literature and thereby restrict to survey reports of 1) Quit to take another job. Finally, we classify the remaining reasons for separation into an *other* category: 1) Retirement or old age, 2) Other family/personal/child obligations, 3) Own illness/injury, 4) School or training, 5) Job was temporary and ended, 6) Unsatisfactory work arrangement, 7) Quit for some other reason, and 8) Discharged/fired.

Other surveys that have been used to study displacements capture a slightly different combination of reasons. The most common surveys used are the DWS and the PSID, although other research has used the HRS, the NLSY, and the SIPP. von Wachter, Handwerker, and Hildreth (2012) compare the DWS with administrative records. Table A3 summarizes definitions of displacement that have been used in worker-side surveys.

[4] Fujita and Moscarini (2017) note that there is recall among workers reporting "on layoff" in the SIPP. We attempt to capture only non-recalled layoffs by requiring that the worker have minimal earnings from that employer in the four quarters following the report of "on layoff." We have conducted robustness checks where we exclude the small share of workers who have earnings starting five quarters after separation from their pre-displacement employer.

2.3 Alignment of survey and administrative measures

Using our aggregated survey categories, Table 1 shows that the survey and administrative measures are aligned, though not perfectly. For example, of the separations where workers report *distress* in the survey data, 28% occur during a mass layoff. In contrast, only 5% of worker reported *quits*, and 6% of worker reported *other* reasons, occurred during a mass layoff.

Even within the aggregated survey categories, the survey and administrative measures are correlated. All of the survey reasons we classify as *other* have lower shares of the mass layoff indicator than the survey reasons we classify as *distress*. Within the survey reasons we classify as *distress*, the fact that the employer went bankrupt/sold business has the highest share of the mass layoff indicator also makes sense.

Despite this alignment between the survey and administrative measures, the administrative measure misses most of the separations that the survey respondents label as due to *distress*. Specifically, 28% of the worker survey-reported *distress* separations are captured by the administrative indicator such that the administrative indicator misses over 70% of the survey-reported *distress* separations.[5]

Panel B of Table 1 shows the misalignment in the other direction. Almost half of the separations that are labelled as an administrative displacement are labelled by workers as not due to *distress*. Among the administrative displacements, 55% of SIPP respondents report a job loss due to *distress*.[6] Thus, the majority of the separations that the administrative measure labels as a displacement correspond to a worker report of displacement, which confirms the finding reported in Davis and von Wachter (2011, pg. 9 n. 9) that "most employment reductions are achieved through layoffs when firms contract by 30 percent or more."[7]

We now turn to graphical evidence on alignment between survey and administrative measures.

[5] Our results are close to von Wachter, Handwerker, and Hildreth (2012). Conditional on a survey report of a displacement, they find substantial variation in alignment depending on the precise administrative definition used. For their preferred administrative definition (row 8), they find that a displacement shows up in the administrative data 23% of the time given it is present in the survey data, while we find it for 28% of separations. Of course, if we focus attention on a narrow category of *distress*, "employer bankrupt or sold business," then the alignment is tighter.

[6] Conditional on the administrative indicator, we find many more survey reports of distress using the SIPP than von Wachter, Handwerker, and Hildreth (2012) find using the DWS. In von Wachter, Handwerker, and Hildreth (2012, Table 4, column (7)), conditional on the firm-side indicator showing distress, they find a report of a displacement in the DWS at most 14% of the time (for various definitions of displacement). This contrasts to 55% in our data. The reason, we think, is that the DWS is notoriously plagued by recall bias.

[7] The Davis and von Wachter (2011) statistic is based on the Job Openings and Labor Turnover Survey (JOLTS), which is an employer-side survey. It is possible a firm reports laying a worker off, while a worker reports a *quit*, or *other* reasons for separation.

Figure 1 shows how separations depend on the employer growth rate. In Panel A, the solid line plots the probability of a worker separating as a function of the employer growth rate, while the histogram plots the distribution of the employer growth rates.[8] The histogram shows that for most observations in our data employers are relatively stable. The solid line displays the canonical hockey-stick shape (Davis, Faberman, and Haltiwanger (2012, Figure 6)) whereby the probability of separating rises rapidly as employers contract.

Panel B of Figure 1 shows the graphical version of the imperfect alignment between survey and administrative indicators of displacement. The figure decomposes the probability of separating in Panel A as a function of the employer growth rate into the three survey reasons: *distress*, *quit* and *other*. Looking at the left-hand side of the graph, we see that among firms contracting by 30% or more there are many survey reports of *quit* and *other* as the reason for separation. Moreover, as employers contract, the probabilities of each survey reason rises. Looking at the right-hand side, there is still a positive probability of worker's reporting distress as the reason for separation, even though the administrative data approach would suggest none.

2.4 Robustness and extensions

Table 2 reports a variety of robustness and extension to our main definitions.

We first consider varying our definition of mass layoff along the dimensions of severity and timing. As can be anticipated from Panel B of Figure 1, when we increase the severity to a 40% contraction, we find slightly tighter alignment; when we decrease to a 5% contraction, we find a weaker relationship. We also vary the timing. In our benchmark we use the growth rate computed over 4 quarters relative to the separation (from $t-3$ to $t+1$). We experiment with varying this to 8 quarters and 16 quarters and find, perhaps unsurprisingly, that alignment is much weaker when we use a wider time period to measure the contraction.

We then consider one of the alternative indicators of a displacement discussed in Appendix C: unemployment insurance (UI) take-up. UI take-up is potentially a cleaner indicator of a layoff because there is a legal sense in which the UI system ensures that the worker was laid off, rather than fired or quit. Using a survey-based measure of UI take-up is still potentially problematic. As

[8] Looking at Table 1, the quarterly separation rate is about 3.0%. This might appear low. Several features of our sample account for this fact. First, the SIPP respondents considered in this paper have relatively stable jobs because we condition on having a year of tenure. Second, the frequency of the table is quarterly. The implied annual separation rate is about 12%.

Meyer, Mok, and Sullivan (2009, Table 8) show, there is substantial underreporting of UI receipts in the SIPP.[9] We find that, conditional on reporting collecting UI, 66% of workers report separating because of distress, while 11% of workers who do not collect UI report separating because of distress.[10]

We also use the information in the SIPP on severance pay to help validate and interpret the survey responses. We find that, conditional on receiving severance pay, it is extremely unlikely that a worker reports a quit.

3 Earnings changes following a separation

The prior section shows that survey and administrative indicators of displacement are imperfectly aligned. This section shows that the consequences of the separation depend on both the administrative and survey classifications. In section 4 we look in more detail at the interaction between the classifications.

3.1 Earnings specification

We estimate the "treatment" effect of several different classes of separations on labor market outcomes in an event study framework. The event study framework was pioneered by Jacobson, LaLonde, and Sullivan (1993) to study the effect of displacements. We also use it to study earnings changes following any separation. This comparison allows us to difference out earnings gains (or losses) that would have happened in the absence in the separation. For notational simplicity, we refer to displaced workers as the treated group in this section.

Consider a treated group of workers who lose their job in a displacement in a particular event quarter y (say 2000:I), and a control group of workers who do not lose their jobs *in that quarter* and were employed at a firm that was relatively stable (i.e., did not growth or shrink). Following

[9] The SIPP misses, on average, about 25% of the dollar value of SIPP receipts. Obviously, for our purposes it would be ideal to be able to decompose this separately on the intensive and extensive margin.

[10] These numbers are not interpretable as take-up rates, but the fact that take-up rates for UI are typically low suggests substantial room for "misalignment." Anderson and Meyer (1997, pg. 925, Table 3, column 3) use administrative data and find that the UI take-up rate among those displaced (using a 5% contraction definition) is 53%, and Cullen and Gruber (2000, pg. 555-556) use the SIPP and find a UI take-up rate of 56%.

Davis and von Wachter (2011, equation 1), we specify the regression

$$e_{it}^{y} = \alpha_i^y + \gamma_t^y + \mathbf{X_{it}}\beta^y + \sum_{k=-3}^{16} \delta_k^y D_{it}^k + u_{it}^y,\ t = k + y \tag{1}$$

where e_{it}^y is real earnings of individual i in quarter t, α_i^y are worker fixed effects, γ_t^y are calendar–quarter fixed effects, X_{it} is a quartic polynomial in the age of worker i in year t, the D_{it}^k are dummy variables equal to 1 in the k^{th} quarter relative to the displacement, and u_{it}^y represents random factors. In this specification, the inclusion of the calendar time dummies, the γ_t^y, means that the δ_k^y measure the earnings path of the time y displaced workers relative to the continuers at the stationary firms. We normalize $\delta_{-3}^y = 0$.[11] The δ_k^y are the coefficients of interest: the effect of being displaced relative to continuing at a stationary firm in the particular quarter.[12]

In our SIPP-LEHD matched data, we have a relatively small number of separators per quarter, so we pool across quarters by stacking datasets corresponding to each of the quarter-specific experiments reflected in equation (1). Specifically, this means keeping only 3 quarters of workers earnings prior to each event quarter and 16 quarters of earnings after the event quarter.[13] Letting y represent a displacement or event quarter and recognizing that $t = k + y$ we have:

$$e_{ik}^y = \sum_y \alpha_i^y + \gamma_t + \mathbf{X_{ik}^y}\beta + \sum_{k'=-3}^{16} \delta_{k'} D_{it}^{k'} + \sum_y \sum_{k'=-3}^{16} \gamma_{k'} E_{ik'}^y + u_{ik}^y. \tag{2}$$

Relative to equation (1), this specification imposes three restrictions. First, the effect of displacement on earnings does not vary across displacement quarters so that $\delta_k^y = \delta_k$. Second, the slope of the path of the earnings of the control group is constant across displacement quarters, up to a level shift. That is, rather than entering γ_t^y we enter $\gamma_t + \sum_{k'=-3}^{16} \gamma_{k'} E_{it}^{k'}$ where $E_{it}^{k'}$ is an indicator for the displacement quarter.[14] We also normalize $\delta_{-3} = 0$.[15] Third, the age-earnings profile does

[11] This is a true normalization because we include a full set of worker effects.

[12] This interpretation contrasts to the notion of displacement in Jacobson, LaLonde, and Sullivan (1993, pg. 691): "Our definition of earnings loss is the change in expected earnings if, several periods prior to date s, it was revealed that the worker would be displaced at date s rather than being able to keep his or her job indefinitely." See Krolikowski (Forthcoming) for further discussion of this point.

[13] In appendix Table A4 we present a stylized example of how a single person's earnings history turns into several potential earnings records in our regression.

[14] Note that the person-displacement quarter fixed effects subsume the average of the time-varying error component in the time that the worker is in the sample (e.g. the average of γ_t). Hence, this specification implicitly allows there to be a time-specific component of earnings.

[15] This is a true normalization because we include a full set of worker effects.

not differ across displacement quarter so that $\beta^y = \beta$.[16] Appendix D discusses several issues with how to compute standard errors for this pooled specification and how we address them.

The sample described above includes the person-quarters in the SIPP that we successfully match to the LEHD. That match requires that we observe LEHD earnings in the current and previous three quarters. To study outcomes subsequent to displacement events, we need to include LEHD earnings for subsequent quarters. As is standard in the literature (see Table A1), we initially restrict to the sample of people with positive earnings in a calendar year for up to four years after the displacement. We allow for less than four years when the LEHD data "runs out" (e.g., for a separation in 2006, we only require positive earnings in 2006, 2007, and 2008). We discuss this sample restriction in detail in section 5.

As our primary earnings variable, we normalize earnings using the average of 2 quarters of workers' earnings prior to displacement. To be precise, if the last quarter of the employment relationship is period $t = 0$, then we use the average earnings in periods $t - 1$ and $t - 2$. Using earnings normalized in this way combines the strengths of the levels and logs specification. Like the levels specification, it allows us to include quarters in which a worker had zero earnings. Like the logs specification, it generates coefficient estimates that are interpretable as percent change in earnings relative to pre-displacement earnings. In addition, like the log specification it weights each worker equally. We are not the first to construct normalized earnings in this way; see, for example, Autor et al. (2014) and Davis and von Wachter (2011).

3.2 Comparison groups

In contrast to a common control group, in which all workers who continue in their jobs constitute the control group (see Krolikowski (Forthcoming) for further discussion), our control group in equation (2) is workers who continue at *relatively stable* firms (which we define as firm growth in the growth interval $[-5\%, +5\%]$).[17] The language of treatment and control implies that we presume that the data approximates an experiment where some workers randomly separate. An empirical

[16] Note that if t is sufficiently bigger than y then we do not include a calendar-quarter times displacement-quarter dummy since there are no earnings records associated with it.

[17] We use this group to minimize the extent to which there are negative or positive shocks in the control group, which might happen if we include continuers at firms that are contracting or expanding rapidly. The use of this control group also aligns with our approach in section 4 where we want a counterfactual where there was no shock (good or bad) at the firm. We experimented with the control group of all workers who continue (regardless of employer growth) and, in practice, the choice of the control group had little effect on the estimates. We use the employer growth screen for the control group as our baseline to maintain consistency with the approach in section 4.

implication of random separations is that the two groups look similar on observable covariates.

Table 3 shows that there are important differences in observable characteristics between our treatment and control groups. The mass layoff separators are younger, have less education, are more likely to be men, and earn less.[18] They also work in smaller firms and in more blue-collar industries.

To address this lack of covariate balance, we turn to propensity score reweighting. The basic idea of propensity score reweighting is to make the control group "look like" the treatment group. Complete details are in Appendix E.

3.3 Displaced worker earnings losses: weighted and unweighted

Panel A of Figure 2 shows that there are large earnings losses immediately after an administrative displacement. The figure plots the earnings trajectories of workers from 3 quarters before to 16 quarters after the separation. While there is a recovery, even three to four years after the displacement, earnings are still lower. This persistence replicates the standard result in the literature. The level of the long-term earnings losses are lower than in some classic studies such as Jacobson, LaLonde, and Sullivan (1993). We suspect that this difference is because we impose a lower tenure requirement (one year, as opposed to six years in Jacobson, LaLonde, and Sullivan (1993)).

Panel B shows that our reweighting procedure makes little difference to the estimated earnings trajectory. Given the large differences in observable characteristics documented in Table 3 and the fact that the reweighting procedure generates balance (see column (2)), this finding might seem surprising. It should not be surprising for a combination of three reasons. First, we have already included worker fixed effects in the estimation, so that level differences in earnings predicted by these characteristics are removed. Second, we include a quartic polynomial in age so that age differences between the two groups—the primary predictor of curvature in earnings—is controlled for in the specification. Finally, we have verified that while reweighting the control group does increase the mean earnings growth in this group, this increase is small relative to displaced worker earnings losses.[19]

[18] The earnings deciles are calculated by taking the average of earnings 2 and 3 quarters prior to the separation for the separators, and for the 2 and 3 quarters prior to the observation of continuing for the continuers. For workers who continue—and thus possibly appear many times in the data—we only take one earnings record to calculate the earnings distribution.

[19] In appendix Figure A3 we show the weighted and unweighted mean earnings trajectory in the control group. Consistent with the fact that the displaced workers have observable characteristics that tend to predict faster earnings

For the remainder of the results in the paper, we find that there are differences in observable characteristics between our "treatment" and "control" groups, but that reweighting makes no difference to our estimates. Hence, we present the reweighted results and do not discuss differences in observable characteristics across groups.

3.4 Decomposing the administrative measure by survey reason

Among those identified as displaced by the administrative indicator, there is significant heterogeneity in the earnings changes based on the survey reason. Figure 3 plots the earnings changes for the administratively indicated displaced workers (as in Figure 2), but split into the three survey categories. Mechanically, the lines in this figure come from estimating three separate regressions of the form given in equation (2). Those reporting a *distress* reason for separation experience large initial drops in earnings and then a gradual recovery. Indeed, the recovery is slightly steeper among the survey *distress* in an administrative displacement than in the administrative displacement overall. The earnings trajectory of the *other* separations are similar to the *distress*, except that the earnings recovery fades out more than three years past the separation. Such substantial earnings losses for this group should not come as a surprise given the fact that many of the survey reports (i.e., retirement, other family obligations, etc.) have implications for subsequent labor market status. In contrast, those reporting a *quit* experience modest earnings *gains* relative to the control group.

This heterogeneity in earnings losses across survey reasons provides evidence that noise in the survey responses does not explain why the administrative and survey measures of displacement are imperfectly aligned. Specifically, that the *quits* do much better than the remaining two survey reasons is consistent with these workers having a very different experience of the mass layoff. The next section takes up the question of how to interpret this heterogeneity in earnings changes by survey report.

4 Recovering earnings losses of a latent displacement

In this section we develop and implement a method to measure the earnings losses of the separations that are caused by the employer contraction. To do so, we combine the information in Section 2 and 3. Section 2 showed that the survey data and administrative data do not always agree on the

growth, reweighting the control group increases the rate of earnings growth, though this effect is quantitatively small.

reason for separation. Section 3 showed that the consequences of an administratively indicated mass layoff differ depending on the worker survey report. Now we use these differences to measure latent earnings losses.

Our approach uses the survey data to help interpret the administrative indicator of displacement. Hence, we treat the administrative and survey reports asymmetrically. An alternative approach would treat administrative and survey measures symmetrically, view them both as noisy measures of the same underlying phenomenon, and use one as an instrument for the other. These two approaches answer different questions. Using the survey measure as an instrument for the administrative measure answers the question, "what are the effects of separations that survey and administrative data agree are due to firm distress?" Our approach answers the question, "what are the effects of the separations that are caused by the firm contracting, and how does that differ by survey reason?" The reason we pursue our approach is that the administrative data approach is the benchmark in the literature, so we seek to supplement it with information in the survey data. Moreover, our approach provides a natural way of combining the descriptive results in the previous two sections.

4.1 Overview

Panel B of Figure 1 plots the probability of reporting each kind of separation as a function of employer growth rates. It shows that the probability of reporting all kinds of separations rises rapidly as the employer contracts. If we assume that the underlying propensities of individuals to separate for a particular reason is independent of the firm growth rates, then the fact that the *quit*, *distress* and *other* probabilities rise as the firm contracts suggests that many of these separations are caused by the firm contraction. The next section formalizes this logic and shows how to use this assumption to also learn about the earnings changes caused by the employer contraction. Sorkin (2018) uses similar reasoning to probabilistically distinguish between separations that are and are not caused by an employer contraction, but does not incorporate survey data.

4.2 Methodology for identifying latent displacements and its consequences

We are interested in estimating the effects of a separation in a mass layoff that is due to economic distress, which we call a *latent displacement* and denote by ML^*. ML^* differs from separations observed in mass layoff (or ML) in that it only contains the separations that are caused by the employer contraction, and not merely coincident with it. We allow the possibility that separations associated with any worker-survey response, *distress*, *quit*, or *other*, are a latent displacement.

We now define some notation. Let s be a particular survey reason for separation, $s \in \{distress, quit, other\}$. In a slight abuse of notation, we will use ML_s and ML_s^* to refer to separations in an observed mass layoff based on administrative data (ML_s) or a latent displacement (ML_s^*) indexed by the worker's report of a particular survey reason. Let $\Delta\mathrm{earn}_k$ be the earnings change in a particular displacement time (δ_k from Section 3). Define ω_s to be the reported share of survey reason s in a mass layoff, while ω_s^* is the latent share.

The standard earnings loss regression is the linear least squares projection corresponding to:[20]

$$\mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{ML}] = \sum_s \omega_s \mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{ML}_s]. \tag{3}$$

We are instead interested in

$$\mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{ML}^*] = \sum_s \omega_s^* \mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{ML}_s^*]. \tag{4}$$

Comparing equations (3) and (4) reveals two reasons why the earnings losses caused by the contraction might differ from the benchmark results. First, the shares might differ. For example, it might be that the benchmark approach overstates the share of quits that are caused by the contraction and so leads to an underestimate of earnings losses. Second, the earnings changes might differ. For example, it might be that quits that are caused by the mass layoff have very different earnings changes than the quits that would have happened anyway.

To estimate the $\mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{ML}_s^* = 1]$, consider the following pointwise relationship (i.e., for all k from Section 3)

$$\mathbb{E}[\Delta\mathrm{earn}|\mathrm{ML}_s] = \pi_s \mathbb{E}[\Delta\mathrm{earn}|\mathrm{ML}_s^*] + (1-\pi_s)\mathbb{E}[\Delta\mathrm{earn}|\text{not } \mathrm{ML}_s^*], \tag{5}$$

where $\pi_s = \Pr(\mathrm{ML}_s^*|\mathrm{ML})$ is the probability that a separation and survey response is caused by

[20]Note that if the coefficients on covariates vary by survey response, then this aggregated version will differ from running the benchmark regression. The benchmark and "aggregated" versions turn out to be nearly identical. See appendix Figure A1.

the firm-level contraction. Below, we use the notation $\Pr(\text{not ML}_s^*|\text{ML}) = 1 - \pi_s$ to refer to the ML separations that are not caused by the mass layoff. This equation says that observed earnings changes given a mass layoff and a survey response are a mix of workers who separate because of the mass layoff and workers who would have separated anyway.

To estimate the ω_s^*, we use the following relationship:

$$\omega_s^* = \frac{\pi_s \omega_s}{\sum_s \pi_s \omega_s}. \tag{6}$$

This equation says that the latent shares differ from the observed shares to the extent that survey responses differ in how likely they are to be causally related to the employer contraction. For example, we find that survey reports of distress are more likely to be caused by the contraction than survey reports of quits.

Let RS_s be notation that we use analogously to ML_s to refer to separations in the relatively stable region of employer growth. Our identifying assumptions are:

Assumption 1: $\Pr(\text{not ML}_s^*|\text{ML}) = \Pr(\text{RS}_s)$;

Assumption 2: $\mathbb{E}[\Delta\text{earn}_k|\text{not ML}_s^*, \text{ML}_s] = \mathbb{E}[\Delta\text{earn}_k|\text{RS}_s]$.

Assumption 1 says that we can estimate the probability that a separation would have happened regardless of what was going on at the firm by looking at the separation probability in the relatively stable region. Assumption 2 says that we can estimate the earnings losses of the separations that would have happened in the absence of the firm-level contraction by looking at the earnings losses of those who separate in the relatively stable region.

Assumption 1 allows us to estimate the probability a separation was caused by the contraction by the excess probability of separation when the employer contracts relative to the relatively stable region:

$$\pi_s = \frac{\Pr(\text{ML}_s) - \Pr(\text{RS}_s)}{\Pr(\text{ML}_s)}. \tag{7}$$

We then rearrange equation (5) and substitute in for our various assumptions to have:

$$\underbrace{\mathbb{E}[\Delta\text{earn}_k|\text{ML}_s^*]}_{\text{latent earnings losses}} = \frac{1}{\pi_s}\mathbb{E}[\Delta\text{earn}_k|\text{ML}_s] - \frac{(1-\pi_s)}{\pi_s}\mathbb{E}[\Delta\text{earn}_k|\text{RS}_s]. \tag{8}$$

This equation shows that two things have to be true for the earnings losses in latent displacements, ML_s^*, to differ from those in the observed displacement, ML. First, there needs to be a difference between ML_s^* and ML_s, formally, $\pi_s < 1$. Second, the earnings losses in ML_s need to differ from the earnings losses in the relatively stable region, formally, $\mathbb{E}[\Delta\text{earn}_k|\text{RS}_s] \neq \mathbb{E}[\Delta\text{earn}_k|\text{ML}_s]$.

As an example of the calculation in equation (5), suppose that the average quit leads to a gain of \$10. Suppose that a quit at a contracting employer leads to a gain of \$5 and that 50% of these are excess quits. Then we infer that these extra quits had a gain of \$0, since $0.5 \times 10 + 0.5 \times 0 = \$5$, where the 0 on the left-hand side is the unknown quantity that we solve for.

Performing the calculations in equations (7) and (8), allows us to use equation (4) to estimate the object of interest.

4.3 Probabilities and shares

The probability of separation for all survey-reported reasons is much higher when firms undergo large contractions than when they are relatively stable. Rows (1) and (2) of Table 4 contain the numerical version of the differences evidence in Figure 1.[21] Converting to the probability that the separations are caused by the employer contraction using equation (7), row (3) of Table 4 shows that the *distress* separations have a much stronger causal connection to firm growth than the *quit* and *other* separations. Specifically, 96% of the *distress* separations are caused by the firm contraction, while only 77% of the *other* separations and 67% of the *quit* separations are caused by the firm contraction. This finding is consistent with the intuition that even at relatively stable firms, workers are likely to be quitting (or separating for other reasons), and so many of these *quits* and *other* separations would have happened anyway.

These different probabilities by survey reason alter the weights placed on different categories of survey separation when computing earnings losses caused by the contraction. The bottom two rows of Table 4 show the shares used in equations (3) and (4) to aggregate the earnings changes by category. As can be anticipated from the different probabilities, this procedure means that we place more weight on the *distress* separation and less weight on the *quit* and *other* separations.

It is helpful to contrast our approach to an approach that would take the survey data at

[21] Summing across the three categories in row (1) we find a total separation probability of 0.102. Given that the definition of a mass layoff is that the firm is contracting by 30% or more, some readers might wonder how to reconcile these two facts. The reconciliation lies in the fact that the workers in our sample are the higher-tenure subset of the workers at the firms.

face value. In particular, if we were to take the survey data at face value and only treat events where the survey and administrative data agree as a latent displacement, then from row (1) of Table 4, we would conclude that only $\frac{0.055}{0.055+0.021+0.026} = 54\%$ of separations in the mass layoff are latent displacements. Instead, our approach allows the *quit* and *other* survey responses to also be latent displacements and, combining information in row (1) and row (3), we find that $\frac{0.055\times0.964+0.021\times0.666+0.026\times0.768}{0.055+0.021+0.026} = 86\%$ of the separations in the mass layoff are latent displacements.[22]

4.4 Earnings losses caused by the contraction

We now compute the earnings losses for each of the three survey reasons for separation that are caused by the contraction.

Panel A of Figure 4 considers the *distress* survey reason. It plots the earnings components of equation (8). The red dashed line reproduces the solid red line from Figure 3, which measures the earnings changes of workers separating in administratively-indicated displacement where the survey reason is also *distress*. The blue line reports the earnings changes where in the administrative data the firm is relatively stable, but the worker's survey reason is *distress*. Significantly, workers reporting *distress* have better post-displacement earnings outcomes during a mass layoff than when the firm is relatively stable. This finding supports the adverse selection logic of Gibbons and Katz (1991) that workers who perceive distress as the reason for the separation do better when there are many workers leaving the firm and there is less scope for selection. The black solid line combines the blue line and red line to recover the latent earnings loss caused by the firm contraction. It is remarkably similar to the red dashed line of all the *distress* responses in a mass layoff. The reason is that we estimate that 96% of the survey-reported *distress* separations in a mass layoff are caused by the firm contraction and so the latent earnings loss places almost all weight on the ML earnings loss.

Panel B of Figure 4 considers the *quit* survey reason. The red dashed line—the ML line—reproduces the blue line from Figure 3, which measures the earnings changes of workers separating in administratively-indicated mass layoff where the survey reason is *quit*. The blue line reports the earnings changes where in the administrative data the firm is relatively stable, but the worker's

[22]There is rounding that explains the difference from the 85% one gets if one does the calculation directly.

survey reason is *quit*. The black line shows that the estimated earnings changes of the quits caused by the mass layoff (ML^*_{quit}) are worse than that measured from looking at the *quit* separations in mass layoffs directly (ML_{quit}). The reason is that the earnings gains to quits when the firm is relatively stable are much bigger than the earnings gains to quits when the firm is contracting. Since we estimate that about a third of the *quits* in the mass layoff are reaping these larger gains, the quits caused by the mass layoff must have worse outcomes. Nevertheless, the difference between ML_{quit} and ML^*_{quit} is not very big—at most a few percentage points.

Panel C of Figure 4 considers the *other* survey reason. The red dashed line—the ML line—reproduces the black line from Figure 3, which measures the earnings changes of workers separating in administratively indicated mass layoff where the survey reason is *other*. The blue line reports the earnings changes where in the administrative data the firm is relatively stable, but the worker's survey reason is *other*. The black line combines these two lines. The earnings changes of the *other* survey reason separations caused by the mass layoff (ML^*_{other}) is quite similar to all the *other* separations in the mass layoff (ML_{other}).

Finally, using equation (4), Figure 5 aggregates the latent measures across survey categories depicted in Figure 4 using the weights in Table 4 to measure the earnings losses of the separations caused by the firm contraction. The figure also reproduces the benchmark results from Figure 2. The earnings losses from the simple administrative-based measure are remarkably close to the earnings losses of the separations caused by the firm contraction.

Why are the observed earnings losses in a mass layoff (ML) so similar to the latent earnings losses (ML^*)? Two surprising features of the data drive this result. First, the sharp rise in the probability of all the survey reasons as employment declines in Figure 1 means that there is a large difference between proximate reasons reported by workers at contracting firms and the ultimate reason for their separation. As a result, the weights in equation (8) are quite high and so the first condition necessary for ML and ML^* to differ is also not found in the data (π_s is close to 1). Second, conditional on the survey reason, what is going on at the firm—whether it is contracting by a lot, or is relatively stable—does not have a large effect on earnings losses: ($\mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{ML}_s]$ is close to $\mathbb{E}[\Delta\mathrm{earn}_k|\mathrm{RS}_s]$). This means that the second condition necessary for the latent and observed measures to differ emphasized in equation (8) is not met.

In summary, the standard practice of using observed ML for overall earnings losses is not misleading, which is reassuring for the literature. Nonetheless, knowledge of the worker reason does contain important information about the consequences of the separation for workers' future labor market outcomes. In terms of earnings losses, we uncover substantial heterogeneity depending on survey responses and the administrative data categories. Yet, conditional on the survey reason for separation, the differences in earnings loss estimates between firms that are contracting and firms that are stable are small. This results in an estimate of the latent earnings losses of displaced workers that is quite similar to the estimate using administrative data alone. Hence, although we find evidence of sources of potential bias, they are quantitatively small and, to an extent, cancel out.

4.5 Implications for the cyclicality of earnings losses

Using administrative data, Davis and von Wachter (2011) emphasize the cyclicality of the earnings losses of displaced workers: workers displaced in recessions experience larger earnings losses than workers displaced in booms. Our framework highlights one mechanism that contributes to this finding. In a recession, the administrative displacements, ML, are more likely to be latent displacements, ML*. Mechanically, the reason is that in recessions labor markets are worse and so workers are less likely to be able to find jobs in anticipation of a displacement and so less likely to have a survey report of quit, which is associated with better outcomes.

To document evidence of this, we extend our previous analysis and link the 2008 SIPP panel to the LEHD. This allows us to contrast the relationship between survey and administrative reports in the 2000s and in the Great Recession.[23] The bottom panel of Table 2 shows that in the recession, the alignment between the survey and administrative measures is tighter than in our benchmark estimates. The bottom panel of Table 4 shows that the "distress" separations make up a greater share of the latent mass layoff separations in the recession than in our benchmark estimates. This evidence is consistent with Davis, Faberman, and Haltiwanger (2012) in that in a recession, fewer of the separations when firms contract are survey-reported quits.

To ask about the quantitative importance of this mechanism, we apply the Great Recession weights to our estimates. The reason we just change the weights is that conceptually, many things change in a recession (i.e., the consequences of each type of separation), and we want to focus on the

[23] We have separations from 2008:II to 2010:II, since the version of the LEHD we have access to ends in 2011:II and we need several quarters of records to verify that a separation actually occurred.

changes in earnings losses that are due solely to the changing composition.[24] Hence, mechanically, we take weights from the bottom panel of Table 4, and apply them to the earnings losses displayed in Figure 4. Figure 6 displays the results.

Aggregating, we find that in our data and sample period the changing composition of job separations in administratively measured displacements can account for very little of the cyclicality of earnings losses. Why is this? While Panel B of Table 4 shows that the weight shifts from *quit* to *distress*, which generates larger earnings losses, the weight also shifts from *other* to *distress*, which generates smaller earnings losses. This of course leaves plenty of room for complementary explanations, such as that in Huckfeldt (2016).

5 The role of zeros in earnings losses

Having used survey data to sort out the reasons for separations, we now turn to using the survey data to understand the labor market outcomes subsequent to the separation. As it turns out, the information in survey data about these labor market outcomes makes a striking difference for understanding labor market outcomes after a true displacement.

5.1 Zeros: unemployed or out of the labor force?

A common practice in estimating displaced worker earnings losses is to exclude earnings histories when a worker exhibits long spells of zero earnings following the separation (see Table A1). The reason is that in administrative data, it is hard to know whether the zero earnings represent periods of being out of the labor force, or periods of looking for work. If we could distinguish between these reasons, however, we might want to include the zero earnings associated with looking for work because these losses represent an extreme reduction in hours following the displacement. In contrast, we might not want to include zeros associated with the being out of the labor force because this state is fundamentally different. So far in this paper we have followed the standard practice of omitting all earnings histories with a calendar-year of zeros in the four years following the separation.

Our link with survey data provides information on whether the zero earning histories that we

[24] As discussed in footnote 23, we also do not have a long enough span of data to estimate earnings losses in the Great Recession.

omit in our benchmark specification represent workers who are out of the labor force or are looking for work. We study the set of workers who have at least one calendar year of zeros following displacement (this is the earnings filter used by Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010), among others; see Table A1).

Panel A of Table 5 shows that over 40% of separators have zero earnings in a calendar year following the separations and are thus omitted from the analysis of earnings losses in Section 3.1. Most— 70% — of the separations that our zeros screen eliminates reported *other* as the survey reason for separation. This finding is intuitive since the *other* category contains many reasons for separation that are correlated with leaving the labor force.

Panel B of Table 5 looks at the activities of the workers in Panel A in the calendar-year in which they have zero earnings. We report all activities that the worker reports in the SIPP in that calendar-year, and so some workers report multiple activities. The Table shows that workers reporting firm *distress* are more likely to remain in the labor force than workers who lose their jobs for other reasons. 40 percent of separations citing firm *distress* report looking for work, while this share is only 6 percent for the other categories.

Panel C of Table 5 demonstrates that these survey responses predict different subsequent labor market outcomes. We aggregate the activities in Panel B across the survey reasons for separation. In the spirit of Flinn and Heckman (1983), we consider whether workers reporting their activity as "looking for work" (which most closely corresponds to the CPS notion of unemployed) are more likely to subsequently find a job than workers reporting the other categories of activities. The bottom panel of Table 5 reports the probability of having positive earnings at any point in the next eight quarters as a function of the activities reported by the workers. Consistent with the informative nature of the survey responses, the highest probability of nonzero earnings is among workers reporting "looking for work" and "employed."

5.2 The role of zeros in earnings losses

To assess the role of zero-earnings in the measurement of post-displacement earnings, we create two additional samples besides our benchmark sample of workers who are consistently employed (*no zeros*). The two alternative samples

- add back in the workers with earnings histories with zero earnings that are dropped by our zeros screen whether it looks like they are unemployed or out of the labor force (*all zeros*); and

- include only workers with earnings histories with zero earnings that identify in the SIPP that they are looking for work in the four quarters following the separation and thus would be classified as unemployed (*some zeros*).

Panel A of Figure 7 reproduces the finding (e.g., Davis and von Wachter (2011)) that there are large differences in earnings losses depending on the two treatments of zeros that are available to researchers using only administrative data. The *no zeros* line shows the standard treatment of zeros. The *all zeros* line includes "all" zeros; i.e., it includes workers who are out of the labor force and unemployed. The difference between these two lines is about 15 percentage points of pre-displacement earnings. Typical analysis of displaced worker earnings losses would stop there and leave it to the reader to make up their minds which line they preferred.[25]

Our use of survey data allows us to add back in only the earnings histories of workers who stay in the labor force following the displacement. The red dashed line in Panel A of Figure 7 shows that doing this—the *some zeros* line—results in earnings losses that are about halfway between the two extreme treatments of zeros. That is, many displaced workers continue to look for work but have significant spells of no earnings following their separation. Relative to the typical treatment of dropping the zeros, adding in these zeros approximately doubles the long-term earnings losses.

Panels B to D in Figure 7 show that these workers who are unemployed following the displacement are exclusively those who report *distress*. In Panel B, which focuses on workers who report *distress*, almost all of the workers with zeros are unemployed and so the *some zeros* line is close to the *all zeros* line. In contrast, for the *quit* and *other* survey reasons the workers with zeros are almost all out of the labor force, so the *some zeros* line is very close to the *no zeros* line.

5.3 Employment among false zeros

Table 5 shows that despite these being quarters with *zero* administrative data earnings, many workers report being employed. Among the quits 89% of workers report being employed, while this number is only 30% among those who separated due to distress. An obvious explanation is that,

[25] Appendix Figure A2 shows the results of two alternative zeros filters. The first filter drops all quarters in which there are zero earnings. The second filter drops all earnings histories in which there are ever quarters of zeros.

while our administrative data covers a large majority of the workforce, it is still possible for an individual to transition to a job not covered by the data. In particular, more informal employment arrangements such as working for a family member might not report to the UI system. Additionally, our version of the LEHD does not contain federal government employment.

Table 6 shows that working for government or family members is less common among workers who separated due to distress than other separations. The table investigates workers who report being employed in the survey, but for whom the administrative data records zero earnings. Part-time work is another kind of employment that might be less likely to be covered and/or reported to the UI system. We find substantial amounts of part-time work among the zeros (34 percent among those citing firm *distress*). Finally, the table indicates that the survey reported earnings are low. Conditional on positive earnings in the SIPP, the mean level of earnings is around $4500 a quarter among workers separating due to *distress*.[26]

6 Conclusion

This paper studies why workers separate from their jobs and how the consequences of these separations depend on the reason. Specifically, we look at workers who are labelled displaced using the standard administrative data approach, and ask what these workers say about why they separated. Almost half of such workers report reasons other than firm distress, including a large share (about 20%) who report quitting to take another job. Similarly, at firms that administrative data would indicate are doing fine, we find evidence that workers separate and give a survey reason of displacement.

We also find that even given the administrative data indicator, there is significant heterogeneity in the consequences of the separations that depends on the survey reason. For example, the workers who report *quit* in an administrative mass layoff experience earnings gains relative to the control group of non-separators.

Surprisingly, this heterogeneity in earnings losses by survey reason, conditional on the administrative data distress indicator, is larger than the heterogeneity in the other direction. That is,

[26]Even though we are looking at a sample of people who report employment in the survey, not all of them actually report positive earnings. Indeed, among the problematic group of survey respondents who reported *distress* in the survey, have zero administrative earnings, and claim to be employed, only 55 percent actually report positive earnings in the survey.

conditional on the survey reason, what is going on at the firm does not have a large impact on the earnings changes of workers.

What the administrative indicator does do, however, is shift the composition of separations. Not surprisingly, survey reports of *distress* account for a much greater share of separations at the mass layoff firms than at the relatively stable firms. Even though the composition of separations shifts, it is still the case that the probability of separating and reporting non-distress survey reasons rise dramatically when the employer contracts.

We then develop a method to combine the information in the survey and administrative data and measure the consequences of the separations that were ultimately caused by the firm contraction. We find that the earnings consequences of the separations ultimately caused by the employer contraction are quite similar to those captured by the standard administrative measure. Two intermediate results drive this finding. First, because the probability of all types of separations rises dramatically, most of the separations in the administratively indicated mass layoff are caused by the mass layoff. Second, the earnings changes associated with each survey report do not depend that much on the state of the employer. Indeed, the result of our method is to increase the alignment of the administrative measure with survey displacement from 54% in the raw data to about 85%.

Additionally, we use the combination of administrative and survey data to shed light on the conceptually distinct issue of how to treat displaced workers with persistent zero earnings. The standard practice in the displaced worker literature is to exclude observations with long stretches of zero earnings. Using the survey data, we can distinguish whether these zero earning individuals were looking for work or not. Including those who were looking for work approximately doubles the estimate of long-term earnings losses following a displacement.

More generally, this paper has demonstrated the usefulness of combining administrative and survey measures of the same outcome. Administrative data are attractive because they provide precise measures of outcomes, often on very large samples. The reason for the outcomes, however, must typically be inferred in the administrative data. The linked survey data provide worker-level information on the reason for the outcomes. Our results show that the standard practice of using a mass layoff indicator in administrative data leads to estimates of earnings losses very similar to the approach proposed in this paper that combines survey and administrative measures.

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Table 1. Survey Reports of Cause of Separation among SIPP Respondents Matched to LEHD Jobs

Panel A: Survey Indicators Captured by Admin Indicators

Detailed Survey Reason for Separation	Share of Separations (1)	Share with ML Indicator (2)
Distress		
On layoff	0.14	0.23
Employer bankrupt/sold business	0.03	0.62
Slack work or business conditions	0.03	0.18
Total	0.20	0.28
Quit		
Quit to take another job	0.32	0.05
Other		
Quit for some other reason	0.14	0.08
Retirement or old age	0.11	0.04
Unsatisfactory work arrangement	0.08	0.04
Discharged/fired	0.07	0.06
Other family/personal/child obligation	0.04	0.04
Own illness/injury	0.03	0.04
Job was temporary and ended	0.01	0.13
School/training	0.01	0.09
Total	0.49	0.06
Total Separations	6,500	0.10
Total Continuers (Unique Persons)	205,600 (28,000)	0.02

Panel B: Admin Indicators Captured by Survey Indicators

	Survey reason for separation	
	Distress	Not Distress
Mass Layoff Indicator		
Yes	55%	45%
No	16%	84%

Source: SIPP-LEHD as explained in text.
This table reports the survey-identified responses for the reason for separation, at a person-quarter frequency. The second column reports the share of total separations represented by the particular reported reason. The final row of Panel A identifies the number of person-quarter continuing jobs in the sample. Sample counts are rounded to the nearest hundred. Approximate sample counts for the rows in Panel A can be inferred by multiplying the entries in column (1) with the number of total separations.

Table 2. Alignment between Survey Indicators and Alternative Indicators

	Survey reason for separation		
	Distress	Quit	Other
ML indicator: baseline			
yes	0.55	0.17	0.28
no	0.16	0.33	0.51
ML: 40% contraction			
yes	0.56	0.15	0.29
no	0.17	0.33	0.50
ML: 5% contraction			
yes	0.51	0.15	0.34
no	0.19	0.32	0.49
ML: 8 quarters			
yes	0.46	0.21	0.33
no	0.16	0.33	0.51
ML: 16 quarters			
yes	0.36	0.25	0.39
no	0.15	0.34	0.51
UI receipt			
yes	0.66	0.03	0.32
no	0.11	0.37	0.52
Severance pay			
yes	0.70	0.04	0.26
no	0.17	0.33	0.50
ML indicator: 2008 SIPP panel			
yes	0.62	0.14	0.25
no	0.30	0.22	0.48

Source: SIPP-LEHD as explained in text.
This table reports a variety of robustness checks on the measures of alignment reported in Panel B of Table 1. The first panel reports the main results. The second and third panels explore sensitivity to the severity of the contraction to define the mass layoff indicator. The benchmark contraction is 30%. The second panel uses a 40% contraction, while the third panel uses a 5% contraction. The fourth and fifth panel explore sensitivity to the timing of the contraction. The benchmark uses four quarters to compute the growth rate to define the mass layoff indicator (from $t-3$ to $t+1$ for a separation in quarter t). The fourth panel uses eight quarters, while the fifth panel uses sixteen quarters. The sixth panel defines an indicator based on whether the worker reported collecting unemployment insurance (UI) in the SIPP in the nine months following the separation. The seventh panel defines an indicator based on whether in the SIPP the worker reported receiving severance pay. The eighth panel reports the benchmark alignment using the 2008 SIPP panel.

Table 3. Characteristics in the Mass Layoff Comparison

	ML Separators Relative to Stationary Continuer Shares	
	unweighted	weighted
	(1)	(2)
Worker Education Levels		
High School or Less	11.75	-0.01
Some College	-2.14	-0.03
College or More	-9.62	0.04
Worker Age Categories		
Age25-34	11.09	-0.03
Age35-44	-0.16	0.05
Age45-54	-8.61	0.00
Age55-59	-2.81	0.00
Age60-74	0.48	-0.01
Worker Earnings Deciles		
Decile 1	3.34	0.00
Decile 2	3.42	-0.02
Decile 3	2.93	0.00
Decile 4	-0.07	0.00
Decile 5	-2.35	0.00
Decile 6	-0.15	0.00
Decile 7	-2.44	-0.01
Decile 8	-3.34	0.00
Decile 9	-2.21	0.02
Decile 10	0.88	0.01
Worker Gender		
Male	11.02	-0.02
Female	-11.02	0.02
Employer Size Categories		
Size 50-99	17.72	-0.04
Size 100-249	9.34	0.02
Size 250-499	4.99	0.00
Size 500-999	0.65	0.01
Size 1000-2499	-6.76	0.01
Size 2500+	-25.95	0.00
Employer Industry		
Other Industries	-7.43	0.00
Construction	5.28	-0.01
Manufacturing	8.26	0.01
Wholesale/Retail/ Trans/Warehousing	1.25	-0.01
Information	4.21	-0.02
Finance/Insurance/Real Estate	1.52	-0.01
Professional/Technical Services	6.54	0.01
Management	5.01	0.02
Health/Education	-24.64	0.00

Source: SIPP-LEHD as explained in text.
This table reports differences in observable characteristics between the administratively defined mass layoff separators to the control group of continuers at stationary firms. Column (1) reports differences in population shares. Within each broad category, the differences thus sum to zero. Column (2) reports differences in the population shares after having reweighted the control group to look like the mass layoff separators.

Table 4. Latent Firm Contribution to Survey Reports

Panel A. Main sample

		Survey reason (s)		
		Distress	*Quit*	*Other*
(1)	Pr(Separation$_s$ | ML)	0.055	0.021	0.026
(2)	Pr(Separation$_s$ | Relatively stable)	0.002	0.007	0.006
(3)	Pr(ML$_s^*$ | ML$_s$)= π_s	0.964	0.666	0.768
(4)	ω_s = Share$_s$ | ML	0.542	0.204	0.254
(5)	ω_s^* = Share$_s$ | ML*	0.612	0.159	0.229

Panel B. SIPP 2008 panel

		Survey reason (s)		
		Distress	*Quit*	*Other*
(1)	Pr(Separation$_s$ | ML)	0.083	0.018	0.033
(2)	Pr(Separation$_s$ | Relatively stable)	0.005	0.004	0.008
(3)	Pr(ML$_s^*$ | ML$_s$)= π_s	0.942	0.833	0.813
(4)	ω_s = Share$_s$ | ML	0.619	0.135	0.246
(5)	ω_s^* = Share$_s$ | ML*	0.661	0.123	0.216

Source: SIPP-LEHD as explained in text.
This table reports the details underlying the construction of latent earnings loss estimates for the main sample (2001 and 2004 panels), and the 2008 panel. For each survey reported reason of separation, the first two rows record the probabilities of separation conditional on an administratively defined mass layoff (1) or when the firm is growing by between -5% and $+5\%$ (relatively stable) (2). The third row converts these conditional probabilities to estimates that each separation was caused by the employer contraction, using equation (7). Rows (4) and (5) show the shares of each survey identified reason for separation in constructing the aggregate earnings changes. The table reports these probabilities based on the reweighted samples detailed in Table 3; see appendix Table A7 for the unweighted version.

Table 5. Interpreting Zero Earnings Following Administrative Mass Layoff

Panel A. Survey reasons associated with zero earnings

	Total	*Distress*	*Quit*	*Other*
Share of separations with zero earnings	0.42			
Composition of separations by survey reason		0.20	0.10	0.70

Panel B. Activities[1]

	Distress	*Quit*	*Other*
Looking for work	0.40	(d)	0.06
Employed	0.30	0.89	0.26
Retired	0.07	(d)	0.36
Other	0.28	(d)	0.36

Panel C. Probability of subsequent positive administrative earnings[2]

Activity when zero earnings	
Looking for work	0.11
Employed	0.10
Retired	0.04
Other	0.08

Source: SIPP-LEHD as explained in text.

This table studies the role of zero earnings. The first row in Panel A reports the share of separations in an administrative mass layoff that meet the criteria of having any calendar-year of zero earnings in a four-year interval following a separation. The second row in Panel A reports the composition of those separations by survey reason. Panel B focuses on the set of workers in Panel A (i.e., those who the zeros earnings screen eliminates). It reports the activities of workers in calendar-years when they have zero earnings. A worker can appear multiple times in the table if there are multiple quarters that they appear in the SIPP-LEHD link in the calendar-years with zero earnings. Panel C reports the probability of subsequent administrative earnings in the calendar-years following the calendar-year with zeros, and grouped by activities in the calendar-year with zero earnings.

[1]Column shares do not sum to one because respondents can identify multiple activities within the quarter.

[2]Earnings in the eight quarters following the survey report. Note that we already condition on this quarter taking place in a calendar year of zero earnings and so mechanically there are no administrative earnings for several quarters in each observation.

(d) indicates output suppressed because of disclosure limitations.

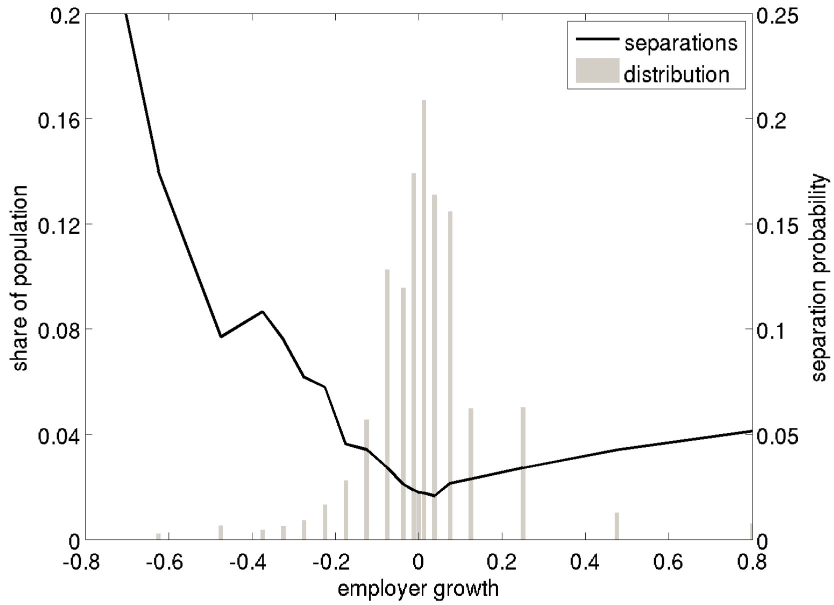
Table 6. SIPP Employment in Quarters with Zero Administrative Data Earnings

	Survey Reason for Separation		
	Distress	*Quit*	*Other*
Share: Work for Government or Family	0.15	0.24	0.22
Share: Part-time Worker	0.34	0.25	0.41
Share with Positive SIPP Earnings	0.55	0.67	0.49
Mean of Positive SIPP Earnings (2009 Dollars)	4,521	4,994	3,921

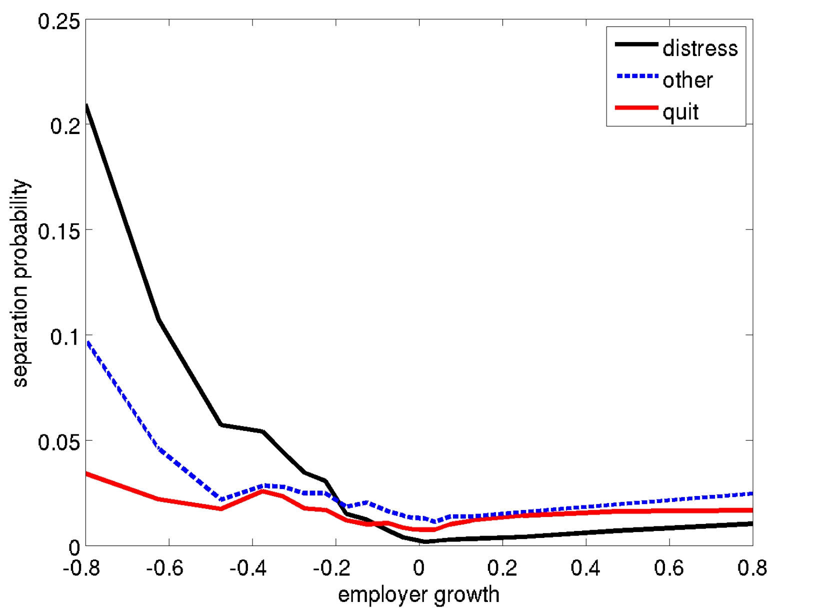
Source: SIPP-LEHD as explained in text.
This table reports worker response in quarters in the first year following a separation in which the worker had zero administrative data earnings but reported being employed in the SIPP. (See the lower panel of Table 5.) The first two rows record the percentage of these SIPP respondents reporting work in either government/family, or part-time circumstances. The last rows report the share of these respondents recording positive earnings in the SIPP, and the average value of those SIPP-based earnings.

Figure 1. Separation Rates by Firm Growth

A. Separation Rates



B. Probability of Separating and a Survey Reason

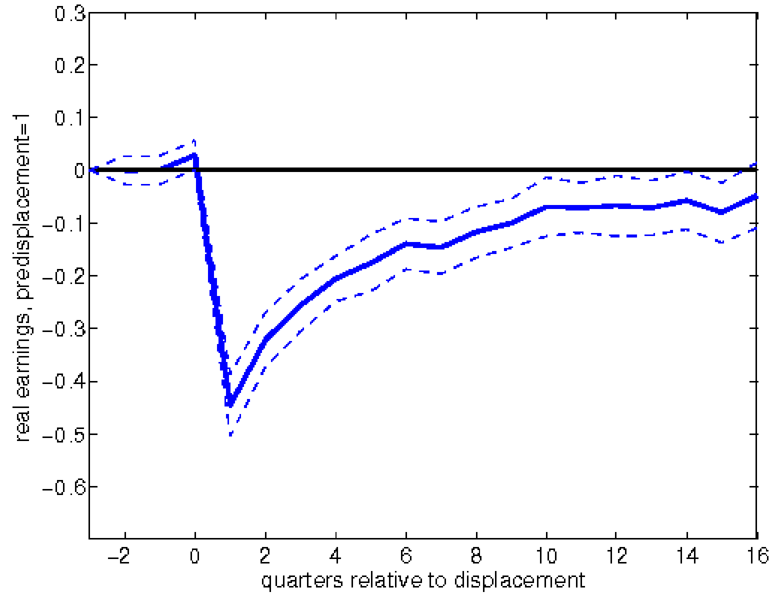


Source: SIPP-LEHD as explained in text.

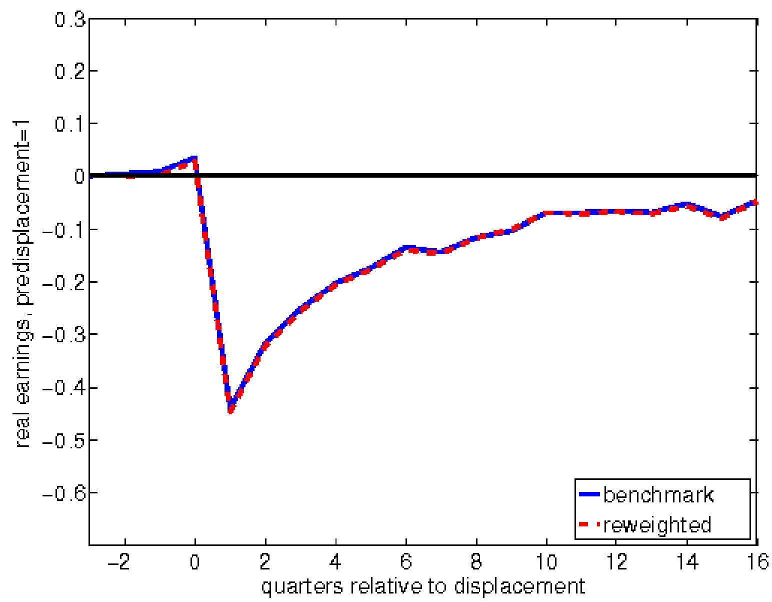
This figure shows how the probability of separating depends on the employer growth rate. In Panel A, the solid line shows the probability of separating, while the histogram shows the distribution of employment as a function of employer growth rate. The bins were selected by hand to allow enough data to satisfy disclosure requirements and allow the calculation of probabilities. The midpoints of the bins are as follows $\{-0.9625, -0.8125, -0.625, -0.475, -0.375, -0.325, -0.275, -0.225, -0.175, -0.075, -0.0375, -0.0125, 0, 0.0125, 0.0375, 0.075, 0.125, 0.250, 0.475, 0.800\}$. The bin at 0 is a mass point and is only present in Panel A. Panel B decomposes the solid line from Panel A by the three survey reasons for separations.

Figure 2. Benchmark Earnings Losses

A. Benchmark Administrative Measure

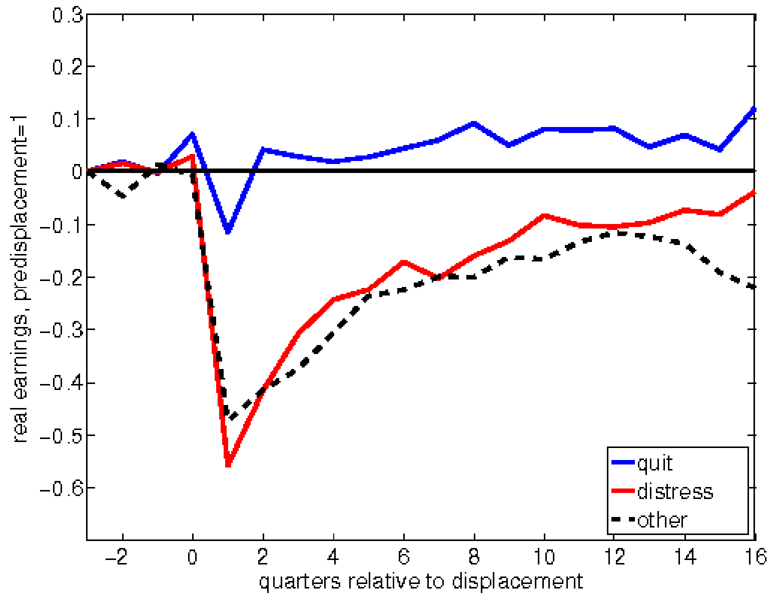


B. Reweighted vs. Unweighted Earnings Changes



Source: SIPP-LEHD as explained in text.
This figure plots earnings changes from comparing administratively defined mass layoff separators to continuers at stationary firms. Panel A reports the baseline unweighted estimates along with 95% confidence intervals. Panel B reports the weighted and unweighted estimates, while suppressing these confidence intervals for the sake of clarity. See equation (2) in the text. The units on the y-axis are fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

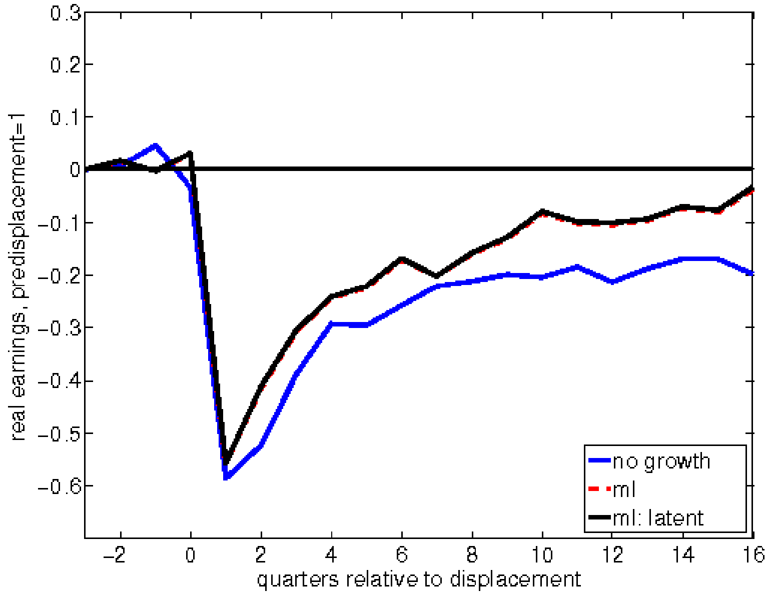
Figure 3. Mass Layoff by Survey Category



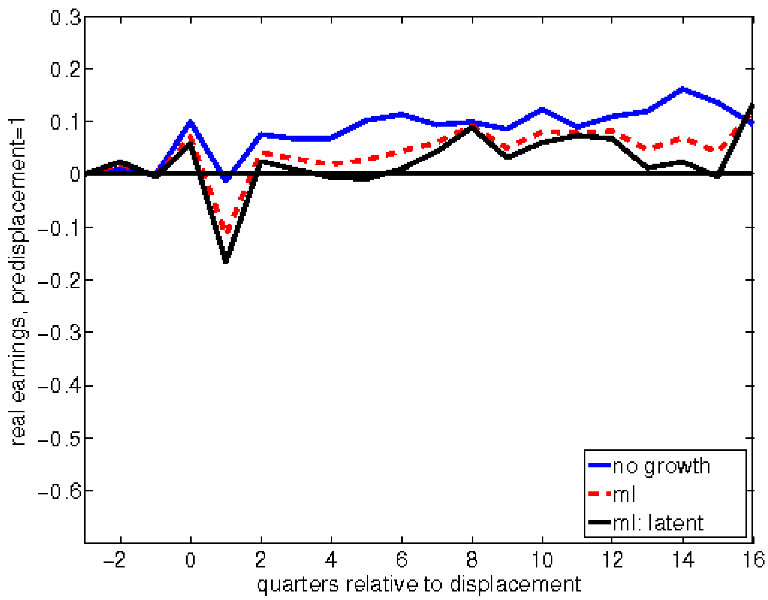
Source: SIPP-LEHD as explained in text.
This figure plots earnings changes from comparing administratively-defined mass layoff separators—split by survey reason for separation—to continuers at stationary firms. It reports the results of three separate regressions. Confidence intervals are suppressed for the sake of clarity. See equation (2) in the text. The units on the y-axis are fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

Figure 4. Earnings Losses in Separations Caused by the Firm Contraction

A. Survey Report of Distress

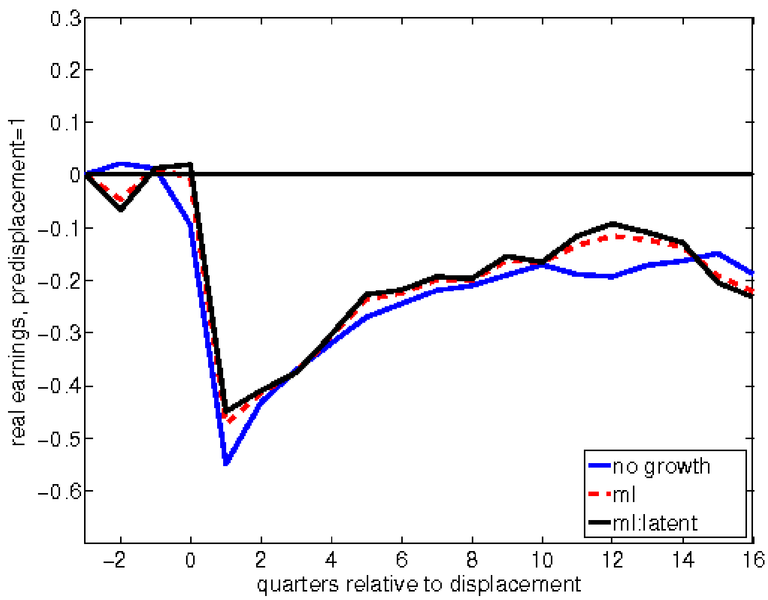


B. Survey Report of Quit



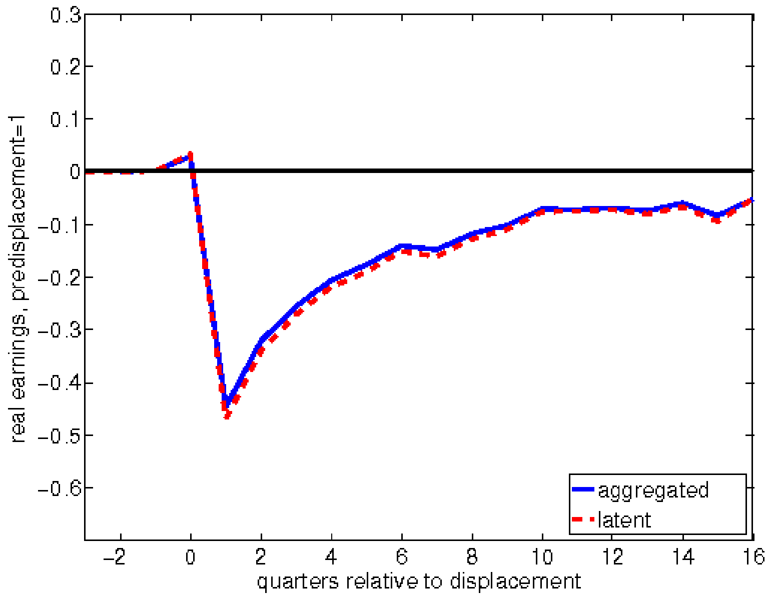
Source: SIPP-LEHD as explained in text.
Each panel plots the results of two regressions. The ML and no growth lines come from estimating versions of equation (2), where the "treatment" group is separators who report a given survey reason when the firm is contracting by 30% or more (ML) and when the firm is growing by between −5% and +5% (no growth). The two lines are then combined pointwise to form the ML: latent line using equation (8) and information in Table 4. The units on the y-axis are fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

C. Survey Report of Other



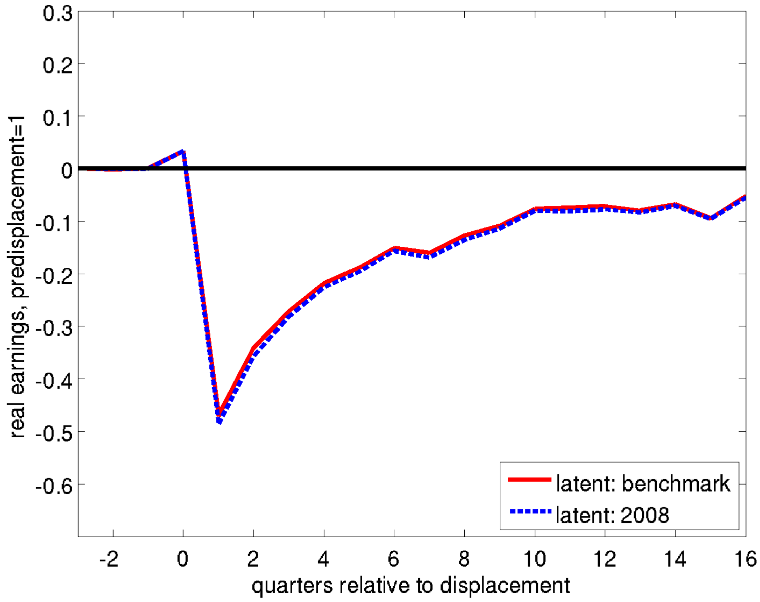
Source: SIPP-LEHD as explained in text.
Each panel plots the results of two regressions. The ML and no growth lines come from estimating versions of equation (2), where the "treatment" group is separators who report a given survey reason when the firm is contracting by 30% or more (ML) and when the firm is growing by between −5% and +5% (no growth). The two lines are then combined pointwise to form the ML: latent line using equation (8) and information in Table 4. The units on the y-axis are fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

Figure 5. Earnings Losses in Separations Caused by the Firm Contraction: Aggregated



Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of separations in a mass layoff that are caused by the contraction (latent) as well as the benchmark approach (aggregated). The latent line is constructed using equation (4) from the latent lines in Figure 4 and the shares in Table 4. The aggregated line is constructed using equation (3). Appendix Figure A1 compares the aggregated line in this figure to the single specification version in Figure 2. The units on the y-axis are fraction of predisplacement earnings, where the predisplacement earnings are normalized to 1.

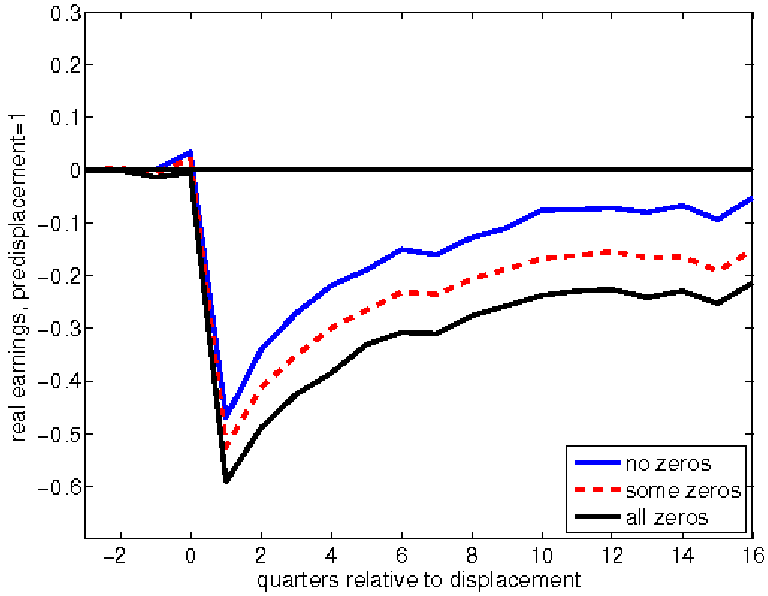
Figure 6. Implications for cyclicality of earnings losses



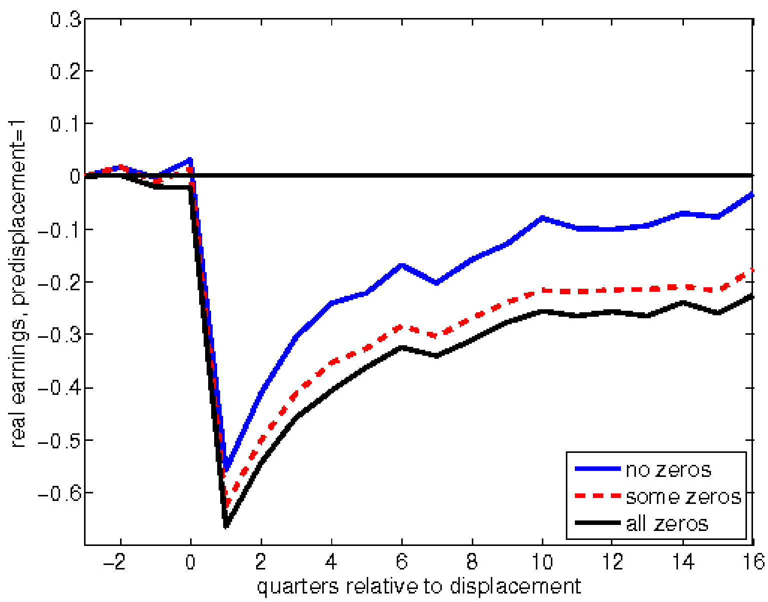
Source: SIPP-LEHD as explained in text.
This figure considers the effects of changing the weights from our benchmark sample to the weights in the 2008 SIPP panel. It plots the latent earnings losses using the weights on separation types from our benchmark sample, as well as from the 2008 SIPP panel (i.e., row 5, of Table 4, Panels A and B), applied to the earnings losses in Figure 4.

Figure 7. Earnings Losses: the Role of Zeros

A. Total Latent Measure

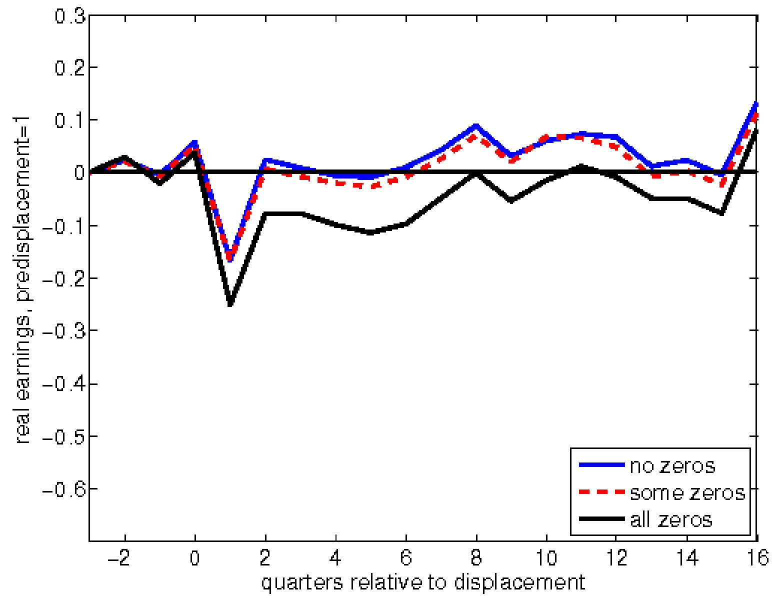


B. Survey Report of Distress

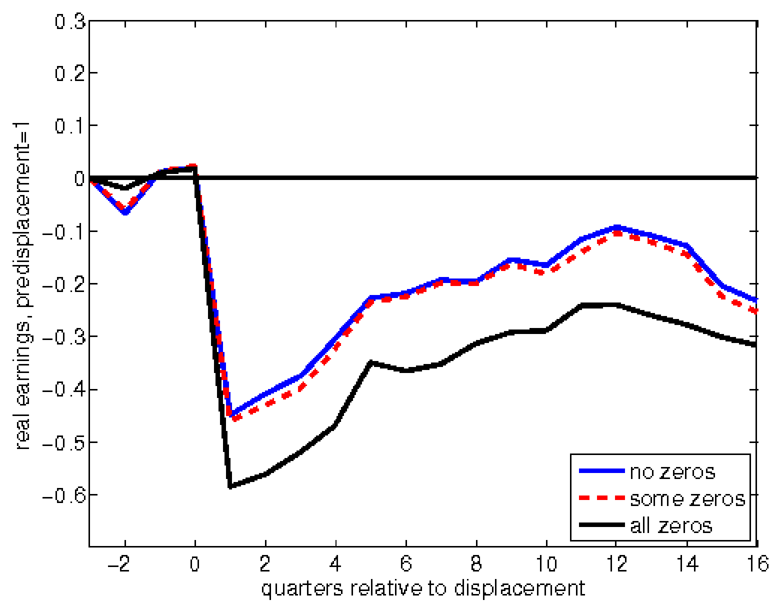


Source: SIPP-LEHD as explained in text.
This figure plots the latent notion of earnings losses in a mass layoff calculated using the method in section 4 and given by equation (8) based on three different treatments of observations with zero earnings. The *no zeros* line drop all earnings histories with a calendar year of zeros post-separation. The *some zeros* line includes the earnings histories dropped in the *no zeros* line where in the year after the separation the worker reports looking for work (being unemployed). The *all zeros* line keeps all earnings histories, which includes people who are either looking for work or out of the labor force. The units on the y-axis are fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

C. Survey Report of Quit



D. Survey Report of Other



Source: SIPP-LEHD as explained in text.
This figure plots the latent notion of earnings losses in a mass layoff calculated using the method in section 4 and given by equation (8) based on three different treatments of observations with zero earnings. The *no zeros* line drop all earnings histories with a calendar year of zeros post-separation. The *some zeros* line includes the earnings histories dropped in the *no zeros* line where in the year after the separation the worker reports looking for work (being unemployed). The *all zeros* line keeps all earnings histories, which includes people who are either looking for work or out of the labor force. The units on the y-axis are a fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

A Appendix: Matching Procedure, Properties of the Match and Variables

A.1 Separators

We match jobs in the SIPP to those in the LEHD in the following manner.

In the SIPP, we start with the universe of jobs with 12 months or more of tenure based on question TSJDATE: "When did ... start this job?". We assign the separations, which are monthly, to the relevant quarter. Row 1 of Table A5 shows the relevant counts. Starting with these jobs (which we refer to as SIPP-quarters) we then find jobs in the LEHD in the following order:

- Whether the worker in the SIPP ever had earnings in the LEHD (row (1) in Table A5);
- Whether the worker in the SIPP ever has earnings in the LEHD (row (2) in Table A5);
- We impose the requirement that the earnings in the LEHD be in the same quarter as the worker appears in the SIPP (row (3) in Table A5);
- We impose a tenure requirement by restricting attention to jobs with positive earnings in quarter t for which the worker also had positive earnings in quarter $t-3$, $t-2$ and $t-1$; (row (4) in Table A5)
- We impose a "full-time" earnings requirement by restricting attention to quarters with earnings that exceed 70% of 480 hours of work at \$4.25 (in 1991 dollars, the Federal minimum wage) (row (5) in Table A5);
- We then match if the job actually ends in the relevant quarter (row (6) in Table A5).

Table A5 provides details on the counts at each step. We start with 22,700 separations in the SIPP (row 1 in Table A5) and are able to match 10,100 of them to the LEHD (row (6) in Table A5).

A.2 Non-separators

For the sample of non-separators, we match the SIPP to the LEHD by looking in in the quarter occupying the majority of the relevant SIPP interview frame, and then follow that job in the LEHD. If we dont find positive earnings in that quarter, we then look on either side of that quarter for positive earnings. We impose a tenure requirement in an identical manner to the separators. Of course, we do not impose a separation requirement.

Table A5 provides further details. We start with 525,900 job-quarters in the SIPP and are able to match 348,100 of them to the LEHD.

A.3 Other Variables

A.3.1 Worker-Level Variables

Among the set of workers that we match, we construct the following variables in the LEHD:

- Total earnings in quarter t: we take the sum across all jobs in the LEHD (not just those passing the earnings test). We winsorize (topcode) at the 99th percentile of earnings in that quarter.[27]
- For workers who separate, we keep track of whether they have any earnings from their pre-separation employer in every quarter following the separation. We also record whether their pre-separation employer is their source of maximum earnings in a particular quarter.

A.3.2 Establishment-Level Variables

We restrict attention to workers earnings at least 35% of 480 hours at the 1991 minimum wage. We then create the following variables at the SEIN quarter level:

- Employment counts in quarter t: the number of workers with earnings above our threshold.

B Appendix: Cleaning Employer IDs

We might record a mass layoff when an employer shuts down, when in fact the employer identification number has just changed. Following Schoeni and Dardia (1996) and Benedetto et al. (2007), we use worker flows across establishments to correct longitudinal linkages.[28]

Table A6 presents a simplified version of Table 3 in Benedetto et al. (2007), which summarizes how we use worker flows to edit longitudinal linkages. The basic idea is that if most workers from an employer move to the same employer and then make up the majority of the new employer then this probably reflects an ID change. If most workers from an employer move to the same employer but make up a smaller share of the new employer, then this is more plausibly an acquisition/merger in which the new ID number swallowed the old ID number. The only difference from Benedetto et al. (2007) is that we use a 70% threshold rather than an 80%. The reason to do this is to be more conservative. It also aligns with Jacobson, LaLonde, and Sullivan (1993) definition of a displacement more tightly so that we know that the JLS mass layoffs are never associated with large flows of workers to a common employer.

When we observe an ID change or a merger/acquisition we go back and change the ID so that we have a consistent ID series. This correction allows us to compute employer-level outcomes.

C Alternative Ways of Identifying Economic Distress

The literature and some government programs contain other ways of attempting to measure separations due to firm distress.

C.1 Government Programs

Some U.S. federal government programs use definitions of mass displacements. These definitions are also displayed in Table A2. In general, these definitions focus on the number of separations (e.g. 50 or more worker separations), rather than the change in employer size (e.g. 30% contraction) as

[27] Couch and Placzek (2010, Web appendix A) topcode at $155,000 in 2000 dollars.

[28] Davis and von Wachter (2011) use an alternative strategy to mitigate concerns about measurement error in employer IDS: they alter their definition of displacement to exclude all cases where the ID disappears.

in the definitions in the economics literature. The BLS Mass Layoff definition has been used in academic research (e.g. Ananat et al. (2011)). The BLS Mass Layoff Program has been discontinued due to budget cuts, which serves to reinforce the value of alternative measures of displacements in administrative data.

C.2 Unemployment Insurance

While UI collection is not commonly used to measure the nature of worker separations, both Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010) report estimates of long-term earnings losses on the subset of workers who collect UI. Some papers also use unconditional UI collection as a measure of displacement: Jacobson, Lalonde, and Sullivan (2005) and Hilger (2016), which uses state UI records and tax records respectively. The goal of this measurement is to isolate separations that are not due to workers being fired for cause. A disadvantage of this approach, however, is that it conditions on future outcomes since it selects those workers who do not find jobs immediately.

C.3 Media Reports

A final alternative measure worth noting is one based on what the media covers as mass layoffs. Hallock (1998) is an outstanding example of this approach.[29] He looks at media reports of mass layoffs at public companies from 1987 to 1995.[30] An interesting feature of this data is that these layoffs are small compared to that reflected in economic studies. Chen et al. (2001, Table 3) replicate Hallock (1998) for 1990 to 1995 and report that the average share of the workforce involved in a layoff identified in this matter is 8.74%, while the median is 4.55%. One interpretation of this fact is that even though a large number of separations is required to attract media attention, public companies are large so this makes up a small share of their size.

D Appendix: Standard Errors

There are several issues concerning computing standard errors for the pooled specification in equation (2). First, insofar as there is heterogeneity in the displaced worker earnings losses, then we expect there to be serial correlation in the standard errors at the individual level. This concern arises even in specification (1). We address this concern by clustering at the person level. Second, a given person-quarter observation might appear several times. For example, if a person continues in a job for several quarters and then loses their job in a mass displacement, then a particular calendar quarter of earnings would show up in two different calendar times. This specification with a given observation potentially appearing multiple times is formally identical to the preferred specification in Dube, Lester, and Reich (2010), and we adopt their solution of clustering at the level of aggregation at which a given observation might appear multiple times.[31]

To summarize, our standard errors have the following structure: $E[u_{ik}^{y} u_{i'k'}^{y'}] \neq 0$ if $i = i'$ or $k + y = k' + y'$. As a result, we use the Cameron, Gelbach, and Miller (2011) two-way clustered

[29] See Farber and Hallock (2009) for additional references.

[30] He searches the *Wall Street Journal* for article abstracts containing the following words: layoff, laid off, downsize, plant closing, or downsizing.

[31] Davis and von Wachter (2011) implicitly have this issue in that their year-by-year estimates are not independent samples.

standard errors where we cluster at the person level and calendar time level. They show that the variance matrix is then $V^{IT} = V^I + V^T - V^{I \cap T}$ where the right-hand side are variance matrices from one-way clustering and I is the set of individuals and T is the set of calendar time periods.[32]

E Appendix: Propensity Score Reweighting

The basic idea of propensity score reweighting is to make the control group "look like" the treatment group. That is, we are interested in estimating the average treatment on the treated (ATT). To operationalize this reweighting, we estimate a propensity score, $\hat{p}$, to be in the treated group including all of the covariates in Table 3. We use a logit functional form. We construct a weight, $\frac{\hat{p}}{1-\hat{p}}$, to be in the control group. We then re-estimate equation (2) using these weights.

The literature has emphasized three implementation issues in propensity score reweighting: normalization, common support and "large weights." Busso, DiNardo, and McCrary (2014) emphasize in their finite-sample Monte Carlo results that it is important to normalize the weights. We normalize the weights so that the number of units in the control group is the same as before reweighting (i.e., the average weight is 1). Common support refers to whether there is overlap in the propensity score distributions between the treatment and control groups. Conceptually, if there is not overlap then the control group is very different from the treated group, and it is harder to imagine that these are randomly assigned. For each comparison, we verify that there is common support. Heuristically, this means that there are not (near) perfect predictors of being displaced. Finally, a concern emphasized by Crump et al. (2014) is that for propensity scores close to 1, the weights blow-up and in the bias-variance tradeoff, a researcher is better off dropping some observations.[33] In practice, the events that we study are relatively rare and so we do not have estimated propensity scores close to 1.

[32] In our application, we have over 30 clusters in the time dimension and over 30,000 dimensions in the person dimension.

[33] They are interested in the average treatment effect (ATE), and so have weights that look like $\frac{p}{1-p}$ and $\frac{1-p}{p}$ and so they recommend trimming weights both at the top and the bottom. We are interested in the average treatment on the treated (ATT) and so only have weights that look like $\frac{p}{1-p}$ and so their approach would only suggest trimming at the top.

Table A1. Sample Selection Restrictions on Worker Side in Administrative Measures

Paper	Dataset	Sample Selection
Jacobson, LaLonde, and Sullivan (1993)	Pennsylvania UI records (1974-1986)	6 or more years of tenure and 31-50 in 1980; men and women; positive earnings in each calendar year between 1974-1986;
Schoeni and Dardia (1996)	California UI Records (1989:I-1994:III)	all workers employed in aerospace in 1989:I (all ages, tenure, and men and women); positive earnings in each calendar year in the dataset;
Bowlus and Vilhuber (2002)	LEHD (1990-1999, 2 states)	full quarter employment 4 quarters before displacement, continually employed until displacement; in full quarter employment 4 quarters after the displacement (implicitly no zeros); 5 years of experience; men
Lengermann and Vilhuber (2002)	Maryland (1985:II - 1997:II)	full-quarter employment; all workers; zeros unclear (some specifications in logs)
Dustmann and Meghir (2005)	German Social Security	oldest worker is 35; "observe from labor force entry onwards"
Abowd, McKinney, and Vilhuber (2009)	LEHD	male and female workers between the ages of 18 and 70, with earnings during the quarter of greater than $250.00.
Couch and Placzek (2010)	Connecticut UI Records	workers born between 1949 and 1979 (19-49 in 1998); six years of continuous employment with the same employer from 1993 through the end of 1998; positive earnings in each year of the panel from 1993 through 2004
Davis and von Wachter (2011)	U.S. Social Security Records	3 years of tenure; 50 or younger; include years with zeros; men only
Andersson et al. (2014)	LEHD	4 quarters of employment prior to separation; no restriction on post-displacement earnings; all workers with earnings in a particular range
von Wachter, Handwerker, and Hildreth (2012)	California UI Records (1990-2000)	4 years of tenure; no post-displacement earnings restrictions; all ages; men and women;
Flaaen, Shapiro and Sorkin (2018) [this paper]	LEHD	25-74 years old in quarter of separation; 1 year of tenure; positive earnings in up to 4 calendar years following separation

Table A2. Administrative Measures of Displacement

Paper	Dataset	Definition
Jacobson, LaLonde, and Sullivan (1993)	Pennsylvania UI records (1974-1986)	in 1979 50 or more employees; employment in year following the separation is 30% below 1970's peak;
Schoeni and Dardia (1996)	California UI Records (1989:I-1994:III)	in 1989:I 50 or more employees; 1994:III employment is less than 1989:I employment
Bowlus and Vilhuber (2002)	LEHD (1990-1999, 2 states)	average from 1990-1999 is 50 or more employees; number of *separators* from $t-1$ to t (quarters) is at least 30% of average employment
Lengermann and Vilhuber (2002)	Maryland (1985:II - 1997:II)	for period they are in the data, employer averages 25 or more employees; reduction in employment of 30% from one quarter to the next
Dustmann and Meghir (2005)	German Social Security	Establishment Closing
Abowd, McKinney, and Vilhuber (2009)	LEHD	Reduction in employment from quarter to quarter is at least 30% of max employment from 1992 to 1997; fewer than 80% of workers move to a common other employer
Couch and Placzek (2010)	Connecticut UI Records	employer has 50 or more employees (not sure on when); separate within a year (before or after) of a 30% drop in employment below maximum employment from 1993 to 1998
Davis and von Wachter (2011)	U.S. Social Security Records	a separation in year t (positive earnings in $t-1$ and zero earnings in t) is a mass displacement if: i) employment in $t-2$ is greater than 50; ii) employment in t is between 1% and 70% of period $t-2$ employment; iii) employment in $t-2$ is less than 130% of $t-3$ employment; iv) employment in $t+1$ is less than 90% of $t-2$ employment
Andersson et al. (2014)	LEHD	25 or more workers in quarter t and a 4-quarter contraction of at least 30%
von Wachter, Handwerker, and Hildreth (2012)	California UI Records (1990-2000)	in 1990:I 50 or more employees; 30% contraction below maximum level at the beginning of the sample period; [robustness exercises with quarter to quarter drops, and plant closings]
Flaaen, Shapiro and Sorkin (2018) [this paper]	LEHD	50 or more workers in quarter $t-3$ and a 4 quarter contraction of 30%, or a 4 quarter gross flow measure of 20% or less
Government Program		**Defintion**
Mass Layoff Program		50 or more workers *filing* for unemployment insurance and not recalled within 31 days; at state UI account level
Worker Adjustment and Retraining Notification Act (WARN)		50-499 workers laid off when laid-off workers are at least 33% of the workforce; or all layoffs involving 500 or more workers at a physical location

Table A3. Survey Measures of Displacement

Survey	Involuntary Job Loss Reasons	Papers
Displaced Worker Survey (DWS) (question wording and recall window changed in 1994)	i) Plant or company closed down or moved; ii) Plant or company operating but lost or left job because of insufficient work; iii) Plant or company operating but lost or left job because position or shift abolished	Kletzer (1989) [reasons i) and iii)]; Topel (1990) [all reasons]; Neal (1995) [reason i)]; Farber (1993) [all reasons]; Gibbons and Katz (1991) [compare i) to(ii) and iii))]
Panel Study of Income Dynamics (PSID)	plant or business closing or due to being laid off or fired (excludes temporary jobs)	Topel (1990), Ruhm (1991), Stevens (1997), Stephens (2001), Stephens (2002), Charles and Stephens (2004), Lindo (2010), Lindo (2011), Krolikowski (Forthcoming)
Health and Retirement Study (HRS)	business closed, or laid off	Couch (1998), Chan and Stevens (1999), Stevens and Chan (2001)
National Longitudinal Study of Youth (NLSY)	plant closing or layoff (exclude people subsequently reemployed)	Kletzer and Fairlie (2003), Krashinsky (2002)
Survey of Income and Program Participation (SIPP)	layoff, slack work, or employer bankruptcy, or because the employer sold the business	Johnson and Mommaerts (2011), Flaaen, Shapiro and Sorkin (2018) [this paper]

The PSID coding, at least for 1969-1970, was based on an open-ended question: “What happened with that job—Did the company go out of business, were you laid off, did you quit, or what?” Boisjoly, Duncan, and Smeeding (1998, pg. 212 n. 5) examine a sample of the original coding and find that approximately 16% of respondents who were coded as “layoff, fired” in 1969-1970 reported being fired. The BLS Job Opening and Labor Turnover Survey (JOLTS) also does not distinguish between “laid off” and “fired” as it has a single category for “layoffs and discharges.” In the SIPP, the ratio of discharged/fired to separations we classify as distress as well as discharged/fired is 27% ($\frac{329}{329+892}$). See Table 1.

Table A4. Illustration of Methodology using Fictional Earnings Record

(1) Earnings	(2) Employer ID	(3) Calendar Time	(4) Event Time 1	(5) Event Time 2	(6) Event Time 3
10000	3653	2000:I	-3		
10000	3653	2000:II	-2	-3	
10000	3653	2000:III	-1	-2	
10000	3653	2000:IV	0	-1	
9500	3653	2001:I	1	0	
0	NA	2001:II	2	1	
8000	4511	2001:III	3	2	
9000	5205	2001:IV	4	3	-3
9000	5205	2002:I	5	4	-2
9000	5205	2002:II	6	5	-1
9000	5205	2002:III	7	6	0
9000	5205	2002:IV	8	7	1
Event			Continue	Sep.	Continue

Table A5. Properties of the SIPP-LEHD Match

		Continuers	Separators
(1)	SIPP	525,900	22,700
(2)	Positive LEHD earnings ever	499,800	22,000
(3)	Positive LEHD earnings in the relevant quarter	466,100	18,900
(4)	4 quarters of earnings before match	418,600	14,700
(5)	Pass an earnings test	374,000	10,500
(6)	Matched	348,100	10,100
(7)	Number of quarters	27	27

Note: This table shows the properties of the match. Row (1) shows the number person-quarters in the SIPP, where quarter is defined as the quarter in which the person appears in the SIPP. Row (2) shows the number of those person-quarters that have positive earnings in the LEHD. Row (3) shows the number that have positive earnings in the LEHD in the same quarter as in the SIPP. Row (4) shows the number that have 4 quarters of LEHD earnings before the match (i.e., that satisfy our tenure requirement). Row (5) shows the number of observations that also pass an earnings test. Finally, row (6) shows the final sample once we have dropped duplicates and the timing of the separation aligns with the SIPP.

Table A6. Successor/predeccessor flow and firm birth/death combinations

Link description	70% of successor comes from predecessor	less than 70% of successor from predecessor
70% of predecessor moves to successor and predecessor exits	ID Change	Acquisition/merger
70% of predecessor moves to successor and predecessor lives on	ID Change	Acquisition/merger

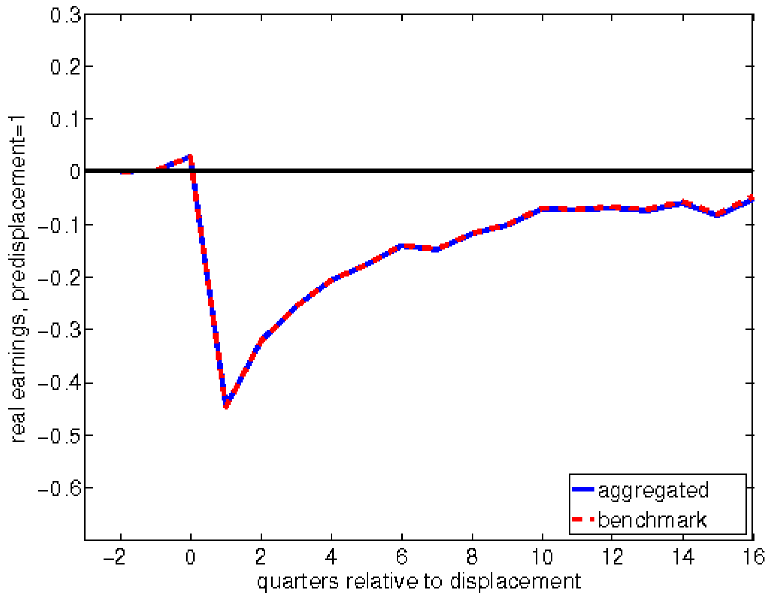
Note: this table is based on Table 3 in Benedetto et al. (2007).

Table A7. Latent Firm Contribution to Survey Reports (unweighted)

	Survey reason (s)		
	Distress	Quit	Other
Pr(Separation$_s$ — ML)	0.055	0.021	0.026
Pr(Separation$_s$ — No growth)	0.001	0.006	0.006
$\Pr(\mathrm{ML}_s^*\|\mathrm{ML}_s) = \pi_s$	0.974	0.726	0.767
$\omega_s = \mathrm{Share}_s\|\mathrm{ML}$	0.54	0.20	0.25
$\omega_s^* = \mathrm{Share}_s\|\mathrm{ML}^*$	0.61	0.17	0.22

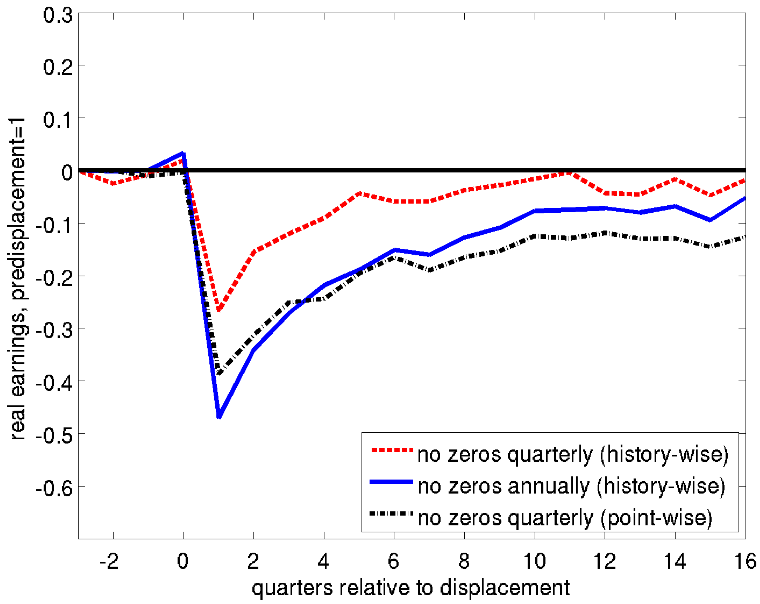
Source: SIPP-LEHD as explained in text.
This table reports the unweighted version of Table 4.

Figure A1. Mass layoff: benchmark and aggregated



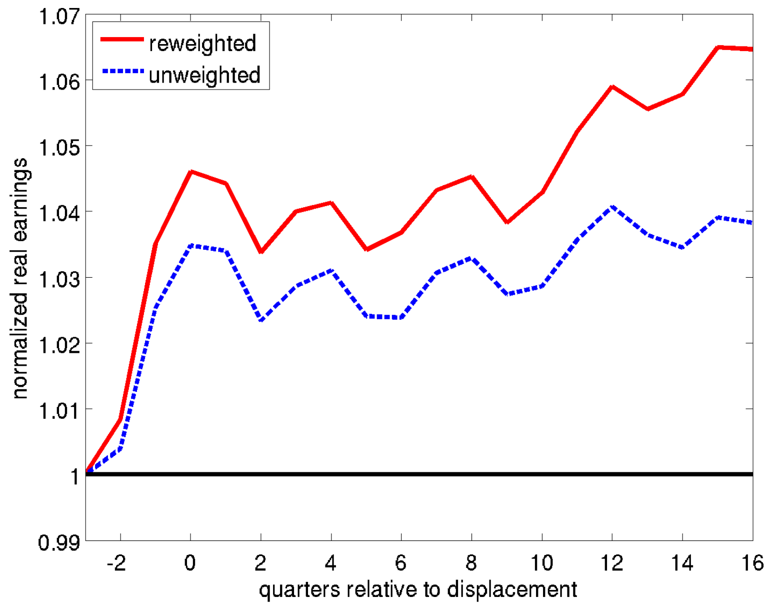
Source: SIPP-LEHD as explained in text.
This figure plots earnings changes from administrative mass layoffs computed in two different ways. The first way is from equation (2), which is also plotted in Panel A of Figure 2. The second way is from equation (3) in section 4, where we have estimated the earnings changes associated with each of the survey responses separately. This line is also plotted in Figure 5. Confidence intervals are suppressed for the sake of clarity. See equation (2) in the text.

Figure A2. Latent earnings losses: Alternative treatment of zeros



Source: SIPP-LEHD as explained in text.
This figure plots the latent notion of earnings losses in a mass layoff calculated using the method in section 4 and given by equation (8) based on three different treatments of observations with zero earnings. The annual no zeros (history-wise) line is our benchmark filter of dropping earnings histories that have a calendar year of zero earnings. The “no zeros quarterly (history-wise)” drops all earnings histories that have a quarter of zeros. The “no zeros (point-wise)” drops all quarterly earnings observations that are zero (but retains the rest of the history). The units on the y-axis are fraction of pre-displacement earnings, where the pre-displacement earnings are normalized to 1.

Figure A3. Control group mean earnings: weighted and unweighted



Source: SIPP-LEHD as explained in text.
This figure plots the mean earnings of the control group period-by-period with and without reweighting. The units on the y-axis are fraction of predisplacement earnings, where the predisplacement earnings are normalized to 1.