Reconsidering the Consequences of Worker Displacements: Survey versus Administrative Measurements*

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Abstract

This paper uses an innovative matched SIPP-LEHD dataset to compare measures of worker displacement and to shed light on the mechanisms for displaced worker earnings losses. Administrative and survey indicators of firm distress are both imperfect. Administrative mass layoffs can contain quits. Survey reports of separations owing to distress may be unreliable. The two definitions of displacement are far from perfectly aligned. Over 40% of the administrative displacement events are not classified by workers as a displacement, while about 70% of survey-based displacements are not captured by the standard administrative data events.

Combining administrative and survey measures provides a more exact picture of reasons for job loss. The paper shows that the persistence of earnings losses following mass layoffs is attenuated when economic distress is jointly indicated by the administrative and survey data, and the puzzling permanence of earnings loss is indeed eliminated at large firms.

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Job loss resulting from economic distress of firms has large and persistent effects on workers' well-being. Workers suffering such displacement are less likely to work in the future and have large and persistent earnings losses if they do find work. To the extent that displacements result from large-scale shocks to firms, they provide information about the consequence of job loss independent from worker characteristics that might cause them to quit voluntarily, be fired for cause, and so on.

This paper compares and contrasts two leading approaches to measuring displacement: firm-side measures from administrative data and worker-side measures from survey data. The leading firm-side indicator defines a displacement as a job loss associated with a substantial decline in employment at the firm. This measure, pioneered by Jacobson, LaLonde, and Sullivan (1993) and used in a number of subsequent, influential studies, uses a threshold of net job loss—typically 30%—to define a displacement. The worker-side indicator is found in surveys that ask workers the reasons for employment transitions. Transitions associated with economic distress at the employer include layoffs, firm closure, bankruptcy, and slack business conditions. Surveys that include such characterizations of job loss include the Current Population Survey (CPS), the Displaced Worker Survey (supplement to CPS), Panel Study of Income Dynamics (PSID), Health and Retirement Study (HRS) and the Survey of Income and Program Participation (SIPP).

The firm-side and worker-side measures may convey different information about firm distress, and both are potentially problematic. Absent a complete shutdown, there is the potential that workers who lose jobs are endogenously selected according to their productivity, match quality, seniority, or other characteristics that might affect their future labor market experience. Additionally, even when a firm is mandating separations because of economic distress, workers may be quitting for reasons of their own totally unrelated to the firm shock.

This paper takes advantage of a firm-survey link to evaluate the firm-side and worker-side measures of displacement in tandem. First, we compare the firm and survey measures to shed light on what events the two measures capture. In particular, we have separate measures from the firm and worker of the incidence of displacement, and can assess the extent to which the firm- and worker-side measures agree or disagree. Second, we estimate the

effects of the different measures of displacement on worker outcomes including earnings, labor market outcomes such as retirement and zero earnings, and receipt of transfers including unemployment benefits and Social Security. Third, by comparing earnings losses when the survey and administrative measures agree and disagree, we can shed light on explanations for the persistence of earnings losses following displacement

Comparing the earnings losses when survey and administrative measures agree and disagree suggests the importance of both selection and quits. When the worker and firm-side measures agree that there was a displacement, there are smaller earnings losses than when the worker indicates a displacement and the firm-side (administrative data) measure does not. This result is consistent with the presence of adverse selection since there is more scope for selection in non-mass layoffs (e.g. Gibbons and Katz (1991)). A comparison of all such separations—administrative mass versus non-mass layoffs—does not reveal evidence of adverse selection because worker-reported quits have much better outcomes and due to the greater fraction of worker-reported quits in administratively-indicated non-mass layoffs than in mass layoffs.

We then compare earnings losses at small and large firms when the survey and administrative measures agree. Remarkably, the earnings of workers who report distress in mass layoffs at large firms completely recover, whereas the earnings of workers who report distress in mass layoffs at smaller firms do not. In both cases, in the sample of workers who report separating due to distress, the mass layoff separators see their earnings recover more than the non-mass layoff separators. We interpret this evidence to suggest that adverse selection might fully account for the persistence of displaced worker earnings losses. Specifically, the market is presumably more aware of mass layoffs at large firms and so can interpret the separation as unrelated to the worker's performance. In contrast, following mass layoffs at smaller firms there might still be some negative inference made about a worker's quality because the market is unaware of the mass layoff.

The paper proceeds as follows. The first section discusses the measures of displacement and the various dimensions of their alignment. It also documents our matched worker-firm dataset. In particular, we have linked jobs in the Survey of Income and Program Participation (SIPP) to the Longitudinal Employer-Household Dynamics dataset (LEHD).

The second section evaluates how the firm and survey measures predict economic outcomes in both the administrative and survey data. This section also uses the data infrastructure to isolate circumstances of mass layoffs to evaluation better where adverse selection may be operating. The third section elaborates on the outcomes from section two to include UI collection, social security payments, disability, and then evaluates the interpretation of zero earnings observations in the data. The final section offers conclusions.

1 Measuring Displacement Using Survey and Administrative Data

1.1 Identifying firm distress in survey data

Survey data provide information from workers about their perceptions of the circumstances surrounding the separation. Using the SIPP, we consider the following four reasons for separation to be due to firm *distress*: 1) On layoff;[1] 2) Employer went bankrupt; 3) Employer sold business; and 4) Slack work or business conditions. The following survey reports we classify as *quits*: 1) Quit to take another job; 2) Unsatisfactory work arrangement; and 3) Quit for some other reason. Finally, we classify the remaining reasons for separation into an *other* category: 1) Retirement or old age; 2) Other family/personal/child obligations; 3) Own illness/injury; 4) School or training; 5) Job was temporary and ended; and 6) Discharged/fired.

Other surveys that have been used to study distress capture a slightly different combination of reasons. The most common surveys used are the Displaced Worker Survey (DWS) and the Panel Study of Income Dynamics (PSID), although other research has used the Health and Retirement Study (HRS), the National Longitudinal Study of Youth (NLSY), and the SIPP. There are a few points worth making. First, within the Displaced Worker Survey there is heterogeneity across studies in the use of different kinds of separations. Notably, after Gibbons and Katz (1991) documented potential differences between earnings losses in plant closings and mass displacements, which they interpret as evidence of adverse selection, some authors have focused exclusively on plant closings. Second, the PSID does not distin-

[1] Fujita and Moscarini (2013) note that there is still a lot of recall among workers reporting "on layoff" in the SIPP. We attempt to capture only non-recalled layoffs by requiring that the worker have no positive earnings from that employer in the 4 quarters following the report of "on layoff."

guish between being laid off and fired. Table A4 summarizes definitions of displacement that have been used in worker-side surveys.

1.2 Identifying firm distress in administrative data: Net worker flows

The standard approach to measuring a mass layoff in administrative data is to classify separations based on information from net worker flows. In particular, a large net contraction in employment is taken as evidence of firm distress. Table A5 highlights the commonality of the Jacobson, LaLonde, and Sullivan (1993) definition of firm distress as a 30% net employment contraction across papers using administrative data.

Let t denote calendar time and N_t be employment at the firm at time t. We use this variable to construct firm-level statistics that bear on the displacement of a worker. Suppose that time t is the last quarter in which the worker is employed (has positive earnings) at an employer. We consider net flows in the year around the separation to abstract from seasonality.

The standard net flow measure is the percent change in emplyment from $t-3$ to $t+1$,

$$NF_t = \frac{N_{t+1} - N_{t-3}}{N_{t-3}}.$$

Typically the net flows measure is used in a binary form: a firm having $NF_t < -0.30$ is in *economic distress* and the worker separations associated with this net contraction are *displaced workers* (and the literature refers to the entire event as a *mass layoff*).

In Appendix B.7 we discuss other ways of measuring separations due to economic distress that have appeared in the literature.

1.3 Data Description

This paper compares survey and administrative measures of displacement. Here we describe the survey and administrative datasets that we use, as well as our methodology for matching them together. We utilize two data sets on individual workers: the Survey of Income and Program Participation (SIPP) and the Longitudinal Employer Household Dynamics (LEHD).

We use the Longitudinal Employer Household Dynamics (LEHD) dataset from the United States Census Bureau, which contains matched worker-enterprise information across time.[2] The LEHD tracks the employment history and earnings profile for a particular worker, as well as demographic characteristics such as age and race. Because the data are created out of wage records from state Unemployment Insurance (UI) and ES-202 data (establishment employment and payroll data), the coverage of the LEHD span nearly the entirety of the private, non-farm payroll of the states included in the program. Over 90 percent of payroll employment is covered by the UI system. 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. Because the LEHD contains the universe of workers at an employer, we are able to construct a measure of employer size, as well as worker flows at the employer level.

The Survey of Income and Program Participation (SIPP) is a U.S. Census Bureau sponsored survey with the purpose of collecting a variety of individual and household information in order to evaluate the effectiveness of federal, state, and local programs. The survey collects information on source and amount of income, labor force participation, demographic characteristics, program eligibility and participation, and taxes, assets and liabilities. The SIPP is a nationally representative series of panels with a sample size of between 14,000 and 36,000 households. This paper uses 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, the SIPP provides information on labor-force participation. Those identified as not working are asked to identify the reason. In addition,

[2]We currently have access to 30 of the 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 a complete discussion of the background and contents of the LEHD files.

the core information of the SIPP on SSI, SSDI, and UI receipts are useful outcome variables to be used following a potential job displacement.

1.4 Matching procedure

We match the employment spells in the SIPP to employment spells in the LEHD to compare reasons for separations in the SIPP with administrative measures in the LEHD. We use a bridge from the person-level identifiers in the SIPP to the person-level identifiers in the LEHD to link *respondents* across the data sources. There is no analogous bridge at the job level. Therefore, within a worker record we link the jobs the respondents report in the SIPP and the jobs that exist for the respondents in the LEHD.

To align with the interest in the displaced worker literature in high tenure workers we restrict attention to SIPP jobs with 12 months of tenure. We then match a SIPP job that ends and an LEHD job on three features.

- The LEHD job has 4 consecutive quarters of positive earnings;
- The worker has no LEHD earnings from the employer in the four quarters following the survey-reported separation; 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 the survey and administrative jobs meet similar tenure thresholds. 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, or that they expected to be recalled and only reported a layoff when they were not recalled. 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, to be consistent with the displaced worker literature (see Table A5), we require that the employer have at least 50 workers three quarters prior to the candidate quarter.

One might worry that when an employer closes in administrative data that it is due to errors in data linkages. In Appendix A.4 we detail how we used employee flows to clean spurious shutdowns and other employer ID changes.

1.5 Relationship Between Worker Reports and Firm Performance

Table 1 shows the various responses to the SIPP reason-for-job-transition question and the number and rates of SIPP respondents in each category for the sample of SIPP person-quarters that we match to the LEHD as described in the previous section. Roughly 20% of transitions are distress-related. In this section we study how well the administrative measure of firm distress aligns with worker reports of distress.

Looking at Table 1, it would appear that the overall separation rate is very 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 table shows person-quarters, so the total for continuers counts multiple quarters for those who do not separate. Hence, Table 1 should not be used to compute a separation rate.

1.6 Graphical evidence on the continuous measures of firm performance

The top panel of Figure 1 shows that the worker distinction between reasons for separations clearly maps into differences in firm performance using the net measure of worker flows. The figure shows the distribution of job flows depending on whether the worker reports a separation because of economic distress or a quit. (The other category is omitted. It looks similar to quits.) While the literature focuses on the binary measures of whether a

firm has a mass layoff, our data allow us to look at the net flow indicator of displacement continuously. The distribution of firm performance is significantly left-shifted for the workers who report layoffs compared to those who report quits, and there are significant mass points corresponding to zero employment growth.[3] The significant differences in the distribution of the firm-side flows as a function of worker response lends credence to the survey reports.

To make the comparison between quits and layoffs even more transparent, the bottom panel of Figure 1 plots the ratio of the pdfs of the job loss measures under worker reports of distress versus quit. The figure reveals that there is a large jump in the ratio around a 40% contraction. Hence, the distributions support the practice of using a discrete thresold of labor flows to signify firm-side economic distress and lend some support to the convention of a 30% cutoff for the net flow measure.[4]

1.7 Tabular evidence on the binary forms of the measures of firm performance

Table 2 demonstrates that even at the granular level of the detailed survey questions there is a distinction in employer-side performance between different worker survey reports. When a worker responds with questions we have classified as distress there is much more likely to be poor employer performance, as proxied by the share of separations that the mass layoff indicator captures.

For survey reports of distress, around 30% of survey reports correspond to the binary indicator of firm-side distress that we have constructed in administrative data, compared to only 5% for other separations. Hence, while the administrative indicator of firm distress supports the survey reports, it also miss about 70% of separations that workers report as due to firm distress. For bankruptcy and employer sold business, there is much less disagreement between the survey and adminstative data.

Panel B of Table 2 shows that the survey measure categorizes about twice as many separations as due to distress relative to the administrative measure. (We defer discussion of UI collection as an indicator until section 3.)

Panel A of Table 3 looks at the survey report from the perspective of the firm-side

[3] A Kolmogorov-Smirnov test for the equality of the quit and layoff distributions rejects at a p-value of 0.

[4] The spike on the right hand side of the graph reflects very few observations.

indicators of distress. The table shows that if the firm-side indicator shows distress, 56% of SIPP respondents report a job loss due to distress. Thus, the majority of the separations that the administrative measures label as due to a mass layoff correspond to worker reports of distress, 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."[5] On the other hand, about 44% of separations that the administrative measure labels as distress are labeled by workers as not distress.

1.8 Size matters

The conventional firm-side measure of displacement is that employment declines 30 percent or more at the firm. One might suspect that such a percentage decline in employment means different things for employers of different sizes, or for multi- versus single-establishment firms. Put starkly, suppose that a firm with 10 equally-sized establishments closes one establishment, and leaves employment unchanged at the other nine. The conventional measure would not flag the job losses associated with this plant closure as displacements. The workers who lost their jobs might disagree. This example suggests that the conventional indicator might understate displacement at large or multi-establishment firms. Our matched survey-administrative data allows us to evaluate this conjecture.

Table 4 shows a detailed breakdown of alignment between survey measures by employer size and whether or not the employer is a single-unit or multi-unit. The alignment between worker reports and administrative measures gets worse at larger employers. In particular, the share of separations that workers report as due to distress that the standard mass layoff indicator also picks up falls from 42 percent at the smallest employers to 11 percent at the largest employers. The match between separations that the administrative measures identify as due to firm distress and what workers report as due to firm distress also declines slightly in employer size, though the evidence is less compelling.

A similar pattern of declining alignment between administrative and survey reports at larger employers emerges in comparing single-unit and multi-unit employers. The alignment,

[5]The Davis and von Wachter (2011) statistic is based on the JOLTS, which is an employer-side survey.

in either direction, between survey reports and administrative measures is stronger at single-units than at multi-units. Because larger employers are more likely to be multi-units, the similarity with the employer size results is unsurprising.[6]

The insight that size matters for the conventional firm-side displacement measure is clearly an important one. It is less clear what to do about it. One idea we pursued is to have the cutoff be size varying, e.g., for the largest firms define a displacement as being associated with a decline of only 10 percent. The problem with this solution is, with a lower threshold of net employment decline, many more separations not related to economic distress would be captured by this indicator. Another potential solution is to use information on *gross* worker flows to characterize firm ditress. This approach is detailed in section C. With respect to size, the gross measure performs slightly better than the net measure: the agreement between this measure and the worker reports falls from 44% at the smallest employers to 17% at the largest employers.

1.9 Alignment by Worker and Industry Characteristics

Appendix Table A11 shows a detailed breakdown of the alignment between the survey measures and administrative measures by age, education and gender. The most striking patterns in alignment are by different levels of education. In particular, the separations that less educated workers report as due to distress are more likely to be picked up by administrative measures *and* the administrative distress separations are more likely to be picked up in survey reports than for workers with more education.

These patterns are distinct from the patterns of *recall bias* documented by Evans and Leighton (1995, table 5). That paper shows that workers with less education suffer from more recall bias and finds no strong patterns by age. That the more educated are more likely to be misclassified in survey data relative to administrative data is consistent with patterns by education reported by von Wachter, Handwerker, and Hildreth (2012, pg. 22).

Appendix table A12 summarizes industry differences in alignment.

[6]The fact that alignment declines at larger employers is worth highlighting because outside of administrative data measures of mass layoffs, there are a number of measures in Appendix B.7 that focus on the number of separations, rather than the change in employer size.

2 Worker Earnings Following Displacements

This section evaluates the consequences of a displacement due to firm distress, using the various measures discussed above. This section focuses on earnings losses, whereas section 3 looks at other outcomes such as the receipt of unemployment insurance and other government transfers, retirement, and the incidence and interpretation of zero earnings.

2.1 Earnings Losses: Specification

We estimate the treatment effect of losing a job through a displacement, defined in several different ways, on labor market outcomes in an event study framework.

Consider a treated group of workers who lose their job in a displacement in a particular event quarter y (say 2000:I), and a control group of workers who do not lose their jobs *in that quarter*. Following Davis and von Wachter (2011, equation 1), 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 k^{th} year 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 nondisplaced. The δ_k^y are the coefficients of interest: the effect of being displaced relative to not in the particular quarter.[7]

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-specfic 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

[7] This contrasts to the notion of displacement that Jacobson, LaLonde, and Sullivan (1993, pg. 691) are interested in: "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."

event quarter.[8] Letting y represent a displacement or event quarter and recognizing that $t = k + y$ we have:

$$e_{ik}^{y} = \sum_{y} \alpha_i^y + \beta X_{ik}^{y} + \sum_{k=-3}^{16} \delta_k E_{iy}^{k} + \sum_{k=-3}^{16} \gamma_k D_{ik}^{y} + u_{ik}^{y}. \tag{2}$$

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 $\sum_{k=-3}^{16} \gamma_k E_{it}^k$ where E_{it}^k is an indicator for the displacement quarter.[9] Third, the age-earnings profile does not differ across displacement quarter so that $\beta^y = \beta$.[10]

There are several issues concerning computing standard errors for this pooled specification. 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.[11]

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

[8] In appendix A.5 we present a stylized example of how a single person's earnings history turns into several potential earnings records in our regression.

[9] 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.

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

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

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.[12]

The sample described above are 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 A6), we restrict to the sample of people with positive earnings in a calendar year for up to 4 years after the displacement in any of the 30 LEHD states that we have available. 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 3.3. In that section, we discuss the incidence of zero earnings as one of the outcomes of displacement.

2.1.1 Earnings loss of displaced workers

Figure 2 plots the δ_k coefficents from specification (2) with the top panel comparing earnings loss following the administrative, mass-layoff measure and the survey-based notion of firm distress. The bottom panel contrasts the earnings loss following a SIPP-reported quit or distress-related job loss. In both panels, the control group is the nonseparators (in the given quarter) in the matched SIPP-LEHD data.

The survey-based indicator of distress-related job loss has broadly the same shape as the administrative trajectories, but there are important differences. First, the immediate decline in earnings is greater in the survey measure—about $1500 more than with the conventional mass layoff measure. Second, the survey data measures yield earnings trajectories that flatten out after 10 quarters while the administrative measures yield earnings trajectories that may appear to continue on the road to recovery. Hence, one of the central findings of the paper is that survey-indicated displacements are followed by worse earnings outcomes in

[12] In our application, we have about about 34 clusters in the time dimension and over 20,000 dimensions in the person dimension.

the short and longer run than the firm-side measure.

The survey data also provide information to evaluate the source of the differences by examining quits. The consequences of survey-reported quits and distress-related job losses are very different. The initial earnings dip following a survey-reported quit is $2000 a quarter compared to $7000 a quarter for a distress-related job loss. The long-term earnings patterns are also different. Earnings of quits recover almost completely.[13] Hence, the survey's information in quits suggests an explanation of why the firm-side indicators of displacement show attenuated earnings loss relative to the survey indicator. The firm-side indicator will in general include separators who leave for their own reasons. Table 3 confirms that over 40% of job leavers under the firm-side indication of distress are indeed in such categories.

The pattern of earnings losses that we document for the survey reported layoffs are similar to the literature.[14] Table 5 shows that the average pre-separation (quarter $t-1$) earnings of the survey mass layoff group was about $12,600. Thus, the earnings dip is about 55 percent and the long-term earnings losses, after 4 years, are about 16 percent.[15] In Jacobson, LaLonde, and Sullivan (1993) the earnings dip is about 42 percent and the long-term earnings losses, after 6 years, are about 26 percent.[16] In Couch and Placzek (2010) the equivalent earnings dip is 32-33 percent and the long-term earnings losses, after 6 years, are 12-15 percent.[17] Finally, in Davis and von Wachter (2011, pg. 17) the *annual* earnings dip is 25 percent in expansions and 39 percent in recessions, while the long-term earnings losses are 10 percent in expansions and 15-20 in recessions.

[13] It is somewhat surprising that the survey-reported quits experience earnings losses at all, since the revealed preference notion of a quit suggests that workers quit to realize raises. Exploring this further, we disaggregate the quits into the "quit for another job" and "quit for some other reason" categories recorded in the SIPP, and estimate the earnings losses separately. As expected, those quitting to employment actually record small gains in earnings, whereas those "quitting for some other reason" experience significant losses. See Figure A4.

[14] We would not necessarily expect our estimates to be identical to the literature because we impose a shorter tenure requirement, include older workers, include fewer periods of pre-separation earnings, and consequently compare post-separations earnings to a period closer to the separation itself.

[15] The earnings dip is $\frac{7000}{12600}$, and the long-term earnings loss is $\frac{2000}{12600}$. For the administrative mass layoffs the dip is $\frac{5517}{12600}$ and long-term losses are $\frac{1500}{12600}$ (using the average of the last three periods).

[16] We arrive at these numbers as follows: we read the initial earnings dip of about $2500 relative to 5 years prior to separation off of figure 2. We base a long-term earnings loss of $1600 on figure 2, as well as the text on page 697. We compute quarterly earnings of $6049 based on table 1. The earnings dip is then $\frac{2500}{6049}$ and the long-term earnings loss is $\frac{1600}{6049}$. These numbers are very close to those in the literature review table, table 1, in Couch and Placzek (2010).

[17] This is based on table 1 in Couch and Placzek (2010).

2.1.2 Severance pay of displaced workers

To varying degrees, all the distress indicators show a predisplacement increase in earnings. One of the celebrated findings in Jacobson, LaLonde, and Sullivan (1993) is the predisplacement earnings dip, though Davis and von Wachter (2011) do not find such a dip. At least in our data, this spike is due to the presence of severance pay. Because the SIPP contains a question about the size of any severance payments, we can remove it from the administrative data earnings and then recompute the earnings losses. Figure 3 shows that without severance pay there is indeed a predisplacement earnings dip of roughly $1000 to $2000 a quarter prior to displacement. Reassuringly, there is very little recorded severance pay among those reporting a quit.[18]

2.2 Interpreting earnings losses: Adverse selection

The previous section established that the earnings losses of displaced workers based on either survey or administrative indicators yield earnings losses that are similar to each other and to the literature. The central puzzle of this literature is why earnings losses are so persistent. There are several classes of explanations: for example, workers might lose human capital (e.g. Topel (1990)), search capital (e.g. Davis and von Wachter (2011), Krolikowski (2013a) and Jung and Kuhn (2012)) or the market might learn that they were adversely selected (e.g. Gibbons and Katz (1991)). Specifically, displaced workers might be selected on the basis of private information of the firm; for example, the worker is low productivity relative to his wage.

Some suggestive evidence for the potential quantitative importance of selection in explaining displaced worker earnings losses comes from looking at workers who self-report being "discharged/fired," which is a form of selection. Recall from Table 2 that only 4% these separations occur in an administratively-indicated mass layoffs compared to 23% of the survey reports of distress. The top panel of Figure 4 shows that self-reported discharged/fired workers suffer large and persistent earnings losses, and the bottom panel shows that these losses are very similar to the self-reported (and mututally exclusive) distress separations. A

[18] Figure A2 plots the path of severance pay comparing layoffs versus quits, as well as the survey vs administrative measures.

natural interpretation of these earnings losses is that the market learns that these workers are low productivity relative to their wage. That this mechanism potentially generates earnings losses the same size as those among the distress separations is suggestive evidence that this mechanism could be important for explaining the earnings losses among the displaced workers.

We now turn to a more formal test for the presence and importance of adverse selection. The canonical test for adverse selection in displaced worker earnings losses compares workers who lose their jobs in a survey-reported plant closing versus layoffs associated with other survey-reports of firm distress (Gibbons and Katz (1991)). The test leverages the idea that there is no scope for selection in plant closings and so the labor market cannot infer that the displaced workers are low marginal product workers relative to their wage,[19] while in other survey-reports of distress there might be scope for selection. Because employers infer that the worker must have been low productivity relative to their wage, in subsequent jobs workers earn less and the displacement has long-term effects on earnings. While this result has been at least somewhat successfully replicated using the Displaced Worker Survey (see Hu and Taber (2011) for an extensive discussion), to the best of our knowledge this test has never been implemented in administrative data.

Implementing this test for adverse selection in administrative data is biased against finding evidence for adverse selection. The test for adverse selection compares earnings losses in large layoffs and small layoffs. A natural way to operationalize small and large layoffs in our data is to use administrative information on whether it was a mass layoff (30% contraction or more) or a non-mass layoff. The prediction of the Gibbons and Katz (1991) test is that earnings losses should be smaller in mass layoffs compared to non-mass layoffs. Two results from section 1 show why this standard test may deliver contaminated results. First, recall that one of the key findings in Table 3 is that both mass layoffs and non-mass layoffs contain many worker-reported quits, and the proportion of worker-reported quits is much greater in non-mass layoffs relative to mass layoffs. Second, a key finding of Panel B of Figure 2 is that on average worker-reported quits have systematically better outcomes than worker-reported

[19] A literature challenges this assumption by arguing that there is nonrandom separation of workers prior to the plant closing (e.g. Bowlus and Vilhuber (2002) and Hilger (2012)). This mechanism biases the researcher against finding evidence of adverse selection.

distress separations. Combined, these two facts suggest that the higher share of quits in non-mass layoffs introduces a composition bias against finding evidence of adverse selection using the Gibbons and Katz (1991) test.

The top panel of Figure 5 demonstrates that the earnings losses from non-mass layoffs are on average smaller than the earnings losses in mass layoffs.[20] Without further information, this would be interpreted as evidence against the importance of adverse selection in explaining displaced worker earnings losses.[21]

Survey data can address the composition bias. The bottom panel of Figure 5 plots the earnings losses from non-mass versus mass layoffs *limiting the sample to workers who also report that they lost their job due to firm distress.* In this subsample, the ordering of earnings losses corresponding to mass vs non-mass layoffs have flipped relative to the top panel. The earnings losses in the non-mass layoffs are larger and more persistent than the earnings losses in mass layoffs, consistent with the presence of adverse selection.[22]

2.3 Testing for adverse selection within employer size class

The logic of adverse selection implies that the degree of public information as to the state of the firm at the time of the worker's separation should impact the worker's future labor market outcomes. Specifically, the labor market is likely to infer that the worker was higher productivity relative to their wage when the market is aware that there was less scope for the firm to select who to layoff. We proxy for market awareness of the state of the firm by comparing the earnings losses at large and small firms (where we split the sample at greater than or equal to 500 employees, which is approximately the size of the firm that the median worker is employed). The prediction is that because the market is more likely to be aware of the state of larger firms there should be greater divergence of worker outcomes between mass layoffs and non-mass layoffs at large firms than small firms.

The top panel of Figure 6 looks at the earnings losses associated with worker reports

[20] The p-value against the null hypothesis of equal earnings losses from +9 to +16 for non-mass and mass layoff separators is 0.187. For −3 to +16 it is 0.000.

[21] We might take the difference between these two lines and interpret this differences as the treatment effect of a mass layoff, where the counterfactual is separating from an employer under different circumstances, rather than not separating, so that the control group is losing a job in a non-mass-layoff in a particular period.

[22] The p-value against the null hypothesis of equality between these two lines from +9 to +16 is 0.060.

of firm distress grouped into four categories: non-mass and mass layoffs at small and large firms. The figure demonstrates that the difference between earnings losses in non-mass and mass layoffs is quantitatively bigger at larger firms, which is consistent with the importance of adverse selection in explaining displaced worker earnings losses.[23] Remarkably, workers who lose their jobs in mass layoffs at large firms exhibit complete recovery. This also provides strong support for the logic of using mass layoffs to attempt to eliminate adverse selection, since in the subsample where it is more plausible that the market is aware of the presence of a mass layoff workers do not suffer long-term earnings losses. As far as we know, our estimates for large firms is the only instance of this approach actually finding complete recovery and therefore suggesting there is no persistent scarring effect from job loss at large firms. By indirection, the results also suggest that mass layoffs at small firms do have a scarring effect—either from adverse selection or market perceptions thereof.[24]

The bottom panel of Figure 6 shows that in non-mass layoffs worker reported quits have smaller long-term earnings losses than worker reported distress separations. In mass layoffs, however, the worker reported quits have long-term—but not short-term—earnings losses that are larger than the worker-reported distress separations.[25] The difference in the short-term earnings losses provides further evidence that the worker distinction between quits and layoffs is behaviorally meaningful in that they predict large differences in outcomes.[26]

[23]The p-value against the null hypothesis of equal earnings losses from +9 to +16 for non-mass and mass layoff separators reporting distress at large firms is 0.058. The p-value against the null hypothesis of equal earnings losses from +9 to +16 for non-mass and mass layoff separators reporting distress at small firms is 0.042

[24]As table A5 highlights, if all separations in mass layoffs at large firms were layoffs then these events would be covered by the WARN Act and potentially trigger other treatments, which would affect the interpretation of the earnings losses. We are not able to assess this possibility with our data.

[25]Unlike for the distress separations where the difference in earnings losses between mass and non-mass layoff separations between +9 and +16 verges on statistically significant at conventional levels, the quit separations do not achieve such significance. Notably, at small firms, the p-value for the difference between earnings in mass and non-mass layoff from +9 to +16 is 0.841 while at large firms it is 0.288.

[26]The astute reader may wonder how to reconcile the path of earnings losses for all mass layoff separations in figure 2, with the two subgroups documented in figure 6. First, the quits have worse long-term outcomes than the distress separations in mass layoffs. Second, there is a small third group, other separations, that also have worse long-term outcomes than the distress separations.

2.4 Implications for whether survey or administrative data deliver the true cost of job loss

The results in this section highlight important heterogeneity in the costs of displacement. Conditional on a survey response of distress, there is important heterogeneity in the earnings losses of workers depending on whether it is a large or small employer, and whether it was a mass layoff or not. Conditional on it being an administrative data mass layoff, there is also heterogeneity based on whether it was a separation from a large or small employer.

3 Other Outcomes

3.1 Using UI Collection as an indicator of economic distress

UI collection is an alternative indicator of economic distress provided by the survey data. Some studies use administrative data on UI collection (not available in the LEHD) as an indicator of economic distress (e.g. BLS Mass Layoff Program, Jacobson, Lalonde, and Sullivan (2005) and Hilger (2012)) while others use survey data (e.g. Keys (2010)). Our matched administrative-survey data allows for a comparison.

The information about UI collection in table 2 demonstrates that UI collection as an indicator for economic distress aligns more closely with survey-reports than the administrative data measures. Specifically, the UI collection indicator picks up twice as many separations that workers label as due to firm distress (62 percent versus 27 and 30 for the administrative data indicators). Superficially, this improved alignment does not come at a cost in alignment in the other direction. Table 3 shows that 40% of the separations that the UI measure captures are not due to survey-reports of firm distress compared to 42% and 44% for the administrative data indicators.

Looking in more detail at the survey-reasons that the UI measure captures, however, shows that using the UI measure skews the composition of the separations towards more problematic survey-reported reasons. A goal of a displacement indicator is to eliminate transitions where workers are fired. Table 2 shows that using the firm-side measures effectively eliminates these worker-reports. In particular, only 4% of these separations are identified as

firm distress by the conventional administrative data indicator. The UI indicator, however, picks up many reports of being fired: 44% of worker reports of "discharged/fired" collect UI. This difference means that 16% of the separations the UI collection indicator flags as due to distress are worker-reported as "discharge/fired" whereas for the mass layoff indicator the corresponding share is 3%.

3.1.1 Social insurance program income of displaced workers

The substantial earnings losses following displacement raises the question of the extent to which government transfer programs, which might provide a form of social insurance, replace the lost earnings. To answer this question, we use data from the SIPP to study the collection of income from social insurance programs: the amount of unemployment benefits a worker receives (including UI) and the path of other government transfers, including Aid for Families with Dependent Children (AFDC), Temporary Assistance for Needy Families (TANF), and Food Stamps. For these outcomes we slightly modify our specification (2) to omit the individual-displacement event fixed effects (the α_i^y) because these outcomes only pertain to workers following displacement. Put differently, an employed worker does not normally collect UI, so it does not make sense to attempt to remove their time-invariant level of UI collection.

Figure 7 demonstrates that workers collect unemployment insurance and other social insurance following separation. The magnitude and time path of these payments reveal that social insurance replaces very little of the longer-term earnings losses.[27] In quarter 0 (the last quarter of employment), for displaced workers average earnings losses without severance pay are about $2000, while average severance pay is about $2000 and average job loss compensation is about $650 for displaced workers. Combined, on average workers replace their earnings losses *in the quarter in which they separate*. Moving to the first quarter after the separation, however, earnings losses hit $7000 while job loss compensation on average climbs to about $800 and severance pay falls to less than $100. Hence, unemployment insurance is an order of magnitude away from replacing the earnings losses. In subsequent

[27] For simplicity we make this quantitative comparison only for the survey-reported displacements, though qualitatively similar statements apply for the administrative data displacements.

quarters, unemployment insurance payments rapidly approach zero, while earnings losses remain.

Figure 8 demonstrates that other non-employment related government transfer programs (e.g. food stamps) do not fill the remaining gap. The average transfers are two orders of magnitude smaller than the earnings losses. It is interesting, though not surprising, that the timing of the transfer payments differs from that of the job loss compensation. In particular, payments peak 2-4 quarters after layoffs (depending on which definition is used).[28]

While government transfers are small among this sample of displaced workers, the sample might be misleading about the size of government transfers to all displaced workers because we eliminated workers with zero earnings in any calendar year. The workers with zero earnings in a calendar year might be the set of workers who are hardest hit and benefit the most from government transfers. Table 8 shows the quarterly averages of various government transfer programs in the year following displacement among the workers with zero earnings in any calendar year for the quarters in which they have zero earnings. The table demonstrates that the takeup, and hence average magnitude, of these programs is low, even among those with no calendar-year earnings. The exception is job loss compensation among those separating due to economic distress, of which almost half of those in this sample receive some support. The average magnitudes are similar to those at the peak in Figure 7.

3.2 Effect of displacement on retirement, reports of disability and Social Security take-up

The previous section studied the consequences for earnings for workers who were consistently employed following displacement. Following the literature, this metric highlights the employment-related consequences of displacement, in particular, changes in the worker's earning power. There are clearly other consequences of displacement. In the next section, we look explicitly at those with zero earnings after displacement. In this section we look at

[28] Meyer, Mok, and Sullivan (2009) show that survey respondents in the SIPP systematically underreport transfer income. The magnitude of the underreporting for some of the programs we study are 20-40% for unemployment insurance (table 8), 60% for worker's compensation (table 9), 20% for food stamps (table 3), 40% for AFDC/TANF. While these magnitudes are large, our discussion above focuses on orders of magnitudes difference, so the main conclusions would not be affected by adjusting for underreporting.

three other outcomes: retirement, collecting Social Security, and worker reports of disability.

We measure each of these outcomes as whether a worker reports in the SIPP being retired, collecting Social Security, or being disabled in either the quarter of the separation, or any of the three quarters following any separation. The treatment group is those who separate due to distress. The control group is the set of workers who continue or who separate not due to distress. This control group differs from the control group used to study displaced worker earnings losses because it include the non-distress separators. The reason to change the control group is that workers retire in the absence of being displaced, while the earnings losses of non-distressed separators have a less clear interpretation.

The specification for retirement is

$$\begin{aligned} retirement_{it} = \beta_0 + \gamma_0 distress_{it} \times sep_{it} + \gamma_1 age60_{it} \times distress_{it} \times sep_{it} + \delta_5^{age} age60_{it} \\ + \sum_{a=1}^{4} \delta_a^{age} agec_{it} + \sum_{e=1}^{3} \delta_e^{educ} educ_{it} + \epsilon_{it}, \end{aligned} \quad (3)$$

where $retirement_{it}$ is an indicator for retired in the current quarter or any of the next three,[29] $distress_{it} \times sep_{it}$ is an indicator variable for a worker separating according to a report (survey or administrative) of firm distress, $age60_{it}$ is an indicator for a worker age 60 or above, $agec_{it}$ is a set of dummy variables for five age categories with the oldest category being age 60 or above, and $educ_{it}$ is a set of dummy variables for three education categories (high school or less, some college, and college or more). In this regression, the coefficient of interest is γ_1, which is the additional probability that a worker who is in the at-risk group (60 or older) and separates due to economic distress retires relative to all workers 60 or older. Because the unit of observation is the person-quarter, we cluster standard errors at the person level. The specification for Social Security replaces the left-hand side variable of equation 3 with an indicator for receiving Social Security in the current or next three quarters.

When a report of disability is the outcome, we modify specification 3 by omitting the interaction $age60_{it} \times distress_{it} \times sep_{it}$ and the coefficient of interest becomes γ_0, the additional probability that a worker who is in the at-risk group (workers of all ages) reports being

[29] We use responses to a survey question about why the worker is not in the labor force to code retirement.

disabled following a separation due to economic distress.

The first row of Table 6 shows that losing a job due to firm distress is strongly associated with retirement. A worker over 60 who separates when administrative data indicates distress is almost 30 percentage points more likely to report being retired in the quarter of the separation or the next 3 quarters than a worker who does not lose their job in this manner (or continues their employment). The survey measure is less strongly correlated with retirement. The survey measure of distress mechanically excludes those workers who report separating due to retirement. Recall from Table 2 that 5% to 7% of workers who report separating due to retirement have the firm-side indicator of distress and that the firm-side indicator of distress captures only about 30% of survey-indicated distress. If there are no other retirements, then putting these numbers together (6% grossed up by a factor of three) reconciles the difference between the survey and firm-side indictors.[30] The second row of table 6 shows that there is a less strong association between separating due to employer distress and collecting Social Security. This may be due to the fact that some workers will delay collecting Social Security until they qualify or reach full retirement age.

Finally, the third row of Table 6 demonstrates that workers who separate due to firm distress are more likely to subsequently report being disabled than workers who either continue their job or separate for other reasons. This provides evidence consistent with the first step in the causal argument of Autor and Duggan (2003) that the SSDI system provides a form of insurance following negative economic shocks. These results demonstrate that losing a job associated with firm distress is correlated with self-reports of disability—the second step would be to establish that these workers subsequently claim SSDI. Unfortunately, the SIPP does not span the necessary length of time to capture the SSDI application process for all workers.

3.3 Zero Earnings: Unemployed or out of the labor force?

Displaced workers may take a long time to find a job, or may drop out of the labor force. In administrative data, it is not possible to distinguish between these outcomes of displacement

[30] This finding that displacement increases the probability of retirement is in contrast to the findings of Chan and Stevens (1999), though they use a different empirical design than we use here.

because all we observe are that the worker has zero earnings and not the corresponding labor force state. Survey data sheds light on the labor force state (unemployment or non-participation) to which these zeros correspond.

In this section we study the set of workers who have at least one calendar year of zeros following displacement. These workers are typically dropped from standard earnings loss regressions (see table A6)—a practice we follow in our earnings loss regressions in section 2. Specifically, we look at the quarters in the year following displacement in which these workers have zero earnings and associate these zeros with the reasons for zero earnings. In the administrative data, earnings may be truly zero because of unemployment or non-participation. Additionally, they may be zero because some positive earnings are not covered by the administrative data.

3.3.1 Labor force state among the zeros

Table 7 demonstrates that 33% of separators have zero earnings for a calendar year and are thus omitted from the regression analysis of earnings loss in Section 2.1.1. Perhaps surprisingly, the incidence of zero earnings for those separating due to distress is not different from the overall incidence. After dividing the non-distressed separators into the other (e.g. retirement) and quit categories, however, we find that the conditioning on positive earnings drops many more other separations than quit separations.

Table 9 shows that workers who lose their job due to firm distress and exhibit zero earnings are more likely to remain in the labor force than workers who lose their jobs for other reasons. Over a third of worker-quarters associated with separations due to firm distress report looking for work, while this share is only 5 percent for other separations. Table 9 also shows that despite these being quarters with *zero* administrative data earnings many workers report being employed. Among the quits 70% of workers report being employed, while this number is only 35% among those who separated due to distress.

3.3.2 Employment among the false zeros

The finding that many of the quarters of zero earnings in administrative data report being employed in survey data raises the question of what kind of employment these workers have that administrative data would miss. There are two obvious possibilities: first, our administrative data only cover 30 states and thus if a worker leaves those 30 states then we would observe zero earnings.[31] Second, 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.

Table 10 investigates these employed individuals among the zero-administrative-data earners in more detail. It tabulates statistics on the worker-quarters with zero administrative data earnings that also report positive earnings in survey data. The table demonstrates that limited geographic coverage of the administrative data is a more common issue among quitters than other separations: about 27 percent of the quitters in this subsample have moved to a state not covered by our version of the LEHD, while only 15 percent of worker-quarters that lost their jobs due to distress have moved states.

A second reason that workers might not show up in administrative data is because of the *nature* of their employment. 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. The table shows that this issue is less common among workers who separated due to distress than other separations. 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 the distressed). Finally, the table indicates that the survey reported earnings are low. Conditional on positive earnings, the mean level of earnings is less than half of that reported for predisplacement earnings among workers separating due to distress documented in table 5.[32]

[31] We are relatively unconstrained compared to classic papers: for example, Jacobson, LaLonde, and Sullivan (1993) only have records from Pennsylvania and Couch and Placzek (2010) only have records from Connecticut. On the other hand, Davis and von Wachter (2011) have Social Security earnings records which are not constrained.

[32] 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 being displaced in the survey, have zero administative earnings, and claim to be employed, only 53 percent actually report positive

4 Conclusion

What happens to workers when they lose their job because of adverse economic conditions at their employer? This question has preoccupied economists, and is of central concern to policymakers, especially in light of the persistent unemployment following the Great Recession. To answer this question, one needs to identify job losses that are indeed associated with economic conditions at the firm. Specifically, are there indicators from either the firm or worker perspective that reliably distinguish between job loss from economic distress and the myriad of other reasons for separations? This paper uses an innovative match between a household survey and employer/employee administrative data to construct survey and administrative indicators of displacement and evaluate whether these indicators provide consistent information. We find that there is substantial overlap between firm and survey indicators of displacement, but also substantial disagreement. The firm-side data only agrees with the worker's report of displacement about 30% of the time, while the worker report only agrees with the firm-side report about 55 - 60% of the time.

Prior research has shown workers to suffer very large and protracted losses in earnings following a displacement. The survey and administrative indicators of displacement have broadly similar profiles for subsequent earnings with the earnings loss being more severe and more persistent from the survey indicator. It was not clear at the outset whether the administrative or survey indicators would have different consequences in the data. There are possibilities for misclassification with both indicators. Displacement indicated by worker flows in the administrative data can include both voluntary quits and selective layoffs. Likewise, survey responses might also cloud the layoff/fire/quit distinction, and workers may have imperfect knowledge of firms' economic condition.

We used the link between survey and administrative data to evaluate the importance of one potential explanation for displaced worker earnings losses: that such workers are adversely selected by their firm. We demonstrated an important composition bias against finding evidence for adverse selection in administrative data: namely, that non-mass layoffs (the ones with more scope for adverse selection), also contain more quits (and other non-

earnings in the survey.

distress separations). Using our survey data to eliminate such separations provides the first evidence using administrative data of the importance of adverse selection in explaining displaced worker earnings losses.

When the worker and administrative indicators of distress agree, there are no permanent earnings losses of workers losing jobs at large firms in mass layoffs. The complete recovery of earnings eliminates the puzzling finding of very persistent earnings losses following mass layoffs at large firms. For mass layoffs at smaller firms, we do find persistent earnings losses. We interpret this finding to suggest that workers who lose jobs at small firms experience a lasting stigma—either they are truly adversely selected or the marketplace perceives them as such. There is no such persistent stigma for mass layoffs at large firms.

Even if adverse selection is the primary mechanism for long-term earnings losses of displaced workers, then this does not change the common interpretation that there are often large costs of displacement to workers. Indeed, across ways of defining the displacement event and different outcomes following a separation the consequences of displacements are quite bad for workers. Unemployment insurance payments and government transfer programs do not come close to compensating for the experienced losses. The survey also allows us to study earnings after worker-identified quits in the administrative data. We find that there are small and temporary earnings losses following quits, as opposed to large and permanent losses following displacements.

Quite apart from the contribution of the paper to the measurement of displacement and the interpretation of displaced worker earnings losses, it has shown the value of linking survey responses to the administrative data to get a better picture of the outcomes post-displacement. In particular, we find that a displacement event increases the probability of a worker reporting a retirement event, and to a lesser extent, collecting Social Security and reporting being disabled. Moreover, the survey information can shed light on the interpretation of zero-earnings observations in the administrative data following a displacement. To the extent that understanding firm-side determinants of nonparticipation or retirement is important, then further links between survey and administrative data may yield more important insights.

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

Detailed Survey Reason For Separation	Obs.	Share of Separations
Distress		
On layoff	631	0.14
Employer bankrupt or sold business	128	0.03
Slack work or business conditions	133	0.03
Total	**892**	**0.20**
Quit		
Quit to take another job	1,397	0.32
Unsatisfactory work arrangement	358	0.08
Quit for some other reason	601	0.14
Total	**2,356**	**0.53**
Other		
Retirement or old age	428	0.10
Other family/personal/child obligation	174	0.04
Own illness/injury	114	0.03
School/training	63	0.01
Job was temporary and ended	50	0.01
Discharged/fired	329	0.07
Total	**1,158**	**0.26**
Total Separations (Unique Persons)	4,406 (4,255)	1.00
Total Continuers (Unique Persons)	176,966 (23,473)	N/A

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 identifies the number of person-quarter continuing jobs in the sample.

Table 2. Relationship between Survey and Administrative Measures

Panel A.

Detail Survey Reason For Separation	Obs.	Mass Layoff Indicator	Collecting UI
Distress			
On layoff	631	0.23	0.65
Employer bankrupt or sold business	128	0.59	0.50
Slack work or business conditions	133	0.16	0.59
Total	**892**	**0.27**	**0.62**
Quit			
Quit to take another job	1,397	0.06	0.04
Unsatisfactory work arrangement	358	0.04	0.10
Quit for some other reason	601	0.08	0.13
Total	**2,356**	**0.06**	**0.07**
Other			
Retirement or old age	428	0.05	0.03
Other family/personal/ child obligation	174	0.03	0.03
Own illness/injury	114	0.04	0.11
School/training	63	0.03	0.03
Job was temporary and ended	50	0.12	0.26
Discharged/fired	329	0.04	0.44
Total	**1,158**	**0.05**	**0.16**
Continuers	176,966	0.03	0.02

Panel B. Overall share of separations labelled displacements

Indicator	Share
Survey	0.20
Net Flow Indicator	0.10
UI collection (survey)	0.21

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 last two columns report the share of the survey response identified by the indicated administrative measure as a displacement. The Mass Layoff Indicator equals 1 when the separation occurs as part of a firm net employment contraction of $> 30\%$. The Collect UI indicator equals 1 when the respondent identifies collecting unemployment insurance in any of the three quarters following a separation.

Table 3. Administrative Indicators Captured by Survey Indicators

Panel A: Summary

	Survey reason for separation	
	Distress	Not Distress
Mass Layoff Indicator		
Yes	56%	44%
No	16%	84%
Collect UI (Survey)		
Yes	60%	40%
No	10%	90%

Panel B: Detail

	Survey Reason for Separation		
	Distress	Quit	Other
Mass Layoff Indicator			
Yes	243	141	54
No	649	2,215	1,104

Source: SIPP-LEHD as explained in text.
This table reports the share of administrative-data-based displacements that the survey-responses also classify as distress. The sample consists of only those individuals separating from a job. See Table 2 for the definition of the mass layoff indicator.

Table 4. Firm Size and Structure

Size	Distress (Survey)	Mass Layoff Indicator Yes	No	Agree
	Yes	58	79	0.42
50-99	No	39	414	0.91
	Agree	0.60	0.84	
	Yes	70	125	0.36
100-249	No	35	626	0.95
	Agree	0.67	0.83	
	Yes	43	124	0.26
250-499	No	32	439	0.93
	Agree	0.57	0.78	
	Yes	29	86	0.25
500-999	No	25	424	0.94
	Agree	0.54	0.83	
	Yes	24	83	0.22
1000-2499	No	21	551	0.96
	Agree	0.53	0.87	
	Yes	19	152	0.11
2500 $\geq$	No	43	865	0.95
	Agree	0.31	0.85	
Structure				
	Yes	175	396	0.31
Single Unit	No	131	1,798	0.93
	Agree	0.57	0.82	
	Yes	68	253	0.21
Multi Unit	No	64	1,521	0.96
	Agree	0.52	0.86	

Source: SIPP-LEHD as explained in text.
This table reports firm characteristics associated with worker-reported separations. Size corresponds to the number of employees at the firm three quarters prior to the separation. The rows and columns corresponding to Yes and No represent counts, whereas the rows and columns corresponding to Agree represent the row and column shares of agreement between the survey and administrative measures. The mass layoff administrative indicator is defined in Table 2.

Table 5. Pre-Separation Earnings (2009 Dollars)

Survey Reason for Separation	Mean	Mean by Quartile				Obs.
		1	2	3	4	
Distress	12,629	5,147	8,379	12,279	24,633	597
Quit	12,349	4,920	8,210	11,914	24,354	1,800
Other	10,806	4,303	7,083	10,768	20,918	533
Continue	13,793	5,726	9,525	13,832	26,089	158,976

Source: SIPP-LEHD as explained in text.
The first column reports the survey-identified reason for separation. The second column reports the mean $t-1$ (pre-separation) earnings. Columns 3 through 6 provide additional average earnings based on calendar-year quarter. The final column report the number of separations according to each survey-reported classification of separation.

Table 6. Retirement and other outcomes

Outcome	Survey Indicator	Mass Layoff Indicator (Admin)
Retirement	0.135	0.292
	(0.052)	(0.077)
Social Security	0.082	0.082
	(0.062)	(0.077)
Disabled	0.050	0.038
	(0.011)	(0.014)

Source: SIPP-LEHD as explained in text.
This table reports the coefficients of interest from 6 separate regressions (standard errors clustered at the individual level are in parentheses). The retirement and Social Security (SS) rows report the coefficients on the interaction of an indicator for age 60 or older with the relevant indicator. The disabled row reports the coefficient on the relevant indicator (without any interactions). The complete regressions, as well as additional specifications, are in Tables A8, A9 and A10.

Table 7. Separators with Zero Earnings

Survey Reason for Separation	Number of Separators	Share with Zero Earnings
Distress	892	0.328
Quit	2,356	0.229
Other	1,158	0.531
Total	4,406	0.329

Source: SIPP-LEHD as explained in text.
This table reports the share of total separations that include any calendar-year of zero earnings in a 4-year interval following a separation. Among continuers, 9.8% have a calendar year of zero earnings. These observations are dropped from the sample constructed to estimate earnings losses in section 2.

Table 8. Social Insurance Take-Up Among Persons with Zero Calendar-Year Earnings

	Survey Reason for Separation		
	Distress	Quit	Other
	2009 Dollars		
Avg Quarterly Job Loss Compensation	816.1	126.0	175.5
Avg Quarterly Government Transfers	29.9	38.8	37.8
	Percent		
Positive Job Loss Compensation	46.9%	6.3%	9.3%
Positive Government Transfers	7.7%	6.2%	6.3%

Source: SIPP-LEHD as explained in text.
The sample consists of respondents who had any calendar-year of zero earnings in a 5-year interval following a separation. This table shows survey-reports of worker social insurance take-up in the four quarters following a separation in which the worker had zero administrative data earnings.

Table 9. Activities in Quarters with Zero Administrative Data Earnings (Rates)

	Survey Reason for Separation		
Survey Reported Activity	Distress	Quit	Other
Employed	0.36	0.69	0.20
Looking for work	0.38	0.05	0.05
Retired	0.06	0.04	0.45
Other	0.33	0.24	0.26
Total	1.05	1.02	1.03
Obs.	609	1,103	1,480

Source: SIPP-LEHD as explained in text.
The sample consists of respondents who had any calendar-year of zero earnings in a 5-year interval following a separation. This table shows survey-reports of worker activities in the four quarters following a separation in which the worker had zero administrative data earnings. Because respondents can identify different activities within the three months in a quarter, the percentages do not sum one.

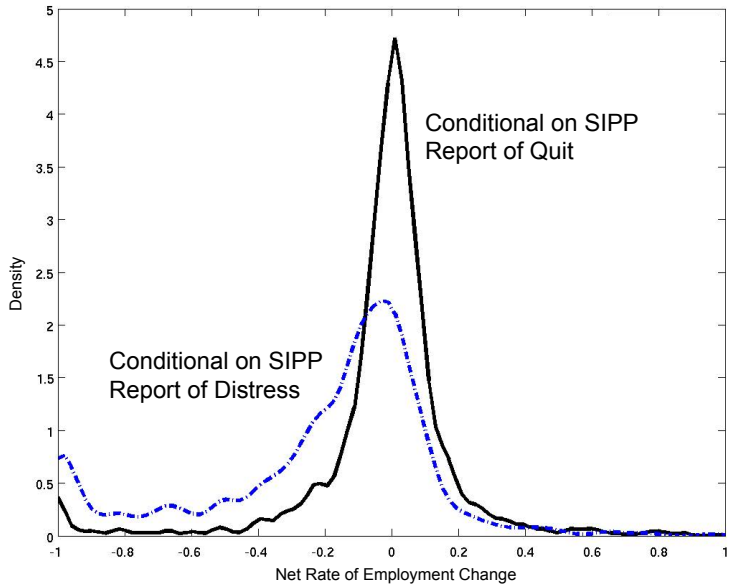
Table 10. SIPP Employment in Quarters with Zero Administrative Data Earnings (Rates)

	Survey Reason for Separation		
	Distress	Quit	Other
Non-LEHD State	0.15	0.28	0.11
Work for Government or Family	0.11	0.21	0.27
Part-time Worker	0.34	0.26	0.41
Share with Positive SIPP Earnings	0.53	0.73	0.47
Mean of Positive SIPP Earnings (2009 Dollars)	5,077	6,044	3,393
Obs.	216	763	297

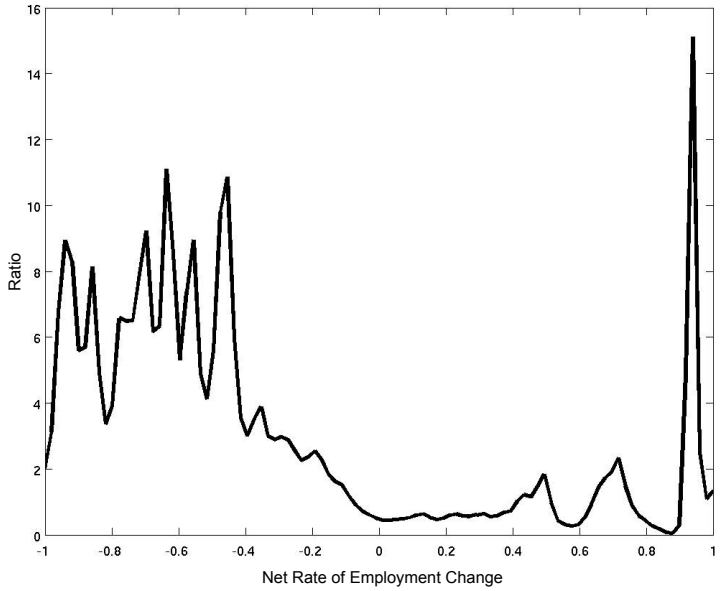
Source: SIPP-LEHD as explained in text.
The sample consists of respondents who had any calendar-year of zero earnings in a 5-year interval following a separation. This table reports worker response in quarters in the first year following a separation in which the worker had zero administrative data earnings but reported positive earnings in the SIPP.

Figure 1. Distribution of Administrative Measure by Survey Reason for Separation

A. Distribution of Distress vs Quit



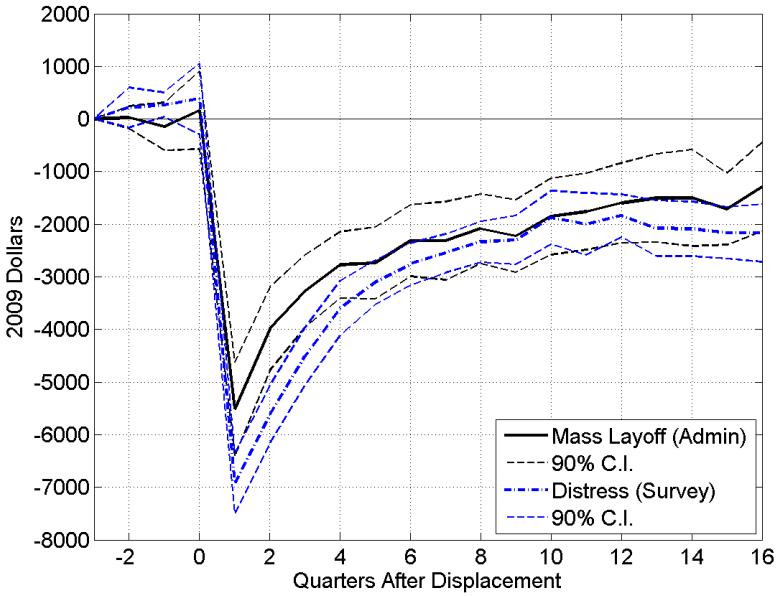
B. Relative Probability of Distress vs Quit



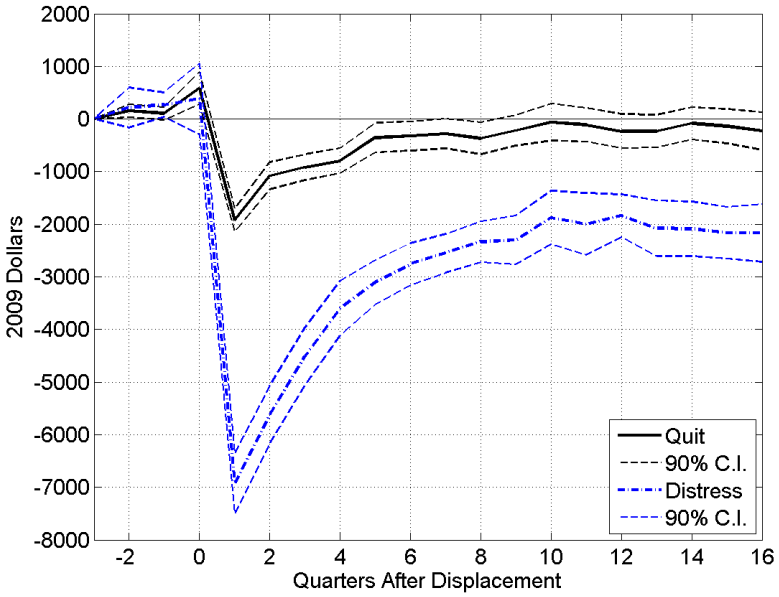
Source: SIPP-LEHD as explained in text.
This figure plots the kernel density estimate of survey-reported quit separations vs survey-repoted distress separations. The top panel plots the estimates based on the percent growth of the firm from $t-3$ to $t+1$ surrounding the separation (net flows), and the bottom panel plots the ratio of the distress and quit pdf shown in the top panel. The net flows measure has been truncated at a value of 1 in the interests of clarity.

Figure 2. Earnings Losses Relative to Non-Separators

A. Survey versus Administrative Measures



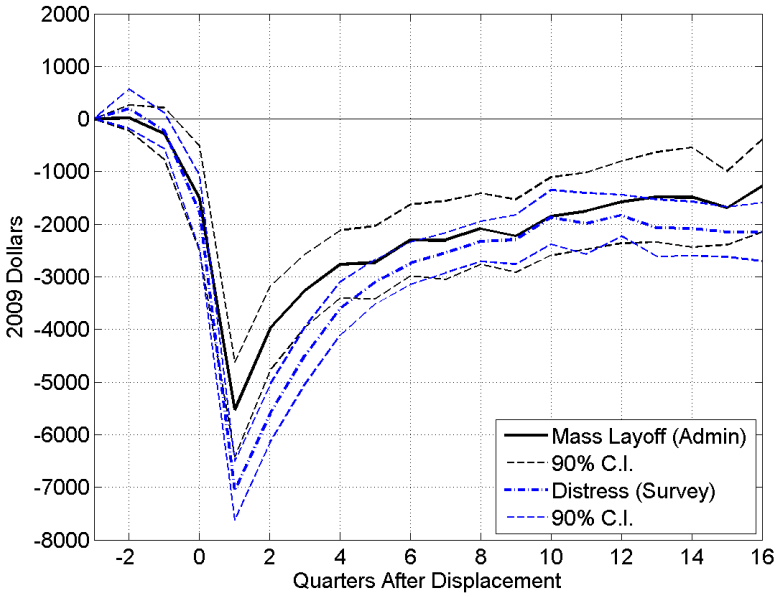
B. Survey Measures: Distress versus Quit



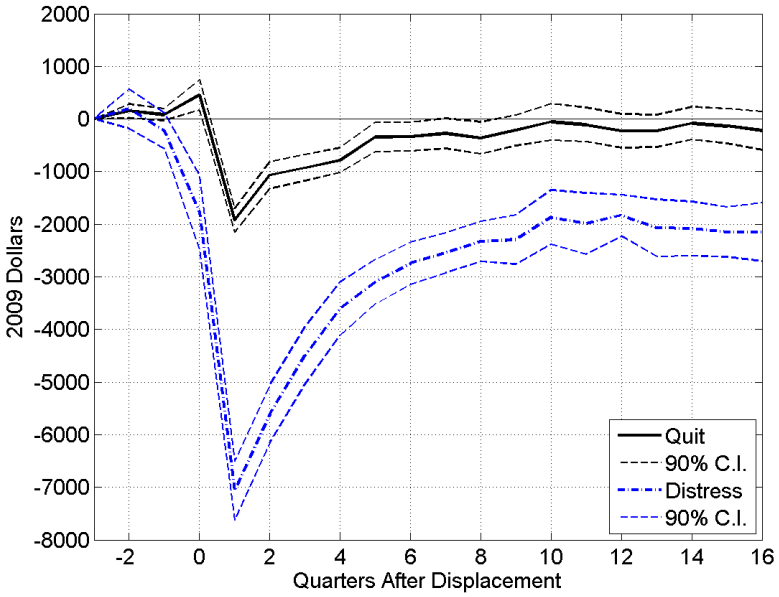
Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. The top panel contrasts the earnings losses of the mass layoff administrative indicator of displacement with the survey measure – those reporting distress in the SIPP. The bottom panel contrasts the outcomes of those reporting distress with those reporting a quit in the SIPP. The values are coefficient estimates from a regression including a quartic in age and individual-quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level.

Figure 3. Earnings Losses without Severance Pay

A. Survey versus Administrative Measures



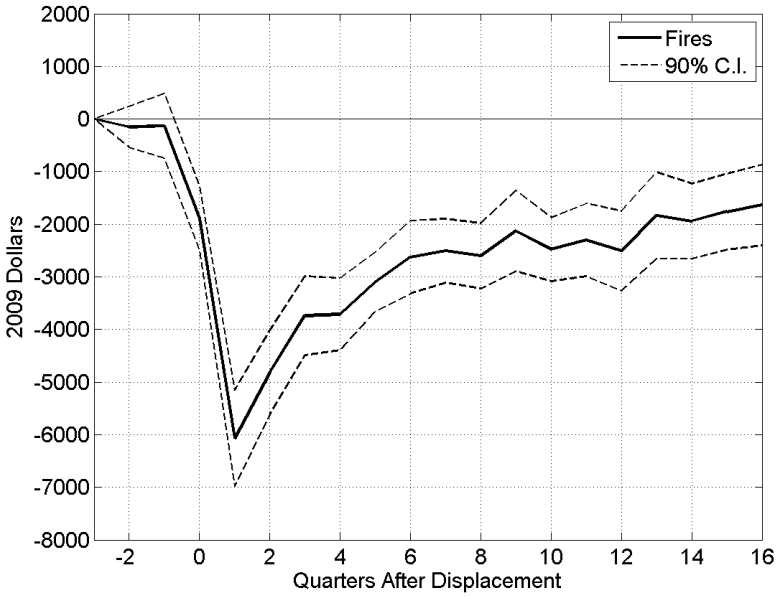
B. Survey Measures: Distress versus Quit



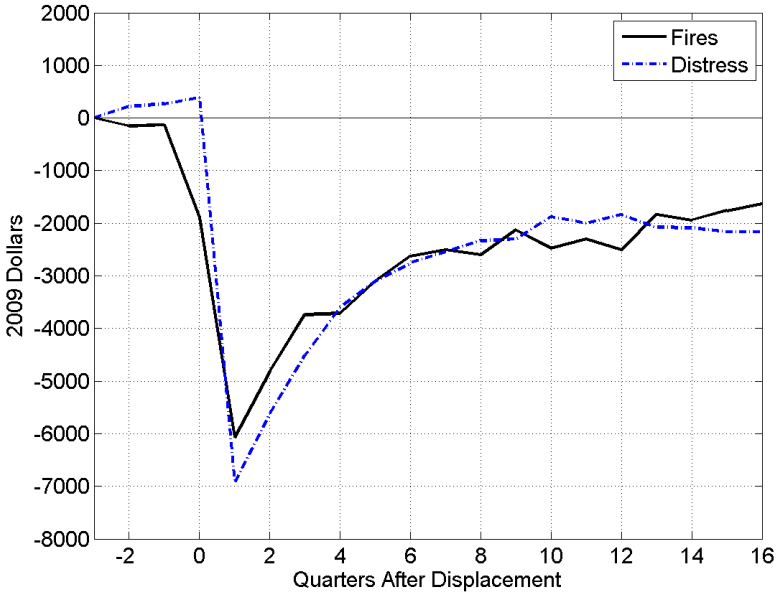
Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. Earnings from the LEHD have been adjusted to remove any SIPP-identified severance payments. The top panel contrasts the mass layoff administrative indicator of displacement with the survey measure – those reporting distress in the SIPP. The bottom panel contrasts the outcomes of those reporting distress with those reporting a quit in the SIPP. The values are coefficient estimates from a regression including a quartic in age and individual/quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level.

Figure 4. Earnings Losses among Survey-Reported Discharged/Fired

A. Discharged/Fired



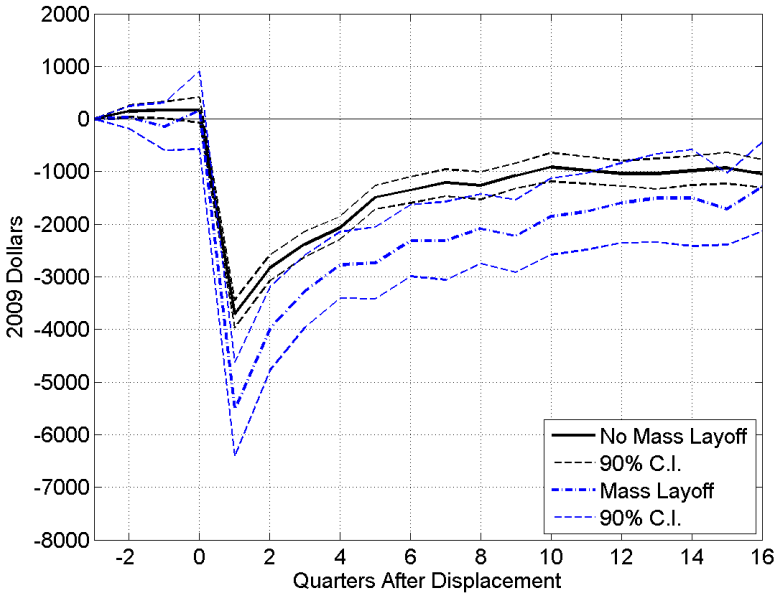
B. Discharged/Fired compared to Distress



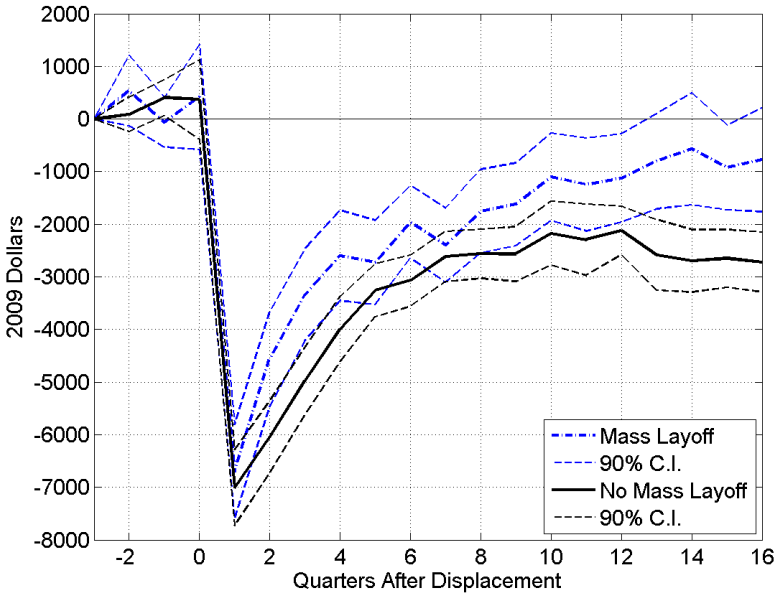
Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. The top panel reports earnings losses among those reporting dischard/fired in the SIPP. The bottom panel contrasts the outcomes of those reporting discharged/fired versus those reporting distress in the SIPP. The values are coefficient estimates from a regression including a quartic in age and individual/quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level.

Figure 5. Earnings Losses in Administrative Mass Layoffs versus Non-Mass Layoffs

A. Full Sample



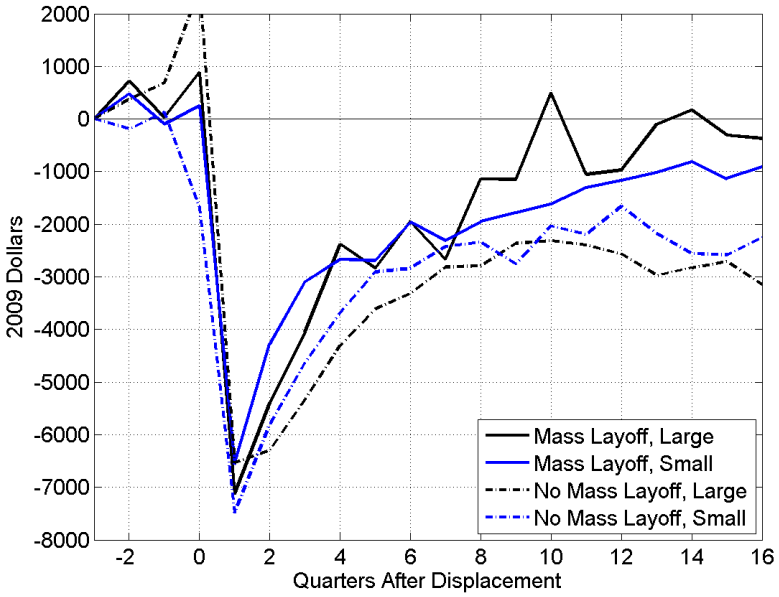
B. Survey-Identified Distress



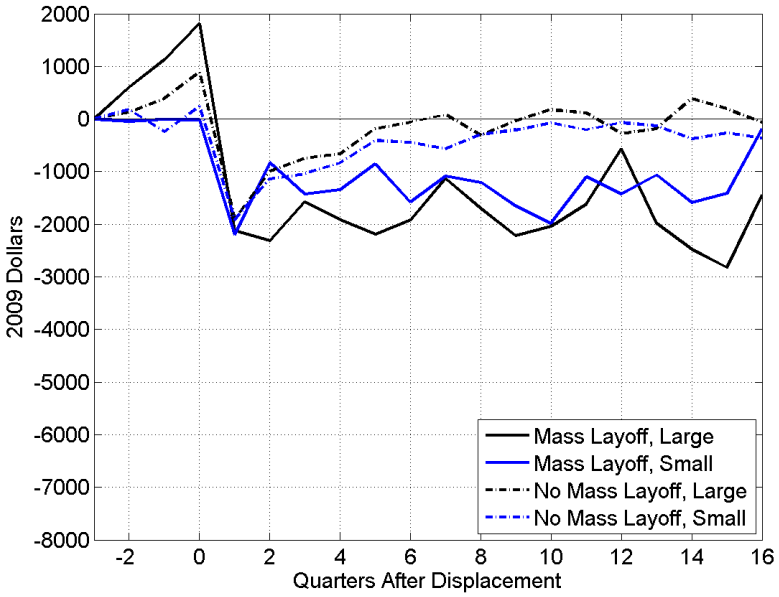
Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. The top panel contrasts the mass layoff indicator of displacement using administrative data for all separations, while the bottom panel removes those who in the survey indicate separating due to a "quit" or "other". The values are coefficient estimates from a regression including a quartic in age and individual/quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level. The p-value of the null of equality in quarters +9 to +16 in panel A is 0.187. In panel B it is 0.060.

Figure 6. Survey Reported Distress in Non-Mass and Mass Layoffs at Small and Large Employers

A. Survey-Identified Distress



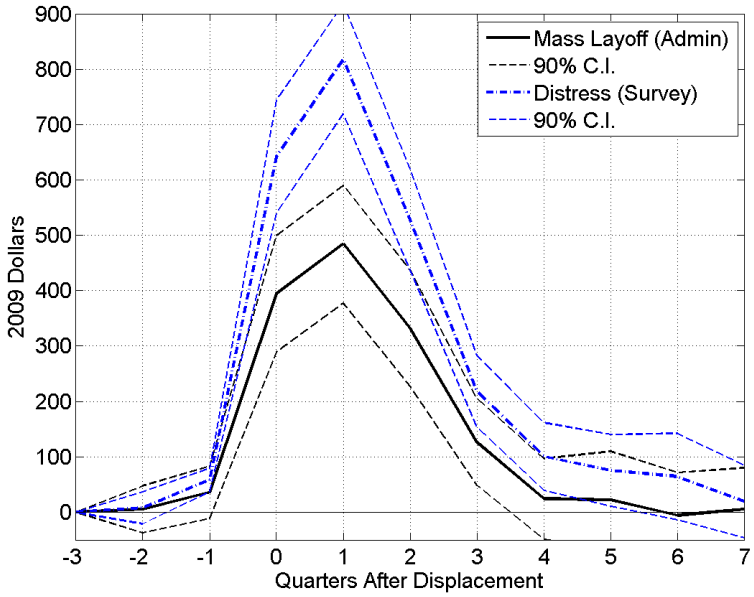
B. Survey-Identified Quit



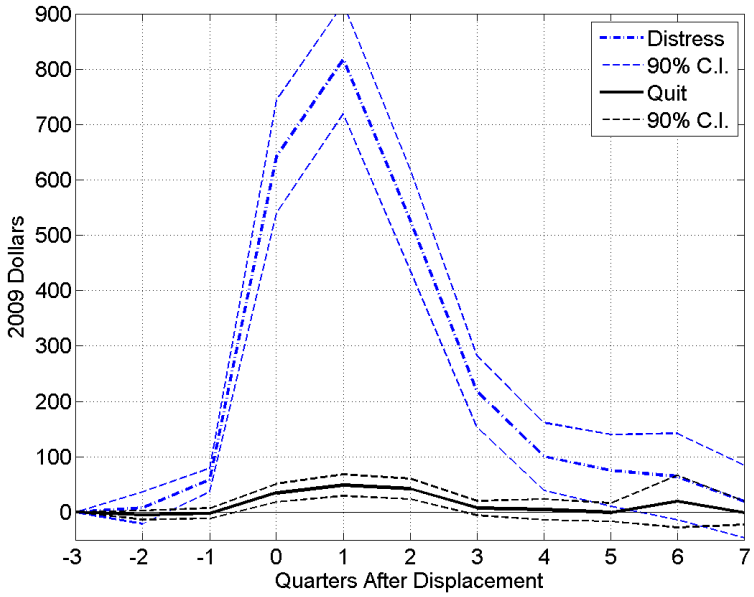
Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. Both panels contrasts the mass layoff indicator of displacement according to the size of the firm, where large corresponds to those employers with ≥ 500 employees. The top panel restricts the sample to those separations identified by the SIPP as distress, while the bottom panel limits the sample to those identified in the SIPP as a quit. The values are coefficient estimates from a regression including a quartic in age and individual/quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level. The p-value of the null of equality in quarters +9 to +16 in panel A between mass layoff and non-mlass layoff at large firms is 0.058, at small firms it is 0.042. In panel B at large firms it is 0.288 and at small firms it is 0.841.

Figure 7. Unemployment Insurance (and other job loss compensation) Relative to Non-Separators

A. Survey versus Administrative Measures



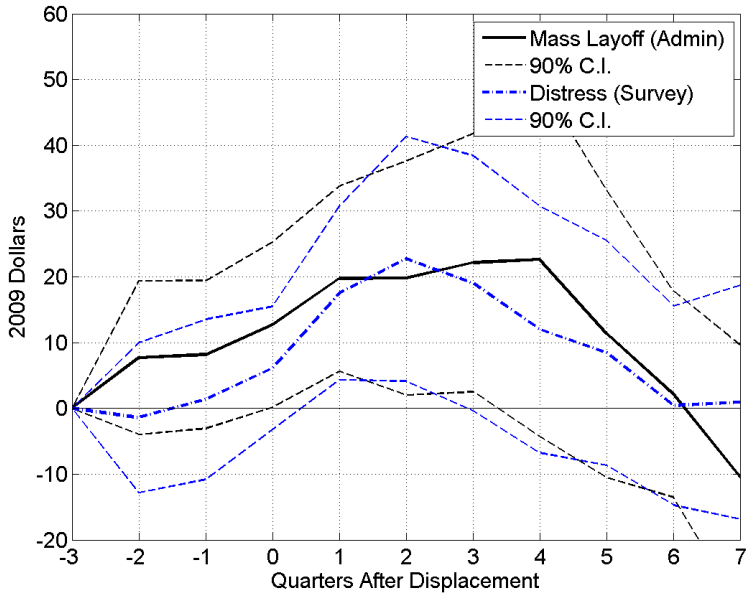
B. Survey Measures: Quit versus Distress



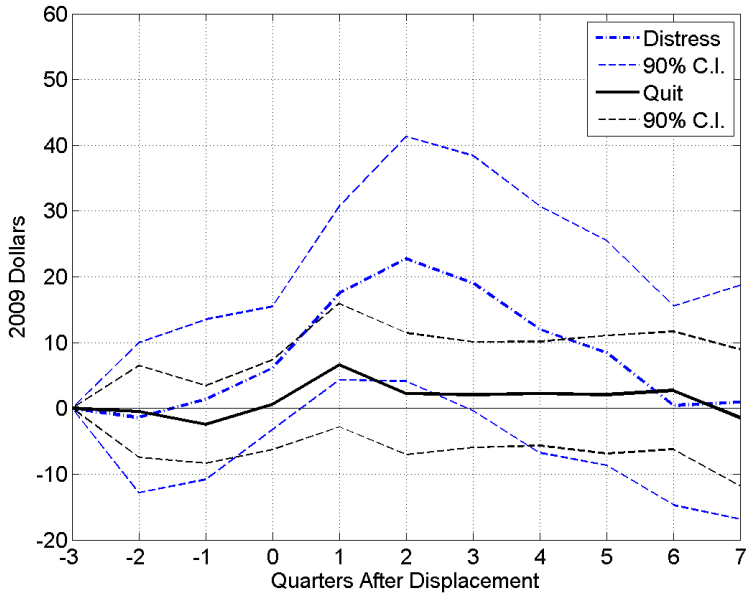
Source: SIPP-LEHD as explained in text.
This figure plots the job loss compensation payments to individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. Job loss compensation includes state unemployment compensation, worker's compensation, own sickness, accident, or disability insurance payments, and employer disability payments. The top panel contrasts the mass layoff administrative indicator of displacement with the survey measure – those reporting distress in the SIPP. The bottom panel contrasts the outcomes of those reporting distress with those reporting a quit in the SIPP. The values are coefficient estimates from a regression including a quartic in age. See equation (2) in the text. Standard errors are clustered at the person level.

Figure 8. Non-employment Related Government Transfers Relative to Non-Separators

A. Survey versus Administrative Measures



B. Survey Measures: Quit versus Distress



Source: SIPP-LEHD as explained in text.
This figure plots the job loss compensation payments to individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. Government transfer payments include AFDC, TANF, and Food Stamps. The top panel contrasts the mass layoff administrative indicator of displacement with the survey measure – those reporting distress in the SIPP. The bottom panel contrasts the outcomes of those reporting distress with those reporting a quit in the SIPP. The values are coefficient estimates from a regression including a quartic in age. See equation (2) in the text. Standard errors are clustered at the person level.

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A Appendix: Matching Procedure and Properties of the Match

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 zero earnings 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 A2 reports the quality of the match. We start with 22,694 separations in the SIPP and are able to match 6833 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 A2 reports the quality of the match. We start with 525,854 job-quarters in the SIPP and are able to match 234,387 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.
- Whether or not have the worker can be characterized as having "full quarter" employment in quarter t: a worker has full quarter employment in quarter t if the worker has positive earnings at the same establishment (SEIN) in quarter $t-1$, t and $t+1$.

A.3.2 Establishment-Level Variables

We construct our notion of employment to be consistent with the worker flows. In particular, 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;
- Separations in quarter t: the number of workers employed in quarter $t-4$ who are not employed in quarter t;
- Hires in quarter t: the number of workers employed in quarter t who were not employed in quarter $t-4$.

A.4 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.[33]

Table A3 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

[33] 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.

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

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

A.5 Samples

In this appendix we describe the criterion for each of the samples that we use in the regression analyses.

A.5.1 Constructing the analysis sample from earnings records

We present a stylized example in table A1 of how the record of an individual worker would turn into two "continuing" earnings histories and one "separating" earnings history. In particular, we keep track of 3 quarters of earnings prior to the separation. Column (1) show the workers earnings quarter by quarter. Column (2) shows the ID of the employer. Column (3) shows calendar time. Column (4) shows one event time records that we would extract. From the perspective of 2000:IV, the worker has 4 quarters of earnings and continues at their employer in the subsequent period. Therefore, when we associate the earnings with these event times, we have a record of a "continuer." From the perspective of 2001:I, however, the worker will separate. Therefore, this set of earnings captures the record of a separator. Finally, from the perspective of 2002:III the worker has satisfied our tenure requirement and is a continuer again.

A.5.2 Sample in section 1.5: Relationship between worker reports and firm performance

The sample used in this section consists of all SIPP worker-quarters that we match to the LEHD subject to the restrictions that the worker be between the ages of 25 and 74 in the relevant quarter, and in quarter $t-1$ their employer had at least 50 employees. In some places in this section we restrict attention to person-quarters that resulted in a separation.

A.5.3 Sample in section 2.1.1: Earnings Losses from Layoffs

The sample used in this section consists of all SIPP worker-quarters that we match to the LEHD subject to the same restrictions as above. In addition, we require that the worker have positive earnings in the calendar year that we find them in the LEHD as well as for

up to four subsequent calendar years (up to 2007). That is, we drop worker-quarters that subsequently have zero earnings.

A.5.4 Sample in section 3.2: Effect of Mass Layoffs on Retirement

The sample is the same as that in section 1.5.

A.5.5 Sample in section 3.3: Zeros

The sample used in this section consists of all SIPP worker-quarters that we match to the LEHD, who separate from a job, and who subsequently record a calendar year of zero earnings (as specified above). In another part of this section, we also restrict our attention to only those who additionally report being employed in the SIPP.

B Appendix: Relationship to the Literature

B.1 Linking administrative and survey data to study displacements

In an important contribution von Wachter, Handwerker, and Hildreth (2012), analyze a link between the Displaced Worker Survey and administrative (UI) records in California. While for issues of comparability with the standard way of measuring displacement this link is ideal, for the purposes of understanding how workers and administrative data classify the same event the DWS is less than ideal. In particular, the DWS only asks workers if they were displaced at any point in the last three years, so it is more difficult to know which job in administrative data the worker is referring to, and issues of recall bias are much more important.[34]

Because the SIPP is higher frequency than the DWS and offers a relatively complete labor market history for the period it covers, it can more precisely date the separation event. The advantages of our survey enables us to compare survey and administrative data descriptions of the same event. That is, the question we ask is: "in both the survey and the administrative data the worker separated in a particular quarter. How are these events described in the two datasets?" In contrast, using the DWS the researcher wonders which quarter the worker refers to, and whether they remembered the separation at all. Indeed, one of the important contributions of von Wachter, Handwerker, and Hildreth (2012) is showing how accounting for salience bias in the DWS changes estimates of displaced workers earnings losses.

Substantively, von Wachter, Handwerker, and Hildreth (2012) find that the survey report yields lower rates of the incidence of displacement than administrative data, while we find that the survey report yields rates that are twice as high. In terms of earnings losses, they do not find the same patterns that we do of larger earnings losses in separations that the survey measure indicates were displacements and the administrative measure indicates were not relative to those separations where survey and administrative measures agree. Because of issues of recall, it is hard to know how to interpret this difference.

B.2 Quit-Layoff distinction

A large literature has documented that there are differences in worker outcomes following worker reports of quits versus layoffs (see results and/or citations in McLaughlin (1991) and Davis (2005)). An important question is how much credence to place on worker reports of reasons for separation. Workers may just label separations with subsequent (or anticipated) bad outcomes as layoffs, and separations with subsequent good outcomes as quits. If this were the case, then the worker report contains no information about the nature of the separation event. Understanding to what extent there are differences in firm performance around worker-reported layoff and quit events sheds light on the sense in which quits and layoffs are distinct, and potentially informs theories of the labor market, in particular, how to model the separation event.

[34] Evans and Leighton (1995, pg. 395) conclude that counts of displacement based on the Displaced Worker Survey are too low by 30-35%. In addition, Oyer (2004) shows that Displaced Worker Survey-style questions lead workers to overstate pre-displacement earnings.

Davis, Faberman, and Haltiwanger (2012, Figure 6) document a distinction in firm performance based on firm reports of quits and layoffs in that the layoff rate rises much more rapidly than the quit rate as firms contract. Relative to that paper, we are able to look at worker reports of layoffs versus quits. Using worker reports allows us to parallel the literature that has documented differences in worker outcomes following quits versus layoffs. We are able to show that worker reports indeed map onto differences in firm performance, and that these differences in firm performance map on to different earnings (and other) outcomes.

B.3 Relationship between gross flows and net flows

Work by Davis, Haltiwanger and coauthors (e.g. Davis and Haltiwanger (1992), Davis and Haltiwanger (1999), Davis, Faberman, and Haltiwanger (2012)) as well as others (e.g. Burgess, Lane, and Stevens (2000)) has emphasized the importance of considering both net and gross flows when looking at labor markets. In appendix C,we show that this distinction is important for understanding displacement indicators. The new measure that we introduce is based on the idea that even when firms are contracting they might differ in their hiring behavior, and these differences reflect how workers perceive the separation event. If a firm is contracting but still engaged in substantial hiring, then a worker is less likely to perceive their separation as due to firm distress. This could be for at least two reasons. The reasoning that motivated the development of the measure is that if the hiring rate remains high while the employer is contracting then a given separation is more likely to have been replaced and so it is less likely that the worker lost their job because their "position was abolished." A complementary reason is that a firm that has stopped hiring is less tied to the broader labor market and so it is more likely that conditions in the firm have deteriorated (relative to the market).

B.4 Incidence of displacements

Another exercise in the literature has been analyzing the rate of job displacement and comparing these rates across different ways of defining a displacement (e.g. Davis and von Wachter (2011, figure 2)). While our exercise is conceptually different—we are primarily interested in understanding how different data sources label the same event—our results allow us to comment on this line of inquiry.

In making the comparison across measures we should not expect survey measures and administrative measures to agree about the incidence of displacement, since they have different goals. Administrative measures based on firm-side performance pick a severe threshold with the hope that most of the workers who separate did so due to economic distress. Insofar as there are enough transitions due to economic distress without the employer contracting by 30%, then administrative measures understate the rate of separations due to economic distress at firms. On the other hand, survey measures should, in principle, capture all separations due to economic distress and so potentially yield higher incidence of displacement. In our data the survey measure picks up many more separations than the administrative measure based on firm-side performance (see the lower panel of table 2). To the extent that survey measures yield lower rates of displacement than administrative measures based

on firm performance, as von Wachter, Handwerker, and Hildreth (2012) find, then this is suggestive of severe recall bias in the survey (as Evans and Leighton (1995) document).

A stark example of this issue arises in the different ways in which UI collection could be used to identify displacements. UI collection could be used in the survey way of asking whether an individual worker collects UI. Or it could be used in the mass layoff way of asking whether at least 50 workers from a single employer collected UI (as in the BLS Mass Layoff Program). Mechanically, the second approach will yield a lower rate of displacement but this is because the mass layoff approach is attempting to tag a different kind of event than the individual collection approach.

B.5 Effect of displacement on other outcomes

The instrument of displacement has been used to study many outcomes. We contribute to this literature by contrasting how survey and firm-side indicators of displacement correlate with many outcomes using a common sample of workers. One important contribution has been using survey data to address a central substantive and methodological issue in papers using administrative data on earnings: how to interpret zero earnings? In the sample of zeros that we study, between 50 and 75% of the quarters of zero earnings following displacements are still in the labor force and of these between one-half and two-thirds are still looking for work.[35]

We now use our results on labor force status following displacement to shed light on a puzzle raised by Fallick, Haltiwanger, and McEntarfer (2012). They find, using the Jacobson, LaLonde, and Sullivan (1993) net flow measure of distress that "earnings outcomes depend much less on whether a job separation is associated with a distressed employer than on whether the separator experienced a jobless spell after the separation" (pg. 2). Moreover they find that "separators from distressed firms experience less nonemployment than do separators from non-distressed firms" (pg. 16). They suggest that their results "appear to be at odds with well-established regularities in the literature regarding distressed separators, layoffs and unemployment" (pg. 15) and highlight that "the distinction between unemployment and nonemployment is likely quite important in this context and deserves further investigation" (pg. 18).

While we condition on labor force attachment slightly differently than do Fallick, Haltiwanger, and McEntarfer (2012), our table 7 finds that the probability of a long spell of nonemployment following a separation is equally likely following a separation due to employer distress or other kinds of separations. Table 9 demonstrates that these zeros are not all alike: the zeros of workers separating due to distress are much more likely to be associated with "unemployment" (looking for work) than the zeros of workers separating for other reasons.

[35] We arrive at this number as follows: in table 9 a share 0.36 of distress separations report being employed and 0.38 report looking for work. Of those reporting being employed, table 10 shows that 53% have positive earnings.

B.6 Adverse selection

B.7 Alternative Ways of Identifying Economic Distress

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

B.7.1 Administrative Measures of Mass Displacement in Government Programs

Some US Federal government programs use definitions of mass displacements. These definitions are also displayed in Table A5. 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.

B.7.2 Administrative Measures of Mass Displacement Based on 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 (2012), 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. In section **??** we present a brief analysis of what events at the firm and what survey responses are associated with UI collection (where the UI collection is measured in survey data).

B.7.3 Measures of Mass Displacement Based on 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.[36] He looks at media reports of mass layoffs at public companies from 1987-1995.[37] 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.

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

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

C Appendix: Gross Flows as an Alternative Administrative Measure

This section proposes a new measure of displacement that is based on *gross* worker flows, that is, the behavior of hires and separations. Specifically, we infer evidence of firm distress when the separations are the dominant percentage of total worker flows at the firms. This new measure labels a firm as being in distress if it slows hiring compared to total worker flows, whereas the standard measure labels a firm in distress when separations exceed hires by a given threshold.

Recall, using the notation from section 1, the standard net flow measure is the percent change in emplyment from $t-3$ to $t+1$ is defined by,

$$NF_t = \frac{N_{t+1} - N_{t-3}}{N_{t-3}}.$$

The gross flow measure is the ratio of hires to total flows,

$$GF_t = \frac{H_{t+1}}{S_{t+1} + H_{t+1}} = 1 - \frac{S_{t+1}}{S_{t+1} + H_{t+1}}.$$

This variable attempts to measure the extent to which a given separation was likely mandated by the net change in employer size. By construction this variable ranges between 0 and 1. When it equals 0, no separations are replaced. In this case, net flows equal gross flows and all separations are due to job flows. When it equals 0.5, employment is constant. Finally, when it approaches 1, any potential separations are overwhelmed by hires. In this case, separations are more plausibly due to worker choices because the employer is not contracting.[38] The additional information present in the gross flows measure means that it somewhat more finely delineates the nature of events at the firm, rather than the size of the event at the firm. A simple example will make this distinction concrete. Suppose that $NF_t = -0.30$, that is, there is a 30% contraction in the size of the employer. When studying worker outcomes, it should be reasonable to distinguish between the case where 60% of the time $t-3$ workforce separates and the case where 30% of the time $t-3$ workforce separates. If it is the case where 60% of the time $t-3$ workforce that separates, then *one-half* the workers were replaced (and

[38] This measure is nearly identical to the ratio of churning flows (worker flows beyond job flows) to worker flows that is standard in the literature (e.g. Burgess, Lane, and Stevens (2000)) and measures the excess rate of worker separations relative to job flows (in an accounting sense). Operating at the annual frequency but with quarterly observations let job reallocation (absolute value of net flows) be $JR_{it} = |N_{it} - N_{it-4}|$. Let total worker flows (the sum of hires and separations) be $WF_{it} = H_{it} + S_{it}$. Let churning flows be those not necessary to accomplish job reallocation $CF_{it} = WF_{it} - JR_{it}$. Then the ratio of churning flows to worker flows provides a measure of excess turnover:

$$\frac{CF_{it}}{WF_{it}} = \frac{WF_{it} - JR_{it}}{WF_{it}} = \frac{H_t + S_t - |H_t - S_t|}{S_t + H_t} = \begin{cases} \frac{2S_t}{S_t+H_t} & H_t \geq S_t \\ \frac{2H_t}{S_t+H_t} & H_t < S_t \end{cases}$$

This definition is very similar to our definition of gross flows:

$$GF_{t-1} = \frac{H_t}{S_t + H_t} \in [0, 1].$$

$GF_t = \frac{1}{3}$). Many of these separations are likely not due to bad events at the firm, but to worker choices or idiosyncratic factors (e.g. being fired). In contrast, if only 30% of the time $t-3$ workforce separates, then $GF_t = 0$ and one can reasonably claim that all separations are due to what is going on at the firm.

To create a binary form of the gross flows measure to match that for the net flows, we also use a cutoff: a firm having $GF_t < 0.20$ is in *economic distress* and the worker separations associated with these low gross flows are *displaced workers*. Below we show that this cutoff captures roughly the same number of separations as the standard net flow measure cutoff.

The top panel of Figure A1 shows the distribution of gross flows according to survey-identified layoffs vs quits. Similar to the figure pertaining to the net flows measure, the distribution is significantly left-shifted for those workers reporting a layoff relative to a quit. The bottom panel of Figure A1 shows the ratio of the distributions of layoff vs quit. This panel provides support for the use of a 0.2 threshold for the binary form of the gross flows measure, as there is a large jump in the ratio at this value.

C.1 Regression on binary forms of Gross Flows vs Net Flows Measures

Having shown that the survey responses do carry information about firm-side measures of distress, we now explore more systematically the relationship between the firm-side indicators and the survey responses. In particular, we consider how the firm-side indicators predict the survey response. In Table A7, we present estimates of a linear probability model in the sample of separators to assess which administrative measures of distress predict worker reports of distress. We follow the literature in using binary versions of the net flows and gross flows measures. The net flow indicator is based on the standard cutoff of employment declining at least 30%. The gross flow indictator is based on a gross flows measure of less than 20% as suggested by Figure A1.

Column (1) shows that the net flow measure of firm distress increases the probability of survey report of distress by 39 percentage points. Column (2) shows that the gross flow measure of distress is associated with a 42 percentage point increase. Columns (3) and (4) demonstrate that the gross flow measure is substantially more indictative of worker reports of distress than the net flow measure. In column (4), when both measures are entered the coefficient on the gross flow indicator is about 16 percentage points higher than the coefficient on the net indicator (35 versus 19), and we can statistically reject their equality (p-value of 0.0255).

Indeed, once we know that according to the gross flow indicator that the firm is in distress, the incremental information in the net flow measure is diminished: the additional probability of a worker report of distress is about 10 percentage points, and the statistical significance is marginal (a p-value of 0.0705).

C.2 Earnings Loss using Gross vs Net Flows Measure

Figure A3 compares the earnings loss outcomes using the net vs gross flows measures.

Table A1. 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 A2. Properties of the SIPP-LEHD Match

	Number of Quarters	SIPP Person Quarters	Positive LEHD Earnings	Matched
Continuers	27	525,854	363,833	234,387
Separate	27	22,694	16,887	6,833
Implied Match Rate				
Continuers			69.2%	44.6%
Separate			74.4%	30.1%

Table A3. 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 A4. 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 (2013b)
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 (2013) [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 A5. 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. (2011)	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 (2013) [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 A6. 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; positive earnings in each calendar year between 1976-1986;
Schoeni and Dardia (1996)	California UI Records (1989:I-1994:III)	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
Lengermann and Vilhuber (2002)	Maryland (1985:II - 1997:II)	full-quarter employment;
Dustmann and Meghir (2005)	German Social Security	oldest worker is 35; "observe from labor force entry onwards"
Abowd, McKinney, and Vilhuber (2009)	LEHD	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; 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; include years with zeros
Andersson et al. (2011)	LEHD	4 quarters of employment prior to separation; no restriction on post-displacement earnings
von Wachter, Handwerker, and Hildreth (2012)	California UI Records (1990-2000)	tenure restriction; no post-displacement earnings restriction
Flaaen, Shapiro and Sorkin (2013) [this paper]	LEHD	25-74 years old; 1 year of tenure; positive earnings in up to 4 calendar years following separation

Table A7. Predicting a Survey Report of Distress From Firm-Level Data

Firm-level distress	(1)	(2)	(3)	(4)
Net Indicator	0.391		0.143	0.192
	(0.024)		(0.038)	(0.053)
Gross Indicator		0.421	0.315	0.351
		(0.023)	(0.037)	(0.048)
Both				-0.094
				(0.076)
Constant	0.164	0.158	0.155	0.154
	(0.006)	(0.006)	(0.006)	(0.006)
Obs.	4,406	4,406	4,406	4,406
R^2	0.085	0.104	0.109	0.110

Source: SIPP-LEHD as explained in text.
This table reports a linear probability model prediction of survey-reports of a distressed separation based from administrative measures of displacement. The sample consists of only those individuals separating from a job. See Table 2 for definitions of the net and gross indicators. Standard errors are clustered at the individual level.

Table A8. Predicting Retirement Using Survey and Administrative Measures

	(1)	(2)	(3)	(4)	(5)	(6)
$\geq 60 \times$ Survey Indicator of Distress		0.145	0.135			
		(0.052)	(0.052)			
$\geq 60 \times$ Net Indicator of Distress				0.292	0.147	
				(0.077)	(0.179)	
$\geq 60 \times$ Gross Indicator of Distress					0.271	0.164
					(0.073)	(0.395)
Survey Indicator of Distress		0.013	0.012			
		(0.004)	(0.004)			
Net Indicator of Distress				0.012	-0.003	
				(0.006)	(0.001)	
Gross Indicator of Distress					0.037	0.018
					(0.009)	(0.062)
Separate to Retire	0.580	0.580				
	(0.023)	(0.023)				
25-34	-0.046	-0.046	-0.056	-0.056	-0.055	-0.055
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
35-44	-0.046	-0.045	-0.056	-0.055	-0.055	-0.055
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
45-54	-0.043	-0.042	-0.052	-0.052	-0.052	-0.051
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
55-59	-0.029	-0.028	-0.035	-0.035	-0.035	-0.034
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
60-74						
(omitted cat.)						
HS or Less	0	0	0	0	0	0
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Some College	0.001	0	0	0	0	0
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
College or more						
(omitted cat.)						
Constant	0.046	0.045	0.056	0.055	0.055	0.055
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
N	181,372	181,372	181,372	181,372	181,372	181,372
R^2	0.130	0.131	0.031	0.033	0.034	0.034

Source: SIPP-LEHD as explained as explained in text.

The outcome variable corresponds to a response of "yes" to a question about whether the respondent is not working because they are retired in the quarter of the separation or any of the following three quarters. The variable "separate to retire" is responding that the reason for the separation is retirement. All indicators are indicators of "distress" and for separating conditional on the characteristic. Standard errors are clustered at the person level.

Table A9. Predicting Social Security Payments Using Survey and Administrative Measures

	(1)	(2)	(3)	(4)	(5)
$\geq 60 \times$ Survey Indicator of Distress		0.077	0.082		
		(0.062)	(0.062)		
$\geq 60 \times$ Net Indicator of Distress				0.082	
				(0.077)	
$\geq 60 \times$ Gross Indicator of Distress					0.061
					(0.071)
Survey Indicator of Distress		-0.001	-0.001		
		(0.001)	(0.001)		
Net Flow Indicator of Distress				-0.002	
				(0.000)	
Gross Flow Indicator of Distress					0
					(0.002)
Separate to Retire	0.252	0.252			
	(0.020)	(0.020)			
25-34	-0.273	-0.277	-0.273	-0.277	-0.277
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
35-44	-0.273	-0.277	-0.272	-0.277	-0.277
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
45-54	-0.272	-0.276	-0.272	-0.276	-0.276
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
55-59	-0.267	-0.269	-0.266	-0.269	-0.269
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
60-74					
(omitted cat.)					
HS or less	0.011	0.011	0.011	0.011	0.011
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Some College	0.007	0.007	0.007	0.007	0.007
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
College or more					
(omitted cat.)					
Constant	0.268	0.272	0.267	0.272	0.272
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
N	181,372	181,372	181,372	181,372	181,372
R^2	0.252	0.245	0.252	0.245	0.245

Source: SIPP-LEHD as explained in the text.

The outcome variable corresponds to a response of "yes" to a question about whether or not the respondent is collecting Social Security in the quarter of the separation or any of the following three quarters. The variable "separate to retire" is responding that the reason for the separation is retirement. All indicators are indicators of "distress" and for separating conditional on the characteristic. Standard errors are clustered at the person level.

Table A10. Predicting Disability Using Survey and Administrative Measures

	(1)	(2)	(3)	(4)	(5)
Survey Indicator of Distress		0.050	0.050		
		(0.011)	(0.011)		
Net Flow Indicator of Distress				0.038	
				(0.014)	
Gross Flow Indicator of Distress					0.040
					(0.014)
Separate due to Injury/Illness	0.774	0.774			
	(0.032)	(0.032)			
25-34	-0.086	-0.086	-0.087	-0.087	-0.087
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
35-44	-0.074	-0.074	-0.075	-0.075	-0.075
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
45-54	-0.049	-0.049	-0.049	-0.049	-0.049
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
55-59	-0.013	-0.013	-0.013	-0.013	-0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
60-74					
(omitted cat.)					
HS or less	0.035	0.035	0.035	0.035	0.035
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Some College	0.032	0.032	0.032	0.032	0.032
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
College or more					
(omitted cat.)					
Constant	0.099	0.099	0.100	0.100	0.100
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
N	181,372	181,372	181,372	181,372	181,372
R^2	0.023	0.024	0.017	0.017	0.017

Source: SIPP-LEHD as explained in text.

The outcome variable corresponds to a response of "yes" to a question about whether the respondent is not working because they are disabled in the quarter of the separation or any of the following three quarters. The variable "separate due to injury/illness" is responding that the reason for the separation is own injury or illness. All indicators are indicators of "distress" and for separating conditional on the characteristic. Standard errors are clustered at the person level.

Table A11. Worker Characteristics

Education	Distress	Net Indicator		
		Yes	No	Share
HS or Less	Yes	116	219	0.35
	No	73	926	0.93
	Agree	0.61	0.81	
Some College	Yes	78	262	0.23
	No	58	1,250	0.96
	Agree	0.57	0.83	
College or More	Yes	49	168	0.23
	No	64	1,143	0.95
	Agree	0.43	0.87	

Age	Distress	Net Indicator		
		Yes	No	Share
25-34	Yes	61	163	0.27
	No	62	1,094	0.95
	Agree	0.50	0.87	
35-44	Yes	72	211	0.25
	No	59	910	0.94
	Agree	0.55	0.81	
45-54	Yes	72	178	0.29
	No	35	693	0.95
	Agree	0.67	0.80	
55-59	Yes	18	58	0.24
	No	20	287	0.93
	Agree	0.47	0.83	
60-74	Yes	20	39	0.34
	No	19	335	0.95
	Agree	0.51	0.90	

Gender	Distress	Net Indicator		
		Yes	No	Share
Male	Yes	125	347	0.26
	No	113	1,562	0.93
	Agree	0.53	0.82	
Female	Yes	118	302	0.28
	No	82	1,757	0.96
	Agree	0.59	0.85	

Source: SIPP-LEHD as explained in text.
See note to Table 4.

Table A12. Industry

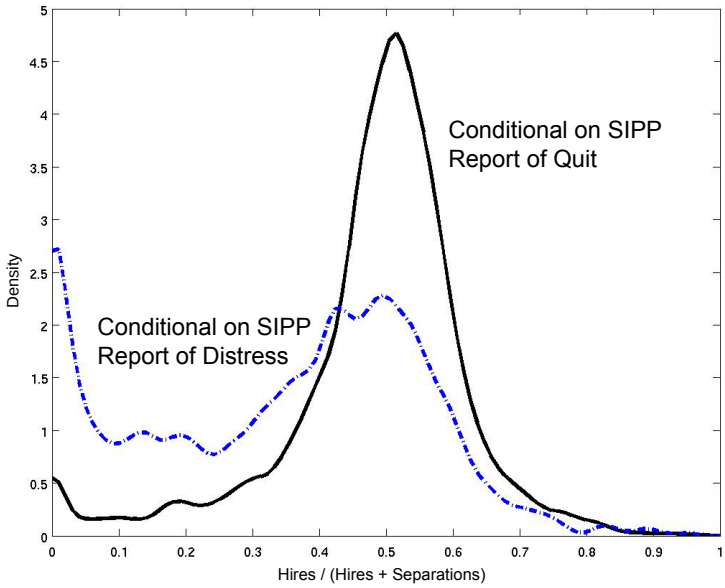
Industry	Distress	Net Indicator			Gross Indicator		
		Yes	No	Share	Yes	No	Share
Construction	Yes	19	52	0.27	16	55	0.23
	No	11	105	0.91	6	110	0.95
	Agree	0.63	0.67		0.73	0.67	
Manufacturing	Yes	80	130	0.38	110	100	0.52
	No	28	393	0.93	58	363	0.86
	Agree	0.74	0.75		0.65	0.78	
Wholesale/retail trade and transport/ warehousing	Yes	45	116	0.28	49	112	0.30
	No	30	611	0.95	29	612	0.95
	Agree	0.60	0.84		0.63	0.85	
Information	Yes	13	34	0.28	14	33	0.30
	No	12	101	0.89	10	103	0.91
	Agree	0.52	0.75		0.58	0.76	
FIRE	Yes	22	60	0.27	20	62	0.24
	No	17	278	0.94	17	278	0.94
	Agree	0.56	0.82		0.54	0.82	
Prof./Tech. Services	Yes	20	66	0.23	22	64	0.26
	No	21	155	0.88	17	159	0.90
	Agree	0.49	0.70		0.56	0.71	
Management of companies/admin/ support	Yes	18	55	0.25	16	57	0.22
	No	15	285	0.95	9	291	0.97
	Agree	0.55	0.84		0.64	0.84	
Health and Education	Yes	13	84	0.13	11	86	0.11
	No	29	881	0.97	23	887	0.97
	Agree	0.31	0.91		0.32	0.91	
Other	Yes	13	52	0.20	13	52	0.20
	No	32	510	0.94	28	514	0.95
	Agree	0.29	0.91		0.32	0.91	

Source: SIPP-LEHD as explained in text.

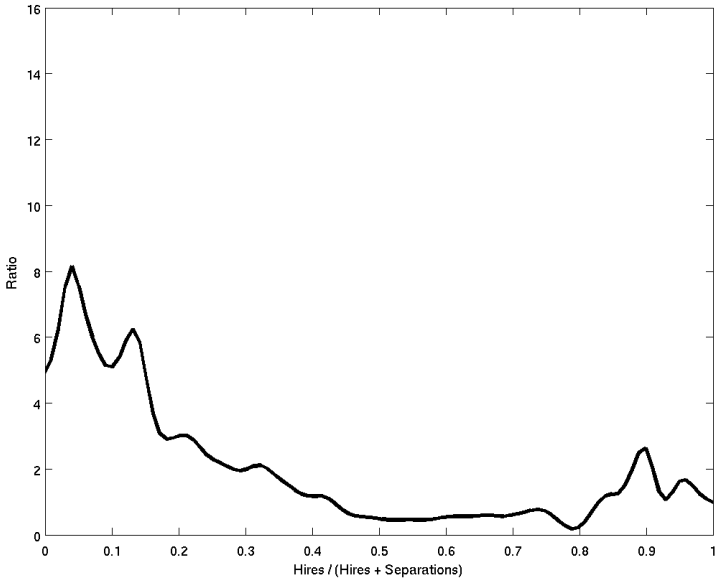
This table reports firm industry characteristics associated with worker-reported separations. The rows and columns corresponding to Yes and No represent counts, whereas the rows and columns corresponding to Agree represent the row and column shares of agreement between the survey and administrative measures. The Net and Gross administrative indicators are defined in Table 2.

Figure A1. Distribution of Gross Flow Measures by Survey Reason for Separation

A. Distribution of Distress vs Quit



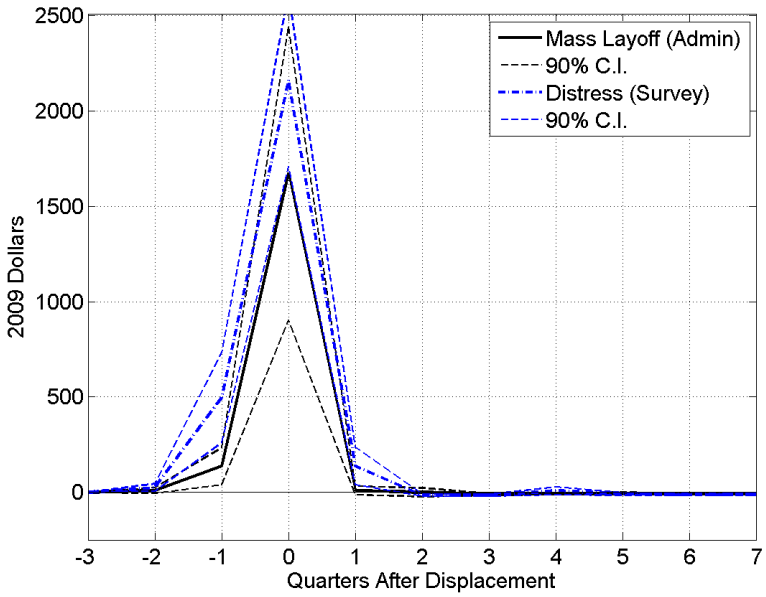
B. Relative Probability of Distress vs Quit



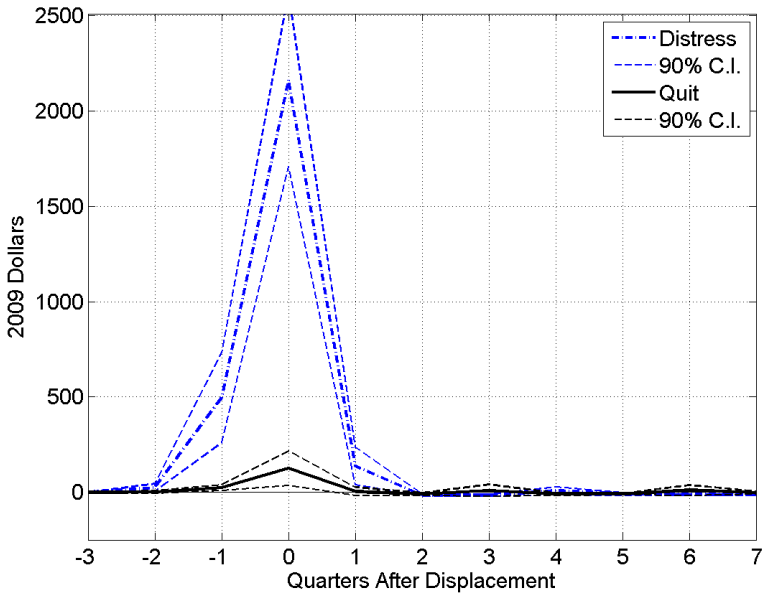
Source: SIPP-LEHD as explained in text.
This figure plots the kernel density estimate of survey-reported quit separations vs survey-repoted distress separations. The top panel plots the estimates based on the ratio of hiring to total worker flows (hires plus separations) at the firm from $t-3$ to $t+1$ surrounding the separation (gross flows), while the bottom panel plots the ratio of the distress and quit pdf shown in the top panel.

Figure A2. Severance Pay

A. Survey versus Administrative Measures



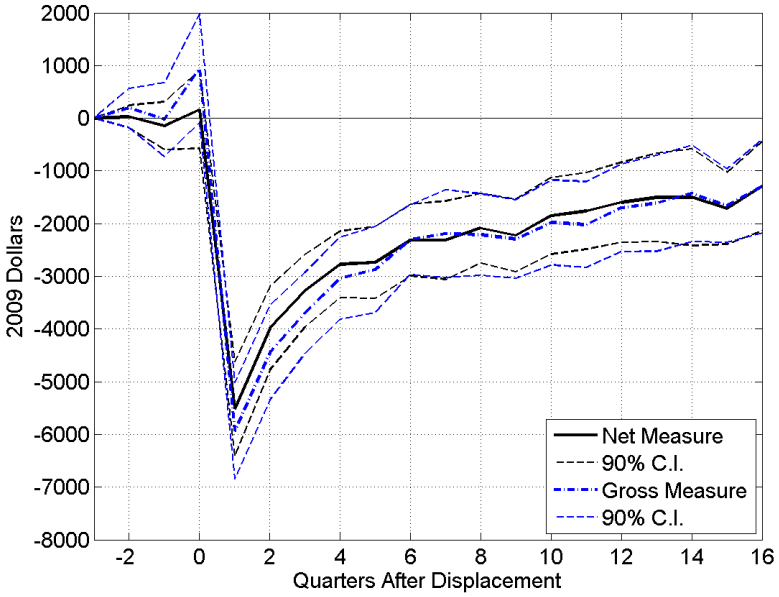
B. Survey Measures: Distress versus Quit



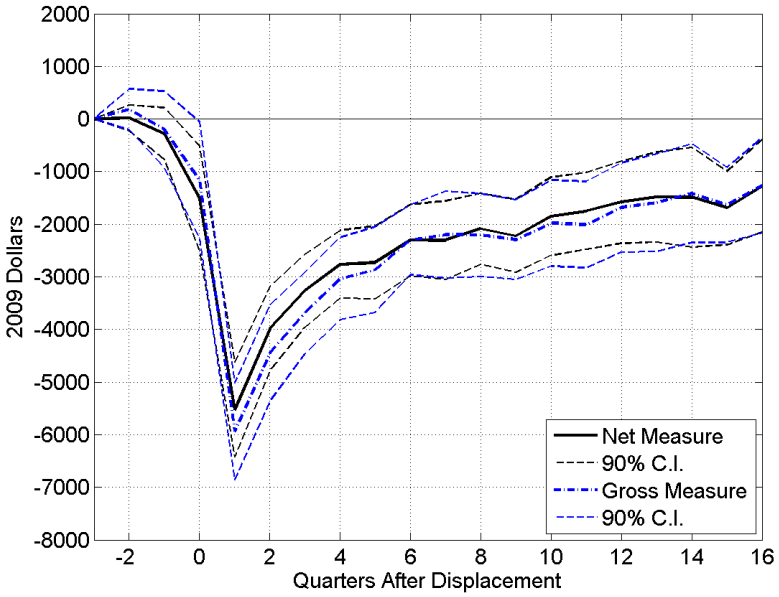
Source: SIPP-LEHD as explained in text.
This figure plots the severance payments of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. The top panel contrasts the (net) administrative measure of displacement with the survey measure – those reporting distress in the SIPP. The bottom panel contrasts the outcomes of those reporting distress with those reporting a quit in the SIPP. The plot is taken from coefficient estimates of a regression including a quartic in age. See equation (2) in the text. Standard errors are clustered at the person level.

Figure A3. Earnings Losses Relative to Non-Separators: Comparing Administrative Measures

A. Earnings Losses

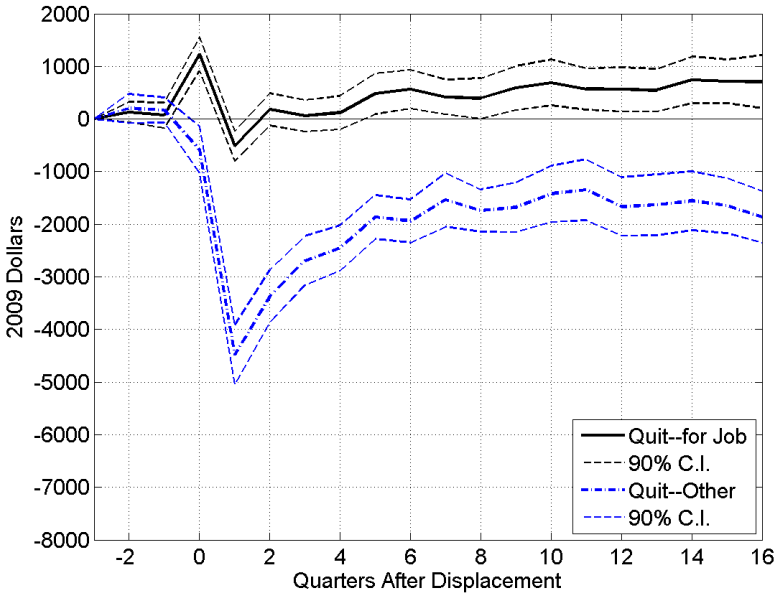


B. Earnings Losses Without Severance Pay



Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. The top panel shows the response to the net and gross flow measures indicators of displacement using administrative data, while the bottom panel removes any severance payments identified by the SIPP. The values are coefficient estimates from a regression including a quartic in age and individual-quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level.

Figure A4. Earnings Losses Relative to Non-Separators: Differentiating Quits



Source: SIPP-LEHD as explained in text.
This figure plots the earnings losses of individuals – according to the identified type of separation – relative to a control group of individuals not separating from a job. The figure compares those identifying quitting for another job to those reporting quitting "for some other reason" or who separated due to "unsatisfactory work arrangements." The values are coefficient estimates from a regression including a quartic in age and individual-quarter fixed effects. See equation (2) in the text. Standard errors are clustered at the person and calendar-year quarter level.