Culling the Herd? Mass Layoff Events and Previous Sickness*

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Abstract

We use Danish administrative data that covers the universe of private-sector firms, workers, and their healthcare transactions and find rich interactions between mass layoff events (MLE) and a comprehensive Mincer-regression-based measure of workers' previous sickness. Firms "cull the herd", selectively laying off previously sick workers during MLEs. Previously sick workers displaced in a MLE suffer very large earnings losses and very large increases in mortality even when we match control workers based on both demographics and previous sickness, and previously-sick workers have a stronger tendency after layoffs to switch into low-wage firms and low-wage occupations after MLEs.

Keywords: Mass Layoff Events, Previous Sickness, Heterogeneous Effects

JEL Classification: J63, I14, C55

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1 Introduction

Layoffs and adverse changes to worker health can have large negative effects on human capital, but previous research treats these two shocks in isolation. We use Danish administrative data that covers the universe of private-sector firms, workers, and their healthcare transactions to study a rich set of interactions between layoffs and shocks to workers' health.

A number of important studies have used mass layoff events (MLEs) to separate quits from involuntary layoffs and to study how these layoffs affect worker outcomes. One branch of this literature shows that MLEs have a pronounced effect on worker earnings, wages and the probability of reattaching to the workforce. For example, Bertheau et al. (2023) show that MLEs in Denmark decrease earnings by 18% one year after layoff. We find similar effect in the first year after a layoff, with earnings rebounding over a decade-long recovery to losses of 3%.

The impact of MLEs is not limited to earnings, and (Sullivan and von Wachter 2009) show that MLE also raise mortality among treated workers. One year after MLEs in Pennsylvania in the 1980's, the average worker has 50%-100% increased annual death hazard. In our data we also find that MLEs increase mortality, with the average workers suffering an immediate increase in mortality probability of 0.2 percentage point (pp) in the year after a MLE and a cumulative mortality probability of $0.45pp$ ten years out.

This opens an interesting question. Was it the layoff itself that led to the increased mortality? Or were the treated workers different on some (previously) unobservable dimension that (a) caused them to be selected into the layoff; and (b) made them more likely to suffer mortality after it? And if so, did this unobserved heterogeneity (c) also contribute to measured earnings losses for these workers after layoffs? An obvious candidate for the unobserved dimension is the previous health of these workers.

In order to answer these questions, we need data that allows us to identify a large number of MLEs and to characterize changes in the health status of all affected workers, before and after layoffs. Ideally, this measure should be based on health conditions that are meaningful to workers' ability to do their jobs but not be mechanically related to worker's job status or earnings, as would be the case when access to health care is mediated by employer-provided health insurance or worker income.

This sickness measure should also capture a comprehensive portrait of sickness over

its many dimensions, as opposed to capturing the incidence of isolated or rarely occurring conditions. Combinations of sickness conditions may affect earnings and mortality but be difficult to identify ex ante, and also may exhibit variation in their intensity before and after more rare events. Finally, the sickness measure should provide continuous variation in sickness intensity across workers and over time. This enables us to study the entire population of workers displaced in MLE, and to find matched control workers for identification.

We merge Danish administrative data that covers the universe of private-sector firms, the universe of private-sector workers, and the universe of the workers' healthcare transactions. Because Denmark has universal healthcare that is virtually free, workers' healthcare transactions are a good representation of their underlying sickness states and free of selection based on employment or earnings (Hummels et al. 2025).

Our healthcare dataset includes every instance in which a patient is prescribed a drug, visits a doctor's office or receives a diagnosis during a hospital visit, all organized according to standard codes. To condense these hundreds of prescription and diagnostic codes into a single Sickness Composite index we use Mincer earnings regressions that capture the correlation between specific codes and earnings in the population of private-sector Danish workers. The coefficient for a particular code is treated as a weight for that diagnosis – a code that is uncorrelated with earnings receives zero weight, while a code that has a large negative correlation with earnings receives much larger weight.

Our Sickness composite is then a weighted average of diagnostic codes for each worker-year, with the values of that index corresponding to the percentage loss (or gain) in earnings relative to the state of full health with no sickness. This index is tightly distributed around zero for most workers but has a long, fat left tail: the sickest 10% have sickness-correlated earnings 8% below the full health state. The sickness composite is also strongly correlated with other objective measures of sickness: the sickest 5% of workers have a 10-year mortality rate 3.1 times higher than the average. Finally, while one-third of the variation in the index is worker-specific, the over-time variation of the sickness composite is dominated by large and temporary changes in its value. This enables us to treat sharp changes in the Sickness Composite as a large and temporary shock to workers' health that pre-dates but possibly interacts with layoffs.

We are particularly interested in whether workers suffering sickness shocks immediately

prior to MLEs are over-selected during layoffs and whether their post-layoff outcomes differ from other workers. Our main sample includes about 300,000 MLE separations between 2002-2018 with detailed information on these separated workers as well as control workers with similar demographics and pre-MLE sickness. We find rich interactions between MLE shocks and workers' previous sickness shocks.

First, firms "cull the herd", i.e. they selectively lay off previously sick workers during MLEs. One year after MLEs, the employment share of the sickest workers decreases by more than half relative to one year before. It is unlikely that these firms know about the workers' interactions with the healthcare system, as access to that system is not mediated by the firms. Rather, it is more likely that sick workers are simply less able to do their jobs, as suggested by the Mincer earnings regressions, and that firms are selecting on the particular dimensions of job performance that are correlated with previous sickness.

Second, previous sickness amplifies the effect of layoffs. Workers who are sick one year before a MLE suffer very large earnings losses and very large increases in mortality, relative to control workers with matched demographics. Ten years after MLEs, the sickest workers experience earnings losses that are 13 times the size of the estimated average MLE effect, and suffer increased mortality that is 16 times the average effect. The effects for both earnings and mortality increase monotonically with previous sickness, and the earnings losses are much larger than one would expect given modest changes in sickness post-layoff. The sharp rise in mortality for the sickest workers after a MLE suggests that our Sickness Composite captures actual sickness states and not, e.g., malingering.

Third, we can match control workers based on both demographics and previous sickness to get a partial effect of MLE on earnings and mortality. When we do that, the earnings and mortality effect of MLE remains large, with the effects being the strongest for the sickest workers. Ten years after MLEs, the earnings loss for the sickest workers are two and a half times the average effect.

We show that an important mechanism for earnings losses is the quality of firms and jobs into which they re-attach post-displacement. Previously-sick workers have a stronger tendency to switch into low-wage firms after MLEs. In addition, earnings losses from switching into low-wage firms is greater for the sickest workers. That is, they appear not only to switch to lower paying firms, but within these firms to take much lower paying jobs. This finding is corroborated with evidence showing that the sickest workers also

switch into lower-wage occupations after MLEs.

Fourth, when we estimate the average MLE effect by explicitly taking into account both previous sickness and selection, we find average treatment effects similar to initial estimates that ignore previous sickness. The reason is that, while MLEs clearly select on sickness, the number of very sick workers is not large enough to dramatically move average treatment effects. However, our results suggest that the average effect may be less interesting than the way in which a previous shock to the workers' health state leads to remarkable heterogeneity in post-layoff effects.

Our work contributes to three distinct literatures at the intersection of layoffs, earnings, and health. The first literature examines the effects of job displacement on earnings. One line of work uses survey data for job displacement (e.g., Topel 1990; Ruhm 1991; Stevens 1997).[1] Another uses administrative data, assuming that job displacements from MLE are mostly layoffs rather than voluntary quits (e.g., Jacobson et al. 1993; Davis and von Wachter 2011).[2]

More recent work has explored heterogeneous MLE effects, including differences across countries (Bertheau et al. 2023), gender (Illing et al. 2024), firms and workers (Athey et al. 2026) and differences across business cycles (Schmieder et al. 2023). Several studies also investigate the mechanism of whether displaced workers sort into lower wage firms (Schmieder et al. 2023; Lachowska et al. 2020).

Relative to this literature, we focus on sickness as a previous shock to workers, show that MLEs impact previously sick workers disproportionately, and that previous sickness substantially amplifies the earnings losses from MLEs. In addition, we show that the mechanism of sorting into lower-wage firms and occupations is more pronounced for previously sick workers.

The second literature studies how MLE affect mortality (e.g., Sullivan and von Wachter 2009; Eliason and Storrie 2009; Browning and Heinesen 2012) or other health outcomes.[3]

[1] Previous studies also examined the effects of displacement on other outcome variables (e.g., Bound et al. 1999; Charles and Stephens 2004).

[2] Flaaen et al. (2019) report empirical evidence consistent with this assumption by merging administrative data with survey data.

[3] Previous studies examined how job displacement affects morbidity, such as hospitalization (e.g., Schaller and Stevens 2015). A more recent study focuses on stayers in mass layoff events and document persistent negative effects on mental and physical health of survivors (Ahammer et al. 2025).

Relative to this literature, we show that, even after we condition on previous sickness, the MLE effect on mortality remains pronounced and increases with the severity of previous sickness. Relative to a demographically matched control group, the sickest 10% of workers are responsible for 80% of excess mortality 10 years after MLE (30% if our control group also matches on previous sickness).

The third literature studies the effect of sickness on earnings. One line of work uses self-reported measures of health (e.g., Charles 2003; Meyer and Mok 2019). Another uses objective measures of health and single-differencing identification (e.g., Dobkin et al. 2018). That is, it compares how changes in health status affect earnings for a given worker, without providing a matched worker control sample. And a final line of work linking sickness to earnings uses double-differencing identification applied to a single objective measure of sickness. Examples include: commuting accidents, Halla and Zweimüller (2013); acute hospitalization, García-Gómez et al. (2013); and hospitalization due to heart attack and stroke, Fadlon and Nielsen (2021).

Relative to this literature, we study a comprehensive list of diagnostic codes aggregated into a labor-market relevant composite using Mincer regressions, and we use double-differencing identification to compare earnings losses after a MLE for a treated worker relative to a control worker with similar sickness levels. An important difference is the incidence of the sickness events studied. While previous authors focused on rare events, affecting between 0.1% and 0.67% of the workforce, we generate continuous variation in sickness states over the entire population. Further, we show that previous sickness magnifies the effect of MLEs even for workers in the bottom 25% of sickness states, with the severity growing as previous sickness becomes more severe.

The paper proceeds as follows. Section 2 describes our data and definition of mass layoff events. Section 3 describes our construction of the sickness composite. Section 4 describes the extent to which mass layoff events select on workers' previous sickness. Section 5 spells out our empirical specification and identification. Section 6 estimates the impact on MLE on earnings and mortality using demographically matched workers, while Section 7 repeats these estimates using workers matched on both demographics and previous sickness and discusses mechanisms of adjustment, in particular the sorting of sick workers into lower paying occupations and lower paying firms. Section 8 calculates the average treatment effect conditional on previous sickness. Section 9 performs robustness

analyses and section 10 concludes.

2 Data and Mass Layoff Events

In this section, we describe our main data sources and how we define mass layoff events. We link a host of registers from Statistics Denmark covering the time period from 1999 to 2019. Our data covers the universe of private-sector firms, the universe of private-sector workers, and the universe of the workers' healthcare transactions.

2.1 Matched Employer-Employee Data

Our data on workers and employers come from the *Integrated Database for Labor Market Research* (IDA). The database records information on workers' labor market outcomes, such as annual labor market earnings, hours worked, wage rates, occupations (according to six-digit DISCO-88 codes), and labor market experience.

The *Population Register* (BEF) provides us with information about basic demographic characteristics such as age, gender and family relationships. In addition, we obtain workers' level of highest completed formal education from the *Education Register* (UDDA). We define three categories of educational level; 1) primary- and secondary education, 2) vocational degrees, and 3) higher education degrees.

Workers are linked to workplaces and firms in week 48 of each year. This link between workers and firms also enables us to study the worker composition at the firm level and worker transitions between firms. To this we add the number of employees and industry code (according to 6-digit DB03 codes) of firms from the *General Firm Statistics* (FIRM).

2.2 Mass Layoff Events

In Denmark, large-scale layoffs are subject to regulations.[4] First, firms are required to enter negotiations with local union representatives before large-scale layoffs, during which the firms must reveal the planned number of affected jobs and their distribution across

[4] Dismissals are considered large-scale if the firm within 30 days dismisses i) 10 employees at workplaces with 20-100 employees, ii) 10% of the employees at workplaces with 100-300 employees, iii) 30 employees or more at workplaces with at least 300 employees.

occupations. The firm must also give the local labor market council advance notifications of at least 30 days before large-scale layoffs.[5] Because of these advanced notification requirements, we focus on worker health shocks occurring a year prior to the layoff itself.

Despite these regulations, employers in the Danish labor market can lay off workers with relative ease overall.[6] This is part of the so-called "flexicurity" model of the Danish labor market, whereby workers receive generous unemployment insurance benefits in return for limited employment protection (see e.g., Kreiner and Svarer 2022).

We follow the mass layoff literature, e.g., Bertheau et al. (2023), and define a worker as displaced in a mass layoff event if the worker separates from firm j in year t, and the following conditions are met: i) Firm j had more than 50 employees in year *t-1*, ii) employment at firm j contracts by more than 30 percent from *t-1* to t, and iii) 3. no more than 20% of the employees in firm j move jointly to a single firm from year *t-1* to year t.

These conditions ensure that the events capture meaningful employment contraction consisting of layoffs rather than quits, and that the events happen in well-established and relatively stable firms (Davis, Faberman, et al. 2006). They also ensure that the discontinuation of firms that we capture in the data reflects firm closure rather than merger or acquisition. We further restrict our analyses to private-sector firms and individuals of working age (18-65) in the year of the mass layoff event. In total, our approach identifies around 300,000 individual mass layoff separations in approximately 3,000 unique firms from 2002 to 2018. We show in Appendix Figure XX that the timing of MLEs, as defined in this manner, corresponds well to the timing of publicly announced layoffs as reported in XXX.

2.3 Healthcare Transactions Data

We observe the *universe* of health care utilization in Denmark, including hospitalizations, doctor visits and prescription drug purchases. The Danish health care system is funded by the government and is virtually free of charge for all residents regardless of employment

[5] The notification is 8 weeks if at least 50% are laid off in firms with at least 100 employees. Bargaining agreements between employer confederations and labor unions may also stipulate different notification requirements.

[6] For example, Bertheau et al. (2023) shows that the incidence of mass layoff events in Denmark is higher than in many other European countries.

status. The universal and free health care system mitigates concerns that access to - and therefore utilization of - health care is conditioned by socioeconomic conditions (Currie and Madrian 1999). We draw data from the following registers, all of which match specific healthcare transactions to specific individual workers in each year.

- The *National Patient Registry* (LPR) covers all in- and outpatient hospitalizations in both private and public hospitals. The register contains all contacts with the hospital system, including diagnoses according to the *International Statistical Classification of Diseases and Related Health Problems, 10th Revision* (ICD-10).

- The *Health Insurance Registry* (SSSI/SYSI) covers all individual interactions with doctors, including the classification of doctors by specialty.

- The *Prescription Drug Database* (LMDB) contains information on all prescribed drugs according to the Anatomical Therapeutic Chemical (ATC) codes.

- The *Sickness Benefits Register* (SGDP) contains information on the number of sick days (as reported by the firm), where either the employer or the worker directly is compensated with sickness benefits for absence from work. The register splits the days absent into regular sick days and days absent due to workplace injuries. In principle, the number of observed sick days corresponds to days exceeding a threshold because below this threshold, sickness benefits are not paid out by the government. .[7] In practice, however, the register records spells below the threshold as well

- The *Death Register* (DOD) contains records of all individual deaths including the date of death, which we use to construct mortality rates.

2.4 Control Workers and Summary Statistics

Our principle tool for analysis will be event studies with double-differencing identification, where we match MLE workers to similar control workers. We use the following two approaches to select control workers.

[7] The threshold for payout of sickness benefits has been raised during the sample window. Before 2007: 14 days, 2008: 15 days, 2009-2011: 21 days, After 2012: 30 days. We explain in section 3 how we deal with this feature of the data.

The first approach is based on demographic exact-matching, where the set of demographic characteristics includes calendar year, age (5-year groups), gender, industry (2-digit) and occupation (3-digit). That is, for each worker who separates in a MLE, we find a random control worker who did not experience a MLE in the event-year within the same year-age-gender-industry-occupation cell. Our estimates using demographic exact-matching capture the combined effects of MLEs and previous sickness shocks on earnings and mortality.

Table 1 compares these control workers (Column (1), "*Control*") to treated workers (Column (2), "*MLE, all*") on a variety of outcomes in year prior to the MLE, $t = -1$. The table shows that "MLE" and "Control" workers are balanced on a long list of outcomes, including the ones not targeted by the matching procedure, such as earnings and hours worked (see the "*Employment*" panel) and marital and children status (see the "*Demographics*" panel). Like in previous studies, this balancing suggests that mass layoffs occur quasi-randomly to workers of similar demographic characteristics as well as pre-MLE labor-market characteristics.

Table 1 also shows that the average displaced worker in our sample is healthy: all the prescription drugs listed in Table 1 have prevalence at or below 5%, all the hospitalization diagnoses have prevalence below 0.5%, and the mean number of sick days, 8.6, is comparable to the background population.[8] The control workers, on the other hand, have similar values of these sickness variables.

Demographic exact-matching is consistent with existing approaches in the literature, and achieves balancing, on average, for displaced workers. However, we observe an additional source of worker heterogeneity, that some displaced workers experience substantial sickness shocks in the year prior to the layoff. In the second approach, we choose the control workers by matching on the same set of demographics characteristics as before and also matching on the sickness composite at $t = -1$.[9] Our demographics matching remains exact, while our previous-sickness matching is nearest distance because sickness

[8] The average amount of sickdays are 8-10 per year among all full-time workers according to Statistics Denmark: `https://www.dst.dk/da/Statistik/emner/arbejde-og-indkomst/fravaer-og-arbejdskonflikter/fravaer-fra-arbejde`.

[9] An alternative to matching on sickness composite is to match individually on each of the full set of specific sickness variables. Because there are several hundred such variables, finding worker matches is infeasible even with our large sample.

composite is a continuous variable. We call this approach demographics-plus-previous-sickness matching. Estimates using this matching approach identify the partial effect of MLEs on earnings and mortality, conditional on previous sickness.

We next describe how we aggregate individual sickness conditions into a single sickness composite measure for each worker.

Table 1: Descriptive Statistics

	Control	MLE					
		All	Sick (P50)	Sick (P25)	Sick (P10)	Sick (P5)	Sick (P1)
Demographics							
Age	35.987	36.041	37.653	39.445	39.894	40.681	42.007
	(12.595)	(12.469)	(12.408)	(12.206)	(11.766)	(11.446)	(10.572)
Female (Dummy)	0.418	0.418	0.396	0.459	0.469	0.476	0.517
	(0.493)	(0.493)	(0.489)	(0.498)	(0.499)	(0.499)	(0.500)
Kids <Age 6 (Dummy)	0.264	0.244	0.256	0.252	0.242	0.237	0.239
	(0.591)	(0.574)	(0.586)	(0.578)	(0.567)	(0.566)	(0.570)
Married (Dummy)	0.421	0.392	0.431	0.459	0.447	0.453	0.495
	(0.494)	(0.488)	(0.495)	(0.498)	(0.497)	(0.498)	(0.500)
Employment							
Earnings (DKK)	320708.7	304174.2	303131.5	290222.3	265089.5	245359.6	191824.0
	(357887.6)	(296581.1)	(221859.3)	(195025.6)	(167152.1)	(160542.9)	(159954.4)
Hours Worked (Yearly)	1336.9	1193.6	1212.7	1197.3	1114.7	1031.9	807.2
	(662.7)	(702.7)	(683.2)	(671.9)	(672.1)	(665.5)	(653.3)
Wages (DKK/Hour)	214.6	220.0	224.4	223.3	219.9	218.7	208.8
	(151.9)	(173.9)	(172.2)	(170.3)	(171.2)	(173.9)	(162.5)
Firm Tenure (Years)	3.833	3.130	3.296	3.440	3.323	3.293	3.983
	(5.622)	(5.157)	(5.336)	(5.499)	(5.421)	(5.392)	(5.665)
Labor Market Experience (Years)	13.756	12.809	14.146	15.208	15.138	15.379	16.065
	(11.303)	(11.111)	(11.129)	(11.089)	(10.703)	(10.469)	(9.889)
Union Membership (Dummy)	0.669	0.682	0.727	0.763	0.778	0.796	0.835
	(0.471)	(0.466)	(0.445)	(0.425)	(0.416)	(0.403)	(0.371)
Health							
Sick days (#)	6.532	8.622	16.967	33.344	73.932	126.772	267.884
	(30.603)	(36.336)	(50.020)	(66.829)	(89.966)	(98.308)	(78.765)
Anti-Depressants (%)	4.081	5.022	9.847	19.407	35.846	36.201	45.288
	(19.784)	(21.839)	(29.796)	(39.549)	(47.956)	(48.060)	(49.786)
Sleep Drugs (%)	2.171	2.544	3.659	5.934	9.557	12.194	17.698
	(14.572)	(15.747)	(18.774)	(23.626)	(29.400)	(32.723)	(38.172)
Stroke Drugs (%)	1.882	1.913	3.008	4.912	5.881	7.079	9.245
	(13.590)	(13.698)	(17.081)	(21.612)	(23.527)	(25.649)	(28.971)
Circulatory Diagnoses (‰)	4.928	4.984	6.426	10.935	16.438	23.741	30.216
	(70.029)	(70.423)	(79.906)	(103.999)	(127.154)	(152.247)	(171.211)
Diabetes Diagnoses (‰)	2.628	3.099	5.910	11.252	13.344	13.957	14.388
	(51.199)	(55.586)	(76.648)	(105.477)	(114.747)	(117.316)	(119.107)
Digestion Diagnoses (‰)	2.785	3.644	6.053	10.029	12.985	15.827	21.583
	(62.517)	(73.034)	(94.567)	(123.033)	(139.667)	(153.272)	(180.664)
Doctor Visits (#)	11.476	12.061	14.544	21.138	27.599	33.416	43.940
	(17.082)	(17.860)	(18.749)	(22.939)	(29.769)	(35.582)	(47.750)
10-year mortality rates (%)	2.186	2.591	3.342	4.607	6.237	8.043	9.856
	(14.622)	(15.887)	(17.972)	(20.964)	(24.183)	(27.197)	(29.813)
Sickness Composite (log-points)	-0.013	-0.017	-0.050	-0.093	-0.172	-0.251	-0.459
	(0.060)	(0.069)	(0.082)	(0.100)	(0.118)	(0.124)	(0.096)
Mass Layoff Event							
Calendar year	2007.406	2007.406	2007.605	2007.826	2007.872	2007.825	2006.916
	(5.154)	(5.154)	(5.055)	(4.958)	(4.757)	(4.626)	(4.228)
Observations	286,505	286,505	139,428	69,500	27,802	13,900	2,780

Notes: Control workers (Column (1)) are exact-matched one-to-one to treated workers (Column (2)) on calendar year, age (5-year groups), gender, industry (2-digit) and occupation (3-digit).

3 Measuring Previous Sickness

A main advantage of our data is that we can use the detailed information in the health registers to assess the workers' sickness prior to mass layoff events. Since we observe hundreds of prescription drugs and hospitalization diagnoses, the aim of this section is to construct a single Sickness Composite measure that aggregates these individual measures of sickness. We then provide validation tests of this index and describe its cross-section and over-time variation.

The desirable features of our Sickness Composite are as follows. One, it is based on sickness conditions that may adversely affect worker's ability to do their jobs. Two, it captures a comprehensive portrait of sickness over its many dimensions. Three, it provides continuous variation across workers and over time. This variation is useful for understanding the evolution of worker's sickness states and for identifying control workers along a continuum of sickness states, which is essential for using diff-in-diff identification of the earnings and mortality effects of layoffs.

3.1 Sickness in the Literature

In this sub-section, we briefly review some common approaches to measuring sickness in the literature, and clarify why they do not have all the key features necessary to study interactions with MLEs.

A number of studies focus on a single discrete event and a diff-in-diff design to study the impact of worker sickness on earnings. Examples include: commuting accidents, Halla and Zweimüller (2013); acute hospitalization, García-Gómez et al. (2013); and hospitalization due to heart attack and stroke, Fadlon and Nielsen (2021). These events are relatively rare – the examples above have a prevalence ranging from 0.1 to 0.67% of their samples.

If we study MLEs using a similarly rare sickness condition, our sample size will be very small because of low prevalence (see section 9). In addition, the discrete events associated with a single sickness condition are unlikely to fully represent the continuum of worker sickness. They omit additional sickness conditions, as well as related sickness conditions that pre-date the rare event or that represent variability in the lasting consequences from it. To illustrate these points, we provide in the robustness section an analysis that uses hospitalization from heart attacks and strokes as an alternative to our Sickness Composite.

Our Sickness Composite captures the intensity of adverse health both across workers and over time. This has some similarities to the long tradition in the medical literature of using comorbidity indices to capture the impact of multiple risk factors on morbidity. For example, the Charlson comorbidity index predicts the mortality of patients from 17 different conditions including heart disease, cancer and AIDS. The Elixhauser comorbidity index uses 30 diseases and its Comorbidity–polypharmacy score includes drugs as well as hospital diagnoses in the index. More recent approaches such as Zeltzer et al. (2023) use machine learning to predict mortality from a long list of potential predictors.

These approaches are conceptually useful but they are custom-built to explain mortality, and so often focus on sub-samples of the population.[10] We are interested in how sickness states affect workers' productivity, the likelihood that firms select on these characteristics when making layoffs, and ultimately their earnings and mortality after layoffs. As a result, we focus on how a comprehensive set of sickness conditions affects the earnings of the Danish private-sector working population as revealed using a Mincer human capital approach.

3.2 Sickness Composite and Its Cross-Section Variation

We construct our sickness composite measure in the following steps. First, we start with a sample of all full-time workers with positive labor market earnings who have not yet experienced a mass layoff event. That is, we exclude workers in the years after they experience a mass layoff event. We utilize all available years in our sample and end up with over 38 million observations.

We then regress log earnings on demographic/mincer-style control characteristics and a vector of 319 separate health care transactions. The controls include age (squared), gender, labor market experience (squared), educational level indicators, and calendar-year fixed effects. The health transactions include dummies for 2-digit ICD-10 diagnosis codes, dummies for 2nd level of ATC drug codes (3-digit), number of visits to primary care physicians, specialist doctors and other medical providers, number of days absent due to sickness and workplace injuries.

Some health care transactions exhibit high degrees of collinearity, so we use LASSO

[10] e.g. The studies of the Elixhauser index typically use samples of hospital patients.

to select 224 out of the original 319 health variables.[11] The resulting Mincer regression coefficients represent *correlations* with earnings and each individual coefficient is conditioned on inclusion of all the other health variables as given. With that in mind, we find plausible estimated coefficients.[12] For example, taking anti-depressants (ATC group N06) is associated with a reduction in annual labor market earnings of 0.072 log points. Hospitalization with a diagnosis of liver disease, including alcoholic liver disease (ICD-10 group K70-K77), is associated with a reduction in annual labor market earnings of 0.046 log points. Each additional sick day and general practitioner visit corresponds to a reduction of annual earnings of 0.001 log points.

Of the 224 measures we use in the index, the only one that does not represent a direct interaction with the health care system is sick days, reported by the firm.[13] This is directly observable by the firm, and captures the intensity of other adverse health conditions. One might be concerned that this captures something other than sickness, such as malingering and absenteeism. That might be particularly acute in the period right before layoffs if workers are able to anticipate the layoffs a full year in advance. However, as Appendix Table A.1 shows, sick days are strongly correlated with a long list of actual interactions with the healthcare system as well as 10-year mortality rates.

In the final step, we utilize the Mincer regression coefficients as weights to aggregate all of the selected sickness variables into a single sickness composite measure that varies by individual workers over time. We do this by predicting log earnings based solely on the selected sickness variables and using this fitted value as our sickness composite.

Table 1 shows that the sickness composite takes a value of -0.017 for the average displaced worker, implying that the worker's earnings are 1.7% below the state of full health with no sickness. The average demographically exact-matched control worker

[11] We select variables using the Bayesian Information Criterion (BIC) function as suggested by Zhang et al. (2010). The full list of pre- and post-selection variables is available upon request.

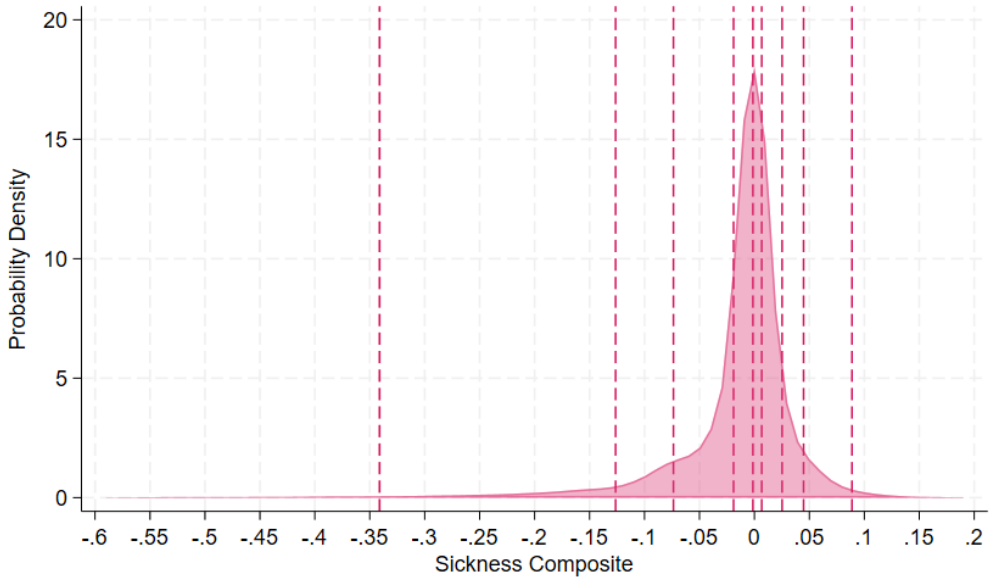
[12] Some sickness variables have positive correlation with log earnings, just as in the medical literature, where some sickness variables have positive correlation with patient survival. e.g. van Walraven et al. (2009), and Sharma et al. (2021).

[13] In section 2, we noted that firms are meant to register sick days so that they can capture payouts for sickdays above some threshold level. The challenge is that firms in practice report many sick spells below this threshold level, and the threshold changed several times during our sample. To address this we have experimented with an alternative specification where we interact the number of sick days with the sick-days reporting regime dummies in the Mincer regression. The resulting sickness composite has the correlation coefficient of 0.96 with our benchmark sickness composite.

has a sickness composite of -0.013. That is, even without using sickness information, demographic exact-matching comes close to capturing the average sickness composite of our displaced workers. But it is the heterogeneity that interests us.

Figure 1 highlights the dispersion of the sickness composite for the MLE workers at $t = -1$, one year before their layoffs. The composite is narrowly distributed around 0 for most workers (the inter-quartile range runs from -0.02 to 0.01), but the distribution has a long fat left tail.[14] In our analyses, we characterize the extent of sickness by referring to workers in the bottom 50/25/10/5/1 percentile of the distribution of the sickness composite measure.

Figure 1: Sickness Composite: Cross-Section Variation



Notes: From the left to the right, the dashed vertical lines represent the sickness-composite percentiles of P1, P5, P10, etc., for the sample of MLE workers at $t = -1$. The cutoff values for P1 through P25 are, respectively, -0.34, -0.13, -0.07, and -0.02.

The remaining columns of Table 1 show an array of descriptive statistics in the year prior to displacement for the mass layoff workers who are sicker than the median: Column (3) shows the workers in the bottom half of the sickness composite distribution (below P50), Column (4) shows those below P25, and so on. These columns show that the sicker workers are a little older, more likely to be female, work fewer hours and have lower earnings than the average displaced worker.

[14] This skewness in the sickness composite tells us that our measure is better at capturing who is very sick than capturing who is very healthy.

Looking at the health panel of this table, we also see that prevalence of prescription drugs, hospitalizations, doctor visits, and sick days all exhibit sharp monotonic increases as we move into lower sickness-composite percentiles. For example, the workers in the sickest quartile (P25) take nearly 4 times as many sick days and are 4 times more likely to take anti-depressants compared to the average displaced worker. Those in the sickest decile (P10) have 9 times more sick days and are 7 times more likely to take anti-depressants.

These healthcare transactions weighted by their correlation with current earnings are inputs into our sickness composite, but long-term mortality is not. Despite that, low values of the sickness composite also correspond to low survival rates. Ten-year mortality rates are 2.6% for the average displaced worker, but over twice that for the sickest decile, and nearly four times that for the sickest percentile (P1). This serves as a useful validation that the Sickness Composite successfully captures workers' underlying sickness states.

The Sickness Composite aggregates all the sickness information in our data for a given worker-year into a single metric that can be interpreted as earnings losses relative to full health. For example, its mean for the sickest decile (P10) is -0.17 log points, or a 15.6% earnings reduction compared to full health, and -0.46 log points for the sickest percentile (P1), or a 37% earnings reduction. These values, and their correlation with mortality and other objective measures of sickness shown in Table 1, suggest that the Sickness Composite captures not just the *presence* of ill-health, but also its *intensity* across the continuum of workers.

3.3 The Over-time Variation of the Sickness Composite

In this sub-section, we provide additional details on how the Sickness Composite varies across workers and over time. We use the sample employed to construct the Sickness Composite, the whole population of private-sector Danish workers, unless otherwise stated.

First we regress the Sickness Composite on worker fixed effects. The $R^2 = 0.32$, indicating that while there are significant differences in the level of sickness across individual workers, two-thirds of the variation in the sickness composite is over time for each worker.

Second, we investigate the persistence of S_{it}, the sickness composite of worker i in year t, by estimating an $AR(1)$ model of $S_{it} = \alpha + \beta S_{it-1} + \epsilon_{it}$, where β is the $AR(1)$

persistence parameter and ϵ_{it} is the error term.[15] We obtain $\hat{\beta} = 0.43$ without worker fixed effects, which suggests that the total persistence of S_{it}, both across-i and across-t, is low. With worker fixed effects, $\hat{\beta}$ drops substantially to 0.13. This result shows that S_{it} has little persistence over time for individual workers, i.e., the over-time variation of S_{it} is dominated by large and temporary swings in its value, which likely represent temporary sickness shocks.

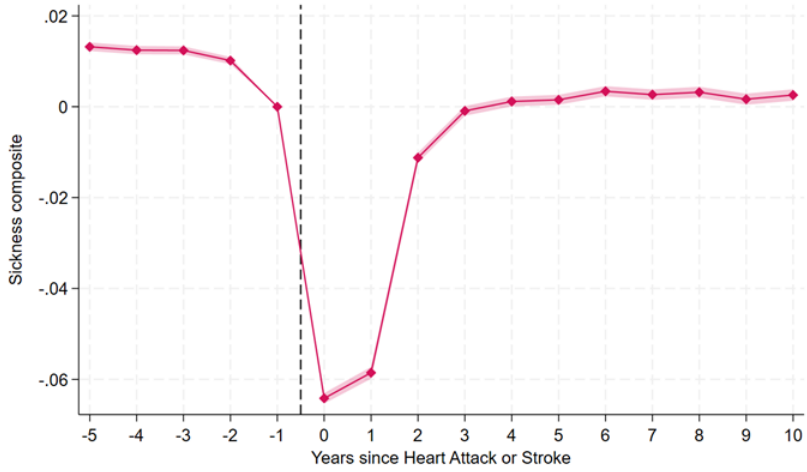
In order to further illustrate this point, we study how S_{it} moves in the years before and after hospitalizations due to heart attacks or strokes (HHAS), a sickness shock commonly used in previous studies. Specifically, we estimate

$$S_{it} = \beta_1 E_i + \sum_k \beta_{0k} \mathbf{1}_{\{t=e+k\}} + \sum_{k \neq -2} \beta_{1k} MLE_i \mathbf{1}_{\{t=e+k\}} + \varepsilon_{it}, \tag{1}$$

where E_i is the dummy for worker i, $\mathbf{1}_{\{t=e+k\}}$ are event-time dummies, and β_{1k} are the coefficients of interest. In Figure 2 we plot the values of these coefficients for the roughly 100,000 private-sector Danish workers who experienced HHAS in our sample. We see that these workers' sickness composite begins to drop a year before the HHAS event, consistent with our conjecture that severe and discrete health events are detectable in the data before they occur. It then drops precipitously in the year of HHAS, and stays low for one more year. It then substantially recovers to a level that is equal to the Sickness Composite one year before the event, and slightly (.015 or 1.5% lower predicted earnings) below $t = -5$ levels. This result provides a useful validation that the over-time variation of the sickness composite successfully captures sickness shocks whose severity is temporary.

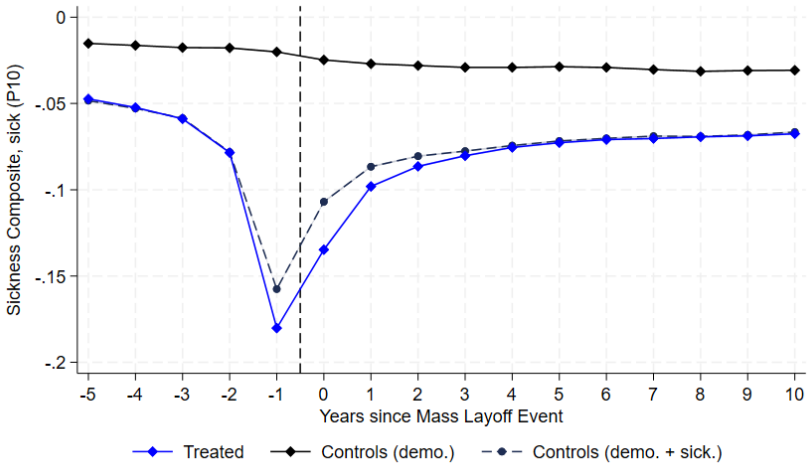
[15] Recall that $S_{it} = \widehat{ln(e)}$, the log earnings loss associated with sickness conditions. We can then think of the evolution of these losses as following $e_{it} = \alpha e_{i,t-1}^{\beta} \epsilon_{it}$.

Figure 2: Sickness Composite for HHAS Workers



Next, we illustrate the over-time variation of S_{it} for the displaced workers around mass layoff events by focusing on those whose sickness composite is in P10, or the lowest decile, in $t = -1$, one year before the mass layoffs. We do so by estimating equation (1), but where the event is the MLE, not a specific sickness event. In Figure 3 we plot the β_{1k} estimates for the P10 MLE workers (solid blue line), for their control workers based on demographics exact-matching (solid black line), and for their controls based on demographics plus previous sickness matching (dashed black line).[16]

Figure 3: Sickness Composite for MLE Workers with Previous Sickness



We learn several important things from this figure. Focusing on the the P10 workers

[16] Results using other percentiles of the sickness composite are similar.

affected by the MLE, their sickness is worse than full health (with $S_{it} = 0$) in every period, consistent with our earlier results that the sickness composite varies substantially across workers. The movement of these workers' Sickness Composite shows a similar pattern to workers affected by hospitalization from heart attacks and strokes from Figure 2: it begins to decline before dropping sharply in $t = -1$, the year of the sickness shock. It then substantially recovers to a level equal to the composite one year before the sickness shock, but below the $t = -5$ levels.

Put another way, the P10 Sickness Composite dummy represents a sharp shock to sickness occurring one year before the MLE, and whose severity is temporary. Recall that the Sickness Composite does not use any workers affected by an MLE to create the index weights; the recovery in the Sickness Composite comes entirely from changes in a worker's access to the healthcare system before and after the MLE.

We can compare this pattern to the Sickness Composite for the demographically matched control workers, which shows a steady downward drift, consistent with aging, but no corresponding drop and recovery around either the sickness event at $t = -1$ or the MLE event at $t = 0$. This means that studies that use demographically matched control workers but ignore sickness shocks prior to mass layoff events will miss the profound difference in expected earnings that we would expect to occur in the short run, solely from the large swings in the Sickness Composite.

Finally, we see a different story when we examine the Sickness Composite for the workers matched on demographics and on sickness in period $t = -1$ who did not experience an MLE layoff. While the matched drop in the sickness composite at $t = -1$ is by construction, the matches in other years are more remarkable, including the pattern of decline and recovery in the periods before and after the MLE. This provides additional evidence that large swings in the sickness composite represent large sickness shocks whose severity is temporary. It also suggests that the direct impact of $t = -1$ sickness shocks on earnings should be similar for our treated and control workers, so that any remaining variation in outcomes is attributable to the mass layoff event itself.

In summary, we have shown that while workers differ in their level of sickness, the overtime variation of the Sickness Composite is dominated by large and temporary swings in its value. This holds for the population of private-sector Danish workers, and is not unique to specific events or specific worker sub-sample. Therefore, we can make use of

heterogeneity in the sickness composite in $t = -1$ to study how temporary sickness shocks one year before mass layoffs interact with those layoffs.

4 Results: Selection on Previous Sickness

We begin our analysis by asking whether firms"cull the herd", that is, select on worker health when making layoffs. It is important to note that firms do not directly observe worker's interactions with the health care system. But they would observe if employees miss work, or if underlying poor health contributed to poor performance on the job. This might be especially acute if workers suffer a pronounced decline in health and performance immediately before layoff decisions are made, as is suggested by Figure 3.

First, we can simply tabulate the mean value of the sickness composite based on $t = -1$ values for leavers and stayers with the same firm. The mean value of sickness composite is -0.017 for the leavers, and -0.009 for the stayers.

Second, in Table 2, we use the sickness composite to see if workers from certain sickness groups are overrepresented in the set of workers laid off. The first column describes the worker sickness bins, P1 and below, P1 to P5 and so on. The second column displays their employment shares in $t - 1$, which simply reflects the definition of the percentiles. The third column displays the normalized stayer share, equal to 100 times the employment share of that sickness bin at $t + 1$ divided by the share of that bin at $t - 1$. If a sickness bin has the same share of overall employment before and after the layoff it has a value of 100; a value below 100 means that the workers in this bin have been selectively laid off.

Table 2: Strength of Selection by Previous Sickness

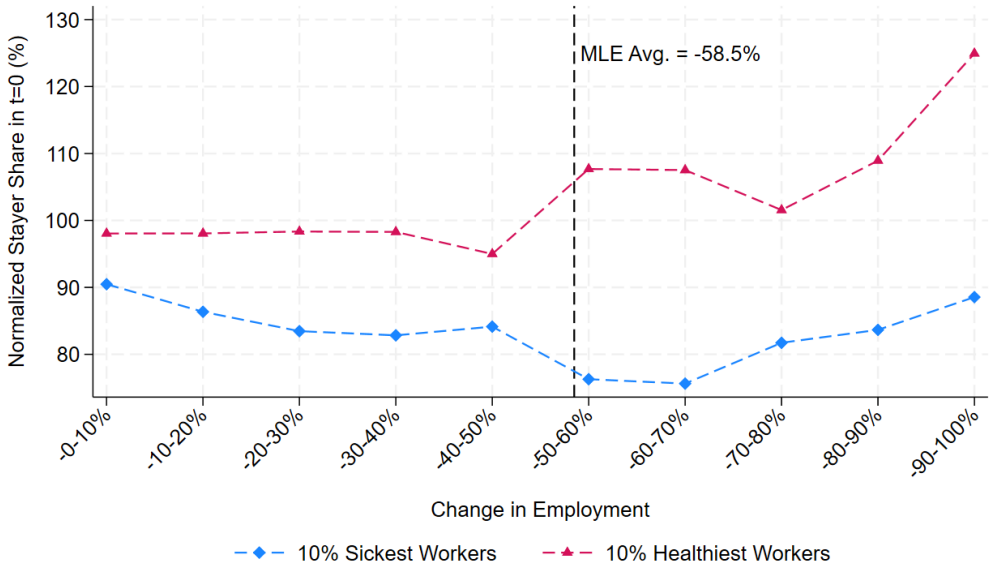
Percentile	Employment share (%), t=-1	Normalized stayer share (%), t=+1
P1	1.0	35.2
P1-P5	4.0	65.7
P5-P10	5.0	87.2
P10-P25	15.0	101.8
P25-P50	25.0	104.2
P50-P75	25.0	108.9

Across our sample of 300,000 laid off workers, we see that the sickest workers are selectively laid off. For example, workers in the sickest percentile are about one-third

of their previous share of employment. Meanwhile, workers above P10 increase their employment shares after layoffs.

Third, the extent of selection may depend on how much of the workforce is laid off. Figure 4 investigates this by focusing on normalized stayer shares for the first decile (most sick) and tenth decile (least sick). We plot normalized stayer shares for all firms in the Danish economy who reduced employment over any one-year period, grouping them along the horizontal axis based on the percentage change in employment. We see that firms who reduce employment by a small amount see relatively weak selection on health, but this grows as layoffs get larger.

Figure 4: Normalized Stayer Share by Change in Employment



Our mass layoff sample requires firms to drop employment by at least 30%, and the average layoff in this sample involves a 58.5% reduction in total employment in that firm. In this range of layoff sizes selection becomes particularly pronounced, with stayer shares for the sickest decile dropping below 80% and stayer shares rising sharply for the healthiest decile. Of course, a plant closure that laid off all workers would result in no selection, but it is notable that the normalized stayer shares – particularly for the healthiest workers – exhibit selection even for changes in employment greater than 80%.

Finally, one might wonder whether firms that engage in mass layoffs have different sickness characteristics than other firms. That is, does the underlying sickness of its workers make firms more likely to engage in mass layoffs?

To evaluate this, we calculated the cutoff values for percentiles of the Sickness Composite in two samples: all Danish firms, and in year $t-1$ for the firms who engage in mass layoffs. If firms who engage in mass layoffs had sicker workers we would see lower cutoff values, i.e. a P10 worker within that firm would have a worse sickness composite on average than a P10 worker in the economy as a whole. That is not the case. The distribution of worker sickness is nearly identical in both samples. It is only *after* layoffs that MLE firms have a different, and better, health distribution than the economy as a whole. See Appendix Table A.2 for details.

5 Empirical Specification

In this section, we present our empirical specification and identification strategy for studying the interaction of mass layoff events and previous sickness. We follow the MLE literature and study the difference-in-differences in outcomes Y between MLE displaced workers ($MLE = 1$) and their matches ($MLE = 0$) around MLEs, indexed to one year before the MLE:

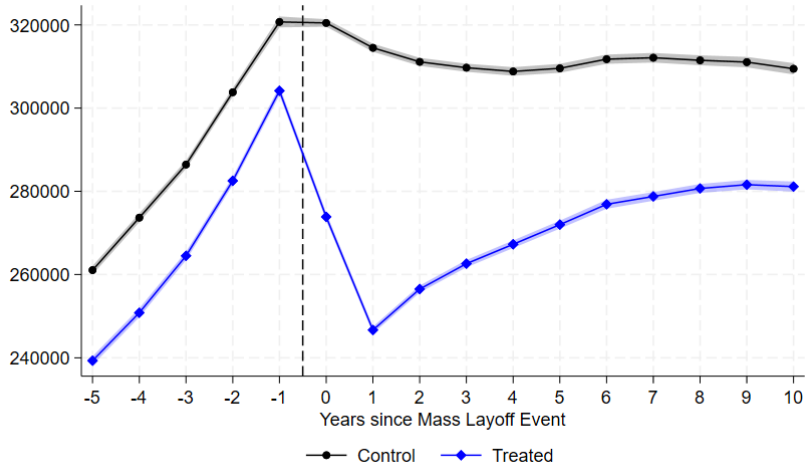
$$Y_{imt} = \alpha_m + \beta_1 MLE_i + \sum_k \beta_{0k} \mathbf{1}_{\{t=e+k\}} + \sum_{k \neq -2} \beta_{1k} MLE_i \mathbf{1}_{\{t=e+k\}} + \varepsilon_{it}, \tag{2}$$

where α_m refers to match-fixed-effects, $\mathbf{1}_{\{t=e+k\}}$ are event-time dummies, and β_{1k} is our coefficients of interest, identifying the causal effects of MLEs under the parallel trends assumption. We estimate Equation (2) with OLS and cluster standard errors at the match-level.

Figure 5 illustrates our empirical strategy. On the vertical axis, we show labor market earnings in DKK ($\approx$ 0.146 USD; 2015-level), and on the horizontal axis, we show years relative to the mass layoff event (year 0). The horizontal axis shows the event year, relative to the MLE; e.g. $t = -1$ means one year before the MLE, and so on. The bright-blue line plots the average earnings for treated workers ($MLE = 1$) around the mass layoff event, while the black line plots the corresponding average earnings for control workers ($MLE = 0$). Treated workers exhibit substantial earnings growth in the years before the displacement, and then a substantial loss of earnings of around 60,000 DKK ($\approx$ 9,000 USD; 2015-level) from year -1 to 1. This corresponds to the first difference in our difference-in-differences approach: differencing the outcome for treated workers relative

to year $t = -1$.

Figure 5: Raw Data Plot and Diff-in-Diff



The control workers track the earnings growth of treated workers well before the MLE, further supporting their validity. The control group exhibits a gradual reduction in earnings in the years after the MLE, but far less than the treated workers.[17] The second differencing in our identification compares treated vs. control workers; i.e. our estimates show the earnings losses that treated workers suffer above and beyond the earnings losses of the control workers. Without the second differencing, we would exaggerate the earnings losses from MLE.

6 The Combined Effects of Previous Sickness and Mass Layoff

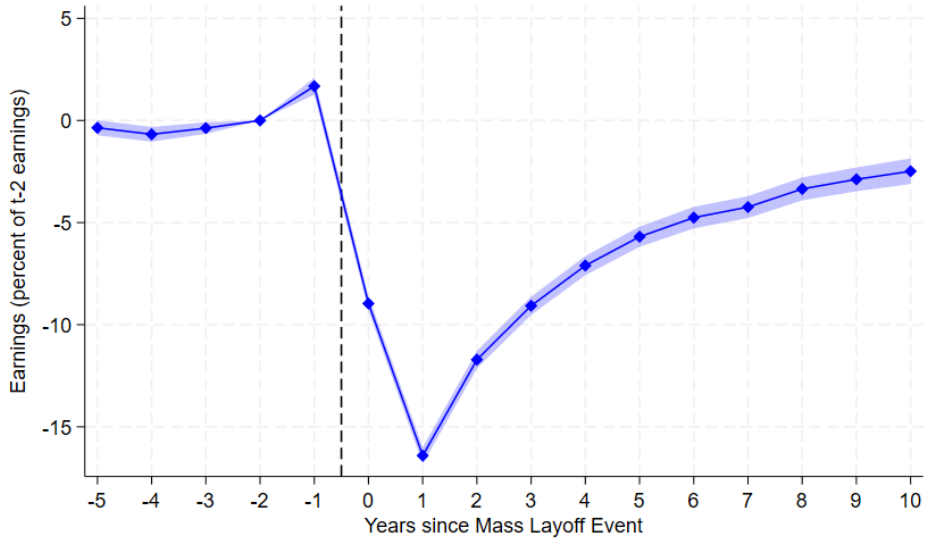
We start by estimating equation (2), ignoring the previous sickness of both treated and control workers, and report our results in Figure 6. The vertical axis shows the treated workers' earnings in year t as percentages of their earnings in year $t = -1$ less percentage changes in control workers' earnings. The shaded area is the 95th percentile confidence band.

[17] The reduction in earnings for control workers is likely a mechanical relationship caused by selecting control workers who, like the treated workers, are employed full-time in the pre-sample. Even if not treated by the MLE, some portion of these workers transition into unemployment or out of the workforce.

One year after the MLE, the treated workers suffer earnings losses that are 16.4% greater than the control group. Ten years after the MLE, their earnings losses remain 2.5% larger than the control workers. These results are similar to previous studies in the MLE literature, and we will use them as the benchmark for comparison.

Recall Figure 3, which showed that the sickest workers suffered an abrupt decline in their health status immediately prior to the layoff. This means that when we estimate equation (2) we are identifying the combined effect of previous sickness and MLE. We will present the combined effects of these two shocks by sickness group in the rest of this section.

Figure 6: Benchmark: Mass Layoff & % Earnings of t-1



6.1 Earnings, Wages, Hours and Employment

To identify the effects of MLE by sickness groups, we re-estimate equation (2) separately for sets of workers belonging to these groups: all workers, and then all workers whose sickness scores in $t = -1$ were below the 50th, 25th, 10th, 5th, and 1st percentiles respectively.[18] We use labor earnings, hourly wages, hours worked, and employment probability (= 1 if a worker is employed in that year) as dependent variables.

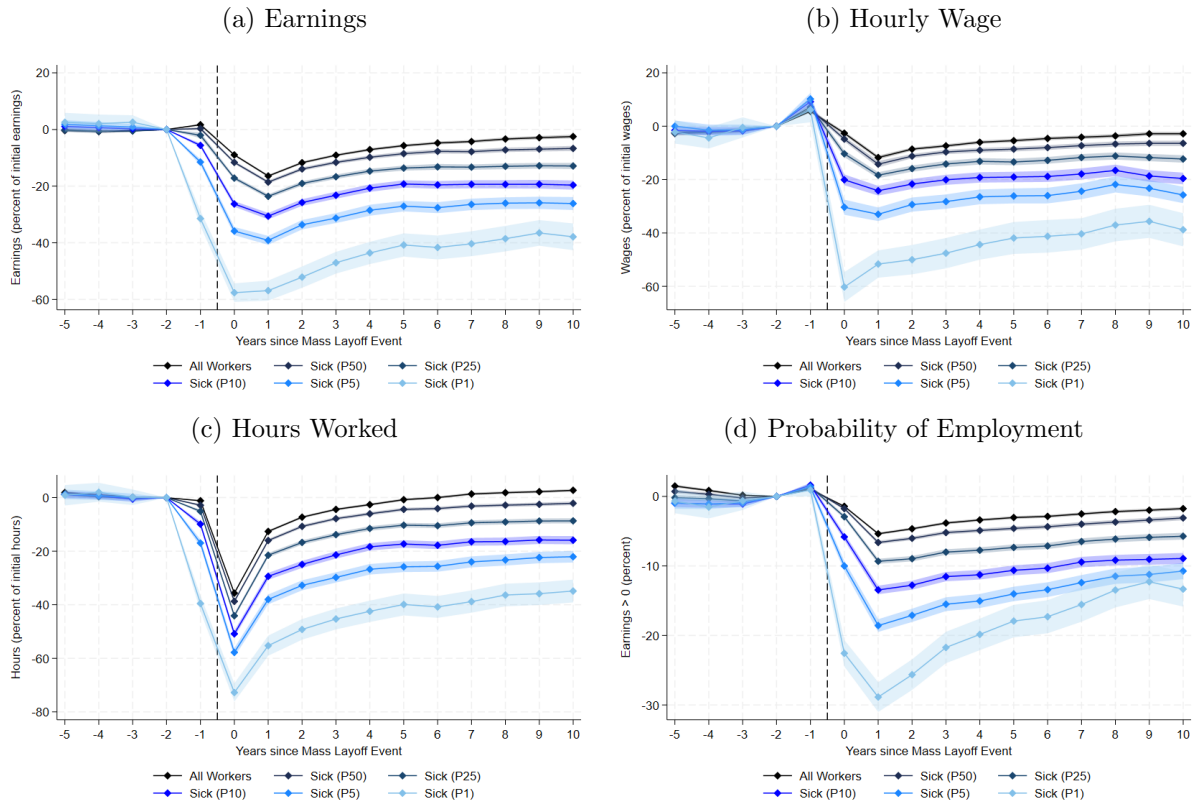
The upper left panel of Figure 7 reports our results for earnings, and the axes are the same as in Figure 6. Earnings losses monotonically increase with the severity of the

[18] These groups are nested, not disjoint.

treated workers' previous sickness, and the differences across the sickness groups are large in magnitude and statistically significant. For example, the P5 sickness workers' short-run earnings losses (at $t = 1$) are 39.1% larger than the control workers, while the P1 sickness workers' short-run losses are 59.6%, in contrast to the all-worker baseline of 16.4%.

More remarkably, these earnings losses are highly persistent. 10 years after the MLE, the P10 and P1 sickness workers' earnings losses remain 19.6% and 37.9%, respectively, greater than the control group, compared to 2.5% for the benchmark.

Figure 7: The Total Effects of Previous Sickness & Mass Layoff



Notes: .

Note that our results for long-run earnings losses capture the combined effect of sickness shocks and MLE and they are are much larger than what one would expect from sickness alone. We can see this in two ways.

First, previous studies of sickness shocks that use similar data and identification have focused on episodes such as commuting accidents, acute hospitalization, or hospitalization associated with heart attacks and strokes (HHAS). In each case they are comparing workers treated by sickness shocks (whose prevalence is much rarer than even our P1 sickness group) to demographically matched workers who did not experience them. These

studies find much smaller earnings losses, 2-3% for commuting accidents, 5% for acute hospitalization, 18% for HHAS.[19] We revisit the use of the single discrete sickness events of HHAS in combination with MLE in section 9.

Second, recall that our Sickness Composite is itself a measure of the earnings losses one would expect for an average Danish worker having that particular combination of interactions with the health care system in that year. Because we know how the Sickness Composite behaved for our displaced worker after the MLE, we can ask: how much of the observed earnings decline can be explained directly from changes in the Sickness Composite?

For example, Figure 3 shows that 10 years after the MLE, P10 workers' mean Sickness Composite is lower than their demographically matched controls by about 0.04 log points in earnings. Figure 7 shows that the actual gap in earnings losses is 19.6%. Therefore, the extra post-MLE earnings losses estimated in Figure 7 not directly explainable by changes in the Sickness Composite alone are 15.6% Results are similar at other percentiles.

Figure 3 also allows us to rule out one possible explanation for why MLE effects might be so pronounced for sicker workers. It might be that MLEs make already sick workers even sicker and this spiraling health situation progressively worsens earnings. However, that does not appear to be true, at least for the mean worker within a given sickness percentile group. Instead, the MLE effect on earnings remains large even as the Sickness Composite improves for the affected workers.

As a final exercise in this section, we decompose earnings into total hours and hourly wage, and the probability of employment[20], and re-estimate our diff-in-diff using these alternative dependent variables. We report the results in the remaining panels of Figure 7.

The results for hours, hourly wage and employment probability show patterns similar to earnings. Across all outcomes, losses monotonically increase with the severity of the

[19] Specifically, Halla and Zweimüller (2013), Austria: Sickness = commuting accident with at least one day of sick leave, with prevalence = 0.67%. Long run earnings loss = 2-3%; García-Gómez et al. (2013), Netherlands: Sickness = acute hospitalization with at least three nights of hospitalization stay, with prevalence = 0.1%. Long run gross income loss = 5%; Fadlon and Nielsen (2021), Denmark: Sickness = hospitalization due to heart attack and stroke with prevalence = 0.25%. Medium run earnings loss = 18%.

[20] Earnings, hours and employment probability are 0 for the unemployed workers, and so we set their hourly wage to 0, in order to have the same sample for all the four dependent variables.

treated workers' previous sickness, and the sick workers' long-run losses are much larger than the benchmark.

For example, immediately after the MLE P1 (P10) workers see a 28.9*pp* (13.5*pp*) reduction in the probability of employment relative to their demographically matched control groups, in contrast to a more modest 5.4*pp* reduction for the entire displaced worker sample. Ten years after the MLE, P1 (P10) workers still have 13.3 *pp* (8.9*pp*) lower probability of employment, in contrast to 1.8*pp* lower for all displaced workers.

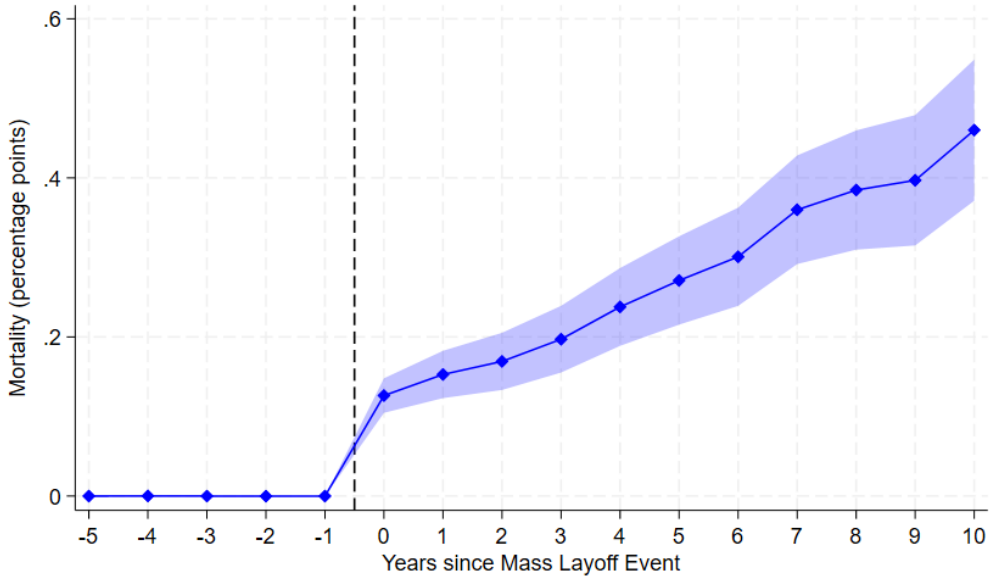
6.2 Results for Cumulative Mortality

In this section, we study the impacts of MLE on mortality and how they vary across sickness groups. We use cumulative mortality as the dependent variable in equation (2), which is the cumulative probability that a treated worker dies sometime between an MLE and t years afterwards (t ranges from 1 through 10).[21]

We start by estimating equation (2) for all treated workers. Figure 8 illustrates our benchmark results, with the vertical axis showing the change in the treated workers' cumulative mortality relative to $t = -1$ minus their control workers' changes, in percentage points (*pp*). Because our sample construction requires all treated and control workers to be alive prior to the MLE, the estimate is 0 by construction all the way up to $t = -1$. One year after the MLE, however, the treated workers suffer a 0.15*pp* increase in cumulative mortality relative to their control workers, and 10 years afterwards, their increase in cumulative mortality stands at 0.46*pp*. These sizable effects on mortality are qualitatively similar to previous studies.

[21] Deceased workers typically drop out of the data sample. To keep these individuals in the sample after death, we create ghost workers by setting the mortality dummy to 1 for the rest of the estimation sample. This works because we are not using any time-varying covariates for that individual after period $t = -1$.

Figure 8: Benchmark: MLE and Cumulative Mortality

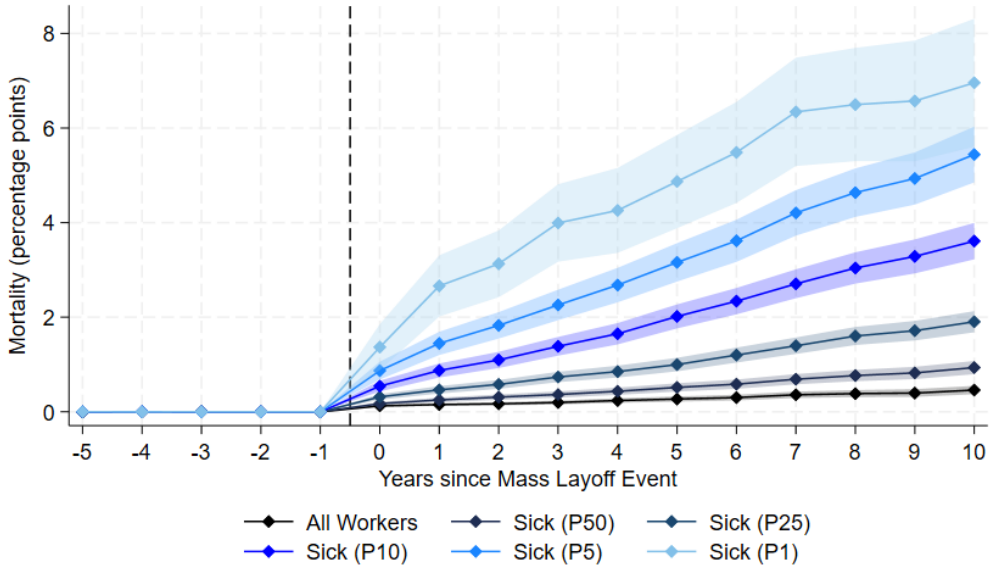


Next, we report results by sickness group in Figure 9. Mortality effects are monotonically increasing in the severity of the treated workers' previous sickness. The differences in estimates across sickness groups are large and statistically significant. One year after the MLE, the P1 sickness workers suffer a 2.7 *pp* increase in mortality, more than 17 times the effect for the all worker benchmark. These effects grow larger over time reaching 7*pp* for P1, 5.4*pp* for P5 and 3.5*pp* for P10, compared to 0.46*pp* for the all-worker benchmark.

How do we square this finding with the result from Figure 3 showing that the average level of the Sickness Composite does not worsen after MLE and in fact recovers to pre-shock levels? We see two possible answers.

One, even among these relatively sick workers, ten-year mortality is rare. Sickness Composites could worsen dramatically for a small percentage of workers and not move mean values much. Two, our Sickness Composite is correlated with 10-year mortality but it is built to explain earnings variation, not mortality directly. This is because we are primarily interested in a measure of sickness that is relevant to firms as they make decisions about pay, and layoffs. It might be that a Comorbidity Composite, built in a similar way but focused on mortality correlates rather than earnings, would capture worsening health outcomes for this small fraction of affected workers.

Figure 9: The Total Effects of Previous Sickness & Mass Layoff on Cumulative Mortality



6.3 Quantifying the Extent of Heterogeneity

In this sub-section, we quantify the heterogeneity effects shown in Figures 7 and 9 by asking, of the total decline in earnings (or decline in mortality, or increase in mortality) experienced by the displaced workers, what fraction of these effects are absorbed by the sickest workers?

We start from the observation that, at event time t, the average outcome of y_t for all workers is $y_t = SH_s \times y_{st} + (1 - SH_s) \times y_{rt}$, where the subscripts s and r represent sick workers (defined at a particular percentile of our Sickness Composite) and remaining workers, respectively, SH_s is the share of workers at that sickness percentile, and y_t can be earnings, mortality, or other outcome variables that we study. We can write a similar expression comparing changes in y_t pre- and post-MLE (a single difference) arising from changes in y_{st} and y_{rt} for a group of treated workers, or compare changes in y_t pre- and post-MLE relative to a control group (a double difference) using predicted changes in y_{st} and y_{rt} relative to control groups (see the Appendix for the details). These double difference predicted changes in outcome variables for each group are constructed from the point estimates reported in the previous section.

Table 3: Contribution of Previous Sickness to Total Effects

	t = 1			t = 10		
	Sickest 1%	Sickest 5%	Sickest 10%	Sickest 1%	Sickest 5%	Sickest 10%
Panel A. Demographics Matching						
Earnings Loss	3.25%	11.02%	17.50%	17.92%	41.53%	63.38%
Hours Loss	2.23%	8.37%	14.63%	N.A.	N.A.	N.A.
Wage Loss	4.26%	13.64%	20.32%	12.49%	40.51%	62.13%
Employment Loss	5.38%	12.53%	25.06%	7.60%	25.43%	50.86%
Mortality Increase	17.41%	47.31%	57.19%	15.12%	59.13%	78.45%
Panel B. Demographics + Previous-Sickness Matching						
Earnings Loss	1.35%	5.94%	10.89%	0.75%	7.66%	13.83%
Hours Loss	0.97%	5.25%	10.50%	N.A.	N.A.	N.A.
Wage Loss	1.75%	13.64%	20.32%	3.70%	10.05%	14.18%
Employment Loss	2.41%	9.88%	16.46%	2.86%	11.41%	21.15%
Mortality Increase	3.28%	19.63%	23.41%	2.15%	25.52%	28.74%

Notes: The long-run results are not available for hours because the benchmark estimates are positive.

We collect results in Table 3, panel A. Here we see that previously sick workers account for a very significant fraction of adverse consequences of displacement. At $t = 1$, the sickest 10% of workers are responsible for 17.5% of earnings losses, 25.1% of employment losses and 57.2% of the increased mortality. In the long run, these shares rise significantly, accounting for 63.4% of earnings losses, 50.9% of employment losses and 78.5% of the mortality increases. Put another way, this group generates about 8 times the number of deaths one would expect if the mortality increases were uniformly distributed across sickness types. If we focus on the sickest percentile of workers, they are responsible for excess deaths that are 15.1 times their population share.

In summary, we have shown that when sickness shocks are followed one year later by mass-layoff shocks, workers suffer very large losses in earnings and very large increases in cumulative mortality both in the short and long run. The results for the components of earnings, hourly wage, hours and employment probability are similar to the overall results for earnings. Relative to previous studies, which examine either sickness shocks or mass layoffs but not both, our results are not only novel but also substantially larger than what can be expected from these previous studies in isolation. This suggest an important interaction between prior sickness and layoffs.

7 The Effect of Mass Layoff Conditional on Previous Sickness

In this section, we take a first step in unpacking the mechanism for why the combined effect of previous sickness and mass layoff is so large. We estimate the partial effect of MLE, conditional on previous sickness, and study how it varies with previous sickness.

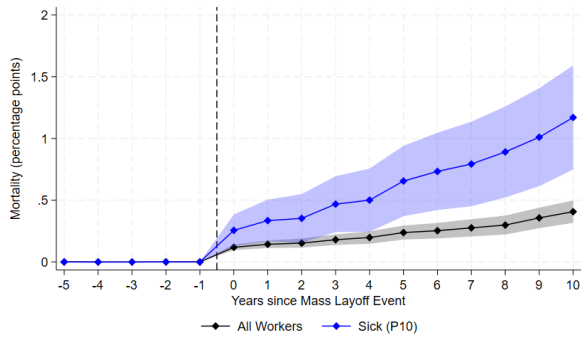
Recalling Figure 3, the use of demographically matched workers fails to track movements in the Sickness Composite for MLE-treated workers who became sick prior to the MLE. However, matching workers on both demographics and on a single year ($t = -1$) of the Sickness Composite provides us with a control group whose mean Sickness Composite tracks the treated workers throughout the sample.

7.1 Results for Cumulative Mortality

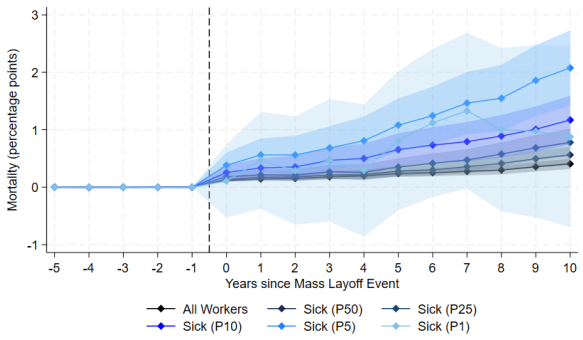
We start by estimating equation (2) with the control workers based on demographics-plus-previous-sickness matching, and with cumulative mortality as the dependent variable. Figure 10 illustrates our results. As compared with Figure 9, the vertical axis of Figure 10 has a smaller scale and the confidence bands of Figure 10 are roughly the same size.

Figure 10: The Effect of MLE on Cumulative Mortality Conditional on Previous Sickness

(a) P10 Sickness Workers



(b) All Sickness Groups



Notes: .

The left panel of Figure 10 shows the estimates for all-workers and the P10 sickness workers. One year after the MLE ($t = 1$), P10 workers' cumulative mortality rises by 0.33 *pp*, relative to the control workers, compared to 0.14*pp* for the all-workers case. By year 10, the gap in cumulative mortality has sharply widened, with P10 sickness workers'

have mortality that is 1.17*pp* higher than the control workers, compared to .41 *pp* for the all-worker benchmark.

The right panel of Figure 10 adds the other sickness groups, from P50 through P1. Remarkably, the partial effects of MLE on cumulative mortality are still monotonically increasing with the severity of the treated workers' previous sickness, even though we have conditioned our estimates using control workers who were just as sick prior to the MLE. This implies that previous sickness amplifies the causal effect of MLE on cumulative mortality.

The seminal work linking MLEs to mortality is Sullivan and von Wachter (2009) who discuss previous sickness as a major threat to identification, but lack data to control for it. We show that the causal effects of MLE on mortality remain statistically significant when using control workers with matched previous sickness. Moreover, these mortality effects are substantially amplified by the severity of the laid-off workers' previous sickness both in the short-run and in the long-run.

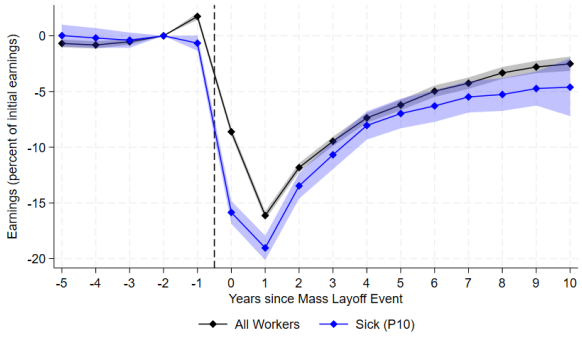
7.2 Results for Earnings

We now move on to the results for earnings. The left panel of Figure 11 shows the estimates for the P10 sickness workers (blue line) and the all-workers benchmark estimates (black line). Because we are now using demographics and sickness matched control workers, these estimates capture the partial effect of MLEs on earnings, controlling for previous sickness.

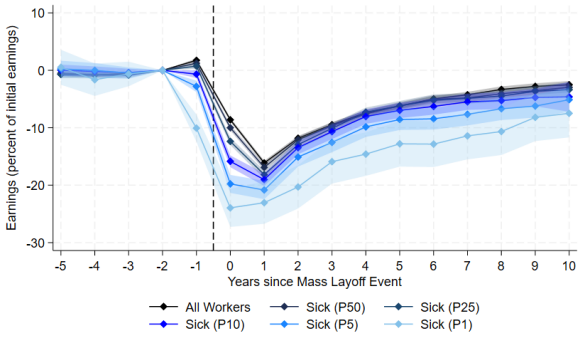
The P10 sickness workers experience earnings losses that are 19% larger than those of the control workers in the short-run ($t = 1$), and losses are 4.6% larger in the long run ($t = 10$). These are smaller differences than when our control workers were only demographically matched. However, there remain statistically significant differences relative to the all-worker baseline, which shows earnings losses of 16.1% and 2.5% in the short and long-run.

Figure 11: The Effect of MLE on Earnings Conditional on Previous Sickness

(a) P10 Sickness Workers



(b) All Sickness Groups



Notes: .

In the right panel of Figure 11, we add the estimates of the other sickness groups. The magnitudes of the earnings loss increase monotonically with the severity of the laid-off workers' previous sickness, a pattern that is similar to Figure 10. The amplification is substantial. For example, the P1 sickness workers' earnings losses are about 23% in the short-run ($t = 1$), and they remain elevated at about 10% relative to the control group at ($t = 8$), which is three times larger than the all-worker benchmark.

Panel B of Table 3 reports the sick workers' contributions to the total increase in excess deaths and the total loss in earnings from MLEs, where we continue to use the estimates of the average MLE effects from Figures 6 and 8 as the benchmark.[22] We see, for example, that at $t = 1$, the sickest 10% of workers are responsible for 10.9% of earnings losses and 23.4% of mortality increases, and that in the long-run, at $t = 10$, their contributions are 13.8% and 28.7%, respectively. These results show that the interaction between previous sickness shocks and MLEs remain significant even after we condition on previous sickness, even though its magnitude is diminished.

7.3 Mechanism for Earnings Results

We start probing the mechanism for the amplification results for earnings by estimating the results for hourly wage and total hours, the two components of earnings, as well as

[22] This is because we obtain very similar estimates for the average MLE effects conditional on previous sickness (Section 8). This approach also makes it easy to compare the results from Panels A and B of Table 3.

for employment probability. The full results for the estimates are in Appendix Figure A.1 and Panel B of Table 3 presents the sick workers' contributions to the total losses. Below, we summarize these results using the sickest 10% of workers as an example, to save space. Relative to the benchmark estimates, the long-run ($t = 10$) loss in hourly wage is the largest, a factor 1.73 difference. This is followed by the loss in employment probability, a factor 1.58 difference, and the loss in total hours, a factor 1.34 difference. On the other hand, Panel B of Table 3 shows that at $t = 10$, these workers account for 6.8% of the total hours losses from MLE, 14.2% of the total wage losses and 21.2% of the total employment losses.

Schmieder et al. (2023) link earnings losses over the business cycle to worker transitions from firms with high average earnings to firms with low average earnings. We adapt their approach to our context, looking at worker transitions between firm types and occupation types after MLE. We are particularly interested in whether previously sick workers are more likely to sort into lower earnings firms or occupations, and also whether the impact of this sorting on the post-MLE earnings of a specific individual differs based on previous sickness.

We estimate

$$\Delta_{dd} w_i = \gamma \hat{\psi}_{J(i)} + \xi \Delta_{dd} \hat{\psi}_{J(i)} + \nu w_i^{occ} + \upsilon \Delta_{dd} w_i^{occ} + \beta I(S_{i,t-1}) + \delta \Delta_{dd} \hat{\psi}_{J(i)} I(S_{i,t-1}) + \hat{\alpha}_i + X\theta + \epsilon_i, \tag{3}$$

$\Delta_{dd} w_i$ are double-differenced log earnings for worker i. Like our calculations for earnings loss in the MLE event studies earlier in the paper, we are calculating changes in earnings before and after an MLE, and relative to a control group. There are two differences. One, rather than estimating changes period by period we take a single long difference of average earnings before the MLE (from $t = -5$ to $t = -1$) to after the MLE, and where after can either reflect the short run ($t = 0$ to $t = 3$) or long run ($t = 7$ to $t = 10$). The double difference comes from comparing these changes for MLE-treated and control workers. Two, we condition on workers being employed at some point in the relevant post-MLE period, so there are no instances of zero earnings, and we can simply estimate log change in earnings. Conditioning on employment is necessary in order to compare firms and occupations pre- and post-MLE.

$\hat{\psi}_{J(i)}$ is the pre-displacement firm fixed effect for worker i implemented by estimating

the Abowd et al. (1999) model. The units for the firm FE are percentage deviations from the average firm, so a value of .05 means that (conditional on worker FE), a firm pays earnings 5% above the average.

$\xi\Delta_{dd}\hat{\psi}_{J(i)}$ is the double-differenced change in the firm fixed effect. We calculate changes in the average firm FE before and after the MLE for the treated worker and subtract changes in the firm FE (if any) for the control worker.

w_i^{occ} is economy-wide average earnings of the occupation held by worker i in their pre-displacement firm, relative to their control worker, and $\Delta_{dd}w_i^{occ}$ measures double-differenced change in that occupation wage from pre- to post-displacement employment.

We include an indictor for the previous sickness of the individual $I(S_{i,t-1})$ both directly and interacted with the double difference change in firm FE and with the double difference change in occupational wage. We experiment with both P10 and P1 sickness indicators. The remaining variables are individual controls including the individual fixed effect from the AKM model (α_i), the year of displacement c, and a vector (X_i) including worker job tenure, experience, and age, with squared terms for each.

Note that previous sickness can enter the change in earnings in three ways: directly through the indicator variable; through sick workers having systematically different values for the DiD change in the firm FE or the DiD change in the occupational wage; or through the interaction term. A non-zero interaction term means that a change in firm or occupation translates into a differential effect on earnings for this worker depending on their previous sickness.

Table 4 provides the descriptive statistics for the mean double-differenced change in earnings, $\Delta_{dd}w_i$, the firm FE, $\Delta_{dd}\hat{\psi}_{J(i)}$, and the change in occupational wage $\Delta_{dd}w_i^{occ}$. The columns correspond to workers sickness types in the short and long-run. Panel A provides estimates for the demographically matched sample, Panel B adds sickness matching.

We focus our discussion on the change in firm FE and occupational wage variables and begin with the demographically matched sample. Considering all displaced workers who re-attach to a firm in the short run, the mean displaced worker sees a change in firm FE of -.017, meaning that (relative to the control worker) the new firm pays average earnings that are 1.7*pp* below what the pre-MLE firm paid. For the P10 workers, the mean is -2.9*pp*, and for the P1 workers -5.0*pp*. Previously sick workers sort into lower paying firms.

Previously sick workers also sort into lower paying occupations, with P10 and P1 workers seeing a 2.3*pp* drop in the average occupational after earnings relative to their control workers. While this situation improves markedly over time for displaced workers as whole, it does not get better for P10 and P1 workers. They have roughly the same reduction in firm FE , and even larger reductions in occupational earnings in the long run. This likely reflects a selection effect in the timing of re-attachment to the workforce. Even within sickness percentiles, workers with lower ability or worse recovery from the shock take longer to reattach, and when they do, they re-attach to lower earning occupations.

We see a more moderate version of the same story when we add sickness matching. Mean earnings changes in the short run are smaller and even positive in the long run. (Recall, unlike earnings estimates shown in Figures 7 and 11, these earnings are conditional on re-attaching to the work force.) However, previously sick displaced workers still attach into worse firms than their sickness-matched control workers, possibly because the control workers are much less likely to switch firms. The occupational earnings changes are still negative, but much smaller once we match on previous sickness.

The data in Table 4 reflect characteristics of firms and occupations into which displaced workers attach. We now examine whether these characteristics help explain the extent to which earnings for specific workers decline.

Table 4: Change in Earnings, Firm Fixed Effects, and Occupational Earnings Distribution by Sickness, Time, and Sample

	Short run (year 0-3)			Long run (year 7-10)		
	All workers	P10	P1	All workers	P10	P1
Panel A. Demographic matched sample						
DiD log Earnings ($\Delta_{dd} w_{ic}$)	-0.073	-0.237	-0.440	0.019	-0.146	-0.245
DiD Firm FE ($\Delta_{dd} \psi_{J(i)c}$)	-0.017	-0.029	-0.050	-0.006	-0.027	-0.054
DiD log Occupational Earnings ($\Delta_{dd} w_{ic}^{occ}$)	-0.008	-0.023	-0.023	-0.005	-0.042	-0.044
Panel B. Demographic + sickness matched sample						
DiD log Earnings ($\Delta_{dd} w_{ic}$)	-0.077	-0.114	-0.145	0.018	0.004	0.028
DiD Firm FE ($\Delta_{dd} \psi_{J(i)c}$)	-0.017	-0.025	-0.032	-0.008	-0.015	-0.021
DiD log Occupational Earnings ($\Delta_{dd} w_{ic}^{occ}$)	-0.008	-0.011	-0.015	-0.004	-0.009	-0.005

Notes: The difference-in-differences (DiD) measures are defined as the within-match difference in changes in average outcomes from the pre-period (t = -5 to -1) to the post-period (t = 0 to 3 or t = 7 to 10) for treated and control workers. Individual and occupational earnings are measured in logs. The sample is restricted to matches in which both workers have observed employment in both periods, defined by non-missing earnings, occupation, and firm fixed effects.

We estimate equation (3) and report the results in Table 5. Panel A reports the demographic matched sample and Panel B adds sickness matching. We include both

short- and long-run changes and either include a P10 or a P1 sickness indicator and their interactions with the DiD Firm FE. (Specifications that featured interactions between sickness indicators and DiD occupational earnings are insignificant and have been omitted for brevity.)

The sickness percentile dummies in Panel A show a direct negative effect on earnings in the short run, and a weaker effect in the long-run. Once we match on sickness in Panel B, these effects go away or even turn positive. This is consistent with the means reported in the first row of Table 4 More interesting and novel to this specification are the DiD firm and occupation effects and their interactions with sickness.

The coefficients on DiD occupational earnings are around .38 in the short run and .67 in the long run. This means that moving to an occupation that pays 10% lower average earnings translates to 3.8% lower (short-run) to 6.7% (long-run) lower earnings for this specific MLE-displaced worker.

Across all specifications, the coefficient on the DiD firm FE are consistently close to 1, with similar interpretation to the occupational earnings coefficient. However, once we add interactions with sickness percentiles, the "pass-through" of firm average earnings to the earnings for a specific MLE treated worker exceeds 1. For example, in Column 8 we see that when a P1 worker sorts into a firm with 10% lower average earnings, they actually receive 17% lower earnings.

Table 5: Sources for Earnings Losses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Demographic matched sample				*Panel B. Demographic + sickness matched sample*			
Dependent variable: DiD log Earnings ($\Delta_{dd} w_{ic}$)	Short run (year 0-3)		Long run (year 7-10)		Short run (year 0-3)		Long run (year 7-10)	
Firm FE (t=-1)	0.021	0.023	0.129***	0.128***	0.032**	0.033**	0.114***	0.114***
	(0.017)	(0.017)	(0.023)	(0.023)	(0.016)	(0.016)	(0.022)	(0.022)
DiD Firm FE ($\Delta_{dd} \psi_{J(i)c}$)	0.937***	0.948***	1.063***	1.070***	0.913***	0.922***	1.057***	1.057***
	(0.012)	(0.012)	(0.015)	(0.015)	(0.012)	(0.012)	(0.015)	(0.015)
Sickness Percentile (Dummy, t=-1)								
P10	-0.123***		-0.065***		0.005		0.034**	
	(0.009)		(0.013)		(0.009)		(0.013)	
P1		-0.242***		-0.075		0.035		0.051
		(0.034)		(0.048)		(0.035)		(0.055)
Sickness Percentile (Dummy, t=-1) x DiD Firm FE								
P10	0.125***		0.124**		0.174***		0.031	
	(0.042)		(0.057)		(0.040)		(0.059)	
P1		0.181		0.400**		0.713***		0.555**
		(0.128)		(0.196)		(0.122)		(0.223)
log Occupational Earnings (t=-1)	-0.132***	-0.126***	-0.204***	-0.202***	-0.125***	-0.125***	-0.183***	-0.184***
	(0.010)	(0.010)	(0.014)	(0.014)	(0.010)	(0.010)	(0.014)	(0.014)
DiD log Occupational Earnings ($\Delta_{dd} w_{ic}^{occ}$)	0.384***	0.386***	0.675***	0.676***	0.372***	0.371***	0.660***	0.660***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)
Observations	166,010	166,010	107,073	107,073	165,524	165,524	107,335	107,335
R-squared	0.072	0.071	0.124	0.124	0.068	0.069	0.120	0.120

Notes: Standard errors in parentheses, $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$. The difference-in-differences (DiD) measures are defined as the within-match difference in changes in average outcomes from the pre-period (t = -5 to -1) to the post-period (t = 0 to 3 or t = 7 to 10) for treated and control workers. Individual and occupational earnings are measured in logs. The sample is restricted to matches in which both workers have observed employment in both periods, defined by non-missing earnings, occupation, and firm fixed effects.

We can use these estimates in combination to see how much of the change in firm and occupation contributes to the overall decline in earnings for the sicker workers. Consider the P1 workers in the demographically matched sample. Their long-run earnings decline is -0.245. Their average change in occupational earnings is -0.044 multiplied by a passthrough elasticity of 0.675, or -0.029. Their average change in firm FE is -.054 multiplied by a passthrough elasticity of 1.47 or -0.079. Adding these together, switching to new firms and new occupations explains (.029+.079)/.245 = 44% of the earnings change. Applying these same calculations, firm and occupation switching explains 39% (24%) of the short run earnings for the P1 (P10) workers in the demographics + sickness matched sample.

8 The Average Effects of MLE Conditional on Previous Sickness

So far we have documented the rich interactions between previous sickness shocks and MLE, such as the substantial heterogeneity of the causal effects of MLE on earnings and

mortality with respect to the severity of previous sickness. In this section, we investigate whether selection on previous sickness significantly changes estimates of average treatment effects. We estimate the average treatment effects of MLE, conditional on previous sickness, and then compare them with the benchmark estimates that do not use previous-sickness data. We then provide a counter-factual of what the average treatment effect would have been without selection on previous sickness.

We follow Flaaen et al. (2019) and specify the average treatment effect as:

$$E(\Delta Y|MLE) = \sum_s wt_s E(\Delta Y_s|MLE_s), \tag{4}$$

where the subscript s represents groups of previous sickness, wt_s denotes the weights for group s, and the dependent variable, Y, can be either earnings or cumulative mortality. Equation (4) says that when we explicitly control for previous sickness in the estimation, the average treatment effect of MLE, $E(\Delta Y|MLE)$, can be computed as the weighted average of the group-specific estimates, $E(\Delta Y_s|MLE_s)$, some of which we have presented previously. Below, we present two versions of the average treatment effects. They use the same group-specific estimates but differ in the weights, wt_s.

In our first approach, we break the sickness composite at $t = -1$ into deciles and estimate equation (2) for each decile using the control workers with similar demographics and previous sickness. We have shown the results for the top decile, P10, previously, and the estimates for the other deciles, such as P20-P10, are available upon request. We then set wt_s to be the shares of the pre-MLE sickness deciles that we observe in our estimation sample, post MLE (at $t = 0$). Therefore, this version of average treatment effects is inclusive of the selection of MLE on previous sickness, which we document in section 4. Because of selection, the value of wt_s exceeds 10% for the sickest deciles, and vice versa.

In our second approach, we employ the same group-specific estimates but fix $wt_s = 10\%$ for every decile of the pre-MLE sickness. This version of average treatment effects provides a counterfactual scenario in which MLE had no selection effect on previous sickness.

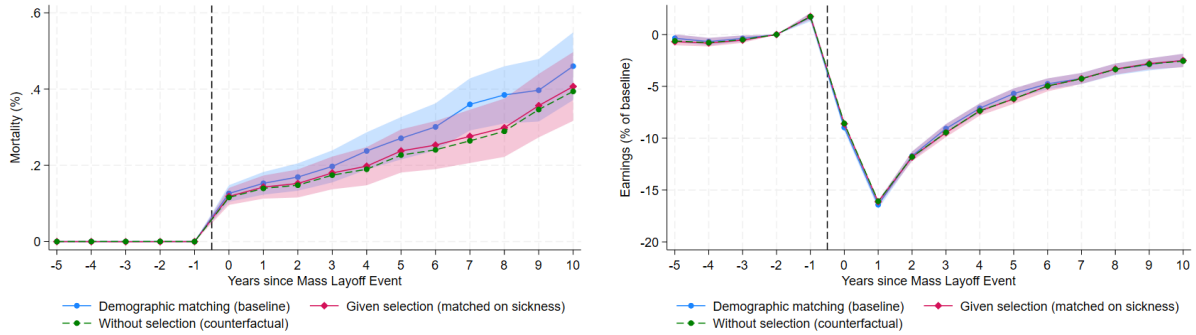
Figure 12 illustrates our results of average treatment effects on mortality and earnings. The blue line shows the benchmark estimates (demographic matching), the red line shows the first average treatment effects, inclusive of selection, and the dotted line provides the

no-selection counterfactual.

Figure 12: The Average MLE Effect without Selection, Conditional on Previous Sickness

(a) Mortality

(b) Earnings



Notes: .

For mortality, our two sets of average treatment effects are similar, and they are both smaller than the benchmark. These differences are sometimes statistically significant, but their magnitudes tend to be small. For earnings, the estimates are very similar for all three approaches. The reason is two-fold.

One, while very sick workers are both selected into the sample and have significantly worse mortality increases and earnings losses relative to the average worker, there are not enough of them to move the average treatment effect. Two, if we naively estimate earnings and mortality effects for the sickest workers relative to a demographically matched control group, we find larger adverse effects from MLE than if we control for sickness. The reason is that the demographically matched control workers are healthier on average than the displaced sick workers. But when we do the same for the healthiest workers we find the opposite, i.e. these workers actually see *improvements* in mortality and earnings relative to a demographically matched control because now the control workers are sicker than a healthy displaced worker. These effects balance each other out when combined into an average treatment effect.

In summary, we have shown that the average treatment effects of MLE conditional on previous sickness are very similar to the estimates that can be obtained without previous-sickness data. This result may be useful for future studies that focus on average treatment effects. However, it remains the case that there are significant interactions between previous sickness shocks and MLEs, and a focus on average treatment effect masks significant differences across workers based on previous sickness.

9 Robustness

In this section, we provide robustness evidence of three kinds. First, we examine whether our estimates are capturing something about prior sickness or whether they instead are simply capturing age effects. Second, we examine the extent to which gender differences in workforce attachment and health may affect our results by focusing on males only. Third, we examine the validity of our Sickness Composite approach by focusing on a single major diagnosis: hospitalization due to heart attacks and strokes.

9.1 Age Heterogeneity

Older workers make more extensive use of the healthcare system and from Table 1 are over-represented among our sickest workers using the Sickness Composite. They may also behave differently after a MLE, e.g. being less likely to retrain for new forms of work and more likely to exit the workforce. This raises the question of whether the heterogeneity across sickness states we have captured throughout the paper reflects sickness per se, or whether it is simply a stand-in for age.

We start by noting that age is included as one of the demographic characteristics on which we exact-match, and so in all our specifications, the treated and matched control workers have very similar average age (Table 1) . In addition, we control for age in the Mincer wage regression that we use to construct the sickness composite measure. Therefore, our main results are unlikely driven by age. But we can ask, if previous sickness amplifes the adverse effects of MLE, is there extra amplification for workers who are both sick and old.

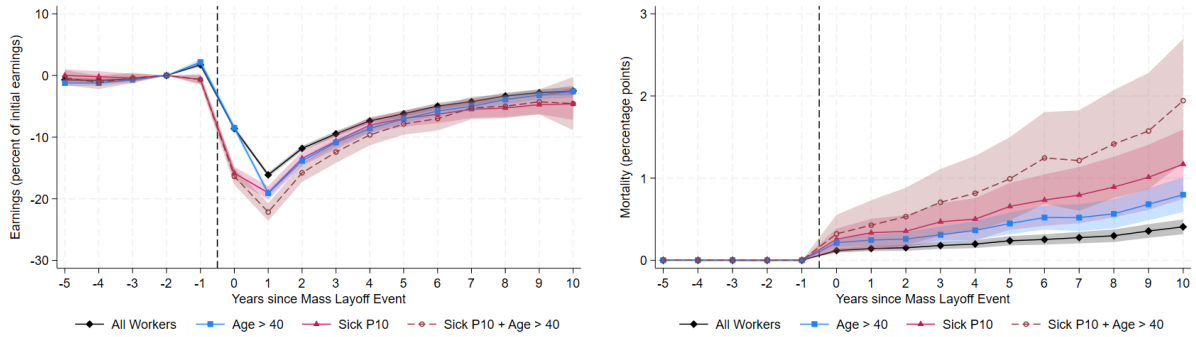
We begin with our estimation procedure from Figure 11, where we have built our control sample using a combination of demographic and sickness matching. We then create two new subgroups of workers: those who are over 40, and those who are over 40 and belong to the P10 sickness percentile. We plot the earnings results for these sub-samples in Figure 13(a), along with the all-worker and P10 worker samples.

These results show that being older also amplifies earnings loss from MLE, but only in the short run. Over longer periods, earnings trajectories for older workers converge back to the all-worker baseline. In contrast, P10 sickness workers decline and stay below the all-worker baseline throughout the sample period. When we combine age and P10

sickness status, we see greater earnings losses than is found using either condition in isolation. However, the additional contribution of age on top of prior sickness is no longer significant after 3 years and the earnings trajectories converge.

Figure 13: The MLE Effects for Old Age and Previous Sickness, Conditional on Previous Sickness

(a) Earnings (b) Mortality



Notes: .

Figure 13(b), focuses on mortality using the same sub-sample approach. Being older amplifies the MLE effects on mortality, though its impact is smaller than P10 sickness in all periods. Older P10 sickness workers suffer even larger mortality increases. Unlike earnings this gap grows in size over time and becomes statistically significant 4 years after the MLE's.

Overall, Figure 13 shows that relative to previous sickness alone, old age and previous sickness further amplify the MLE effects on earnings and mortality. While the extra amplification effects for earnings losses are concentrated in the short-run, those for mortality increases take place in the long-run and remain persistent.

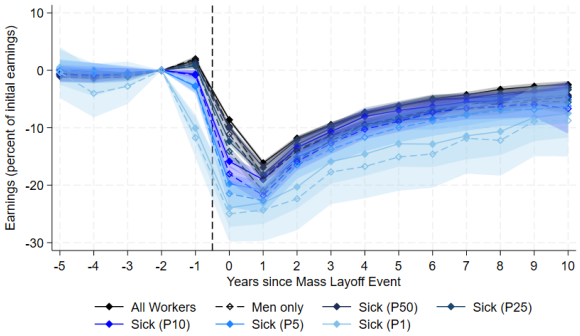
9.2 Males Only

There are gender differences in access to medical services, and Table 1 shows that women are over-represented in our sickest worker percentiles. This could be problematic for our estimation if these conditions reflect pregnancy- and birth-related access to health care, as these are likely to also impact employment and earnings post-MLE.

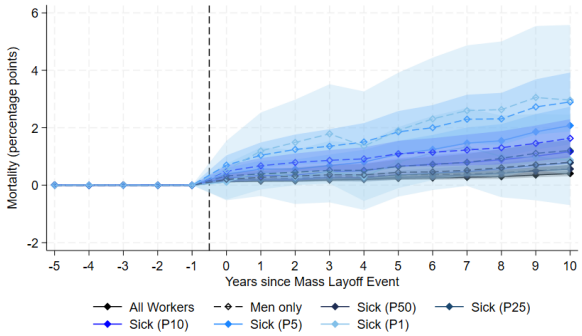
Rather than attempt to tabulate the impact of all pregnancy-related conditions appearing in the data, we simply restrict our sample to males only to determine whether the main results go through. This reduces our sample size by about one-third.

Figure 14: The MLE Effects for Men, Conditional on Previous Sickness

(a) Earnings



(b) Mortality



Notes: .

Again, we match on previous sickness plus demographics. Figure 14(a) shows our earnings results and Figure 14(b) shows our mortality results. For each sickness percentile and for all workers, we repeat the plot from Figure 11 as the benchmark (solid lines), and then report the additional plots for the males-only sub-sample (dashed lines).

Similar to our main results, the partial effects of MLE's on men's earnings losses and mortality increases remain large, and they also increase with the displaced workers' previous sickness. In addition, the estimates for men tend to be larger in magnitude than those in our main results, especially those for mortality.[23]

9.3 Heart Attacks or Strokes

A strength of our Sickness Composite is that we can characterize sickness levels for the entire treated population and find control workers with similar aggregate scores on the composite. A weakness is that the treated and control workers might have different underlying medical states, with different recovery windows and different implications for workforce participation.

To address this, we experiment with using a single severe condition, hospitalizations due to heart attacks or strokes (HHAS). This allows us to match on workers who also suffered HHAS at $t-1$ and then assess whether an MLE leads to larger earnings losses and mortality increases for HHAS workers than for all workers. This strength of this

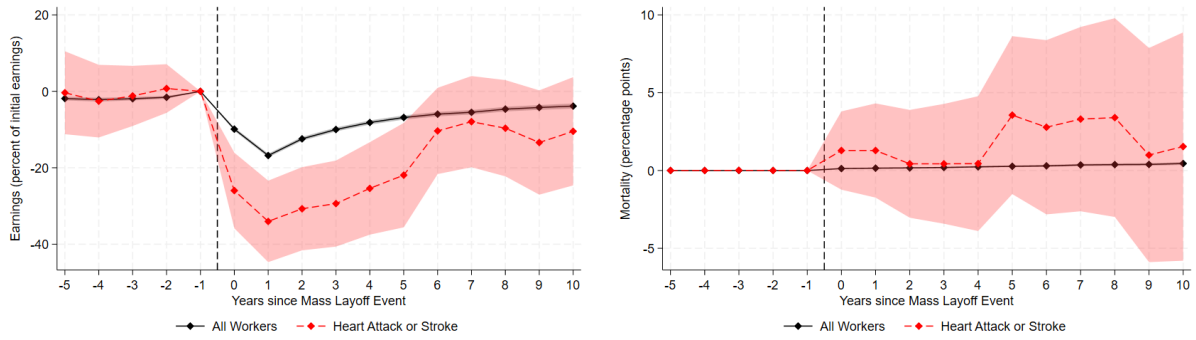
[23] For example, for the P10 sickness workers, men's mortality increases by about 1.6 *pp* in the long-run ($t = 10$), vs. the 1.2 *pp* increase in our main results, and the difference is statistically significant.

approach is that treated and control workers have the same condition (though they may differ in intensity).

The downside of this approach is that HHAS events are rare, with a prevalence of 0.25% in our data. Therefore, the size of our estimation sample shrinks to 234 treated-control worker pairs per event year, from around 300,000 pairs in our main specification.

Figure 15: The MLE Effects for Heart Attack or Stroke

(a) Earnings (b) Mortality



Notes: .

We report our results in Figure 15, where the solid lines are the benchmark estimates employing all workers in the MLE relative to their sickness-matched controls. The left panel of Figure 15 shows that one year after an MLE, the laid-off workers who suffered HHAS one year before the MLE experienced the extra earnings loss of 35%, relative to the control workers who also suffered HHAS in the same years as they did.

The magnitude of this loss is twice as large as the benchmark loss for all MLE workers, and the estimate of the loss is statistically significant despite the small sample size. The extra earnings loss from the interaction of HHAS and MLE (relative to MLE) remain numerically large and statistically significant through $t = 5$. These results show that HHAS substantially amplifies MLE earnings losses.[24]

Figure 15(b) focuses on mortality. The point estimates indicate that HHAS amplifies the mortality effects of MLEs, but the estimates are statistically insignificant. A likely explanation is that mortality is rare, which makes it difficult to discern differential effects from a sample size of only 234 worker pairs.

[24] Fadlon and Nielsen (2021) show that HHAS lead to substantial short- and medium-run earnings losses. They do not explore how HHAS interact with MLEs.

10 Conclusions

A number of important studies have used mass layoff events (MLEs) to separate quits from involuntary layoffs and to study how these layoffs affect worker outcomes. MLEs have a pronounced affect on the earnings and on the morality rates of displaced workers. This opens an interesting question.

Was it the layoff itself that led to the increased mortality? Or were the treated workers different on some (previously) unobservable dimension that (a) caused them to be selected into the layoff; and (b) made them more likely to suffer mortality after it? And if so, did this unobserved heterogeneity (c) also contribute to measured earnings losses for these workers after layoffs? An obvious candidate for the unobserved dimension is the previous sickness of workers, which a second literature has shown is also strongly related to worker earnings and mortality.

To answer these questions it is important to understand how previous sickness shocks interact with mass layoffs. We use Danish administrative data that covers the universe of private-sector firms, workers, and their healthcare transactions and find rich interactions between mass layoff events and worker's pre-MLE health.

We develop a comprehensive Mincer-regression-based measure of workers' previous sickness that, in the population of private-sector Danish workers, is strongly associated with both earnings and future mortality. It also provides a useful measure of sickness shocks because its over-time variation for individuals is dominated by large but temporary swings in its value. Unlike previous studies that focus on discrete and rare events such as severe hospitalizations and car accidents, this measure is continuous and comprehensive for the population. This enables us to study the effect of MLEs across a continuum of worker sickness, and to better identify control workers who match on both demographics and previous sickness.

This approach provides us with a set of novel findings. First, firms "cull the herd", selectively laying off previously sick workers during MLEs. Second, previously sick workers who are displaced in a MLE, including those at the bottom 25% of sickness states, suffer large earnings losses and large increases in mortality. These effects are especially large if we match only on demographics – for example, the sickest 10% of workers are responsible for 80% of excess mortality after a layoff. But the differential effect of previous sickness on post-MLE outcomes persists even when we match control workers based on

both demographics and previous sickness.

Third, an important mechanism for this finding is that previously-sick workers have a stronger tendency after layoffs to switch into low-wage firms and low-wage occupations after MLEs. They also suffer much larger earnings losses from a switch into low-wage firms than healthy workers. Fourth, while sickness drives remarkable heterogeneity in effects, the selective layoff of sick workers is not strong enough to invalidate estimates of average treatment effects found in previous MLE research.

More broadly, our findings might be useful for future work and policy. Here are three examples. One, the sickness composite provides a continuous and comprehensive measure of worker sickness that could be employed in the studies without individual-level data for sickness, because the weights we have estimated, at the population level, might be useful for other samples.

Two, an important question related to the churn of workers through the labor market is the extent to which firms hire and fire based on productivity. Individual workers' productivity can be very difficult to observe, but our measure of previous sickness might provide a useful source of variation.

Three, the strong interactions between previous sickness and layoffs may have important implications for the design of unemployment insurance and return-to-work programs. This includes both the recognition of which workers are selected into layoffs and their differential ability post layoff to reattach to the workforce in high-paying jobs.

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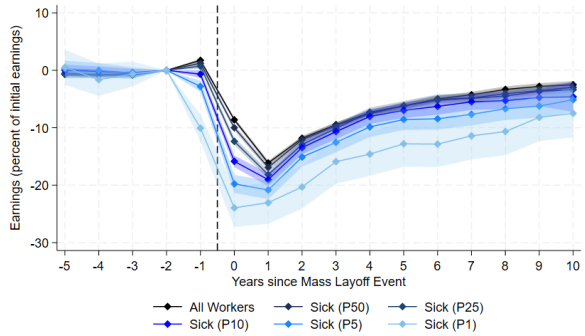
A Appendix

Table A.1: Correlation Between Sick Days and Health Outcomes

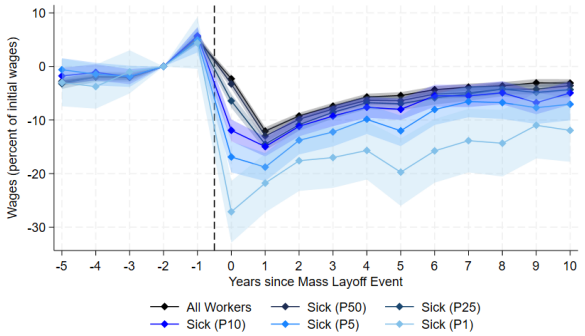
	(1)	(2)
Dependent variable: Sick days	Univariate Regressions	Multivariate Regression
10-year mortality rate	7.810***	3.111***
	(0.026)	(0.025)
Doctor visits	0.227***	0.165***
	(0.000)	(0.000)
Antidepressants	23.643***	18.773***
	(0.018)	(0.019)
Diabetes drugs	5.146***	0.654***
	(0.044)	(0.043)
Sleep drugs	16.377***	7.809***
	(0.024)	(0.024)
Heart drugs	10.119***	-0.650***
	(0.048)	(0.050)
Stroke drugs	10.329***	2.654***
	(0.025)	(0.026)
Insomnia	18.780***	4.453***
	(1.007)	(0.990)
Sleep disorders	8.982***	3.199***
	(0.109)	(0.107)
Heart attack	35.746***	18.802***
	(0.137)	(0.145)
Hemorrhage	74.336***	61.061***
	(0.385)	(0.377)
Infarction	55.794***	43.517***
	(0.166)	(0.163)
Poisoning	23.701***	3.623***
	(0.125)	(0.137)
Diabetes	9.563***	2.950***
	(0.068)	(0.067)
Severe stress	85.945***	75.759***
	(0.248)	(0.242)
Depression	78.809***	56.805***
	(0.397)	(0.389)
Work problems	92.316***	84.036***
	(0.508)	(0.496)
Burnout	52.716***	44.535***
	(0.742)	(0.724)
Alcohol diagnoses	16.848***	4.724***
	(0.097)	(0.096)
Digestive disorders	9.089***	3.676***
	(0.062)	(0.061)
Blood pressure	14.141***	8.169***
	(0.095)	(0.093)
Circulatory	19.467***	9.974***
	(0.052)	(0.057)
Accident	8.066***	6.327***
	(0.019)	(0.018)
Suicide attempts	27.756***	5.231***
	(0.185)	(0.202)
Metabolism	13.095***	6.503***
	(0.156)	(0.152)
Observations	69,927,949	69,927,949
R^2	-	0.055
Year FE	Yes	Yes
Mincer Controls	Yes	Yes

Figure A.1: The Total Effects of Previous Sickness & Mass Layoff (Conditional on Previous Sickness)

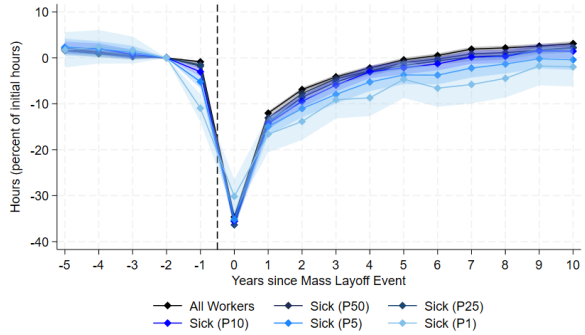
(a) Earnings



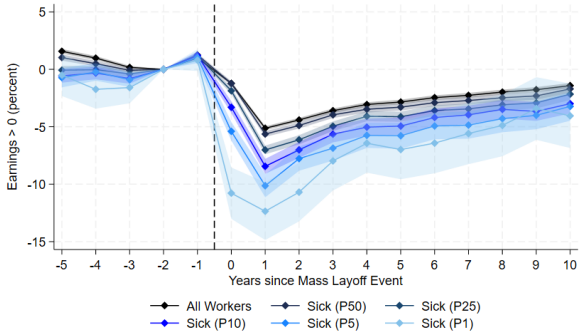
(b) Hourly Wage



(c) Hours Worked



(d) Probability of Employment



Notes: .

Figure A.2: (log) Occupational Earnings around Mass Layoffs (P10 versus All Workers)

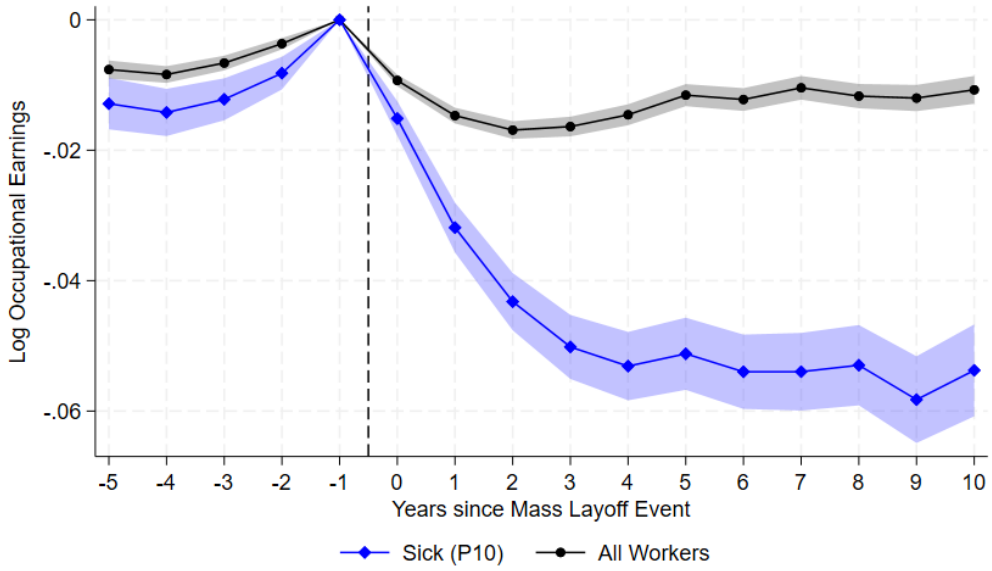


Table A.2: Share of sick workers in different layoff scenarios (%)

	Change in Employment			
	-0-10%	-10-20%	<-30%	MLE
P1	1.06	1.13	1.04	1.21
P5	5.16	5.38	4.98	5.69
P10	10.26	10.59	9.90	10.91
P25	25.51	25.89	24.40	25.47
P50	49.31	49.69	48.16	48.88

Notes: This table reports the share of sick workers across firms facing different layoff scenarios. The rows refer to percentiles of the sick-worker share (P1, P5, P10, P25, and P50), while the columns refer to layoff scenarios defined by employment reductions (0–10%, 10–20%, more than 30%, and mass layoff events). The sample consists of private-sector firms with more than 50 employees.