Reconsidering the Consequences of Worker Displacements: Firm versus Worker Perspective *

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December 1, 2016

Abstract

Displaced workers suffer persistent earnings losses. This stark finding has been established by following workers in administrative data after mass layoffs under the presumption that these are involuntary job losses owing to economic distress. Using linked survey and administrative data, this paper examines this presumption by matching worker-supplied reasons for separations with what is happening at the firm. The paper documents substantially different earnings dynamics in mass layoffs depending on the reason the worker gives for the separation. Using a new methodology to account for the increase in the probability of separation among all types of survey responses in a mass layoff, the paper finds earnings loss estimates that are surprisingly close to those using only administrative data. Finally, the survey-administrative link allows the decomposition of earnings losses due to subsequent nonemployment into non-participation and unemployment. Including the zero earnings of those identified as being unemployed roughly doubles the magnitude of the persistent earnings losses.

JEL Codes: J63, J65, J26, J0

Keywords: Unemployment, Job Loss, Earnings Losses

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Why do workers separate from their employers, what are the consequences of these separations, and how do they depend on the reason for the separation? A significant literature follows workers after they separate from their jobs. One important branch of this literature focuses on separations during a mass layoff. 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. While following workers after such mass layoffs has been tremendously fruitful (e.g., Jacobson, LaLonde, and Sullivan (1993) and Davis and von Wachter (2011)), it opens the question whether all separations in these events are due to economic distress. This paper treats economic distress as a latent event and uses information from both the firm and the worker to infer the reason and the earnings consequence of the separation.

Specifically, this paper addresses the following simple questions: what do workers who separate when their firm contracts rapidly by 30% or more—whom the literature typically refers to as displaced workers—say about why they separated? How do the consequences of the separation vary with the reasons workers give? Finally, what does this comparison say about the costs to workers of firms contracting?

To answer these questions, we link survey and administrative data. From the administrative data we learn whether the firm was shrinking—and by how much—when the worker separated. This allows us to construct an administrative measure of displacement. From the survey data we learn the worker's assessment of the reason for the separation. In particular, from the survey we know whether the worker thought the separation was due to firm distress; that is, it was a survey-based displacement.

There is substantial disagreement between the survey and administrative measures of displacement. Just over half of the survey reports agree with the administrative data in saying that there was economic distress at the firm. Among the survey reports that do not align with the administrative data, almost 20% of them report quitting to take another job, while the remaining 30% report separating for some other reason such as retiring, going back to school or taking care of family (which we label "other" reasons for separation).

We propose that this misalignment between survey reports and administrative data arises because workers report the proximate rather than the ultimate cause of their separation. To demonstrate this, we compute how the report of a particular survey reason for separation depends on the

firm growth rate. The probability of all forms of separation—i.e., quit to take another job, and other—rises rapidly as firms start to contract. This reproduces the finding of Davis, Faberman, and Haltiwanger (2012, Figure 6) for employer-side survey reports. Under assumptions that we detail below, this indicates that the firm contraction causes many of the worker separations that workers report as being unrelated to the contraction.

The distinction between the proximate and ultimate cause of the separation uncovers important heterogeneity in the consequence of the separation. The consequences of the administratively-labelled displacement depends on the survey-reported reason for displacement. Distress and other separations experience large—and somewhat persistent—earnings losses. On the other hand, workers reporting quit to take another job experience earnings gains relative to a control group of non-separators.

We then turn to understanding the effect of the ultimate cause: what are the earnings losses of the separations that were caused by the firm contraction? We answer this question in two steps. First, which separations were ultimately caused by the firm contracting? Second, what are the earnings losses of the separations that were ultimately caused by the firm contracting?

To answer these questions, we distinguish probabilistically between the separations that would have happened in the absence of a firm-level shock, and those that are related to the firm-level contraction. The key assumption is that the workers at the stationary firms (i.e., neither growing nor shrinking) provide a counterfactual to the workers at firms that undergo a large contraction. This assumption follows the tradition in the displaced worker literature that at large firms the growth rate is orthogonal to worker characteristics. In support of this assumption, while there are some observable differences between workers at firms that contract and firms that are stationary (and between workers that separate at contracting firms and workers that continue at stationary firms), reweighting on the basis of these characteristics makes no difference to our estimates.

To learn which separations were ultimately caused by the firm contracting, we compare the probability of separating and reporting a particular survey reason at contracting and stationary firms. The excess probability at the contracting firm gives the share of a particular kind of separation that is related to the firm contracting. We find that almost all of the survey reports of distress at the contracting firms are related to the contraction. In contrast, fewer of the quit and other survey reported separations are related to the contraction.

To learn about the earnings losses of the separations that were ultimately caused by the firm contracting, we condition on a particular survey reason and compare the earnings changes of workers who separate from the contracting and stationary firms. We assume that the observed earnings changes of the workers separating from contracting firms are a linear combination of two earnings changes: first, the earnings changes in the absence of a firm-level contraction, and, second, the latent earnings changes that are related to the firm-level contraction. To back out the latent earnings changes, we use our assumption that the separations at stationary firms provide an estimate of the earnings changes in the absence of a firm-level contraction.

As an intermediate step to recovering the latent earnings changes, we find that given the survey reason earnings changes differ depending on whether the worker separated from a stationary or contracting firm. Specifically, workers separating and reporting distress do *worse* at stationary firms than at contracting firms. This finding is consistent with the adverse selection logic of Gibbons and Katz (1991), and confirms the practice of restricting to large firm contractions. In contrast, workers separating and reporting quit do *better* at stationary firms than at contracting firms. This finding is consistent with the idea that workers at the contracting firms that quit to take a job are less choosy than those at stationary firms. Finally, there are not large differences in the earnings consequences of separation where the workers report other reasons for separation.

We then aggregate the various pieces of our method to construct the earnings changes of the separations caused by the firm contraction. Despite the substantial heterogeneity in earnings changes across survey reports, we find that our latent measure of earnings changes is remarkably similar to the measure of earnings losses using only administrative data.

Finally, we 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 on its surface 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. Workers who do not appear in the DWS might either have forgotten that they were displaced. Alternatively, 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 respondents 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, but 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.

1 Survey and administrative data

1.1 Data description

We use two data sets: 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. 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. 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 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 several 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.

- The LEHD job has 4 consecutive quarters of positive earnings that exceed a minimal threshold (earning the minimum wage at 70% of full-time equivalent hours);
- 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]

[1] We have access to all 50 states that participate in the LEHD program, and the data we have available runs through 2008. 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.

[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.

and

- 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 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.

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. This means that 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 25-74 in that calendar year. 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 when an employer ID disappears (and an employer appears to have shut down) in administrative data that it is due to errors in data linkages. 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 not related 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 Identifying displacements in administrative data

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).[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 6 year window.

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

2.2 Identifying displacements in survey data

Survey data provide 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.

Because of the large number of survey responses in the SIPP and the reasonably small sample sizes, we classify the survey responses into three groups: *distress*, *quit* and *other*. We map the following four reasons for separation to be due to firm *distress*: 1) On layoff;[5] 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

[4] While this cut-off may seem arbitrary, the findings in this paper suggest that it does a reasonable job of picking up mainly separations that are related to the employer contraction, in a sense that we develop more formally in Section 4.

[5] Fujita and Moscarini (2015) 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 4 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.

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.

2.3 Terminology

At this point, it is useful to clarify the relationship between the various terms used in this paper. A *mass layoff* (ML) is a separation that occurs when the firm contracts by 30% or more. The workers who separated are *administrative displacements.* We sometimes refer to the firm that contracts by 30% or more as being in *economic distress.* Using the survey data, when a worker reports that there was economic *distress* at the firm, then we refer to this as a *survey displacement*, or a survey reason of *distress.* Similarly, *quit* and *other* are also survey reasons for the separation.

Ultimately, this paper is interested in what we refer to as ML*. This corresponds to a separation that occurs in a mass layoff (ML) and is due to the economic distress at the firm. This separation is a *true displacement.*

2.4 Alignment of survey and administrative indicators

There are a large number of separations of each of three classes of survey reports. Table 1 shows about 20% of the separations are reported as due to *distress*, 30% as due to a *quit*, and 50% for *other* reasons.

Using our aggregated survey categories, the survey and administrative measures are correlated. 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, occur when there is a mass layoff. Even more strikingly, only 2% of the continuing jobs occur in quarters when there is a mass layoff at the employer.

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 employer 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, while 28% of the worker survey-reported *distress* separations are captured by the administrative indicator, this means that the administrative indicator misses over 70% of the survey-reported *distress* separations.[6]

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*.[7] 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."[8]

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.[9] The histogram shows that for most observations in our data employers are neither growing nor shrinking. The solid line displays the

[6]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 the presence 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.

[7]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.

[8]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.

[9]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%.

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 probability of all survey reasons rise. 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.

3 Earnings changes following a separation

The prior section showed 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. While this event study framework was pioneered by Jacobson, LaLonde, and Sullivan (1993) to study the effect of displacements, it is useful to study the earnings changes following any separation because it means that we do not mechanically attribute earnings gains (or losses) to separations if these were expected. 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 and were employed at a firm that was stationary (i.e. did not growth or shrink) *in that quarter*. Following Davis and

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

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

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 kth 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 $\delta_0^y = 0$. 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.[10]

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 three quarters of workers earnings prior to each event quarter and 16 quarters of earnings post event quarter.[11] Letting y represent a displacement or event quarter and recognizing that $t = k + y$ we have:

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

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.[12] We normalize $\delta_0 = 0$. Third, the age-earnings profile does not differ across displacement quarter so that $\beta^y = \beta$.[13] Appendix D discusses several issues with how to

[10] This 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 (2016) for further discussion of this point.

[11] 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.

[12] 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.

[13] Note that if t is sufficiently bigger than y then we do not include a calendar-quarter times displacement-quarter

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 required 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 4 years after the displacement. We allow for less than 4 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 log 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 which is all workers who continue (see Krolikowski (2016) for further discussion), our control group in Equation (2) is workers who continue at *no growth* firms (which we define as firm growth in the growth interval $[-5\%, +5\%]$).[14] The language of treatment and control implies that we presume that the data approximates an experiment where some workers randomly separate. An empirical implication of random separations is that the two groups look similar on observable covariates.

dummy since there are no earnings records associated with it.

[14] 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 and, in practice, the choice of the control group had little effect on estimates. We maintain the novelty to be consistent with the approach in section 4.

Table 2 shows that there are important differences in observable characteristics between the treatment and control groups for the canonical comparison of the administrative mass layoff indicators relative to continuers. The table shows the difference in population shares of several characteristics between the treatment and the control group. The mass layoff separators are younger, have less education, are more likely to be men and earn less.[15] 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 replicates the standard result in the literature.

Panel B shows that our reweighting procedure makes no difference to the estimated earnings trajectory. Given the large differences in observable characteristics documented in Table 2 and the fact that the reweighting procedure generates balance (see column (2)), this finding might seem surprising. Because we have already included worker fixed effects in estimation, however, level differences in earnings predicted by these characteristics are removed. So reweighting only changes estimates if these characteristics predict different slopes of earnings. Evidently, these characteristics do not predict large enough differences in slope to matter.

The finding that there are differences in observable characteristics between different groups of workers but that reweighting makes no difference to our estimates repeats for the remainder of the results in the paper. As such, we present the reweighted results but do not discuss differences in observable characteristics across groups.

[15] 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.

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. 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 measurement error 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 true displacement

In this section we develop and implement a method to measure the earnings losses of the separations that are related to 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 reason for separation. Section 3 showed that the consequences of an administratively indicated mass layoff differ depending on the worker survey report.

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 shows why we do not want to interpret the survey responses as reflecting the ultimate causes of the separation. Instead, we interpret the survey responses as sometimes reflecting the proximate cause of the separation. As discussed in Section 2, the figure 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.

The fact that the *quit* and *other* probabilities rise as the firm contracts suggests that many of these separations are related to the firm contraction. This inference is justified by the assumption that firm growth rates are independent of the underlying propensities of individuals to separate for quit or other reasons. The next section formalizes this logic and shows how to use this assumption to learn about the earnings changes related to the employer contraction. Sorkin (2015) uses similar reasoning to probabilistically distinguish between separations that are related and unrelated to an employer contraction, but does not incorporate survey data.

4.2 Methodology for identifying true 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 *true 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 now define some notation. Let s be a particular survey reason for separation, $s \in \{distress, quit, other\}$. 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 true displacement (ML_s^*) when a worker reports a particular survey reason. Let

Δearn_k be the earnings change in a particular displacement time (δ_k from Section 3). Define ω_s to be the share of survey reason s in a mass layoff, while ω_s^* is the share of survey reason s in the separations in the mass layoff that are related to the firm contraction.

The standard earnings loss regression is equivalent to:[16]

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

We are instead interested in:

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

Comparing equations (3) and (4) reveals two reasons why the earnings losses related to 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 related to 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 related to the mass layoff have very different earnings changes than the quits that would have happened anyway.

To estimate the $\mathbb{E}[\Delta\text{earn}_k|\text{ML}_s^*]$, we assume that the following pointwise relationship (i.e. for all k from Section 3) holds

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

where $\pi_s = \Pr(\text{ML}_s^*|\text{ML}_s)$ is the probability that a separation and survey response is related to the firm-level contraction. Below, we use the notation $\Pr(\text{not ML}_s^*) = 1 - \pi_s$ to refer to the ML separations that are not related to 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.

[16] 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 identical. See Appendix Figure A1.

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 the survey responses are differently related to the employer contraction. For example, we find that survey reports of distress are more likely to be related to the contraction than survey reports of quits.

Our identifying assumptions are:

Assumption 1: $\Pr(\text{not ML}_s^*) = \Pr(\text{no growth}_s)$;

Assumption 2: $\mathbb{E}[\Delta\text{earn}_k|\text{not ML}_s^*] = \mathbb{E}[\Delta\text{earn}_k|\text{no growth}_s]$.

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 stationary, or no growth, 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 stationary, or no growth, region.

Assumption 1 allows us to estimate the probability a separation was related to the contraction by:

$$\pi_s = \frac{\Pr(\text{ML}_s) - \Pr(\text{no growth}_s)}{\Pr(\text{ML}_s)}. \tag{7}$$

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

$$\underbrace{\mathbb{E}[\Delta\text{earn}_k|\text{ML}_s^*]}_{\text{latent earnings losses}} = \frac{1}{\pi_s}\mathbb{E}[\Delta\text{earn}_k|\text{ML}_s] - \frac{(1-\pi_s)}{\pi_s}\mathbb{E}[\Delta\text{earn}_k|\text{no growth}_s]. \tag{8}$$

This equation shows that two things have to be true for the earnings losses in ML* to differ from those in ML. First, there needs to be a difference between ML* and ML, formally, that $\pi_s < 1$. Second, the earnings losses in ML need to differ from the earnings losses in the stationary or no growth region, formally, $\mathbb{E}[\Delta\text{earn}_k|\text{no growth}_s] \neq \mathbb{E}[\Delta\text{earn}_k|\text{ML}_s]$.

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.

Aggregating up the results of equation (7) and (8) allows us to substitute in to equation (4) and estimate the object of interest.

4.3 Probabilities and shares

The probability of all survey reported reasons of separation are much higher when firms undergo large contractions than when they are stationary. Rows (1) and (2) of Table 3 contain the numerical version of the differences evidence in Figure 1.[17] Converting to the probability that the separations are related to the employer contraction using equation (7), row (3) of Table 3 shows that the *distress* separations are much more related to firm growth than the *quit* and *other* separations. Specifically, 96% of the *distress* separations are related to the firm contraction, while only 77% of the *other* separations and 67% of the *quit* separations are related to the firm contraction. This finding is consistent with the intuition that even at stationary 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. Overall, 86% of separations in a mass layoff are related to the employer contraction.

These different probabilities of being related to the contraction by survey reason alters the weights placed on different categories of survey separation when computing earnings losses related to the contraction. The bottom two rows of Table 3 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.

4.4 Earnings losses related to the contraction

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

[17] 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. So firms can contract by reducing the number of lower-tenure workers.

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 stationary, but the worker's survey reason is *distress*. Significantly, workers reporting *distress* have better post-displacement earnings outcomes during a mass layoff than when there is no growth at the firm (i.e. it is not growing or shrinking). This finding is consistent with 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 97% of the survey-reported *distress* separations in a mass layoff are related to 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 stationary, but the worker's survey reason is *quit*. The black line shows that the estimated earnings changes of the quits related to 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 stationary 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 related to 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 stationary, but the worker's survey reason is *other*. The black line combines these two lines. The earnings changes of the *other* survey

reason related to the mass layoff (ML^*_{other}) quite similar to all the *other* separation 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 3 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 related to the firm contraction.

Why are the earnings losses in ML so similar to ML*? Two surprising features of the data drive this result. First, conditional on the survey reason, what is going on at the firm—whether it is contracting by a lot, or is stationary—does not have a large effect on earnings losses. Specifically, the variation in earnings changes across survey reasons holding the firm growth rate constant in Figure 3 is much bigger than the variation in earning changes within survey reasons but changing the firm growth rate. (As is evident in Panel B of Figure 1, the firm growth rate does have a large effect on the composition of these reasons.) This means that the first condition necessary for the latent and observed measures to differ emphasized in equation (8) is not met. Second, the sharp rise in the probability of all the survey reasons 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 second condition necessary for ML and ML* to differ is also not found in the data.

Hence, the standard practice of using observed ML is not misleading. Nonetheless, knowledge of the worker reason contains important information about the consequences of the separation for workers' future labor market outcomes.

5 Accounting for zeros: unemployed or out of the labor force?

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 large difference for understanding labor market outcomes after a true displacement.

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 the being out of the labor force zeros because this is a fundamentally different state. So far in this paper we have followed the standard practice of omitting all earnings histories with enough zeros following the separation.

Our link with survey data provides information on whether the zero earning histories that we 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. Specifically, we look at the quarters in the year following displacement in which these workers have zero earnings and associate these zeros with the survey-reported reasons for zero earnings. (We also report results on the secondary issue of whether the administrative zeros are truly zero because some earnings are not covered by the administrative data.)

Table 4 shows that over 40% of separators have zero earnings in a calendar year following the separations and are thus omitted from the regression analysis of earnings loss in Section 3.1. The results by survey response align with expectations. The *other* category contains many reasons for separation that are correlated with leaving the labor force, and indeed around 70 percent of these observations are excluded from our baseline earnings analysis. In contrast, relatively few (10 percent) of the *quit* separations are removed due to a calendar year of zero earnings. Finally, one in five separators identifying *distress* record zero earnings for a calendar year and are excluded.

The second part of Table 4 demonstrates that workers citing firm *distress* and have zero earnings are more likely to remain in the labor force than workers who lose their jobs for other reasons. Forty percent of worker-quarters associated with separations citing firm *distress* report looking for work, while this share is only 6 percent for the other categories.

5.1 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 consistent employed (*no zeros*):

- add back in all the 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 those 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 6 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 zero* shows the standard treatment of zeros. The *all zeros* line includes "all" zeros, that is, 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.

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 6 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.

Panel B - D Figure 6 shows 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 zero* line is close to the *all zero* 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 zero* line is very close to the *no zero* line.

5.2 Employment among false zeros

Table 4 shows that despite these being quarters with *zero* administrative data earnings, many workers report being employed. Among the quits 88% of workers report being employed, while this number is only 30% among those who separated due to distress. An obvious explanation is that, 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 and our version of the LEHD does not contain Federal government employment.

Table 5 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*.[18]

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 survey *quits* 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 indicator is larger than the heterogeneity in the other direction. That is, conditional on the survey reason, what is going on at the firm does not have a large impact on the earnings changes of workers.

[18] 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.

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 stationary firms. Even though the composition of separations shifts, it is still the case that the probability of separating and reporting each survey reason rises 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, the earnings changes associated with each survey report do not depend that much on the state of the employer. Second, because the probability of all types of separations rises dramatically, most of the separations in the administratively indicated mass layoff are related to the mass layoff.

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 outcome, 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 application to displaced workers shows that combining the survey and administrative data to consider jointly the the firm and worker perspective can provide a much more complete picture of the reasons for a displacement and its effect.

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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	Share with ML Indicator
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	6500	0.10
Total Continuers (Unique Persons)	205600 (28000)	0.02

Panel B: Admin Indicators Captured by Survey Indicators

	Survey reason for separation	
	Distress	Not Distress
Mass Layoff Indicator		
Yes	55%	45%
No	18%	82%

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.

Table 2. 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 3. Latent Firm Contribution to Survey Reports

		Survey reason (s)		
		Distress	*Quit*	*Other*
(1)	$\Pr(\text{Separation}_s \mid \text{ML})$	0.055	0.021	0.026
(2)	$\Pr(\text{Separation}_s \mid \text{No growth})$	0.002	0.007	0.006
(3)	$\Pr(\text{ML}_s^* \mid \text{ML}_s) = \pi_s$	0.964	0.666	0.768
(4)	$\omega_s = \text{Share}_s \mid \text{ML}$	0.542	0.204	0.254
(5)	$\omega_s^* = \text{Share}_s \mid \text{ML}^*$	0.612	0.159	0.229

Source: SIPP-LEHD as explained in text.
This table reports the details underlying the construction of latent earnings loss estimates. 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% (no growth) (2). The third row converts these conditional probabilities to estimates that each separation was related to 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.
This table reports these probabilities based on the re-weighted samples detailed in Table 2; see Appendix Table A7 for the unweighted version.

Table 4. Accounting for Separators with Zero Earnings

	Survey Reason for Separation			
	Distress	*Quit*	*Other*	Total
Share with Zero Earnings	0.20	0.10	0.70	0.42
Survey response in period with zero earnings[1]				
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	

Source: SIPP-LEHD as explained in text.
This table reports the share of separators that include any calendar-year of zero earnings in a 4-year interval following a separation. The lower panel shows the survey-reports of worker activities in the four quarters that include zero administrative earnings.
[1]Column shares do not sum to one because respondents can identify multiple activities within the three months in a quarter.
(d) indicates output suppressed because of disclosure limitations.

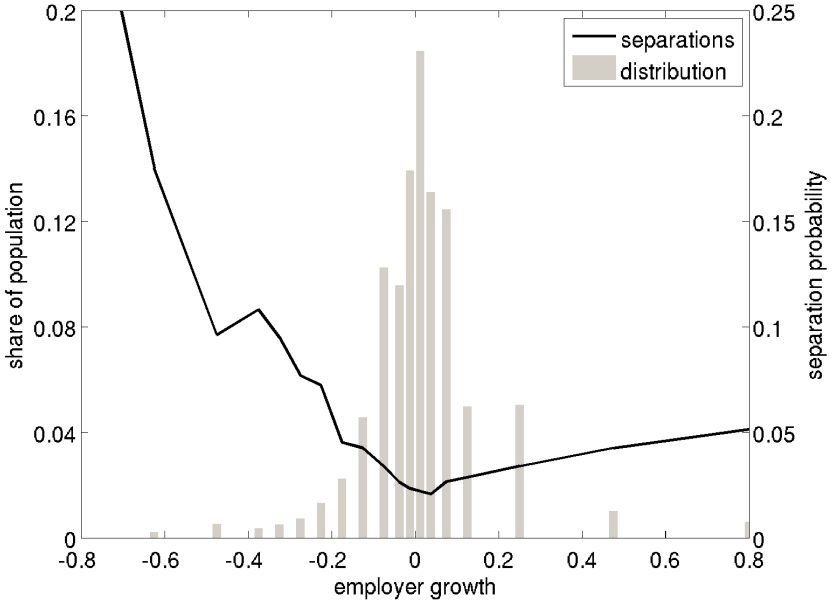
Table 5. SIPP Employment in Quarters with Zero Administrative Data Earnings (Rates)

	Survey Reason for Separation		
	Distress	*Quit*	*Other*
Work for Government or Family	0.15	0.24	0.22
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

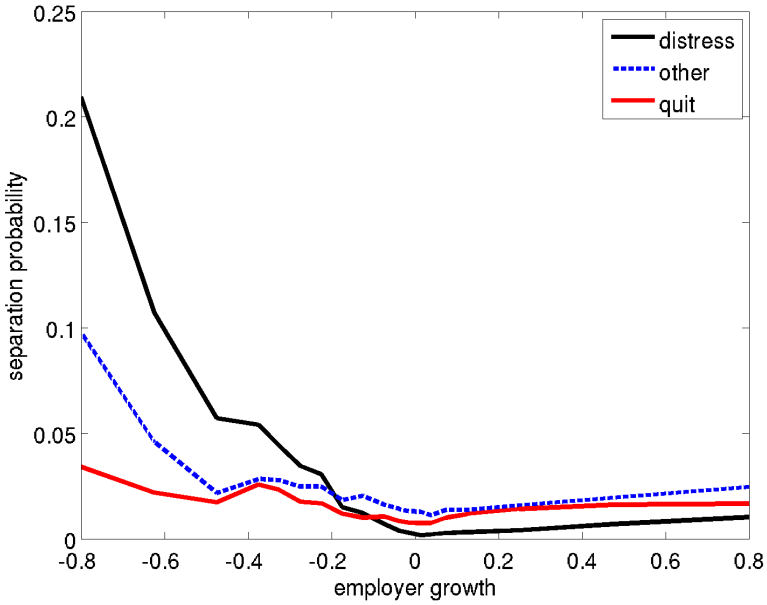
Source: SIPP-LEHD as explained in text.
This table reports worker response in quarters in the first year following a separation in which the worker had zero administrative data earnings but reported being employed in the SIPP. (See the lower panel of Table 4.) The first two rows record the percentage 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



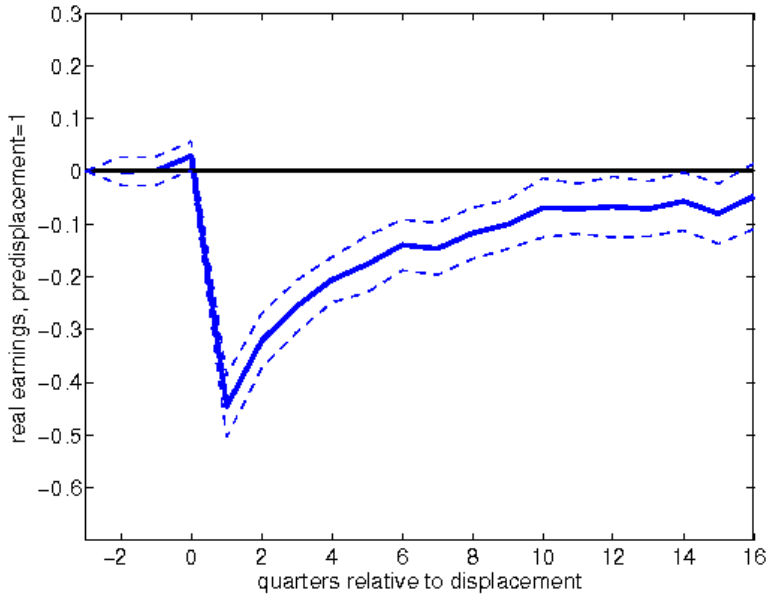
B. Probability of Separating and a Survey Reason



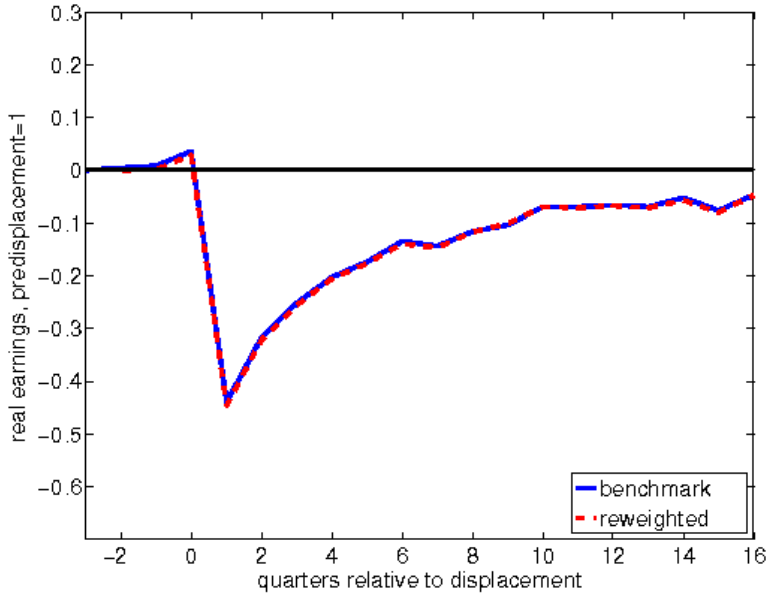
Source: SIPP-LEHD as explained in text.
This figure shows how the probability of separating depends on the employer growth rate. In Panel A, the solid line shows the probability of separating, while the histogram shows the distribution of employment as a function of employer growth rate. Panel B decomposes the solid line from Panel A by the three survey reasons for separations.

Figure 2. Benchmark Earnings Losses

A. Benchmark Administrative Measure

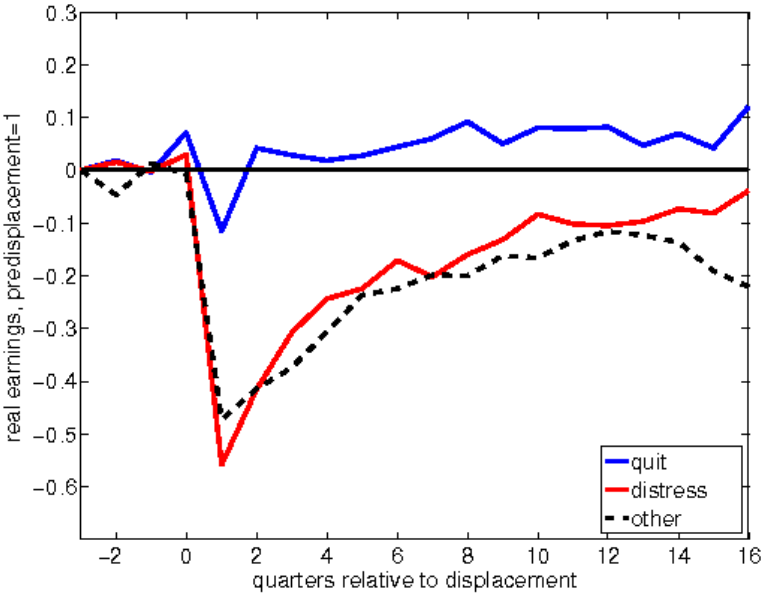


B. Reweighted vs. Unweighted Earnings Changes



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 predisplacement earnings, where the predisplacement earnings are normalized to 1.

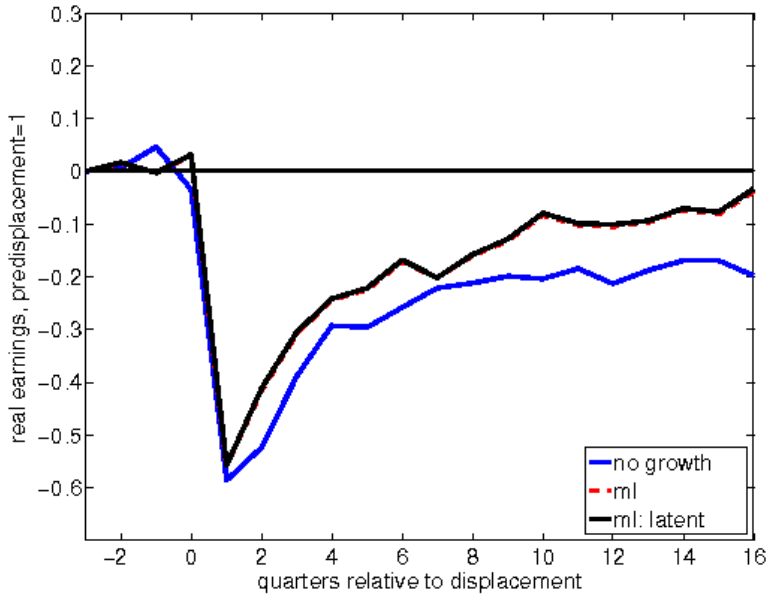
Figure 3. Mass Layoff by Survey Category



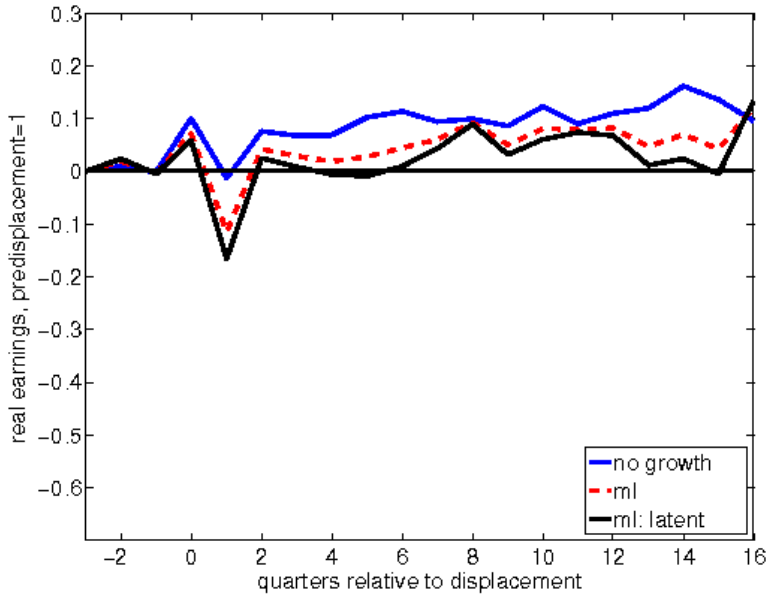
Source: SIPP-LEHD as explained in text.
This figure plots earnings changes from comparing administratively-defined mass layoff separators—split by survey reason for separation—to continuers at stationary firms. It reports the results of three separate regressions. Confidence intervals are suppressed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

Figure 4. Earnings Losses in Separations Related to the Firm Contraction

A. Survey Report of Distress



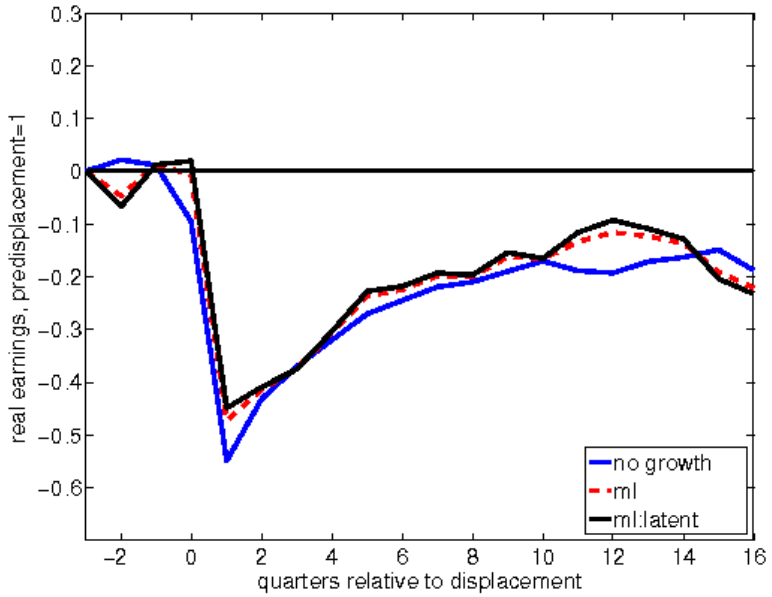
B. Survey Report of Quit



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 3. The units on the y-axis are fraction of predisplacement earnings, where the predisplacement earnings are normalized to 1.

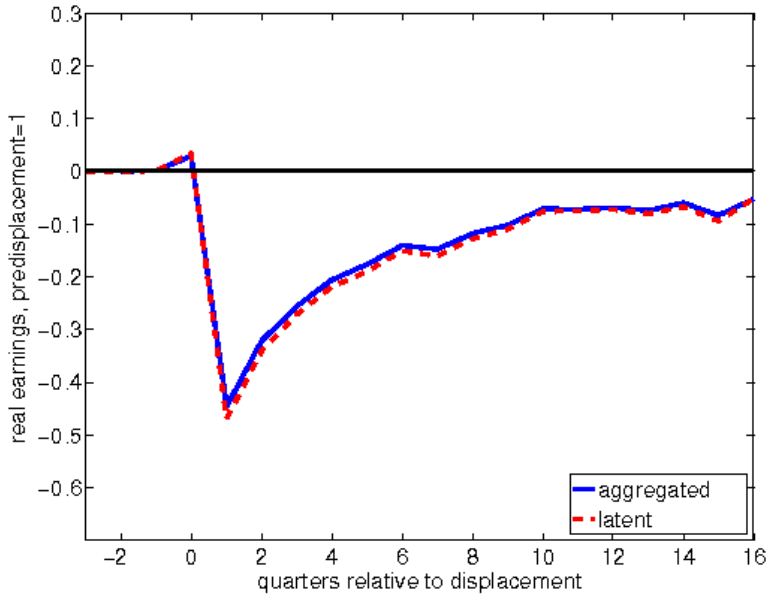
Figure 4. Earnings Losses in Separations Related to the Firm Contraction

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 3. The units on the y-axis are fraction of predisplacement earnings, where the predisplacement earnings are normalized to 1.

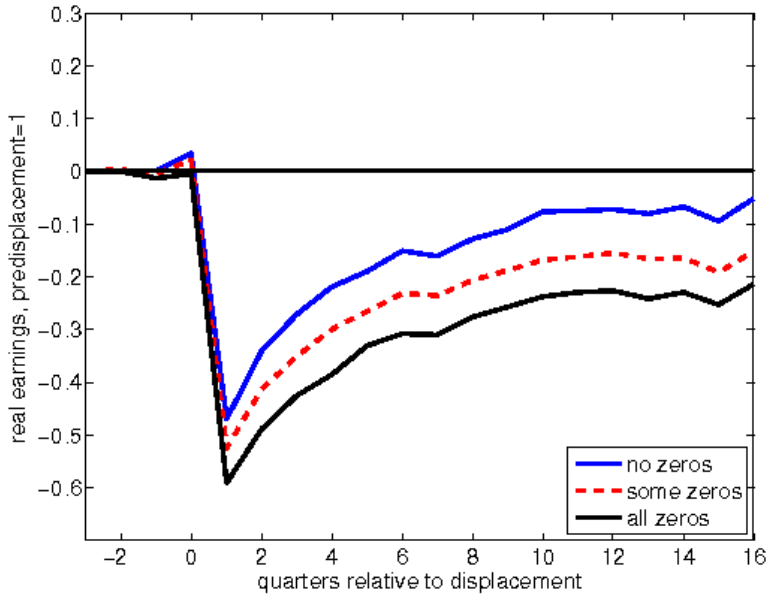
Figure 5. Earnings Losses in Separations Related to the Firm Contraction: Aggregated



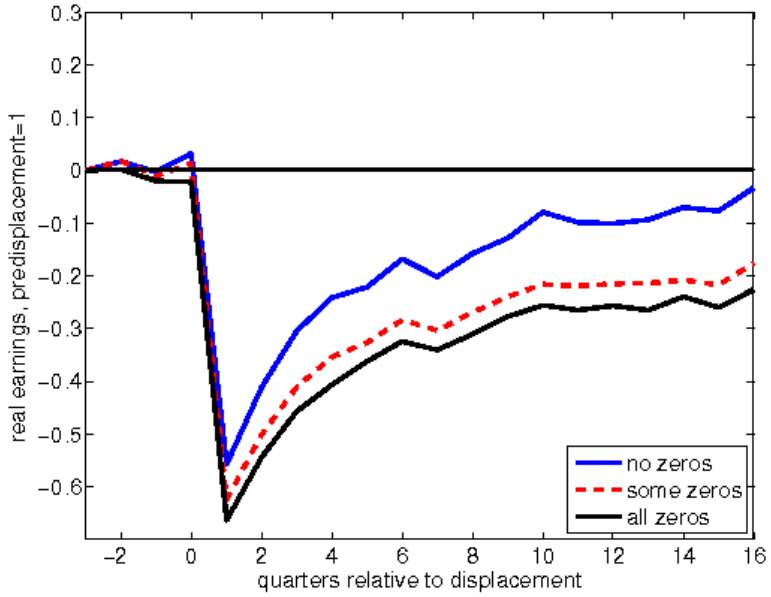
Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of separations in a mass layoff that are related to the contraction (latent) as well as the benchmark approach (aggregated). The latent line is constructed using equation (4) from the latent lines in Figure 4 and the shares in Table 3. 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. Earnings Losses: the Role of Zeros

A. Total Latent Measure

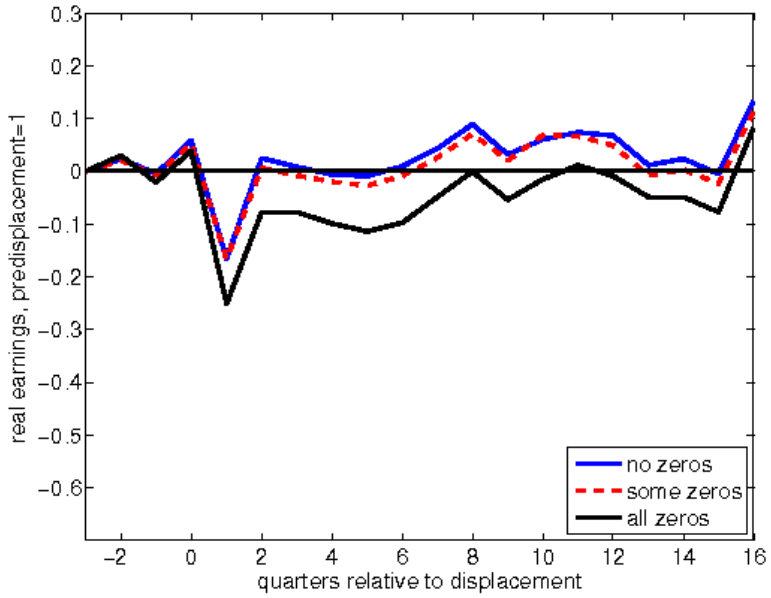


B. Survey Report of Distress

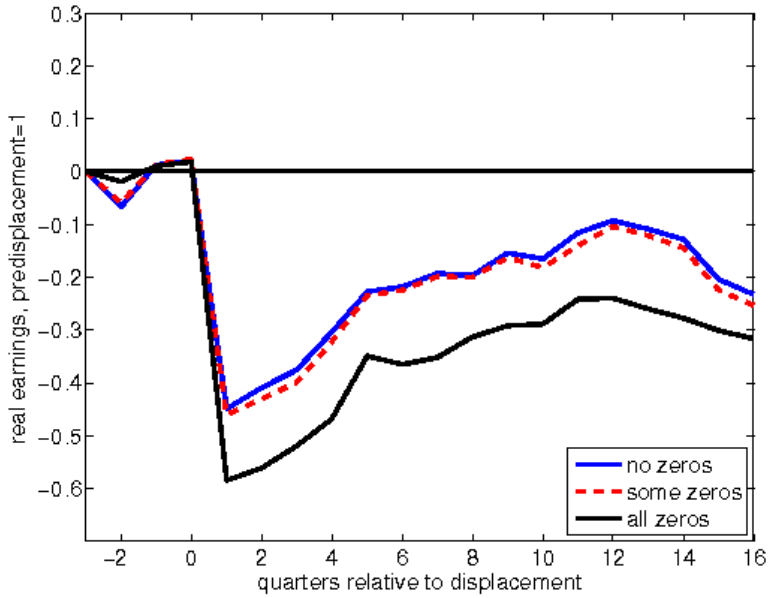


Source: SIPP-LEHD as explained in text.
This figure plots the latent notion of earnings losses in a mass layoff calculated using the method in section 4 and given by equation (8) based on three different treatments of observations with zero earnings. The *no zeros* line drop all earnings histories with 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 predisplacement earnings, where the predisplacement 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 fraction of predisplacement earnings, where the predisplacement 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.

In the LEHD, we create a universe of jobs among workers also in the SIPP based on the following three criteria:

- 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 $t-1$;
- 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);
- We match the notion of separation by restricting attention to jobs where the last quarter of positive earnings is quarter t and the worker has earnings below the threshold described in the previous bullet from the same employer in quarters $t+1$, $t+2$, $t+3$ and $t+4$.

This generates two lists of jobs. We then combine them in the following way:

- If a worker had a SIPP job that ended in quarter t that met our criteria, we examined all LEHD jobs for that worker that ended in quarter $t-1$, t, or $t+1$.
- If the previous step generated multiple LEHD jobs per SIPP job, then we selected a unique job in the following order of priority:
  - If a given SIPP job generated multiple matches, we prioritized the match that was exact in terms of timing;
  - If there were two jobs that met our criteria, we picked the one with the highest earnings in the quarter before the separation;
  - It is possible to have two jobs that both match inexactly and have the same earnings. In this case we took one at random.
  - If a given LEHD job matched to both a separating job and a continuing job then we kept the separating job (this can happen if in the first month of the quarter a worker is employed, and then separates in the third month—in the second month this job would be reported as continuing while in the third it would be reported as separating);
  - For remaining duplicates, we picked a job at random.

Table A5 provides more details on the matching process and match rates. We start with 22,700 separations in the SIPP and are able to match 10,100 of them to the LEHD.

A.2 Non-separators

For the sample of non-separators, we impose a tenure requirement in an identical manner. Of course, we do not impose a separation requirement. The other difference is that to generate the list of candidate jobs in the LEHD we require that the job match in the exact quarter, rather than in a two quarter window.

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.[19]
- 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.[20]

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.

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

[20] 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.

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 US 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 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.[21] He looks at media reports of mass layoffs at public companies from 1987-1995.[22] 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 1990-1995 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.

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

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

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.[23]

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 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.[24]

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 2. 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 trade-off a researcher is better off dropping some observations.[25] In practice, the events that we study are relatively rare and so we do not have estimated propensity scores close to 1.

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

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

[25] 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 (2015) [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 (2015) [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 (2016)
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 (2015) [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
SIPP person-quarters	525,900	22,700
Positive LEHD earnings	499,800	22,000
4 quarters of LEHD earnings	488,000	21,500
4 quarters of LEHD earnings and pass earnings test	473,600	20,100
Matched	348,100	10,100
Number of quarters	27	27

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

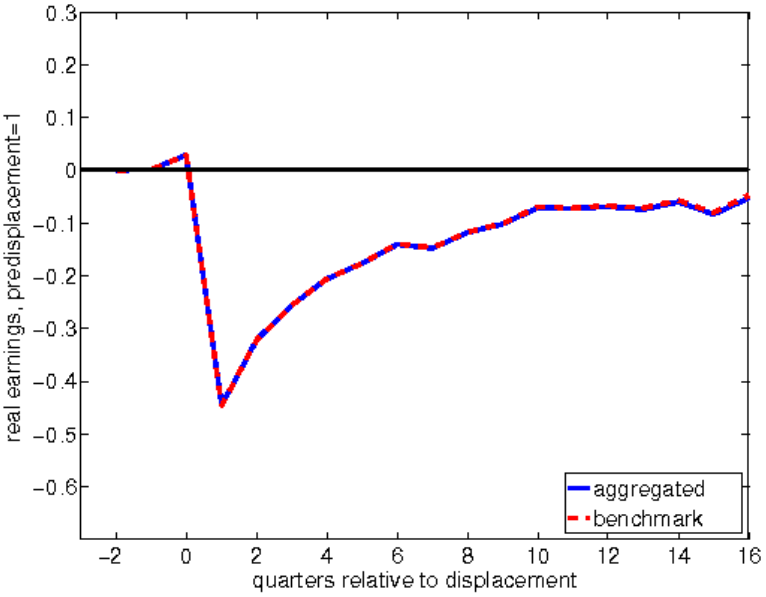
Note: this table is based on Table 3 in Benedetto et al. (2007).

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

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

Source: SIPP-LEHD as explained in text.
This table reports the unweighted version of Table 3.

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.